Do Local Economic Development Programs Work? Evidence from the Federal Empowerment Zone Program*

Matias Busso
University of Michigan
matiasb@umich.edu

Patrick Kline
Yale University
patrick.kline@yale.edu

Abstract

This paper evaluates the impact of Round I of the federal urban Empowerment Zone (EZ) program on neighborhood level labor and housing market outcomes over the period 1994-2000. Using four decades of Census data in conjunction with information on the proposed boundaries of rejected EZs, we find that neighborhoods receiving EZ designation experienced substantial improvements in labor market conditions and moderate increases in rents relative to rejected and future zones. These effects were accompanied by small changes in the demographic composition of the neighborhoods, though evidence from disaggregate Census tabulations suggests that these changes account for little of the observed improvements.

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Local economic development programs are an important, yet understudied, feature of the U.S. tax and expenditure system. Timothy Bartik (2002) estimates that state and local governments spend \$20-30 billion per year on economic development programs with an additional \$6 billion per annum coming from the federal government. However, little academic work has been done examining the impact of these expenditures on local communities, largely because of the small scale and general diversity of most such programs.[1] This paper evaluates the federal urban Empowerment Zone (EZ) program, which constitutes one of the largest standardized federal interventions in impoverished urban American neighborhoods since President Johnson's Model Cities program.

With a mandate to revitalize distressed urban communities, the EZ program represents a nexus between social welfare policy and economic development efforts. Unlike conventional anti-poverty programs, Empowerment Zones aim to help the poor by subsidizing demand for their services at local firms, which has made them one of the few social welfare programs popular on both sides of the congressional aisle. In an era where non-entitlement spending on social welfare programs has been scaled back dramatically, the federal Empowerment Zone program has enjoyed rapid growth. After the initial funding of six first round EZs and two "supplemental" EZs in 1994, fifteen more cities were awarded zones in 1999, followed by another eight in 2001. An additional forty-nine urban areas were concurrently granted smaller Enterprise Communities (ECs) which entailed a reduced package of benefits. The enthusiasm for spatially targeted tax credits has led to the birth of a variety of new zones, each modifying the original EZ concept in different ways.[2] Most recently, the justification for tax abatement zones has been expanded to include disaster relief. For example, in the wake of the September 11th attacks, parts of New York city were designated "Liberty Zones" and granted a variety of localized tax credits; while, in 2006, Congress passed legislation authorizing a set of "Gulf Opportunity Zones" for areas stricken by Hurricane Katrina.

These recent forays of the IRS into the business of local economic development should merit the attention of economists. The GAO (1999) estimates that the first round Empowerment Zones will cost \$2.5 billion over the course of the ten year program. Given that EZ neighborhoods have a total population of under a million people, subsidies of this magnitude, when directed to such relatively small urban areas, might be expected to have important effects upon the behavior of firms and workers. Measuring the nature and magnitude of these

[1]See Bartik (1991) and the volume by Nolan and Wong (2004) for a review.

[2]In addition to urban EZs and ECs, there are a series of rural EZs and ECs, Enhanced Enterprise Communities (EECs), and 28 urban and 12 rural "Renewal Communities" entitled to benefits similar in magnitude to EZs.

behavioral responses is crucial for understanding the equity-efficiency tradeoffs inherent in geographically targeted transfers.[3]

The EZ program was pre-dated by a series of state initiated “enterprise zones” which varied dramatically in scale, purpose, and implementation.[4] A modest literature evaluating the state level programs reaches mixed conclusions reflecting, in part, the enormous diversity of the programs under examination.[5] Some programs only provide for investment subsidies while others include employment tax credits; some state zones cover hundreds of square miles, while others are focused on particular neighborhoods within a few cities. Besides differences in the structure of the programs themselves, a number of methodological problems hinder clear interpretation of the enterprise zone literature. Many of the early studies faced difficulties obtaining data corresponding to the boundaries of the state zones, relying instead upon evaluations at higher levels of aggregation such as the zip code or city which likely reduced the statistical power of the estimates. Furthermore, most studies rely upon simple variants of the differences in differences research design without examining in any detail the suitability of the control groups being used to proxy the counterfactual behavior of the zones (a notable exception being Boarnet and Bogart (1996)). Finally, all of the studies of which we are aware save for Papke (1994) calculate standard errors ignoring issues of spatial and temporal dependence in the data making it difficult to assess exactly how precise previous studies have been and whether the differences in results are attributable to chance.

The federal EZ program is much larger in scope and scale than its state level precursors and involves a standardized package of fiscal benefits applied to neighborhoods defined in terms of 1990 census tracts. Unlike most state level zones, the EZ program ties business tax credits to the employment of local residents and includes a series of large block grants aimed at reducing poverty and improving local infrastructure. The only large scale study of the impact of EZ designation is an interim evaluation (Hebert et al., 2001) performed for HUD by Abt Associates in conjunction with the Urban Institute, which finds that EZs had large effects on job creation, with increases in local payrolls on the order of 10%.

The Abt study suffers from a number of important weaknesses. First, it relies upon within city comparisons of census tracts which are likely to overstate the effect of the program if EZ designation merely reallocates jobs between neighborhoods. Second, the matching algorithm used to find controls for the EZ tracts is poorly documented and standard errors are

[3]See Nichols and Zeckhauser (1982) for an introduction to the economics of targeting.

[4]See Papke (1993) and Hebert et al. (2001) for a history of the Empowerment and Enterprise Zone ideas.

[5]See Papke (1993, 1994), Boarnet and Bogart (1996), Bondonio (2003), Bondonio and Engberg (2000), Elvery (2003), and Engberg and Greenbaum (1999). Peters and Peters and Fisher (2002) provide a review.

not provided making it difficult to draw strong conclusions regarding the results. Moreover, important questions exist about the quality and representativeness of the Dunn and Bradstreet data used in the analysis.[6] Third, since local governments designed Empowerment Zone boundaries, it is possible that census tracts awarded EZs would have improved relative to other tracts in the same city even in the absence of EZ designation if the boundaries were drawn based upon trends emerging at the beginning of the 1990s. Finally, the study provides no guidance as to whether the jobs being created in EZs were staffed by local residents, whether the neighborhood composition of EZ residents changed, and whether poverty, unemployment, or the local housing market responded to the treatment–questions that are key to evaluating the success or failure of the program.

This paper uses four decades of census data on local neighborhoods in conjunction with proprietary EZ application data obtained from HUD to assess the impact of Round I EZ designation on residential sorting behavior and local labor and housing market outcomes over the period 1994-2000.[7] Unlike previous studies we use census tracts in rejected and future Empowerment Zones as controls for first round EZs. Since these tracts were nominated for EZ designation 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 present an extensive body of evidence indicating that these controls serve as good proxies for the counterfactual behavior of EZ tracts over the 1990s. Moreover, because most of our control tracts are in different cities than those winning EZs, they are substantially less susceptible to contamination by spillover or general equilibrium effects than those of previous studies. We use a variety of semiparametric methods to adjust for the small observable differences that do exist between our control tracts and EZs and to increase the statistical power of our analysis.

We find that neighborhoods receiving EZ designation experienced substantial improvements in the labor market outcomes of zone residents and moderate increases in housing values and rents relative to observationally equivalent tracts in rejected and future zones. These effects were accompanied by small changes in the demographic composition of the neighborhoods. We provide evidence from disaggregate census tabulations that the observed improvements in the local labor market conditions of EZ neighborhoods are unlikely to have resulted from these demographic changes alone. Employment rates, for example, seem to

[6]See Heeringa and Haeussler (1993) and Appendix A of the Abt report.

[7]The outcomes are: poverty, employment, unemployment, owner occupied housing values, rents, mean earnings, population, the fraction of houses that are vacant, the fraction of the neighborhood that is black, the fraction of residents who live in the same house as five years ago, and the fraction of residents who hold a college degree.

have increased even among young high school dropouts. However, given the high rates of turnover in EZ neighborhoods we cannot determine whether the benefits of EZ designation were captured by pre-existing residents or new arrivals with similar demographic characteristics.

An impact analysis is performed indicating that the EZ program created approximately $1 billion of additional wage and salary earnings in EZ neighborhoods and another $1 billion in property wealth. A comparison of IRS data with our impact estimates suggests that the tax credits associated with EZ designation are unlikely to have been the only source of the observed employment gains. Rather, we conclude that the block grants and outside funds leveraged by EZ designation, perhaps in conjunction with changes in expectations associated with EZ status, are likely to have contributed substantially to the changes in the local labor market.

The remainder of the paper is structured as follows: Section I provides background on the EZ program, Section II discusses the expected impact of EZ benefits, and Section III describes the data used. Section IV introduces the identification strategy and details the methodology used, Section V discusses results and tests for violations of the assumptions underlying our identification strategy. Section VI provides an impact analysis and Section VII concludes.

I. A Crash Course in Empowerment

The federal Empowerment Zone program is a series of spatially targeted tax incentives and block grants designed to encourage economic, physical, and social investment in the neediest urban and rural areas in the United States. Talk of a federal program caught on early in President Clinton's first term following the 1992 Los Angeles riots. In 1993, Congress authorized the creation of a series of Empowerment Zones and smaller Enterprise Communities (ECs) that were to be administered by the Department of Housing and Urban Development (HUD) and awarded via a competitive application process.

Communities were invited to create their own plans for an EZ and submit them to HUD for consideration. Plans included the boundaries of the proposed zone, how community development funds would be used, and how state and local governments and community organizations would take actions to complement the federal assistance. In addition to providing a guidebook to communities hoping to apply, HUD held a series of regional workshops to explain the EZ initiative and the requisite application process. Nominating local governments were required to draw up EZ boundaries in terms of census tracts, list key demographic characteristics of each proposed tract including the 1990 poverty rate as measured in the Decennial Census, and specify whether the tracts were contiguous or located in the central business district.[8]

HUD initially 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 but were awarded full EZ designation two years later. Forty-nine rejected cities were awarded ECs. Table 1 shows summary statistics of EZ neighborhoods by city. The average Round I EZ spanned 10.6 square miles, contained 117,399 people, and had a 1990 poverty rate of 45%. Most zones are contiguous groupings of census tracts, although some EZs, such as the one in Chicago pictured in Figure 1, cover multiple disjoint groupings of tracts.

EZ designation brought with it a host of fiscal and procedural benefits, which we briefly summarize here:[9]

1. 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.[10] 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.

2. Title XX Social Services Block Grant (SSBG) Funds —Each EZ became eligible for $100 million in SSBG funds, while each SEZ was eligible for $3 million in SSBG funds. These funds could be used for such purposes as: training programs, youth services, promotion of home ownership, and emergency housing assistance.

3. Section 108 Loan Guarantees/Economic Development Initiative (EDI) Grants —EDI funds are large flexible grants which are meant to be used in conjunction with other sources of HUD funding to facilitate large scale physical development projects. The two SEZ's, Los Angeles and Cleveland, received EDI grants of $125 and $87 million respectively. The six original EZs were not eligible for these grants. Section 108 Loan Guarantees allow local governments to obtain loans for economic development projects. Los Angeles received $325 million in 108 loan guarantees and Cleveland received $87 million.

[8]For example, the application asked "Does any tract that includes the central business district have a poverty rate of less than 35%?" and "Do all census tracts of the nominated zone have 20% or more poverty rate?"
[9]See IRS (2004) for more details.
[10]Firms located in the two supplemental Empowerment Zones did not become eligible for the tax credit until 1999.

4. Enterprise Zone Facility Bonds —State and local governments can issue tax-exempt bonds to provide loans to qualified businesses to finance certain property. A business cannot receive more than $3 million in bond financing per zone or $20 million across all zones nationwide.

5. Increased Section 179 Expensing —Section 179 of the Internal Revenue Code provides write-offs for depreciable, tangible property owned by businesses in designated zones. Qualified target area business taxpayers could write off $20,000 more than the usual first-year maximum (which in 1994 was $18,000).

6. Regulatory Waivers/Priority in Other Federal Programs —Qualified EZ/EC areas were given priority in other Federal assistance programs. Furthermore, as part of their applications, EZ/EC applicants were encouraged to request any waivers in Federal program requirements or restrictions that were felt to be necessary for the successful implementation of their local revitalization strategy.

The subsidies available to zone businesses increased substantially over the first four years of the program with the surprise introduction of two additional wage credits (the Work Opportunity Tax credit and the Welfare to Work Tax Credit),[11] an expansion of the EZ Facility Bonds program, and changes in the treatment of capital gains realized from the sale of EZ assets. By all accounts, the degree of *potential* fiscal intervention in EZ neighborhoods was substantial.[12]

Nevertheless, it is difficult to assess exactly how extensive participation in the program has been. GAO (1999) estimated that the EZ program would cost $2.5 billion over its ten year life with 95 percent of the costs coming from the employment credit.[13] IRS data show that, in the year 2000, close to five hundred corporations, and over five thousand individuals, claimed EZ Employment Credits worth a total of approximately $23.5 and $22 million, respectively.[14] Roughly $200 million in employment credits were claimed over the period 1994 to 2000, with the amount claimed each year trending up steadily over time. So despite the slow ratcheting up of participation, reasonably large tax subsidies have been

[11] Work Opportunity Tax Credits enabled businesses to claim up to $2,400 per worker in tax credits for first year wages paid to qualifying employees such as ex-felons, and youth ages 18-24 who are zone residents. Welfare to Work Tax Credits allow businesses to claim credits for up to $3,500 of first year and $5,000 of second year wages paid to workers who are long-term recipients of family assistance.

[12] While the SSBG and EDI funds were fungible, the wage credits and capital write offs were relatively narrowly targeted. Wages paid to workers employed for less than ninety days or relatives were not eligible for the wage credits nor were payments to unofficial workers not on the payroll. Similarly, for a business to be eligible for the tax exempt bond financing or the increased Section 179 expensing it must be able to demonstrate that the majority of its income is earned within the zone and that 35% of its employees are zone residents.

[13] The EZ program has subsequently been extended to expire in 2009.

[14] These figures come from GAO (2004).

dispensed to EZ neighborhoods in the form of wage subsidies. In contrast, only 17 EZ facility bonds were issued before 2000 totalling approximately $50 million, so the impact of the tax exempt bond financing is probably minimal.

Survey data provide information about who participated in the tax incentives and why. A 1997 survey of zone businesses conducted by HUD found that most firms were unaware of the existence of the EZ program, that only 11% claimed to be using the wage tax credit, and only 4% claimed to be using the Section 179 deductions.[15] Such figures mask heterogeneity in participation rates by firm size. The HUD survey found that large firms used the tax credits more intensively with 63% and 30% utilization rates for the wage subsidies and capital write-offs respectively.[16] Another survey conducted by the GAO (1999) found that 55% of large urban businesses using the employment credits were manufacturing firms. The most commonly cited reasons for not using the wage credits were that firms were either unaware of the benefits or did not qualify for them because their employees lived outside of the zone. However, even among large firms, 27% responded that they were not aware of the credit. The low rates of participation in the Section 179 write-off program were most often attributed to lack of knowledge about the program and ineligibility due to lack of profits or qualifying investments. Since tax credits can only be claimed against a company's taxable profits, many small firms (15%), appear to have been unable to take advantage of the program due to insufficient taxable income.

Although the tax benefits accompanying EZ designation were somewhat underutilized by firms, the General Accounting Office (2004) estimates that state agencies had drawn down approximately 60% of Round I SSBG funds by 2003 and were on target to fully expend their allocations by the expiration of the program in 2010. More difficult to measure is the degree of outside investment leveraged by EZ designation. While the first round EZs were allocated roughly $800 million dollars in SSBG and EDI funds, the annual reports of the various EZs suggest that massive amounts of outside capital have accompanied the grant spending. HUD (2003) claims that $12 billion in public and private investment have been raised from Federal "seed" money accompanying the broader EZ/EC program. Our own analysis of HUD data suggests that the amount spent on first round EZs over the period 1994-2000 is substantially less than this, but still much greater than the initial amount of block grant funding allocated.

Table 2 summarizes information from HUD's internal performance monitoring system

[15] These figures come from Hebert et al. (2001).

[16] See tables 3-13, 3-14 and 3-15 in Hebert et al. (2001). The sample sizes used in the survey are not large enough to make strong inferences regarding the relationship between size and participation.

on the amount of money spent on various program activities by source. Audits by HUD's Office of Inspector General[17] and the GAO (2006)[18] have called the accuracy of these data into question, so the figures reported should be interpreted with caution. The six original EZs reported spending roughly $2 billion by 2000, with more than four dollars of outside money accompanying every dollar of SSBG funds. The most commonly reported use of funds was enhancing access to capital. One-stop capital shops providing loans to EZ businesses and entrepreneurs were a component of the plans of most EZs. In Detroit, a consortium of lenders provided $1.2 billion to be used in a local loan pool. Although these funds are listed as being spent, it is difficult to know what fraction were actually loaned out. Analysis of the HUD data in Hebert et al. (2001) indicates that the total size of all loan pools across the six original EZs was only $79 million. The second most common use of the funds was business development which included technical and financial assistance. Third and fourth most common respectively were expenditures on housing development and public safety.

Compiling the tax and expenditure information together and allowing for biases in the reporting behavior of EZs, we estimate that the EZ program resulted in expenditures over the period 1994-2000 of between one and three billion dollars. While this amount of expenditure is below what was originally envisaged at the inception of the program, it is still quite substantial considering that together the EZs constitute a 92 square mile area containing less than a million residents.

II. Expected Impact

The benefits accompanying EZ designation might be expected to impact a number of features of local communities.[19] Here we consider the aggregate variables most likely to respond to the treatment and the economic interpretation of those responses.

The wage subsidies should have two effects on local labor markets, both militating towards increased employment of zone residents. First, there should be a scale effect in that the average cost of labor should fall and production should expand. Second, there should be a substitution effect as outside workers are replaced by cheaper zone workers. If outside workers are relatively unwilling to relocate to EZ neighborhoods and zone residents vary

[17] See Chouteau (1999) and Wolfe (2003).

[18] While the GAO could not find suitable documentation corroborating the dollar amount spent on each program, they were able to verify HUD data on the number of activities undertaken. Their analysis of this data indicated that "community development" projects which include "workforce development, human services, education, and assistance to businesses" accounted for more than 50 percent of the activities implemented in the 6 original urban EZs.

[19] See Papke (1993) for a general equilibrium model of the effects of localized tax incentives.

substantially in their disutility of work, then we might expect any employment increases to be accompanied by corresponding increases in local wages.

If firms are only willing to hire the most qualified workers from a neighborhood, then employment gains need not be accompanied by reductions in poverty as the relatively high skilled workers will merely shift from one job to another. Likewise, if EZ neighborhoods lack residents with the sorts of skills desired by firms then the wage subsidies may not be successful in increasing neighborhood employment as firms will not find it profitable to hire unproductive workers even at a substantial discount.

To the extent that block grants and other subsidies increase the profitability of local businesses, such as by alleviating capital constraints, providing technical assistance, or reducing crime, a scale effect should ensue, leading to an increase in the number of jobs inside EZs.[20] Moreover, if, as suggested by HUD's administrative data, a substantial portion of funds are being invested in workforce development and the matching of workers to local employers, we should expect local employment of zone residents to increase. Funds spent on improvement of infrastructure and physical redevelopment might also be expected to temporarily increase local employment in the form of construction jobs.

Housing markets should respond in tandem with zone labor markets. Firms and residential developers[21] may bid up the price of zone land in pursuit of EZ benefits if those benefits are deemed valuable. Likewise, block grants and outside investments in physical development and community safety are likely to improve the amenities associated with EZs, possibly stimulating residential demand in the area.[22] The asset values of land and owner occupied housing may rise quickly if expectations of future market conditions are influenced by EZ designation and there are obstacles in the short run to increasing housing supply. Rental rates, by contrast, will reflect supply and demand conditions in the spot market for housing. However if zone amenities improve, or if outside workers seek to migrate to the zone in anticipation of future neighborhood improvements, quality adjusted rents will rise.[23]

[20]Reductions in the price of capital should also bring with them a substitution effect as capital is substituted for labor. In theory this effect could outweigh the scale effect and yield negative employment effects if capital and low skilled labor are gross substitutes. We consider such extreme cases implausible. However, the substitutability of capital and low-skill labor may be expected to result in fairly small net impacts on employment.

[21]EDI and SSBG funds are targeted towards the development of affordable housing and the promotion of home ownership. In practice, these funds, in conjunction with the Low-Income Housing Tax Credit, are often spent in public-private physical development projects.

[22]According to Hebert et al. (2001) the majority of EZ businesses reported in 2000 that neighborhood conditions were "much improved" or "somewhat improved" since 1997.

[23]In some of the zone cities rents are regulated meaning that housing will be rationed.

Over longer time horizons the supply of housing may increase or the quality of the housing stock may adjust, both of which should moderate any price effects.

Since most zone residents are renters, large increases in rents may lead to gentrification and neighborhood churning as more affluent newcomers displace prior zone residents. To the extent that gentrification does occur, it should be reflected in changes in the demographic composition of zone neighborhoods. Increases in the price of land might also be expected to bring with them reductions in the fraction of units in a neighborhood that are vacant. However, local landlords may postpone the sale of vacant units to developers if property values are expected to rise faster than the interest rate. Therefore the expected impact of EZ designation on the fraction of units vacant is ambiguous.

III. Data

To perform the analysis we constructed a detailed panel dataset combining information from the Decennial Census, the County/City Databook, and HUD. The primary data source utilized is the Neighborhood Change Database (NCDB) which is a panel of census tracts spanning the period 1970-2000 constructed by Geolytics and the Urban Institute. Appendix I provides more detailed information about this dataset and how it was constructed. Tract level Decennial Census information from the NCDB was merged with relevant editions of the County/City Databook to yield a hierarchical longitudinal dataset with four decades worth of information on cities and tracts.[24]

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.[25] These applications contain the tract composition of rejected zones, along with information regarding the number of political stakeholders involved in each proposed zone.[26] We merged this information with data from HUD's web site detailing the tract composition of future zones to create a composite set of rejected and future zones to serve as controls for EZs in our empirical work. 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.

[24] Tracts that crossed city boundaries were assigned to the city containing the highest fraction of their population.

[25] The scoring information is not in the public domain and was not released to us by HUD.

[26] Since the applications proposed EZs in terms of 1990 census tracts and the NCDB uses 2000 census tract definitions we use the Census Tract Relationship Files of the U.S. Census Bureau to map the former into the latter.

IV. Methodology

A. Identification Strategy

The credibility of any non-experimental evaluation hinges critically upon the nature of the treatment assignment mechanism. In order to receive EZ designation, tracts had to pass two stages of selection. First, they had to be nominated by local officials for inclusion in an EZ. Second, the EZ proposal of which they were a part had to be chosen by HUD. While little is known about the initial nomination process, HUD's decision making process has been fairly well documented. EZ applications were ranked and scored according to their ability to meet four criteria: economic opportunity, community-based partnership, sustainable community development, and a strategic vision for change. Explicit eligibility criteria specified minimum rates of poverty and unemployment and maximum population thresholds for groups of proposed census tracts as measured in the 1990 Census.[27] The authorizing legislation also reserved designations for nominees with certain characteristics.[28] Scores were assigned to each application by an interagency review team consisting of approximately 90 individuals. HUD's Department of Community Planning and Development oversaw the review team. After the HUD committee submitted its scores and recommendations the selection decisions were made by HUD Secretary Cisneros in consultation with a 26 member oversight organization known as the Community Empowerment Board. The CEB was chaired by Vice President Gore and staffed by cabinet secretaries and other high ranking officials. After designations were made the CEB was used to coordinate support for EZs and ECs from other agencies.

Following allegations of impropriety in the popular press an investigation was conducted by the HUD inspector general finding some irregularities in the scoring process including that some of the lower ranked EC applications were considered for awards.[29] However, the audit indicated that all six of the first round EZs were chosen from a list of 22 applications designated as "strong" by the HUD selection committee. Wallace (2003) analyzes the assignment process, finding that political variables are poor predictors of EZ designation. Rather,

[27] All zone tracts were required to have poverty rates above twenty percent. Moreover, ninety percent of zone tracts were required to have poverty rates of at least twenty-five percent and fifty percent were required to have poverty rates of at least thirty-five percent. Tract unemployment rates were required to exceed 6.3%. The maximum population allowed within a zone was 200,000 or the greater of 50,000 or ten percent of the population of the most populous city within the nominated area.

[28] For example one urban EZ had to be located in an area where the most populous city contained 500,000 or fewer people. Another EZ was required to be in an area that included two states and had a combined population of 50,000 or less.

[29] See Greer (1995). Secretary Cisneros informed the inspector general's office that "he used the [HUD] staff's general input, as well as his personal knowledge and perspectives on individual community needs, commitment and leadership, in making the final designations and award decisions."

variables such as community participation, size of the empowerment zone, and poverty were the best predictors of receipt of treatment.

We will compare the experience over the 1990s of Round I EZs to tracts in rejected and later round zones with similar historical Census characteristics.[30] Since much of the data used by HUD to select zones came from the 1990 Census it seems reasonable to believe that rejected and future zones with similar census covariates can serve as suitable controls for winning zones. We present a variety of evidence including a series of "false experiments" suggesting that this is indeed the case. Because some of the control zones used in this approach received treatment in the form of ECs, we expect that the resulting estimates of the impact of EZ designation will be biased towards zero, making our estimates relatively conservative.[31]

Since the majority of rejected and future zones are located in different cities than treated zones, we are able to assess the sensitivity of our estimates to geographic spillover effects. This is an important advantage of our work over the Abt study (and many of the studies of state level enterprise zones) which relied entirely upon within city comparisons. Two sorts of local spillovers are plausible. First, some of the "leveraged" outside funds flowing to EZs may have been diverted from other impoverished neighborhoods in the same cities or metropolitan areas. Such reallocations would serve to exaggerate the impact of EZ designation found by a within-city estimator since the control tracts would actually be receiving a negative treatment. Second, any true impact of EZ designation on labor or housing market conditions in EZ neighborhoods may spillover into adjacent neighborhoods. This could bias a within city estimator in the opposite direction, though the expected sign depends upon the outcome in question and the underlying economic parameters governing the process.[32] Without prior information on the size of these two spillover effects, one cannot know which effect will dominate or the composite direction of bias.

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

[30]Use of rejected applicants as controls as a means of mitigating selection biases 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.

[31]ECs did not receive wage tax benefits but were allocated \$3 million in SSBG funds and made eligible for tax exempt bond financing. As mentioned earlier, the bond financing does not appear to have been heavily utilized.

[32]Though one would normally expect improvements in the amenity value of one neighborhood to yield housing price increases in both that neighborhood and adjacent neighborhoods, it is possible, if neighborhoods are gross substitutes, for the prices of adjacent neighborhoods to be negatively correlated. Similarly, it is possible for job growth inside of EZs to occur at the expense of neighborhoods outside of EZs if firms merely relocate between neighborhoods without expanding total employment.

cities. A cursory inspection of Table 1 indicates that the three largest US cities all 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 construct a set of "placebo zones" in each city receiving an EZ. Each placebo zone contains the same number of census tracts as the actual EZ in that city and possesses similar demographic characteristics. We compare the experience of these placebo zones over the 1990s to that of the rejected and later round zones and find no appreciable differences, bolstering our confidence in the credibility of our findings.

B. Econometric Model

Let outcomes in application tract i in city c in decade t be represented by Y_{ict}.[33] Suppose that these outcomes are generated by a model of the form:

$$Y_{ict} = \mu_t \left(D_{ict}, Y_{ict-1}, X_{ict-1}, Z_{ct-1}, \eta_{ct}, \varepsilon_{ict} \right) + \theta_i \tag{1}$$

where $\mu_t(.)$ is some function indexed by time, D_{ict} is a treatment dummy, Y_{ict-1} is the tract outcome lagged, X_{ict-1} is a vector of predetermined tract characteristics, Z_{ct-1} is a vector of predetermined city wide characteristics, θ_i is a tract fixed effect, η_{ct} is a random city specific year shock, and ε_{ict} is a serially correlated tract specific error term which is assumed to be independent of all other right-hand-side variables.

The class of stochastic processes encompassed by (1) is capable of capturing many of the key features one would expect to see in a panel of census tracts. It allows for mean reverting tract and city specific shocks and for conditional correlation of outcomes across tracts within a city and within tracts across time. Moreover, substantial heterogeneity across tracts is permitted, both in their mean outcomes and in their potential responses to EZ designation.

It will be convenient to reexpress the dependence of the function $\mu_t(D_{ict}, .)$ on EZ designation by writing $\mu_t(D_{ict}, .) = D_{ict}\mu_t^1(.) + (1 - D_{ict})\mu_t^0(.)$. The (contemporaneous) effect of EZ designation on outcomes in a given tract may now be defined as $\beta_i = \mu_t^1(.) - \mu_t^0(.)$. Note that this effect is a potentially nonlinear function of the predetermined covariates Y_{ict-1}, X_{ict-1}, and Z_{ct-1}. This reflects the notion that neighborhoods with different degrees of pre-existing economic distress are likely to exhibit different responses to EZ designation.

[33] From this point on we use the phrase "application tract" interchangeably with "proposed tract" to refer to application and future EZ tracts.

In order to eliminate the tract fixed effect θ_i, let us rewrite (1) in first differences using the potential outcomes notation of Neyman (1923) and Rubin (1974):

$$\begin{aligned} \Delta Y_{ict}^1 &= \beta_i + h_t\left(\Omega_{it}, U_{ict}\right) \\ \Delta Y_{ict}^0 &= h_t\left(\Omega_{it}, U_{ict}\right) \end{aligned} \quad (2)$$

where $h_t(.) = \mu_t^0(.) - \mu_{t-1}^0(.)$, $\Omega_{it} = (Y_{ict-1}, X_{ict-1}, Z_{ct-1},\ Y_{ict-2}, X_{ict-2}, Z_{ct-2})$, and $U_{ict} = (\eta_{ct}, \varepsilon_{ict}, \eta_{ct-1}, \varepsilon_{ict-1})$. Superscripts index potential outcomes under different treatment states. Because we have only one post-treatment decade in the data we only consider static treatment schemes (i.e. we do not consider potential outcomes associated with two decades of EZ designation or one decade of designation followed by a decade of nondesignation). Thus, ΔY_{ict}^1 represents the change in Y_{ict} a tract would have experienced over the 1990s had it been awarded an EZ at the beginning of the decade, while ΔY_{ict}^0 represents the change that would have occurred over the 1990s without an EZ. Because we only observe one of these potential outcomes per tract we may write $\Delta Y_{ict} = \Delta Y_{ict}^1 D_{ict} + \Delta Y_{ict}^0 (1 - D_{ict})$.

Suppose that application tracts were awarded Empowerment Zone status by HUD based upon the history of their Census covariates available in 1990 and other random factors. We model this selection mechanism as $D_{ict} = 1$ if $D_{ict}^* > 0$ and 0 otherwise where[34]

$$D_{ict}^* = \lambda \Omega_{it} + v_{ict} \quad (3)$$

λ is a coefficient vector and v_{ict} is a random error assumed to be independent of Ω_{it} and U_{ict}—an assumption we display here for future reference:

$$v_{ict} \perp \left(\Omega_{it}, U_{ict}\right) \quad (4)$$

In words, this means that conditional on covariates, EZ designation is independent of the experience a proposed census tract would have had over the 1990s in the absence of treatment. This assumption directly implies that the distribution of untreated potential tract outcomes $f\left(\Delta Y_{ict}^0 | D_{ict}, \Omega_{it}\right)$ is independent of whether or not a tract actually received treatment so that $f\left(\Delta Y_{ict}^0 | D_{ict}, \Omega_{it}\right) = f\left(\Delta Y_{ict}^0 | \Omega_{it}\right)$. Rosenbaum and Rubin (1983) term this the Conditional Independence Assumption (CIA) and it forms the cornerstone of our difference-in differences identification strategy. The CIA has the following important implication:

$$E\left[\Delta Y_{ict}^0 | \Omega_{it}, D_{ict} = 0\right] = E\left[\Delta Y_{ict}^0 | \Omega_{it}, D_{ict} = 1\right] \quad (5)$$

[34]This abstracts from the two step nature of the selection process inherent in EZ assignment. See Appendix II for a justification of the approach taken here.

which states that, conditional on covariates, EZ and non-EZ tracts would, on average, be expected to experience the same changes in outcomes during the 1990s in the absence of treatment.

Recall that the tract specific impact of EZ designation β_i is itself a function of the covariates. A standard parameter of interest in the program evaluation literature is the mean effect of treatment on the treated (Heckman and Robb, 1985), which may be defined as:

$$TT = E\left[\Delta Y_{ict}^1 - \Delta Y_{ict}^0 | D_{ict} = 1\right] = E\left[\beta_i | D_{ict} = 1\right]$$

As the name suggests, this concept measures the average impact of the program on those who take it up, or in this case, those tracts awarded EZ designation. Since EZ tracts have roughly similar numbers of people, weighting the effect on each tract equally approximates the national impact on EZ residents.

Estimating TT requires identifying two moments. The first $E\left[\Delta Y_{ict}^1 | D_{ict} = 1\right]$ is trivially identified by the unweighted sample mean of treated observations on ΔY_{ict}. The second moment, $E\left[\Delta Y_{ict}^0 | D_{ict} = 1\right]$, is the counterfactual mean of the treated observations had they not been treated—a quantity with no directly observable sample analogue. We use two approaches to estimating $E\left[\Delta Y_{ict}^0 | D_{ict} = 1\right]$.

The first approach suggested by condition (5) is to approximate the function $E[\Delta Y_{ict}^0 | \Omega_{it}, D_{ict} = 0]$ using a parametric model and then to use that model to compute an estimate of $E\left[\Delta Y_{ict}^0 | D_{ict} = 1\right] = \int E\left[\Delta Y_{ict}^0 | \Omega_{it}, D_{ict} = 0\right] dF\left(\Omega_{it} | D_{ict} = 1\right)$. We do this by fitting a flexible regression model to the untreated tracts and using the estimated regression coefficients to impute the counterfactual mean outcomes of each treated tract. The average difference between imputed counterfactual outcomes and actual values among treated tracts is then computed as an estimator of TT. This procedure, which can be thought of as a variant of the classic Blinder (1973) and Oaxaca (1973) approach to decomposing wage distributions, can be shown to consistently estimate TT given a sufficiently flexible model for $E\left[\Delta Y_{ict}^0 | \Omega_{it}\right]$ (see Imbens, Newey, and Ridder, 2007). Thus for each tract we have an estimate of the tract specific treatment effect $\widehat{\beta}_i = \Delta Y_{ict}^1 - \Delta \widehat{Y}_{ict}^0\left(\Omega_{it}\right)$ where $\Delta \widehat{Y}_{ict}^0\left(\Omega_{it}\right) = \widehat{E}\left[\Delta Y_{ict}^0 | \Omega_{it}\right]$ is the prediction from a parametric linear regression function. We then estimate TT using:

$$\widehat{\text{B-O}} = \frac{1}{N_1} \sum_{i \in \{D=1\}} \widehat{\beta}_i$$

The second approach is to estimate the counterfactual mean $E\left[\Delta Y_{ict}^0 | D_{ict} = 1\right]$ via propensity score reweighting.[35] The basic idea of the propensity score approach is to reweight the data in a manner that balances the distribution of covariates across treated and untreated tracts. This is accomplished by upweighting untreated tracts that "look like" treated tracts based upon their observables. Once the distribution of covariates is balanced across treatment and control groups a simple comparison of weighted means will, under the assumptions made thus far, identify TT. Moreover, the performance of the reweighting estimator in balancing the distribution of observables across groups can easily be assessed directly by comparing reweighted covariate moments.

A key assumption necessary for propensity score based approaches to identify TT is,

$$P\left(D_{ict}=1|\Omega_{it}\right)<1 \tag{6}$$

This assumption, which is often referred to as the "common support" condition, states that no value of the covariates can deterministically predict receipt of treatment. The failure of this condition would present the possibility that some tracts with particular configurations of covariates would only be capable of being observed in the treated state, thereby preventing the construction of valid controls. As suggested by Heckman et al. (1998b) and Crump et al. (2006) we present results where observations with very high estimated propensity scores are dropped from the sample. This approach safeguards against violations of the overlap condition in finite samples and can substantially reduce the sampling variance of the estimator.[36]

Conditions (4) and (6) in conjunction with the results of Rosenbaum (1987) imply that[37]

$$E\left[\Delta Y_{ict}^{0}|D_{ict}=1\right]=E\left[\omega\left(\Omega_{it}\right)\Delta Y_{ict}^{0}|D_{ict}=0\right] \tag{7}$$

where $\omega\left(\Omega_{it}\right)=\frac{p(\Omega_{it})}{1-p(\Omega_{it})}\frac{1-\pi}{\pi}$, $p\left(\Omega_{it}\right)=P\left(D_{ict}=1|\Omega_{it}\right)$, and $\pi=P\left(D_{ict}=1\right)$. Thus the covariate distribution of untreated tracts can be made to mimic that of treated tracts by weighting observations by their conditional odds of treatment $\frac{p(\Omega_{it})}{1-p(\Omega_{it})}$ times the inverse of their unconditional odds $\frac{1-\pi}{\pi}$. Equation (7) simplifies estimation considerably since rather

[35]Propensity score reweighting was proposed in the survey statistics literature by Horvitz and Thompson (1952) and adapted to causal inference by Rosenbaum (1987). In the economics literature such estimators have been used in a cross-sectional context by DiNardo et al. (1996) and extended to the panel setting by Abadie (2005). Recent work by Hirano, Imbens, and Ridder (2003) demonstrates that properly implemented reweighting estimators are asymptotically efficient in the class of semiparametric estimators.

[36]Trimming slightly modifies the estimand to $E\left[\Delta Y_{ict}^{1}-\Delta Y_{ict}^{0}|\Delta D_{ict}=1,P\left(\Delta D_{ict}=1|\Omega_{it}\right)<k\right]$ where k is a scalar constant. As suggested by Crump et al. (2006) we choose $k=0.9$ throughout the paper. In most specifications this results in the trimming of a very small fraction (approximately 1%) of the sample.

[37]Proofs of conditions (7) and (8) are provided in Appendix III.

than estimating a very high dimensional conditional expectation, for which different tuning parameters might be required for different outcomes, one need only estimate a single propensity score $p(\Omega_{it}) = P(D_{ict} = 1|\Omega_{it})$ (Rosenbaum and Rubin, 1983).[38] In practice we estimate $p(\Omega_{it})$ via a logit and π by $\frac{N_1}{N_1+N_0}$ the fraction of treated tracts in the estimation sample.

A useful corollary of (7) is that:

$$E[\omega(\Omega_{it})|D_{ict} = 0] = 1 \tag{8}$$

Which merely states that the mean weight among the controls should equal one. We impose the sample analogue of this adding up condition when calculating our estimates in order to reflect the theoretical condition in (8).[39]

Given estimates $\widehat{p}(\Omega_{it})$ and $\widehat{\pi}$ we estimate $E[\omega(\Omega)\Delta Y^0_{ict}|D_{ict} = 0]$ with its sample analogue

$$\frac{1}{N_0}\sum \frac{\widehat{p}(\Omega_{it})}{1-\widehat{p}(\Omega_{it})}\frac{1-\widehat{\pi}}{\widehat{\pi}}\Delta Y^0_{ict}$$

We then estimate TT by computing the weighted difference-in-difference (WDD):

$$\widehat{WDD} = \frac{1}{N_1}\sum_{i\in\{D=1\}}\Delta Y^1_{ict} - \frac{1}{N_0}\sum_{i\in\{D=0\}}\frac{\widehat{p}(\Omega_{it})}{1-\widehat{p}(\Omega_{it})}\frac{1-\widehat{\pi}}{\widehat{\pi}}\Delta Y^0_{ict}$$

Consistency follows subject to the usual regularity conditions by an appropriate law of large numbers.

Throughout the paper we show results from both the Blinder-Oaxaca (B-O) and reweighting approaches.[40] We prefer the reweighting based estimates on the grounds that they allow us to directly assess the suitability of our specification of the propensity score via visual inspection of covariate balance and simple diagnostics for the logit which are not outcome specific. It is also easier to check whether the overlap condition is satisfied with the reweighting approach than the B-O approach. On the other hand, a strength of the parametric B-O

[38]As pointed out by Heckman et al. (1998a), propensity score approaches do not escape the curse of dimensionality since the function $p(\Omega_{it})$ is unknown. The effects on asymptotic bias and variance of adjusting for the propensity score instead of the underlying covariates of which it is a function are ambiguous (see section 7 of that paper).

[39]Equation (8) actually provides us with an overidentifying restriction that can be used as a specification test on our model. Very large deviations from 1 of the mean estimated weight among untreated tracts are a sign of misspecification. In Appendix Table A4 we conduct formal tests of this restriction.

[40]See DiNardo (2002) for a discussion of the reweighting interpretation of Blinder-Oaxaca and Imbens, Newey, and Ridder (2007) for a demonstration of the first order equivalence of the two approaches.

approach is that it can reliably estimate treatment effects even in the absence of overlap if the parametric model upon which it relies is approximately correct.[41]

C. Inference Procedures

Confidence intervals and p-values for all estimators are obtained via a pairwise block bootstrapping algorithm described in Appendix IV. This procedure, which is analogous to cluster robust inference, resamples cities rather than tracts in order to preserve the within city dependence in the data. Because we are interested in evaluating the effect of EZ designation on a variety of outcomes, we use a sequential multiple testing procedure suggested by Benjamini and Hochberg (1995) to control the False Discovery Rate (FDR) of our inferences. The False Discovery Rate is defined as the expected fraction of rejections that are false and is closely related to the probability of a type I error. Details of the multiple testing procedure, which is a function of the single hypothesis p-values, are given in Appendix IV. For convenience we also report single hypothesis confidence intervals and p-values. From this point on, we shall refer to outcomes as "significant" at a given level of confidence if the estimated p-value ensures control of the FDR to the specified level. In general, the multiple testing procedure requires substantially lower p-values for a given level of significance than an equivalent single equation test. Failure to reject a single hypothesis in this multiple testing framework is equivalent to a failure to reject the joint null hypothesis that all of the treatment effects are zero.

V. Results

A. Characteristics of EZs and Controls

Table 3 shows average characteristics of winning and losing proposed zones before and after reweighting.[42] For our baseline specification we restrict the sample to zones in cities with population greater than 100,000. While the residents of rejected and future zones are poor and have high rates of unemployment we see from columns one and four of Table 3 that

[41]Another advantage implied by the results of Chen, Hong, and Tarozzi (2004) is that the B-O approach, which is a variant of their CEP-GMM estimator, reaches the semiparametric efficiency bound under weaker regularity conditions than propensity score reweighting.

[42]The variables included in the reweighting logits are reported in Appendix V. Our baseline specification minimizes the Akaike Information Criteria (see Appendix Table A2). City population could not be included in the conditioning set because it came too close to perfectly predicting EZ receipt. That we cannot mimic the city population distribution of EZs via reweighting should be apparent from the list of winning cities in Table 1. To examine whether imbalance in city-wide population affects our DD results we try adding a third order polynomial in 1990 city population to our Blinder-Oaxaca estimator and experiment with a variety of different sample restrictions, each with a different distribution of city size.

they are not quite as poor or detached from the labor force as residents of EZ areas. After reweighting, however, the mean characteristics of the two groups become substantially more comparable.

Figure 2 shows the time series behavior of the EZ and control tracts with and without reweighting. When reweighting methods are applied to the pooled set of controls their history over the past two decades mirrors that of actual Empowerment Zones remarkably well. There is no dip in outcomes prior to EZ designation of the sort found by Ashenfelter (1978) in studying training programs and for some outcomes the time series behavior of the treatment and control groups over the three decades prior to treatment is almost indistinguishable. One can actually see most of our results from these graphs themselves. The key labor market variables (employment, unemployment, and poverty) all seem to have improved in EZ neighborhoods relative to reweighted controls over the 1990s. A few demographic variables such as the fraction of the population with college degrees also appear to have been impacted by the program.

Columns two and three of Table 3 indicate that control tracts in treated cities have somewhat different characteristics from those in untreated cities. Moreover, our earlier discussion of spillover effects suggested that the use of controls in treated cities has the potential to confound a differences in differences estimator. Table 4 investigates whether pooling control tracts in treated cities with those in rejected cities is likely to introduce important biases into our analysis. This is accomplished by applying our difference in differences estimators to the sample of controls, coding tracts in future EZs in treated cities as the treated group and all other control tracts as untreated. The first column gives the results of a "naive" difference-in-differences analysis without covariate adjustments, the second column presents the results of our preferred reweighted difference-in-differences estimator, the third column shows the results of the regression based Blinder-Oaxaca estimator, and the fourth column adds a third order polynomial in city population to the Blinder-Oaxaca model.

From the first column of Table 4 we see that over the 1990s, control tracts in treated cities experienced smaller increases in the share of residents with college degrees, slightly lower increases in rents, and a greater increase in the fraction of vacant houses than other controls. After conditioning on pre-treatment characteristics all of these relationships disappear. In fact, the magnitude of the differential experience of the two sets of controls over the 1990s tends to be very close to zero, though the reweighting estimator finds a rather large difference in the behavior of mean earnings. This aberrant earnings result disappears in the Blinder-Oaxaca based estimates. We take this as evidence that the two sets of control tracts are roughly exchangeable conditional on predetermined characteristics. In our subsequent analysis we pool together the two sets of controls in order to gain power and to improve the degree of covariate overlap with the EZ tracts.[43]

B. Baseline Results

Table 5 presents numerical estimates of the impact of EZ designation on EZ neighborhoods. The naive DD estimator finds a large (29.7%) increase in the value of owner occupied housing, a 4 percentage point increase in the fraction of the neighborhood that is employed, a 4.1 percentage point decrease in the fraction of the neighborhood that is unemployed, and a 4.9 percentage point decrease in poverty. Reweighting the DD estimator for covariate imbalance changes the magnitude (though not the sign) of many of the point estimates. The estimated impact on housing values falls to 22.4 percent, while the impact on rents rises dramatically to 7.7% and becomes statistically significant. The reweighting estimator also finds a significant 2.3 percentage point increase in the fraction of residents with a college degree and a 2.6 percentage point decrease in the fraction of residents that are black. The estimated impacts on the labor market variables (employment, unemployment, earnings, and poverty) remain essentially unchanged.

For comparison we also report regression based Blinder-Oaxaca estimates in Column 3. The Blinder-Oaxaca method yields point estimates similar to those found by the reweighting estimator though the statistical precision of the estimates sometimes differs. It finds smaller (though still significant) effects of EZ designation on housing values, rents, poverty, unemployment, and employment. However, the estimated effects on the demographic composition of EZ neighborhoods are small and indistinguishable from zero.

Taken together the WDD and B-O estimates suggest that EZs were effective in increasing the demand for the services of local residents. Employment rates rose, while unemployment and poverty rates fell. Housing markets also seem to have adjusted. Housing values increased as did, to a lesser extent, rents. Though the population of EZ neighborhoods does not appear to have changed substantially, the fraction college educated may have increased by as much as a third over 1990 levels, indicating that some changes in neighborhood composition took place. The magnitude and sign of the estimated impact on percent black is also consistent with this interpretation.

[43] See Appendix Table A5 for baseline results using the rejected tracts only. Dropping control tracts in treated cities reduces the power of the analysis but does not substantially affect the point estimates.

The general similarity between the reweighted and naive DD estimates reinforces our presumption that rejected and future EZ tracts are suitable controls for EZ tracts. To the extent that unadjusted comparisons are inaccurate, they seem to yield biases in the estimated impact on housing market and demographic outcomes. The difference between the reweighted and naive estimates suggest that Empowerment Zones were awarded to areas that would have experienced increases in percent black and decreases in rents and the fraction college educated relative to rejected tracts in the absence of treatment. It is also estimated that EZ housing values would have risen relative to rejected tracts without EZ designation, perhaps because of regional differences in the timing of the housing market boom of the late 1990s.

Column four assesses the importance of leaving city size out of the propensity score (see footnote 42) by adding a third order polynomial in city size to the regression model for the Blinder-Oaxaca specification. This parametrically corrects the estimator for any smooth relationship between changes in the outcomes and city population but substantially reduces the power of the analysis due to collinearity between city population and the other city level covariates.[44] We see from Column 4 that this estimator yields essentially the same results as the original WDD estimator that ignores city size but the estimates are less precise. Appendix Table A5 presents further robustness checks, exploring the sensitivity of the estimates to changes in the sample of cities included in the treatment and control groups, and again finds that the conclusions reached by our preferred WDD estimator are essentially unchanged.

C. Tests of the Conditional Independence Assumption

Despite the robustness of the results to modifications of the estimation sample and estimation technique, one may still question the conditional independence assumption (4) underlying our identification strategy. If unmeasured factors correlated with the future performance of neighborhoods influenced the process by which zones were awarded the treatment our estimates will be biased. To address such concerns, we now perform tests of the assumptions underlying our research design, starting with a series of "false experiments" involving the application of our estimator to samples in which none of the "treated" units received treatment. These experiments may be thought of as tests of the overidentifying restrictions provided by our statistical model.

[44]This collinearity is especially pernicious in our setup as we have only 74 control cities. Our baseline B-O specification includes two lags of four city level covariates. Adding a third order polynomial in 1990 city population yields 11 city level parameters to be estimated from 74 aggregate observations.

The first such experiment involves applying our reweighting estimator to outcomes in 1990 before the EZs were assigned. Finding a non-zero "effect" in this time period would be an indication that either our conditioning set is insufficiently rich to characterize the dynamics of sample census tracts in the absence of treatment, or, that there is selection on the 1990 error components η_{c90} and ε_{ic90}.[45] The latter alternative is consistent with the notion that EZs were assigned based upon 1990 census characteristics (which include the innovations η_{c90} and ε_{ic90}) but would require that the 1990 innovation variance be a large fraction of the total cross sectional variance of outcomes over that period, an alternative we consider implausible given the frequency of our data. Thus, we interpret this false experiment as primarily a test of the specification of our conditioning set. Omitting important variables will make treated and untreated units uncomparable in the absence of treatment, yielding spurious estimated "treatment effects" over the 1980's. Table 6, however, shows that none of the estimators find any statistically significant effects in 1990 and that most of the point estimates are quite small. The preferred WDD estimator in column three fails to reject any of the hypotheses at even the 10% FDR level. Thus, it seems that the experience of the treated and untreated tracts with similar covariates was nearly identical over the 1980's, lending credence to the notion that they are comparable over the 1990's.

One may, however, feel uncomfortable with the supposition that the 1990s were simply more of the same. Indeed, Glaeser and Shapiro (2003) provide evidence that national trends in the performance of cities over the 1990s differed from those in the previous decade. Returning to our basic model which can be rewritten compactly as,

$$\Delta Y_{ict} = \beta_i D_{ict} + h_t\left(\Omega_{it}, U_{ict}\right) \tag{9}$$

one may suspect that city specific trends $\Delta\eta_{ct}$ were correlated with treatment status over the 1990s but not the 1980s, perhaps because HUD officials were able to perceive such trends as they emerged near the inception of the program. Hence, the latent index determining EZ assignment might be better represented by an equation of the form:

$$D^*_{ict} = \lambda\Omega_{it} + \rho\Delta\eta_{ct} + v_{ict} \tag{10}$$

In the case where $\rho \neq 0$, the CIA condition is violated and the WDD estimator will not, in general, be consistent.

To test for such a problem we create a series of placebo zones in each treated city and

[45]As described in Appendix IV, the variables used in the reweighting procedure are from 1970 and 1980, so there is no mechanical reason to expect that the 1990 outcomes would be identical across treatment and control groups.

compare their performance over the 1990s to that of future and rejected tracts using the WDD estimator. A finding of nonzero "treatment effects" would indicate a problem with the CIA assumption underlying our analysis. In order to construct the placebo zones we estimated a pooled propensity score model for all tracts in treated cities (see Appendix V for details) and then performed nearest neighbor propensity score matching without replacement in each city, choosing exactly one control tract for each treated EZ tract. This yields a set of placebo zones of the same size and with approximately the same census characteristics as each real EZ.

Figure 3 shows the EZ and placebo EZ tracts in Chicago. Tracts shaded black are the actual EZs designated by HUD, while those shaded grey are placebo zones. The placebo tracts tend to be geographically clustered in much the same way as actual EZs, reflecting the underlying spatial correlation of many of the covariates used in the analysis. One potentially troublesome feature of the placebo zones is that they tend to be located near actual EZ tracts. As discussed in Section IV, if EZ designation did in fact have an impact, the effects may have spilled over into adjacent communities. For this reason we also create two additional sets of placebo zones with the restriction that they be outside or inside of a one square mile radius of an EZ tract.

Table 7 shows the results of applying the WDD and B-O estimators to each set of placebo tracts.[46] The first column presents results for the pooled set of placebo tracts. None of the outcomes register statistically significant differences across placebo and control zones. Even if one were to ignore the multiple testing procedure, the only outcome close to registering a statistically significant effect is housing rents which despite the large point estimate possesses a single equation 95% confidence interval that includes zero. The second column shows the results of repeating the exercise with placebo tracts less than a mile from an EZ tract. Again, none of the differences are statistically significant. Finally, the third column examines the "impact" of the program on tracts a mile or more away from EZ tracts, yielding nearly identical results. The Blinder-Oaxaca estimates in columns four through six yield the same conclusions.

The general agreement in Table 7 between the estimated impacts on closeby and far-away placebo tracts reassures us that any spillover effects that might have accompanied EZ designation are either offsetting or imperceptibly small. Moreover, the general failure to

[46]In order to avoid complications we discard later round zones in the same city as first round EZs from the set of control zones. This results in a modest reduction in the total number of observations used in this part of the analysis.

find any significant differences between the treatment and control groups across all three specifications bolsters our confidence in the assumptions underlying our research design.

As a final check on our research design we try converting the outcome variables to scaled within city ranks.[47] If our results are merely picking up city specific shocks then the rank of an average EZ tract in its city wide distribution of poverty rates, for example, should not change over the 1990s relative to the rank of a similar rejected tract in its city-wide 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.[48]

Table 8 shows the results of applying the WDD and B-O 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 1 indicates that EZ designation led EZ neighborhoods to fall 5.5 percentiles in the within city distribution of tract poverty rates. The results are in close agreement with the findings of Table 5, the only substantive difference being that the estimated effect on housing values falls to the point of statistical insignificance. Since housing values also exhibited large (though insignificant) point estimates in the false experiment in Table 6, we take this as evidence that the estimated impacts on housing values may not be robust. Column 2 of Table 8 shows that the Blinder-Oaxaca estimator with population controls yields point estimates similar to the reweighting estimator though the precision of the estimates is reduced. The remaining columns show that application of the reweighting and Blinder-Oaxaca estimators to the percentile outcomes over the 1980s and in the set of placebo tracts yields very small and statistically insignificant point estimates.

In conclusion, we interpret the results of the exercises considered in this section as demonstrating that the estimates provided in Table 5 are unlikely to have been generated by spurious correlation with city wide trends or by misspecification of the multivariate stochastic process generating tract level outcomes.

[47] In a previous version of this paper we experimented with a 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 our false experiment tests. This poor performance was caused by difficulties in finding suitable control tracts in rejected cities which are usually quite small. We believe the following percentile rank approach to be a much more transparent and robust approach to making within city comparisons.

[48] In other words, for any outcome Y_{ict} we form a new outcome $P_{ict} = rank_{cy}\left(Y_{ict}\right)/N_c$ where $rank_{cy}$ is the rank of Y_{ict} in the city wide distribution of the variable in that year and N_c is the number of tracts in the relevant city.

D. Composition Constant Effects

An obvious concern with our difference in difference results is that some of the estimated labor market effects may be due to compositional changes in the residential population of EZs. Inspection of Table 3 indicates that residential mobility is quite high in EZ neighborhoods with only 56% of 1990 residents in the same house as in 1985. Although we have no statistics regarding mobility into and out of the Empowerment Zones, we think it likely that substantial neighborhood churning occurs between decades even if the demographic characteristics of EZ neighborhoods tend to remain relatively stable. For this reason we consider it impossible to determine with available data whether prior residents or new arrivals gained most from the EZ program. What can be done, however, is to assess whether the demographic groups that tended to live in EZs prior to EZ designation benefitted from the program. In this section we use tract level tabulations of labor market outcomes within detailed demographic cells to evaluate whether changes in demographic composition are driving our results. This is done by estimating within cell impacts and then averaging them using 1990 cell frequencies (see Appendix VI for details).

Table 9 displays racial composition constant effects on employment, unemployment, and poverty calculated from race specific employment rates. Estimates are calculated by using as the outcome variable the change in each tract's race specific labor market rate weighted by the 1990 racial shares. This adjustment does little to change our earlier conclusions from Table 5. Although the point estimates are slightly smaller, we still find substantial and statistically significant effects on employment, unemployment, and poverty. We also find that the fraction of residents with a college degree increased holding racial composition constant, suggesting that much of the estimated influx of the college educated to EZ neighborhoods occurred among blacks.

In order to determine whether the estimated labor market effects are due to changes in the age or educational composition of residents we also examine the impact of EZ designation on the racial composition constant employment rates of 16-19 year old high school graduates and dropouts. Surprisingly, we find very large and statistically significant employment effects on high school dropouts, most of whom, by virtue of our fixed weighting scheme, are black. Similar sized effects are present for high school graduates. We find no effect on students currently enrolled in high school which is unremarkable given that baseline employment rates of such youth are very low. In sum, EZs seem to have resulted in improvements in employment among young people who have either just graduated high school or dropped out – the two groups most likely to be actively seeking work. These youth, especially the dropouts, are unlikely to represent gentrifying families of the sort that one would think could confound interpretation of the previous results.

Our reading of this evidence is that changes in the demographic composition of the neighborhood are unlikely to have generated the large effects on labor market outcomes documented in Tables 5 and 8. This conclusion is broadly consistent with the anecdotal accounts of EZ stakeholders summarized in GAO (2006). The GAO assembled focus groups composed of EZ administrators, state and local officials, and EZ subgrantees and solicited testimonials regarding the impact of EZ designation in each city. The typical response was that EZs positively impacted labor and housing market outcomes, but that some of the observed improvements were the result of neighborhood turnover.

VI. Impact Analysis

Our comparison of EZ neighborhoods to rejected and future EZ tracts in other cities strongly suggests that EZ designation substantially affected local labor and housing market conditions. EZs led to increases in local rates of employment on the order of four percentage points and roughly similar sized decreases in unemployment and poverty rates. The price of renting in EZs increased by around seven percent, while the value of owner occupied housing appears to have increased by nearly triple this amount (though the results of our robustness checks cast some doubt upon the validity of the latter estimates).

When compared with baseline employment, unemployment, and poverty rates of thirty six, fourteen, and forty six percent respectively, the estimated labor market impacts of EZ designation are quite substantial. Table 10 provides calculations converting the estimated treatment effects from Table 5 into effects on totals. The calculations yield an estimated increase in EZ employment of roughly 30,000 individuals, a decrease in unemployment of approximately 13,000 individuals, and a decrease in the poverty headcount of around 50,000 people. It is worth reiterating here that these estimates may well understate the true effect of EZ designation on residential neighborhoods since many of the control zones in our study received some smaller consolation treatment.

Combining the tax credits with the block grants and outside funds, we estimate that the amount of money actually spent in EZ neighborhoods over the course of our sample period is between one and three billion dollars. If we assume that the workers employed because of the EZ program earn the mean annual earnings of EZ residents, and that a third of the employment relationships created will be terminated each year with no effect on future employment probabilities, using a social discount factor of .9, we get a discounted present value of roughly \$1.1 billion in extra output.[49]

A different approach is to use the housing market to value the impact of the program. EZ designation is estimated to have increased total annual rents paid by \$78 million while the total value of owner-occupied housing is estimated to have increased by \$470 million. If we use a 10% discount rate to convert the rent flow into an asset value and add it to the increase in total housing value we get a total increase in wealth of \$1.2 billion. Even if we discard the estimated impact on housing values, which we have reason to suspect, we still get an estimated increase in wealth of \$780 million which is fairly close to our estimates based upon the labor market. While these calculations are clearly flawed measures of the value of EZ designation, we believe they provide a reasonable illustration of the scale of the benefits generated by the program.

A key question raised by the estimates in this paper is why the EZs were able to have such a large impact on the EZ labor market. It is difficult with existing data to disentangle the relative contribution of grants and tax incentives in improving EZ neighborhoods. A lower bound estimate of the number of EZ employees for which firms claimed EZ wage credits can be obtained by dividing the total expenditure on credits in 2000 by the maximum credit of \$3,000. This yields 15,000 employees. IRS analysis of 1996 tax return data suggests that this bound is quite loose as over a quarter of corporations claimed total credits less than the maximum for a single employee. If we instead divide the total expenditure by \$2,000 we get roughly 23,000 employees claimed by firms. While this latter number is close to the estimated increase in employment, it seems likely that most of the credits were claimed on inframarginal hires or pre-existing workers. In fact, only 45% of firms surveyed by HUD who reported using the wage credits responded that the credits were "important" or "very important" for hiring decisions.[50] Thus we find the notion that the tax incentives are wholly responsible for the observed employment increases to be implausible.

The possibility that block grants and outside funding played an important role in redeveloping EZ neighborhoods is important for understanding the likely effects of the later round EZs and various disaster oriented zones, both of which rely almost entirely upon tax subsidies. The experience of the Round I EZs suggests that government entities may be able to play an important role in coordinating expectations among a wide group of non-profit, public, and private entities interested in investing in disadvantaged neighborhoods. The

[49] The formula used here is $PV = \frac{E}{1-\beta(1-\delta)}$ where E is earnings, $\beta = .95$, and $\delta = 1/3$. The metric used for E is 1990 dollars.

[50] Hebert et al. (2001) Exhibit 3-18.

role of public seed money in leveraging outside investments in local economic development has been understudied.[51] Relatively small grants, in conjunction with sustained political support at the federal level, seem to have been successful in leveraging substantial outside investments in Round I EZ neighborhoods. These investments may have been responsible for stimulating the demand for EZ labor, perhaps through a series of local multiplier effects of the sort contemplated by regional planners (e.g. Treyz et al, 1992) or a form of local increasing returns as considered by Rauch (1993).

While it is difficult to directly assess the impact of the non-tax expenditures on the physical and economic environment of EZ neighborhoods, there is some evidence that zone amenities improved over the 1990s. The 1997 wave of the HUD survey found that 45% of zone businesses perceived the neighborhood as an "improved" or "somewhat improved" place to do business since 1994/1995, while the 2000 wave of the survey found that 53% of businesses perceived such improvements since 1997, a statistically significant difference. The most common cited impediment to doing business in zones was crime and public safety in both surveys though concerns over crime seem to have been somewhat less prevalent in 2000. Without equivalent survey data in rejected areas we cannot disentangle these reporting patterns from general trends in the US economy over the 1990s, however, we think it reasonable to suspect that the billions of dollars spent in these neighborhoods might have resulted in substantial improvements to their public safety, physical appearance, and local infrastructure.

VII. Conclusion

Our comparison of EZ neighborhoods to rejected and future EZ tracts in other cities strongly suggests that EZ designation substantially improved local labor and housing market conditions in EZ neighborhoods. The implications of these findings for the study of local economic development policies are manifold. First, it appears that the combination of tax credits and grants can be effective at stimulating local labor demand in areas with very low labor force participation rates. That this can occur without large changes in average earnings suggests either that labor force participation in such neighborhoods is very responsive to wages or that job proximity itself affects participation perhaps due to reductions in the cost of learning about vacancies or the cost of commuting to work.[52] Second, in the case of the EZs, the impact of these demand subsidies does not seem to have been captured by the relatively well off;

[51] Andreoni (1998) has modeled the role of seed money in determining charitable contributions. To our knowledge the role of seed money in spurring economic development has not been explored in the academic literature.

[52] This latter alternative is often associated with Kain's (1968) "Spatial Mismatch Hypothesis".

economic development and poverty reduction seem to have accompanied one another in the manner originally hoped for by proponents of the program. Indeed, our use of disaggregate Census tabulations suggests that even young high school dropouts experienced improved labor market prospects as a result of the program. Third, while the treated communities appear to have avoided large scale gentrification over the period examined in this study, policymakers should consider carefully the potential impact of demand side interventions on the local cost of living. Given that the vast majority of EZ residents rent their homes, small changes in the cost of zone living can be expected to impose large burdens on the roughly two thirds of the EZ population who do not work. Tradeoffs of this sort should be taken into account when attempting to determine the incidence of the EZ subsidies. If authorities wish to use EZs as anti-poverty programs they may wish to consider combining housing assistance or incentives for the development of mixed income housing as complements to demand side subsidies.

Though our results appear to corroborate the findings of the Abt study, we cannot, with our data, ascertain whether the employment gains of local residents are the result of job growth or the substitution of local workers for outside workers. A detailed analysis of matched employer-employee data might yield insights into whether the scale or substitution effects are responsible for generating the local employment gains observed. More research is also needed to determine whether any job creation that is occurring is due to existing firms expanding, new firms being born, or outside firms relocating.

Finally, this evaluation has only examined the first six years of the EZ program. Very little is known about the dynamics of neighborhood interventions. The decisions of residents, developers, and landlords that lead to neighborhood gentrification and turnover may respond to changes in housing values and rents with a lag. Moreover, as the program comes to a close, firms may move out of zones or close up altogether, reversing any employment gains in the process. Understanding these issues is key to determining the long run winners and losers of EZ designation.

Appendix I: Data Description and Details

NCDB. The NCDB remaps data from 1970, 1980, and 1990 tracts to 2000 tract boundaries. Coverage in 1970 and 1980 is limited as the US was not entirely divided into tracts at that time, although most areas that were not covered were rural. By 1990 the US was fully divided into census tracts. The remapping process involves mapping tracts in each decade using a GIS program and determining when tract boundaries changed. In the event of a change weights were assigned to tracts from earlier periods based upon population overlap in order to ensure accurate computation of count totals, means, and fractions. Details of the process are given in Appendix J of NCDB Users Guide Provided by Geolytics.

County/City Databook. We extract from the County/City Databook (CCD) variables that are not part of the Decennial Census of population (and therefore are not in the NCDB) such as crime rate, percentage of workers in the manufacturing sector and percentage of workers working in the government. When possible, city level variables were constructed by aggregating the NCDB tract information by city using Geocorr correspondences between tracts and cities. Cross referencing the constructed variables to their analogues in the CCD yielded virtually identical figures.

HUD. We have information on 73 of the 78 applications sent to HUD. We have repeatedly requested the 5 missing applications to no avail. Our dataset also includes all census tracts that belong to any urban EZ, EC, EEC, RC, or UEZ of all the first three rounds. (See Table A1 for more details).

Geocorr. The MABLE/Geocorr engine generates files showing the correspondence between a wide variety of Census and cartographic geographies in the United States. We use Geocorr 2000 to match each census tract to one or more places (cities, townships, villages, etc.). Each census tract that crosses city boundaries was allocated to the city where the majority of the tract's population is located.

Missing Data Some variables used in the estimation procedure exhibited mild missing data problems. Approximately 8.6% of the tracts in our estimation sample had missing mean 1990 housing values and 1.4% had missing mean 1990 rents. Overall we lost approximately 13% of the sample in our baseline specification because of missing values of the control variables. All tables in the paper restrict the estimation sample to the set of tracts (both treated and untreated) with complete covariate information. In results not shown we tried imputing the missing values via sequential regression methods and performed a full case analysis. This procedure yielded very similar results for all outcomes except for housing values which exhibited moderately smaller point estimates.

Appendix II: Alternative Derivation of Propensity Score Model

The assignment model in (3) ignores the two step nature of EZ treatment assignment. Here we demonstrate that the hierarchical nature of the assignment process does not present any additional complications to our analysis. Let P_{ic} be an indicator for whether a tract is proposed, W_c an indicator for whether a city wins an EZ, and D_{ic} an indicator for whether a tract gets EZ designation. For a tract to receive EZ designation it must be proposed and its city-wide proposal must be accepted by HUD, so that:

$$D_{ic} = P_{ic} \times W_c$$

Suppose that tract proposal is a function of covariates Ω_{ic}, unobserved trends ε_{ic}, and a random error ξ_{ic} so that

$$\begin{aligned} P_{ic}^{*} &= \lambda\Omega_{ic} + \rho\varepsilon_{ic} + \xi_{ic} \\ P_{ic} &= I\left[P_{ic}^{*} > 0\right] \end{aligned}$$

Note that when $\rho \neq 0$ there is selection on unobservables in the proposal process. In contrast assume that HUD's decision to award zones is based solely upon the distribution of covariates in a city and random factors independent of the future performance of the proposed neighborhoods, so that

$$\begin{aligned} W_c^{*} &= T\left(F_c\left(\Omega\right)\right) + \zeta_c \\ W_c &= I\left[W_c^{*} > 0\right] \end{aligned}$$

where $F_c\left(\Omega\right)$ is the Empirical Distribution Function (EDF) of covariates in city c, $T\left(.\right)$ is some functional of the EDF, and ζ_c is a random error in the assignment process.

The above equations in conjunction with (2) imply that

$$\Delta Y_{ict}^{1}, \Delta Y_{ict}^{0} \perp D_{ic} | \Omega_{ic}, P_{ic} = 1 \tag{A.1.}$$

In words, proposed tracts are comparable conditional on their individual level covariates. This follows because $U_{ict} \perp \zeta_c, T\left(F_c\left(\Omega\right)\right)$ for any functional $T\left(.\right)$ – i.e. because conditional on a tract's own covariate levels, its outcomes don't depend on the citywide distribution of covariates or the random assignment error. These are the key assumptions implicit in (2). In results not shown, we have tested the assumption that tract outcomes do not depend on the citywide distribution of covariates by including the characteristics of neighboring tracts in regressions and in our reweighting logits. We find virtually identical results. We take this as evidence that cross-tract dependence in the evolution of outcomes is minimal.

By the Rosenbaum & Rubin (1983) theorem $(A.1.)$ implies

$$\Delta Y_{ict}^{1}, \Delta Y_{ict}^{0} \perp D_{ic} | P\left(D_{ic} = 1 | \Omega_{ic}, P_{ic} = 1\right)$$

Now note that

$$\begin{aligned} P\left(D_{ict} = 1 | \Omega_{ic}, P_{ic} = 1\right) &= P\left(W_c = 1 | \Omega_{ic}, P_{ic} = 1\right) \\ &= E\left[I\left[T\left(F_c\left(\Omega\right)\right) < -\zeta_c\right] | \Omega_{ic}, P_{ic} = 1\right] \\ &= h\left(\Omega_{ic}\right) \end{aligned} \tag{A.2.}$$

where $h\left(.\right)$ is some function. Thus $P\left(D_{ic} = 1 | \Omega_{ic}, P_{ic} = 1\right)$ varies across tracts within a given city. This may seem puzzling given that conditional on being proposed entire cities must either win or lose EZ designation. However, we are not considering $P(W_c = 1|T(F_c(\Omega)), P_{ic} = 1)$ but rather $P(W_c = 1|\Omega_{ic}, P_{ic} = 1)$. The former quantity only varies across cities and is what we are thinking about when we say "the probability of winning." The latter quantity is the probability of a tract being in a winning city given its characteristics and is what the Rosenbaum and Rubin theorem requires we condition on when making inferences. This quantity is consistently estimated via a flexible logit of tract assignment on tract level covariates.

Note also that $(A.2.)$ can be rewritten in a latent variable framework as

$$\begin{aligned} D^*_{ic} &= h(\Omega_{ic}) + \vartheta_{ic} \\ D_{ic} &= I[D^*_{ic} > 0] \end{aligned}$$

where $\vartheta_{ic} \perp \Omega_{ic}, U_{ic}|P_{ic} = 1$ which is equivalent to the expression in (3).

Appendix III: Proofs

Proof of (7)

$$\begin{aligned} E\left[\Delta Y^0_{ict}|D_{ict} = 1\right] &= E\left[E\left[\Delta Y^0_{ict}|D_{ict} = 1, \Omega_{it}\right]|D_{ict} = 1\right] \\ &= E\left[E\left[\Delta Y^0_{ict}|D_{ict} = 0, \Omega_{it}\right]|D_{ict} = 1\right] \\ &= \int E\left[\Delta Y^0_{ict}|D_{ict} = 0, \Omega_{it}\right] dF(\Omega_{it}|D_{ict} = 1) \\ &= \int E\left[\Delta Y^0_{ict}|D_{ict} = 0, \Omega_{it}\right] \frac{dF(\Omega_{it}|D_{ict} = 1)}{dF(\Omega_{it}|D_{ict} = 0)} dF(\Omega_{it}|D_{ict} = 0) \\ &= \int E\left[\Delta Y^0_{ict}|D_{ict} = 0, \Omega_{it}\right] \omega(\Omega_{it})\, dF(\Omega_{it}|D_{ict} = 0) \\ &= E\left[\omega(\Omega_{it})\, E\left[\Delta Y^0_{ict}|D_{ict} = 0, \Omega_{it}\right]|D_{ict} = 0\right] \\ &= E\left[\omega(\Omega_{it})\, \Delta Y^0_{ict}|D_{ict} = 0\right] \end{aligned}$$

by Bayes rule,

$$\omega(\Omega) = \frac{dF(\Omega|D_{ict} = 1)}{dF(\Omega|D_{ict} = 0)} = \frac{P(D_{ict} = 1|\Omega)}{1 - P(D_{ict} = 1|\Omega)} \frac{1 - P(D_{ict} = 1)}{P(D_{ict} = 1)} = \frac{p(\Omega)}{1 - p(\Omega)} \frac{1 - \pi}{\pi}$$

Proof of (8)

$$E[\omega(\Omega_{it})|D_{ict} = 0] = \int \omega(\Omega_{it})\, dF(\Omega_{it}|D_{ict} = 0) = \int dF(\Omega_{it}|D_{ict} = 1) = 1$$

Appendix IV: Inference Procedures

Bootstrap Procedures

We use a nonparametric block bootstrap procedure to assess the sampling variability of the WDD estimator. The steps used are as follows:

1. Sample 8 treated cities and 74 untreated cities with replacement from the original sample.

2. Estimate the propensity score.

3. Compute the statistic of interest T_b^k.

4. Go to step 1 if number of reps is less than 9999, otherwise stop.

We used the empirical bootstrap distribution of T_b^k to calculate single equation p-values and confidence intervals. Asymmetric bootstrap confidence intervals and p-values were constructed using the method described by Davidson and Mackinnon (2004, pp.187-188). P-values and confidence intervals for Naive and OLS models used a studentized bootstrap procedure in order to obtain an asymptotic refinement. None of the tests involving reweighted estimators were studentized.

Benjamini and Hochberg Multiple Testing Procedure

It is well known that conducting multiple tests with a fixed rejection probability does not control the probability of making at least one Type I error across all tests. Standard solutions to the multiple testing problem such as the use of Bonferonni bounds are overly conservative when the tests are correlated or when some of the nulls are false. Benjamini and Hochberg (1995) propose a procedure that controls what they term the False Discovery Rate. Define F as the number of falsely rejected nulls, C as the number of correctly rejected nulls and $R = F + C$ as the total number of rejected hypotheses. The fraction of rejections that are false is a random variable $Q = F/R$. If we define $Q = 0$ in the case where $R = 0$, then the false discovery rate can be written $FDR = E[Q]$. Note that $E[Q] = P(R > 1)\,E[Q|R > 1]$ and so the FDR can be thought of as the probability of rejecting a null times the expected fraction of rejections that are false given that at least one rejection has occurred. In the case where all nulls are true, the false discovery rate equals the probability of a Type I error (also known as the Family Wide Error Rate) since when all rejections are false $FDR = P(R > 1) = P(F > 1)$. When some nulls are false however, the FDR differs from the probability of making a Type I error. It can be shown that in general $FDR \leq P(F > 1)$. As the fraction of nulls that are false increases, the two concepts diverge and the greater will be the gain in power from controlling the FDR instead of $P(F > 1)$.

From a practical perspective, control of the FDR may better approximate the nature of confidence that researchers desire when attempting to make multiple inferences since the seriousness of a false rejection presumably declines in proportion to the total number of rejections made. Control of the FDR provides an average level of confidence in the decisions made rather than a level of confidence in the entire joint decision. However, control of the FDR also provides a proper test of the joint null that all hypotheses are true, for under such a null, the FDR is equivalent to the Family Wide Error Rate and rejection of a single hypothesis constitutes a rejection of the joint null at the specified level. Failure to reject a single hypothesis in the FDR multiple testing framework constitutes a failure to reject the joint null. Because the FDR approach does not rely upon normality, we have a rather robust replacement for conventional χ^2 tests of joint nulls which are known to have poor finite sample performance.

The Benjamini and Hochberg procedure is conducted by listing the p-values $p_1, p_2, ..., p_m$ of the individual tests in increasing order. The level α test procedure rejects all null hypotheses with $p_i < p_k$ where k is the largest i for which $p_i < \frac{i}{m}\alpha$. For convenience we conduct the procedure at three different levels of α. Benjamini and Hochberg's procedure is robust to arbitrary correlation between the tests and maintains control of the FDR regardless of the fraction of nulls which are false.

Appendix V: Specification of Reweighting and Blinder-Oaxaca Estimators

The covariates used in the study are given in Table A3. For the WDD estimator applied to outcomes over the period 1990-2000 we used a linear logit specification with two lags (i.e. 1990 and 1980 values) of all time varying tract and city level variables. For the WDD false experiment which was computed on outcomes over the period 1980-1990, we used two lags (i.e. 1980 and 1970 values) of all time varying tract and city level variables except the following for which we only included one lag due to the presence of frequent missing values in 1970: log rents, log housing values, % travel less than 20 minutes, citywide % employment in manufacturing, citywide % employment in government, and citywide crime rate.The Blinder-Oaxaca models use the same set of covariates as the reweighting logits but also include squares of all tract level variables and interaction terms between tract level poverty, unemployment, population, and housing values.

To construct placebo zones we performed nearest neighbor matching without replacement on a propensity score estimated on all tracts in the eight cities receiving EZs. The propensity score was estimated on the sample of all tracts in the eight treated cities, using a logit of assignment status on two lags of all time varying tract and city level variables, a set of city dummies, and the interaction of the lags of tract level poverty, unemployment, and population with the city dummies. In calculating, the treatment effect on the placebo zones we replaced the treated tracts by the placebo tracts and proceed to compute $\widehat{\text{OB}}$ and $\widehat{WDD}$ (using the previously estimated weights).

We showed in Appendix III that $E[\omega(\Omega_{it})|D_{ict} = 0] = 1$. This provides us with an overidentifying restriction that can be used as a specification test. Large deviations of the mean estimated weight among untreated observations from one are a sign of model misspecification. Appendix Table A4 presents the log of the mean weight, its confidence interval and a pvalue of the null that the population mean weight equals one. Confidence intervals and pvalues were calculated via the block bootstrap. In all models we fail to reject the null that the sum of weights among the untreated is one at conventional levels of significance. We also present two standard measures of goodness of fit: (1) The pseudo r-squared which is defined as $1 - \frac{\log L_0}{\log L_u}$ where L_0 is the likelihood function restricted to all coefficients being zero and L_u is the unrestricted maximized likelihood and (2) The relative frequency of correct positive predictions of treatment. Finally, to get a sense of how much overlap exists in the propensity score distribution across treatment and controls we show the number of treated tracts per untreated tract by strata of the propensity score.

Appendix VI: Construction of Composition Constant Changes

Let p^u_{rt} be the fraction of individuals in some universe u (e.g. 16-19 year old dropouts) belonging to race r at time t and h^u_{rt} the hazard of individuals in such categories experiencing one of the outcomes in Table 9 – e.g. employment, unemployment, poverty, or college education. The mean hazard rates at time t can be written

$$R^u_t = \sum_r p^u_{rt} h^u_{rt}$$

The "composition constant" rate in 2000 assigns 1990 weights to the 2000 hazards

$$\widetilde{R}^u_{00} = \sum_r p^u_{r90} h^u_{r00}$$

So that the composition constant change in rates is

$$\begin{aligned} D^u_{00} &= \widetilde{R}_{00} - R_{90} \\ &= \sum_r p^u_{r90} \left(h^u_{r00} - h^u_{r90}\right) \end{aligned}$$

The construction of the composition constant changes was hampered somewhat by the fact that some tracts had no members of a particular racial group in some years preventing estimation of the hazards. This did not present a problem in the case where one of the p^u_{r90} 's was missing for in such cases regardless of what is imputed for h^u_{r90} the entire term will be zero. But when p^u_{r00} was missing and p^u_{r90} was not we faced a nontrivial censoring problem. We solved this problem by imputing missing values of h^u_{r00} using a linear regression of the observed hazards on all of the covariates used in our reweighting logits plus a dummy for being in an EZ. Imputations were constructed as the sum of the predicted values from the imputation regression plus a draw from a normal distribution with standard deviation equal to the residual mean squared error of the imputation regression. With these imputed hazards we proceeded to compute values of D_{00} for all tracts capable of inclusion in the universe (e.g. all tracts having 16-19 year old dropouts).

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Table 1: 1990 Characteristics of First Round Empowerment Zones

City	Total Population	Population Rank	Population in EZ	Poverty Rate in EZ	Unemp. Rate in EZ	EZ Area (sq. 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
Cleveland	505,556	23	52,985	47	27	6.3
Detroit	1,027,974	7	106,273	47	28	19.5
Los Angeles	3,512,777	2	234,829	40	19	26.1
New York	7,320,621	1	204,625	42	18	6.3
Philadelphia	1,594,339	5	52,440	50	23	4.3

Source: NCDB and HUD

Table 2: Total Spending, by category

	SSBG	Outside Money	Total
<u>*Total*</u>	$386,105,051	$2,847,510,204	$3,233,615,255
<u>*Expenditure by category*</u>			
Access to Capital	$82,614,577	$1,483,436,971	$1,566,051,548
Business Assistance	$56,263,375	$481,612,338	$537,875,713
Workforce Development	$48,040,383	$49,081,906	$97,122,289
Social Improvement	$76,367,835	$163,449,118	$239,816,953
Public Safety	$17,625,210	$254,618,150	$272,243,360
Physical Development	$14,266,234	$82,484,595	$96,750,829
Housing	$71,064,126	$325,951,575	$397,015,701
Capacity Improvement	$19,863,311	$6,875,551	$26,738,862
<u>*Average annual expenditure*</u>			
Access to Capital per firm			$20,881
Business Assistance per firm			$7,172
Workforce Development per unemployed person			$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: HUD PERMS data, Brashares (2000), and Decennial Census

Table 3: Sample Characteristics (1990)

	EZ's	Rejected/ Future Zones (outside EZ cities)	Rejected/ Future Zones (inside EZ cities)	Rejected/ Future Zones	Rejected/ Future Zones Reweighted	Unproposed tracts in treated cities
	[1]	[2]	[3]	[4]	[5]	[6]
Mean (census tracts)						
% Black	0.686	0.540	0.717	0.570	0.677	0.298
Employment Rate	0.379	0.466	0.438	0.461	0.380	0.559
Log(pop)	7.747	7.931	8.068	7.954	7.863	7.954
Log(Rent)	5.857	5.838	5.988	5.863	5.907	6.272
Log(House Value)	10.701	10.829	10.654	10.800	10.593	11.760
Log(Mean Earnings)	9.637	9.591	9.684	9.606	9.627	10.013
Poverty Rate	0.460	0.395	0.388	0.393	0.446	0.188
% Vacant Houses	0.147	0.141	0.121	0.138	0.135	0.069
Unemployment Rate	0.231	0.160	0.206	0.167	0.232	0.100
% In same house	0.560	0.494	0.570	0.506	0.555	0.579
% Travel less 20 min	0.473	0.668	0.447	0.632	0.466	0.429
Prop. age 65+	0.312	0.299	0.325	0.304	0.316	0.232
Prop. female-headed HH	0.623	0.555	0.593	0.562	0.631	0.326
Prop. Latino population	0.220	0.176	0.177	0.176	0.246	0.199
Prop. age <18	0.118	0.121	0.103	0.118	0.103	0.127
% College	0.056	0.090	0.060	0.085	0.053	0.196
% High school dropouts	0.311	0.260	0.291	0.265	0.319	0.191
Prop. of HHs with public assistance	0.353	0.241	0.293	0.250	0.362	0.135
Mean (city)						
Avg. across tracts % black	0.438	0.307	0.480	0.335	0.447	0.333
Total crime / population* 100	0.081	0.105	0.093	0.103	0.083	0.081
% College degree	0.175	0.143	0.168	0.147	0.173	0.148
% of workers in city government	0.049	0.047	0.043	0.046	0.047	0.079
Observations (number of census tracts)	257	1364	271	1635	1635	4495

Table 4: Balance of Control Samples

Difference-in-Differences Estimates

Model		Naïve [1]	Reweighted [2]	Blinder-Oaxaca [3]	B.O. City Pop. [4]
Log(pop)	Coeff.	-0.022	-0.027	0.014	-0.057
	CI	[-0.071 , 0.018]	[-0.141 , 0.099]	[-0.083 , 0.113]	[-0.293 , 0.373]
	p-val	*0.283*	*0.559*	*0.753*	*0.574*
% In same house	Coeff.	0.005	-0.005	-0.007	-0.021
	CI	[-0.013 , 0.030]	[-0.073 , 0.046]	[-0.046 , 0.038]	[-0.140 , 0.062]
	p-val	*0.692*	*0.747*	*0.898*	*0.541*
% Black	Coeff.	0.002	-0.016	-0.003	-0.020
	CI	[-0.032 , 0.037]	[-0.065 , 0.017]	[-0.030 , 0.028]	[-0.113 , 0.112]
	p-val	*0.797*	*0.270*	*0.990*	*0.583*
% College	Coeff.	-0.010***	0.008*	0.000	0.009
	CI	[-0.016 , -0.005]	[0.004 , 0.043]	[-0.020 , 0.017]	[-0.092 , 0.067]
	p-val	*0.000*	*0.014*	*0.998*	*0.758*
Employment Rate	Coeff.	-0.010	0.017	0.017	0.038
	CI	[-0.043 , 0.030]	[-0.008 , 0.075]	[-0.007 , 0.052]	[-0.065 , 0.142]
	p-val	*0.695*	*0.118*	*0.152*	*0.227*
Unemployment Rate	Coeff.	-0.011	-0.002	-0.005	-0.004
	CI	[-0.034 , 0.019]	[-0.047 , 0.033]	[-0.038 , 0.020]	[-0.117 , 0.056]
	p-val	*0.427*	*0.886*	*0.635*	*0.829*
Log(Mean Earnings)	Coeff.	0.003	0.111*	0.028	0.064
	CI	[-0.055 , 0.049]	[0.072 , 0.297]	[-0.078 , 0.097]	[-0.204 , 0.243]
	p-val	*0.972*	*0.006*	*0.624*	*0.464*
Poverty Rate	Coeff.	0.016	-0.010	-0.013	-0.064
	CI	[-0.014 , 0.042]	[-0.060 , 0.045]	[-0.056 , 0.026]	[-0.166 , 0.159]
	p-val	*0.331*	*0.533*	*0.406*	*0.325*
Log(House Value)	Coeff.	0.118	0.095	-0.009	0.149
	CI	[-0.023 , 0.309]	[-0.027 , 0.470]	[-0.203 , 0.223]	[-0.727 , 0.522]
	p-val	*0.092*	*0.092*	*0.958*	*0.561*
Log(Rent)	Coeff.	-0.063*	0.029	0.030	0.121
	CI	[-0.110 , -0.009]	[-0.061 , 0.099]	[-0.021 , 0.101]	[-0.100 , 0.380]
	p-val	*0.022*	*0.414*	*0.196*	*0.180*
% Vacant Houses	Coeff.	0.029*	0.015	-0.005	0.009
	CI	[0.006 , 0.056]	[-0.019 , 0.043]	[-0.045 , 0.028]	[-0.098 , 0.090]
	p-val	*0.014*	*0.288*	*0.662*	*0.817*
Number of Tracts		1635	1502	1625	1625
Number of Cities		79	79	79	79

Estimators: All columns show difference-in-difference estimates in which the change in outcomes over the period 1990-2000 among control tracts in cities winning an EZ is compared with the change in outcomes among control tracts in rejected and future zones in other cities. **[1]** *Naïve* refers to difference in difference estimates without covariate adjustments. **[2]** *Reweighted* refers to propensity score reweighted estimates in which the propensity score was calculated using 1990 and 1980 tract and city level characteristics. **[3]** *Blinder-Oaxaca* computes counterfactual means of control tracts in treated cities via regression methods. **[4]** *B.O. City Pop.* is the Blinder-Oaxaca estimator augmented to include a 3rd order polynomial in 1990 city population. (See Sections IV-B, V-A and Appendix V for details).

Inference: 95% C*onfidence intervals (CI)* and *p-values* were obtained via a pairwise block bootstrap that resampled zones in order to preserve the within zone dependence of the data. See Appendix IV for details. *Significance levels*. A multiple testing procedure described in the Appendix was used to control the False Discovery Rate (FDR) to prespecified levels. The procedure yields lower threshold p-values for fixed level tests than in the single equation case. Stars indicate that a hypothesis can be rejected while controlling the FDR to specified levels: * rejected at 10% FDR, ** rejected at 5% FDR and *** rejected at 1% FDR.

Table 5: Impact of EZ Designation

Difference-in-Differences Estimates

Model		**Naïve** [1]	**Reweighted** [2]	**Blinder-Oaxaca** [3]	**B.O. City Pop.** [4]
Log(pop)	Coeff.	-0.035	0.005	0.024	0.049
	CI	[-0.109 , 0.056]	[-0.085 , 0.105]	[-0.040 , 0.096]	[-0.024 , 0.187]
	p-val	*0.427*	*0.839*	*0.472*	*0.167*
% In same house	Coeff.	-0.009	-0.014	0.001	0.003
	CI	[-0.036 , 0.013]	[-0.052 , 0.006]	[-0.022 , 0.021]	[-0.023 , 0.032]
	p-val	*0.476*	*0.092*	*0.972*	*0.879*
% Black	Coeff.	-0.037	-0.026**	-0.012	-0.020
	CI	[-0.074 , 0.011]	[-0.069 , -0.005]	[-0.036 , 0.014]	[-0.048 , 0.011]
	p-val	*0.131*	*0.026*	*0.346*	*0.164*
% College	Coeff.	0.010	0.023**	0.012	0.014
	CI	[-0.005 , 0.025]	[0.010 , 0.047]	[-0.004 , 0.024]	[-0.006 , 0.027]
	p-val	*0.180*	*0.011*	*0.157*	*0.163*
Employment Rate	Coeff.	0.040**	0.038***	0.020*	0.023
	CI	[0.010 , 0.074]	[0.025 , 0.084]	[0.003 , 0.041]	[0.002 , 0.050]
	p-val	*0.009*	*0.000*	*0.022*	*0.039*
Unemployment Rate	Coeff.	-0.041**	-0.040**	-0.031**	-0.034*
	CI	[-0.072 , -0.013]	[-0.079 , -0.019]	[-0.053 , -0.013]	[-0.057 , -0.012]
	p-val	*0.005*	*0.012*	*0.003*	*0.007*
Log(Mean Earnings)	Coeff.	0.012	0.017	0.028	0.029
	CI	[-0.068 , 0.100]	[-0.050 , 0.114]	[-0.044 , 0.093]	[-0.049 , 0.102]
	p-val	*0.759*	*0.543*	*0.425*	*0.348*
Poverty Rate	Coeff.	-0.049***	-0.050***	-0.038**	-0.044
	CI	[-0.091 , -0.016]	[-0.103 , -0.028]	[-0.058 , -0.013]	[-0.067 , -0.007]
	p-val	*0.000*	*0.000*	*0.008*	*0.030*
Log(House Value)	Coeff.	0.297***	0.224**	0.158	0.183
	CI	[0.093 , 0.538]	[0.078 , 0.506]	[0.006 , 0.332]	[0.005 , 0.367]
	p-val	*0.001*	*0.020*	*0.040*	*0.044*
Log(Rent)	Coeff.	0.005	0.077***	0.044	0.054
	CI	[-0.061 , 0.070]	[0.053 , 0.155]	[-0.001 , 0.099]	[-0.003 , 0.130]
	p-val	*0.896*	*0.001*	*0.056*	*0.061*
% Vacant Houses	Coeff.	0.023	-0.001	0.014	0.006
	CI	[-0.006 , 0.048]	[-0.036 , 0.025]	[-0.007 , 0.037]	[-0.031 , 0.027]
	p-val	*0.128*	*0.681*	*0.177*	*0.790*
Number of Tracts		1892	1869	1892	1892
Number of Cities		82	82	82	82

Estimators: All columns show difference-in-difference estimates in which the change in outcomes over the period 1990-2000 among EZ tracts is compared with the change in outcomes among tracts in rejected and future zones. **[1]** *Naïve* refers to difference in difference estimates without covariate adjustment. **[2]** Reweighted refers to propensity score reweighted estimates in which the propensity score was calculated using 1990 and 1980 tract and city level characteristics. **[3]** Blinder-Oaxaca computes counterfactual means of EZ tracts via regression methods. **[4]** B.O. City Pop. is the Blinder-Oaxaca estimator augmented to include a 3rd order polynomial in 1990 city population. (See Section IV-B and Appendix V for details).

Inference: 95% C*onfidence intervals (CI)* and *p-values* were obtained via a pairwise block bootstrap that resampled zones in order to preserve the within zone dependence of the data. See Appendix IV for details. *Significance levels*. A multiple testing procedure described in the Appendix was used to control the False Discovery Rate (FDR) to prespecified levels. The procedure yields lower threshold p-values for fixed level tests than in the single equation case. Stars indicate that a hypothesis can be rejected while controlling the FDR to specified levels: * rejected at 10% FDR, ** rejected at 5% FDR and *** rejected at 1% FDR.

Table 6: False Experiment I (Lagged Model)

Difference-in-Differences Estimates

Model		Naïve [1]	Reweighted [2]	Blinder-Oaxaca [3]	B.O. City Pop. [4]
Log(pop)	Coeff.	-0.055	0.018	0.016	0.020
	CI	[-0.235 , 0.079]	[-0.081 , 0.122]	[-0.069 , 0.126]	[-0.069 , 0.114]
	p-val	*0.589*	*0.621*	*0.597*	*0.533*
% In same house	Coeff.	0.010	0.001	0.012	0.012
	CI	[-0.012 , 0.041]	[-0.033 , 0.029]	[-0.027 , 0.045]	[-0.030 , 0.046]
	p-val	*0.404*	*0.794*	*0.567*	*0.609*
% Black	Coeff.	-0.050	-0.024	-0.018	-0.017
	CI	[-0.109 , 0.036]	[-0.068 , 0.011]	[-0.066 , 0.022]	[-0.061 , 0.010]
	p-val	*0.228*	*0.170*	*0.385*	*0.185*
% College	Coeff.	0.003	0.001	0.004	0.009
	CI	[-0.007 , 0.011]	[-0.012 , 0.009]	[-0.010 , 0.013]	[-0.009 , 0.018]
	p-val	*0.460*	*0.681*	*0.643*	*0.258*
Employment Rate	Coeff.	0.015	-0.016	-0.017	-0.012
	CI	[-0.015 , 0.039]	[-0.058 , -0.001]	[-0.042 , 0.015]	[-0.041 , 0.021]
	p-val	*0.338*	*0.046*	*0.248*	*0.410*
Unemployment Rate	Coeff.	0.010	0.012	0.004	-0.006
	CI	[-0.009 , 0.035]	[0.001 , 0.054]	[-0.020 , 0.035]	[-0.031 , 0.026]
	p-val	*0.314*	*0.045*	*0.633*	*0.758*
Log(Mean Earnings)	Coeff.	0.007	-0.013	0.019	0.037
	CI	[-0.076 , 0.064]	[-0.109 , 0.049]	[-0.068 , 0.085]	[-0.060 , 0.115]
	p-val	*0.836*	*0.578*	*0.687*	*0.364*
Poverty Rate	Coeff.	-0.022	0.034*	0.020	0.010
	CI	[-0.050 , 0.011]	[0.021 , 0.087]	[-0.019 , 0.059]	[-0.028 , 0.050]
	p-val	*0.206*	*0.005*	*0.238*	*0.448*
Log(House Value)	Coeff.	0.091	-0.050	-0.087	-0.100
	CI	[-0.124 , 0.284]	[-0.282 , 0.134]	[-0.292 , 0.170]	[-0.300 , 0.148]
	p-val	*0.400*	*0.515*	*0.442*	*0.391*
Log(Rent)	Coeff.	0.036	-0.041	-0.006	-0.011
	CI	[-0.078 , 0.129]	[-0.139 , 0.005]	[-0.085 , 0.102]	[-0.084 , 0.092]
	p-val	*0.502*	*0.063*	*0.931*	*0.817*
% Vacant Houses	Coeff.	-0.002	0.015	-0.002	-0.004
	CI	[-0.029 , 0.027]	[-0.011 , 0.041]	[-0.032 , 0.017]	[-0.031 , 0.018]
	p-val	*0.992*	*0.247*	*0.710*	*0.593*
Number of Tracts		1891	1882	1891	1891
Number of Cities		82	82	82	82

Estimators: All columns show difference-in-difference estimates in which the change in outcomes over the period 1980-1990 among EZ tracts is compared with the change in outcomes among tracts in rejected and future zones. **[1]** *Naïve* refers to difference in difference estimates without covariate adjustment. **[2]** Reweighted refers to propensity score reweighted estimates in which the propensity score was calculated using 1980 and 1970 tract and city level characteristics. **[3]** Blinder-Oaxaca computes counterfactual means of EZ tracts via regression methods. **[4]** B.O. City Pop. is the Blinder-Oaxaca estimator augmented to include a 3rd order polynomial in 1980 city population. (See Sections IV-B, V-C and Appendix V for details).

Inference: 95% C*onfidence intervals (CI)* and *p-values* were obtained via a pairwise block bootstrap that resampled zones in order to preserve the within zone dependence of the data. See Appendix IV for details. *Significance levels*. A multiple testing procedure described in the Appendix was used to control the False Discovery Rate (FDR) to prespecified levels. The procedure yields lower threshold p-values for fixed level tests than in the single equation case. Stars indicate that a hypothesis can be rejected while controlling the FDR to specified levels: * rejected at 10% FDR, ** rejected at 5% FDR and *** rejected at 1% FDR.

Table 7: False Experiment II (Placebo Zones)

Difference-in-Differences Estimates

Model		Reweighted			Blinder-Oaxaca City Pop.		
Sample		**All**	**Close Tracts**	**Faraway Tracts**	**All**	**Close Tracts**	**Faraway Tracts**
		[1]	[2]	[3]	[4]	[5]	[6]
Log(pop)	Coeff.	0.018	0.046	0.049	0.052	0.071	0.024
	CI	[-0.130 , 0.459]	[-0.087 , 0.481]	[-0.093 , 0.509]	[-0.024 , 0.166]	[-0.004 , 0.203]	[-0.049 , 0.143]
	p-val	*0.894*	*0.523*	*0.565*	*0.149*	*0.062*	*0.415*
% In same house	Coeff.	-0.005	-0.007	-0.002	0.010	0.013	0.006
	CI	[-0.098 , 0.034]	[-0.099 , 0.036]	[-0.093 , 0.037]	[-0.013 , 0.040]	[-0.006 , 0.048]	[-0.022 , 0.039]
	p-val	*0.677*	*0.586*	*0.830*	*0.333*	*0.158*	*0.616*
% Black	Coeff.	0.015	0.016	0.030	-0.010	-0.006	-0.015
	CI	[-0.163 , 0.077]	[-0.159 , 0.083]	[-0.150 , 0.090]	[-0.037 , 0.020]	[-0.035 , 0.028]	[-0.048 , 0.020]
	p-val	*0.639*	*0.633*	*0.441*	*0.367*	*0.612*	*0.343*
% College	Coeff.	0.014	0.011	0.028	0.011	0.008	0.024
	CI	[-0.012 , 0.086]	[-0.010 , 0.079]	[-0.009 , 0.109]	[-0.018 , 0.035]	[-0.018 , 0.033]	[-0.018 , 0.061]
	p-val	*0.255*	*0.245*	*0.142*	*0.472*	*0.561*	*0.275*
Employment Rate	Coeff.	0.002	-0.004	-0.003	0.001	-0.005	0.016
	CI	[-0.023 , 0.061]	[-0.031 , 0.052]	[-0.029 , 0.053]	[-0.026 , 0.033]	[-0.029 , 0.021]	[-0.015 , 0.051]
	p-val	*0.544*	*0.828*	*0.786*	*0.904*	*0.645*	*0.294*
Unemployment Rate	Coeff.	-0.018	-0.005	-0.023	-0.009	-0.001	-0.018
	CI	[-0.082 , 0.118]	[-0.068 , 0.128]	[-0.080 , 0.120]	[-0.028 , 0.012]	[-0.016 , 0.018]	[-0.039 , 0.006]
	p-val	*0.349*	*0.545*	*0.311*	*0.384*	*0.942*	*0.152*
Log(Mean Earnings)	Coeff.	0.005	-0.006	0.010	0.040	0.045	0.019
	CI	[-0.128 , 0.130]	[-0.148 , 0.116]	[-0.127 , 0.153]	[-0.020 , 0.111]	[-0.032 , 0.129]	[-0.051 , 0.106]
	p-val	*0.952*	*0.873*	*0.897*	*0.144*	*0.194*	*0.557*
Poverty Rate	Coeff.	-0.013	0.002	0.002	-0.022	-0.021	-0.022
	CI	[-0.092 , 0.041]	[-0.072 , 0.052]	[-0.071 , 0.050]	[-0.052 , 0.015]	[-0.055 , 0.023]	[-0.054 , 0.016]
	p-val	*0.657*	*0.928*	*0.980*	*0.202*	*0.243*	*0.235*
Log(House Value)	Coeff.	0.058	0.058	0.005	0.152	0.143	0.135
	CI	[-0.160 , 1.556]	[-0.181 , 1.584]	[-0.217 , 1.484]	[-0.008 , 0.309]	[-0.060 , 0.341]	[-0.028 , 0.287]
	p-val	*0.741*	*0.736*	*0.955*	*0.057*	*0.103*	*0.081*
Log(Rent)	Coeff.	0.055	0.047	0.073	0.042	0.032	0.051
	CI	[-0.024 , 0.143]	[-0.024 , 0.125]	[-0.006 , 0.166]	[-0.009 , 0.117]	[-0.020 , 0.112]	[-0.008 , 0.138]
	p-val	*0.114*	*0.148*	*0.066*	*0.107*	*0.193*	*0.092*
% Vacant Houses	Coeff.	0.006	0.009	-0.006	-0.003	-0.002	-0.007
	CI	[-0.109 , 0.038]	[-0.107 , 0.042]	[-0.123 , 0.026]	[-0.043 , 0.017]	[-0.044 , 0.019]	[-0.047 , 0.012]
	p-val	*0.840*	*0.744*	*0.676*	*0.613*	*0.652*	*0.405*
Number of Tracts		1892	1867	1892	1892	1867	1892
Number of Cities		82	82	82	82	82	82

Estimators: All columns show difference-in-difference estimates in which the change in outcomes over the period 1990-2000 among EZ tracts is compared with the change in outcomes among tracts in rejected and future zones. **[1]** *Naïve* refers to difference in difference estimates without covariate adjustment. **[2]** Reweighted refers to propensity score reweighted estimates in which the propensity score was calculated using 1990 and 1980 tract and city level characteristics. **[3]** Blinder-Oaxaca computes counterfactual means of EZ tracts via regression methods. **[4]** B.O. City Pop. is the Blinder-Oaxaca estimator augmented to include a 3rd order polynomial in 1990 city population. (See Sections IV-B, V-C and Appendix V for details) .

Definition of placebo zones. All: tracts inside treated EZ cities but outside the EZ that are a nearest neighbor match for an EZ tract based upon the estimated pscore. **Close/Faraway:** Tracts inside treated EZ cities but outside the EZ and less/more than a mile away from it, that are a nearest neighbor match for an EZ tract based upon the estimated pscore.

Inference: 95% Confidence intervals (CI) and p-values were obtained via a pairwise block bootstrap that resampled zones in order to preserve the within zone dependence of the data. See Appendix IV for details. *Significance levels* . A multiple testing procedure described in the Appendix was used to control the False Discovery Rate (FDR) to prespecified levels. The procedure yields lower threshold pvalues for fixed level tests than in the single equation case. Stars indicate that a hypothesis can be rejected while controlling the FDR to specified levels: * rejected at 10% FDR, ** rejected at 5% FDR and *** rejected at 1% FDR.

Table 8: Impact of EZ Designation on Percentile Rank Outcomes

Difference-in-Differences Estimates

		Real Experiment		*False Experiment*		*Placebo Experiment*	
Model		**Reweighted**	**B.O. City Pop.**	**Reweighted**	**B.O. City Pop.**	**Reweighted**	**B.O. City Pop.**
		[1]	[2]	[3]	[4]	[5]	[6]
Log(pop)	Coeff.	-0.001	0.000	0.010	0.014	-0.009	0.010
	CI	[-0.036 , 0.037]	[-0.036 , 0.074]	[-0.032 , 0.051]	[-0.025 , 0.048]	[-0.073 , 0.248]	[-0.021 , 0.074]
	p-val	*0.984*	*0.979*	*0.545*	*0.340*	*0.601*	*0.553*
% In same house	Coeff.	-0.018	0.034	0.012	0.026	-0.012	0.037
	CI	[-0.125 , 0.021]	[-0.021 , 0.085]	[-0.073 , 0.086]	[-0.070 , 0.107]	[-0.293 , 0.074]	[-0.027 , 0.103]
	p-val	*0.144*	*0.182*	*0.984*	*0.593*	*0.594*	*0.193*
% Black	Coeff.	-0.031	-0.031	-0.003	0.020	0.006	-0.016
	CI	[-0.072 , 0.005]	[-0.059 , 0.009]	[-0.024 , 0.026]	[-0.007 , 0.048]	[-0.090 , 0.068]	[-0.045 , 0.018]
	p-val	*0.076*	*0.106*	*0.929*	*0.110*	*0.669*	*0.287*
% College	Coeff.	0.055*	0.034	-0.009	0.031	-0.002	0.012
	CI	[0.003 , 0.115]	[-0.024 , 0.079]	[-0.063 , 0.018]	[-0.014 , 0.062]	[-0.091 , 0.176]	[-0.065 , 0.075]
	p-val	*0.036*	*0.262*	*0.266*	*0.157*	*0.987*	*0.761*
Employment Rate	Coeff.	0.049**	0.028	-0.019	-0.015	-0.013	-0.010
	CI	[0.016 , 0.107]	[-0.016 , 0.095]	[-0.074 , 0.003]	[-0.058 , 0.049]	[-0.059 , 0.117]	[-0.065 , 0.080]
	p-val	*0.005*	*0.216*	*0.066*	*0.607*	*0.707*	*0.655*
Unemployment Rate	Coeff.	-0.060	-0.064	0.006	-0.034	-0.011	-0.023
	CI	[-0.125 , 0.007]	[-0.119 , -0.003]	[-0.019 , 0.076]	[-0.113 , 0.004]	[-0.095 , 0.166]	[-0.080 , 0.033]
	p-val	*0.072*	*0.041*	*0.207*	*0.067*	*0.777*	*0.451*
Log(Mean Earnings)	Coeff.	0.053	0.059	0.019	0.027	0.025	0.033
	CI	[-0.004 , 0.143]	[-0.003 , 0.122]	[-0.029 , 0.105]	[-0.044 , 0.083]	[-0.180 , 0.126]	[-0.016 , 0.103]
	p-val	*0.067*	*0.057*	*0.256*	*0.320*	*0.521*	*0.167*
Poverty Rate	Coeff.	-0.055**	-0.053	0.010	0.000	-0.004	-0.019
	CI	[-0.106 , -0.015]	[-0.079 , -0.019]	[-0.014 , 0.047]	[-0.030 , 0.020]	[-0.189 , 0.072]	[-0.066 , 0.024]
	p-val	*0.010*	*0.014*	*0.271*	*0.799*	*0.969*	*0.418*
Log(House Value)	Coeff.	0.082	0.057	0.002	-0.005	-0.005	0.010
	CI	[-0.009 , 0.184]	[-0.044 , 0.148]	[-0.051 , 0.056]	[-0.091 , 0.077]	[-0.115 , 0.449]	[-0.072 , 0.097]
	p-val	*0.070*	*0.239*	*0.804*	*0.943*	*0.851*	*0.798*
Log(Rent)	Coeff.	0.069**	0.050	-0.011	0.017	0.050	0.040
	CI	[0.038 , 0.142]	[-0.003 , 0.178]	[-0.067 , 0.037]	[-0.024 , 0.074]	[-0.027 , 0.164]	[-0.036 , 0.185]
	p-val	*0.004*	*0.062*	*0.394*	*0.402*	*0.219*	*0.330*
% Vacant Houses	Coeff.	0.004	0.015	-0.040	-0.064	0.027	-0.011
	CI	[-0.140 , 0.092]	[-0.079 , 0.103]	[-0.135 , 0.039]	[-0.182 , 0.041]	[-0.601 , 0.152]	[-0.089 , 0.055]
	p-val	*0.813*	*0.782*	*0.275*	*0.208*	*0.749*	*0.657*
Number of Tracts		1869	1882	1882	1882	1892	1892
Number of Cities		82	82	82	82	82	82

Note. For details regarding estimation and experiments see Sections IV-B, V-C and Appendix V as well as notes to Tables 4-7. **Inference:** 95% Confidence intervals (CI) and p-values were obtained via a pairwise block bootstrap. Stars indicate that a hypothesis can be rejected while controlling the FDR to specified levels: * rejected at 10% FDR, ** rejected at 5% FDR and *** rejected at 1% FDR.

Table 9: Composition-Constant Impact of EZ Designation

Difference-in-Differences Estimates

		Real Experiment		*Placebo Experiment*	
Model		**Reweighted**	**B.O. City Pop.**	**Reweighted**	**B.O. City Pop.**
		[1]	[2]	[3]	[4]
Employment Rate	Coeff.	0.033***	0.024	-0.002	0.004
	CI	[0.019 , 0.079]	[0.000 , 0.054]	[-0.031 , 0.061]	[-0.025 , 0.040]
	p-val	0.001	0.053	0.867	0.760
Unemployment Rate	Coeff.	-0.029**	-0.023	-0.014	-0.003
	CI	[-0.066 , -0.003]	[-0.044 , 0.000]	[-0.078 , 0.141]	[-0.020 , 0.016]
	p-val	0.035	0.047	0.442	0.797
Poverty Rate	Coeff.	-0.045***	-0.036	-0.012	-0.022
	CI	[-0.097 , -0.025]	[-0.061 , 0.001]	[-0.109 , 0.043]	[-0.057 , 0.016]
	p-val	0.000	0.055	0.653	0.198
Employment Rate 16-19 in HS	Coeff.	-0.003	-0.008	0.022	0.009
	CI	[-0.059 , 0.061]	[-0.105 , 0.029]	[-0.042 , 0.164]	[-0.069 , 0.055]
	p-val	0.804	0.507	0.277	0.855
Employment Rate 16-19 HS drop.	Coeff.	0.103**	0.101	0.040	0.062
	CI	[0.039 , 0.193]	[-0.072 , 0.188]	[-0.117 , 0.165]	[-0.104 , 0.163]
	p-val	0.008	0.143	0.570	0.337
Employment Rate 16-19 HS grad.	Coeff.	0.129**	0.096	0.032	0.062
	CI	[0.046 , 0.369]	[-0.040 , 0.300]	[-0.132 , 0.360]	[-0.064 , 0.199]
	p-val	0.012	0.141	0.529	0.318
% College	Coeff.	0.012	-0.018	0.010	0.029
	CI	[-0.022 , 0.040]	[-0.083 , 0.084]	[-0.030 , 0.054]	[-0.042 , 0.092]
	p-val	0.443	0.472	*0.621*	*0.306*
Number of Tracts		1869	1869	1892	1892
Number of Cities		82	82	82	82

Note: *Racial-composition-constant outcomes* fix the racial composition of a census tract to that observed in 1990. For details see Sections IV-B, V-D and Appendix V as well as notes to Tables 4-7. *Inference:* 95% Confidence intervals (CI) and p-values were obtained via a pairwise block bootstrap. Stars indicate that a hypothesis can be rejected while controlling the FDR to specified levels: * rejected at 10% FDR, ** rejected at 5% FDR and *** rejected at 1% FDR.

Table 10: Impact Calculations

Panel (A)	**Decennial census data** *(inside Empowerment Zones, 1990)*		**Notes**
Population	Total Population	967,851	
	Total Population 16+	718,202	
	Employed	259,271	
	Persons in labor force	330,373	
Earnings	Average annual wage (in 1990 dollars)	16,182	
Panel (B)	**Estimated effects of the Empowerment Zones program**		
Labor Market	Decrease in the unemployed between 1995 and 2000	13,215	0.041 x Persons in labor force
	Decrease in poverty headcount between 1995 and 2000	48,393	0.049 x Total population
	Employment Increase between 1995 and 2000	27,292	0.039 x Population 16+
	Wage value of the number of jobs created	441,634,034	(0.039 x Population 16+) x Ave. Annual wage
	Present discounted value of jobs created	1,104,085,085	Wage value/[1-β(1-d)], with d=1/3 (separation rate) and β=0.9 (social disc. factor)
Housing Market	Average change in annual tractwide rent	319	0.077 * Median annual rent inside EZs
	Total increase in annual rents inside EZ	78,011,523	Average increase in monthly rent x 12 x # of rented houses
	Present Value	780,115,228	Total Annual Rents / 0.1
	Average change in tract-wide owner occupied housing value	9,707	0.224 * Median housing value inside EZs
	Total increase in owner-occupied housing value inside EZ	474,497,043	Average change in tract-wide value x # of owner occupied houses
	Total increase of value of EZ housing units	1,254,612,272	Increase in rent asset value + Increase in value benefiting EZ residents

Note: The coefficients 0.040 (for unemployment rate), 0.050 (for poverty rate), and 0.038 (for employment rate), 0.077 (for rent) and 0.224 (for housing values) were obtained from Column 2 of Table 5.

Figure 1: Chicago Empowerment Zone

Figure 2: Time Series Characteristics

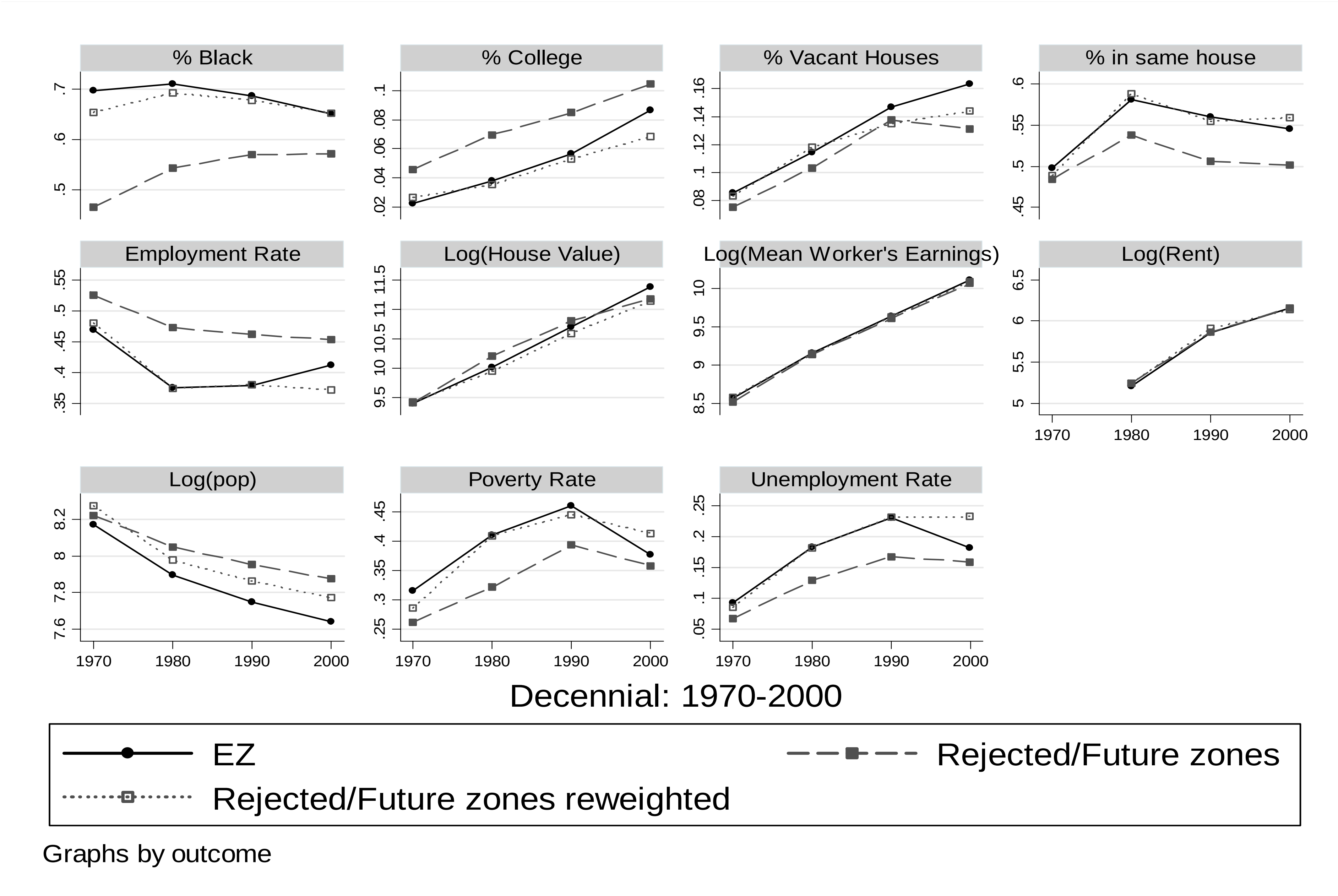

Figure 3
Chicago: Empowerment Zone and Placebo Tracts

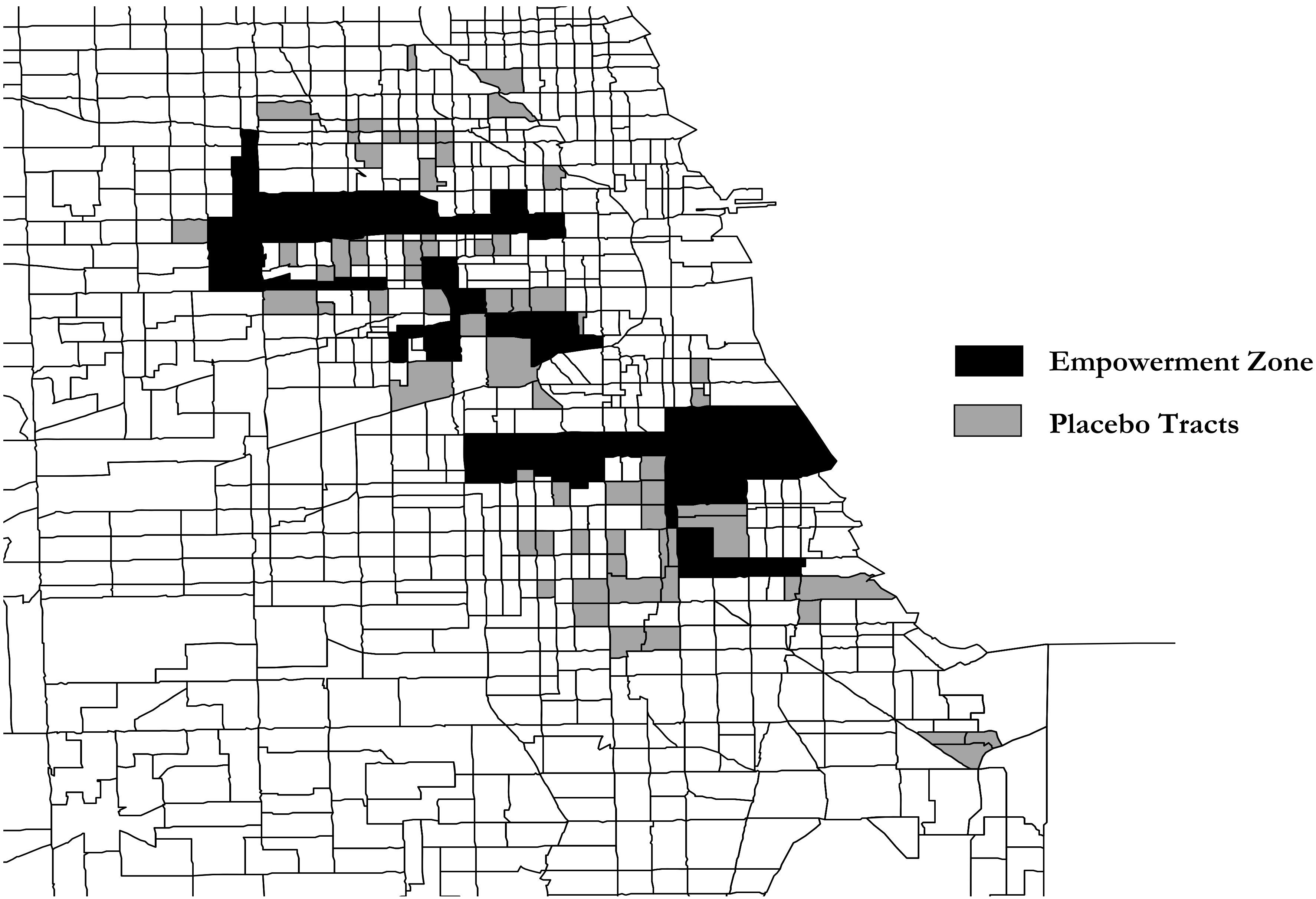

Table A1: Treatment by city

City	EZ (Round I) (1994)	Application (1994)	Round 1 (1994)	Round 2 (2000)	Round 3 (2002)
Akron		x	EC-1		
Albany		x	EC-1		
Albuquerque		x	EC-1		
Anniston		x			
Atlanta	x	x			
Austin		x			
Baltimore	x	x			
Benton Harbor		x			
Birmingham		x	EC-1		
Boston		x	EEC-1	EZ-2	
Bridgeport		x	EC-1		
Buffalo					RC
Charleston		x			RC
Charlotte		x	EC-1		
Chattanooga					RC
Chester		x			
Chicago	x	x			RC
Cincinnati				EZ-2	
Cleveland	x	x			
Columbia				EZ-2	
Columbus				EZ-2	
Corpus Christi					RC
Cumberland				EZ-2	
Dallas		x	EC-1		
Denver		x	EC-1		
Des Moines		x	EC-1		
Detroit	x	x			RC
El Paso		x	EC-1	EZ-2	
Fairbanks		x			
Flint		x			RC
Fort Lauderdale		x			
Fort Worth		x			
Fresno		x			EZ-3
Gary		x		EZ-2	
Greeley		x			
Hamilton					RC
Harrisburg		x	EC-1		
Hartford		x			
Houston		x	EEC-1		
Huntington				EZ-2	
Indianapolis		x	EC-1		
Jackson		x	EC-1		
Jacksonville		x			EZ-3
Kansas		x	EEC-1		
Knoxville		x		EZ-2	
Lake Charles		x			
Las Vegas		x	EC-1		
Lawrence					RC
Little Rock		x	EC-1		EZ-3
Los Angeles	x	x			RC
Louisville		x	EC-1		
Lowell					RC
Manchester		x	EC-1		
Memphis		x			RC
Miami		x	EC-1	EZ-2	
Milwaukee		x			RC
Minneapolis		x	EC-1	EZ-2	
Mobile		x			RC
Monroe		x			RC
Muskegon		x	EC-1		
Nashville-Davidso		x	EC-1		
New Haven		x	EC-1	EZ-2	
New Orleans		x			RC
New York	x	x			
Newburgh		x	EC-1		
Niagara Falls					RC
Norfolk		x	EC-1	EZ-2	
Oakland		x	EEC-1		
Ogden		x	EC-1		
Oklahoma		x	EC-1		EZ-3
Omaha		x	EC-1		
Orange		x			
Peoria		x			
Philadelphia	x	x			RC
Phoenix		x	EC-1		
Pine Bluff		x			
Pittsburgh		x	EC-1		
Port Arthur		x			
Portland		x	EC-1		
Providence		x	EC-1		
Richmond		x			
Rochester		x			RC
Sacramento		x			
San Antonio		x	EC-1		EZ-3
San Diego		x			RC
San Francisco					RC
Santa Ana				EZ-2	
Savannah		x			
Schenectady					RC
Seattle		x	EC-1		
Shreveport		x			
Sioux		x			
Springfield		x	EC-1		
St. Louis		x	EC-1	EZ-2	
St. Paul		x	EC-1		
Steubenville		x			
Syracuse					EZ-3
Tacoma		x			RC
Tampa		x	EC-1		
Tucson		x			EZ-3
Waco		x	EC-1		
Washington		x	EC-1		UEnZ
Wilmington		x	EC-1		
Yakima					RC
Yonkers					EZ-3
Youngstown					

Note: 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 Empowrment Zone awarded in Round II (2000), RC refers to Renewal Community awarded in Round III (2002), EZ-3 refers to Empowrment Zone awarded in Round III (2002) and UEnZ Urban Enterprise Zone awarded in Round III (2002)

Table A2: Logit Model Selection

	BIC		AIC	
Model:	*1 Lag*	*2 Lags*	*1 Lag*	*2 Lags*
Linear models				
(1) Basic	1413	1347	1391	1308
(2) Basic + Other outcomes	1405	1342	1366	1270
(3) Basic + Other outcomes + Other tract-level covariates	1207	1236	1096	1025
(4) Basic + Other outcomes + Other tract-level covariates + City covariates	1182	1135	1049	880
*Linear + Squares models**				
(5) Basic	1413	1347	1391	1308
(6) Basic + Other outcomes	1405	1342	1366	1270
(7) Basic + Other outcomes + Other tract-level covariates	1207	1236	1096	1025

Covariates included in each model:

Basic: Poverty Rate, Log(pop), Unemployment Rate.

Other outcomes: % Black, % Travel less 20 min, Employment Rate, Log(Rent), Log(House Value), Log(Mean Earnings), % Vacant Houses, % in same house, % College degree.

Other tract-level covariates: Prop. female-headed HH, Log family income*, Prop. Latino population, Prop. age <18, Prop. age 65+, % High school dropouts, Log rent, Log house value, Prop. of HHs with public assistance.

City level covariates: % black, Total crime / population* 100, % 65+ years old, % College degree, % of workers in manufacturing, % of workers in city government.

Note: * These models include linear and squared terms of tract level variables.

Table A3. Logits

		Real Experiment							False Experiment				
		Baseline	Big City	All	No NY-LA	No CLE-LA	Balance		Baseline	Big City	All	No NY-LA	No CLE-LA
% Black	[1980]	1.482	1.889	0.275	-0.846	-2.834	-7.95	[1970]	1.184	1.179	0.692	1.311	2.044
		[0.141]	[0.056]*	[0.717]	[0.244]	[0.454]	[0.009]***		[0.086]*	[0.090]*	[0.400]	[0.065]*	[0.019]**
	[1990]	-1.576	-1.856	-1.321	-0.647	1.404	3.832	[1980]	-0.746	-0.716	-1.063	-1.984	-1.345
		[0.210]	[0.116]	[0.231]	[0.529]	[0.711]	[0.184]		[0.413]	[0.438]	[0.216]	[0.019]**	[0.337]
% College	[1980]	-7.112	-7.886	-5.638	-1.957	2.519	23.706	[1970]	-10.398	-10.976	-12.682	-17.509	-15.034
		[0.060]*	[0.025]**	[0.072]*	[0.614]	[0.599]	[0.000]***		[0.006]***	[0.005]***	[0.024]**	[0.001]***	[0.041]**
	[1990]	6.778	7.74	4.09	0.826	11.394	2.523	[1980]	-0.422	-1.103	0.581	1.487	6.916
		[0.078]*	[0.035]**	[0.139]	[0.631]	[0.006]***	[0.551]		[0.871]	[0.654]	[0.840]	[0.595]	[0.074]*
% High school dropouts	[1980]	-1.802	-2.17	1.477	1.25	7.615	9.024	[1970]	2.479	2.009	1.961	-1.265	0.56
		[0.472]	[0.419]	[0.633]	[0.701]	[0.002]***	[0.020]**		[0.612]	[0.680]	[0.723]	[0.831]	[0.866]
	[1990]	5.056	5.105	6.461	6.242	6.84	-3.261	[1980]	-0.119	-0.27	1.847	0.966	7.168
		[0.035]**	[0.022]**	[0.021]**	[0.056]*	[0.006]***	[0.302]		[0.975]	[0.945]	[0.623]	[0.801]	[0.015]**
% Travel less 20 min	[1980]	0.206	-0.154	-0.054	0.846	-2.376	-8.078	[1980]	-2.238	-2.339	-1.542	-1.62	-5.95
		[0.774]	[0.823]	[0.959]	[0.376]	[0.025]**	[0.000]***		[0.024]**	[0.009]***	[0.131]	[0.201]	[0.000]***
	[1990]	-3.171	-3.725	-1.575	-3.411	-9.303	-19.747						
		[0.064]*	[0.023]**	[0.398]	[0.053]*	[0.000]***	[0.000]***						
% Vacant Houses	[1980]	-2.783	-3.286	-1.791	-2.364	-2.722	6.71	[1970]	0.307	0.024	3.722	0.459	1.174
		[0.260]	[0.169]	[0.554]	[0.408]	[0.138]	[0.001]***		[0.861]	[0.990]	[0.093]*	[0.856]	[0.608]
	[1990]	2.577	2.314	4.714	4.456	0.986	-6.374	[1980]	-3.98	-4.365	-3.865	-1.663	-6.137
		[0.098]*	[0.157]	[0.000]***	[0.000]***	[0.671]	[0.004]***		[0.044]**	[0.045]**	[0.093]*	[0.466]	[0.023]**
% in same house	[1980]	-0.883	-0.987	-1.629	-2.122	4.448	4.872	[1970]	0.235	-0.285	0.094	-0.387	-0.119
		[0.430]	[0.335]	[0.134]	[0.027]**	[0.000]***	[0.003]***		[0.863]	[0.823]	[0.957]	[0.857]	[0.936]
	[1990]	2.667	2.246	3.029	2.211	4.59	7.93	[1980]	-0.263	-0.418	-0.241	-0.587	2.679
		[0.069]*	[0.148]	[0.031]**	[0.137]	[0.003]***	[0.007]***		[0.804]	[0.718]	[0.855]	[0.705]	[0.008]***
Employment Rate	[1980]	-3.512	-4.222	-0.832	-4.855	-8.2	2.199	[1970]	-1.044	-0.354	2.38	2.274	3.015
		[0.225]	[0.136]	[0.814]	[0.181]	[0.040]**	[0.438]		[0.695]	[0.897]	[0.467]	[0.541]	[0.427]
	[1990]	2.429	2.624	9.254	9.919	7.347	8.501	[1980]	-2.772	-3.105	2.236	0.789	-5.462
		[0.502]	[0.490]	[0.000]***	[0.000]***	[0.042]**	[0.004]***		[0.513]	[0.464]	[0.460]	[0.798]	[0.225]
Log(Area)	[1980]	-0.247	-0.181	-0.754	-0.678	-0.352	-0.615	[1970]	-0.393	-0.36	-0.825	-0.99	-0.485
		[0.469]	[0.588]	[0.002]***	[0.002]***	[0.187]	[0.069]*		[0.192]	[0.238]	[0.000]***	[0.000]***	[0.104]
Log(House Value)	[1980]	-1.094	-1.036	-1.94	-2.047	-1.386	-1.732	[1980]	-0.677	-0.711	-1.249	-0.732	-1.281
		[0.005]***	[0.020]**	[0.000]***	[0.000]***	[0.058]*	[0.035]**		[0.123]	[0.118]	[0.044]**	[0.148]	[0.069]*
	[1990]	-0.396	-0.309	-0.866	-0.131	-2.022	-2.815						
		[0.387]	[0.459]	[0.111]	[0.713]	[0.000]***	[0.000]***						
Log(Mean Earnings)	[1980]	-0.119	0.028	0.03	-0.596	-0.992	-4.703	[1970]	3.901	3.94	3.264	3.387	7.368
		[0.886]	[0.972]	[0.978]	[0.552]	[0.243]	[0.001]***		[0.003]***	[0.002]***	[0.007]***	[0.007]***	[0.001]***
	[1990]	0.87	0.766	0.898	1.472	1.344	0.582	[1980]	1.125	1.181	1.066	0.627	-0.229
		[0.038]**	[0.081]*	[0.082]*	[0.000]***	[0.046]**	[0.624]		[0.134]	[0.140]	[0.219]	[0.516]	[0.787]
Log(Rent)	[1980]	-0.388	0.017	-0.328	-0.365	-0.385	-5.461	[1980]	0.725	0.979	-0.197	-0.342	1.24
		[0.743]	[0.988]	[0.829]	[0.816]	[0.661]	[0.000]***		[0.451]	[0.333]	[0.833]	[0.693]	[0.241]
	[1990]	-0.525	-0.637	-2.309	-1.447	1.514	3.22	[1970]	-0.737	-0.713	-0.526	-0.402	-1.322
		[0.499]	[0.422]	[0.017]**	[0.124]	[0.192]	[0.008]***		[0.114]	[0.140]	[0.307]	[0.519]	[0.021]**
Log(pop)	[1980]	-1.257	-1.018	-1.205	-1.266	-2.34	-0.014	[1980]	0.366	0.38	0.038	0.006	1.052
		[0.009]***	[0.052]*	[0.002]***	[0.010]***	[0.031]**	[0.991]		[0.403]	[0.395]	[0.944]	[0.992]	[0.006]***
	[1990]	0.987	0.761	0.929	1.07	2.615	1.326						
		[0.062]*	[0.181]	[0.006]***	[0.005]***	[0.033]**	[0.314]						

* significant at 10%; ** significant at 5%; *** significant at 1%. Clustered-robust pvalues in brackets

Table A3. Logits *(Cont.)*

		Real Experiment							False Experiment				
		Baseline	Big City	All	No NY-LA	No CLE-LA	Balance		Baseline	Big City	All	No NY-LA	No CLE-LA
Poverty Rate	[1980]	1.335	1.663	1.878	0.695	4.279	-1.167	[1970]	4.164	3.954	3.145	-0.76	5.606
		[0.409]	[0.323]	[0.311]	[0.703]	[0.215]	[0.769]		[0.009]***	[0.018]**	[0.151]	[0.664]	[0.010]**
	[1990]	4.018	3.96	5.422	6.032	5.849	3.823	[1980]	5.302	5.48	7.44	6.742	5.587
		[0.011]**	[0.014]**	[0.000]***	[0.000]***	[0.044]**	[0.149]		[0.001]***	[0.002]***	[0.000]***	[0.000]***	[0.009]***
Prop. Latino population	[1980]	-3.816	-3.757	-0.45	-0.619	-7.791	-8.159	[1970]	-0.558	-0.82	0.47	0.57	-3.201
		[0.016]**	[0.021]**	[0.867]	[0.843]	[0.017]**	[0.021]**		[0.646]	[0.487]	[0.776]	[0.727]	[0.061]*
	[1990]	6.158	6.535	2.247	3.18	11.393	9.342	[1980]	3.06	3.461	1.966	2.722	6.349
		[0.016]**	[0.013]**	[0.383]	[0.347]	[0.015]**	[0.000]***		[0.046]**	[0.020]**	[0.337]	[0.278]	[0.002]***
Prop. age 65+	[1980]	-3.022	-4.613	-5.906	-3.97	-5.533	-14.262	[1970]	0.296	0.365	0.827	-1.112	-0.07
		[0.350]	[0.111]	[0.037]**	[0.027]**	[0.209]	[0.027]**		[0.903]	[0.878]	[0.732]	[0.652]	[0.980]
	[1990]	-8.383	-7.448	-12.481	-11.73	-12.038	-5.988	[1980]	-10.339	-10.893	-11.898	-10.211	-13.855
		[0.003]***	[0.006]***	[0.001]***	[0.001]***	[0.002]***	[0.064]*		[0.000]***	[0.000]***	[0.000]***	[0.000]***	[0.000]***
Prop. age <18	[1980]	-2.42	-3.184	-1.863	-2.455	-5.487	-7.649	[1970]	-0.984	0.462	2.01	-0.261	6.307
		[0.276]	[0.102]	[0.466]	[0.210]	[0.221]	[0.188]		[0.797]	[0.910]	[0.672]	[0.962]	[0.255]
	[1990]	0.657	1.34	1.874	1.7	3.34	3.574	[1980]	-2.859	-3.027	-1.432	-2.757	-8.337
		[0.736]	[0.477]	[0.408]	[0.391]	[0.337]	[0.497]		[0.263]	[0.239]	[0.605]	[0.289]	[0.022]**
Prop. female-headed HH	[1980]	-2.613	-2.855	-1.56	-1.159	-5.7	-4.285	[1970]	1.662	1.918	4.211	2.978	2.692
		[0.015]**	[0.003]***	[0.140]	[0.314]	[0.000]***	[0.256]		[0.348]	[0.283]	[0.055]*	[0.211]	[0.133]
	[1990]	-0.01	-0.062	0.382	1.049	0.214	0.456	[1980]	-3.121	-3.08	-3.122	-1.46	-5.018
		[0.992]	[0.946]	[0.753]	[0.355]	[0.869]	[0.808]		[0.000]***	[0.000]***	[0.000]***	[0.099]*	[0.000]***
Prop. of HHs with public assistan	[1980]	7.117	6.861	5.561	7.039	7.591	5.829	[1970]	-0.416	0.248	-3.375	1.062	-2.115
		[0.000]***	[0.000]***	[0.004]***	[0.000]***	[0.023]**	[0.353]		[0.894]	[0.935]	[0.438]	[0.778]	[0.543]
	[1990]	1.484	1.825	3.889	2.051	2.736	-0.017	[1980]	5.866	5.693	7.093	7.268	7.617
		[0.199]	[0.123]	[0.027]**	[0.151]	[0.166]	[0.996]		[0.002]***	[0.003]***	[0.000]***	[0.000]***	[0.007]***
Unemployment Rate	[1980]	-3.537	-3.681	-4.502	-1.322	-0.315	15.025	[1970]	-0.129	1.076	-2.872	-6.211	0.289
		[0.231]	[0.220]	[0.315]	[0.689]	[0.908]	[0.000]***		[0.978]	[0.820]	[0.610]	[0.321]	[0.955]
	[1990]	-1.431	-0.888	-1.634	-2.676	3.132	3.962	[1980]	-2.352	-3.026	-0.767	-0.292	2.072
		[0.291]	[0.533]	[0.282]	[0.082]*	[0.195]	[0.372]		[0.388]	[0.303]	[0.785]	[0.913]	[0.490]
% of workers in city government	[1980]	-55.044	-44.041	80.061	-63.682	-62.182	-190.819	[1980]	-20.672	-16.928	-9.072	-2.729	-26
		[0.087]*	[0.068]*	[0.002]***	[0.051]*	[0.039]**	[0.034]**		[0.237]	[0.293]	[0.575]	[0.850]	[0.195]
	[1990]	42.793	34.769	-109.78	34.27	28.817	76.057						
		[0.047]**	[0.011]**	[0.000]***	[0.133]	[0.172]	[0.389]						
% of workers in manufacturing	[1980]	-34.106	-24.077	-19.647	22.921	-47.946	-92.395	[1980]	3.029	2.368	1.699	-9.367	-0.078
		[0.010]**	[0.021]**	[0.667]	[0.524]	[0.000]***	[0.000]***		[0.533]	[0.617]	[0.775]	[0.085]*	[0.990]
	[1990]	63.893	47.931	35.374	-48.203	80.004	142.721						
		[0.001]***	[0.002]***	[0.640]	[0.410]	[0.000]***	[0.000]***						
Avg. across tracts % black	[1980]	20.675	17.082	27.777	4.664	21.784	43.019	[1970]	6.186	4.891	-4.675	-21.697	-0.096
		[0.052]*	[0.084]*	[0.025]**	[0.674]	[0.108]	[0.021]**		[0.581]	[0.645]	[0.672]	[0.014]**	[0.995]
	[1990]	-10.717	-8.047	-4.863	12.45	-9.452	-20.394	[1980]	0.075	1.208	11.978	26.357	4.881
		[0.254]	[0.367]	[0.639]	[0.167]	[0.429]	[0.214]		[0.993]	[0.877]	[0.171]	[0.000]***	[0.645]
Total crime / population* 100	[1980]	21.31	20.382	56.392	67.035	51.116	53.809	[1980]	4.845	-2.868	-11.601	-29.952	-1.479
		[0.193]	[0.080]*	[0.114]	[0.006]***	[0.012]**	[0.033]**		[0.658]	[0.595]	[0.464]	[0.044]**	[0.925]
	[1990]	-25.329	-23.89	-65.141	-72.652	-53.908	-45.003						
		[0.009]***	[0.006]***	[0.001]***	[0.000]***	[0.000]***	[0.027]**						
Constant	[1980]	8.925	6.951	21.014	15.7	15.045	81.345	[1970]	-39.069	-40.327	-28.545	-25.012	-53.686
		[0.380]	[0.484]	[0.119]	[0.178]	[0.141]	[0.000]***		[0.026]**	[0.030]**	[0.084]*	[0.083]*	[0.014]**
Observations		1892	2067	1785	1769	1621	1635		1891	2054	1784	1768	1621

* significant at 10%; ** significant at 5%; *** significant at 1%. Clustered-robust pvalues in brackets

Table A4. Specification Checks

	Baseline [1]	All [2]	No NY or LA [3]	No CLE or LA [4]	Rejected [5]	Balance [6]
Log (Mean weight) for the untreated	0.014	0.056	-0.020	-0.154	2.291	1.441
CI	[-1.421 , 1.038]	[-3.330 , 1.005]	[-0.604 , 1.449]	[-0.383 , 0.976]	[-4.490 , 7.017]	[-4.686 , 5.131]
p-value	0.670	0.767	0.388	0.764	0.332	0.331
Pseudo R^2	0.476	0.474	0.584	0.569	0.721	0.799
Ratio of treated/untreated tracts s.t. pscore in [0.25-0.50]	0.076	0.076	0.065	0.068	0.055	0.042
Ratio of treated/untreated tracts s.t. pscore in [min(D=0)-0.25]	0.619	0.565	0.566	0.682	0.378	0.607
Ratio of treated/untreated tracts s.t. pscore in [0.50-0.75]	1.717	1.814	1.708	1.519	2.211	1.500
Ratio of treated/untreated tracts s.t. pscore in [0.75-max(D=0)]	3.917	3.385	7.444	5.500	16.300	33.667
% of treated tracts s.t. pscore > max{pscore|D=0}	7.393	7.393	6.218	16.854	4.297	2.214

Columns [1]-[5] show specification tests for the treatment equation estimation estimated via a logit; Column [6] show specification tests for a logit model in which the dependent variable is 1 if the obs. is a control tract in an EZ city and 0 if it is in a rejected/future tract in a non-EZ city.[1] *Baseline* sample refers to a sample of treated and rejected/future zones in EZ cities; [2] *All* refers the complete sample of accepted and rejected/future zones (i.e. no constraint on population or number of tracts); [3] *No NY or LA* presents results on a sample that excludes New York City and Los Angeles. [4] *No Clevaland or LA* presents results on a sample that excludes the SEZs Cleveland and Los Angeles. [5] *Rejected* uses as controls only census tracts outside treated cities. [6] Balance refers to the model estimated on a sample of control tracts.

Note: For an explanation of the mean weight test see Appendix IV. P-values and confidence intervals were obtained by block bootstrap.

Table A5: Impact of EZ Designation (Robustness Checks)

Difference-in-Differences Estimates

		No NY or LA [1]	No Cleveland or LA [2]	Rejected [3]	All [4]
Log(pop)	Coeff.	-0.056**	-0.026	-0.071	-0.009
	CI	[-0.207 , -0.024]	[-0.159 , 0.012]	[-0.217 , 0.084]	[-0.096 , 0.073]
	p-val	*0.022*	*0.083*	*0.231*	*0.765*
% In same house	Coeff.	0.002	-0.007	0.002	-0.013*
	CI	[-0.021 , 0.072]	[-0.041 , 0.055]	[-0.060 , 0.061]	[-0.050 , 0.001]
	p-val	*0.541*	*0.773*	*0.960*	*0.055*
% Black	Coeff.	-0.005	-0.024*	-0.037	-0.025*
	CI	[-0.038 , 0.031]	[-0.075 , -0.004]	[-0.124 , 0.012]	[-0.069 , 0.000]
	p-val	*0.994*	*0.030*	*0.103*	*0.046*
% College	Coeff.	0.027**	0.025***	0.024	0.024**
	CI	[0.011 , 0.062]	[0.017 , 0.060]	[-0.018 , 0.067]	[0.009 , 0.047]
	p-val	*0.012*	*0.000*	*0.204*	*0.012*
Employment Rate	Coeff.	0.046***	0.042***	0.036	0.043***
	CI	[0.028 , 0.103]	[0.038 , 0.100]	[-0.003 , 0.092]	[0.030 , 0.090]
	p-val	*0.000*	*0.000*	*0.061*	*0.000*
Unemployment Rate	Coeff.	-0.056***	-0.042***	-0.028	-0.042**
	CI	[-0.106 , -0.042]	[-0.078 , -0.035]	[-0.075 , 0.055]	[-0.078 , -0.019]
	p-val	*0.001*	*0.000*	*0.364*	*0.006*
Log(Mean Earnings)	Coeff.	0.002	0.027	0.017	0.018
	CI	[-0.088 , 0.113]	[-0.058 , 0.182]	[-0.134 , 0.140]	[-0.044 , 0.113]
	p-val	*0.871*	*0.400*	*0.792*	*0.514*
Poverty Rate	Coeff.	-0.055***	-0.064***	-0.054	-0.052***
	CI	[-0.125 , -0.036]	[-0.151 , -0.051]	[-0.121 , -0.016]	[-0.105 , -0.031]
	p-val	*0.000*	*0.000*	*0.014*	*0.000*
Log(House Value)	Coeff.	0.115	0.234	0.117	0.211**
	CI	[-0.288 , 0.287]	[-0.049 , 0.502]	[-0.015 , 0.686]	[0.051 , 0.471]
	p-val	*0.922*	*0.094*	*0.070*	*0.017*
Log(Rent)	Coeff.	0.064**	0.081***	0.069	0.064**
	CI	[0.027 , 0.154]	[0.064 , 0.195]	[-0.035 , 0.168]	[0.029 , 0.129]
	p-val	*0.006*	*0.001*	*0.134*	*0.003*
% Vacant Houses	Coeff.	0.015*	0.006	0.034	-0.002
	CI	[-0.001 , 0.065]	[-0.012 , 0.050]	[-0.020 , 0.085]	[-0.035 , 0.021]
	p-val	*0.059*	*0.248*	*0.163*	*0.579*
Number of Tracts		1742	1736	1480	2042
Number of Cities		80	80	82	104

Estimators: All columns show reweighted difference-in-difference estimates in which the change in outcomes over the period 1990-2000 among tracts in EZs is compared with the change in outcomes among tracts in rejected and future zones. **[1]** *No NY or LA* presents results on a sample that excludes New York City and Los Angeles. [2] *No Cleveland or LA* presents results on a sample that excludes the SEZs Cleveland and Los Angeles. [3] Rejected uses as controls only census tracts nominated for round I EZs that were rejected by HUD. [4] All presents results on the complete sample of accepted and rejected/future zones (i.e. no constraint on population or number of tracts). (See Section IV-B, V-B and Appendix V for details).

Inference: 95% C*onfidence intervals (CI)* and *p-values* were obtained via a pairwise block bootstrap that resampled zones in order to preserve the within zone dependence of the data. See Appendix IV for details. *Significance levels*. A multiple testing procedure described in the Appendix was used to control the False Discovery Rate (FDR) to prespecified levels. The procedure yields lower threshold p-values for fixed level tests than in the single equation case. Stars indicate that a hypothesis can be rejected while controlling the FDR to specified levels: * rejected at 10% FDR, ** rejected at 5% FDR and *** rejected at 1% FDR.