
Rethinking Empowerment: Evidence from Local Empowerment Zone Programs

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Abstract

The legacy of the federal government's Empowerment Zone initiative is contested. The evidence undergirding the initiative's legacy, however, is based primarily on models that estimate national effects. We use an alternative evaluation strategy that places greater emphasis on local Empowerment Zones as distinct programs. Our findings show that several cities did produce improvements that likely can be attributed to the EZ initiative. The results, however, are not consistent across outcomes or cities. Our findings suggest that what happens locally is a vital concern for federal urban policy and also informative for local communities with responsibility for crafting and executing revitalization strategies.

Keywords

empowerment zones, urban revitalization, federal urban policy

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The legacy of the federal government's Empowerment Zone (EZ) initiative is contested. As the principal federal urban policy of the 1990s, the EZ and Enterprise Communities (EC) initiative was intended to create economic opportunities in distressed communities. Although several evaluations concluded that the changes in neighborhood conditions that were observed in EZ communities could not be attributed to the initiative, others have concluded that the combination of market-oriented tools and block grants produced measurable benefits. However, to date, the evidence undergirding the legacy of federal EZs has been primarily based on econometric models designed to estimate the initiative's national effects. The possibility that EZ program effects may vary locally has been largely overlooked.

Evidence about local effects is important because EZ programs and the outcomes they produced were often substantially different. This is especially true for the original round I EZs where flexible federal block grants enhanced the opportunities that communities enjoyed to create distinctive local programs. In addition, local capacity was important to program success because federal guidelines emphasized local programming and collaborative policy making. Consequently, it may be appropriate to think of EZs as distinct local programs spawned by the EZ/EC initiative as opposed to a uniform national program that was implemented in several sites. If so, examining local results is necessary to develop a comprehensive understanding of the initiative's performance and careful attention to local results is warranted to learn what worked at the local level and whether the initiative's effects were consistent across different settings, treatments, and outcomes.

The legacy of the EZ/EC initiative has important consequences for the ongoing development of federal urban policy. The initiative was a sharp break from the 1980s, when most place-based federal urban aid was terminated or subjected to significant funding reductions. It also ended more than a decade of gridlock in Washington over what role the federal government should play in urban areas. As originally conceived, EZs incorporated elements of a supply-side approach to economic revival (tax incentives, regulatory relief) as well as more traditional grants-in-aid assistance to support local programs and strategies targeted to distressed neighborhoods (e.g., economic development, affordable housing, workforce development, education, human services, public safety, etc.).

If the conventional wisdom concludes that the EZ/EC initiative failed, urban distress will seem less tractable and government's efforts to tackle urban problems will seem feckless and futile (see, e.g., Lemann 1994). On the other hand, if the initiative was efficacious in ways that were not readily detected or understood by evaluations that focused on national effects, "we risk

failures of proven models when they are spread without a clear understanding of the critical programmatic or contextual factors that accounted for the success of the original model,” as Smyth and Schorr (2009, p. 15) recently observed.

This article contributes to the legacy of federal EZs by examining the local programs and outcomes in the original six round I EZs—Atlanta, Baltimore, Chicago, Detroit, New York, and Philadelphia. Our findings show that although several local programs did produce improvements that likely can be attributed to the initiative, the results are not consistent across outcomes or cities. Rather than producing a consistent national effect, EZ programs produced disparate local outcomes.

Not a Typical Federal Program

According to the U.S. Department of Housing and Urban Development (HUD), the lead federal agency for the urban EZs, the initiative was “not a typical federal program” (HUD 1994a). The EZ/EC initiative was based on four principles: (1) expanding economic opportunity, (2) promoting sustainable community development, (3) fostering community-based partnerships, and (4) crafting a strategic vision for change. A distinctive feature of the initiative was its emphasis on promoting sustainable community development (Gittell et al. 1998). The application guide asserted that “economic development can only be successful when part of a coordinated and comprehensive strategy that includes physical development as well as human development” (HUD 1994a, 8). Thus, unlike federal enterprise zone proposals that were discussed in the early 1980s, which featured only tax incentives and regulatory relief, almost exclusively focused on businesses, and provided little—if any—attention to communities (Clarke 1982), the EZ initiative aimed to revitalize distressed communities.

The initiative’s guidelines suggested that targeted economic development programs should be crafted by a local process of comprehensive planning and program integration (Liebschutz 1995; Galston and Tibbetts 1994). In addition, “the community” was to be mobilized and included in the policy-making process (Gittell et al. 1998; Rubin 1994). The guidelines also implied that the problems of distressed urban communities required an ongoing, institutionalized response; it was insufficient merely to have ad hoc programs to address discrete problems. However, rather than imposing a standard program, federal guidelines encouraged cities to develop strategic plans to reflect local conditions and to create a governance process to ensure effective coordination and implementation of their plans (Rubin 1994).

The original six EZ designees each received a \$100 million block grant over a 10-year period that allowed local actors to plan deliberately and to develop a variety of programmatic strategies that reflected local opportunities, needs, and constraints. EZs also were eligible for \$150 million in federal tax credits (see HUD 2003) including an EZ wage tax credit that was worth as much as \$3,000 per employee, nonrecognition of capital gains on the sale of assets and partial exclusion of capital gains on stock sales, increased deductions for businesses that invest in machinery and equipment, and tax-favored bond financing (EZ facility bonds). Although many states had enacted enterprise zone programs in the 1980s that provided state and local tax incentives to stimulate development in distressed areas, the EZ initiative was the first time that federal tax incentives were available to promote revitalization in high-poverty neighborhoods. In addition, federal agencies were directed to give priority consideration to EZ communities that applied for regulatory relief or federal financial aid to support activities related to their local strategic plans (HUD 1994b).

Local EZ Programs

To describe the focus of local EZ initiatives, our analysis relied on information obtained from the Performance, Monitoring, Review, and Management Reports for each EZ city. Those data were supplemented with information gathered from local field research in selected EZ cities and reports from HUD-sponsored evaluations of the initiative (Wright et al. 1996, 1997; Hebert et al. 2001).¹

Because the EZ block grant funds were directly controlled by each city's implementing authority, they are the best indicator of each city's priorities. To identify the top local priorities, we assigned the EZ funds for each activity to a program category and then summed the value of the largest program categories in each city until the total proportion was at least 50% of the reported EZ allocations.²

In several EZ cities (Atlanta, New York, and Philadelphia) business development received a majority of EZ funds. In Baltimore, workforce development (41%) was the top priority, with business development (32%) second. Within cities where business development was a top priority, the means used to encourage economic opportunity varied. In Baltimore and Philadelphia, business development programs focused on providing access to capital by creating loan programs. In New York, emphasis was placed on funding a variety of specific projects as well as business finance. Atlanta also provided funds for business finance while underwriting two large redevelopment projects.

By contrast, the EZ programs in Chicago and Detroit emphasized human needs, with priority given to human services and housing. Chicago allocated \$34 million in EZ funds to 72 different human services programs and an additional \$19.5 million to 23 housing programs. Detroit awarded \$60 million of its EZ funds to human services programs.

The variation in local priorities suggests that there was no national EZ program. The EZ/EC initiative sparked very different local strategies that used a variety of different programs to achieve their objectives.

The Local Context

In addition to variation in local strategies and programs, there were important differences in the institutional context through which zone programs were implemented (see Table 1). Chicago and Philadelphia chose to rely on existing city agencies, whereas Atlanta, Baltimore, Detroit, and New York placed responsibility for their EZ programs in newly created nonprofit corporations that stood outside the formal structure of city government. The thinking in these cities was that a separate corporate entity would have greater flexibility for getting programs up and running quickly. Participants also observed that placing responsibility for EZ program implementation in a nonprofit entity would depoliticize the EZ and make it easier to leverage private funds.

In addition, EZ governing entities varied in terms of their degree of decentralization of decision making. Baltimore, New York, and Philadelphia created two-tiered governance entities; the first tier focused on zone-wide issues and the second tier dealt with issues unique to each zone (all three cities had zones composed of multiple, noncontiguous areas). The two-tiered structure allowed project and program designs, funding priorities, and agencies or organizations to carry out EZ activities to vary within individual zones. Atlanta, Chicago, and Detroit, on the other hand, relied on a single-tier governance structure.

All EZ cities included representation for residents and community-based organizations in their governance process. According to HUD's interim outcomes report (Hebert et al. 2001), the extent of resident or neighborhood representation on EZ governing boards as of 2000 (the initiative's midpoint) varied from a low of 22% in New York to more than half of all board seats in Chicago, Detroit, and Philadelphia. Board representation, however, did not always translate into influence; the degree of citizen influence varied across cities regardless of the extent of zone neighborhood incorporation on local governing bodies (see Table 1). Also, field research indicated that in most cities resident influence waned as the program moved from planning to implementation (Wright et al. 1996, 1997; Hebert et al. 2001; Gittel et al. 1998; Gittel, Newman, and Francois Pierre-Louis 2001).

Table 1. Characteristics of Empowerment Zone (EZ) Cities and Their EZ Neighborhoods

	Atlanta	Baltimore	Chicago	Detroit	New York	Philadelphia
City demographics (1990) ^a						
Population (thousands)	394	736	2,783	1,027	7,322	1,585
% poverty	27.3	21.9	21.6	32.4	19.3	20.3
% unemployed	9.2	9.2	11.3	19.7	9.0	9.7
Zone demographics (1990) ^a						
No. of zones	1	3	3	3	2	3
Population	49,966	72,725	200,182	101,307	191,161	39,108
% poverty	54.7	41.8	49.1	47.9	43.2	52.1
% unemployed	17.5	15.0	24.6	28.9	17.7	23.7
Citizen influence						
Planning ^b	Determined	Determined	Major	Major	Major	Minor
Strategies	Major	Major	Major	Major	Minor and major	Major
Programs and activities						
Implementation ^c						
Overall resident influence	Active, but not influential	Influential	Active, but not influential	Active, but not influential	Present, but not active	Influential
Governance structure ^b						
Type	Nonprofit	Nonprofit	City govt.	Nonprofit	Mixed ^d	City govt.
Levels	Single tier	Two tiers	Single tier	Single tier	Two tiers	Two tiers
No. of board members ^c	17	30	14	25	45	41 ^e
% residents or community-based organizations ^c	47.0	40.0	62.0	52.0	22.0	54.0 ^e

a. Source: Census of Population and Housing (1990).

b. Source: Wright et al. (1996).

c. Source: Hebert et al. (2001).

d. The New York EZ is actually two separate EZs, each governed by a different organization. Responsibility for the South Bronx EZ was vested in the Bronx Overall Economic Development Corporation, an existing local government agency, and the Harlem EZ was overseen by a newly created nonprofit organization, the Upper Manhattan Empowerment Zone Development Corporation.

e. Combined totals for Philadelphia and Camden EZs.

Another important factor that was likely to affect program implementation was local capacity. Although all six EZ cities had extensive prior experience with federal urban programs (e.g., urban renewal, model cities, Community Development Block Grant, Urban Development Action Grant, etc.) their capacity for initiating and coordinating neighborhood revitalization efforts varied widely. One manifestation of this was the maturity of each city's community development industry. According to a study of community development corporations (CDCs) and their changing support systems that included 21 major cities (Walker 2002), both the size and quality of the CDC industry varied across the original six EZ cities. Baltimore was among the cities rated highest in terms of CDC industry quality, whereas the other EZ cities ranked in the middle range. In terms of CDC industry size, Chicago, New York, and Philadelphia all were ranked as large, Baltimore and Detroit were ranked in the midrange, and Atlanta was ranked as small.

Findings of the Existing Evaluations

Several evaluations of the EZ/EC initiative have been produced. Studies conducted by the Government Accountability Office (GAO 2006) and Oakley and Tsao (2006) concluded that while conditions did improve in some of the EZs, the changes could not be attributed to the initiative. Although both of these studies assessed the EZ/EC initiative with a combination of local and national analyses, their conclusions emphasized the national findings and treated evidence of local program effects as aberrations that were overshadowed by the lack of statistically significant national effects.

Several other evaluations using national models generally found positive, statistically significant effects across a range of outcomes (see Busso and Kline 2008; Ham, Imrohoroglu, and Swenson 2009; Hanson 2009; Krupka and Noonan 2009). However, these evaluations did not examine the local effects of the EZ initiative, nor did their designs allow them to comment on how—if at all—EZ effects may have varied across settings, treatments, and outcomes (see the online appendix, available at <http://oucp.emory.edu/ourwork/research/ez.html>, for a summary of these studies).

Evaluation Strategy and Program Design

Urban revitalization initiatives are difficult to evaluate because urban communities are open, dynamic systems. Several features of the EZ initiative further complicate the task of evaluation. First, local program activities were designed to overlap and complement one another to craft a comprehensive

attack on numerous, related urban problems (Liebschutz 1995). Even when changes in conditions are observed in the zones, it is difficult to know which particular program activities or combinations of program activities are responsible. Second, many EZ policy tools, especially market-oriented tools such as tax incentives, are indirect (Rubin 1994), which makes it more difficult to link program activities and outcomes. Third, in theory, local interventions were expected to be synergistic: For example, reducing crime was expected to encourage investment; investment was expected to generate job growth; job growth was expected to increase incomes and housing values as residents purchased homes in the zone to live near where they work, encouraging the development of a stable community and creating new markets for consumer-oriented businesses. Finally, by encouraging local planning, HUD allowed substantial variation among local EZ programs, making it difficult to have a common evaluation framework that could be applied across sites.

Many of these problems simply cannot be surmounted. In particular, the initiative's design precludes linking outcomes to discrete program activities because many different programs that were expected to produce synergies were simultaneously implemented and the level of information on individual activities HUD required EZ cities to report was not very detailed (see the online appendix for further discussion). Therefore, it is practically impossible to establish links between outcomes and specific programs or activities.

Although many elements of the EZ/EC initiative complicated the task of evaluation, one aspect of its design did make evaluation more feasible. Federal guidelines required that the geography of each city's EZ correspond to census tracts. By comparing data aggregated on the census tract level at different points in time for census tracts in EZs (treatment) to data for comparable census tracts outside the zones (control), a quasi-experimental design can be developed to more rigorously evaluate the effects of the EZ initiative.

Comparing Evaluation Methods

There are four important ways in which our evaluation approach differs from the existing evaluations: (1) We conceive of the EZ initiative as a collection of local programs rather than a uniform national program, (2) we analyze a different (though overlapping) set of cases, (3) we use multiple measures and methods to operationalize our constructs, and (4) we use a different analytic strategy to estimate program effects.

Treatment group. There is wide variation across the existing evaluations in terms of what constitutes an EZ, and thus the composition of the treatment

group varies across studies. Most studies include a different set of cities than we do. Our decision to focus on the original six EZs is based on concerns about the construct validity of the treatment—that is, ensuring comparability of available policy tools across EZ neighborhoods. Some studies (GAO 2006; Busso and Kline 2008) included Cleveland and Los Angeles. While these cities were designees as “supplemental” EZs in round I, the federal resources and policy tools available to the original six EZ cities were different from what was awarded to Cleveland and Los Angeles (e.g., their federal funds came from a different source that permitted only economic development as an eligible activity, and these cities did not have access to the federal tax incentives in the early years of their initiatives).

Oakley and Tsao (2006) examined four of the original six EZs, eliminating Atlanta and Philadelphia. Although Oakley and Tsao note that Atlanta’s EZ program was terminated when the city received Renewal Community (RC) designation in 2001 (before the EZ designation had expired), Atlanta’s EZ activities continued by virtue of a special agreement with HUD that allowed the city to continue to expend its remaining EZ funds in EZ-designated neighborhoods. In addition, Atlanta’s RC program did not get off the ground until 2005. Consequently, we included Atlanta in our set of EZ cases.

Oakley and Tsao (2006) also eliminated the Philadelphia–Camden EZ because it was a bistate zone. We have taken a different tack in addressing this difficulty. Rather than eliminate the Philadelphia–Camden zone entirely, we have included only the Philadelphia part of the zone as one of our cases. Despite apparent federal intentions to encourage interstate collaboration, the two cities’ EZs were essentially independent initiatives. Each city had its own budget (Philadelphia received \$71 million of the \$100 million) and separate governing entities, strategies, and programs.

Krupka and Noonan (2009) included both round I and round II EZs in their study. Although round II EZs—designated in 1998—were authorized to receive as much as \$100 million in federal grant support (as did round I EZs), the funding came from HUD rather than Social Services Block Grants and was subjected to the annual appropriations process rather than authorized in one bill. Although round II urban EZs anticipated grants of \$10 million per year throughout their 10-year designation period, the actual amount appropriated was far less, with each EZ receiving only about \$24 million between 1999 and 2003 (Stoker and Rich 2006).

Control group. A key requirement for evaluating the effects of the EZ initiative is to establish a counterfactual to estimate what would have happened in EZ neighborhoods in the absence of the intervention. Like many of the existing evaluations, we use propensity score matching to construct a control group

of census tracts for each city (Rosenbaum and Rubin 1983; Oakes and Johnson 2006). We selected control census tracts from the set of all other *eligible* tracts *within the city*.

By contrast, GAO selected their control tracts from all census tracts within a 5-mile radius of EZ tracts, which included tracts that may not have been eligible and tracts located in other local government jurisdictions. Busso and Kline (2008) and Hanson (2009) used the proposed zone census tracts in rejected round I EZ cities and the designated zone tracts in cities that were selected as EZs, ECs, or RCs in subsequent rounds.³ Both studies note that the use of census tracts in different cities than the treated zones allows one to control for spillover effects that may increase or diminish estimates of the effects of EZ designation. Krupka and Noonan (2009) used zone tracts in cities that were selected as EZs in round III. They contend that the round III zone tracts are likely to be comparable to the round I EZ tracts in terms of their unobservable characteristics. Ham, Imrohoroglu, and Swenson (2009) used non-EZ/EC/RC census tracts to constitute three different sets of control groups: one based on the nearest control tract to a zone tract, one based on all tracts that were contiguous to zone tracts, and one that included all nonzone tracts in the state.

We believe there are two general problems with the control groups used in the other studies. First, including census tracts that received other variations of the zone treatment weakens the construct validity of the control group and diminishes the ability to detect an effect. Second, using census tracts in different cities as controls for EZ tracts may overlook important differences in the local context (e.g., city and regional economy, political and institutional context, strength of community development industry, etc.) that may diminish the comparability between treatment and control census tracts. This concern is especially salient since Busso and Kline (2008) found no evidence of spillover effects in their analysis.

Data and indicators. All of the existing evaluations used census-derived indicators of neighborhood outcomes, which limited measurement to two points in time, 1990 and 2000. Neither date aligns well with the timing of the implementation of the EZ initiative. The baseline measure is five years prior to the start of the EZ initiative, and the postintervention measurement is at the midpoint of the EZ intervention. Beyond this, EZ cities varied widely in terms of how quickly they were able to translate their plans into action, making it unlikely that all of the EZ cities were at comparable points in the implementation of their initiatives when the 2000 census was conducted. The GAO (2006) report, however, also used two indicators of economic growth from a private, proprietary source (number of business establishments and number of jobs) at three time points (1995, 1999, 2004).

Although we also used some census-derived indicators of neighborhood outcomes, we included multiple measures (jobs, poverty, unemployment, housing investment, and business investment) that we believe capture a broader range of neighborhood conditions. We also derive those measures from multiple sources, which limits plausible threats to validity from mono-method bias, and for multiple time points, which allows better alignment between the time period for measuring effects and the time period of the intervention (see the online appendix for a discussion of data sources and indicators).

Estimating effects. Perhaps the most important feature of our evaluation design is how we estimate program effects. The GAO (2006) and Oakley and Tsao (2006) studies used econometric models that treated the EZ initiative as a “program” that was implemented at several different sites across the country. Consequently, they pooled census tracts across the different cities and compared the mean outcomes for tract areas within the EZs to matched tracts outside the zones. Both of these evaluations also included local statistical models that analyzed outcomes in each city separately but downplayed findings that were inconsistent with the national models.

All of the other studies cited in our review also used a regression-based approach for estimating national EZ effects, generally employing some variation of a difference-in-differences estimation in comparing outcomes in EZ census tracts to those in their control groups. However, these studies widely varied in terms of how the effects were defined and estimated as well as how counterfactuals were used in deriving those estimates (see the online appendix for further discussion).

We chose neither to utilize a regression-based analysis strategy nor to pool our data across sites. Instead, we used propensity score matching to construct comparable matched pairs of treatment and control census tracts for each EZ city and estimated the average treatment effect on the treated tracts (ATT). We believe this method more closely approximates the real-world contrasts between treatment and control conditions than regression-adjusted estimates. As Oakes and Johnson (2006, 374) point out, regression strategies fail to address the problem of off-support inference: “Averages and other statistical procedures that summarize information may end up obscuring fundamental differences between considered objects.”⁴

We chose not to pool our data because we believe it is more appropriate to think of the EZs as a series of community trials. As Hannan (2006, 349) observes, “The test of the hypothesis that the intervention had an effect must compare variation at the condition level [treatment, control] against the variation at the unit level [cities]—not against the variation at the member [census tracts] level.”⁵

For each of the original six EZ cities, we included in our analysis all EZ census tracts and all census tracts in each city that were not included in an EZ but met the federal requirements for designation. Propensity scores were calculated using Stata's PSMATCH2 module for the included census tracts in each city based on 21 covariates (see the online appendix for a detailed discussion of this procedure). Although 85 of the 126 comparisons between EZ and control census tracts were statistically significant prior to matching, only 3 comparisons remained statistically significant after matching (all between $p < .05$ and $p < .10$), and none of these were for our critical variables (see the online appendix, Tables A-3–A-8).

Having established group comparability, the next step in our analysis was to calculate the ATT for our outcome measures, which was estimated as follows,

$$\widehat{ATT} = \frac{\sum y_{e_i} - y_{u_i^*}}{n_e} = \frac{\sum \Delta_e}{n_e} = \bar{\Delta}_e,$$

where y_{e_i} is the outcome (e.g., $POV_{2000} - POV_{1990}$) for an exposed (e) census tract (i), $y_{u_i^*}$ is the outcome of the matched unexposed census tract (counterfactual substitute), n_e is the sample size of the matched pairs, and Δ_e is the pair-matched difference in outcomes.

Local Evaluation Results

Table 2 presents the ATT estimates and their standard errors, significance levels, and confidence intervals for our five outcome measures: (1) the percentage change in the number of jobs, 1996–2004; (2) the percentage change in the number of persons with income below the poverty level, 1989–1999; (3) the percentage change in the number of persons unemployed, 1990–2000; (4) the three-year moving average trend in the constant dollar value of home mortgage loans, 1994–2006; and (5) the three-year moving average trend in the constant dollar value of business loans, 1996–2006. The ATT estimates were bootstrapped with each bootstrap draw yielding a reestimation of the effects, including the initial steps of estimating the propensity scores and common support. For each city, the bootstrapping analysis included 1,000 samples, with each sample producing an ATT estimate and the distribution of these sample means yielding the bootstrapped standard error.

In all of the EZ cities except Atlanta, Table 2 shows a positive effect for employment change, indicating that, on average, the percentage change in

Table 2. Empowerment Zone Effects by City

	ATT	Bootstrap SE	z	P > z	Normal-based 95% CI
Atlanta (n = 16 matched pairs)					
Employment change, 1996–2004	-29.65	49.73	-0.60	.55	-127.11 67.81
Poverty change, 1990–2000	-17.49	32.85	-0.53	.59	-81.87 46.88
Unemployment change, 1990–2000	22.43	53.33	0.42	.67	-82.09 126.97
Home mortgage trend, 1994–2006	-889.31	909.71	-0.98	.33	-2672.31 893.67
Business lending trend, 1996–2006	-8.75	197.67	-0.04	.97	-396.17 378.67
Baltimore (n = 20 matched pairs)					
Employment change, 1996–2004	7.34	26.95	0.27	.79	-45.48 60.17
Poverty change, 1990–2000	-14.51	14.52	-1.00	.32	-42.97 13.96
Unemployment change, 1990–2000	-30.55	29.22	-1.05	.30	-87.83 26.73
Home mortgage trend, 1994–2006	102.58	610.21	0.17	.87	-1093.41 1298.57
Business lending trend, 1996–2006	118.97	106.55	1.12	.26	-89.86 327.79
Chicago (n = 68 matched pairs)					
Employment change, 1996–2004	13.83	13.56	1.02	.31	-12.74 40.41
Poverty change, 1990–2000	0.75	9.40	0.08	.94	-17.68 19.17
Unemployment change, 1990–2000	-6.91	12.54	-0.55	.58	-31.49 17.68
Home mortgage trend, 1994–2006	-197.57	588.54	-0.34	.74	-1351.10 955.96
Business lending trend, 1996–2006	52.94	52.76	1.00	.32	-50.47 156.36
Detroit (n = 34 matched pairs)					
Employment change, 1996–2004	13.00	15.94	0.82	.42	-18.24 44.25
Poverty change, 1990–2000	-0.84	8.55	-0.10	.92	-17.59 15.92
Unemployment change, 1990–2000	10.97	14.89	0.74	.46	-18.21 40.15
Home mortgage trend, 1994–2006	-35.1	123.52	-0.28	.78	-277.19 206.99
Business lending trend, 1996–2006	0.85	48.84	0.02	.99	-94.87 96.57

(continued)

Table 2. (continued)

New York (<i>n</i> = 45 matched pairs)									
Employment change, 1996–2004	25.89	18.92	1.37	.17	–11.19	62.96			
Poverty change, 1990–2000	5.19	11.08	0.47	.64	–16.52	26.90			
Unemployment change, 1990–2000	28.46	18.93	1.50	.13	–8.65	65.56			
Home mortgage trend, 1994–2006	279.89	326.79	0.86	.39	–360.61	920.39			
Business lending trend, 1996–2006	42.05	68.78	0.61	.54	–92.75	176.85			
Philadelphia (<i>n</i> = 8 matched pairs)									
Employment change, 1996–2004	13.86	46.39	0.30	.77	–77.07	104.79			
Poverty change, 1990–2000	–12.79	18.03	–0.71	.48	–48.11	22.54			
Unemployment change, 1990–2000	–32.22	40.25	–0.80	.42	–111.11	46.68			
Home mortgage trend, 1994–2006	40.44	795.24	0.05	.96	–1518.19	1599.08			
Business lending trend, 1996–2006	95.73	92.58	1.03	.30	–85.72	277.18			

Note: ATT = average treatment effect on the treated tracts; CI = confidence interval.

Table 3. How Empowerment Zone Neighborhoods Fared Compared to Their Control Group Counterparts

City	Jobs	Poverty	Unemployment	Housing investment	Business investment
Atlanta	Worse	Better	Worse	Worse	Worse
Baltimore	Better	Better	Better	Better	Better
Chicago	Better	Worse	Better	Worse	Better
Detroit	Better	Better	Worse	Worse	Better
New York	Better	Worse	Worse	Better	Better
Philadelphia	Better	Better	Better	Better	Better

the number of jobs between 1996 and 2004 was higher in EZ census tracts than was the case for the control census tracts. A similar pattern was found for business investment; the same five cities achieved a steeper trajectory for business investment between 1996 and 2006 in their EZ census tracts than was the case for the control census tracts. Atlanta reported a negative outcome.

Table 2 shows that the percentage change in the poverty population between 1989 and 1999 was lower in four of the six EZ cities (Atlanta, Baltimore, Detroit, and Philadelphia). This indicates that EZ census tracts in those cities experienced either a decrease or a smaller increase in their poverty populations than was the case for the control census tracts. Three EZ cities (Baltimore, Chicago, and Philadelphia) showed a positive effect (decrease or smaller percentage increase) for the change in the percentage of persons unemployed between 1990 and 2000. In all three of these cities the effects for unemployment change were greater than those achieved for poverty change. Finally, three EZ cities (Baltimore, New York, and Philadelphia) achieved positive outcomes for housing investment.

Table 3 summarizes how EZ neighborhoods fared in comparison to their control group counterparts. The findings suggest that Baltimore and Philadelphia had the most effective EZ initiatives; both cities recorded better outcomes for their EZ census tracts than for their control census tracts, on average, across all five outcome measures.

Table 3 also suggests that the EZ initiatives were more effective at improving economic conditions than was the case for other outcomes. Five of the six cities (all but Atlanta) reported gains for their EZ census tracts for the employment change and business investment indicators. However, only four cities achieved better outcomes for poverty reduction, and only three reduced unemployment and increased housing investment.

There is little evidence to suggest that the strategic and programmatic focus of EZ local programs directly translated into improved outcomes. While the data show that many of the cities that focused on business development had better outcomes related to job creation and business investment, this pattern does not hold for Atlanta. On the other hand, cities with local EZ programs that emphasized human services (Chicago, Detroit) did not always see greater reductions in poverty and unemployment; the findings in both cities were mixed.

Cities that were more effective in increasing zone business awareness and utilization of the EZ tax incentives were more likely to generate positive outcomes. Hebert and colleagues (2001) included as part of the HUD-sponsored interim outcome assessment a survey of zone businesses to assess their knowledge and utilization of EZ tax incentives. Baltimore and Philadelphia were the two cities that ranked highest in terms of business awareness and utilization of EZ tax benefits. These were also the cities that had positive outcomes across all five outcome indicators. Atlanta, on the other hand, ranked at or near the bottom among the six EZ cities in terms of business awareness and utilization of EZ incentives and had worse outcomes in EZ neighborhoods than control neighborhoods for four of the five indicators examined.

Finally, it is important to note that although the program effects we report in Tables 2 and 3 are not statistically significant, they are robust. Although we used a different evaluation strategy and a broader set of outcome measures, our findings in Atlanta, Baltimore, and Philadelphia directly align with those reported by GAO (2006) and Oakley and Tsao (2006), the other evaluations that examined local outcomes (see the online appendix for further discussion). For Detroit, we find similar effects for poverty and economic growth but differ on unemployment. The findings in Chicago and New York are mixed across the three studies.

Statistical Conclusion Validity

As Shadish, Cook, and Campbell (2002) point out, confidence in the inferences drawn from causal studies is dependent on the ability to rule out plausible alternative explanations that could weaken claims about the presence (or absence) of a causal effect.⁶ Our study has weak statistical conclusion validity primarily because the matched-pair sample sizes in the EZ cities are simply too small to achieve statistical significance.

While the interpretation of effect size is a subjective judgment premised on the nature of the intervention and the state of knowledge about the program, the effect sizes required to attain statistical significance in the EZ initiative

are well beyond what most observers would consider reasonable. Based on Cohen's (1988) classification, all six EZ cities would need to attain moderate ($d \geq .3$) to large ($d \geq .5$) effects to attain statistical significance, though it is highly likely policy makers, program officials, and beneficiaries would accept smaller effects as substantively meaningful evidence of the efficacy of the EZ initiative.

The usual way to mitigate this problem is to aggregate data across sites to increase the number of observations and boost statistical power, as did Hanson (2009; $N = 1,262$), Krupka and Noonan (2009; $N = 106,957$), and the GAO (2006; $N = 866$) and Oakley and Tsao (2006; $N = 953$) in the development of their national models. However, this procedure assumes that the intervention is consistent across sites, a claim that we have rejected on the basis of our analysis of local program activities and the results of extensive field research (Hebert et al. 2001; Wright et al. 1996, 1997). In addition, combining poor and good local performers in the same model to estimate national program effects dilutes and masks potentially important local effects.⁷

Our view is that the nature of the EZ initiative and the limitations of evaluation research make the usual tests of statistical significance insurmountable hurdles in this context that fail to distinguish what works from what does not. Although this is a subjective judgment, we think it is consistent with the concerns expressed by Shadish, Cook, and Campbell (2002, 43), who note, "Because the implication of nonsignificance is that a cause and effect do not covary—a conclusion that can be wrong and have serious consequences—threats to statistical conclusion validity are partly about why a researcher might be wrong in claiming not to find a significant effect using null hypothesis significance testing." Beyond this, they observe, "Some scientists wrongly think that nonsignificance implies a zero effect when it is more often true that such effect sizes are different from zero."

Lessons Learned from the EZ Initiative

Several local EZ programs produced improvements in EZ neighborhoods that can likely be attributed to the initiative. These local gains were detected by three independent evaluations. Although the evaluation results show that several EZs achieved measurable improvements in zone conditions, the gains were modest. None of the local EZ programs brought about a fundamental transformation of distressed urban neighborhoods. However, given the problems of evaluating urban revitalization initiatives, these gains are noteworthy and deserve further exploration.

Sound urban policy must be built on a clear understanding of what works and why. As the conventional wisdom surrounding the EZ initiative develops, the results attained by local EZ programs should be acknowledged. In light of the gains achieved in several EZ cities, to conclude that the EZ initiative was not successful would be a bum rap (especially in Baltimore and Philadelphia, sites that showed gains on all five of the indicators included in our evaluation). On the other hand, to conclude that the EZ initiative worked without explaining why will make sound urban policies more difficult to replicate. A key question that warrants further attention is why positive results were achieved in some settings, treatments, and outcomes but not in others. This question is particularly salient as a new administration is about to launch a new place-based strategy for improving urban communities (e.g., Promise Neighborhoods, Choice Neighborhoods).

Our evaluation findings suggest that what happens locally is a vital concern for federal urban policy and also informative for local communities with responsibility for crafting and executing revitalization strategies. Local differences in EZ performance suggest that a constructive research agenda for urbanists would examine why some local EZs produced positive results while others did not and why some types of outcomes were more likely to be achieved than others. Although it is beyond the scope of this article to explain what distinguished successful and unsuccessful local programs, our ongoing EZ project suggests that governance and local capacity are critical factors. From this standpoint, urban revitalization policy making may parallel urban regime theory. Clarence Stone (1989, 227) has argued that the essential problem of urban governance is to create and sustain the capacity to act: "Governance requires the power to combine necessary elements for a publicly significant result." Local EZ programs that featured effective collaborative governance structures and processes and could rely on experienced and capable local agencies were more likely to produce positive outcomes.

If local governance and capacity are critical to the success of urban revitalization programs, it is regrettable that federal urban policy during the past decade has turned away from block grants to support local governance, programs, and capacity building (Stoker and Rich 2006). In our view, this recent shift in federal urban policy is wrongheaded. While tax incentives are important, the EZ experience demonstrates that the combination of market-oriented policy tools and block grants *can be* an effective means to revitalize distressed urban communities. The task of urban revitalization is too difficult and the need is too great to abandon this promising strategy.

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Notes

1. The authors were participants in these studies. Rich served as the field research associate for Atlanta and Stoker served as the field research associate for Baltimore.
2. See the accompanying online appendix, available at <http://oucp/emory.edu/ourwork/research/ez.html>, for more details on classifying local Empowerment Zone (EZ) programs.
3. Round I Enterprise Communities received \$3 million in block grants but no federal tax incentives. Round II EZs (1998) received approximately \$25 million in block grant funds and federal tax incentives. Round III EZs and Renewal Communities (2001) received only federal tax incentives.
4. Oakes and Johnson (2006, p. 375) add, "It is not difficult to show that parameter estimates may be based not on comparisons between actual persons [census tracts] but rather on extrapolation, interpolation, regression smoothing, and imputation more generally," an approach that was used in the other studies.
5. Hannan (2006, p. 349) adds that the implication of testing the effect hypothesis with the residual error of the member units is larger F statistics (smaller p values) where "ineffective interventions will be more likely to be declared worthwhile, and even effective interventions will be over-interpreted."
6. Space limitations prevent us from presenting a comprehensive threats to validity discussion. Readers interested in this topic are urged to see our extended version of this evaluation, which is available online.
7. As we noted earlier, this procedure also implies that the critical test of the effect of the intervention is comparing variation at the condition level against variation at the member level, which Hannan (2006, 349) notes would jeopardize "the integrity of the inferential process . . . [since] the nesting of units within interventions were ignored." The degrees of freedom for the critical F test of the effects of the EZ intervention therefore is $2 \times (6 - 1) = 10$, which is likely to be too weak to detect an effect regardless of the method used.

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