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

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This paper empirically assesses the incidence and efficiency of Round I of the federal urban Empowerment Zone (EZ) program using confidential microdata from the Decennial Census and the Longitudinal Business Database. Using rejected and future applicants to the EZ program as controls, we find that EZ designation substantially increased employment in zone neighborhoods and generated wage increases for local workers without corresponding increases in population or the local cost of living. The results suggest the efficiency costs of first Round EZs were relatively modest. (*JEL* H26, H77, J31, R23, R58)

A growing class of "place based" policies explicitly target transfers toward particular geographic areas rather than groups of individuals.[1] Economists have traditionally expressed little support for such programs, fearing they will generate large distortions in economic behavior.[2] Indeed, standard models of spatial equilibrium suggest mobile workers and firms will arbitrage the benefits associated with local policies by relocating across the boundaries of targeted areas. Local land prices ought then to rise and offset any welfare gains that might otherwise accrue to prior residents.

We critically examine this conjecture by conducting an evaluation of Round I of the federal urban Empowerment Zone (EZ) program—one of the largest place based policies in the United States. Using rejected and future applicants to the EZ program as controls, we find that EZs generated jobs in targeted communities

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[1] See Bartik (2002) and Glaeser and Gottlieb (2008) for reviews. Nichols and Zeckhauser (1982) provide a useful general discussion of the welfare economics of targeted transfers.

[2] Kain and Persky (1969) provide an early critique of proposals for "gilding the ghetto." Glaeser and Gottlieb (2008, p. 197) exemplify the conventional view, stating that "the rationale for spending federal dollars to try to encourage less advantaged people to stay in economically weak places is itself extremely weak." See Greenstone and Looney (2010) for an opposing view.

and raised local earnings without generating large increases in population or housing rents. Our findings build on an active literature on smaller state level "enterprise zones" which, perhaps because of heterogeneity in methods and programs studied, has found mixed evidence on the effectiveness of these programs at generating jobs.[3] Our estimates also inform the recent literatures on spatial bias in national tax policies (Albouy 2009), local environmental policies (Greenstone and Gallagher 2008), and industrial and regional policies (Wren and Taylor 1999; Criscuolo et al. 2007; Bronzini and de Blasio 2006), the efficiency consequences of which all depend upon the mobility of workers and firms. Our work extends these literatures by conducting the first microfounded equilibrium welfare evaluation of a large scale place based policy using geographically detailed microdata on firms, workers, and commuting patterns.

In an initial contribution, we develop a tractable spatial equilibrium model of Empowerment Zones with landlords, firms, and mobile workers who make labor supply and commuting decisions. The incidence and efficiency of local subsidies are shown to depend critically upon the distribution of agents' preferences over residential and commuting options. If most agents are inframarginal in their commuting and residential decisions, deadweight loss will be small and local workers will reap the benefits of place based interventions. If, on the other hand, agents have nearly identical preferences, as in the classic models of Rosen (1979) and Roback (1982), deadweight loss will be substantial and government expenditures will be capitalized into land rents. We show, using arguments similar to Chetty (2009), that our model allows for simple approximations to the incidence and deadweight loss of EZs via a set of reduced form elasticities quantifying the program's impact on the wages of local zone workers and commuters, the rental rate of zone housing, and the number of zone jobs for local residents and commuters.

Our empirical work centers on estimating these impacts using confidential microdata from the Decennial Census and the Longitudinal Business Database (LBD). These data provide us with two independent sources of information on local employment and allow us to adjust for changes over time in the composition of workers and firms. Crucial to our analysis, the Journey to Work component of the census microdata allows us to separate the impacts of EZ designation on zone workers and zone residents. Because Empowerment Zones usually constitute a small fraction of a city's area, zone residents who work typically do so outside of the zone. Likewise, most zone workers are commuters who live outside the zone. EZs subsidize the employment of workers who live and work in the zone, and involve block grants which may indirectly subsidize commuters, making it critical for us to be able to distinguish between these populations across the period of our study, a task which is infeasible given publicly available data sources.

To identify the causal impacts of EZ designation we construct a set of control zones based upon previously confidential data obtained from the Department of Housing and Urban Development on the census tract composition of rejected and later round Empowerment Zones. Since these tracts were nominated for designation

[3] See Papke (1993, 1994); Boarnet and Bogart (1996); Bondonio (2003); Bondonio and Engberg (2000); and Engberg and Greenbaum (1999). Peters and Fisher (2002) provide a review. More recent studies include Bondonio and Greenbaum (2007); Elvery (2009); Ham et al. (2011); and Neumark and Kolko (2010).

by their local governments, they are likely to share unobserved traits and trends in common with first round EZs which also underwent a local nomination phase. We demonstrate that, after some basic adjustments, the pretreatment levels and trends in these control zones closely mirror those of the EZs. Having demonstrated suitable balance, we assess causal impacts of the EZ program using an adjusted difference in differences estimator. To account for the clustered nature of our data, and the fact that only six EZs were awarded over our sample period, we rely on a wild bootstrap testing procedure studied by Cameron, Gelbach, and Miller (2008) to conduct inference.

Point estimates from our main specifications suggest that neighborhoods receiving EZ designation experienced substantial (12 to 21 percent) increases in total employment relative to observationally equivalent tracts in rejected and future zones. The weekly wages paid to zone residents working inside the zone also appear to have increased significantly (by approximately 8 to 13 percent) relative to controls. Yet despite these improvements in the zone labor market, we find only a small insignificant influx of households to zone neighborhoods. Rental and vacancy rates appear stable over the duration of the study suggesting that most workers consider zone neighborhoods poor substitutes for residence in areas outside of the zone.

To assess whether our results are confounded by citywide shocks, we conduct a variety of robustness checks meant to examine whether our control tracts provide a suitable proxy for the counterfactual behavior of EZs over the 1990s. We construct a set of "placebo" zones in EZ counties with pretreatment characteristics similar to real EZs. We then compute difference in differences impacts comparing these placebo zones to rejected and future control zones, which reassuringly results in small insignificant estimated effects. We also show that our qualitative results remain when tract level outcomes are converted into percentiles in their citywide distribution, indicating that our results are not driven by rank preserving citywide shocks.

We conclude with a quantitative assessment of the program's incidence and a calculation of deadweight costs. Though our estimates are imprecise, we find that EZ designation generated wage increases for workers from zone neighborhoods worth approximately $296 million per year. Based upon two independent estimates of the number of zone jobs created for zone residents, we find that the tax credits associated with designation yielded relatively modest deadweight costs equal to roughly 13 percent of the flow cost of the subsidy, though allowing for the possibility that EZ tax credits shifted workers out of jobs at firms ineligible for the credit and incorporating upper bound estimates of the marginal cost of raising the funds for the subsidy inflates this figure to as much as 48 percent.

The remainder of the paper is structured as follows: Section I provides background on the EZ program, Section II develops a general equilibrium model of EZs, and Section III introduces our empirical strategy, Section IV describes the data used, Section V outlines our main results, Section VI tests for violations of the assumptions underlying our research design, Section VII conducts a welfare analysis, and Section VIII concludes.

I. The Empowerment Zone Program

The federal Empowerment Zone program is a series of spatially targeted tax incentives and block grants designed to encourage economic, physical, and social

TABLE 1—1990 CHARACTERISICS OF FIRST ROUND EMPOWERMENT ZONES (EZ)

City	Total population	Population rank	Population in EZ	Poverty rate in EZ	Unemployment rate in EZ	EZ area (square miles)	Number of census tracts
Atlanta	395,337	37	43,792	58	20	8.1	20
Baltimore	736,014	13	72,725	42	16	7.1	23
Chicago	2,783,484	3	200,182	49	28	14.3	81
Detroit	1,027,974	7	106,273	47	28	19.5	42
New York	7,320,621	1	204,625	42	18	6.3	51
Philadelphia/Camden	1,594,339	5	52,440	50	23	4.3	17

Source: 1990 Decennial Census and HUD.

investment in the neediest urban and rural areas in the United States. In 1993 Congress authorized the Department of Housing and Urban Development (HUD) to award Empowerment Zones to local communities via a competitive application process. Local governments were invited to submit proposals for an EZ defined in terms of 1990 census tracts subject to certain restrictions on the characteristics of each proposed tract.[4]

HUD awarded EZs to six urban communities: Atlanta, Baltimore, Chicago, Detroit, New York City, and Philadelphia/Camden. Two additional cities, Los Angeles and Cleveland, received "supplemental" EZ (SEZ) designation while 49 rejected cities were awarded smaller enterprise communities (ECs) as consolation prizes.[5] Table 1 shows summary statistics of EZ neighborhoods by city. The average Round I EZ spanned 10 square miles, contained 113,340 people, and had a 1990 poverty rate of 48 percent. Most zones are contiguous groupings of census tracts, although some EZs, such as the one in Chicago pictured in Appendix Figure A1, cover multiple disjoint groupings of tracts.

EZ designation brought with it a host of fiscal and procedural benefits, the most important of which are the following:[6]

(i) Employment Tax Credits—Starting in 1994, firms operating in the six original EZs became eligible for a credit of up to 20 percent of the first $15,000 in wages earned in that year by each employee who lived and worked in the community. Tax credits for each such employee were available to a business for as long as ten years, with the maximum annual credit per employee declining over time. This was a substantial subsidy given that, in 1990, the average EZ worker only earned approximately $16,000 in wage and salary income.

[4] All zone tracts were required to have poverty rates above 20 percent. Moreover, 90 percent of zone tracts were required to have poverty rates of at least 25 percent and 50 percent were required to have poverty rates of at least 35 percent. Tract unemployment rates were required to exceed 6.3 percent. The maximum population allowed within a zone was 200,000 or the greater of 50,000 or 10 percent of the population of the most populous city within the nominated area.

[5] ECs were not entitled to tax credits but were allocated $3 million in SSBG funds and made eligible for tax-exempt bond financing. SEZs were awarded block grants similar to those received by EZs but did not become eligible for the EZ tax credit until 1999.

[6] See IRS (2004) for more details. Other benefits appear not to have been heavily utilized. See Hebert et al. (2001), General Accounting Office (2004), and Government Accountability Office (2006).

Table 2—Total Spending

	SSBG	Outside money	Total
Total (in million $)	386	2,848	3,234
Expenditure by category (in million $)			
Access to capital	83	1,483	1,566
Business assistance	56	482	538
Workforce development	48	49	97
Social improvement	76	163	240
Public safety	18	255	272
Physical development	14	82	97
Housing	71	326	397
Capacity improvement	20	7	27
Average annual expenditure (in $)			
Access to capital per firm			20,881
Business assistance per firm			7,172
Workforce development per unemployed			261
Social improvement per housing unit			138
Public safety per person			56
Physical development per poor person			44
Housing per housing unit			229
Capacity improvement per EZ			891,295

Source: Appendix F of Hebert et al. (2001).

(ii) Title XX Social Services Block Grant (SSBG) Funds—Each EZ became eligible for $100 million in SSBG funds. These funds could be used for such purposes as: business assistance, infrastructure investment, physical development, training programs, youth services, promotion of home ownership, and emergency housing assistance.

Evidence from the General Accounting Office (1999) and Hebert et al. (2001) suggests that participation in the tax credit program was incomplete and most common among large firms who were more likely to have positive taxable income. Roughly $200 million in employment credits was claimed over the period 1994–2000, with the amount claimed each year trending up steadily over time. IRS data show that, in the year 2000, close to 500 corporations, and over 5,000 individuals, claimed EZ Employment Credits worth a total of approximately $55 million.[7]

Table 2 summarizes information compiled from HUD's internal performance monitoring system on the amount of money allocated to various program activities by source. By 2000, the first round EZs had spent roughly $400 million dollars in SSBG funds. However, large quantities of outside capital accompanied the grant spending. The six EZs reported allocating roughly $3 billion to local projects by 2000, with more than $7 of outside money accompanying every $1 of SSBG funds.[8]

[7] These figures come from General Accounting Office (2004).

[8] The most commonly reported use of funds was enhancing access to capital. One-stop capital shops were a component of the plans of most EZs, training local entrepreneurs to develop business plans and apply for loans either from local organizations or commercial banks. The second most common use of funds was business development which involved technical and financial assistance. Some EZs developed business incubators for this purpose or invested in the physical revitalization of commercial corridors. See Hebert et al. (2001) and Appendix IV of Government Accountability Office (2006) for detailed descriptions of the projects implemented in particular zones.

Audits by HUD's Office of Inspector General and the Government Accountability Office (2006) have called the accuracy of these data into question, suggesting that they should be interpreted as loose upper bounds on the amount of money raised, particularly since it is difficult to ascertain how any outside funds would have been spent in the absence of the program.[9]

In sum, the six Round I EZs constitute a 60 square mile area containing less than 700,000 residents. Federal expenditures on EZ wage credits and block grants amounted to roughly \$850 per resident over the first six years of the program (1994–2000). And HUD's internal records suggest that as much as \$4,000 per resident of outside investment may also have been leveraged over this period though we suspect this figure to be a substantial overestimate.

II. Model

We turn now to the development of a spatial equilibrium model allowing a welfare analysis of the EZ program. The framework adopted is a variant of the classic equilibrium models of Rosen (1979) and Roback (1982) extended to allow for heterogeneity, labor supply decisions, commuting, elastic housing supply, and imperfect compliance in the EZ wage credit program. The decisions of workers are modeled in a discrete choice framework as in Bayer, Ferreira, and McMillan (2007) with an emphasis on the distinction between place of residence and place of work as in, for example, Baum-Snow (2007). After developing the model, we show that a set of reduced form elasticities of the sort discussed by Chetty (2009) can be used to approximate the EZ program's deadweight loss.

Assume a continuum of agents of measure one and a finite collection $\mathcal{N} = \{\mathcal{N}_0, \mathcal{N}_1\}$ of neighborhoods in which they may live or work consisting of neighborhoods inside ($\mathcal{N}_1$) or outside ($\mathcal{N}_0$) of an Empowerment Zone. Neighborhoods have fixed bundles of amenities consumed by local residents and used by local firms in production. Commuting between neighborhoods is costly. To deal with imperfect compliance with the EZ tax credit we introduce two sectors of the economy: a first sector of covered firms likely to participate in the EZ wage credit program and a second sector of firms likely to be ineligible for (or unaware of) the program. It is useful to think of sector one as consisting of large establishments and sector two as small family run businesses.

Agents choose a neighborhood to live in, whether to work, and (if so) a neighborhood and sector in which to work. Each agent inelastically demands a single unit of housing which they rent at market rates. Write the utility of individual i living in community $j \in \mathcal{N}$ and working in community $k \in \{\emptyset, \mathcal{N}\}$ and sector $s \in \{1, 2\}$ as

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

$$= v_{jks} + \varepsilon_{ijks},$$

[9] See Chouteau (1999) and Wolfe (2003). Hebert et al. (2001, p. 5) report that "most of the leveraged dollars are accounted for by a \$1.2 billion commitment by a lending consortium of Detroit banks."

where w_{jks} is the wage a worker from neighborhood j receives when working in sector s of neighborhood k, r_j is the local rent level, κ_{jk} is the cost associated with commuting to work in location k given residence in j, A_j is the mean consumption value of local amenities, and v_{jks} is the mean utility (across individuals) of each choice. The wage for nonworkers $(w_{\emptyset})$ is the dollar value of leisure which we normalize to zero without loss of generality. We likewise normalize $\kappa_{j\emptyset} = 0$. The individual and choice specific error terms ε_{ijks} represent heterogeneity in the valuation of local amenities, the value of leisure, tastes for work in the two sectors, and commuting costs.[10] The ε_{ijks} are independently and identically distributed across individuals and assumed to possess a continuous multivariate distribution independent of v_{jks}.

Heterogeneity is substantively important as it allows some workers to be inframarginal with respect to their residential and work location choices; thereby creating the potential for economic rents. Traditional models of spatial equilibrium are predicated upon the absence of such rents.[11] A Rosen-Roback type model, for example, would start by specifying that $u_{ijks} = \bar{u}$. Such indifference implies that the incidence of a local subsidy cannot fall on pre-existing residents. Heterogeneity weakens this knife edge result and yields stakeholders capable of differentially benefitting (or suffering) from local policies.

Define a set of indicator variables $\{D_{ijks}\}$ equal to one if and only if $\max_{j'k's'}\{u_{ij'k's'}\} = u_{ijks}$ for worker i, where $j' \in \mathcal{N}$, $k' \in \{\emptyset, \mathcal{N}\}$, and $s' \in \{1, 2\}$. Then the measure of agents in each residential/work location is $N_{jks} = P(D_{ijks} = 1 \mid \{v_{j'k's'}\})$. Denote the average utility of agents as $V = E_{\varepsilon}\left[\max_{j'k's'}\{u_{ij'k's'}\}\right]$ where the expectation operator E_{ε} is defined over the heterogeneity terms $\varepsilon_{ij'k's'}$. The choice probabilities N_{jks} and the average valuation V are easily shown to obey the following relationship:[12]

$$\frac{d}{dv_{jks}} V = N_{jks}, \tag{1}$$

which amounts to a generalization of Roy's Identity for a representative agent with indirect utility function V. This relationship will prove useful in our analysis of social welfare.

We turn now to the demand side of the model. Goods are produced in each neighborhood k and sector s with a constant returns to scale technology $F(K_{ks}, B_k L_{ks}) = B_k L_{ks} f(\chi_{ks})$ where the arguments K_{ks} and L_{ks} refer to total capital and labor inputs respectively, $\chi_{ks} = \frac{K_{ks}}{B_k L_{ks}}$ is the capital to effective labor ratio, and B_k is the local productivity level which may depend upon infrastructure investments, natural

[10] It is useful to allow for the possibility that some zone residents face a higher cost of commuting to work inside the zone than outside the zone as might happen if some residents live on the border of the zone or are located near public transportation more integrated with one neighborhood than another. This will allow some zone workers to prefer working outside the zone even if wages are equalized across all neighborhoods.

[11] See, for example, the traditional urban economics models covered in Glaeser (2008).

[12] Proof:

$$\frac{dV}{dv_{jks}} = E_{\varepsilon}\left[\frac{d}{dv_{jks}} \max_{j'k's'}\{u_{ij'k's'}\}\right] = E_{\varepsilon}\left[I\left[\max_{j'k's'}\{u_{ij'k's'}\} = u_{ijks}\right]\right] = P(D_{ijks} = 1 \mid \{v_{j'k's'}\}) = N_{jks}.$$

We are grateful to David Card for help in simplifying an earlier version of this proof.

features of the physical environment (e.g., access to a body of water, proximity to downtown), and crime levels.[13] Productivity differences across neighborhoods yield unequal derived demands for inputs across space. Because the supply elasticity of workers to any given location is finite in the presence of taste heterogeneity and commuting costs, these unequal factor demands result in unequal wages across neighborhoods.

Workers from different neighborhoods are assumed to be perfect (and homogeneous) substitutes in production so that $L_{ks} = \sum_{j \in \mathcal{N}} L_{jks}$ where L_{jks} is the labor input of workers from neighborhood j to firms in neighborhood k and sector s.[14] The EZ tax credit program induces a cost difference for zone firms between workers residing inside of the zone (whose wages are subsidized at rate τ) and zone commuters who are unsubsidized. Hence at any given wage, zone employers strictly prefer zone residents, which means that at an interior equilibrium zone firms must pay different wages to residents and commuters.

We assume capital is supplied at fixed rental rate ρ to all neighborhoods and sectors and that output is sold on an international market at price one.[15] Our fixed ρ assumption reflects the notion that urban neighborhoods are small in relation to global capital markets and that modern financial institutions, unlike workers, do not exhibit substantial preferences regarding the neighborhoods to which their funds flow. Define the indicator variable $\delta_{jks} = I[\, j \in \mathcal{N}_1, k \in \mathcal{N}_1, s = 1]$ which equals one for jobs subject to the wage subsidy and zero otherwise. Firms equate the marginal product of each factor to its corresponding after-tax cost so that

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

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

The second of these conditions may be inverted to yield $\chi_{k,s} = \chi = h(\rho)$ where $h'(.) \leq 0$. We may then rewrite the condition for wages as

$$w_{jks} = \frac{B_k R(\rho)}{1 - \tau\delta_{jks}}, \tag{2}$$

where $R(\rho) = f\big(h(\rho)\big) - h(\rho)\rho$ is the marginal product of a "raw" unit of labor. The fact that zone and nonzone workers are perfect substitutes implies that the tax subsidy for zone workers will be completely transferred into their wages. Zone jobs in the higher paying sector are not rationed because workers have idiosyncratic tastes for working in different sectors.

Finally, we allow for upward sloping housing supply curves in each neighborhood as in Moretti (2011, 2013) and Notowidigdo (2010). Each neighborhood has

[13] See Kline (2010) for an analysis of this sort of model when B_k exhibits agglomeration effects.

[14] See Card (2009) for recent evidence on the high degree of substitutability between low skilled workers of the sort that work and live in EZ neighborhoods. In the online Appendix we derive an extended version of the model which incorporates productivity differences among workers and show that it yields similar conclusions.

[15] It is straightforward to extend the model to the case where output is sold locally and prices are endogenous. Since we have no data on local product prices we omit this feature from our analysis.

a continuum of risk neutral land owners distributed on the unit interval. Each land owner may develop a unit of housing on her plot of land in neighborhood j at a cost which is continuously distributed across owners according to the CDF $G_j(.)$ with strictly positive support. These costs might include the time cost of rehabilitating a boarded up vacant unit or the pecuniary cost of creating a new structure on an open lot.

If a unit of housing is built, the owner rents the unit out and receives payoff r_j minus the cost of constructing the unit, otherwise she receives nothing. Let H_j represent the number of units rented out in community j. Optimization implies that the marginal landowner in each neighborhood breaks even on house construction so that

$$G_j^{-1}(H_j) = r_j. \tag{3}$$

To close the model we assume the housing market clears which requires

$$H_j = \sum_k \sum_s N_{jks}. \tag{4}$$

The model's predictions for the response of zone neighborhoods to EZ designation are now easily derived. The EZ program involved two treatments—a wage tax credit (τ) and a block grant which we model as affecting local productivity (B_k) and amenity (A_j) levels. From (2) we see that the EZ wage subsidies should raise the wages of local zone workers and hence their employment at EZ firms in the covered sector. Because the tax credits have no effect on wages in the uncovered sector, employment may fall at such firms as workers switch their employment to the more lucrative covered sector. Likewise, because the wage subsidies yield no increase in the wages of nonresident commuters their employment may also be expected to fall slightly as some workers decide to move to the neighborhood to take advantage of the higher wages for residents.

Any productive effect of the block grants however, may counteract these negative employment effects. Note that (2) implies

$$\frac{d \ln w_{jks}}{d \ln B_k} = 1. \tag{5}$$

Thus productivity changes proportionally boost the wages of all workers in a neighborhood regardless of their place of residence. This may be expected to yield a large employment response among nonresident zone commuters who likely view most jobs within a sector with the same commuting distance as close substitutes. It may also counteract any negative employment effects at smaller firms not covered by the tax credit.

Finally, depending on the distribution of workers' tastes for living in zone neighborhoods and features of the housing supply locus, the rental rate of housing in zone neighborhoods may increase as agents seek to move to the zone in order to take advantage of higher local wage levels and any possible increases in local amenity value. If workers have relatively homogeneous residential preferences and the housing stock is fixed, we should see large increases in rental rates, while if housing is easily supplied we should see an increase in population and little change in rental rates. If, however, few workers are on the margin of moving to distressed neighborhoods we should see little response in either population or rental rates.

We turn now to an analysis of the model's welfare implications. Total social welfare in this economy is the sum of total worker utility and the utility of landlords which may now be written compactly as follows:

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

the first term giving the average (which is also the total) utility of workers and the second the total profits of landowners.

Consider first the block grant which we model as affecting local productivity and amenity levels. The marginal social benefit of an improvement in the local productivity level of community m may be written as

$$\frac{d}{dB_m} W \bigg|_{\tau=0} = \sum_j \sum_k \sum_s N_{jks} \left[\frac{dw_{jks}}{dB_m} - \frac{dr_j}{dB_m} \right] + \sum_j \frac{dr_j}{dB_m} H_j, \tag{6}$$

where we have made repeated use of the relationship given in (1). The first line gives the effect of the productivity change on workers and the second line the effect on housing producers.[16] A remarkable feature of this welfare calculation is that it does not include any terms of the form $\frac{dN_{jks}}{dB_m}$. This is a result of optimization which makes the marginal agent indifferent between alternatives despite the fact that the micro-level decision is discrete. Thus, to first order, the welfare implications of zone grants are the same as the implications of changing prices on an immobile population.

In an economy without behavioral responses, price changes simply generate transfers of wealth between market participants, which, in our framework, have no aggregate welfare implications. Substituting the market clearing conditions (3) and (4) into (6) and simplifying yields

$$\begin{aligned} \frac{d}{dB_m} W \bigg|_{\tau=0} &= \sum_j \sum_k \sum_s N_{jks} \frac{dw_{jks}}{dB_m}, \\ &= R(\rho) N_{.m} \end{aligned} \tag{7}$$

where $N_{.m} = \sum_j \sum_s N_{jms}$ is the total number of jobs in neighborhood m and the second line follows from (2). Note that this is simply the total increase in output the economy would experience due to an increase in the local productivity level if the behavior of firms, workers, and landlords were unchanged.

Now consider an increase in amenities. By similar reasoning it can be shown that

$$\frac{d}{dA_m} W \bigg|_{\tau=0} = N_{m.}, \tag{8}$$

[16] Note that in a Rosen-Roback model the welfare consequences of any increases in local wages would be perfectly offset by increases in the local cost of living. By assumption such a model requires $dV = 0$.

where $N_{m.} = \sum_k \sum_s N_{mks}$ is the total number of residents of neighborhood m. Again, the intuition is that, to first order, improving amenities in neighborhood m is equivalent to making an in-kind transfer to an immobile population.

Finally, consider the wage tax credit. A derivation equivalent to that in (6) and (7) yields

$$\frac{d}{d\tau} W = \sum_{j \in \mathcal{N}_1} \sum_{k \in \mathcal{N}_1} N_{jk1} w_{jk1} \frac{d \ln w_{jk1}}{d\tau}. \tag{9}$$

Thus, in contrast to the case of block grants, the total welfare effects of the wage subsidy depend to first order on price changes. This is because of the ad valorem nature of the subsidy which makes the size of the transfer from the federal government to zone employers contingent upon the base wage. So even if no firms or workers move, an increase in the wage will increase the total transfer to the local economy.

The marginal cost of an increase in the ad valorem wage subsidy is

$$\begin{aligned} \frac{d}{d\tau} \sum_{j \in \mathcal{N}_1} \sum_{k \in \mathcal{N}_1} N_{jk1} w_{jk1} \tau &= \sum_{j \in \mathcal{N}_1} \sum_{k \in \mathcal{N}_1} N_{jk1} w_{jk1} \left(1 + \tau \frac{d \ln N_{jk1}}{d\tau} + \tau \frac{d \ln w_{jk1}}{d\tau} \right) \\ &= \sum_{j \in \mathcal{N}_1} \sum_{k \in \mathcal{N}_1} N_{jk1} w_{jk1} \left(\frac{d \ln w_{jk1}}{d\tau} + \tau \frac{d \ln N_{jk1}}{d\tau} \right) \end{aligned}$$

where in the second line we have made use of the fact that (2) implies $\frac{d \ln w_{jk1}}{d\tau} = \frac{1}{1 - \tau \delta_{jk1}}$. The extra term in this expression relative to (9) constitutes the marginal deadweight loss of the wage subsidies; it reflects the fact that marginal entrants have first order effects on program cost even if they value the resulting net wage increases little.

The total deadweight loss of the tax subsidy may be written as

$$\begin{aligned} DWL_\tau &= \sum_{j \in \mathcal{N}_1} \sum_{k \in \mathcal{N}_1} N_{jk1} w_{jk1} \int_0^{d\tau} t \frac{d \ln N_{jk1}}{dt} dt \\ &\approx \frac{1}{2} \psi d\tau^2 \sum_{j \in \mathcal{N}_1} \sum_{k \in \mathcal{N}_1} N_{jk1} w_{jk1}, \end{aligned} \tag{10}$$

where in the second line we have assumed a constant semi-elasticity of local covered employment $\psi = \frac{d \ln N_{jk1}}{d\tau}$. The efficiency cost of the employment credit is proportional to ψ and the local wage bill at zone firms in the covered sector and is increasing in the square of the tax change. This formula corresponds to the standard Harberger (1964) formula for approximating deadweight loss with the number of covered sector jobs in the zone as the "good" being subsidized. It is also analogous to results found in local public finance models of between-city equilibrium (e.g., Albouy 2009) where the local employment elasticity serves as a key input to calculations of the deadweight loss induced by local taxes. A key difference with such papers is that the present elasticity depends critically upon worker heterogeneity which generates different conclusions regarding program incidence.

Note that in the absence of heterogeneity among workers ψ will be large and the employment credits will be "wasted" on workers indifferent about the prospect of switching between neighborhoods, sectors, and labor force states. If, however, few nonzone residents are on the margin of moving to an EZ (as might be the case if EZs are perceived by most to be undesirable locations in which to live) and few EZ residents are on the margin of working (as might be the case if public assistance receipt provides disincentives to work among a large fraction of the local population) then ψ will be small and the deadweight loss of the program will be small.

The block grant investments may yield additional deadweight losses if their total cost C exceeds the value of the resulting amenity and productivity increases. Suppose every dollar of block grants proportionally raises zone neighborhood amenity levels by a factor of λ_a and zone neighborhood productivity levels by λ_b. Then we may approximate the deadweight loss associated with the block grants by assuming marginal welfare effects are constant as follows:

$$(11) \qquad DWL_G \approx C\left[1 - \lambda_a \sum_{j \in \mathcal{N}_1} \frac{dW}{d\ln A_j}\bigg|_{\tau=0} - \lambda_b \sum_{k \in \mathcal{N}_1} \frac{dW}{d\ln B_k}\bigg|_{\tau=0}\right]$$

$$= C\left[1 - \lambda_a \sum_{j \in \mathcal{N}_1} A_j N_{j.} - \lambda_b \sum_{j} \sum_{k \in \mathcal{N}_1} \sum_{s} N_{jks} w_{jks}\right],$$

where the second line follows from (7) and (8). If the block grants are wasted on unproductive investments, as is likely if the funds are mismanaged or mistargeted relative to the needs of local firms, the program's deadweight costs could be substantial. If, however, local public goods are underprovided in zone neighborhoods the social return on these local investments may dramatically exceed their cost.

III. Empirical Strategy

Our theoretical discussion highlights the point that the incidence and efficiency of EZ designation are both empirical questions incapable of being answered on prior grounds. The incidence of the program hinges critically upon the manner in which factor prices change. Wage increases in the zone will benefit workers with a preference for working in the zone while residents who prefer to take leisure will be unaffected. Rent increases will benefit zone landlords but reduce the disposable income of zone residents. Residents outside the zone may also reap some benefit from EZ designation if the productivity of zone jobs rises or rental rates for housing fall in response to any population losses. But the total economy wide gain associated with the program will be small relative to its cost if workers are highly responsive to the wage subsidies or if the block grants are wasted on unproductive investments.

Our empirical tasks, then, are threefold. First, we must identify the impact of EZ designation on local price levels in order to assess the program's incidence. Second, to compute an estimate of deadweight loss due to the program's tax credits, we need to determine ψ which corresponds to the effect of the wage subsidies on the number of covered sector zone jobs for zone residents. Third, we need to isolate the cost effectiveness of the block grants which will require determining the impact of EZ designation on the wages of nonresident zone workers, who according to (5) should experience wage increases in proportion to any productivity increases dB_k. With knowledge of $d\ln B_k$ and information on the cost C of the EZ investments we may in turn identify the productivity effect λ_b of the block grants. Note that without more assumptions the model does not allow point identification of the amenity value λ_a of the block grants from reduced form impacts alone. However, provided housing supply is not perfectly elastic, if the impact on rents of designation is nearly zero we can be assured that λ_a is small as well. We return to this issue again in Section VII.

Our research design for accomplishing these tasks will be to compare the experience over the 1990s of census tracts in Round I EZs to tracts in rejected and later round zones with similar characteristics.[17] This approach has a number of advantages. First, tracts in rejected zones, like those in winning zones, were nominated by their local governments for inclusion in an EZ proposal. If the nomination process was similar in winning and losing cities this ought to yield a set of control tracts with both observable and unobservable characteristics similar to EZs. Second, our control zones consist of contiguous clusters of poor census tracts just like real EZs. If spillovers exist across census tracts or if poor tracts surrounded by other poor tracts have important unmeasured characteristics then such agglomerated controls may be necessary for identifying causal effects. Finally, the majority of rejected and future zones are located in different cities than treated zones which reduces the sensitivity of our estimates to geographic spillover effects.

Though the use of rejected tracts as controls has many advantages, one may still be concerned that the cities that won first round EZs are fundamentally different from losing cities. A cursory inspection of Table 1 indicates that two of the three largest US cities won EZs, while the remaining winners are large manufacturing intensive cities. If large cities experienced fundamentally different conditions over the 1990s than small cities, the comparison of observationally equivalent census tracts in winning and losing zones will be biased.

To further explore this possibility, we carefully examine pre-trends in EZ and control tracts for signs of imbalance after having adjusted carefully for tract and zone characteristics. We also conduct a number of robustness tests aimed at assessing the credibility of our differences-in-differences research design. First, we construct a set of "placebo zones" in treated cities with characteristics similar to real zones. If our research design is confounded by citywide shocks we should find nonzero effects on these placebo zones as well. Second, we examine how the outcomes of EZ tracts change in the citywide distribution of tract level outcomes relative to controls. This approach, which is a nonparametric variant of the traditional differences-in-differences-in-differences (DDD) research design, is robust to arbitrary rank preserving city specific shocks.

[17] Boarnet and Bogart (1996) take a similar approach in their evaluation of the New Jersey enterprise zone. Use of rejected applicants as controls has a long history in the literature on econometric evaluation of employment and training programs. See the monograph by Bell et al. (1995) for a review.

A. *Econometric Methods*

In our comparison of EZ neighborhoods to tracts in rejected and future zones we will rely on simple generalizations of standard differences-in-differences estimators. Specifically, we estimate program impacts using tract level regressions of the form:

$$\Delta Y_{tzc} = \beta T_z + \mathbf{X}'_{n(t)} \boldsymbol{\alpha}^x + \mathbf{P}'_c \boldsymbol{\alpha}^p + e_{tzc}, \tag{12}$$

where ΔY_{tzc} is the change in some outcome (e.g., log population) over the 1990s in census tract t of proposed zone z in city c, T_z is an indicator for whether proposed zone z receives an EZ in 1994, $\mathbf{P}_c$ is a vector of mean city-level characteristics, and $\mathbf{X}_{n(t)}$ is a vector of distance weighted averages of tract level proxies for trends in local productivity and amenities within a given radius-based neighborhood $n(t)$ of tract t. The coefficient β provides an adjusted difference in difference estimate of the impact of the EZ program on EZ tracts.

To allow for flexible patterns of treatment effect heterogeneity, we also estimate interacted regressions of the form:

$$\Delta Y_{tzc} = \mu^1 T_z + (1 - T_z) \times \mathbf{X}'_{n(t)} \boldsymbol{\alpha}^x + (1 - T_z) \times \mathbf{P}'_c \boldsymbol{\alpha}^p + e_{tzc}, \tag{13}$$

where $\mu^1 \equiv E[\Delta Y_{tzc} | T_z = 1]$. This specification models the mean change in outcomes among the control tracts as a linear function of $\mathbf{X}_{n(t)}$ and $\mathbf{P}_c$, but is agnostic regarding the conditional expectation function among the treated tracts. That is, least squares estimation of (13) simply yields the mean $(\hat{\mu}^1)$ among the treated tracts and the coefficients $(\hat{\boldsymbol{\alpha}}^x, \hat{\boldsymbol{\alpha}}^p)$ associated with a linear regression of ΔY_{tzc} on the elements of $(\mathbf{X}_{n(t)}, \mathbf{P}_c)$ in the control $(T_z = 0)$ sample. Given these estimates, an estimate $\widehat{ATT}$ of the average treatment effect on treated tracts may be formed as

$$\widehat{ATT} \equiv \hat{\mu}^1 - \frac{1}{N_1} \sum_t T_t (\mathbf{X}'_{n(t)} \hat{\boldsymbol{\alpha}}^x + \mathbf{P}'_c \hat{\boldsymbol{\alpha}}^p), \tag{14}$$

where T_t is a tract level indicator for whether tract t is in a treated zone, and $N_1 = \sum_t T_t$ is the number of treated tracts.[18] Note that $\widehat{ATT}$ is simply the average forecast error in the treated sample of a regression model fit to the controls. Provided that the linear model for the controls is suitable, this approach will identify the average impact of EZ designation on EZ tracts in the presence of treatment effect heterogeneity arbitrarily dependent upon the covariates. The cost of this additional flexibility is that this estimator will tend to exhibit greater sampling variability than OLS estimation of (12) which assumes common regression coefficients in the treatment and control samples.

Kline (2011) shows that the estimator in (14) possesses a dual interpretation as a propensity score reweighting estimator with weights derived from a log-logistic propensity score model, leading us to term this approach a Parametric Reweighting (PW) specification. We use the implicit propensity score weights associated with

[18] See Appendix A for details.

this estimator in the next section to assess the extent to which our regression model is able to balance the distribution of covariates across the EZ and control samples over time. As described in Appendix A, these PW weights have the appealing property of exactly balancing the mean of any covariate included in the regression model across the treatment and control groups.

Throughout our analysis, we allow for arbitrary within city spatial correlation in the errors e_{tzc} when conducting inference. Because we have only six treated zones, standard cluster-robust variance estimation methods relying upon first order asymptotics may yield poor control over the probability of making type I errors. To deal with this problem we use a clustered wild bootstrap-*t* procedure explored in Cameron, Gelbach, and Miller (2008) which, under some conditions (Mammen 1993; Kline and Santos 2012), may yield improvements in the performance of cluster-robust methods in small samples. In the online Appendix to this paper, we report the results of a Monte Carlo study demonstrating that this procedure effectively controls the size of Wald tests in a variety of data generating processes mimicking the design of our data.

IV. Data

Our analysis relies upon confidential household and establishment level microdata from the Decennial Census, the Standard Statistical Establishment List (SSEL), and the Longitudinal Business Database (LBD) which we use to construct a panel of census tract level outcomes and covariates. The bulk of our data come from the 1980, 1990, and 2000 long-form Decennial Censuses of Population and Housing (US Census Bureau 1980, 1990a, 2000a). Geographic identifiers on the 1980 and 2000 files use codes pertaining to the census geographic boundaries of their vintage. We map block of residence and block of work identifiers in 2000 to 1990 census tracts using the Census Block Relationship Files (CBRF) (US Census Bureau 2000b). To map 1980 geographic identifiers to 1990 tracts we use the Census Tract Relationship Files (CTRF) (US Census Bureau 1990b). We then compute quantities of interest in each year by 1990 tract of residence and tract of work, a process which relies critically upon the Journey to Work component of the Decennial Census microdata which distinguishes between place of work and place of residence. All quantities are computed using census sampling weights and, in the case of 1980 variables, weights accounting for the imperfect correspondence between 1980 and 1990 geographies. We also adjust for nonresponse using an additional set of inverse probability weights described in Appendix B.

To supplement our census analysis, we use establishment data from the Longitudinal Business Database (LBD) files for the years 1987–2002 (US Census Bureau 1987–2002a). The LBD provides longitudinally linked establishment-level data for all establishments with paid employees contained in the Census Bureau's Standard Statistical Establishment List (SSEL) (US Census Bureau 1987–2002b). Data contained on these files comes primarily from the Economic Census and is supplemented with tax records from the Internal Revenue Service. We coded each establishment to a 1990 census tract using an algorithm described in Appendix B based on the raw street addresses provided on the SSEL. In addition to establishments' locations, we observe each establishment's age, size (number of employees), payroll, industry, and whether the establishment belongs to a multi-establishment firm. Average characteristics in each tract are computed adjusting for an estimated probability of being missed by the geocoding algorithm. Because the quality of the LBD data is higher in Economic Census years, we use 1992 instead of 1990 as the base year when examining changes over the 1990s in LBD based variables.

City level covariates are obtained from the County/City Databook (CCD) for the years 1980 and 1990 (US Census Bureau 1988, 1994). This yields values of city level variables such as crime rate, percentage of workers in the manufacturing sector, and percentage of workers working in the government. In cases where zones span multiple cities, we assign all tracts the characteristics of the largest city in the metropolitan area. We also use metropolitan housing price data from the Office of Federal Housing Enterprise Oversight (OFHEO) to control for changes in metropolitan housing market conditions in the early 1990s (Federal Housing Finance Agency 2012).

Finally, in order to construct a suitable control group for EZs, we obtained 73 of the 78 first round EZ applications submitted to HUD by nominating jurisdictions via a Freedom of Information Act request. These applications contain the tract composition of rejected zones which we merged with publicly available data on the tract composition of future zones to create a composite set of controls for use in our analysis. Appendix Table A1 details the composition of the cities in our evaluation sample, whether they applied for a Round I EZ, and the treatments (if any) they received.

A. *Prices/Composition Adjustments*

Given well known problems with the measurement of hours worked in the census (e.g., Baum-Snow and Neal 2009), we work with a weekly wage concept. Wages are computed by dividing annual labor income by weeks worked in the previous year. We exclude from our analysis wage observations based on allocated earnings or weeks. Owner occupied housing values and rents are self-reported in the census as interval valued variables. We assign each response to its interval midpoint and drop allocated values.

To remove the influence of changes in demographic composition on tract level measures of behavior and prices we compute composition constant outcomes by tract for wages, housing values, and rents using fixed effects regressions. The regression specifications used to adjust tract outcomes differ slightly for individual level outcomes aggregated by residence tract, for individual outcomes aggregated by place of work tract, and for housing characteristics.[19]

[19] In each case, a regression model was estimated on a pooled sample of micro-data that included all observations with nonmissing values of the dependent variable from 1980, 1990, and 2000. Each regression specification included a full vector of tract-year dummy variables. For individual level outcomes aggregated by residence tract and for housing characteristics, the tract-year dummy variables indicate an individual's residence tract or the tract in which a housing structure was located. For individual level outcomes aggregated by place of work tract, tract-year dummy variables indicate the tract in which an individual worked. For individual outcomes, the regression specifications included a quartic in age; dummy variables for black, non-Hispanic, and other race (white non-Hispanic omitted); a dummy variable for female; and dummy variables for high school dropout, any past college attendance, and actively enrolled in school (non-enrolled high school graduate omitted). For housing outcomes, we included dummy variables for the number of bedrooms, the number of rooms, three building age categories, two-way interaction terms between bedrooms and rooms, and two-way interaction terms between bedrooms and building age. We computed composition constant mean outcomes by evaluating the estimated regression equation using a constant mix of included explanatory variables for each tract across the three years.

Consider the adjustment of the mean of an outcome Y_{ijzt} which, in an abuse of notation, we take to denote the outcome of individual or housing unit i in tract j, zone z, and year t. A zone is either an EZ, a control zone, or the non-EZ, non-control portion of a county containing an EZ or control. We estimated the following regression equations separately by state on a pooled sample of individual respondents to the 1980, 1990, and 2000 long form Decennial Censuses:[20]

$$Y_{ijzt} = \eta_{jt}^{0} + \mathbf{X}'_{ijzt}\boldsymbol{\eta}_{z}^{x} + \epsilon_{ijzt},$$

where $\mathbf{X}_{ijzt}$ is a vector of covariates for individual or housing unit i in tract j, zone z, and year t. Note that the mean OLS residual is zero for each tract-year because of the included tract-year fixed effects η_{jt}^{0}. Hence we may decompose the change in the tract level mean $\overline{Y}_{j,t}$ between 1990 and 2000 into a composition constant change and a composition effect as follows:

$$\overline{Y}_{j,2000} - \overline{Y}_{j,1990} = \underbrace{(\hat{\eta}_{j,2000}^{0} - \hat{\eta}_{j,1990}^{0})}_{\text{composition constant change}} + \underbrace{(\overline{\mathbf{X}}_{j,2000} - \overline{\mathbf{X}}_{j,1990})'\hat{\boldsymbol{\eta}}_{z}^{x}}_{\text{composition effect}},$$

where the $\overline{\mathbf{X}}_{j,t}$ refer to tract by year averages of covariate values. The composition constant change $(\hat{\eta}_{j,2000}^{0} - \hat{\eta}_{j,1990}^{0})$ is the difference between the two estimated tract-year fixed effects while the composition effect $(\overline{\mathbf{X}}_{j,2000} - \overline{\mathbf{X}}_{j,1990})'\hat{\boldsymbol{\eta}}_{z}^{x}$ is a linear combination of the changes in mean tract characteristics. Columns labeled "Composition Adjusted" report results using the former quantity as a dependent variable.[21]

B. *Estimation Sample/Comparability of EZs and Controls*

Our analysis focuses on the six original EZs which received both tax credits and block grants and restricts the sample of controls to zones containing at least ten census tracts in cities with population greater than 100,000.[22] We also drop all control tracts with 1990 poverty and unemployment rates below the minimum thresholds specified in the EZ eligibility criteria and tracts with fewer than 200 households or 500 residents in 1990.[23] This yields a baseline estimation sample of 234 EZ tracts in six cities and 1,429 controls distributed across sixty three cities.

Tables 3A and 3B provide average characteristics of EZ and control tracts in 1990 along with changes in these characteristics over the period 1980–1990. To summarize this information, and to reduce multiple testing problems, we also include six indices of neighborhood quality that are linear combinations of the underlying

[20] We have also experimented with more complicated specifications that allow the η_{z}^{x} coefficients to change over time by demographic group. These yield similar final results but sometimes erratic predictions for small demographic cells.

[21] Results using the latter component as a dependent variable can be obtained as the difference between the unadjusted and composition adjusted impacts. Bootstrapped p-values for these impacts are available from the authors upon request.

[22] Census tracts in the two SEZs are dropped from our baseline analysis because they were not eligible for wage tax credits during our sample period. We also drop the Washington, DC Enterprise Zone (EnZ) from our sample because it received a wage tax credit but not block grants and hence cannot be properly characterized as an EZ or a control.

[23] Zone tracts were required to have poverty rates in excess of 20 percent and unemployment rates in excess of 6.3 percent as measured in the 1990 census.

Table 3A—Pre-Treatment Sample Means
(*Levels in 1990/1992*[a])

	EZs (1)	"Rejected/ future zones" (2)	"Rejected/ future zones reweighted" (3)	p-value of difference between (1) and (2) (4)	p-value of difference between (1) and (3) (5)
Census tracts characteristics					
Economic index (residents)	0.000	0.581	0.083	0.004	0.586
Employment rate	0.366	0.438	0.372	0.003	0.643
Unemployment rate	0.241	0.182	0.229	0.066	0.489
Poverty rate	0.480	0.424	0.471	0.077	0.699
Economic index (workers–JTW)	0.000	−0.142	−0.232	0.539	0.125
log (jobs)–JTW	6.577	6.966	6.682	0.100	0.361
log (weekly wage of zone workers)–JTW	5.963	5.893	5.928	0.014	0.139
log (weekly wage of zone residents)	5.555	5.456	5.444	0.139	0.038
Economic index (Workers–LBD)[a]	0.000	0.310	0.081	0.194	0.527
log (jobs)–LBD[a]	5.774	6.340	5.992	0.165	0.281
log (establishments)–LBD[a]	3.106	3.559	3.204	0.006	0.287
log (average earnings per worker)–LBD[a]	2.968	2.951	2.954	0.669	0.812
Demographic index	0.000	0.582	−0.070	0.001	0.430
Percent households female-headed	0.567	0.516	0.576	0.008	0.308
Percent college	0.067	0.077	0.059	0.372	0.186
Percent high school dropouts	0.316	0.275	0.313	0.004	0.748
Percent black	0.739	0.610	0.757	0.015	0.362
Percent Hispanic	0.180	0.163	0.171	0.832	0.496
Population index	0.000	−0.125	0.067	0.672	0.707
log (population)	7.773	7.887	7.832	0.365	0.491
log (households)	6.923	6.996	6.923	0.593	0.997
Percent same house as five years ago	0.573	0.509	0.579	0.051	0.745
Housing index	0.000	0.175	−0.134	0.559	0.691
log (rent)	5.350	5.370	5.295	0.831	0.565
log (housing value)	10.490	10.566	10.310	0.750	0.448
Percent houses that are vacant	0.166	0.143	0.146	0.371	0.415
City characteristics					
Total crime/population × 100	0.099	0.105	0.099	0.710	1.000
Average across tracts percent black	0.478	0.343	0.478	0.045	1.000
Percent workers in manufacturing	0.156	0.156	0.156	0.986	1.000
Percent workers in city government	0.065	0.045	0.065	0.389	1.000
log (city population)	14.533	13.056	13.757	0.001	0.000
Observations (number of census tracts)	234	1,429	1,429	1,663	1,663

Notes: Indices are linear combination of the covariates listed below them, see Section IV for details. Column 1 reports sample means for census tracts inside EZs. Column 2 shows means for control tracts in rejected or future treated areas (listed in Table A1). Column 3 reports means for control tracts after parametric reweighting (see Section III for details.) Column 4 presents wild bootstrap p-values for a test of the null hypothesis that the mean in column 1 equals the mean in column 2. Similarly, column 5 reports p-values for the equality of means in columns 1 and 3.

[a] For LBD variables, columns 1, 2, and 3 show the levels in 1992.

Sources: Variables marked as JTW are based on the Journey-to-Work component of the Decennial Census. Variables marked as LBD come from the Longitudinal Business Database. All other tract level covariates come from the census. City covariates are from the County/City Databook.

Table 3B—Pre-Treatment Sample Means
(*Changes 1980–1990/1987–1992*[a])

	EZs (1)	"Rejected/ future zones" (2)	"Rejected/ future zones reweighted" (3)	p-value of difference between (1) and (2) (4)	p-value of difference between (1) and (3) (5)
Census tracts characteristics					
Economic index (residents)	0.000	−0.170	0.028	0.324	0.689
Employment rate	0.009	−0.013	0.001	0.098	0.066
Unemployment rate	0.048	0.042	0.045	0.693	0.680
Poverty rate	0.042	0.061	0.025	0.298	0.122
Economic index (workers–JTW)	0.000	−0.011	−0.047	0.973	0.828
log (jobs)–JTW	−0.199	−0.124	−0.181	0.448	0.857
log (weekly wage of zone workers)–JTW	0.531	0.560	0.555	0.072	0.104
log (weekly wage of zone residents)	0.535	0.470	0.481	0.489	0.384
Economic index (workers–LBD)[a]	0.000	−0.129	−0.128	0.138	0.148
log (jobs)–LBD[a]	−0.089	−0.093	−0.102	0.916	0.672
log (establishments)–LBD[a]	−0.076	−0.081	−0.079	0.800	0.918
log (average earnings per worker)–LBD[a]	0.243	0.174	0.177	0.038	0.133
Demographic index	0.000	−0.169	−0.101	0.067	0.126
Percent households female-headed	0.062	0.066	0.071	0.344	0.094
Percent college	0.026	0.014	0.016	0.021	0.068
Percent high school dropouts	0.025	0.019	0.029	0.297	0.576
Percent black	0.025	0.035	0.015	0.476	0.127
Percent Hispanic	0.018	0.023	0.020	0.659	0.805
Population index	0.000	0.298	−0.037	0.188	0.719
log (population)	−0.209	−0.117	−0.206	0.300	0.937
log (households)	−0.175	−0.110	−0.184	0.123	0.746
Percent same house as five years ago	−0.022	−0.028	−0.029	0.647	0.689
Housing index	0.000	−0.006	0.163	0.979	0.311
log (rent)	0.600	0.608	0.622	0.883	0.527
log (housing value)	0.653	0.600	0.668	0.697	0.800
Percent houses that are vacant	0.037	0.037	0.020	0.997	0.581
City characteristics					
Total crime/population $\times$ 100	0.009	0.013	0.014	0.568	0.413
Average across tracts percent black	0.060	0.052	0.066	0.773	0.410
Percent workers in manufacturing	−0.070	−0.061	−0.070	0.255	0.968
Percent workers in city government	0.022	−0.003	−0.002	0.245	0.470
log (city population)	−0.064	−0.014	−0.064	0.200	1.000
Observations (number of census tracts)	234	1,429	1,429	1,663	1,663

Notes: Indices are linear combination of the covariates listed below them, see Section IV for details. Column 1 shows the mean change between 1980 and 1990 in EZs, columns 2 and 3 report the change and the reweighted change in control areas. Columns 4 and 5 show the bootstrap p-values of the difference between columns 2 and 1 and between columns 3 and 1, respectively.

[a] For LBD variables, columns 1, 2, and 3 present the change between 1987 and 1992.

Sources: Variables marked as JTW are based on the Journey-to-Work component of the Decennial Census. Variables marked as LBD come from the Longitudinal Business Database. All other tract level covariates come from the census. City covariates are from the County/City Databook.

variables scaled by their standard deviations.[24] These indices are normalized to have mean zero and standard deviation one in the EZ sample in 1990.

Column 4 of Tables 3A and 3B provide cluster robust wild bootstrapped p-values for tests of the null hypothesis that mean pretreatment levels and trends are equal across the EZ and control samples. While the residents of rejected and future zones are poor and have high rates of unemployment, we see from columns 1, 2, and 4 that they are not quite as poor or detached from the labor force as residents of EZ areas. In general, EZ tracts appear to be more distressed than controls. Moreover, Table 3B indicates that while trends over the 1980–1990 period are similar between the EZs and controls, some minor differences are present. For example, trends in college share and our two measures of worker wages are slightly imbalanced, although no systematic pattern is apparent from these trend differences.

To deal with these imbalances, we rely on our parametric regression adjustments to control for a wide array of predesignation tract and zone characteristics.[25] All tract level covariates used in our regression adjustments save for central business district status are averaged across tracts using a spatial kernel method.[26] Because some of these covariates are lagged values of outcomes we wish to investigate via regression based methods, we construct our kernel weighted spatial averages $\mathbf{X}_{n(t)}$ omitting the actual tract level outcome $\mathbf{X}_t$ in order reduce the threat of division bias (Borjas 1980) in our later results.

The third column of Tables 3A and 3B use the regression based weights, described in Kline (2011) and in Appendix A, to reweight the controls to mimic the covariate distribution of the treated observations using the same covariates. After reweighting, both pretreatment levels and trends in tract characteristics exhibit dramatically improved balance despite the fact that the majority of these variables were not included in the reweighting procedure.

[24] The indices are sums of the form $\frac{1}{L}\sum_{l=1}^{L}\frac{\overline{X}^{l}_{j,t}}{\sigma^{l}_{90}}$, where σ^{l}_{90} is the cross-sectional standard deviation of the covariate l in question in 1990 among the EZ tracts and the tract covariate average $\overline{X}^{l}_{j,t}$ has been multiplied by -1 where appropriate so as to make the sum an index of neighborhood quality. The following variables were multiplied by -1 in constructing the indices: unemployment rate, poverty rate, percent of households female headed, percent dropout, percent black, percent Hispanic, vacancy rate.

[25] City and Zone Level Covariates: Change in log of city population 1980–1990, Change in city employment rate 1980–1990, Proportion of city population black (1990), Total city crime/population $\times$ 100 (1990), Proportion of city employment in manufacturing (1990), Proportion of city employment in city government (1990), log area in square miles of zone, log OFHEO metropolitan housing price index (1991 and 1992 values).

Tract Level Covariates: Indicator for tract in central business district (1990), Indicator Tract Poverty $>$ 25 percent (1990), Indicator Tract Poverty $>$ 35 percent (1990), Unemployment rate (1990), Employment to population ratio (1990), Fraction of 1980 adults still present in tract in 1990, Change in proportion of employed tract residents commuting $<$ 25 minutes (1980–1990), Change in proportion of tract workers with college degree (1980–1990), Proportion Hispanic (1990), Proportion Hispanic (1980), Proportion black (1990), Proportion black (1980), Proportion of structures vacant (1990), Proportion of structures vacant (1980), Mean building age (1990), Proportion $<$ 18 years old (1990), Proportion $<$ 18 years old (1980), Proportion of households female headed (1990), Proportion of households female headed (1980), Proportion $\geq$ 65 years old (1990), Proportion $\geq$ 65 years old (1980), Proportion of population who are high school dropouts (1990), Proportion of population who are high school dropouts (1980), Change in log of number of jobs (1990–1980), Change in mean log of housing values (1980–1990), Change in mean log of rent (1980–1990), Change in log of tract population (1980–1990), Change in log of households (1980–1990), Change in mean log wage of tract residents (1980–1990), Change in mean log wage of tract workers (1980–1990), Change in mean log annual earnings of tract residents (1980–1990), Change in mean log annual earnings of tract workers (1980–1990), Change in log of tract employment–LBD (1987–1992), Change in log of average earnings per tract worker–LBD (1987–1992), Change in log # of establishments–LBD (1987–1992)

[26] Specifically, for each control variable, the spatial moving average assigned to a tract, j, is the kernel weighted mean value of the control variable among a set of neighboring tracts $N(j)$, defined as those tracts (other than j itself) whose centroid falls within one mile of the centroid of tract j. The weight given to each tract in the set $N(j)$ is given by a truncated (at one mile) normal kernel with a standard deviation of 0.5 miles applied to the distance between the centroid of the neighboring tract and the centroid of tract j.

For example, reweighting moves the 1990 mean among control tracts of each element (the employment rate, the poverty rate, and unemployment rate) of our resident economic index closer to its corresponding 1990 mean among EZ tracts. Means of city level variables included in the regression model match exactly. Column 5 of Table 3A provides cluster robust wild bootstrapped p-values indicating that we cannot reject the null hypothesis that the mean pretreatment tract characteristics are identical after reweighting. The only serious pretreatment discrepancy is in 1990 city population, which is to be expected because EZs were awarded to many of the largest US cities. We revisit the importance of this discrepancy in Section VI. In Appendix Table A2 we document that second moments are also well balanced.

Reweighting yields similar improvements in pretreatment trends. For instance, mean 1980–1990 changes among control tracts in the elements of the worker economic index (jobs, weekly wages of zone residents, and weekly wages of zone workers) all move closer to their corresponding 1980–1990 mean change among EZ tracts. Column 5 of Table 3B shows that pretreatment trends among control tracts are in general statistically indistinguishable from those among EZs after reweighting.

To illustrate these findings visually, Figure 1 shows the mean behavior of our six indices in the EZ and control tracts before and after reweighting across the three decades in our sample. After reweighting is applied to the pooled set of controls, their history over the past two decades mirrors that of actual Empowerment Zones remarkably well. One can see evidence from these graphs of important post-treatment impacts of EZ designation on several dimensions of neighborhood quality including our indices of the economic opportunity of zone workers and zone residents and our housing market index. Plots for some individual census based variables are provided in Appendix Figure A2.

Figure 2 provides complementary evidence from the Longitudinal Business Database at annual frequencies. We see that, after reweighting, treated and control tracts exhibit similar patterns in economic activity prior to the start of the program in 1994. Notably, there is no evidence of an Ashenfelter (1978) style dip prior to program enactment. By 1997, LBD based measures of employment and the number of establishments begin to rise and continue to diverge. This timing is consistent with administrative data documenting a delay of several years in the usage of program benefits by firms.

V. Results

We turn now to our baseline differences-in-differences estimates of the impact of EZ designation. To deal with the hierarchical nature of our data we report standard errors clustered at the city level. As noted earlier, with only six treated clusters, these standard errors may give a misleading impression when used for testing in conjunction with the usual critical values based upon a normal approximation. To circumvent this problem, we use wild bootstrapped p-values to test the null hypothesis of no treatment effect as suggested by Cameron, Gelbach, and Miller (2008).[27] Stars in the tables, indicating significance levels, are based on these p-values. Unsurprisingly, the bootstrapped p-values tend to be substantially

[27] See the online Appendix for details.

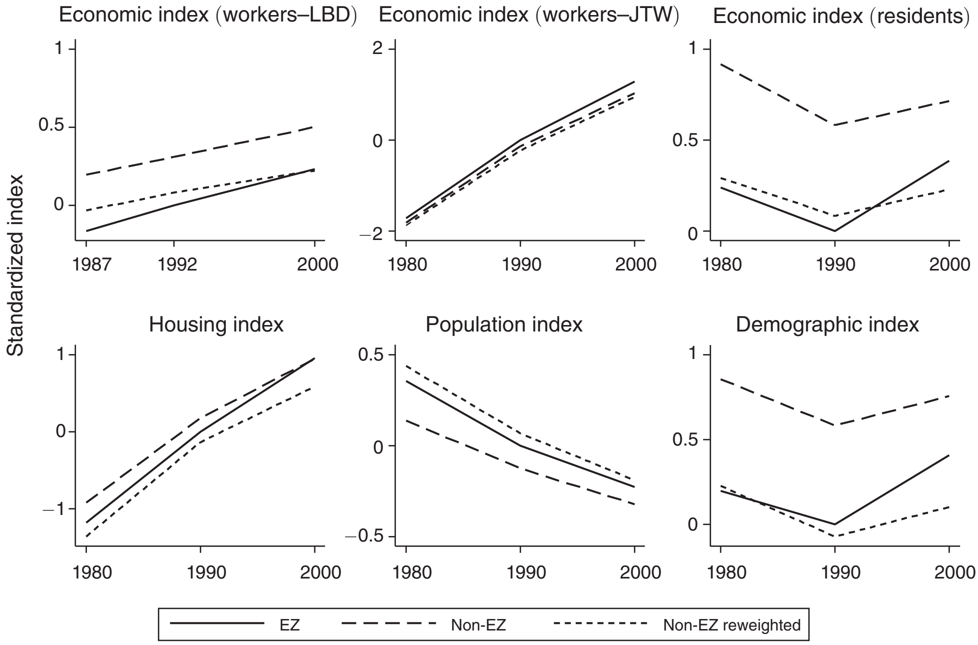

FIGURE 1. MEANS BY YEAR AND TREATMENT STATUS

Notes: Figure depicts means of the listed variables in EZ tracts and controls. Reweighted lines correspond to weighted means using implicit propensity score weights described in Section III and Appendix A. Indices are linear combinations of thematically similar tract characteristics. See Section IV for details. Components of Economic index (residents): employment rate, unemployment rate, and poverty rate; Economic index (workers–JTW): log(jobs, JTW), log(hourly wage of zone workers–JTW), and log(hourly wage of zone residents–JTW); Demographic index: percent households female headed, percent college, percent dropout, percent black, and percent Hispanic; Population index: log(population), log(households), percent same house as five years ago; Housing index: log(rent), log(housing value), percent of houses that are vacant.

larger than would be obtained with the usual normal approximation. Because the empirical bootstrap distributions of our test statistics differ substantially across estimators and outcomes, our analytical standard errors and *p*-values occasionally move in opposite directions across specifications.

Table 4 presents estimates of the impact of EZ designation on economic activity in EZ neighborhoods as measured in the LBD. As mentioned earlier, the LBD estimates compare EZ and control tracts over the interval 1992–2000 because an economic census was conducted in 1992. Column 1 reports simple differences-in-differences estimates which yield large (12.2 percent) positive effects on the number of jobs, modest insignificant increases in the number of establishments, and small insignificant decreases in average earnings per worker. Column 2 shows that after adjusting the differences-in-differences estimates for covariate imbalance via OLS the estimated impact on jobs jumps to nearly 18 percent, while the impact on establishments achieves statistical significance. Column 3 gives the results of our regression based reweighting estimator which yields even larger jobs impacts. We also detect in these specifications larger increases in the number of establishments. The general tendency for covariate adjustment to increase the point estimates suggests that EZs may have been awarded to economically declining neighborhoods.

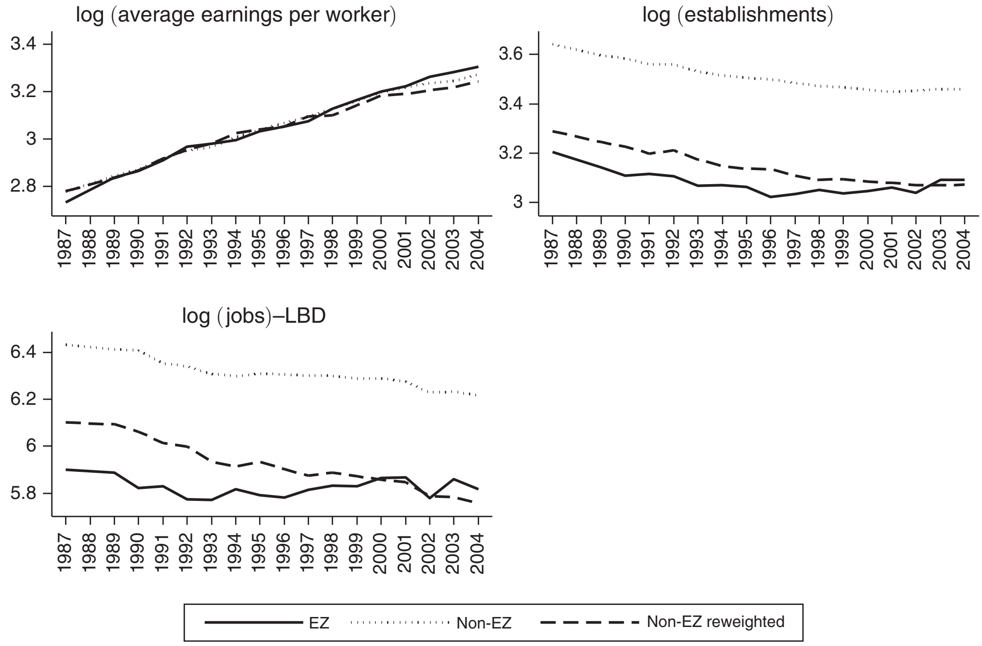

Figure 2. Jobs, Wages, and Establishments (LBD)

Notes: Figure depicts means of the listed LBD variables in EZ tracts and controls. Reweighted lines correspond to weighted means using implicit propensity score weights described in Section III and Appendix A.

The second panel of Table 4 computes impacts on firms located in the zone in 1992. This attenuates the estimated job impacts suggesting that some of the overall employment impact is due to firm births. The negative impacts on the number of establishments in this restricted sample indicate that designation may have also increased firm death rates.[28] The bottom two panels of the table break impacts down by 1992 establishment size. Though the estimates are quite noisy, we find that employment increased only at establishments that were already large in the 1992 Economic Census. These findings are consistent with the survey evidence in Hebert et al. (2001) that large firms were more likely to take advantage of the tax credits and suggest an important role for this feature of the program. We also see some evidence that EZ designation is associated with employment reductions and elevated death hazards among small firms, though these estimates are not statistically significant.

Table 5 provides estimates of the number of jobs created based upon the Journey to Work component of the Decennial Census. The estimated impacts lie in the range 12–19 percent, which is reassuringly similar to the range of estimates obtained from the LBD. By crossing census questions on place of work with place of residence we can determine who occupied any jobs that were created. The second panel of Table 5 reports the results of this exercise. Though all specifications find that the largest employment increases in the zone occurred among zone residents, the magnitude and precision of the results vary with the specification used. Parametric reweighting

[28] The net impacts in the first panel suggest the effect on births is larger than the corresponding effect on deaths.

Table 4—Wage and Jobs Impacts
(*Longitudinal Business Database–LBD*)

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

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.

estimation yields an estimated impact of approximately 18 percent that is borderline significant. Impacts on the employment of nonresident commuters are insignificant but are estimated to be substantial, suggesting the wage credits may not be the only source of increased labor demand in the zones.

Table 6 provides estimates of the impact of EZ designation on the log weekly wages of individuals broken down by place of residence and place of work. To remove the influence of changes in neighborhood composition over time we also report results where the wages have been regression adjusted for individual characteristics at the micro-level via the procedure described in Section IV.

Table 5—Employment Impacts
(*Census, Journey-to-Work–JTW*)

	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.

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

Though all of our point estimates suggest modest wage increases for zone residents, we lack the power to reject the null hypothesis of no effect except in our OLS specification. Adjusting for individual characteristics has little effect other than to slightly increase precision. No detectable wage effects are present for zone workers as a whole. However because only roughly 10 percent of zone workers are zone residents, it is important to further disaggregate these estimates.

The second panel of Table 6 provides wage impacts broken down jointly by place of residence and place of work. Here we find large (8 to 13 percent) wage increases among zone residents who work in the zone, with covariate adjustments leading to larger point estimates. Accounting for composition leads these estimates to rise slightly, typically by less than a percentage point. We also find in some specifications that the wages of resident commuters increased which may reflect spillovers in the demand for labor across zone boundaries. Nonresident commuters exhibit no statistically perceptible wage increase suggesting, in conjunction with the jobs increases for commuters, that the elasticity of supply of commuter labor to the zone is very large. As pointed out in our welfare analysis however, our confidence intervals include economically substantial wage effects, which would suggest a smaller supply elasticity of commuter labor to the zone.

The third panel of Table 6 reports estimated impacts on annual earnings broken down by place of residence and place of work. The qualitative pattern of results is similar to that found for weekly wages though the point estimates are somewhat larger because of small responses in annual weeks worked.[29]

[29] In unreported results, we also investigated impacts on household earned income and household public assistance income. We found no effect on household public assistance income and a positive effect on household earned income similar to the effect on the annual earnings of residents.

Table 6—Wage Impacts
(*Census, Journey-to-Work–JTW*)

	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	0.047	0.040	0.026	0.053	0.050
	(0.035)	(0.021)	(0.037)	(0.032)	(0.015)**	(0.033)
log (weekly wage income of zone workers)	−0.010	0.011	0.003	0.001	0.017	0.010
	(0.026)	(0.030)	(0.031)	(0.024)	(0.026)	(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	0.127	0.112	0.088	0.133	0.121
	(0.045)	(0.041)**	(0.055)*	(0.046)	(0.051)**	(0.051)**
log (weekly wage income of nonresidents working in zone)	−0.014	−0.015	−0.010	0.006	0.005	0.006
	(0.029)	(0.033)	(0.035)	(0.023)	(0.027)	(0.030)
log (weekly wage income of zone residents working outside zone)	0.023	0.043	0.047	0.006	0.036	0.045
	(0.028)	(0.034)	(0.031)*	(0.025)	(0.024)	(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	0.244	0.219	0.108	0.184	0.166
	(0.062)**	(0.075)**	(0.074)**	(0.074)	(0.085)*	(0.078)
log (annual wage income of nonresidents working in zone)	−0.023	−0.022	−0.012	−0.002	0.000	0.005
	(0.040)	(0.038)	(0.043)	(0.031)	(0.026)	(0.035)
log (annual wage income of zone residents working outside zone)	0.020	0.040	0.038	−0.005	0.031	0.035
	(0.038)	(0.052)	(0.043)	(0.030)	(0.036)	(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.

Table 7 examines the impact of EZ designation on the housing market. As in Table 6, we use the census microdata to construct adjusted estimates that hold tract dwelling characteristics constant over time. Owner occupied housing values exhibit dramatic increases of nearly a third across all specifications and samples. Adjusting for building characteristics has little effect on these impacts. Rental rates on the other hand exhibit no perceptible increase in any specification.

This large discrepancy between rental rates and housing values is, at first glance, troublesome. We suspect these findings reflect the fact that census measures of owner occupied housing values and rents are self-reported. If housing markets in such neighborhoods are relatively illiquid, residents may overestimate the extent to which EZ designation has changed the value of their residence. Rents, on the other hand, are easy to assess as they are usually paid monthly. Moreover, many units in such neighborhoods may be rent controlled which could (at least temporarily) limit upward pressure on measured rents.[30]

[30] We thank an anonymous referee for this suggestion.

Table 7—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.

To examine these conjectures in more detail, we compute housing value and rental rate impacts on households who report living in a different house five years ago. Since homeowners in this category purchased their dwelling recently they are more likely to have an accurate sense of its market value. Although our estimates are imprecise, we find that the housing value impacts in this subpopulation are substantially smaller than our earlier estimates as might be expected if long time residents are overconfident about their neighborhood's prospects. We also find that impacts on rental rates are somewhat larger (though still insignificant) in this sample. We take this as suggestive evidence that, over longer horizons, rental rates may in fact rise.

Table 8 documents that neither total tract population nor the number of zone households seem to have been substantially affected by zone designation. Population registers a modest increase in the parametric reweighting specification, but this change is not statistically significant, and coupled with the negligible impacts on the number of households, suggests at most a slight increase in average household size. We also fail to find an appreciable effect on the fraction of housing units that are vacant.

Finally, if rents or other local prices had increased substantially one would expect outmigration rates to rise as lower skilled groups are priced out of the neighborhood. Yet Table 8 provides no evidence of an impact on the fraction of households living in the same house as five years ago. We do, however, find a small increase in the fraction of college graduates in these neighborhoods which suggests that when prior residents do leave, they may be replaced by a somewhat different mix of new arrivals, even if the total flow of new arrivals is essentially constant.

Overall, these findings suggest that EZ designation created jobs in zone neighborhoods, that both zone and nonzone residents obtained employment in these neighborhoods that would not have otherwise been available, and that earnings increased substantially for local workers. While housing prices rose, we find little evidence of

Table 8—Population and Mobility Impacts

	Naïve (1)	OLS (2)	PW (3)	Observations (4)
log (households)	−0.007	−0.003	0.020	1,653
	(0.071)	(0.036)	(0.073)	
log (population)	−0.014	0.028	0.060	1,656
	(0.055)	(0.035)	(0.059)	
Percent same house as five years ago	−0.004	−0.001	−0.006	1,656
	(0.008)	(0.012)	(0.011)	
Percent houses that are vacant	0.016	−0.007	−0.010	1,653
	(0.013)	(0.009)	(0.013)	
Percent black	−0.018	−0.011	−0.015	1,656
	(0.015)	(0.009)	(0.015)	
Percent college	0.015	0.020	0.021	1,656
	(0.006)	(0.006)***	(0.007)***	

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.

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

important increases in the local cost of living for prior residents. We also fail to find significant increases in population though the composition of that population may have shifted to some extent. These results suggest that while commuting patterns may be relatively sensitive to changes in incentives, the residential choices of workers are (over the horizon studied) quite rigid, presumably because zone neighborhoods are poor substitutes for less distressed areas. The evidence also suggests an important role for both the wage credit and block grant features of the EZ program which appear to have disproportionately raised employment at large firms, raised wages among local workers, and still raised the employment of nonresident commuters albeit by less than local residents.

VI. Robustness

If unmeasured factors correlated with the future performance of neighborhoods influenced the process by which zones were awarded, our estimates will be biased. To address such concerns, we now perform two tests of the assumptions underlying our research design.

Our first test is to create a series of "placebo" zones in treated cities and compare their performance over the 1990s to that of control tracts using our differences-in-differences estimators. A finding of nonzero treatment effects in this sample would suggest that our analysis is confounded by city specific shocks.

To construct the placebo zones we estimated a pooled propensity score model for tracts in treated cities (see Appendix C for details) and then performed nearest neighbor propensity score matching without replacement in each city. We restrict the set of potential placebo tracts to obey the minimum poverty and unemployment eligibility criteria in 1990 along with our usual restrictions on tract population and the number of households. We also discard tracts located within a mile of actual EZs as they might experience spillovers. This yields a set of placebo zones of nearly the same size and with approximately the same census characteristics as each real EZ.[31]

Table 9 shows the results of applying our differences-in-differences estimators to our sample of placebo tracts. After reweighting, none of the outcomes register statistically significant differences across placebo and control zones. Moreover, no systematic pattern is apparent from the placebo point estimates as a whole.

As a second check on our research design we convert the outcome variables to scaled within city ranks.[32] If our results are merely picking up city specific shocks then the rank of an average EZ tract in its citywide distribution of mean tract rental rates, for example, should not change over the 1990s relative to the rank of a similar rejected tract in its citywide distribution. We scale our ranks by the number of tracts in each city so that the transformed outcomes can be thought of as percentiles which are comparable across cities of different absolute size.[33]

Columns 5–7 of Table 9 show the results of applying the three differences-in-differences estimators to the transformed outcomes. The point estimates represent the average impact of EZ designation on the percentile rank of EZ neighborhoods. For example, column 5 indicates that EZ designation led EZ neighborhoods to rise, on average, 2.7 percentiles in the within city distribution of jobs per tract. The results are in agreement with the findings of Tables 4–7 which we take as evidence that our prior results are unlikely to have been generated by spurious correlation with citywide trends.

Finally, Appendix Table A3 provides impact estimates in three alternative estimation samples. The first sample relies entirely upon rejected Round I applicants for controls and hence discards later round zones. The second sample drops New York city which may have been subject to different shocks during the sample period. The third sample adds the two SEZs (Cleveland and Los Angeles) to the sample. Much the same pattern of results is present in each sample with the rejected sample finding somewhat larger impacts and the SEZ sample yielding somewhat greater precision.

VII. Welfare Analysis

Our empirical analysis suggests that EZ designation generated important changes in local price levels and behavior. The model developed in Section II provides a framework for assessing the welfare consequences of these changes. We begin by

[31] The number of placebo tracts is somewhat less than the number of EZ tracts because some cities did not have enough tracts that met the eligibility criteria.

[32] In a previous version of this paper we experimented with a reweighted difference-in-differences-in-differences (DDD) estimator that sought to find within city controls for both actual and rejected EZ tracts. This estimator performed quite poorly severely failing a number of robustness tests. This poor performance was caused by difficulties in finding suitable control tracts in rejected cities. We believe the following percentile rank approach to be a much more transparent and robust approach to making within city comparisons.

[33] That is, for any outcome Y_{tzc} in tract t of zone z in city c, we form a new outcome $\tilde{Y}_{tzc} = rank_c(Y_{tzc})/N_c$ where $rank_c$ is the track rank (the lowest value receives rank 1, the highest rank N_c) of Y_{tzc} in the citywide distribution of the variable in that year and N_c is the number of tracts in the relevant city.

Table 9—Robustness Checks

	Placebo				Percentile			
	Naïve (1)	OLS (2)	PW (3)	Observations (4)	Naïve (5)	OLS (6)	PW (7)	Observations (8)
log (jobs)—LBD	−0.085	−0.024	−0.026	1,574	0.027	0.022	0.023	1,651
	(0.095)	(0.093)	(0.105)		(0.007)**	(0.009)*	(0.010)**	
log (establishments)	0.021	0.029	0.042	1,574	0.014	0.002	0.000	1,651
	(0.035)	(0.023)	(0.040)		(0.007)	(0.006)	(0.007)	
log (average earnings per worker)	−0.028	0.013	0.017	1,574	0.006	−0.010	−0.009	1,651
	(0.018)	(0.023)	(0.026)		(0.011)	(0.011)	(0.014)	
log (jobs)—JTW	0.089	0.065	0.032	1,575	0.054	0.036	0.029	1,656
	(0.059)	(0.049)	(0.074)		(0.010)**	(0.015)*	(0.014)**	
log (weekly wage of zone residents)	0.004	0.008	0.017	1,575	0.032	0.022	0.015	1,653
	(0.020)	(0.012)	(0.023)		(0.016)	(0.015)	(0.019)	
log (weekly wage of zone workers)	−0.023	−0.013	−0.009	1,571	0.016	0.024	0.020	1,652
	(0.018)	(0.032)	(0.025)		(0.018)	(0.028)	(0.027)	
log (rent)	0.003	−0.001	0.007	1,575	0.019	0.015	0.022	1,653
	(0.041)	(0.029)	(0.040)		(0.006)*	(0.009)	(0.008)*	
log (housing value)	0.186	0.060	0.083	1,529	0.110	0.089	0.104	1,581
	(0.070)	(0.047)	(0.089)		(0.049)**	(0.029)**	(0.049)*	
log (households)	0.085	0.017	0.030	1,575	−0.013	−0.020	−0.009	1,653
	(0.061)	(0.038)	(0.064)		(0.022)	(0.013)	(0.023)	
log (population)	0.027	0.014	0.016	1,578	−0.002	−0.005	0.005	1,656
	(0.057)	(0.039)	(0.058)		(0.012)	(0.009)	(0.013)	
Percent same house as five years ago	0.019	0.011	0.009	1,578	0.028	0.018	0.008	1,656
	(0.010)	(0.008)	(0.013)		(0.030)	(0.032)	(0.033)	
Percent houses that are vacant	0.019	−0.005	−0.002	1,575	−0.006	−0.042	−0.034	1,653
	(0.019)	(0.011)	(0.018)		(0.029)	(0.016)	(0.029)	

Notes: Timing: Variables labeled as LBD are analyzed over the period 1992–2000, all other outcomes are analyzed over the period 1990–2000. Columns: Columns 1–3 give differences-in-differences (DD) estimates on a sample of untreated placebo tracts chosen by nearest neighbor matching, (see Appendix C). Columns 5–7 give DD impacts on percentile ranks of outcomes (see Section VI). Columns 4 and 5 present the number of observations used to estimate each outcome. Estimators: Columns labeled "Naïve" report a DD estimate without controls. Columns labeled "OLS" report DD estimate 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.

Sources: Variables marked as JTW are based on the Journey-to-Work component of the Decennial Census. Variables marked as LBD come from the Longitudinal Business Database. All other tract level covariates come from the census.

considering the incidence of EZ designation on program stakeholders. Derivations analogous to those in (6) reveal that the total impact of the program on workers may be written as

$$
\begin{aligned}
(15) \qquad dV &= \sum_j \sum_k \sum_s N_{jks}[w_{jks}\, d\ln w_{jks} - r_j\, d\ln r_j + A_j\, d\ln A_j] \\
&\approx d\ln w^{local} \sum_{j \in \mathcal{N}_1} \sum_{k \in \mathcal{N}_1} \sum_s N_{jks} w_{jks} + d\ln w^{commute} \sum_{j \in \mathcal{N}_0} \sum_{k \in \mathcal{N}_1} \sum_s N_{jks} w_{jks} \\
&\quad + d\ln A^{EZ} \sum_{j \in \mathcal{N}_1} N_{j.} A_j - d\ln r^{NEZ} \sum_{j \in \mathcal{N}_0} N_{j.} r_j - d\ln r^{EZ} \sum_{j \in \mathcal{N}_1} N_{j.} r_j,
\end{aligned}
$$

where $d \ln w^{local}$ is the average impact on the wages of zone resident workers, $d \ln w^{commute}$ is the corresponding impact on nonresident commuters, $d \ln A^{EZ}$ is the average increase in zone amenities, $d \ln r^{NEZ}$ is the average impact on rental rates of housing outside of the zone, and $d \ln r^{EZ}$ is the average impact on rental rates of housing inside the zone.

Hence, to first order, the program's benefits may be measured as: (i) the total earnings increase for zone resident workers, (ii) the earnings increase for nonresident commuters, (iii) the value of any improvements in local amenities, and (iv) the value of any rent reductions that occur outside the zone due to population decreases. These benefits to workers are offset by any increases in the cost of living in the zone which may be measured in terms of the total zone rental cost. Our estimates suggest little effect on population or rents inside the zone so we assume for simplicity that zone amenities and rents outside the zone were both unaffected by designation $(d \ln A^{EZ} = d \ln r^{NEZ} \approx 0)$. Note that these assumptions provide a lower bound estimate of the benefits of EZ designation since we expect that amenity levels were positively impacted by the program, if only slightly.[34] Also noteworthy is that this accounting of benefits assumes perfect competition and hence ignores any economic rents that might accrue to business owners which will again lead us to understate the true social benefits of the EZ program.

Table 10 provides calculations converting our treatment effect estimates from Section V into effects on totals corresponding to the terms in (15). We rely on the results of our OLS specifications which tend to be most precise. Our "baseline" scenario takes our point estimates at face value even when statistically insignificant. To convey the uncertainty in our estimates we report 90 percent confidence intervals for the relevant impacts and also report a "pessimistic" scenario where impacts take on their least favorable values in these intervals.

Approximately 38,000 zone residents worked in EZs in 2000 with a payroll of roughly \$800 million. Our estimate of the program's impact on the wages of local residents is roughly 13 percent which translates into a \$109 million increase in annual earnings for zone residents who work in the zone.[35] This figure is above the \$55 million in wage credits disbursed in 2000 but the lower limit of our 90 percent confidence interval for this impact amounts to only \$38 million in increased wages. It is in fact possible for the wages of zone residents to rise by more than the total amount of credits if the block grants were productive. Though imprecise, our point estimates of the impact of the program on the wages of nonresident zone workers (and the corresponding impacts on employment of nonresident commuters) suggest that such productivity effects may have indeed been present. We found a statistically insignificant 0.5 percent increase in the wages of nonresident EZ workers in response to designation but cannot rule out more substantial effects. However in our "pessimistic" scenario we simply set this impact equal to zero. We also failed to find significant increases in the wages of the roughly 141,000 zone residents who in 2000 lived in the zone but worked elsewhere. Our OLS point estimate of a 3.3 percent increase in this group's weekly wages

[34] For example, Hebert et al. (2001) document 14 brownfield cleanup programs, 37 neighborhood beautification projects, and 23 parks and playgrounds built or rehabilitated as part of the EZ program. In unreported results we also found some evidence of small reductions in rates of violent crime in EZ cities.

[35] Our results are in log points. We compute impacts relative to 2000 levels for expositional ease. Similar results obtain if we take 1990 levels as the base.

Table 10—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.

would yield roughly \$118 million in additional annual earnings but, in our pessimistic scenario, we set this impact to zero as well.

Potentially offsetting the estimated increases in the earnings of local workers is the possibility of small increases in housing rents. Approximately 190,000 EZ households rented their dwellings in 2000 with total annual rental payments of \$900 million. Our estimates of the impact of designation on rents are small and statistically insignificant. But the upper limit of a 90 percent confidence interval includes impacts as large as 7.3 percent. Thus, a pessimistic interpretation of the rent impacts would amount to an aggregate transfer from renters to landlords of \$67 million per year. To verify that for the subpopulation of local workers the positive effects of the wage increases outweigh the negative effects of any rent increases we compute confidence intervals for the estimated impact on $q \equiv \ln w^{local} - s \ln r^{EZ}$, where $s \equiv \frac{r^{EZ}}{w^{local}}$ is the budget share of rental housing. We set s to 0.25, which is approximately the ratio of total rents to total earnings among local zone workers in the 1990 microdata. Because $dq = \frac{dw^{local} - dr^{EZ}}{w^{local}}$, the reweighted difference in difference impact on q provides an approximation to the percentage increase in disposable income of local workers. A 90 percent wild bootstrapped confidence interval for dq is provided in Table 10 and shown to have a lower bound of 3.4 percent. Thus, we conclude that, at least for local workers, the earnings increases associated with the program outweigh any increases in cost of living.

Finally, an additional 46,000 EZ households own their homes which were in aggregate worth \$4.8 billion in 2000. Our estimates suggest EZs boosted housing values by approximately 28 percent, which amounts to approximately \$1.35 billion in additional wealth. Our scepticism of these results leads us to also consider an alternative scenario where the housing value impacts are set to the lower limit of their confidence interval, which is below even the increase reported by new residents, whom we believe have more accurate information regarding their housing prices. This pessimistic scenario still yields a \$500 million windfall to owner occupiers in the zone.

In sum, the point estimates in our baseline scenario imply that total worker earnings rose by roughly \$296 million per year while rents rose by only \$5.5 million per year and housing wealth rose for owner occupiers by roughly \$1.35 billion. Under our pessimistic scenario, aggregate earnings rose by only \$36 million, rents rose by \$67 million, and housing wealth rose by \$500 million. Even under this worst case interpretation, we still find that earnings rose more for local workers than did rents. But nonworking households (or households working outside the zone) may have suffered cost of living increases making them strictly worse off.

We turn now to an analysis of the program's deadweight loss. We start with the tax credits, whose efficiency consequences depend critically upon the number of zone jobs created for zone workers in the covered sector. Unfortunately, we cannot directly identify which jobs are in the covered sector as the census lacks information about employer characteristics and the LBD does not report worker residence. Our estimates from Table 5 indicate that EZs generated a roughly 15 percent increase in the number of zone jobs for zone residents. Since many local jobs are in the uncovered sector, this figure is likely to provide a substantial underestimate of the impact on covered local employment $(d \ln N_{jk1})$, with the degree of understatement depending upon the relative size of the covered and uncovered sectors.

It is important then to supplement our analysis with auxiliary sources of information. Recall that \$55 million in wage credits was disbursed to EZ firms in 2000. The maximum allowable credit per worker is \$3,000. In most cases the full credit will be claimed, but to be conservative, let us suppose that \$2,500 was claimed on the average worker. This yields 22,000 workers on whom the credit was claimed—roughly 60 percent of the local workforce. Therefore, if all of the jobs created were in the covered sector and there was no negative impact on uncovered employment we would estimate that employment expanded by roughly $\frac{15 \text{ percent}}{60 \text{ percent}} = 25$ percent in the covered sector. We use this as our baseline estimate as we suspect that employment may actually have increased in the uncovered sector as well in response to the block grants.

However, our model suggests that if the block grants were ineffective employment at firms in the uncovered sector might actually fall in response to the credit. Therefore we also consider an inflated estimate of the covered sector employment response obtained from survey data.[36] A 1999 General Accounting Office survey found that among firms making use of the wage tax credit, a third indicated that the credits were

[36] Our LBD-based estimates of the employment impacts on small firms also suggest that EZs may have reduced employment at small firms likely to be in the uncovered sector.

"very important" or "extremely important" for the hiring decision.[37] Hence, a more pessimistic estimate may be obtained by assuming that one third of the credits claimed resulted in jobs that would not otherwise have occurred $\left(\text{i.e., } d\ln N_{jk1} = \frac{1}{3}\right)$.

Using our baseline estimate we can compute the jobs semi-elasticity as $\psi^{base} = \frac{1/4}{0.2} = \frac{5}{4}$.[38] Plugging this number into (10) yields an estimated deadweight loss associated with the employment tax credit of $\frac{1}{2} \times \frac{5}{4} \times 0.2 \times \$55$ million $= \$6.9$ million or roughly 13 percent of the flow cost of the subsidy.[39] A corresponding calculation using the pessimistic value of $\psi^{pess} = \frac{1/3}{0.2} = \frac{5}{3}$ results in a deadweight loss of roughly 18 percent. We consider this figure a substantial overestimate both because it presumes substantial flows from the uncovered to the covered sector and because the zone wage credit should offset preexisting payroll taxes and hence, to some extent, actually reduce the amount of distortion in hiring decisions. Of course, these estimates must be inflated to take into account the marginal cost of funds. An upper bound estimate of this parameter is provided by Feldstein (1999) who obtains a deadweight cost of thirty cents of every dollar raised. This yields an upper bound composite deadweight loss estimate of approximately 48 percent of the subsidy.

As noted in Section II, the block grants accompanying EZ designation may yield either a deadweight loss or a net welfare gain depending upon how effectively they were spent. We have already assumed that EZs had no effect on amenity levels, so we set $\lambda_a = 0$ in (11). Roughly $C = \$400$ million worth of federal block grants was invested in zone neighborhoods over the sample period. A worst case estimate then is that all \$400 million worth of block grants was wasted on unproductive activities, a hypothesis we cannot reject. Although we failed to detect statistically significant impacts on the wages of nonresident commuters, we caution that our point estimates do not rule out the possibility that the block grants were cost effective. To illustrate the sensitivity of such a calculation, note that the zone workforce $\left(\sum_{k\in\mathcal{N}_1} N_{.k}\right)$ consisted of approximately 400,000 workers in 2000 with approximately \$15 million in annual earnings. Even a 0.5 percent effect on productivity of the sort suggested by our OLS point estimates would yield $0.005 \times \$15$ billion $= \$75$ million in additional earnings per year. Assuming a social discount rate of 10 percent yields an annuitized value for this earnings stream of \$750 million which is well above the \$400 million cost of the block grants over the period of study. Isolating the effectiveness of local block grant spending is a priority for future research.

VIII. Conclusion

Our comparison of EZ neighborhoods to rejected and future tracts revealed important impacts of EZ designation on local price levels and behavior. Designation seems to have resulted in substantial increases in zone employment along with increases in the wages of zone residents working in the zone. These changes in the zone labor

[37] See Table III.1 of General Accounting Office (1999).

[38] This figure is substantially smaller than the intra-metropolitan job elasticity estimates surveyed by Bartik (1991). A potential explanation for this discrepancy is that these tax credits are tied to residence in distressed neighborhoods which the bulk of workers find relatively undesirable.

[39] We have made use here of the fact that $\tau \sum_{j\in\mathcal{N}_1} \sum_{k\in\mathcal{N}_1} N_{jk1} w_{jk1}$ is the size of the aggregate subsidy when firms are able to claim a credit of 20 percent on the wage bill of every covered worker.

market appear not to have been accompanied by dramatic changes in the local cost of living. Population and housing rents remained roughly constant, though evidence on the rental rates of new arrivals to the neighborhood suggests that rents may eventually rise. Though we find very large increases in the price of owner occupied housing, we suspect the magnitude of these results is to some extent a reflection of the manner in which housing value data are collected in the census. However, these results may also foretell future increases in the local cost of living.

The conclusion of our welfare analysis is that the EZ program appears to have successfully transferred income to a small spatially concentrated labor force with modest deadweight losses aside from the usual cost of raising the funds for the subsidy itself. We caution however that our study provides only a short run evaluation of the EZ program. Administrative data indicate that participation in the EZ tax credit program increased only gradually over time and, as evidenced by our annual analysis of LBD data, it took many years for some economic outcomes to respond. The responses of firms, population, and prices may well differ substantially over longer periods of time, if EZ subsidies in fact persist over such horizons. If however, these subsidies eventually lapse as originally intended, an important question will be whether they have lasting effects, a subject studied in a different context by Kline and Moretti (2011).

Finally, we emphasize that many of our empirical results are imprecise and should not necessarily be expected to generalize to later round and future zones. Additional zones targeting less heavily distressed communities may yield larger distortions as such communities may be closer substitutes with surrounding areas. Moreover, later round zones utilize different combinations of benefits. While we find it plausible that the mix of large block grants and wage credits accompanying EZs would yield different results than their smaller state level predecessors, more work is necessary to disentangle the effectiveness of various combinations of spatial subsidies.

Appendix A: Methods

A1. *Computation of PW Estimator*

We run a pooled tract-level regression of the form

$$\Delta Y_{tzc} = \mu^1 T_z + (1 - T_z) \times \mathbf{X}'_{n(t)} \boldsymbol{\alpha}^x + (1 - T_z) \times \mathbf{P}'_c \boldsymbol{\alpha}^p + e_{tzc},$$

where $\mathbf{X}_{n(t)}$ is assumed to include a constant. Note that because this regression is fully interacted, $\hat{\mu}^1$ will evaluate to the mean of ΔY_{tzc} among the EZ tracts. Let $\mathbf{Z}_t \equiv [\mathbf{X}_{n(t)}, \mathbf{P}_c]$ and $\hat{\boldsymbol{\alpha}} \equiv [\hat{\boldsymbol{\alpha}}^x, \hat{\boldsymbol{\alpha}}^p]'$. The counterfactual mean estimate for treated observations may be computed as

$$\begin{aligned}
\hat{\mu}^0 &= \frac{1}{N_1} \sum_t T_t \mathbf{Z}'_t \hat{\boldsymbol{\alpha}} \\
&= \frac{1}{N_1} \sum_t T_t \mathbf{Z}'_t \left[\left(\sum_l (1 - T_l) \mathbf{Z}_l \mathbf{Z}'_l \right)^{-1} \sum_m (1 - T_m) \mathbf{Z}_m \Delta Y_{mzc} \right] \\
&= \sum_m (1 - T_m) \omega_m \Delta Y_{mzc},
\end{aligned}$$

where the $\omega_m \equiv \frac{1}{N_1} \sum_t T_t \mathbf{Z}_t' \big(\sum_l (1 - T_l) \mathbf{Z}_l \mathbf{Z}_l' \big)^{-1} \mathbf{Z}_m$ are weights obeying $\sum_m (1 - T_m) \omega_m = 1$. It is straightforward to verify that for any covariate $\mathbf{Q}_t \in \mathbf{Z}_t$, $\sum_t (1 - T_t) \omega_t \mathbf{Q}_t = \frac{1}{N_1} \sum_t T_t \mathbf{Q}_t$. Hence the regression weights yield reweighted covariate means among the controls numerically equivalent to the corresponding covariate means in the treatment group. See Kline (2011) for the interpretation of this procedure as a propensity score reweighting estimator. We use these weights in computing the reweighted control means reported in Figures 1 and Tables 3A and 3B. Tract level covariate means are not perfectly balanced in Tables 3A and 3B because we condition on distance weighted averages of covariates rather than tract level variables themselves.

The treatment effect estimator in (14) may be written $\widehat{ATT} = \hat{\mu}^1 - \hat{\mu}^0$, which is the quantity reported in our PW impact estimates. An analytical variance estimate may be computed as

$$\widehat{Var}(\widehat{ATT}) = \hat{V}_1 + \left(\frac{1}{N_1} \sum_t T_t \mathbf{Z}_t' \right) \hat{\mathbf{V}}_0 \left(\frac{1}{N_1} \sum_t T_t \mathbf{Z}_t' \right)',$$

where $\hat{\mathbf{V}}_0$ is the standard OLS cluster robust estimator of the covariance matrix of the estimated parameters $(\hat{\boldsymbol{\alpha}}^x, \hat{\boldsymbol{\alpha}}^p)$ and $\hat{V}_1$ is the corresponding variance estimate for $\hat{\mu}^1$. We use this analytical variance estimate to construct an asymptotic pivot for use in our wild bootstrap procedure.

A2. *Wild Bootstrap Inference*

As suggested by Cameron, Gelbach, and Miller (2008) we conduct inference using a cluster robust percentile-t wild bootstrap procedure with Rademacher weights. We impose the null hypothesis that the coefficient on the EZ dummy is zero when computing our residuals for resampling. Bootstrap *p*-values are computed by assessing the fraction of bootstrap test statistics greater in absolute value than the sample test statistic. All bootstrap tests use 1,999 repetitions.

The confidence intervals in Section VII are constructed via test inversion. That is, we conduct a grid search over null hypothetical values for the treatment effect in question, compute the corresponding restricted residuals, and the wild bootstrapped *p*-value. Our 90 percent confidence intervals correspond to the set of points with estimated *p*-values above 0.1.

Appendix B: Data

B1. *Missingness/Weighting*

We exclude observations with missing and allocated values when constructing several of the tract-level variables included in the analysis. In most of these cases, we correct for the potential introduction of nonrandom selection by weighting nonmissing observations by the inverse of an estimate of the probability of the observation's inclusion.

A first set of missingness weights (applied to Decennial Census data) equals the inverse of the probability of an individual having a valid (nonmissing and nonallocated) place of work variable conditional on observable traits and on the individual being employed. We estimate that conditional probability with a linear probability model that includes main effects and all two-way interactions of age (under 20, 20–39, 40–64, and 65+), sex, race (black, white, and other), and education (dropout, high school grad, some college, and bachelors) and includes main effects for class of worker, wage decile (where missing wages are treated as an eleventh decile), and tract of residence. The model is estimated separately by county, year, and EZ assignment status according to tract of residence. Predicted values were winsorized to lie in the interval [0.025, 1]. These weights are applied when computing tract aggregates of quantities defined by individuals' places of work. Those aggregates include numbers of jobs and total earnings for tract workers residing in the zone, for tract workers residing outside of the zone, and for tract residents working outside of the zone.

A second set of missingness weights (applied to Decennial Census data) equals the inverse of the probability that an individual has a valid (nonmissing and nonallocated) place of work variable conditional on observable traits and on the individual being employed and having a nonallocated wage. We again estimate that conditional probability with a linear probability model that includes main effects and all two-way interactions of age, sex, race, and education and includes main effects for class of worker, wage decile, and tract of residence. The model is estimated separately by county, year, and EZ assignment status in the tract of residence. Predicted values are again winsorized to lie in the interval [0.025, 1]. These weights are applied when computing mean wages by individuals' places of work. These variables include mean log wages of tract workers residing in the zone, mean log wages of tract workers residing outside of the zone, and mean log wages of tract residents employed outside of the zone.

A third set of weights (applied to LBD data) equals the inverse of the probability that an establishment received a valid geocode during our geocoding algorithm conditional on observable establishment traits. Because the set of potential covariates was much smaller in this case the probabilities were estimated using parametric logit models. The explanatory variables in these models were dummies for establishment age (full vector of indicators for each possible age), establishment size (defined by total employment categories; 0–99, 100–249, 250–499, 500–999, and 1000+), and one-digit industry categories. Separate missingness models were estimated for single establishment firms and establishments belonging to multi-establishment firms within each county-year combination. These weights were applied in construction of all LBD based variables.

For some tract-years, we did not observe any tract workers in particular place of residence/place of work cells. For example some tracts lack any workers who reside in the zone containing the tract (local workers). To deal with this problem we replaced the change in the log of the number of local workers with the gross change divided by the average number of local workers in the two periods as suggested by Davis, Haltiwanger, and Schuh (1996). This measure varies between -2 and 2 and is well defined for tracts that have at least one local worker in either 1990 or 2000. For most tracts this measure yields values very close to the change in logs. Equivalent replacements were made for the change in the log of the number of nonlocal workers and the change in the log of the number of tract residents working outside the zone. Again, the approximation to the log change is very good in cases were the log change is well defined.

For tracts with no local workers sampled we stochastically impute the mean log wage of such workers. We first regress the mean log wage of local workers on a large set of contemporaneous tract level covariates and averages of those covariates over the three decades in tracts for which the mean log wage of local workers is well defined.[40] A separate regression is run for each Decennial year by EZ treatment status. We then impute a mean log wage/earnings of local workers for tracts missing that variable by assigning the sum of the linear prediction from this regression and a draw from a normal distribution with mean zero and standard deviation equal to the root mean squared error from the regression. Similar point estimates (and inferences) are obtained from the imputed and nonimputed variables.

B2. *Geocoding Algorithm*

Our analysis of business data from the SSEL and LBD required that each establishment be coded to a 1990 census tract. While a census tract variable appears on the SSEL files for 1992 and later, the values are very often missing. Instead of using the existing tract variable, we implemented an algorithm to assign establishments to census tracts based on their raw street addresses. Our algorithm consisted of three steps. First we attempted to code each address in each cross-section of the SSEL to a 2000 Census block.[41] For this step, we used the SAS/GIS batch geocoding module (invoked by the "percentGCBATCH" macro). Second, using the longitudinal links provided by the LBD, we filled in establishment-years with missing geocodes with the codes assigned to the same establishment in neighboring years. Third, we assigned each establishment a 1990 census tract based on its assigned 2000 census block.

The SSEL provides at least one street address field for each establishment in each annual cross-section. For single establishment firms, a mailing address is nearly always provided, and a physical address is sometimes provided. SSEL documentation suggests that the physical address field should be nonmissing in each case in which a single establishment firm's physical address and mailing address differ.

[40] The covariates included in this regression are: log wage of tract residents, log wage of tract workers, average over three decades of log wage of tract residents, average over three decades of log wage of tract workers, kernel weighted average across neighboring tracts of log wage of zone residents working in tract, kernel weighted average across neighboring tracts of log wage of nonzone commuters working in tract, kernel weighted average across neighboring tracts of log wage of tract residents working outside zone, averages across three decades of the three kernel weighted average variables, fraction of tract residents with a commute less than 25 minutes, fraction of tract residents who are black, fraction of tract residents who are Hispanic, fraction of tract residents who are high school dropouts, fraction of tract residents with college attendance, fraction of tract residents greater than 65 years old, fraction of tract residents less than 18 years old, fraction of tract residents who are employed, fraction of tract residents below the poverty line, log of tract population, log of tract area, log of the number of households living in the tract, an indicator for whether the tract was in the central business district in 1990, the distance to the central business district, and a vector of state-city fixed effects.

[41] We tested our geocoding algorithm using both 1990 TIGER/Line data and 2000 TIGER/Line data. An advantage of using the 1990 TIGER/Line files is that all coded establishments receive a 1990 census block code, a unit within which treatment status does not vary (EZs were awarded to collections of 1990 census tracts, which nest 1990 census blocks). We found however that the rate at which we successfully assigned geocodes was higher by several percentage points using 2000 TIGER/Line files than when using 1990 TIGER/Line files. While the mapping from 2000 census blocks to 1990 census tracts is not one-to-one, less than 0.5 percent of 2000 census blocks overlap multiple 1990 census blocks in the counties containing an EZ or control zone. We decided that the benefit of the higher successful geocoding rate outweighed the cost of slight mis-measurement of treatment assignment.

For establishments belonging to multi-establishment firms, only a physical address is provided.

As the first step of our geocoding process, we applied the following algorithm to all SSEL physical and mailing addresses of establishments located in counties containing an EZ or a control zone. Note that for single establishment firms, we attempted to code two addresses when two addresses were provided.

(i) Import 2000 TIGER/Line data into SAS/GIS spatial datasets.

(ii) Geocode SSEL address data using the SAS/GIS batch geocoder.

(iii) Set aside all observations that received a geocode in step 2. Proceed using only observations that have not yet received a geocode.

(iv) If all items on the following list have been reached, go to step 6. Otherwise, proceed and perform the first task on the following list that has not yet been performed.

   (a) Remove all punctuation marks.

   (b) Replace ordinal words with their numeric equivalents (e.g., third becomes 3rd).

   (c) Remove gaps between two groups of numbers appearing at the beginning of address strings (e.g., “123 45 Elm St” becomes “12345 Elm St”).

   (d) Remove official US Postal Service secondary address identifiers and all characters that follow them (e.g., “123 Elm St Suite 1” becomes “123 Elm St”).

   (e) Abbreviate all official US Postal Service primary address identifiers with their official abbreviations (e.g., “123 Elm Street” becomes “123 Elm St”).

   (f) Remove spaces between adjacent letters commonly used to identify cardinal directions (e.g., “123 S W Elm St” becomes “123 SW Elm St”).

(v) Return to step 2.

(vi) Stop.

In cases in which a physical address was successfully geocoded, we assigned the establishment the geocode associated with that address. In cases in which we were unable to assign a geocode to a physical address (usually because none was provided), we assigned the establishment the geocode associated with its mailing address.

In the second step of our geocoding process, we exploited the longitudinal links provided by the LBD to impute missing geocodes for establishments that were successfully coded in some, but not all, of the years in which they appeared in the SSEL. If an establishment's first observation to receive a successful geocode occurred in year t, we assigned the year t geocode to any observations for years prior to t. Similarly, if an establishment's last observation to receive a successful geocode occurred in year t, we assigned the year t geocode to any observations for years later than t. When an observation on the "interior" of an establishment's panel failed to receive a geocode, the observation was assigned the geocode of the nearest successfully geocoded observation. When an interior observation of this sort was equally close to two successfully geocoded observations, we chose between the geocodes of those two observations randomly, giving each a 0.5 probability of being selected.

In the final step of our geocoding process, we assigned each successfully coded establishment-year a 1990 census tract based on the 2000 census block assigned in the first two steps. To do this, we constructed a many-to-many crosswalk file relating 2000 census blocks to 1990 census tracts. We began by downloading the census provided Census Block Relationship File relating 1990 census tabulation blocks to 2000 census tabulation blocks. The Census Block Relationship File has one observation for each 1990 census tabulation block and 2000 census tabulation block pair with a nonempty intersection. We created a 1990 census tract variable from the provided 1990 census block variable and dropped any duplicate observations of 1990 census tract and 2000 census block. We then merged this file by 2000 census block to the list of geocoded addresses. In cases in which a 2000 census block mapped to N 1990 census tracts, we duplicated the firm's observation N times, assigned one observation to each potential 1990 census tract, and assigned weight $1/N$ to each of those observations in any subsequent analysis.

Appendix C: Construction of Placebo Zones

To construct placebo zones we performed nearest neighbor matching without replacement on a propensity score estimated on all tracts in the six cities receiving Round I EZs. To ensure a broad enough donor pool of placebo tracts, we define city broadly to include other municipalities in the same counties as the city itself. The propensity score was estimated on the pooled sample using a logit of assignment status on a large number of 1980 and 1990 covariates. The 1990 covariates include a vector of city indicators interacted with the fraction of households below the poverty line, a vector of city indicators interacted with the fraction unemployed, a vector of city indicators interacted with the log of tract population, a vector of city indicators interacted with the log of the number of jobs in the tract, the fraction black, the fraction Hispanic, the fraction who were high school dropouts, the fraction older than 65 years in age, the fraction less than 18 years old, the fraction of structures that were vacant, the fraction of households headed by a female, a tract building age index, and dummy indicators for tract poverty share below 25 percent and for tract poverty share below 35 percent. A similar list of 1980 covariates was used including the fraction black, the fraction Hispanic, the fraction who were high school dropouts, the fraction older than 65 years old, the fraction less than 18 years old, the fraction of structures that are vacant, and the fraction of households headed by a female.

Finally, we included 1980 to 1990 changes in the following variables: the fraction of employed workers with commute times less than 25 minutes, the log of the number of tract households, the mean log of tract rent, the mean log of tract housing value, the log of tract population, the log of tract jobs, the percent of tract workers with a college degree, the mean log wages of tract workers, and the mean log wages of tract residents. We also included 1987 to 1992 changes in the log of average tract wages (LBD) and the log of tract employment (LBD).

Appendix D: Tables and Figures

Table A1—Treatment by City

City	Sample	EZ-1	Application	Round I	Round II	Round III
Akron, OH (Summit)	X		X	EC-1		
Albany, GA (Dougherty)			X	EC-1		
Albuquerque, NM (Bernalillo)	X		X	EC-1		
Anniston, AL			X			
Atlanta, GA	X	X	X			RC
Austin, TX	X		X			
Baltimore, MD	X	X	X			
Bellmead, TX			X	EC-1		
Benton Harbor, MI			X			
Boston, MA			X	EEC-1	EZ-2	
Bridgeport, CT	X		X	EC-1		
Buffalo, NY/Lackawanna, NY	X					RC
Camden, NJ						RC
Charleston, SC	X					RC
Charleston, WV			X			
Charlotte, NC (Mecklenburg)	X		X	EC-1		
Chattanooga, TN	X					RC
Chester, PA			X			
Chicago, IL	X	X	X			RC
Cincinnati, OH	X				EZ-2	
Cleveland, OH			X	SEZ-1		
Columbia, SC	X				EZ-2	
Columbus, OH	X				EZ-2	
Corpus Christi, TX	X					RC
Cumberland, NJ					EZ-2	
Dallas, TX	X		X	EC-1		
Denver, CO	X		X	EC-1		
Des Moines, IA (Polk)			X	EC-1		
Detroit, MI	X	X	X			RC
East Chicago, IN	X		X		EZ-2	
East St Louis, IL	X		X	EC-1	EZ-2	
El Paso, TX	X		X	EC-1	EZ-2	
Evans, CO			X			RC
Fairbanks, AK			X			
Flint, MI	X		X			RC
Fort Lauderdale, FL			X			
Fort Worth, TX			X			
Fresno, CA	X		X			EZ-3
Gary, IN	X		X		EZ-2	
Greeley, CO			X			RC
Hamilton, OH						RC
Hammond, IN	X		X		EZ-2	
Harrisburg, PA (Dauphin)			X	EC-1		
Hartford, CT	X		X			
Houston, TX	X		X	EEC-1		
Huntington, WV					EZ-2	
Indianapolis, IN (Marion)	X		X	EC-1		
Ironton, OH					EZ-2	

(*Continued*)

Table A1—Treatment by City (*Continued*)

City	Sample	EZ-1	Application	Round I	Round II	Round III
Jackson, MI (Hinds)	X		X	EC-1		
Jacksonville, FL	X		X			EZ-3
Kansas City, KS	X		X	EEC-1		
Kansas City, MO	X		X	EEC-1		
Knoxville, TN	X		X		EZ-2	
Lake Charles, LA			X			
Las Vegas, NV (Clark)			X	EC-1		
Lawrence, MA						RC
Little Rock, AR (Pulaski)	X		X	EC-1		EZ-3
Los Angeles, CA	X		X	SEZ-1		RC
Louisville, KY	X		X	EC-1		
Lowell, MA	X					RC
Manchester, NH			X	EC-1		
Memphis, TN	X		X			RC
Miami, FL	X		X	EC-1	EZ-2	
Milwaukee, WI	X		X			RC
Minneapolis, MN	X		X	EC-1	EZ-2	
Mobile, AL	X		X			RC
Monroe, LA			X			RC
Muskegon, MI			X	EC-1		
Nashville, TN (Davidson)			X	EC-1		
New Haven, CT			X	EC-1	EZ-2	
New Orleans, LA	X		X			RC
New York, NY	X	X	X			
Newark, NJ	X					RC
Niagara Falls, NY						RC
Norfolk, VA	X		X	EC-1	EZ-2	
Oakland, CA	X		X	EEC-1		
Ogden, UT (Weber)			X	EC-1		
Oklahoma City, OK	X		X	EC-1		EZ-3
Omaha, NE (Douglas)	X		X	EC-1		
Orange, TX			X			
Peoria, IL	X		X			
Philadelphia, PA	X	X	X			RC
Phoenix, AZ	X		X	EC-1		
Pine Bluff, AR			X			
Pittsburgh, PA	X		X	EC-1		
Port Arthur, TX			X			
Portland, OR	X		X	EC-1		
Portsmouth, VA	X		X	EC-1	EZ-2	
Providence, RI	X		X	EC-1		
Richmond, VA	X		X			
Rochester, NY	X		X			RC
Sacramento, CA			X			
San Antonio, TX	X		X	EC-1		EZ-3
San Diego, CA	X		X			RC
San Francisco, CA						RC
Santa Ana, CA					EZ-2	
Savannah, GA	X		X			
Schenectady, NY						RC
Shreveport, LA			X			
Sioux City, IA			X			
Springfield, MA (Hampden)	X		X	EC-1		
St. Louis, MO	X		X	EC-1	EZ-2	
St. Paul, MN (Ramsey)	X		X	EC-1		
Steubenville, OH			X			
Sumter, SC	X				EZ-2	
Syracuse, NY	X					EZ-3
Tacoma, WA			X			RC
Tampa, FL	X		X	EC-1		
Tucson, AZ	X		X			EZ-3

(*Continued*)

Table A1—Treatment by City (*Continued*)

City	Sample	EZ-1	Application	Round I	Round II	Round III
Waco, TX			X	EC-1		
Washington, DC			X	EC-1		EnZ
Whitehall, AR			X			
Wilmington, DE (New Castle)			X	EC-1		
Yakima, WA						RC
Yonkers, NY						EZ-3
Youngstown, OH			X			

Notes: Sample refers to the estimation sample. EZ-1 refers to cities in the treated group (Empowerment Zones in Round I in 1994). Application refers to cities that applied to get an EZ-1. SEZ-1 refers to cities that received a Supplemental Empowerment Zone (Round I, 1996). EC-1 refers to Enterprise Community awarded in Round I (1994), EEC-1 refers to Enhanced Enterprise Community awarded in Round I (1994), EZ-2 refers to Empowerment Zone awarded in Round II (2000), RC refers to Renewal Community awarded in Round III (2002), EZ-3 refers to Empowerment Zone awarded in Round III (2002) and EnZ refers to the Enterprise Zone awarded in Round III (2002).

Table A2—Second Moments in 1990 Treatment and Controls

	EZs (1)	Rejected/ future zones (2)	Rejected/ future zones reweighted (3)	*p*-value of difference between (1) and (2) (4)	*p*-value of difference between (1) and (3) (5)
Census tracts characteristics					
Economic index (residents)	0.996	1.014	0.632	0.985	0.507
Employment rate	0.146	0.203	0.147	0.000	0.873
Unemployment rate	0.071	0.039	0.059	0.139	0.498
Poverty rate	0.251	0.197	0.243	0.110	0.742
Economic index (workers–JTW)	0.996	0.750	0.733	0.258	0.158
log (jobs)–JTW	44.715	50.049	45.626	0.103	0.564
log (hourly wage of zone workers)–JTW	35.673	34.814	35.214	0.013	0.104
log (hourly wage of zone residents)	30.926	29.815	29.678	0.127	0.037
Economic index (workers–LBD)[a]	0.996	1.112	0.951	0.282	0.671
log (jobs)–LBD[a]	35.718	42.529	37.940	0.157	0.364
log (establishments)–LBD[a]	10.584	13.841	11.075	0.002	0.471
log (average earnings per worker)–LBD[a]	8.965	8.844	8.870	0.613	0.775
Demographic index	0.996	1.596	0.967	0.008	0.913
Percent households female-headed	0.340	0.281	0.345	0.004	0.747
Percent college	0.008	0.012	0.009	0.187	0.611
Percent high school dropouts	0.107	0.081	0.105	0.003	0.762
Percent black	0.653	0.500	0.689	0.068	0.605
Percent Hispanic	0.114	0.096	0.113	0.683	0.933
Population index	0.996	0.879	1.175	0.869	0.547
log (population)	60.797	62.539	61.571	0.416	0.591
log (households)	48.288	49.229	48.244	0.627	0.969
Percent same house as five years ago	0.339	0.276	0.348	0.059	0.682
Housing index	0.995	0.993	0.791	0.994	0.496
log (rent)	28.759	28.983	28.146	0.819	0.573
log (housing value)	110.667	112.040	106.581	0.787	0.458
Percent houses that are vacant	0.039	0.027	0.028	0.273	0.286

(*Continued*)

Table A2—Second Moments in 1990 Treatment and Controls (*Continued*)

	EZs (1)	Rejected/ future zones (2)	Rejected/ future zones reweighted (3)	*p*-value of difference between (1) and (2) (4)	*p*-value of difference between (1) and (3) (5)
City characteristics					
Total crime/population × 100	0.011	0.012	0.011	0.737	1.000
Average across tracts percent black	0.258	0.154	0.258	0.176	1.000
Percent workers in manufacturing	0.026	0.027	0.026	0.861	1.000
Percent workers in city government	0.005	0.003	0.005	0.487	1.000
log (city population)	212.022	171.201	188.673	0.000	0.000
Observations (number of census tracts)	234	1,429	1,429	1,663	1,663

Notes: Column 1 reports the uncentered second moment for census tracts inside EZs. Column 2 shows the uncentered second moment for control tracts in rejected or future treated areas (listed in Table A1). Column 3 reports the uncentered second moment for control tracts after parametric reweighting (see Section III for details). Column 4 presents wild bootstrap *p*-values for a test of the null hypothesis that the uncentered second moment in column 1 equals the uncentered second moment in column 2. Similarly, column 5 reports *p*-values for the equality of uncentered second moments in columns 1 and 3.

[a]For LBD variables, columns 1, 2, and 3 show the uncentered second moment in 1987.

Sources: Variables marked as JTW are based on the Journey-to-Work component of the Decennial Census. Variables marked as LBD come from the Longitudinal Business Database. All other tract level covariates come from the census. City covariates are from the County/City Databook.

Table A3—Robustness Checks (*Alternative samples*)

Model	Naïve (1)	OLS (2)	PW (3)	Observations (4)
Panel A. Without New York census tracts				
log (jobs)–LBD	0.074	0.142	0.156	1,602
	(0.033)	(0.041)**	(0.061)**	
log (establishments)	0.004	0.034	0.048	1,602
	(0.029)	(0.015)*	(0.035)	
log (average earnings per worker)	−0.026	−0.002	0.004	1,602
	(0.013)*	(0.019)	(0.016)	
log (jobs)–JTW	0.187	0.159	0.164	1,605
	(0.080)	(0.068)*	(0.096)**	
log (zone jobs held by zone residents)	0.115	0.116	0.142	1,603
	(0.100)	(0.073)	(0.115)	
log (zone jobs held by nonresidents)	0.154	0.106	0.100	1,605
	(0.062)	(0.066)	(0.081)	
log (nonzone jobs held by zone residents)	0.001	0.063	0.087	1,605
	(0.072)	(0.070)	(0.069)	
log (weekly wage income of zone residents)[a]	0.052	0.063	0.068	1,604
	(0.022)	(0.013)**	(0.023)***	
log (weekly wage income of zone workers)[a]	0.017	0.019	0.015	1,601
	(0.023)	(0.027)	(0.026)	
log (weekly wage income of zone residents working in zone)[a]	0.092	0.129	0.127	1,597
	(0.056)	(0.055)**	(0.059)*	
log (weekly wage income of nonresidents working in zone)[a]	0.019	0.005	0.006	1,593
	(0.023)	(0.027)	(0.029)	
log (weekly wage income of zone residents working outside zone)[a]	0.017	0.028	0.031	1,592
	(0.027)	(0.025)	(0.029)	
log (rent)[a]	0.014	−0.006	0.002	1,604
	(0.033)	(0.028)	(0.032)	

(*Continued*)

TABLE A3—ROBUSTNESS CHECKS (*Alternative samples*) (*Continued*)

Model	Naïve (1)	OLS (2)	PW (3)	Observations (4)
log (housing value)[a]	0.438 (0.120)	0.285 (0.073)*	0.325 (0.131)*	1,546
log (households)	−0.076 (0.044)	−0.039 (0.033)	−0.046 (0.047)	1,604
log (population)	−0.074 (0.024)**	−0.010 (0.030)	−0.007 (0.029)	1,607
Percent same house as five years ago	−0.001 (0.010)	0.005 (0.014)	0.002 (0.011)	1,607
Percent vacant houses	0.012 (0.015)	−0.006 (0.010)	−0.008 (0.015)	1,604
log(hourly wage) − 0.25 log(rent)[a]	0.085	0.126	0.122	1,590
Panel B. Rejected census tracks as controls				
log (jobs)–LBD	0.087 (0.055)	0.198 (0.068)**	0.219 (0.083)***	1,100
log (establishments)	0.006 (0.031)	0.048 (0.037)	0.060 (0.043)	1,100
log (average earnings per worker)	−0.030 (0.013)*	0.027 (0.033)	0.056 (0.036)	1,100
log (jobs)–JTW	0.204 (0.071)*	0.238 (0.084)**	0.223 (0.109)**	1,107
log (zone jobs held by zone residents)	0.165 (0.098)	0.232 (0.098)*	0.327 (0.123)**	1,105
log (zone jobs held by nonresidents)	0.182 (0.058)*	0.189 (0.084)*	0.162 (0.100)*	1,106
log (nonzone jobs held by zone residents)	−0.001 (0.058)	0.164 (0.071)	0.239 (0.081)**	1,104
log (weekly wage income of zone residents)[a]	0.008 (0.031)	0.046 (0.026)	0.035 (0.039)	1,104
log (weekly wage income of zone workers)[a]	−0.002 (0.025)	0.062 (0.017)***	0.043 (0.030)**	1,105
log (weekly wage income of zone residents working in zone)[a]	0.082 (0.046)	0.173 (0.068)**	0.141 (0.070)*	1,099
log (weekly wage income of nonresidents working in zone)[a]	0.001 (0.025)	0.049 (0.018)**	0.047 (0.033)*	1,100
log (weekly wage income of zone residents working outside zone)[a]	−0.018 (0.026)	0.008 (0.028)	0.018 (0.038)	1,093
log (rent)[a]	−0.011 (0.029)	0.004 (0.037)	0.043 (0.038)	1,104
log (housing value)[a]	0.390 (0.137)*	0.346 (0.083)***	0.383 (0.148)**	1,050
log (households)	0.017 (0.072)	0.057 (0.035)	0.089 (0.080)	1,104
log (population)	−0.019 (0.058)	0.067 (0.054)	0.135 (0.070)*	1,105
Percent same house as five years ago	−0.001 (0.008)	0.020 (0.013)	0.020 (0.013)	1,105
Percent vacant houses	0.033 (0.014)**	0.002 (0.014)	−0.012 (0.017)	1,104
log(hourly wage) − 0.25 log(rent)[a]	0.083 (0.046)	0.171 (0.068)**	0.128 (0.071)	1,095

(*Continued*)

Table A3—Robustness Checks (*Alternative samples*) (*Continued*)

Model	Naïve (1)	OLS (2)	PW (3)	Observations (4)
Panel C. Including Supplemental Empowerment Zones—SEZs				
log (jobs)—LBD	0.132	0.198	0.218	1,718
	(0.039)***	(0.049)***	(0.062)***	
log (establishments)	0.055	0.069	0.075	1,718
	(0.025)	(0.020)***	(0.034)***	
log (average earnings per worker)	−0.033	0.000	0.003	1,718
	(0.015)**	(0.016)	(0.020)	
log (jobs)—JTW	0.187	0.150	0.131	1,724
	(0.050)**	(0.051)**	(0.069)**	
log (zone jobs held by zone residents)	0.131	0.150	0.180	1,720
	(0.074)	(0.057)**	(0.089)**	
log (zone jobs held by nonresidents)	0.174	0.116	0.085	1,724
	(0.042)**	(0.054)*	(0.061)	
log (non-zone jobs held by zone residents)	0.045	0.093	0.134	1,722
	(0.049)	(0.052)	(0.050)*	
log (weekly wage income of zone residents)[a]	0.011	0.052	0.051	1,721
	(0.028)	(0.014)**	(0.029)*	
log (weekly wage income of zone workers)[a]	−0.010	0.020	0.017	1,720
	(0.020)	(0.022)	(0.026)	
log (weekly wage income of zone residents working in zone)[a]	0.072	0.128	0.122	1,714
	(0.039)*	(0.043)***	(0.046)**	
log (weekly wage income of nonresidents working in zone)[a]	−0.011	0.005	0.010	1,711
	(0.021)	(0.025)	(0.028)	
log (weekly wage income of zone residents working outside zone)[a]	−0.004	0.049	0.058	1,707
	(0.023)	(0.022)*	(0.025)**	
log (rent)[a]	−0.003	0.022	0.032	1,721
	(0.037)	(0.026)	(0.041)	
log (housing value)[a]	0.324	0.301	0.335	1,648
	(0.110)**	(0.052)***	(0.126)**	
log (households)	0.023	0.016	0.035	1,721
	(0.061)	(0.032)	(0.063)	
log (population)	0.009	0.040	0.069	1,724
	(0.049)	(0.030)	(0.053)	
Percent same house as five years ago	−0.002	−0.002	−0.007	1,724
	(0.009)	(0.010)	(0.011)	
Percent vacant houses	0.022	−0.001	−0.004	1,721
	(0.012)*	(0.009)	(0.012)	
log(hourly wage) − 0.25 log(rent)[a]	0.070	0.119	0.111	1,707
	(0.038)*	(0.046)***	(0.045)**	

Notes: "Adjusted" outcomes controls for demographic changes at the micro-level (see Section IV). Timing: Variables labeled as LBD are analyzed over the period 1992–2000, all other outcomes are analyzed over the period 1990–2000. The estimation sample includes the baseline sample and the two Supplemental EZs as treated (Los Angeles and Cleveland). Estimators: Column 1 labeled "Naïve" report a DD estimate without controls. Column 2 labeled "OLS" report the OLS DD estimate controlling for lagged city and tract level characteristics. Column 3 labeled "PW" report parametric reweighting DD estimates. Asymptotic standard errors are shown in parentheses and are clustered by city. Asterisks reflect significance level obtained by a clustered wild bootstrap-t procedure described in Appendix A.

[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).

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Sources: Variables marked as JTW are based on the Journey-to-Work component of the Decennial Census. Variables marked as LBD come from the Longitudinal Business Database. All other tract level covariates come from the census.

Figure A1. Chicago Empowerment Zone

Source: US Department of Housing and Urban Development.

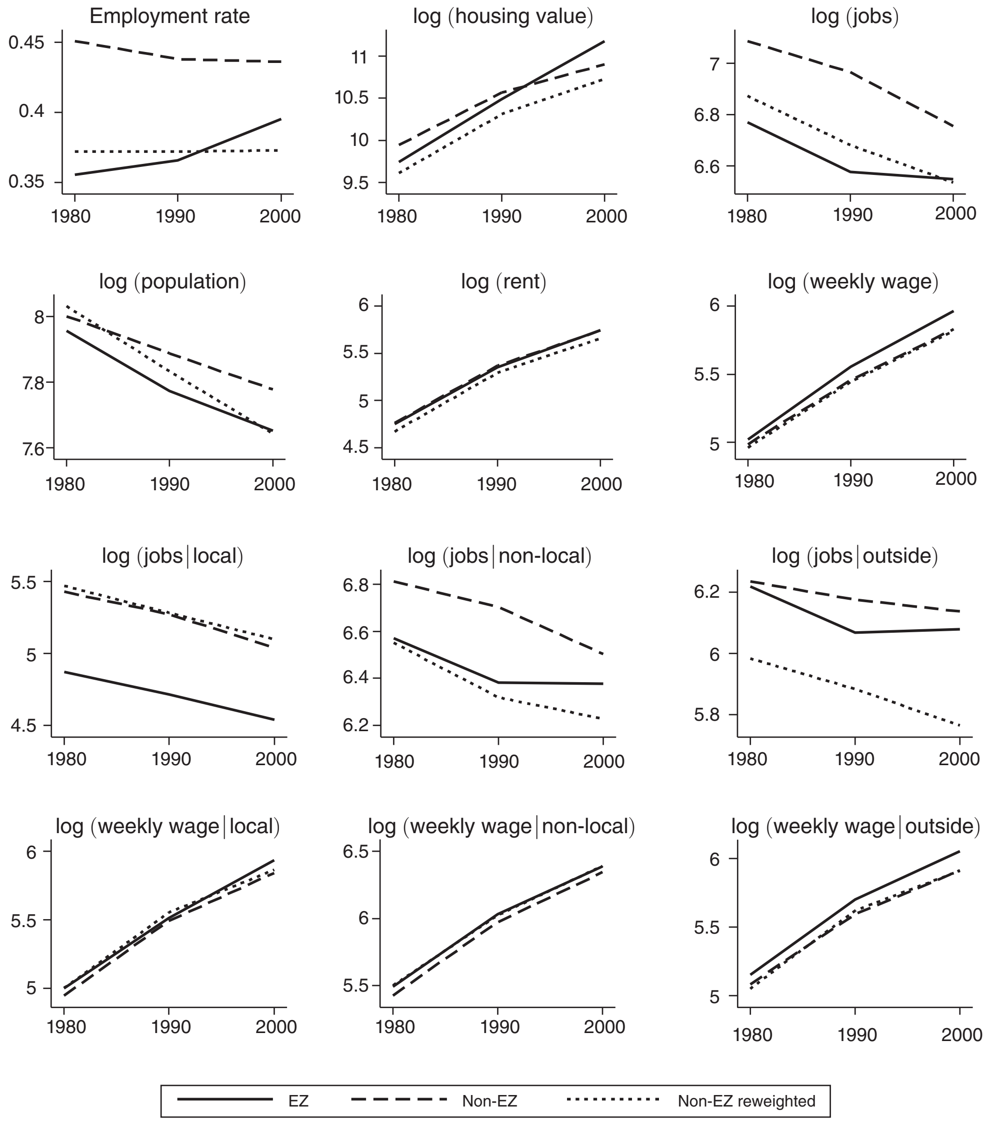

Figure A2. Means by Year and Treatment Status

Notes: Local refers to workers who live and work inside an EZ; non-local refers to workers that live outside but work inside an EZ; outside refers to workers who live in but work outside an EZ. Table 3B presents wild bootstrap *p*-values for a test of the null hypothesis that the pre-treatment levels and trends are the same for the first six variables. For the last six variables, the wild bootstrapped *p*-values of the reweighted difference between 1980 and 1990 are as follows: log (jobs | local): 0.508; log (jobs | non-local): 0.817; log (jobs | outside): 0.783; log (wage | local): 0.650; log (wage | non-local): 0.622; log (wage | outside): 0.650.

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