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Do spatially targeted redevelopment programs spillover?[☆]

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ABSTRACT

This paper estimates spillover effects from a spatially-targeted redevelopment program, the Federal Empowerment Zone (EZ), on neighboring and economically similar areas. EZs are a set of generous tax incentives and grants aimed at small, economically depressed areas of large U.S. cities. We find areas that border or are economically similar to EZ locations experience a decline in the number of establishments and employment compared to areas that border or are similar to rejected EZ applicants. We also demonstrate that using spillover prone areas to estimate program effects causes upward bias when the spillover is negative. We find that for many of our estimates, spillovers more than offset positive program effects, although there are instances when the net effect is small and positive.

1. Introduction

It is common practice for state and local governments in the United States to offer incentives aimed at encouraging economic redevelopment. The U.S. Federal Government is also involved in spatially-targeted economic redevelopment by designating tax incentives and grants through the Empowerment Zone (EZ) program. The EZ program designates parts of cities where the federal government offers generous incentives for establishments to relocate and invest; the Department of Housing and Urban Development estimates the program's annual value at $11 billion.[1] Federal involvement in spatially targeted economic development raises concerns about the efficiency of such activity. One concern is that spatially targeted redevelopment policy may result in spillovers on neighboring or competing areas. Despite the growing number of evaluations of spatially targeted redevelopment policies, most of the previous literature ignores the potential for these programs to cause spillover effects in neighboring areas; with the notable exceptions of Dye and Merriman (2000), Weber et al. (2007), Chirinko and Wilson (2008), Neumark and Kolko (2010), and Ham et al. (2011).

This paper empirically tests to what extent the Federal Empowerment Zone program causes spillovers on neighboring and economically close areas. In theory, spillover effects could be positive or negative. If spatially targeted policies are successful at attracting establishments from outside the immediate area or creating new establishments and new jobs there may be a positive effect on neighboring areas through the forces of agglomeration. Spillover effects could also be negative if the incentives offered by the program cause establishments to leave neighboring areas in favor of the targeted area, or if establishments and jobs in neighboring areas are destroyed through competition from targeted areas. If spatially targeted incentives cause spillovers, these effects should be considered in any analysis of these policies. In addition, because many evaluations of spatially targeted redevelopment policies use areas that are either geographically or economically close as a control group, understanding spillovers informs the methodology used to evaluate policy. If spillovers occur on comparison areas used as a benchmark for evaluation, the estimated effects of policy are biased, as the presence of spillovers violates the no interference

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[1] This estimate includes dollars allocated for Renewal Community and Enterprise Community areas in addition to the larger and more generous Empowerment Zone program. The annual tax expenditure budget estimates the forgone revenue associated with these programs at about $1.7 billion annually, substantially lower than the HUD value estimates. Some of the difference between the HUD and tax expenditure estimates might be explained by direct spending involved in the program through social service block grants, although these grants totaled less than $1 billion and were only allocated once at the start of the program.

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assumption between treatment and control groups necessary for policy evaluation (Rosenbaum, 2007).[2]

To test for spillover effects from the EZ program, we compare how outcomes in areas that are close to the EZ designated areas changed with the introduction of the program, relative to the change for areas that are close to rejected applicants of the program. We test for spillovers using *geographically* close groups that share a census tract border with either the actual EZ areas or rejected applicants. We also test for spillovers using *economically* close groups along several dimensions, including those that met the criteria for eligibility under the program, and those that are similar along multiple dimensions as measured by a propensity score.

Using data from the Dun and Bradstreet Marketplace database, we find that areas sharing a common census tract border with EZ locations experience a decline of as many as 16 establishments in the short term (1 year after the program starts) compared to areas that border the rejected applicants. The negative spillover from the EZ program grows to a loss of as many as 20 establishments in the longer term (5 years). We find similar negative spillover effects on establishments in areas that are economically close to EZ locations. Losses are especially strong in the retail and service sector, where previous research shows nearly all new establishment gains in targeted areas occur. Employment at establishments in geographically close areas also declines, by as much as 90 employees in the short term and 264 in the longer term. Employment losses are larger in economically similar areas, with estimates showing as many as 430 jobs lost in these areas in the long term. We demonstrate that using spillover prone areas as a comparison group to estimate program effects produces results that suggest substantial gains in the number of establishments in targeted areas. Most of our estimates suggest that the size of the spillovers more than offsets gains from the program, although there are instances where the net effect is still small and positive.

The remainder of the paper starts by discussing the advantages of using the Federal Empowerment Zone to study spillover effects of spatially targeted economic redevelopment policy. Section 3 outlines our identification strategy. Section 4 describes the data we use to estimate the spillover effects of EZs on establishment location in surrounding areas. Section 5 discusses our empirical results, and the final section of the paper offers concluding comments.

2. Why use the Federal Empowerment Zone to examine spillovers?

The Federal Empowerment Zone program is a good candidate to examine potential spillover effects for several reasons. First, the program clearly defines areas where incentives are available and the economic criteria for areas to be eligible for them, allowing identification of geographically and economically close areas that are likely to be prone to spillover effects.[3] In 1994, EZ status was designated in parts of six cities (Atlanta, Baltimore, Chicago, Detroit, New York, and Philadelphia/Camden), leaving ample time for the effects of the program to take hold and for data generation.

Second, the EZ program offers generous incentives for establishments to locate in designated areas (and hire residents of those areas), and there is at least some evidence that the program was successful in improving targeted areas, although we would describe this evidence as mixed.[4] The most robust finding, shown by Krupka and Noonan (2009), Hanson (2009), and Busso et al. (2010), is that the EZ increased local property values in an economically and statistically significant way. Oakley and Tsao (2006) find that some EZ areas experienced reduced poverty and unemployment, but overall they find no statistically significant positive outcomes for zone residents. Busso et al. (2010) report as much as a 19% increase in jobs available to zone residents; however, they find no measurable effect on wages. Hanson (2009) finds no effect of zone designation on the employment rate of zone residents. Ham et al. (2011) find that the federal EZ program is responsible for a substantial reduction in unemployment, increase in employment, and increase in wage and salary income for zone residents. Some results in Ham et al. (2011) rely on using areas that are geographically close to EZs as a comparison group, and the magnitude of the positive effects diminish considerably when excluding these areas from the comparison group.[5]

In terms of establishment re-location, Hanson and Rohlin (2011a) find that the EZ is responsible for attracting new establishments to the area, and the effect is quite large for establishments in the retail (about 40 new establishments) and service (about 5 new establishments) sectors. In addition, Hanson and Rohlin (2011b) find that the EZ is responsible for industry level churning of establishments in the EZ area—with retail and service establishments gaining share at the expense of other sectors.

Third, the EZ program requires establishments to locate within the defined geographic area and hire residents of the same area. The incentives are a package of tax benefits that include up to a $3000 per employee tax credit for wages paid, and incentives for investment in capital.[6] In addition to the tax benefits offered for establishments, local governments may issue tax exempt bonds to assist establishments in the purchase of property. Designation also came with a one-time allocation of $100 million in social service block grants for use in the designated area.[7] For more detail on the incentives associated with EZ areas see Hanson (2009).

Lastly, not all applicants are granted EZ status, making for a useful control/comparison group to study the spillover effects from designation on surrounding areas. Areas that applied for EZ designation but were rejected, received a less generous package of assistance called Enterprise Communities (EC). We use areas that were adjacent to the EC areas to build a counter-factual for what would have happened in areas that are adjacent to actual EZ areas. The fact that EC areas receive some benefits is advantageous for our purposes, as the department of Housing and Urban Development maintains a record of their borders. Importantly, the EC benefits were inconsequential with respect to the EZ benefits—not allowing use of the wage tax credit or all of the capital incentives, and receiving only $3 million in block grants, so any spillovers from being near them would likely be extremely small.

[2] If there are positive spillover effects on comparison areas, then the effect of these programs would be underestimated. If there are negative effects on comparison areas, then the effect of these programs would be overestimated.

[3] EZ areas are defined by groupings of 1990 census tracts. The Census Bureau defines census tracts as "statistical subdivisions of counties". Census Tracts average 4000 residents and range from 2500 to 8000 residents. Every Metropolitan Area or Urbanized Area in the United States is completely divided into census tracts. Since the primary concern in defining tracts is the population, the land area of tracts varies widely.

[4] There is a literature that examines the effect of U.S. state-level geographically targeted incentive programs that we would also describe as finding mixed results. Several studies find positive effects including Papke (1994), O'Keefe (2004), and Billings (2008), and others finding small or no net effects including Boarnet and Bogart (1996), Bondonio and Engberg (2000), Greenbaum and Engberg (2004), Bondonio and Greenbaum (2007), Elvery (2009), and Neumark and Kolko (2010). See Buss (2001) for a comprehensive review of this literature. For recent evaluations of similar international programs see Hilber et al. (2011), Zhang (2011) and Accetturo and de Blasio (2012).

[5] Ham et al. (2011) also examine the federal EC program, and state Enterprise Zone programs using a similar methodology. They find substantial positive and statistically significant effects in almost all cases.

[6] The incentives to invest in capital are an increased expensing allowance that applies to a broader set of purchases than typical expensing and allowing establishments to postpone the reporting of gains from capital sold in the EZ.

[7] Social service block grants subsidize a variety of services including: day care for children, employment services, counseling, legal services, transportation, education, and substance abuse recovery.

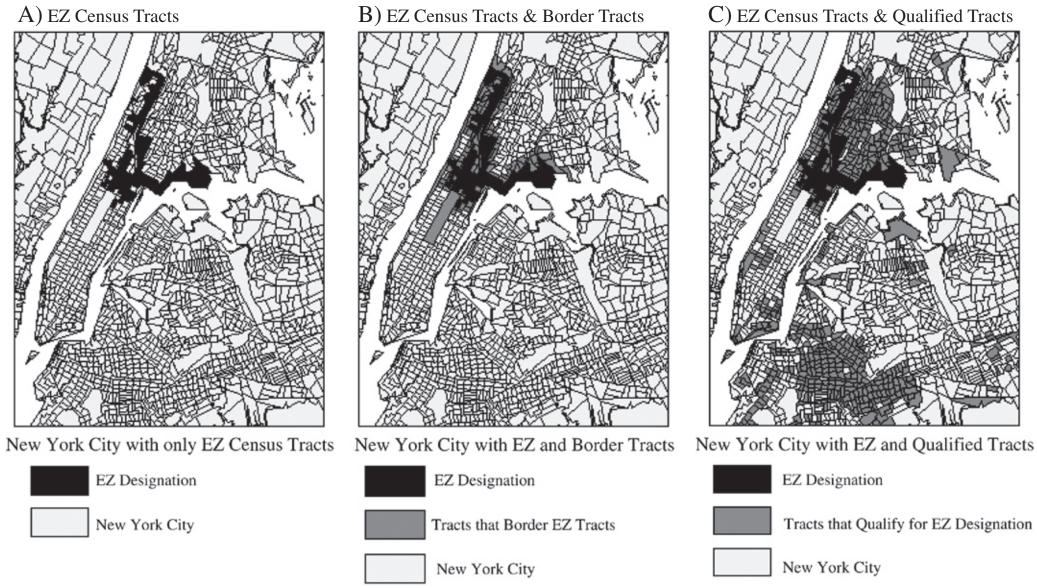

Fig. 1. New York City Empowerment Zone, bordering, and qualified census tracts.

3. Identifying spillover effects

The primary concern with identifying spillovers from economic redevelopment incentives is to construct a counter-factual for what would have happened to neighboring or economically close areas in the absence of the policy. We believe this makes the selection into treatment problem that is typical in this literature more tractable, as we do not have to deal with our treatment (the geographically and economically close areas) being selected because they are more/less likely to be successful in the absence of incentives.[8] We measure potential spillovers by the number of establishments and employment at establishments in areas close (geographically and economically) to EZ designated areas. We create control groups for the spillover areas using areas that were geographically and economically close to EC areas, (ECs applied for, but did not receive the EZ designation). There are four types of areas relevant to our identification strategy, summarized below:

- EZ areas, census tracts where the incentives are actually available.
- Census tracts near (either geographically or economically) EZ areas, where we test for spillovers.
- EC areas, census tracts that applied for EZ status, but were denied and instead given a far less generous form of assistance.
- Census tracts near (either geographically or economically) EC areas, we use these as the control group when testing for spillovers.

Areas close to EC designations make a good control group for areas close to EZ designations. They are more similar than other census tracts based on observable 1990 census characteristics. In addition, areas adjacent to ECs are not likely subject to spillovers from the program because they are in different cities. Finally, areas adjacent to ECs probably share some of the same unobservable characteristics as areas adjacent to EZs because they were not included in the original application.

In order to develop the counter-factual, first, we compare areas that are geographically close to EZ areas with areas that are geographically close to rejected applicants. The rejected applicants received a less-generous incentive package called Enterprise Communities. We fully expect that if the EC caused any type of spillover it would be substantially smaller than that caused by the EZ.[9] We map all EZ and EC areas using Geographic Information Systems software to identify census tracts that share a border with these areas. To give an example of EZs and spillover areas, Panel A of Fig. 1 shows the New York City EZ, and Panel B shows the surrounding area where we test for spillover effects.

Columns (1)–(3) of Table 1 show a comparison of average 1990 census characteristics and the change in characteristics from 1980 to 1990 for all census tracts, and areas geographically close to EZs and ECs (the comparison area). Table 1 shows that areas surrounding EZ designated areas were worse off in 1990 than all other tracts along several economic measures including poverty rates, unemployment, residents with a college degree, median income, home ownership, and the percent receiving some form of public assistance. They also tended to have a larger percentage of non-whites and female headed households, and an older housing stock. Table 1 also shows that areas near EZs had higher growth in poverty and unemployment, while having slower growth in median income, and the percentage of residents with a college degree than all other census tracts.

Given the large differences between areas surrounding EZs and other census tracts, finding a comparison group that is similar along observables will help reduce bias when estimating spillovers.[10] Areas surrounding the rejected applicants (EC areas) were more similar to areas surrounding EZs before the program began than other census tracts, although they were still better off along the dimensions we measure. In addition, areas surrounding ECs changed in a similar manner to EZs between 1980 and 1990, especially relative to all census tracts.

Our strategy to identify spillovers is to compare census tracts close to EZs with those close to ECs, and measure outcomes before the program began and after the program took effect. This amounts to a

[8] It could be that the initial application did not include geographically or economically close areas because they were more/less likely to be successful in the absence of incentives. If this is the case, there is still a selection effect; however, using areas that were qualified but left out of the initial application as the control group eliminates it.

[9] If, however, ECs did result in a negative spillover our method would understate the spillover caused near EZ areas; if the ECs resulted in a positive spillover, our method would overstate the spillover caused near EZ areas.

[10] The bias from comparing with all other areas could work both ways. If areas surrounding EZs are more likely to grow faster because they have a worse starting point, results would be bias toward finding positive spillovers (or less negative). If areas surrounding EZs are more likely to continue to decline, results would be biased toward finding negative spillovers (or less positive). Estimates using all other census tracts as a comparison group show a large positive spillover from the EZ onto neighboring areas.

difference-in-difference estimation, with the following econometric specification:

$$\Delta Y = \alpha + \beta_1 \left(EZ_{geo}\right) + X^{'}\delta + \varepsilon, \text{ if } EZ_{geo} \text{ or } EC_{geo} = 1 \quad (1)$$

where ΔY represents the change in the number of establishments or the number of employees at establishments between 1994 (one year before the program started) and 1996 (one year after the program started), we also estimate a longer term impact by taking the difference between 1994 and 2000.[11] EZ_{geo} is a dummy variable that equals one when the census tract borders an EZ area and zero otherwise. X includes city-fixed effects as well as a set of pre-treatment characteristics of census tracts including: the poverty rate, unemployment rate, percent non-white, percent with a college degree, median income, home ownership rate, median home value, percent of female headed households, percent receiving public assistance, average age of housing stock and average age of housing stock squared, and the 1980–1990 change in all of these variables. Because we are concerned about correlation between the control variables and proximity to an EZ causing bias in our estimate of β_1 we estimate Eq. (1) with and without the X variables. We estimate Eq. (1) using only census tracts that either border an EZ or border an EC—excluding all other census tracts (also excluding actual EZ and EC areas).

Eq. (1) tests for spillovers from the EZ program in areas that are geographically close to designated areas, but we would also like to test for spillovers in areas that are economically close to the EZ areas. To do this, we need two groups of census tracts, one that is economically close to EZ areas and is likely subject to spillover effects and another group that is a control/comparison group not subject to spillover effects. As with the geographically close spillover prone areas, we choose economically close areas from within EZ cities and the control/comparison areas from EC cities. We define economically close areas in two different ways: those that qualified according to the criteria of the EZ program, and areas that were most similar to those included in the zone according to pre-treatment characteristics (using a propensity score model).

The first definition of areas that are economically close to treatment areas uses the criteria for eligibility under the program. According to the rules of the program, EZ applicants must have at least 20% of residents living in poverty and at least a 6.3% unemployment rate. We test the spillover effects on areas that were qualified and part of the same city as an EZ, but not included in EZ areas by comparing them with areas that were qualified and part of the same city as an EC, but not included in EC areas. Panel C of Fig. 1 shows an example of the New York City EZ and the qualified but excluded census tracts we use to test for spillovers.

These areas may be particularly prone to spillovers from the program because although they qualified for EZ status, localities chose not to include them in the application. They may draw from the same workforce, or service the same customer base as EZ areas, or they may be in direct competition for establishments. Columns (4) and (5) of Table 1 show how similar areas that qualified for EZ status but were not included in the initial application are across our control and treatment cities. Notice that the qualified areas in our control cities (areas that qualified for EZ, but reside in an EC city) are more similar to the spillover prone areas than all other tracts along all observable dimensions.

Our estimating equation to test for spillover effects using the group of tracts that qualified for EZ status, but did not receive incentives is:

$$\Delta Y = \alpha + \beta_1 \left(EZ_{qual}\right) + X^{'}\delta + \varepsilon, \text{ if } EZ_{qual} \text{ or } EC_{qual} = 1 \quad (2)$$

where EZ_{qual} equals one if the census tract is in a city with an EZ, and is qualified, and zero otherwise. We use only tracts that met the eligibility criteria under the program and are located in cities with an EZ or EC.

The second definition of economically close areas uses a propensity score model to identify areas that are similar to the actual EZ along several dimensions prior to EZ designation; we use these areas to test for possible spillovers from the program. To construct a control/comparison group we use areas that were similar to EC areas prior to designation along the same dimensions.

We implement this strategy in two steps; the first is to estimate a propensity score model using pre-treatment data to find characteristics associated with applying for an EZ, and the second is to find areas that were most similar to actual applicants. We do this individually for each city in our sample, so the spillover and comparison areas are city specific. We estimate the following linear probability model separately for each city in our sample to create propensity scores[12]:

$$EZ = \alpha + X^{'}\delta + \varepsilon \quad (3)$$

where EZ is a zero/one variable indicating EZ status in cities that received an EZ and EC status in cities that did not. X includes the unemployment rate, percent of non-white residents, percent of residents with a college degree, median income of the census tract, the homeownership rate, median house value, percent of female headed households, percent of residents receiving public assistance, median age of the housing stock, and median age of the housing stock squared. All variables are measured prior to EZ designation in 1990 and the unit of observation is the census tract.

Table 2 shows sample output for estimating Eq. (2) for a select group of cities. As Table 2 shows, the variables that best predict EZ/EC status differ by city. In both Chicago and New York the percentage of residents living in poverty, and the percent that are non-white are significant predictors of EZ status, while in Atlanta none of the variables are significant. In EC cities, poverty is also a good predictor of being included in the application, while race is not. Other predictors matter more in EC cities, such as the homeownership rate and the percent of residents receiving public assistance. These results highlight the lack of continuity in choosing which census tracts became part of the application for the program, a fact that we believe suggests that there may be important unobservable influences driving the selection process.

We use the city specific beta coefficients from Eq. (2) to create a predicted value, or propensity score, and define our treatment and control areas based on those values. This approach follows the use of the propensity score by Crump et al. (2009) as a way to trim samples and estimate average treatment effects. We are searching for the sample that is most similar to the actual EZ/EC areas to check for spillover effects, so we define it by the propensity scores that are closest to the actual EZ/EC. We trim our sample using census tracts with a propensity score in the top decile of each city.

Columns (6) and (7) of Table 1 show summary statistics for the areas that are economically close to EZ and EC areas using those in the top decile as the propensity score cut-off. As with using qualified but not chosen areas, the propensity score method helps to choose areas that were economically similar to the EZ areas before the program to test for spillovers. Columns (6) and (7) show that areas economically close to ECs are more similar to areas economically close to EZs than the All Tracts group.

We also test to be sure that the propensity score areas are more similar to actual EZ and EC areas, than other areas in the city. Again, the point is to find areas that may be subject to spillovers from the EZ policy, and the propensity score does this by identifying areas that

[11] We stop at 2000 because in 2001 more cities began receiving EZ status, effectively making some of our comparison areas treated areas.

[12] We also tried estimating a propensity score model using probit regressions, these results have even fewer statistically significant variables than the linear probability results presented here.

Table 1
Spill over and comparison area summary statistics (standard deviation in parenthesis).

	All tracts	Geographically close to EZ	Geographically close to EC	Qualified for assistance (EZ cities)	Qualified for assistance (EC cities)	Propensity score 90th percentile (EZ cities)	Propensity score 90th percentile (EC cities)
Poverty rate 1990	0.1305	0.3174	0.2199	0.3688	0.3288	0.4509	0.3131
	(0.1188)	(0.186)	(0.1462)	(0.132)	(0.1196)	(0.2459)	(0.2591)
Δ in poverty rate 1980–1990	0.0405	0.0278	0.0282	0.0387	0.0789	0.0742	0.0611
	(0.0964)	(0.1191)	(0.0956)	(0.1223)	(0.0948)	(0.18)	(0.1776)
Unemployment rate 1990	0.0665	0.1603	0.0986	0.1848	0.1379	0.1887	0.1302
	(0.0493)	(0.1067)	(0.0684)	(0.0927)	(0.0678)	(0.1527)	(0.1336)
Δ in unemployment rate 1980–1990	0.014	0.0197	0.0132	0.0364	0.0399	0.0482	0.0436
	(0.0486)	(0.0730)	(0.0598)	(0.0846)	(0.0636)	(0.1292)	(0.1043)
Percent non-white 1990	0.1988	0.6784	0.4296	0.772	0.5684	0.8437	0.7471
	(0.2569)	(0.3397)	(0.3356)	(0.246)	(0.302)	(0.2002)	(0.2893)
Δ in percent non-white 1980–1990	−0.1225	0.0629	0.0599	0.0926	0.0812	0.0748	0.0523
	(0.3759)	(0.1284)	(0.1242)	(0.1372)	(0.1317)	(0.2321)	(0.1788)
Percent with college degree 1990	0.2152	0.1379	0.179	0.092	0.1097	0.0563	0.1089
	(0.1622)	(0.1755)	(0.1573)	(0.0899)	(0.1061)	(0.0943)	(0.1384)
Δ in percent with college degree 1980–1990	0.0568	0.0377	0.0463	0.0284	0.0142	0.0047	0.0195
	(0.0688)	(0.0808)	(0.0725)	(0.0544)	(0.0569)	(0.0741)	(0.0755)
Median income (thousands) 1990	49.1	32.1	38.4	26.4	27.2	14.2	16.6
	(22.3)	(23.9)	(19.1)	(11.7)	(10.1)	(16.1)	(15.6)
Δ in median income (thousands) 1980–1990	12.1	3.2	4.3	1.6	−1.2	−1.1	−2.1
	(15.7)	(13.9)	(13.1)	(10.3)	(9.32)	(18.6)	(12.5)
Home ownership rate 1990	0.6525	0.3385	0.4717	0.2821	0.3787	0.1105	0.3041
	(0.2226)	(0.2346)	(0.2344)	(0.2138)	(0.2188)	(0.1466)	(0.2131)
Δ in home ownership rate 1980–1990	−0.0178	0.0086	−0.0123	0.0001	−0.0186	−0.0049	−0.0162
	(0.0937)	(0.0966)	(0.079)	(0.0857)	(0.0683)	(0.1057)	(0.0668)
Median house value (thousands) 1990	117.8	74.8	103.4	92.2	79.9	37.2	52.8
	(111)	(97.9)	(88.2)	(99.4)	(73)	(60.3)	(67)
Δ in median house value (thousands) 1980–1990	130.9	78.7	104.7	94.5	80.3	48.9	74.2
	(117.1)	(98.9)	(87.4)	(99.3)	(72.7)	(63)	(68.9)
Percent female household heads 1990	0.217	0.5053	0.3668	0.5644	0.4446	0.5217	0.3643
	(0.1513)	(0.256)	(0.205)	(0.1888)	(0.1901)	(0.3285)	(0.3165)
Δ in percent female household heads 1980–1990	0.0669	0.0713	0.0556	0.0798	0.081	0.0514	0.0447
	(0.1109)	(0.1599)	(0.137)	(0.1531)	(0.1351)	(0.1839)	(0.1478)
Percent receiving public assistance 1990	0.0761	0.2398	0.1318	0.2788	0.1861	0.3328	0.183
	(0.0769)	(0.1648)	(0.1066)	(0.1294)	(0.1079)	(0.2157)	(0.1823)
Δ in percent receiving public assistance 1980–1990	0.0162	0.0038	0.0039	0.0021	0.0255	0.0085	0.0255
	(0.0570)	(0.0964)	(0.0653)	(0.0977)	(0.0721)	(0.1360)	(0.0911)
Average age of housing stock 1990	27.7423	41.2193	36.1745	40.6058	35.2956	36.446	35.4346
	(11.0603)	(8.488)	(9.5766)	(8.259)	(9.7267)	(9.1787)	(9.2801)
Δ in average age of housing stock 1980–1990	5.0741	4.82	5.874	4.8968	5.6032	2.7337	5.7561
	(4.3157)	(5.3725)	(4.6468)	(5.4889)	(4.6782)	(7.3061)	(6.0654)

Notes: Data from 1980 and 1990 census tract areas. Geographically close areas are adjacent to EZ and EC boundaries according to HUD definitions of these areas.

were similar along a several dimensions prior to the arrival of the policy. Table 3 shows the results of propensity score balancing tests that suggest that the propensity score is picking areas that are most similar to actual EZ/EC areas. The third column of Table 3 shows the difference between actual EZ/ECs and all other census tracts in EZ/EC cities. This column shows that the differences are large—24 percentage points difference in poverty rates, 22 percentage points non-white residents, for example. The final column of Table 3 tests the difference between actual EZ/ECs and the areas where we test for spillovers (those scoring in the top decile of the p-score distribution). Although the tests suggest that there are still some differences between these areas, the magnitude of the differences is substantially less—always in favor of the areas looking closer to EZ/ECs. We take the balancing test results as evidence that the propensity score is choosing areas that are similar to actual EZ/ECs, and that these areas are a reasonable place to look for spillovers from the policy. Unlike many applications of the propensity score, we are not using it as a method to identify a comparison area for policy evaluation; we are using it to identify areas that may be subject to spillovers from a policy.

Our estimating equation to test for spillover effects using the group of tracts that were similar to EZ areas, but did not receive incentives is:

$$\Delta Y = \alpha + \beta_1\left(EZ_{pscore}\right) + X'\delta + \varepsilon, \text{ if } EZ_{pscore} \text{ or } EC_{pscore} > \text{Top Decile} \quad (4)$$

where EZ_{pscore} equals one if the tract has a propensity score in the top decile of the city distribution. We estimate Eq. (4) using only tracts with a propensity score in the top decile and located in cities that received either an EZ or an EC.

4. Data

The unit of analysis in our data is the census tract, and our source for the number of establishments is the Dun and Bradstreet (D&B) Marketplace database.[13] The data consist of 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 of the establishment at the zip code level, and the two-digit Standard Industrial Classification (SIC) code.

The D&B data is aggregated at the zip code level. To map the zip code level data on local establishments to census tracts, we use a correspondence to match the geography of the EZ and EC designated areas and our spillover prone areas. The correspondence determines what percent of each zip code lies in a given census tract and assigns

[13] Although the D&B does not contain all business activity in the U.S., the omissions from the data are considered sufficiently random so that the data is representative of the spatial distribution of the business activity (Holmes, 1998; Rosenthal and Strange, 2003).

Table 2
Linear probability model results for select cities.

	Empowerment zone cities			Enterprise community cities		
	New York	Chicago	Atlanta	LA	Houston	DC
Poverty rate	0.213***	0.189*	0.611	0.703***	0.419**	1.417***
	(0.0516)	(0.112)	(0.437)	(0.120)	(0.170)	(0.372)
Unemployment rate	−0.0399	0.234*	0.631	1.207***	−0.0250	−1.244**
	(0.0760)	(0.139)	(0.715)	(0.283)	(0.324)	(0.545)
Percent non-white	0.0376**	−0.0911**	0.0925	0.00917	0.0113	−0.0586
	(0.0162)	(0.0434)	(0.201)	(0.0447)	(0.0540)	(0.212)
Percent with college degree	−0.0146	−0.0492	−0.324	0.293***	−0.0362	−0.185
	(0.0338)	(0.0860)	(0.302)	(0.0899)	(0.0827)	(0.257)
Median income	0.000480*	−0.000322	0.00149	−0.000578	0.00161**	−0.000705
	(0.000250)	(0.000779)	(0.00173)	(0.000545)	(0.000798)	(0.00132)
Home ownership rate	−0.0234	−0.134**	−0.248	0.369***	−0.169***	0.190
	(0.0198)	(0.0592)	(0.164)	(0.0486)	(0.0514)	(0.134)
Median house value	−8.94e−08***	−6.98e−08	3.94e−07	1.54e−07**	−4.64e−08	1.74e−07
	(2.76e−08)	(1.24e−07)	(4.42e−07)	(6.94e−08)	(1.47e−07)	(1.92e−07)
Percent female household heads	0.00146	0.0436	−0.325	0.0782	−0.0959	0.0835
	(0.0325)	(0.0681)	(0.303)	(0.0926)	(0.0960)	(0.162)
Percent receiving public assistance	0.0255	0.368***	0.415	1.574***	0.835***	0.226
	(0.0584)	(0.122)	(0.623)	(0.164)	(0.289)	(0.473)
Average age of housing stock	0.00143	−0.00506	−0.0167	−0.00939	−0.0328***	0.00464
	(0.00263)	(0.00712)	(0.0276)	(0.00608)	(0.00439)	(0.0138)
Average age of housing stock squared	−1.45e−05	1.69e−05	0.000380	0.000126	0.000756***	−6.33e−05
	(3.52e−05)	(9.54e−05)	(0.000414)	(9.76e−05)	(8.45e−05)	(0.000192)
Constant	−0.0542	0.255*	0.126	−0.417***	0.244***	−0.149
	(0.0521)	(0.138)	(0.489)	(0.102)	(0.0621)	(0.343)
N	2166	865	132	923	525	183
R^2	0.079	0.273	0.3	0.212	0.174	0.281

Notes: Linear Results show characteristics associated with choice of inclusion in EZ application at the census tract level. Regressions use only tracts in the city where the application is made.
*** $p<0.01$.
* $p<0.1$.
** $p<0.05$.

that percent of zip code employment or establishments to the census tract. The list of EZ and EC census tracts was obtained by personal correspondence with the Department of Housing and Urban Development, and is also partially available through their webpage. After mapping EZ and EC areas, we locate areas that border them using ArcGIS software.

One advantage of using the D&B data over census data is that it is generated closer to the time when EZs are implemented (1995) than census data that is only available every ten years. Another advantage is that establishments and employment in the D&B data are based on location of where work takes place, an explicit requirement for eligibility in the program.[14] The program requires that establishments must locate in the EZ and employees must work (and live) in the EZ to claim tax credits. Although neither the census nor the D&B matches both where the employee live and work, the D&B offer a count of employees that actually work in the EZ. Census data only match where the employee lives, and given the small size of EZ areas it is likely that many residents are employed outside of the designated area.

We supplement the D&B data with census data from the 1990 census and changes from the 1980 to 1990 census to control for economic and demographic factors of the census tracts before designation in some specifications. We also use the 1990 census data to estimate our propensity score model to find areas that are economically close to actual EZ and EC areas based on several dimensions.

[14] The federal government also offers a less generous Work Opportunity or Welfare to Work tax credit to establishments outside of the EZ area who employ youth (aged 18 to 24) living in EZ areas. These tax credits are only available for the first two years of employment, while the standard EZ employment credit is available regardless of employee tenure.

5. Results

5.1. Number of establishments

Table 4 shows the results of estimating Eqs. (1), (2), and (4) using the number of business establishments as the dependent variable. Estimates include both short (one year) and long (five years) term intervals after the start of the program and we estimate with and without control variables. The first four columns show results for spillovers from the EZ program on geographically close areas, the next four show spillover results for areas that qualified for the program, but were not part of the application, and the final four columns show results for areas that were similar along multiple observed dimensions as measured by a propensity score.[15]

Results estimated without controlling for other pre-treatment characteristics show that census tracts sharing a border with EZ designated areas experienced a decline of about 24 establishments in the short term and 30 establishments in the longer term, both statistically significant at the one percent level. Estimates controlling for 1990 census characteristics and the trend in these characteristics between 1980 and 1990, show a slightly more modest decline in the number of establishments for census tracts sharing a border with EZ designated areas, establishments in the short term and 18 establishments over the longer term (statistically significant at conventional levels).

Spillovers from the EZ program may also occur in areas not as geographically close, but economically (and demographically) similar to EZ areas. These may be areas that draw from the same workforce, or service the same customer base as EZ areas, or they may be in direct competition for establishments. The middle four columns of Table 4

[15] Coefficients for the change in 1980 to 1990 census tract attributes are available upon request from the authors.

Table 3
Propensity score balancing test between actual EZ/EC areas and EZ/EC$_{pscore}$ areas.

	Actual EZ/EC	Tracts in EZ/EC cities		Tracts in top decile of EZ/EC$_{pscore}$	
Poverty rate	0.4111	0.1671	0.2439***	0.3844	0.0266***
Unemployment rate	0.1772	0.0851	0.0921***	0.1604	0.0167***
Percent non-white	0.7087	0.3762	0.3324***	0.7970	−0.0883***
Percent with college degree	0.0855	0.2319	−0.1464***	0.0801	0.0054
Median income (thousands)	22.03	47.61	−25.57***	15.36	6.67***
Home ownership rate	0.3053	0.4982	−0.1929***	0.1975	0.1077***
Median house value	68,621	141,557	−72,936***	44,763	23,858***
Percent female household heads	0.5573	0.3020	0.2552***	0.4457	0.1115***
Percent receiving public assistance	0.2732	0.1055	0.1677***	0.2605	0.0127
Average age of housing stock	37.87	33.81	4.06***	35.98	1.88***

EZ$_{pscore}$ indicates tracts that were in the top decile of the propensity score distribution for each EZ or EC city individually. Column (3) shows the results of significance tests between characteristics of actual EZ and EC areas and all other census tracts in cities with an EZ or EC. Column (5) shows results of significance tests between characteristics of actual EZ/EC areas and the EZ/EC$_{pscore}$ areas. Although there are still significant differences between EZ/EC and E/EC$_{pscore}$ areas, these are substantially smaller than the difference between EZ/ECs and all other tracts in EZ/EC cities. *** $p<0.01$.

show the results of estimating Eq. (2) to test for spillovers in areas that qualified for EZ status, but were not included in the local application. In the short term, areas that were qualified but not included in the EZ designated boundaries experience a decline of between 25 and 28 establishments (depending on controls for pre-treatment characteristics and trends), statistically significant at the one percent level. The long term estimates suggest that qualified areas have a loss of between 20 and 28 establishments, depending on what controls are used, but statistical significance remains.

As another way to check for the presence of spillovers from the EZ program on economically and demographically similar areas, we estimate spillover effects on areas that were most similar with areas receiving an EZ along several observable characteristics. We identify similar areas using the propensity score model in Eq. (2), and consider all areas with a propensity score in the top decile of the distribution (specific to each city) as areas for potential spillovers. We estimate the spillover effects on census tracts that have a propensity score in the top decile, following Eq. (4), the last four columns of Table 4 show these results. Areas in the top decile of the propensity score distribution show a loss of between 19 and 33 establishments in the short term (depending on whether control variables are included), statistically significant at conventional levels. Longer term results show the negative spillover

Table 4
Spillover effects of targeted tax incentives on the number of establishments (standard errors clustered at city level in parenthesis).

Spillover area	EZ$_{geo}$				EZ$_{qual}$				EZ$_{pscore}$			
	Short term		Long term		Short term		Long term		Short term		Long term	
EZ$_{close}$	−24.23***	−15.21**	−30.28***	−18.17***	−28.29***	−25.05***	−28.48***	−20.70***	−19.38**	−27.96*	−33.15***	−36.48
	(6.212)	(6.808)	(5.497)	(5.761)	(3.847)	(6.425)	(3.872)	(6.003)	(8.502)	(15.99)	(9.717)	(22.58)
Poverty rate		63.10		58.16		10.38		30.71		246.6**		211.5**
		(50.58)		(40.42)		(54.53)		(48.99)		(99.54)		(106.7)
Unemployment rate		48.94		46.21		185.5***		142.6***		−85.57		−51.54
		(80.29)		(74.07)		(62.79)		(54.48)		(102.0)		(86.60)
Percent non-white		−29.29**		−27.47***		8.501		6.136		46.73		47.50
		(12.05)		(10.60)		(12.87)		(12.79)		(37.39)		(57.00)
Percent with college degree		37.97		22.09		117.9**		93.26*		164.0*		185.8
		(41.59)		(44.13)		(57.06)		(55.71)		(98.31)		(113.5)
Median income		0.163		0.474		−0.307		−0.166		0.735		1.004
		(0.528)		(0.458)		(0.901)		(0.875)		(1.163)		(1.246)
Home ownership rate		−47.21**		−46.06*		−36.03*		−21.02		8.360		52.54
		(23.51)		(25.71)		(18.56)		(21.05)		(45.68)		(74.76)
Median house value		−0.200		−0.133		−0.308**		−0.221		−0.321		−0.565
		(0.170)		(0.182)		(0.147)		(0.168)		(0.301)		(0.496)
Percent female household heads		15.55		10.14		−58.37		−52.38		22.13		−13.37
		(36.48)		(29.24)		(40.88)		(36.15)		(60.92)		(76.92)
Percent receiving public assistance		−135.3**		−126.9**		−138.3***		−124.6***		−190.2**		−116.5
		(64.49)		(57.13)		(44.46)		(38.38)		(92.32)		(91.25)
Average age of housing stock		−5.162*		−5.757*		−5.051***		−6.779***		−11.43		−17.15
		(2.739)		(3.004)		(1.939)		(2.486)		(7.658)		(14.46)
Average age of housing stock squared		0.0309		0.0367		0.0456*		0.0649**		0.138		0.201
		(0.0395)		(0.0402)		(0.0250)		(0.0257)		(0.0953)		(0.171)
Constant	72.10***	231.3***	66.25***	232.7***	65.83***	232.2***	55.24***	243.2***	72.97***	185.5	72.94***	298.5
	(3.083)	(48.90)	(2.810)	(53.56)	(3.473)	(58.76)	(3.645)	(69.67)	(6.955)	(125.2)	(8.920)	(227.0)
City fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Includes 1980 to 1990 census trends		Yes		Yes		Yes		Yes		Yes		Yes
Observations	2312	2225	2312	2225	3171	3089	3171	3089	694	470	694	470
R-squared	0.101	0.105	0.058	0.128	0.076	0.084	0.053	0.071	0.135	0.137	0.082	0.099

Notes: Regressions with EZ$_{geo}$ as treatment include only census tracts that border EZ (treatment) or EC (control) areas. Regressions with EZ$_{qual}$ as treatment include only census tracts that qualified for Empowerment Zone status under the rules of the program. To qualify, applicants must have at least 20% of residents living in poverty and at least a 6.3% unemployment. Regressions with EZ$_{pscore}$ as treatment include only tracts in the top decile of the propensity score distribution (estimated at the city level) that did not actually receive an EZ or EC.

*** $p<0.01$.

** $p<0.05$.

* $p<0.1$.

Table 5
Spill-over effects of targeted tax incentives on the number of employees (standard errors clustered at city level in parenthesis).

Spillover area	EZ_{geo}				EZ_{qual}				EZ_{pscore}			
	Short term		Long term		Short term		Long term		Short term		Long term	
EZ_{close}	−219.5*	−52.39	−476.8**	−185.4	−335.4***	−196.7	−599.9***	−483.1***	−328.2	−612.7	−889.9**	−1223
	(123.2)	(175.6)	(207.0)	(336.8)	(95.80)	(132.4)	(144.1)	(174.3)	(235.3)	(533.3)	(361.3)	(825.8)
Poverty rate		203.1		1273		2390**		3380*		3391		4114
		(925.3)		(1482)		(1177)		(1790)		(2566)		(3788)
Unemployment rate		1293		3711		712.7		3873*		−1929		−483.2
		(1950)		(3250)		(1653)		(2090)		(1721)		(3038)
Percent non-white		−53.28		−538.1*		77.88		−244.2		1275		1595
		(216.0)		(278.1)		(299.2)		(383.4)		(1412)		(2149)
Percent with college degree		−22.79		−1773		−3316		−3358		1961		4321
		(1459)		(2394)		(2093)		(2740)		(2591)		(3928)
Median income		−1.073		18.04		50.10*		70.36*		4.771		17.17
		(12.42)		(19.63)		(29.62)		(42.54)		(30.97)		(45.74)
Home ownership rate		−1024		−2386		−610.3		−1281		2013		2554
		(914.3)		(1481)		(621.9)		(1022)		(1830)		(2899)
Median house value		2.673		3.725		2.090		1.203		−12.14		−19.91
		(5.702)		(9.785)		(4.885)		(8.584)		(11.98)		(18.32)
Percent female household heads		−402.2		−84.79		−83.81		670.5		634.5		1229
		(676.4)		(940.1)		(804.4)		(1053)		(1914)		(2875)
Percent receiving public assistance		−1506		−4071*		−1068		−3516**		−1780		−3053
		(1419)		(2361)		(1048)		(1549)		(2105)		(3789)
Average age of housing stock		−89.16**		−142.2**		−91.73		−173.3*		−418.6		−611.9
		(43.16)		(70.71)		(58.92)		(89.09)		(351.8)		(545.1)
Average age of housing stock squared		0.753		1.307		0.713		1.552*		5.051		7.232
		(0.641)		(1.033)		(0.684)		(0.820)		(4.141)		(6.425)
Constant	767.3***	3459***	1210***	5311***	587.7***	1369	936.2***	3025	792.2***	6800	1435***	10,330
	(78.58)	(944.8)	(113.4)	(1437)	(87.80)	(1745)	(140.1)	(2717)	(227.5)	(5538)	(353.3)	(8566)
City fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Includes 1980 to 1990 census trends		Yes		Yes		Yes		Yes		Yes		Yes
Observations	2312	2225	2312	2225	3171	3089	3171	3089	694	470	694	470
R-squared	0.001	0.023	0.002	0.040	0.003	0.025	0.004	0.030	0.003	0.089	0.009	0.079

Notes: Regressions with EZ_{geo} as treatment include only census tracts that border EZ (treatment) or EC (control) areas. Regressions with EZ_{qual} as treatment include only census tracts that qualified for Empowerment Zone status under the rules of the program. To qualify, applicants must have at least 20% of residents living in poverty and at least a 6.3% unemployment. Regressions with EZ_{pscore} as treatment include only tracts in the top decile of the propensity score distribution (estimated at the city level) that did not actually receive an EZ or EC.

* $p<0.1$.
** $p<0.05$.
*** $p<0.01$.

for census tracts in the top decile of the propensity score distribution grows to between 28 and 36 establishments, but statistical significance at conventional levels is lost.[16]

We also examine the possibility that spillovers are different in areas that are *both* geographically and economically close to EZs. This amounts to running regressions as in Eqs. (1), (2), and (4), but using the interaction between EZ_{Geo} and either EZ_{Qual} or EZ_{pscore} as the variable of interest. These results, available on request, show that spillovers in areas that are both geographically and economically close are larger in magnitude (statistical significance is the same) than the spillovers on tracts that are either geographically or economically close. In the short term, these areas lose additional 12–15 establishments, and in the longer term, this difference shrinks to an additional loss of between 5 and 7 establishments.

5.2. Number of employees

Table 5 displays the results of estimating Eqs. (1), (2), and (4) using employment at business establishments located in spillover prone and comparison areas as the dependent variable. Again, we estimate a short (one year) and long (five year) term effect and produce estimates with and without control variables. As a group, these results are quite imprecise compared to the business establishment results, although they all suggest a negative spillover effect from the EZ program on employment in geographically and economically close areas.

The magnitude of the spillover on geographically close areas in the short term ranges from a loss of between 52 and 219 employees in areas neighboring EZs. Only the results estimated without control variables approaches statistical significance, where we can reject the null hypothesis of no spillovers at the ten percent level. In the longer term, the magnitude of the spillover on geographically close areas grows to a loss of between 185 (control variables) and 476 employees (no controls) at establishments in neighboring areas. Only the estimate without control variables is statistically different than zero.

The magnitude of the spillover effect on employment at establishments in areas that are economically similar and located in the same city as EZ areas is larger than the estimated effect for geographically close areas. Areas that were qualified for the EZ program, but not included in an application experience a decline of between 196 (controls) and 335 (no controls) employees in the short term, only statistically significant for the no controls specification. In the longer term, the size of the spillover on qualified areas grows to a loss of between 483 and 600 employees, and the estimates are quite precise—statistically significant in both specifications at the one percent level. Finally, testing for spillovers in areas that match characteristics of EZ designated areas through a propensity score shows large losses (between 328 and 1223 employees), but these results are imprecise, with only one

[16] As a robustness check, we test for spillover effects using the top quartile of census tracts in the propensity score distribution as the treatment. These results show a loss of between 17 and 19 establishments in the short term and between 16 and 23 establishments in the longer term, statistically significant in all specifications with or without using control variables.

Table 6
Spillover effects of targeted tax incentives on the number of establishments in the service and retail industries (standard errors clustered at city level in parenthesis).

Spillover area	EZ_{geo}				EZ_{qual}				EZ_{pscore}			
	Short-term		Long-term		Short-term		Long-term		Short-term		Long-term	
EZ_{close}	−16.00***	−11.58**	−19.88***	−13.26***	−20.37***	−18.91***	−19.63***	−15.16***	−13.94**	−18.35	−21.29***	−23.86*
	(4.853)	(4.986)	(4.182)	(4.158)	(2.588)	(4.777)	(2.666)	(4.420)	(5.937)	(11.14)	(6.336)	(14.23)
Poverty rate		43.36		36.76		−4.580		6.714		172.2**		144.3**
		(36.69)		(28.58)		(38.13)		(33.10)		(70.18)		(69.98)
Unemployment rate		39.25		36.63		123.0***		99.79***		−48.33		−36.10
		(53.29)		(45.47)		(42.71)		(37.64)		(73.37)		(58.46)
Percent non-white		−21.68**		−20.20***		6.436		5.262		27.69		28.30
		(8.649)		(7.358)		(8.889)		(8.530)		(23.56)		(35.01)
Percent with college degree		57.55**		50.34**		119.3***		106.7***		122.9*		140.5*
		(24.30)		(23.05)		(36.41)		(31.69)		(70.30)		(74.31)
Median income		−0.00991		0.203		−0.447		−0.378		0.515		0.787
		(0.359)		(0.301)		(0.568)		(0.523)		(0.861)		(0.824)
Home ownership rate		−29.18**		−26.13**		−20.36*		−8.338		−5.165		28.27
		(12.81)		(12.16)		(10.73)		(11.73)		(28.80)		(45.05)
Median house value		−0.192*		−0.152		−0.256***		−0.211**		−0.166		−0.353
		(0.1000)		(0.0943)		(0.0879)		(0.0932)		(0.195)		(0.308)
Percent female household heads		13.85		5.924		−43.16		−44.72*		24.51		−5.845
		(25.85)		(20.24)		(28.27)		(24.09)		(42.63)		(48.56)
Percent receiving public assistance		−91.60**		−79.97**		−81.38***		−71.00***		−151.2**		−78.70
		(43.60)		(34.36)		(29.90)		(22.76)		(69.46)		(55.21)
Average age of housing stock		−2.912		−3.337		−2.402*		−4.015**		−5.833		−10.01
		(1.911)		(2.075)		(1.239)		(1.687)		(4.257)		(8.669)
Average age of housing stock squared		0.0121		0.0178		0.0170		0.0356**		0.0700		0.115
		(0.0278)		(0.0281)		(0.0171)		(0.0177)		(0.0552)		(0.103)
Constant	54.93***	151.5***	49.86***	150.7***	50.14***	151.6***	42.24***	168.3***	55.46***	95.73	53.70***	174.9
	(2.149)	(33.77)	(1.849)	(36.79)	(2.367)	(37.25)	(2.490)	(47.24)	(4.859)	(72.78)	(5.678)	(136.5)
City fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Includes 1980 to 1990 census trends		Yes		Yes		Yes		Yes		Yes		Yes
Observations	2312	2225	2312	2225	3171	3089	3171	3089	694	470	694	470
R-squared	0.005	0.118	0.010	0.151	0.016	0.089	0.014	0.074	0.008	0.144	0.017	0.109

Notes: Results include only firms in the retail and service industries. Regressions with EZ_{geo} as treatment include only census tracts that border EZ (treatment) or EC (control) areas. Regressions with EZ_{qual} as treatment include only census tracts that qualified for Empowerment Zone status under the rules of the program. To qualify, applicants must have at least 20% of residents living in poverty and at least a 6.3% unemployment. Regressions with EZ_{pscore} as treatment include only tracts in the top decile of the propensity score distribution (estimated at the city level) that did not actually receive an EZ or EC.

*** $p<0.01$.

** $p<0.05$.

* $p<0.1$.

specification yielding a result that is statistically meaningful at the ten percent level.[17]

5.3. Spillovers in the retail and service industry

Previous work (Hanson and Rohlin, 2011b) finds the Empowerment Zone tax incentives are most effective at attracting new establishments in the retail and service industries. Given the success of the program at attracting new retail and service establishments, we test for spillovers in adjacent areas in these industries separately.[18] The location-specific constraints of the program allow retail and service establishments to literally move across the street to gain eligibility for program benefits while not losing any of their local customer base. To test for spillovers in the retail and service industry, we run regressions as in Eqs. (1), (2), and (4), but limit the sample to establishments in these industries.

Table 6 shows estimation results for retail and service establishments measuring spillovers on both geographically and economically close areas (measured using qualifying areas and by propensity score). The results examining the retail and service sector separately show that indeed the negative spillover is strong in these industries. The EZ program is responsible for a loss of between 11.5 (controls) and 16 (no controls) retail and service establishments in geographically close areas in the short term, and between 13 (controls) and 19 (no controls) in the longer term, or about two-thirds the size of the total establishment loss. Looking across areas that are economically similar to EZs, shows that qualified areas lost between 14 and 20 retail and service establishments in the short term (depending on the specification) and about the same amount in the longer term, again about two-thirds of the size of total establishment losses. All of these results are statistically significant at conventional levels. Spillover areas identified with the p-score method produce results that are similar in magnitude, but lose statistical significance in some specifications.

Table 7 shows estimates for spillovers on the retail and service industry using employment as the dependent variable. Again, these results suggest a negative spillover effect in both economically similar and geographically close areas from the EZ program. The magnitude of the negative spillover effect on employment in the retail and service sectors ranges from a loss of between 23 and 90 employees in the short term for geographically close areas, to a loss of between 430 and 456 employees in the longer term for areas with a close propensity score. As with the full sample employment results the retail and service sector results for employment are less precise; however we achieve statistical significance in 7 of 12 specifications.

[17] We also estimate spillover effects using logs. The primary reason for doing so is that the level regressions assume that census tracts across areas close to EZs and ECs should gain the same number of firms or employees in the absence of the program, and the log regressions assume that the growth rates should be the same. The log regressions show the same sign and statistical significance as the results in Tables 4 and 5, with magnitudes ranging from a loss of 15–77% of firms depending on the spillover area, the time elapsed, and the controls.

[18] The effect on industries outside the service and retail sector is negative and between a half to a third of the effect on the service and retail sector, depending of the specification and the time interval. These results are also statistically significant in nearly every case.

Table 7
Spillover effects of targeted tax incentives on the number of employees in the service and retail industries (standard errors clustered at city level in parenthesis).

Spillover area	EZ_{geo}				EZ_{qual}				EZ_{pscore}			
	Short-term		Long-term		Short-term		Long-term		Short-term		Long-term	
EZ_{close}	−89.56*	23.36	−264.5***	−179.2**	−138.5***	−140.8**	−274.1***	−307.6***	−42.77	−62.90	−431.5**	−456.1
	(48.10)	(64.48)	(64.08)	(90.85)	(40.81)	(55.58)	(70.70)	(106.6)	(95.59)	(96.01)	(170.8)	(302.2)
Poverty rate		−125.0		41.22		81.93		298.1		589.5		1524
		(372.8)		(565.7)		(437.5)		(615.4)		(861.8)		(1496)
Unemployment rate		−1563*		453.6		10.02		1895**		−1531*		−889.0
		(829.2)		(1104)		(504.0)		(811.5)		(888.1)		(1708)
Percent non-white		−136.9		−361.2***		99.08		23.16		−37.21		241.6
		(95.43)		(133.8)		(139.9)		(177.5)		(296.8)		(728.3)
Percent with college degree		1094*		657.7		−1055		−1167		−72.07		1870
		(562.5)		(574.7)		(1278)		(993.5)		(777.1)		(1459)
Median income		−6.599		2.139		11.57		14.82		−12.07		0.775
		(5.083)		(5.879)		(11.80)		(11.32)		(11.83)		(18.96)
Home ownership rate		58.83		−592.0**		−291.0		−632.4**		65.08		220.5
		(246.6)		(236.8)		(253.9)		(288.3)		(248.5)		(978.5)
Median house value		−3.338**		−3.570**		−2.973*		−4.107*		−1.899		−6.856
		(1.616)		(1.733)		(1.602)		(2.270)		(1.867)		(6.103)
Percent female household heads		−116.4		164.9		−498.4		−195.6		965.7*		1011
		(292.4)		(420.6)		(314.2)		(393.2)		(511.2)		(1098)
Percent receiving public assistance		240.0		−992.3		−271.2		−1623***		−1685**		−2357
		(573.8)		(844.6)		(318.6)		(513.7)		(651.4)		(1743)
Average age of housing stock		−39.81*		−68.01**		−42.16*		−112.8**		−50.99		−210.8
		(20.84)		(29.19)		(23.33)		(47.54)		(34.30)		(178.3)
Average age of housing stock squared		0.241		0.463		0.401		1.187***		0.660		2.663
		(0.302)		(0.439)		(0.311)		(0.437)		(0.506)		(2.148)
Constant	438.0***	1942***	662.1***	2938***	341.1***	1517**	517.6***	3143**	384.1***	1581**	827.2***	4152
	(28.13)	(422.1)	(39.25)	(615.0)	(39.21)	(635.4)	(69.11)	(1334)	(87.11)	(791.2)	(162.9)	(2855)
City fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Includes 1980 to 1990 census trends		Yes		Yes		Yes		Yes		Yes		Yes
Observations	2312	2225	2312	2225	3171	3089	3171	3089	694	470	694	470
R-squared	0.001	0.062	0.005	0.100	0.003	0.024	0.004	0.028	0.000	0.121	0.010	0.073

Notes: Results include only firms in the retail and service industries. Regressions with EZ_{geo} as treatment include only census tracts that border EZ (treatment) or EC (control) areas. Regressions with EZ_{qual} as treatment include only census tracts that qualified for Empowerment Zone status under the rules of the program. To qualify, applicants must have at least 20% of residents living in poverty and at least a 6.3% unemployment. Regressions with EZ_{pscore} as treatment include only tracts in the top decile of the propensity score distribution (estimated at the city level) that did not actually receive an EZ or EC.

* $p<0.1$.
*** $p<0.01$.
** $p<0.05$.

5.4. Robustness of primary results

The primary concern with the results in Tables 4–7 is that there may be some unobserved variables correlated with being near an EZ that are also correlated with the number of establishments or employment. We believe our identification strategy of using areas close to EC locations eliminates many of these concerns, as both our control and treatment areas are not part of the original application process; however, it is still possible we have not adequately accounted for all possible missing variables. To further explore the robustness of our primary results, we implement the strategy in Udry (1996) that examines how adding control variables changes the coefficient of interest. The

Table 8
Robustness of primary findings to adding additional control variables.

	EZ_{geo}		EZ_{qual}		EZ_{pscore}	
	Short-term	Long-term	Short-term	Long-term	Short-term	Long-term
Number of establishments (EZ coefficient)						
No controls	−24.23***	−30.28***	−28.29***	−28.48***	−19.38**	−33.15***
Standard controls	−15.21***	−18.17***	−25.05***	−20.70***	−27.96*	−36.48
Cross products and squares	−13.92**	−15.50***	−25.75***	−19.69***	1.53	−2.45
No controls = Cross products and squares	Cannot reject	Reject	Cannot reject	Cannot reject	Cannot reject	Cannot reject
Standard controls = Cross products and squares	Cannot reject	Cannot reject	Cannot reject	Cannot reject	Reject	Cannot reject
Number of employees (EZ coefficient)						
No controls	−219.5*	−476.8**	−335.4***	−599.9***	−328.2	−889.9**
Standard controls	−52.39	−185.4	−196.7	−483.1***	−612.7	−1223
Cross products and squares	62.51	12.40	−144.8	−383.4**	−232.0	−49.81
No controls = Cross products and squares	Cannot reject	Cannot reject	Cannot reject	Cannot reject	Cannot reject	Cannot reject
Standard controls = Cross products and squares	Cannot reject	Cannot reject	Cannot reject	Cannot reject	Cannot reject	Cannot reject

Cross products and squares regressions follow Eq. (5) in the text and control for the following variables, their squared terms, their 1980–1990 trends, and all cross products: poverty rate, unemployment rate, percent non-white, percent with college degree, median income, home ownership rate, median house value, percent female household heads, percent receiving public assistance, average age of housing stock.

*** $p<0.01$.
** $p<0.05$.
* $p<0.1$.

idea is that if we add control variables that are relevant to how the number of establishments change and it does not change our results, this provides some evidence that missing variables would not change the results either. This is further strengthened by adding in all cross-products of control variables and their squared terms to ensure we have the proper specification. This robustness check involves re-estimating Eqs. (1), (2), and (4) as:

$$\Delta Y = \alpha + \beta_1(EZ_{close}) + \sum_{i=1}^{N} X_i + X_i^2 + X_i X_{j \neq i} + \varepsilon. \quad (5)$$

Where EZ_{close} represents one of the three types of closeness we describe in Eqs. (1), (2), and (4) depending on the specification, and X represents all control variables in the previous regressions.

Table 8 shows the results of estimating Eq. (5) for the various types of spillovers we measure in the short and long term and for both the number of establishments and number of employees. The number of establishment results is extremely similar to the results with controls in Table 3, and we cannot reject the null hypothesis that they are the same in either the case of geographic spillovers or spillovers in qualified areas. These results are also quite similar to the no controls results, although we are able to reject the null that the coefficients are the same in one specification. The propensity score results do not hold up to adding these additional square and cross product terms, although they actually start to lose significance when adding any controls. This is likely due to the much smaller sample size and addition of several variables straining the degrees of freedom—it seems to be asking too much of the model. The employment results are not nearly as robust—in many cases, the sign flips and the magnitudes are much different from specifications with no controls or some controls. We still cannot reject the null hypothesis that these coefficients are equal, but this is because of the large standard errors on the estimates with squares and cross products.

5.5. Using an improper comparison group: all other census tracts

As a way to see how the choice of comparison group matters (see Greenstone et al. (2010) for an excellent example measuring the spillover effects from new manufacturing facilities), we estimate the spillover effects from the EZ program by comparing both economically and geographically close areas to all other census tracts in the U.S. (excluding tracts that actually received a designation). Our estimating equation for this comparison is:

$$\Delta Y = \alpha + \beta_1(EZ_{close}) + X'\delta + \varepsilon, \text{ if } EZ, EC \neq 1 \quad (6)$$

where EZ_{close} equals one for tracts that are either geographically or economically close to actual EZs depending on the specification, and zero otherwise. These regressions include the full set of census tracts outside of EZ/EC areas, with no attempt to construct a comparison area that is similar to the spillover prone areas.

Table 9 shows the results of estimating Eq. (6) across the various spillover areas in both the short and longer term estimated with

Table 9
Spillover effects of targeted tax incentives on the number of establishments measured against all other census tracts (standard errors clustered at city level in parenthesis).

Spillover area	EZ_{geo}				EZ_{qual}				EZ_{pscore}			
	Short-term		Long-term		Short-term		Long-term		Short-term		Long-term	
EZ_{close}	−4.190	3.010	−2.345	10.61***	−18.04***	6.334**	−13.46***	19.97***	4.177*	11.29**	2.539	20.42***
	(6.015)	(3.056)	(5.268)	(2.849)	(2.898)	(3.186)	(2.286)	(2.846)	(5.115)	(4.689)	(4.031)	(4.268)
Poverty rate		83.54***		129.3***		83.36***		128.8***		83.05***		128.5***
		(10.98)		(12.13)		(10.98)		(12.13)		(10.97)		(12.12)
Unemployment rate		−20.81		−75.82***		−21.15		−76.78***		−19.25		−72.53***
		(23.29)		(23.11)		(23.22)		(23.09)		(23.14)		(23.01)
Percent non-white		−8.870***		−3.606		−8.681***		−3.031		−8.395***		−2.845
		(2.793)		(2.838)		(2.807)		(2.843)		(2.812)		(2.846)
Percent with college degree		32.49***		29.19***		32.16***		28.19***		32.08***		28.57***
		(6.297)		(6.807)		(6.310)		(6.819)		(6.311)		(6.818)
Median income		0.520***		0.369***		0.521***		0.370***		0.517***		0.366***
		(0.0868)		(0.0850)		(0.0867)		(0.0849)		(0.0868)		(0.0851)
Home ownership rate		−12.39***		13.09***		−12.53***		12.65***		−12.46***		12.92***
		(3.671)		(4.042)		(3.665)		(4.036)		(3.666)		(4.037)
Median house value		−0.221***		−0.268***		−0.222***		−0.272***		−0.222***		−0.269***
		(0.0217)		(0.0227)		(0.0216)		(0.0227)		(0.0217)		(0.0227)
Percent female household heads		−36.90***		−85.46***		−37.31***		−86.74***		−37.01***		−85.57***
		(8.678)		(8.796)		(8.685)		(8.787)		(8.677)		(8.792)
Percent receiving public assistance		−31.52**		−33.10**		−35.84**		−46.46***		−36.42**		−40.86**
		(15.61)		(16.64)		(16.46)		(17.31)		(15.95)		(17.07)
Average age of housing stock		1.670***		0.519**		1.678***		0.544**		1.655***		0.486**
		(0.195)		(0.217)		(0.194)		(0.217)		(0.196)		(0.218)
Average age of housing stock squared		−0.0543***		−0.0461***		−0.0545***		−0.0466***		−0.0540***		−0.0453***
		(0.00399)		(0.00415)		(0.00398)		(0.00415)		(0.00400)		(0.00416)
Constant	64.56***	51.92***	77.97***	89.05***	64.56***	52.37***	77.96***	90.46***	64.56***	52.49***	77.96***	89.98***
	(0.359)	(4.645)	(0.433)	(5.254)	(0.359)	(4.682)	(0.433)	(5.277)	(0.359)	(4.679)	(0.433)	(5.282)
City fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Includes 1980 to 1990 census trends		Yes		Yes		Yes		Yes		Yes		Yes
Observations	57,119	45,916	57,119	45,916	57,119	45,916	57,119	45,916	57,119	45,916	57,119	45,916
R-squared	0.033	0.119	0.022	0.122	0.034	0.119	0.022	0.123	0.033	0.119	0.022	0.122

Robust standard errors in parentheses.
Notes: EZ_{geo} results include only census tracts that border EZs in the treatment, but includes all other tracts (except for actual EZ or EC areas) in the control group. EZ_{qual} results include only tracts that qualified for Empowerment Zone status under the rules of the program and were located in EZ cities in the treatment, but includes all other census tracts (except actual EZ or EC areas) in the control group. EZ_{pscore} results include only tracts in the top decile of the propensity score distribution (estimated at the city level) and were located in EZ cities in the treatment, but includes all other tracts (except for actual EZ or EC areas) in the control.
*** $p<0.01$.
** $p<0.05$.
* $p<0.1$.

and without control variables with the number of establishments as the dependent variable. These results highlight that it is difficult to determine if any spillovers exist from the EZ program when using an improper comparison group. For both the geographically close and qualified areas, the sign of the estimates switches depending on whether control variables are included. The magnitude of the estimates ranges from increasing the number of establishments by 20 in the long term to decreasing the number of establishments by 18 in the short term in qualified areas. The estimates testing for spillovers using the propensity score to trim the spillover group all show a positive spillover effect of the program, but the magnitude ranges anywhere from an increase of 2 to 20 establishments. Statistical significance of these estimates also varies considerably across the specifications.

Table 10 displays the results of estimating Eq. (6) across the various spillover areas in both the short and longer term estimated with and without control variables using employees as the dependent variable. Ten of the 12 specifications produce results that suggest positive employment spillovers for neighboring or economically close areas, while only two suggest a negative effect. The size of the effect estimated this way is substantial, suggesting gains of between 230 and 360 employees in geographically close areas and between 170 and 380 in economically close areas, depending on the specification. All except one of the positive results is statistically significant at conventional levels.

Using all other census tracts as a comparison group for spillover prone areas is inappropriate because these areas differ along observable dimensions as shown in Table 1, and are also likely to differ along unobservable dimensions as the treated (spillover prone) areas were not included in the original EZ application although they were either qualified, geographically close, or both. Results in Tables 9 and 10 highlight that failing to carefully consider observable and unobservable dimensions of a comparison group can produce results that are biased and inconsistent.

5.6. Comparing spillovers and program effects

The existence of spillovers from spatially targeted redevelopment programs has two primary implications: geographically and economically close areas of the same city make a poor comparison group to evaluate economic redevelopment programs, and analysis of these programs may want to consider measuring targeted area gains net of losses in areas subject to spillovers. The negative spillovers we find for census tracts adjacent to targeted areas imply using them as a comparison group will cause upward bias in estimates of the program effect. To demonstrate the severity of the bias, we use the spillover areas as a comparison group to find the effect of the EZ program on treated areas by running the following regression:

$$\Delta Y = \alpha + \beta_1(EZ) + X'\delta + \varepsilon, \text{ if EZ or } EZ_{close} = 1. \tag{7}$$

We estimate Eq. (7) using both the geographically and economically close variants of EZ_{close}. Table 11 shows estimation results using the spillover areas as a control to measure the effect of the EZ program on treated areas with establishments as the dependent variable, Table 12 shows the results for employment.

Table 10
Spillover effects of targeted tax incentives on the number of employees measured against all other census tracts (standard errors clustered at city level in parenthesis).

Spill over area	EZ_{geo}				EZ_{qual}				EZ_{pscore}			
	Short-term		Long-term		Short-term		Long-term		Short-term		Long-term	
EZ_{close}	230.2**	281.2***	245.3	362.7**	−38.80	173.6**	−134.7*	199.2**	239.1***	380.8***	169.1**	323.5**
	(98.17)	(95.31)	(157.8)	(160.9)	(74.21)	(83.49)	(73.61)	(94.70)	(75.39)	(106.2)	(84.84)	(126.3)
Poverty rate		357.5		1480***		355.4		1478***		343.5		1470***
		(257.4)		(300.4)		(257.8)		(301.9)		(258.1)		(301.6)
Unemployment rate		1809*		790.6		1818*		804.0		1878*		860.1
		(980.6)		(830.0)		(978.6)		(828.7)		(980.4)		(823.4)
Percent non-white		364.5***		257.3***		365.9***		258.2***		377.1***		265.7***
		(99.45)		(91.48)		(99.90)		(92.05)		(101.1)		(92.37)
Percent with college degree		−145.8		−343.5		−150.0		−347.3		−155.3		−348.7
		(214.2)		(228.9)		(214.7)		(228.4)		(214.6)		(228.6)
Median income		7.036***		15.38***		7.131***		15.51***		7.017***		15.42***
		(1.508)		(1.967)		(1.509)		(1.964)		(1.509)		(1.969)
Home ownership rate		−671.2***		−774.1***		−677.1***		−781.3***		−675.3***		−778.9***
		(147.5)		(131.6)		(147.7)		(131.4)		(147.6)		(131.5)
Median house value		−2.489***		−4.150***		−2.521***		−4.187***		−2.511***		−4.168***
		(0.467)		(0.650)		(0.467)		(0.652)		(0.468)		(0.651)
Percent female household heads		−1374***		−1371***		−1382***		−1379***		−1375***		−1369***
		(298.4)		(261.4)		(299.3)		(260.7)		(298.7)		(261.0)
Percent receiving public assistance		−919.4**		−1421***		−995.6**		−1500***		−1047**		−1503***
		(421.5)		(447.0)		(449.0)		(461.8)		(436.9)		(450.6)
Average age of housing stock		24.65***		16.38***		24.62***		16.29***		23.92***		15.61***
		(5.270)		(5.996)		(5.230)		(5.909)		(5.295)		(5.993)
Average age of housing stock squared		−0.767***		−0.880***		−0.765***		−0.876***		−0.748***		−0.860***
		(0.0932)		(0.112)		(0.0926)		(0.110)		(0.0937)		(0.112)
Constant	635.4***	987.5***	937.8***	1450***	635.5***	996.1***	938.0***	1459***	635.5***	1003***	938.0***	1461***
	(7.956)	(145.8)	(9.669)	(138.1)	(7.955)	(147.9)	(9.667)	(140.4)	(7.955)	(148.2)	(9.667)	(140.2)
City fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Includes 1980 to 1990 census trends		Yes		Yes		Yes		Yes		Yes		Yes
Observations	57,119	45,916	57,119	45,916	57,119	45,916	57,119	45,916	57,119	45,916	57,119	45,916
R-squared	0.013	0.027	0.008	0.034	0.013	0.027	0.008	0.034	0.013	0.027	0.008	0.034

Notes: EZ_{geo} results include only census tracts that border EZs in the treatment, but includes all other tracts (except for actual EZ or EC areas) in the control group. EZ_{qual} results include only tracts that qualified for Empowerment Zone status under the rules of the program and were located in EZ cities in the treatment, but includes all other census tracts (except actual EZ or EC areas) in the control group. EZ_{pscore} results include only tracts in the top decile of the propensity score distribution (estimated at the city level) and were located in EZ cities in the treatment, but includes all other tracts (except for actual EZ or EC areas) in the control.

** $p<0.05$.
*** $p<0.01$.
* $p<0.1$.

Table 11
Effect of the EZ program on the number of establishments measured using spillover areas as controls (standard errors clustered at city level in parenthesis).

Comparison area	EZ_{geo}				EZ_{qual}				EZ_{pscore}			
	Short-term		Long-term		Short-term		Long-term		Short-term		Long-term	
EZ_{close}	12.57**	18.36***	18.39***	22.99***	22.90***	24.48***	27.59***	27.35***	6.855	14.38*	14.57***	15.48**
	(6.316)	(6.850)	(5.674)	(5.450)	(3.678)	(4.959)	(3.401)	(3.917)	(5.888)	(8.336)	(4.970)	(6.115)
Poverty rate		126.8**		89.71*		121.6**		100.4**		136.2*		106.6*
		(53.67)		(46.97)		(57.14)		(45.29)		(71.31)		(58.02)
Unemployment rate		17.63		7.499		79.71*		51.50		33.13		33.23
		(55.36)		(45.49)		(42.76)		(31.54)		(56.82)		(43.76)
Percent non-white		−40.60***		−29.64**		−26.23**		−20.39*		−32.51**		−25.79*
		(13.27)		(12.05)		(12.29)		(11.05)		(16.57)		(15.35)
Percent with college degree		−9.441		8.234		121.5*		124.7**		44.42		85.02
		(55.39)		(55.31)		(66.09)		(62.19)		(91.05)		(88.25)
Median income		0.390		−0.0123		0.821		0.715		1.423		1.204
		(0.581)		(0.463)		(0.974)		(0.721)		(1.207)		(0.944)
Home ownership rate		−53.40**		−45.09**		−33.24**		−25.32*		−67.62***		−57.36**
		(20.96)		(18.88)		(15.03)		(13.04)		(25.54)		(23.46)
Median house value		−0.0956		−0.0973		−0.156		−0.114		−0.214		−0.191
		(0.145)		(0.114)		(0.122)		(0.102)		(0.179)		(0.141)
Percent female household heads		18.21		−29.62		−10.77		−35.77		−9.743		−51.98
		(43.80)		(38.52)		(36.21)		(31.14)		(49.31)		(42.22)
Percent receiving public assistance		−192.4***		−144.7**		−159.9***		−111.5**		−189.8***		−139.7*
		(57.23)		(68.15)		(44.56)		(52.79)		(58.37)		(71.79)
Average age of housing stock		−0.198		−1.370		−1.414		−1.665		−0.523		−0.428
		(3.958)		(3.434)		(2.587)		(2.222)		(4.352)		(3.701)
Average age of housing stock squared		−0.0188		0.00923		0.00357		0.0149		−0.0120		−0.00557
		(0.0629)		(0.0549)		(0.0405)		(0.0352)		(0.0691)		(0.0592)
Constant	47.87***	104.7*	35.97***	122.7**	37.54***	80.97	26.76***	72.77*	53.59***	92.04	39.79***	83.11
	(5.394)	(60.13)	(4.725)	(48.36)	(1.654)	(54.08)	(1.305)	(39.15)	(4.886)	(77.63)	(3.851)	(57.35)
City fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Includes 1980 to 1990 census trends		Yes		Yes		Yes		Yes		Yes		Yes
Observations	1816	1725	1816	1725	2709	2593	2709	2593	1690	1518	1690	1518
R-squared	0.002	0.085	0.005	0.097	0.014	0.096	0.024	0.116	0.001	0.089	0.003	0.110

Robust standard errors in parentheses.
Notes: EZ_{geo} results include only census tracts that border EZs in the control, and includes all EZ tracts as treatment. EZ_{qual} results include only tracts that qualified for Empowerment Zone status under the rules of the program and were located in EZ cities in the control, and all EZ tracts as treatment. EZ_{pscore} results include only tracts in the top decile of the propensity score distribution (estimated at the city level) and located in EZ cities in the control, and all EZ tracts as treatment.
** $p<0.05$.
*** $p<0.01$.
* $p<0.1$.

These results highlight the bias that occurs when choosing an area prone to spillovers to measure the effect of the EZ program. Measuring the program effect using the spillover prone areas as a comparison group, the EZ has a sizable positive effect—creating between 12 and 18 establishments in geographically close areas in the short term, and between 18 and 23 establishments over the longer term (depending on the specification). The biased measure of EZ success is statistically significant in all cases using geographically close areas as a comparison group. Measuring the program effect using economically close areas as the comparison group suggests a similar positive effect of the EZ program—an increase of between 6 and 25 firms in the short term (depending on comparison group and specification) and between 14 and 28 in the longer term (depending on comparison group and specification). All but two of the results using economically similar areas as a comparison group are statistically significant at conventional levels, and one of those is statistically significant at the ten percent level.

Results for employment generated by the EZ program using the spillover prone areas as a comparison group are also positive across the board. These results suggest substantial gains from the EZ program, although the estimates are less precise than the establishment results. Using geographically close areas as a comparison group suggests gains of between 81 and 152 employees in the short term, and between 233 and 279 in the longer term, but none of these is statistically meaningful. Using economically close areas as the comparison group produces larger estimates—an increase of between 164 and 378 employees in the short term, and between 421 and 630 in the longer term. These results are also more precise as all but two specifications produce statistically significant results.

Subtracting the negative effect on the spillover prone areas from the biased estimates in Tables 9 and 10 (using only estimates of both spillovers and program effects that are statistically significant), the net effect on the number of establishments from the program is actually negative in six of the nine cases (between zero and negative one in two cases). Under the most pessimistic estimates, the net effect of the program is a loss of about 18 establishments. Under the most optimistic estimates, the net effect is an increase of about 7 establishments—a gain of 27 in EZ areas, and a loss of 20 in spillover prone areas (using the long term results from the EZ qualified areas estimated with control variables).

The net effect on employment (again using only statistically significant estimates) is positive in three cases, but gains from the program are almost completely offset by losses in spillover areas. The largest net gain is 124 employees (using the long term results from the EZ qualified areas estimated with controls)—a gain of 607 in EZ areas, with a loss of 483 in spillover prone areas. The most pessimistic estimate is a net loss of 469 employees (using the long term results from the propensity score estimated without controls)—890 employees lost in spillover areas, compared with only 421 gained in EZ areas.

6. Discussion and conclusion

This paper offers an empirical test of spillovers from a prominent spatially targeted economic redevelopment program. Estimates suggest

Table 12
Effect of the EZ program on the number of employees measured using spillover areas as controls (standard errors clustered at city level in parenthesis).

Comparison area	EZ_{geo}				EZ_{qual}				EZ_{pscore}			
	Short-term		Long-term		Short-term		Long-term		Short-term		Long-term	
EZ_{close}	81.11	152.9	233.3	279.7	376.6***	378.2***	630.2***	607.0***	164.9*	211.9	421.6***	436.6***
	(115.3)	(162.4)	(191.8)	(243.8)	(75.94)	(110.1)	(88.92)	(112.8)	(88.94)	(147.5)	(111.9)	(158.8)
Poverty rate		1357		3028**		2524**		3748***		1742		2723*
		(981.6)		(1363)		(1226)		(1323)		(1231)		(1474)
Unemployment rate		982.4		666.5		284.2		1067		910.9		1223
		(1209)		(1312)		(1342)		(1105)		(1205)		(1291)
Percent non-white		−548.0**		−1021***		−435.8*		−803.7***		−487.6*		−878.2***
		(217.9)		(268.6)		(229.2)		(250.3)		(253.9)		(326.4)
Percent with college degree		1092		229.6		950.2		1559		1816		2133
		(1129)		(1197)		(1720)		(1650)		(1844)		(1842)
Median income		4.752		6.998		41.30		49.32**		28.80		26.96
		(12.01)		(13.22)		(26.14)		(23.17)		(23.20)		(24.58)
Home ownership rate		−712.2*		−1152**		−201.9		−522.1		−834.1		−1383**
		(428.4)		(510.0)		(311.2)		(354.1)		(515.9)		(612.0)
Median house value		0.0397		0.339		−1.493		−2.946		−2.481		−4.747
		(2.839)		(4.502)		(2.078)		(2.335)		(2.850)		(3.187)
Percent female household heads		165.8		9.795		690.0		489.9		59.26		−277.6
		(738.4)		(789.9)		(810.6)		(735.3)		(806.1)		(850.6)
Percent receiving public assistance		−1907*		−3581***		−1306		−2803***		−1863*		−3477***
		(1043)		(1273)		(880.4)		(1025)		(1055)		(1293)
Average age of housing stock		26.60		29.61		2.107		−15.31		−14.84		−27.71
		(75.97)		(105.4)		(56.25)		(63.69)		(78.66)		(98.43)
Average age of housing stock squared		−0.904		−0.854		−0.375		−0.0285		−0.175		0.0663
		(1.223)		(1.676)		(0.872)		(0.987)		(1.251)		(1.550)
Constant	547.8***	887.0	733.1***	1345	252.3***	−862.9	336.3***	−420.2	464.0***	723.8	544.9***	1651
	(94.86)	(1142)	(173.2)	(1608)	(38.33)	(1478)	(33.69)	(1309)	(60.09)	(1327)	(75.74)	(1590)
City fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Includes 1980 to 1990 census trends		Yes		Yes		Yes		Yes		Yes		Yes
Observations	1816	1725	1816	1725	2709	2593	2709	2593	1690	1518	1690	1518
R-squared	0.000	0.038	0.001	0.053	0.009	0.038	0.019	0.072	0.001	0.044	0.004	0.068

Notes: EZ_{geo} results include only census tracts that border EZs in the control, and include all EZ tracts as treatment. EZ_{qual} results include only tracts that qualified for Empowerment Zone status under the rules of the program and were located in EZ cities in the control, and all EZ tracts as treatment. EZ_{pscore} results include only tracts in the top decile of the propensity score distribution (estimated at the city level) and located in EZ cities in the control, and all EZ tracts as treatment.

*** $p<0.01$.
* $p<0.1$.
** $p<0.05$.

that the EZ program is responsible for considerable negative spillovers on neighboring and economically similar areas, both in terms of the number of establishments located in these areas and employment at local establishments. We find that losses are especially strong in the retail and service industries.

Given the EZ program uses tight geographic targeting in densely populated urban areas, establishments can benefit by literally moving across the street into the EZ to enjoy the benefits of the program without incurring relocation costs associated with moving further from a customer base, employees, or losing other advantages of the immediate location. Establishments' relocating from spillover prone areas into EZ areas seems to be at least some of the cause of spillovers, as Hanson and Rohlin (2011a) show that the EZ is responsible for attracting new business establishments.

Spillovers caused by relocation suggest a zero net effect from the program; however, some of our estimates suggest a negative net effect of the program. Negative net effects could be the result of spillovers causing job (and establishment) destruction in neighboring and similar areas, possibly through increased competition from establishments subsidized by the EZ program or factor price increases. The D&B data does not differentiate establishments and jobs that move from those being destroyed, but, if the goal of policy makers is to induce relocation, it seems that even this modest objective may come at a cost of destroying jobs and establishments in areas that compete with targeted places.

It is still possible that redevelopment programs are successful even if the only measureable outcomes in targeted areas come at the expense of neighboring areas.[19] Programs may provide better access to jobs for those most in need, or retail access for those living in isolation. In this way, the benefits from an additional establishment may be greater in targeted neighborhoods than they are in neighboring areas. Of course, measuring success by this type of metric is harder to imagine when spillovers occur in economically, not only geographically, close areas as we show here, but it remains a possibility.

Our findings suggest that spillovers should be an important consideration for policy makers when deciding how and where to target redevelopment programs. Accounting for costs and benefits over a broader geographic (and economically similar) area may be appropriate, as any gains in targeted areas may come at the expense of areas prone to spillovers from the policy. Our findings also suggest caution when choosing a comparison group to evaluate economic redevelopment policies. Spillovers from these policies on geographically and economically close areas suggest that they are not useful as a comparison group for evaluation because they themselves are affected by the treatment so that using them violates the no interference between

[19] It may also be that benefits and spillovers manifest in other outcome measures such as property values. We attempt to measure spillovers on property values using 1990–2000 census tract median property values as the dependent variable with the same identification strategy. These results are quite erratic, changing sign, magnitude and statistical significance depending on the specification and comparison area we use. This may be a function of the quality of these data, as property values are self-reported in the census. It may also be that there is a heterogeneous spillover for properties based on distance or access to EZ areas.

units assumption necessary for unbiased estimates of the program effect.

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