Chapter 8
Place-based Policies

Place-based policies are strategies to revitalize a particular area, without targeting people explicitly, but targeting based on place of work or place or residence instead. Place-based policies are popular among governments. They usually come as tax subsidies, or government spending in particular areas.

We can think of MTO as an indirect place-based policy (Neumark and Simpson, 2015), but we will focus on direct policies. Economists tended no to think of place-based policies as potentially welfare-improving for many reasons. For starters, in a Roback-style spatial equilibrium model (Roback, 1982), any differences in wages capitalize into rents, and there would not be any net gains in utility. On the other hand, spatial equilibrium implies that the gains from any local program will not be local at all. Any gains from the program will dissipate through space. They would make individuals who were in the margin of moving to the subsidized area effectively move to it, reaping the wage gains (Kline and Moretti, 2014)

Still, there are a few reasons why place-based policies may still be effective (Neumark and Simpson, 2015):

- Agglomeration economies: In presence of agglomeration economies, which are an externality, the spatial equilibrium need not be efficient. In fact, we saw that we may want to make cities larger if agglomeration externalities are big (Henderson, 1974). So it may make sense to redirect economic activity to areas that have higher agglomeration economies.
- Knowledge spillovers: Attracting high-skilled workers to an area may be benefitial in terms of residential externalities.
- Industrial clustering: In order for the increasing returns to scale from density to kick in, it may be necessary for several firms within the same industry to cluster. The case for this is stronger if agglomeration externalities are stronger within industry.
- Spatial mismatch: Workers may not be able to match with firms optimally in depressed areas. If there were any mismatch to begin with, any temporary depression in an area may make it stronger, leading many people out of work. However, revitalizing the depressed area may be only a temporary fix, as workers will stay

in that area and not look for other opportunities outside of it. Kline and Moretti (2013) show that in a matching model, a temporary downturn may lead firms to inefficiently low hiring.

8.1 Assessing the Incidence and Efficiency of a Prominent Place Based Policy

Busso et al. (2013) evaluate the effects of a large place-based policy in the United States, the Empowerment Zone program. In doing so, they provide a state-of-the-art example of how spatial equilibrium concepts can be integrated with reduced-form methods to estimate the welfare gains from these kinds of policies. Their paper is an application of a "sufficient statistics" approach (Chetty, 2009) that combines tools from structural models and reduced-form estimation.

The Empowerment Zone (EZ) program started in 1993, and provided tax incentives for six urban communities in cities in the US: Atlanta, Baltimore, Chicago, Detroit, New York City and Philadelphia/Camden. The incentives were local in nature, with each area averaging 10 square miles in area. The program had two components: employment tax incentives of the order of 3000 dollars per employee, and up to a 100 million dollars in grants for infrastructure, business assistance, etc ... Total spending in the program was 3234 million dollars.

8.1.1 Model

Busso et al. (2013) formulate a Rosen-Roback style model for the location choices of workers, and include the EZ tax incentives and grants in the problem. There is a continuum of workers and discrete neighborhoods $\mathcal{N}$, of which $\mathcal{N}_0$ are outside the EZ and $\mathcal{N}_1$ are inside the EZ. There are two economic sectors $s = \{1,2\}$ The utility for individual i of living in neighborhood j and working in neighborhood k is

$$u_{ijks} = w_{jks} - r_j - \kappa_{jk} + A_j + \varepsilon_{ijks}. \tag{8.1}$$

w_{jks} are wages, r_j are rents, κ_{jk} is a commuting cost, A_j are local amenities and ε_{ijks} is an i.i.d. idiosyncratic error term. Notice that in absence of the error term, all workers would locate in the same place, and any place-based policy would dissipate across the neighborhood. With the error term, there is heterogeneity in the choices of workers. Some of them are inframarginal, so a small tax incentive would not change their location.

We proceed in an analogous fashion to previous chapters. First, we find the probability of a worker living and working in a particular pair of neighborhoods and a sector. We let D_{ijks} equal 1 if individual i chooses neighbourhoods j,k and sector s.

$$D_{ijks} = \mathbb{1}\left(max_{j',k',s'}\ u_{ij'k's'} = u_{ijks}\right). \tag{8.2}$$

Then the number of workers in these communities and sector is:

$$N_{jks} = Pr(D_{ijks} = 1) \tag{8.3}$$

After this, Busso et al. (2013) provide a key insight that will allow us to make welfare statements in this model. Let $v_{ijks} = w_{jks} - r_j - \kappa_{jk} + A_j$ be all the characteristics of utility that do not vary by individual. Now let welfare be the average utility across neighborhoods:

$$V = E_\varepsilon\left[max_{j'k's'}\left\{u_{ij'k's'}\right\}\right] \tag{8.4}$$

This average utility varies with the local components of utility according to:

$$\begin{aligned} \frac{dV}{dv_{jks}} &= E_\varepsilon\left[\frac{d}{dv_{jks}} max_{j'k's'}\left\{u_{ij'k's'}\right\}\right] \\ &= E_\varepsilon\left[\mathbb{1}\left[max_{j'k's'}\left\{u_{ij'k's'}\right\} = u_{ijks}\right]\right] \\ &= Pr\left(D_{ijks} = 1\right) = N_{jks} \end{aligned} \tag{8.5}$$

This is a powerful result, because it allows us to relate an unobservable quantity, the gain in welfare, with an observable quantity, which is just the number of people who choose a particular pair of neighborhoods and a sector. Intuitively, as the local determinants of utility change, both existing residents and new movers gain. But for a small change, movers do not gain much because they were indifferent across locations. So the first-order gain is that of residents.

We now turn to firms. There is a representative firm in each location, with a CRS production function $F(K_{ks}, B_k L_{ks}$. K is capital, L is labor and B_k is local productivity. Because of CRS, we can rewrite this as $B_k L_{ks} f(\chi_{ks})$, where $\chi_{ks} = \frac{K_{ks}}{B_k L_{ks}}$ is capital per effective units of labor. Firms in sector 1 receive a subsidy. When these firms hire workers from outside the EZ, they pay full wages. But for workers inside the EZ, they get a wage subisdy τ. The firm's first order conditions are:

$$B_k\left[f(\chi_{ks}) - \chi_{ks} f'(\chi_{ks})\right] = w_{jks}(1 - \delta_{jks}), \tag{8.6}$$

$$f'(\chi_{ks}) = \rho. \tag{8.7}$$

Here, ρ is the cost of capital, which is the same everywhere. δ_{jks} is 1 for workers inside the *EZ* and in sector 1.

Let $\chi = h(\rho)$ be the optimal capital labor ratio. Then the wage FOC can be rewritten as:

$$w_{jks} = \frac{B_k\left[f(h(\rho)) - h(\rho)\rho\right]}{1 - \tau\delta_{jks}} \tag{8.8}$$

This shows that the tax subsidies are translated into their wages. One would expect that everyone would move to the EZ as a response, but this does not happen because some workers prefer to work outside of it.

This also shows the effects of the grants on wages. Wages should vary 1 on 1 with B_k, which captures the local component of productivity. Note that these effects are not sector-specific, and that these wage increases may induce commuting and migration towards the EZ area.

Let us move on to the housing component of the model. In each location j, there is a continuum of land owners. Each land owner may provide a unit of housing at a cost, distributed with CDF $G(j)$. For a price of housing r_j, the last landowner that provides housing must break even.

$$G_j^{-1}(H_j) = r_j. \tag{8.9}$$

The labor and housing market clearing conditions are:

$$L_{ks} = \sum_{j \in \mathscr{N}} L_{jks}, \tag{8.10}$$

$$H_j = \sum_k \sum_s N_{jks} \tag{8.11}$$

8.1.2 Welfare Analysis

Total welfare in this model is the sum of the utility of workers and the benefits to landlords:

$$W = V + \sum_j \left[r_j H_j - \int_0^{H_j} G_j^{-1}(x) dx \right] \tag{8.12}$$

We can differentiate this expression to see the effects of the EZ program on welfare. The effect of the block grants is:

$$\frac{d}{dB_m} W \bigg|_{\tau=0} = \sum_j \sum_k \sum_s N_{jks} \left[\frac{dw_{jks}}{dB_m} - \frac{dr_j}{dB_m} \right] + \sum_j \frac{dr_j}{dB_m} H_j \tag{8.13}$$

This does not depend on $\frac{dN_{jks}}{dB_m}$, the movers, because of the same reason dV did not consider the movers. They are indifferent across locations, so the grants do not affect them to a first order. Using (8.11) and (8.8), this equals

$$\frac{d}{dB_m} W \bigg|_{\tau=0} = \left[f(\chi_{ks}) - \chi_{ks} f'(\chi_{ks}) \right] \sum_j \sum_s N_{jms}. \tag{8.14}$$

which is just the marginal productivity of labor times the number of workers in neighborhood m.

The effect of the tax subsidies is:

$$\frac{d}{d\tau} = \sum_{j\in\mathscr{N}_1}\sum_{k\in\mathscr{N}_1} N_{jk1} w_{jk1} \frac{d\ln w_{jk1}}{d\tau} \tag{8.15}$$

which depends on the increase of wages attributed to the program. There is however, a deadweight loss from the tax subsidies. The marginal cost of the subsidies is $\frac{d}{d\tau}\sum_{j\in\mathscr{N}_1}\sum_{k\in\mathscr{N}_1} N_{jk1} w_{jk1}\tau$. Out of this cost, part of it is offset by the increase in wages. The other part, however is lost by the mobility of workers. Busso et al. (2013) show that for a subsidy increase $d\tau$ the deadweight loss is approximately

$$DWL_{\tau} = \frac{1}{2}\frac{d\ln N_{jk1}}{d\tau^2} \sum_{j\in\mathscr{N}_1}\sum_{k\in\mathscr{N}_1} N_{jk1} w_{jk1} \tag{8.16}$$

Notice that all these terms can be estimated from reduced-form estimates of the effects of the program on employment, wages and population.

8.1.3 Empirics

Busso et al. (2013) compare census tracts that received EZ status to zones that applied for EZ status and were rejected, and to zones that entered the program later. The comparison, in the spirit of Greenstone et al. (2010), assumes that this comparison controls for unobservables that may be specific to EZ-worthy areas. The regression of interest is

$$\Delta Y_{tzc} = \beta T_z + \mathbf{X}'_n(t)\alpha^x + \mathbf{P}'_c\alpha^p + e_{tzc} \tag{8.17}$$

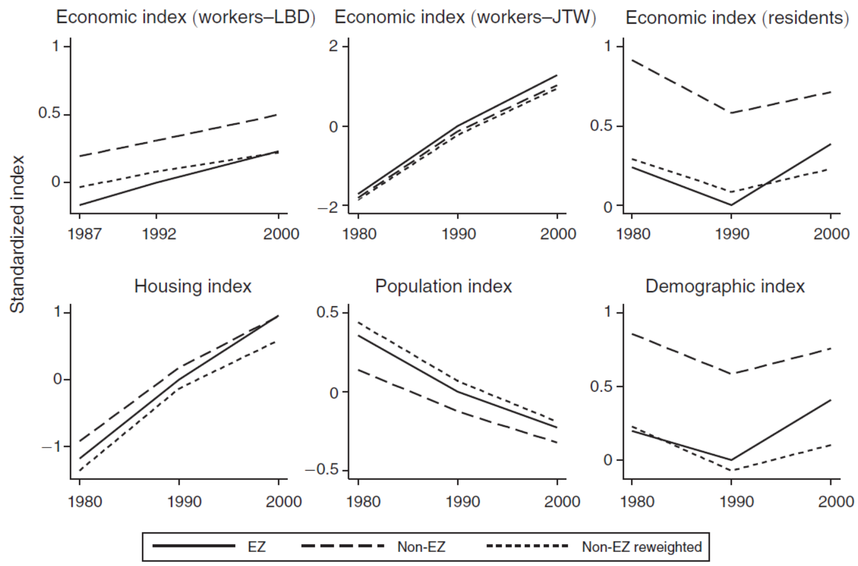
Y_{tzc} is an outcome for census tract t in zone z and county c. β is the coefficient of interest. Note that the regression is in differences so the fixed effects cancel out, and the time effects would enter the error term. X are neighbourhood characteristics and county characteristics. By including these, they allow heterogeneous trends over these characteristics. They have the luxury of having confidential data at the tract level from the 1980, 1990 and 2000 censuses. For their preferred estimates, they adjust the control group using a propensity score matching method.

Figure 8.1 shows that the matching method is doing its job. It compares the evolution over time of outcomes for EZ an Non-EZ tracts. These do not look too similar in the beginning, but they do look similar after the Non-EZ group has been adjusted through matching.

Figure 8.2 provides a glimpse of the effects of the EZ program. After 1997, wages start to diverge between the EZ group and the adjusted control group. The visual evidence for the number of jobs and establishments is also clear: note the change in the trend in the EZ line after 1997 relative to the controls.

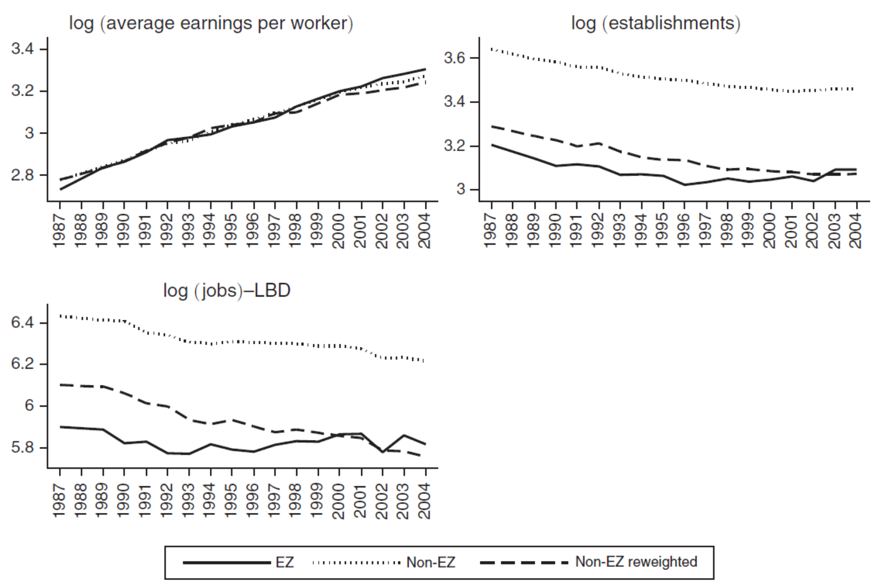
Table 8.1 shows estimates of the impact on wages and jobs. Their preferred estimates are the PW estimates, which compare the EZ tracts to the adjusted control

Fig. 8.1 Means by Year and Treatment Status



Source: Busso et al. (2013)

Fig. 8.2 Job, Wages, and Establishments (LBD)



Source: Busso et al. (2013)

Non-EZ tracts. There is some evidence of increases in the number of jobs, on the order of 14% for existing firms. The point estimates for the impacts on wages are actually non-significant.

Table 8.1 Wages and Jobs Impacts

	Naïve (1)	OLS (2)	PW (3)	Observations (4)
All firms				
log (jobs)	0.122 (0.048)*	0.179 (0.051)***	0.213 (0.072)***	1,651
log (establishments)	0.028 (0.027)	0.041 (0.017)**	0.057 (0.036)*	1,651
log (average earnings per worker)	−0.018 (0.013)	−0.002 (0.017)	0.001 (0.018)	1,651
All firms present in 1992				
log (jobs)	0.042 (0.044)	0.107 (0.053)	0.143 (0.068)*	1,650
log (establishments)	−0.057 (0.033)	−0.022 (0.027)	−0.013 (0.035)	1,650
log (average earnings per worker)	−0.022 (0.020)	−0.007 (0.020)	0.003 (0.027)	1,650
Five or fewer employees				
log (jobs)	−0.155 (0.108)	−0.048 (0.086)	−0.035 (0.115)	1,577
log (establishments)	−0.093 (0.074)	−0.064 (0.059)	−0.059 (0.082)	1,577
log (average earnings per worker)	−0.026 (0.025)	0.011 (0.027)	0.009 (0.032)	1,577
Six or more employees				
log (jobs)	0.065 (0.070)	0.119 (0.060)	0.150 (0.092)	1,635
log (establishments)	0.007 (0.021)	0.030 (0.019)	0.043 (0.031)	1,635
log (average earnings per worker)	−0.023 (0.023)	−0.016 (0.021)	−0.004 (0.026)	1,635

Notes: Each entry gives the 1992–2000 differences-in-differences (DD) estimate of EZ designation on the outcome presented in each row. Column 1 reports DD estimates without controls; column 2 reports DD estimates controlling for lagged city and tract level characteristics; column 3 reports parametric reweighting DD estimates. See Section IV for list of covariates. Column 4 shows the number of observations used in the estimation of the treatment effect for each outcome. Asymptotic standard errors are shown in parentheses and are clustered by city (63 clusters). Asterisks reflect significance level obtained by a clustered wild bootstrap-*t* procedure described in Appendix A.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Source: Busso et al. (2013)

To see if the new jobs are being held by EZ residents or non EZ residents, table 8.2 looks at the impact on jobs for residents and non-residents. Recall that in order to be eligible for an EZ tax subsidy, the worker has to live and work in the EZ. The first row shows that there is some evidence of increases in jobs for workers who live and work in the EZ. This estimate also seems different from the estimates for other groups, suggesting that the gains in jobs are not dissipating through space.

Table 8.2 Employment Impacts

	Naïve (1)	OLS (2)	PW (3)	Observations (4)
log (jobs)	0.187	0.145	0.122	1,656
	(0.062)	(0.061)*	(0.085)	
By place of residence and place of work				
log (zone jobs held by zone residents)	0.166	0.150	0.176	1,653
	(0.088)	(0.072)	(0.103)*	
log (zone jobs held by nonresidents)	0.161	0.097	0.064	1,656
	(0.050)*	(0.059)	(0.073)	
log (nonzone jobs held by zone residents)	0.033	0.084	0.123	1,654
	(0.060)	(0.062)	(0.061)	

Notes: Each entry gives the 1990–2000 differences-in-differences (DD) estimate of EZ designation on the outcome presented in each row. Column 1 reports DD estimates without controls; column 2 reports DD estimates controlling for lagged city and tract level characteristics; column 3 reports parametric reweighting DD estimates. See Section IV for list of covariates. Column 4 shows the number of observations used in the estimation of the treatment effect for each outcome. Asymptotic standard errors are shown in parentheses and are clustered by city (63 clusters). Asterisks reflect significance level obtained by a clustered wild bootstrap-*t* procedure described in Appendix A.

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**Significant at the 5 percent level.
*Significant at the 10 percent level.

Source: Busso et al. (2013)

This is important because it highlights the role of imperfect mobility. Because households have preferences over places, when the wage subsidies kick in, not everyone moves to the EZ. Only the marginal workers do. Because of this, the inframarginal workers in the EZ are able to get some benefits from the policy, relative to workers outside of the EZ. Table 8.3 drives this point across. The wage impacts are only visible for the EZ subsidy eligible workers, and are on the order of 12%.

Busso et al. (2013) also estimate a substantial impact of the EZ policy coming through the housing market. Table 8.4 shows two facts. First, rents did not increase much. So workers who rent may actually be benefiting from this policy, because the extra wages did not capitalize into rents. Instead, they capitalized in house values. This may be because of the relative large amount of grant subsidies compared to wage subsidies.

Last, Busso et al. (2013) use the sufficient statistics machinery to estimate the welfare benefits and the deadweight loss from the program. In their pessimistic scenario, they assume that there are not any spillovers of the policy for workers outside the zone. In their baseline scenario they do account for these spillovers. The increase

Table 8.3 Wage Impacts

	Unadjusted			Composition-adjusted		
	Naïve (1)	OLS (2)	PW (3)	Naïve (4)	OLS (5)	PW (6)
Panel A. Weekly wages						
log (weekly wage income of zone residents)	0.037 (0.035)	0.047 (0.021)	0.040 (0.037)	0.026 (0.032)	0.053 (0.015)**	0.050 (0.033)
log (weekly wage income of zone workers)	−0.010 (0.026)	0.011 (0.030)	0.003 (0.031)	0.001 (0.024)	0.017 (0.026)	0.010 (0.029)
Panel B. Weekly wages by place of residence and place of work						
log (weekly wage income of zone residents working in zone)	0.078 (0.045)	0.127 (0.041)**	0.112 (0.055)*	0.088 (0.046)	0.133 (0.051)**	0.121 (0.051)**
log (weekly wage income of nonresidents working in zone)	−0.014 (0.029)	−0.015 (0.033)	−0.010 (0.035)	0.006 (0.023)	0.005 (0.027)	0.006 (0.030)
log (weekly wage income of zone residents working outside zone)	0.023 (0.028)	0.043 (0.034)	0.047 (0.031)*	0.006 (0.025)	0.036 (0.024)	0.045 (0.027)*
Panel C. Annual wage income by place of residence and place of work						
log (annual wage income of zone residents working in zone)	0.181 (0.062)**	0.244 (0.075)**	0.219 (0.074)**	0.108 (0.074)	0.184 (0.085)*	0.166 (0.078)
log (annual wage income of nonresidents working in zone)	−0.023 (0.040)	−0.022 (0.038)	−0.012 (0.043)	−0.002 (0.031)	0.000 (0.026)	0.005 (0.035)
log (annual wage income of zone residents working outside zone)	0.020 (0.038)	0.040 (0.052)	0.038 (0.043)	−0.005 (0.030)	0.031 (0.036)	0.035 (0.035)

Notes: Each entry gives the 1990–2000 differences-in-differences (DD) estimate of EZ designation on the outcome presented in each row. Columns 4–6 adjust the outcomes for demographic changes at the micro-level (see Section IV). Columns labeled "Naïve" report DD estimates without controls. Columns labeled "OLS" report the DD estimates controlling for lagged city and tract level characteristics. Columns labeled "PW" report parametric reweighting DD estimates. Asymptotic standard errors are shown in parentheses and are clustered by city (63 clusters). Asterisks reflect significance level obtained by a clustered wild bootstrap-*t* procedure described in Appendix A.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Source: Busso et al. (2013)

in payroll is estimated to be around 300 million dollars, and the increase in house values is around 1350 million dollars. Their costs estimates are 400 million for the grants, and about 50 million for the wage subsidies. The program seems to have been effective, although it is hard to isolate the impact of each policy separately. Their estimate of the deadweight loss from the program is only about 7 million dollars.

Table 8.4 Housing Impacts

	Unadjusted			Composition-adjusted		
	Naïve (1)	OLS (2)	PW (3)	Naïve (4)	OLS (5)	PW (6)
log (rent)	0.023	0.019	0.029	0.014	0.006	0.018
	(0.032)	(0.030)	(0.032)	(0.028)	(0.026)	(0.027)
log (rent of new residents)	0.055	0.038	0.055	0.044	0.028	0.046
	(0.045)	(0.037)	(0.045)	(0.040)	(0.033)	(0.039)
log (house value)	0.370	0.281	0.311	0.371	0.281	0.317
	(0.129)*	(0.065)**	(0.142)	(0.125)*	(0.064)**	(0.138)*
log (house value of new residents)	0.208	0.143	0.142	0.246	0.164	0.171
	(0.145)	(0.104)	(0.163)	(0.131)	(0.098)	(0.151)

Notes: Each entry gives the 1990–2000 differences-in-differences (DD) estimate of EZ designation on the outcome presented in each row. Columns 4–6 adjust the outcomes for demographic changes at the micro-level (see Section IV). Columns labeled "Naïve" report DD estimates without controls. Columns labeled "OLS" report the DD estimates controlling for lagged city and tract level characteristics. Columns labeled "PW" report parametric reweighting DD estimates. Asymptotic standard errors are shown in parentheses and are clustered by city (63 clusters.) Asterisks reflect significance level obtained by a clustered wild bootstrap-*t* procedure described in Appendix A.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Source: Busso et al. (2013)

Table 8.5 Welfare Analysis

	Total workers/ people/ households	Total annual payroll/ rents/housing value (in billion $)	OLS impact on wages/ rents/housing values	Increase in annual payroll/rents/housing value (in million $) Baseline scenario (1)	Pessimistic scenario (2)
Panel A. Total impact of the program					
Zone residents working in zone	38,331	0.8	0.133	108.5	37.5
Zone residents working outside zone	140,708	3.3	0.036	117.5	0.0
Nonresidents working in zone	365,918	14.0	0.005	69.9	0.0
House renters in the zone	189,982	0.9	0.006	5.5	66.9
House owners in the zone	46,161	4.8	0.281	1350.4	499.8

	OLS impact	Confidence interval
Panel B. Average impact of the program		
log (weekly wage of zone residents working in zone)[a]	0.133	[0.046; 0.248]
log (weekly wage of nonresidents working in zone)[a]	0.005	[−0.055; 0.076]
log (weekly wage of zone residents working outside zone)[a]	0.036	[−0.011; 0.100]
log (rent)[a]	0.006	[−0.054; 0.073]
log (housing value)[a]	0.281	[0.104; 0.426]
log (weekly wage of zone residents working in zone) − 0.25 log(rent)[a]	0.128	[0.034; 0.253]
log (zone jobs held by zone residents)	0.150	[−0.003; 0.326]

Notes: See Section VII for details. Price variables in panel B have been adjusted via a procedure described in Section IV. Confidence intervals were constructed by inverting a wild bootstrap *t*-test.

[a] Denotes outcomes that have been adjusted for demographic or, in the case of rents and housing values, quality changes at the micro-level (see Section IV). "Baseline scenario" uses OLS point estimates in computing impacts. "Pessimistic scenario" uses lower limit of 90 percent confidence intervals for impacts on earnings of zone residents working in zone and housing values and upper limit of confidence interval for rent impacts.

Source: Busso et al. (2013)