

# Results of the federal urban Empowerment Zone program

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A growing number of “place-based” policies target economic support to specific geographic areas, rather than to individuals. Economists have traditionally expressed skepticism that these programs actually benefit the residents of communities receiving support.<sup>1</sup> Indeed, standard economic models of spatial equilibrium suggest mobile workers and firms will take advantage of 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 examine these predictions by evaluating the economic effects of Round I of the federal urban Empowerment Zone program, one of the largest place-based policies in the United States.<sup>2</sup> Our findings build on an active literature on smaller, state-level programs.<sup>3</sup>

## The Empowerment Zone program

The federal Empowerment Zone program is a collection of tax incentives and block grants designed to encourage economic, physical, and social investment in the neediest urban areas of the United States. Round I of the program began in 1993, with the Department of Housing and Urban Development assigning Empowerment Zone status to poor neighborhoods in six metropolitan areas: Atlanta, Baltimore, Chicago, Detroit, New York City, and Philadelphia-Camden. Two additional cities, Los Angeles and Cleveland, received “supplemental” Empowerment Zone designation, while 49 other cities that had applied for Empowerment Zone designation were instead awarded smaller enterprise communities.<sup>4</sup> Table 1 shows characteristics of the six selected communities. On average, each Round I Empowerment Zone covered 10 square miles, had a population of 113,340, and a 1990 poverty rate of 48 percent.

Empowerment Zone designation brought with it a host of benefits. Two of the most important were an employment tax credit provided to local firms and a large Social Service Block Grant designed to facilitate local investment. Empowerment Zone designation entitled local employers to a credit of up to 20 percent of the first \$15,000 in wages paid to each employee who lived and worked in the community. This tax

credit was available to a business for up to ten years, with the maximum annual credit per employee declining over time. Since the average Empowerment Zone worker in 1990 earned only around \$16,000, this \$3,000 credit represented a substantial subsidy.

Participation in the tax credit program appears to have been incomplete, and most common among large firms (that were more likely to have positive taxable income).<sup>5</sup> Approximately \$200 million in employment credits was claimed from 1994 through 2000, with the amount claimed each year steadily increasing over time. In 2000, nearly 500 corporations and over 5,000 individuals claimed Empowerment Zone employment credits for a total of around \$55 million.<sup>6</sup>

Each Empowerment Zone was also eligible for \$100 million in Social Service Block Grant funds. These funds could be used in a variety of ways, including for business assistance, infrastructure investment, physical development, training programs, youth services, promotion of homeownership, and emergency housing assistance. By 2000, the first round Empowerment Zones had spent approximately \$400 million in block grant funds.

Together, the six Round I Empowerment Zones constitute a 60 square mile area with fewer than 700,000 residents. Federal expenditures on wage credits and block grants amounted to approximately \$850 per capita over the first six years of the program, from 1994 through 2000.

## Incentive effects of Empowerment Zones

The Empowerment Zone subsidies stimulate the demand for labor and land in targeted areas. This may result in both costs and benefits for workers and residents living inside and outside the zone, and also for zone landlords. The tax credits raise the value to Empowerment Zone firms of employing workers who live in the zone. For example, a firm that could profitably employ a local worker for \$15,000 in the absence of the subsidy, can employ the same worker for \$18,000 when offered a \$3,000 employment tax credit. In simple models with competitive labor markets, this leads to an increase in the wages paid by Empowerment Zone firms to local workers of \$3,000 per year. Effectively, the wage credit is an income transfer to local workers.

The block grants may also raise wages, by making local firms more productive through infrastructure investments and initiatives promoting safety and other local public goods. These productivity improvements should transfer into the wages of all zone workers whether they live in the zone or not.

**Table 1**  
**1990 Characteristics 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)
Atlanta	395,337	37	43,792	58%	20%	8.1
Baltimore	736,014	13	72,725	42	16	7.1
Chicago	2,783,484	3	200,182	49	28	14.3
Detroit	1,027,974	7	106,273	47	28	19.5
New York	7,320,621	1	204,625	42	18	6.3
Philadelphia-Camden	1,594,339	5	52,440	50	23	4.3

If workers were immobile, these wage increases would be the end of the story, and we could simply compare the cost of the program to the total effect on earnings inside the Empowerment Zone. However, people can easily change neighborhoods. If households move into the zone in pursuit of the local benefits generated by Empowerment Zone designation, the price of housing may rise. In such a case, the transfer to local residents will be captured in part by zone landlords.

While household mobility can yield unintended consequences, it is unclear how many households would be persuaded to move into a distressed neighborhood by the prospect of a \$3,000 earnings subsidy. This depends on the distribution of household preferences; if all households share the same valuation of neighborhood amenities, then movement into the zone will raise housing prices until the values of living inside and outside the zone are equalized. However, if households differ substantially in their valuation of neighborhoods, then it is possible that only a few will be willing to move into an Empowerment Zone in response to subsidies.

Central to our empirical analysis then are the following questions: (1) how many additional jobs are created in Empowerment Zone neighborhoods? and (2) how much does the local cost of living rise in response to Empowerment Zone designation? Intuitively we know that if many new jobs are created, then the population being subsidized will change. While some of those receiving new jobs may be prior zone residents, many of them are likely to be outsiders who moved into the zone. In either case, local job creation is a sign that government policy has substantially shifted the spatial distribution of jobs, which economists usually consider a sign of inefficiency unless there are important preexisting distortions in the labor market (e.g., from the minimum wage or payroll taxes).<sup>7</sup>

Effects of the program on the cost of living determine how much local landlords gain from zone designation, which depends on how easy it is to build and provide housing services in distressed neighborhoods. In many areas, the housing stock will be underutilized, in which case housing costs may not be very sensitive to population growth. But if regulations and land use restrictions make it difficult to build, then housing prices may rise substantially in response to Empowerment Zone designation.

## Evaluating the effects of Empowerment Zone designation

We now turn to our analysis of the empirical effects of Empowerment Zone designation. Our study covers the period from 1990 to 2000, which includes the first six years of the Empowerment Zone program (which began in 1994). To measure economic outcomes, we utilize confidential microdata from the Decennial Census and the Longitudinal Business Database (LBD).<sup>8</sup> These data provide two independent sources of information on local employment and allow us to separate the effects of Empowerment Zone designation on zone workers and zone residents.

Our research design for isolating the effects of the Empowerment Zone program is to compare the experience of census tracts in Round I Empowerment Zones to tracts with similar characteristics in rejected Round I and later round zones.<sup>9</sup> This approach has a number of advantages. First, tracts in both selected and rejected zones were nominated by their local governments, so, assuming that the nomination process was similar across cities, control tracts in rejected zones should be similar to those in selected zones on both observable and unobservable characteristics. Second, our control zones consist of contiguous clusters of poor census tracts, just like the actual Empowerment Zones. Finally, the majority of rejected and future zones are located in different cities than selected zones, which reduces the sensitivity of our estimates to geographic spillover effects.

Despite the advantages of using rejected tracts as controls, there may still be concerns that cities selected in the first round of the Empowerment Zone program differ in fundamental ways from those that were not selected. Table 1 shows that two of the three largest cities in the United States were selected to have Empowerment Zones; the other areas selected are large manufacturing-intensive cities. If large cities experienced fundamentally different conditions over the 1990s than did small cities, the comparison of census tracts in selected and rejected zones will be biased. To address these concerns, we conducted a number of robustness tests, including within-city comparisons and application of our research design to a set of false “placebo zones.” These exercises provided little evidence of bias.<sup>10</sup>

## Results

Using a difference-in-differences estimator, we compared changes over the 1990s in census tracts included in a Round I Empowerment Zone to changes over the same period in our control tracts. Some of our key results are provided in Table 2. We find that Empowerment Zone designation created jobs in zone neighborhoods, and that earnings increased substantially for local workers. Although housing prices rose, there is little evidence of significant 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 somewhat. For example, we find a small increase in the proportion of college graduates in zone neighborhoods.

The fact that there is an effect on jobs but not on population suggests that while the distance workers are willing to commute may be relatively sensitive to changes in incentives, the residential choices of workers over the time period are quite rigid, presumably because zone neighborhoods remain less desirable places to reside in the eyes of most households. The evidence also suggests an important role for both the wage credit and block grant features of the Empowerment Zone program—though imprecise, our point estimates indicate Empowerment Zone designation raised the employment of both nonresident commuters and local residents.

## Societal value

Our empirical analysis indicates that Empowerment Zone designation generated important changes in local price levels and behavior. In order to assess the net economic consequences of these changes, we consider the effects of Empowerment Zone designation on program stakeholders. The program's benefits may be measured as the sum of the total earnings increase for zone resident workers and the earnings increase for nonresident commuters. 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.

Table 3 provides calculations converting our treatment effect estimates into dollar amounts. Our “baseline” scenario takes point estimates at face value, even when not statistically significant. To convey the uncertainty in our estimates, we also report a “pessimistic” scenario where effects are given their least favorable values within a 90 percent confidence interval.

Approximately 38,000 zone residents worked within a zone in 2000, with a payroll of approximately \$800 million. Our estimate of the program's effect on the wages of local workers is around 13 percent, which translates into a \$109 million increase in annual earnings for zone residents who work in the zone. This figure is above the \$55 million in wage credits paid in 2000. It is in fact possible for the wages of zone residents to rise by more than the total amount of credits, if

**Table 2**  
Selected Effects of Round I Empowerment Zone Designations, 1990–2000

Outcome	Estimated Effect
Log of Jobs (data from Longitudinal Business Database)	0.179***
Log of Jobs (data from U.S. Census)	0.145*
Log of Zone Jobs Held by Zone Residents	0.150
Log of Zone Jobs Held by Nonresidents	0.097
Log of Weekly Wage Income of Zone Residents	0.053**
Log of Weekly Wage Income of Zone Workers	0.017
Log of Weekly Wage Income of Zone Residents Working in Zone	0.133**
Log of Weekly Wage Income of Nonresidents Working in Zone	0.005
Log of Rent	0.006
Log of House Value	0.281**
Log of Population	0.028
Percentage Black	-0.011
Percentage with College Degree <sup>a</sup>	0.020***

**Notes:** Estimated impacts derived from regression-adjusted difference-in-differences model. Statistical significance levels based on a Wild bootstrap t-test are indicated as \*\*\* 1 percent; \*\* 5 percent; \* 10 percent. For more details, see M. Busso, J. Gregory, and P. Kline, “Assessing the Incidence and Efficiency of a Prominent Place Based Policy,” *American Economic Review* 103, No. 2 (2013): 897–947.

<sup>a</sup>Educational attainment was self-reported.

the block grants were productive, and our point estimates suggest that such productivity effects may have indeed been present. We found a statistically insignificant 0.5 percent increase in the wages of nonresident Empowerment Zone workers, but cannot rule out more substantial effects. For our pessimistic scenario, we set this effect to zero. We also failed to find significant increases in the wages of the 141,000 zone residents who in 2000 worked outside the zone. Our point estimate of a 3.3 percent increase in this group's weekly wages would yield approximately \$118 in additional annual earnings; in our pessimistic scenario, we set this effect to zero.

Potentially offsetting the estimated increases in the earnings of local workers is the possibility of small increases in housing rents. Approximately 190,000 Empowerment Zone households rented their dwellings in 2000, with total annual rental payments of \$900 million. Our estimates of the effect of Empowerment Zone designation on rents are small and not statistically significant, although the upper limit of a 90 percent confidence interval includes effects as large as 7.3 percent. A pessimistic interpretation of rent effects would amount to an aggregate transfer from renters to landlords of \$67 million per year. Thus, we conclude that, at least for local workers, the earnings increases associated with the program outweigh any increases in the cost of living.

Finally, an additional 46,000 Empowerment Zone households own their homes, which were worth a total of \$4.8 billion in 2000. Our estimates suggest that Empowerment

**Table 3**  
**Welfare Analysis**

	Total Workers/ People/ Households	Total Annual Pay- roll/Rents/ Hous- ing Value (in Billion \$)	Effect on Wages/ Rents/ Housing Values	Increase in Annual Payroll/Rents/ Housing Value (in Million \$)	
				Baseline Scenario	Pessimistic Scenario
<i>Total Effect of the Program On:</i>					
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
Nonresidents Working in Zone	365,918	14	0.005	69.9	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

**Notes:** “Baseline scenario” uses regression adjusted difference-in-differences estimates in computing effects. “Pessimistic scenario” uses a lower limit of 90 percent confidence intervals for effects on earnings of zone residents working in zone and housing values and upper limit of confidence interval for rent effects.

Zones boosted housing values by approximately 28 percent, which amounts to around \$1.35 billion in additional wealth. These estimates may be overstated because housing values are self-reported in our data. Thus, we also consider an alternative scenario where the housing value effects 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 values. This pessimistic scenario still yields a \$500 million windfall to owner-occupiers in the zone.

Taken together, 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.

## Conclusions

Our comparison of Empowerment Zone neighborhoods to rejected and future tracts revealed important effects of Empowerment Zone 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 market appear not to have been accompanied by dramatic changes in the local cost of living. Population and housing rents remained fairly 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 Empowerment Zone program appears to have successfully transferred income to a small spatially concentrated labor force. We caution, however, that our study provides only a short-run evaluation of the Empowerment Zone program. Administrative data indicate that participation in the Empowerment Zone tax credit program increased only gradually over time and 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 Empowerment Zone 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.<sup>11</sup>

Finally, we emphasize that many of our empirical estimates 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 and yield large population movement into those zones. Later round zones also utilize different combinations of benefits. While we find it plausible that the mix of large block grants and wage credits accompanying Empowerment Zones would yield different results than their smaller, state-level predecessors, more work is necessary to disentangle the effectiveness of various combinations of spatial subsidies. ■

<sup>1</sup>See, for example, E. L. Glaeser and J. D. Gottlieb, “The Economics of Place-Making Policies,” *Brookings Papers on Economic Activity* (Spring 2008): 155–239.

<sup>2</sup>This article is based on M. Busso, J. Gregory, and P. Kline, “Assessing the Incidence and Efficiency of a Prominent Place-Based Policy,” *American Economic Review* 103, No. 2 (2013): 897–947.

<sup>3</sup>See, for example, D. Neumark and J. Kolko, “Do Enterprise Zones Create Jobs? Evidence from California’s Enterprise Zone Program,” *Journal of Urban Economics* 68, No. 1 (2010): 1–19; and J. C. Ham, C. Swenson, A. Imrohoroglu, and H. Song, “Government Programs Can Improve Local Labor Markets: Evidence from State Enterprise Zones, Federal Empowerment Zones and Federal Enterprise Community,” *Journal of Public Economics* 95, No. 7–8 (2011): 779–797.

<sup>4</sup>Supplemental Empowerment Zones received block grants similar to those received by Empowerment Zones, but did not become eligible for tax credits until 1999. Enterprise communities did not receive tax credits, but did receive block grant funding and were eligible for tax-exempt bond financing.

<sup>5</sup>U.S. General Accounting Office, “Community Development: Businesses’ Use of Empowerment Zone Tax Incentives,” GAO/RCED-99-253, 1999; and S. Hebert, A. Vidal, G. Mills, F. James, and D. Gruenstein, “Interim Assessment of the Empowerment Zones and Enterprise Communities (EZ/EC) Program: A Progress Report,” Washington, DC: U.S. Department of Housing and Urban Development, Office of Policy Development and Research, 2001, at [http://www.huduser.org/publications/econdev/ezec\\_rpt.html](http://www.huduser.org/publications/econdev/ezec_rpt.html)

<sup>6</sup>U.S. General Accounting Office, “Community Development: Federal Revitalization Programs are Being Implemented, but Data on the Use of Tax Programs are Limited,” 04-306, 2004.

<sup>7</sup>A model where spatial hiring subsidies actually enhance efficiency is considered in P. Kline and E. Moretti, “Place Based Policies with Unemployment,” *American Economic Review* 103, No. 3 (2013): 238–243.

<sup>8</sup>Our analysis was conducted inside the Berkeley, Michigan, and Suitland Census Research Data Centers.

<sup>9</sup>Round I Empowerment Zones were awarded via a competitive application process. We were able to obtain the census tract composition of proposed Round I zones that were not selected for inclusion in the program. Two additional rounds of Empowerment Zones followed the initial Round I empowerment zones along with a set of large Renewal Communities with similar benefits. All of these zones were used as controls.

<sup>10</sup>See M. Busso et al., “Assessing the Incidence and Efficiency of a Prominent Place-Based Policy” for full details.

<sup>11</sup>This subject was studied in a different context by P. Kline and E. Moretti, “Local Economic Development, Agglomeration Economies, and the Big Push: 100 Years of Evidence from the Tennessee Valley Authority,” Unpublished manuscript, 2011.