Targeted Business Incentives and Local Labor Markets

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This paper uses a regression discontinuity design to examine the effects of geographically targeted business incentives on local labor markets. Unlike elsewhere in the U.S., enterprise zone (EZ) designations in Texas are determined in part by a cutoff rule based on census block group poverty rates. Exploiting this discontinuity as a source of quasi-experimental variation in investment and hiring incentives across areas, I find that EZ designation has a positive effect on resident employment, increasing opportunities mainly in lower-paying industries. While business sitings spurred by the program are more geographically diffuse, EZ designation is associated with increases in home values.

I. Introduction

There remains substantial controversy over the efficacy of local economic development initiatives, and in particular place-based programs that create incentives for businesses to invest in or employ workers from certain regions. Much of the controversy in the U.S. centers on enterprise zones (EZs), which are aimed at encouraging economic development in blighted cities and neighborhoods. Companies that locate in or hire from EZs are generally eligible for certain regulatory and tax relief. EZ programs have proliferated in the U.S.; over 40 states have adopted such programs, and state and local governments now spend between $20 and $30 billion annually on economic development initiatives (Bartik 2002). With state zone programs increasingly prevalent, and with the federal government now administering many of its own place-based initiatives, it is all the more important to understand if and how these programs affect economic activity in targeted areas.

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While EZs are often hailed by politicians as vital economic development tools, the evidence on their actual effects is decidedly mixed. Early work on EZ programs, which frequently used propensity score matching techniques in an attempt to address omitted variable and selection problems, came to conflicting conclusions on the effects of zone designation on local labor markets (e.g., Bondonio and Engberg 2000, O'Keefe 2004). However, more recent work that better accounts for unobservable differences among areas that could affect assignment to EZs and influence local labor market outcomes has also produced mixed results. For instance, Billings (2009) and Neumark and Kolko (2010) use narrow buffers outside EZs as control groups for EZs. While Billings (2009) documents positive employment effects, Neumark and Kolko (2010) find no effects. However, though potentially mitigating bias owing to omitted variables, using areas geographically close to EZs as control groups could amplify bias due to spillovers.

Meanwhile, Busso et al. (2011), who compare outcomes in census tracts that are designated federal Empowerment Zones to outcomes in tracts considered for but initially denied Empowerment Zone status, find that zone designation increases employment.[1] Using a double-difference estimation approach, Ham et al. (2011) also find that state and federal zone programs have positive effects on neighborhood conditions and employment. Similarly, using a two-stage strategy to evaluate French EZs, Gobillon et al. (2011) find that designation reduces the duration of unemployment spells for workers in zones. In each of these studies, though, nonrandom selection of cities or neighborhoods into or out of treatment represents a major concern.

This paper adopts a new approach to evaluating the impact of EZs by taking advantage of the unique structure of the Texas EZ Program. In contrast to most other states, where localities often must apply for EZ designations, Texas designates areas as EZs on a non-competitive basis. In particular, any census block group that meets a minimum poverty criterion is automatically an EZ. This rule-based assignment of EZ status facilitates a regression discontinuity (RD) design in which I exploit the formula determining block group EZ designation as a source of quasi-experimental variation in investment and hiring incentives across geographic areas. My identification strategy not only does not depend only on nearby areas as counterfactuals, but also circumvents omitted variable and selection problems that have plagued many past evaluations of place-based programs.

[1] Ham et al. (2011) discuss the similarities and differences between federal and state zone programs.

The Texas EZ Program creates explicit incentives to hire from, although not necessarily create jobs in, areas designated as EZs. Consistent with the program's incentive structure, I find using rich administrative data derived from unemployment insurance records that EZ designation increases resident employment in block groups with poverty rates near 20% by 1-2% per year. The employment effects are concentrated in jobs paying less than $40,000 annually and are largely in the construction, manufacturing, retail trade, and wholesale trade industries. Estimates based on resident survey data corroborate these results and also point to measurable impacts of zone designation on local property values. Further, while I cannot entirely rule out spillovers, EZs do not appear to be merely shifting jobs from nearby areas. Not surprisingly given the structure of the program, which places fewer constraints on where firms locate than from which areas they hire, the effects on job creation in poor areas are less precisely estimated. The results are consistent with a more diffuse effect of the program on the locations of the new jobs in which residents of targeted regions are employed.

Focusing on one state's EZ program allows me to isolate the outcomes most in line with its stated goals and to leverage particular institutional details to help identify the impacts on local labor markets. This would be more difficult with a broader sample given the variation across state and federal zone programs in the types of areas targeted, the types of incentives offered, and the generosity of those incentives. In part because of such variation, the results in this paper do not necessarily extend to all zone programs. Moreover, the RD design only permits me to identify the local average treatment effect of the program; that is, the results are only valid for a narrow group of census block groups with poverty rates near 20%. Designating much richer or much poorer neighborhoods as EZs would not necessarily have the same impacts I find for the specific group of communities I consider in this analysis. Nonetheless, my results shed new light on the efficacy of place-based programs aimed at revitalizing blighted regions.

The paper is organized as follows. The next section provides an overview of Texas' EZ Program. Section III discusses my strategy for identifying the impact of EZ designation in Texas, which relies on a discontinuity generated by the formula the state uses to determine EZ status for census block groups. Section IV describes the data I use in the analysis. Section V presents the main results on the effects of EZs on resident and workplace employment as well as a series of robustness tests. I delve into the heterogeneous effects of the program across different types of jobs in Section VI before considering possible spillovers in Section VII. Section VIII examines the impacts of EZ designation on other neighborhood conditions. Section IX concludes.

II. The Texas Enterprise Zone Program

A. Program Structure

First introduced in late 1980s, the Texas EZ Program is aimed at providing a means by which the state government and municipalities can reduce regulatory hurdles and provide certain incentives to encourage private investment and hiring in blighted areas. Under the current program, which was created in 2003, participating businesses (known as Enterprise Projects) receive a combination of state and local benefits for up to five years. These benefits take several forms. First, a single Enterprise Project can apply for state sales and use tax refunds of up to $1.25 million over five years on qualified expenditures on machinery and equipment, building materials, electricity and natural gas, and construction labor; the precise refund is tied to the amount of the capital investment and the number of jobs created or retained at the site.[2] Local communities must also offer incentives to designated projects; these incentives may include tax abatement, utility rate reductions, public service expansion (e.g., road improvements), tax increment financing, expedited permitting, or other incentives.[3]

In contrast to program rules in many other states, businesses in Texas need not necessarily locate in an EZ to receive benefits, nor does locating in an EZ guarantee that a business will receive benefits.[4] To be eligible for benefits, a business located in an EZ must ensure that 25% of its new employees will meet economically disadvantaged or EZ residence requirements.[5] A business located outside an EZ is eligible to receive benefits if it commits that 35% or more of its new employees will meet economically disadvantaged or EZ residence requirements. Therefore,

[2] For the complete refund schedule, see Appendix Table A1.

[3] For example, Lubbock offers utility rate reductions and property tax abatements. Baytown offers property tax abatements, industrial district agreements, waivers of permit fees, waivers of water and sewer tapping fees, and one-stop permitting. Houston offers property tax abatements and a small business revolving loan fund.

[4] For example, in California, which Elvery (2009) and Neumark and Kolko (2010) consider in their analyses, businesses are eligible for benefits only if they are located in an EZ.

[5] An economically disadvantaged person is defined as one who (a) was unemployed for at least 3 months before obtaining employment with a qualified business, (b) receives public assistance benefits, (c) has a physical or mental disability, (d) is homeless, (e) is a foster child, (f) is on parole or was recently released from a state correctional facility, or (g) is an individual whose family income meets the low- or moderate-income limits under the Section 8 program. To the extent that verifying economically disadvantaged status is more difficult than verifying residency, firms may have a preference for the latter. In any case, if Enterprise Projects hire economically disadvantaged persons as opposed to EZ residents, it will attenuate the effects of the state's program on EZ resident employment.

more so than the number of jobs that exist in distressed areas, the program might be expected to increase the number of jobs held by the residents of those areas. Due in part to data limitations, past research has typically only examined one or the other, either using employer-side data on the location of jobs (e.g., Bondonio and Engberg 2000, O'Keefe 2004) or resident-side information on employment or unemployment (e.g., Elvery 2009, Gobillon et al. 2011).[6]

Meanwhile, locating in or hiring from an EZ does not ensure a business will receive benefits. The local jurisdiction must nominate businesses for Enterprise Project designations. Nominations are reviewed by the State Office of Economic Development, which makes a final determination about awards. Legislation limits the number of designations to 105 per biennium.[7]

The costs of the program are difficult to calculate, in part because the burden is shared by state and local governments and in part because different localities supplement the state's incentives with different (and often project-specific) benefits. The Texas Comptroller of Public Accounts (CPA) estimates that the program cost the state alone $33.6 million during fiscal years 2008 and 2009; during that period, businesses receiving refunds pledged $5.7 billion in capital investment and over 13,000 new jobs (Texas CPA 2010). However, this does not immediately imply that the program is cost effective; not only do the CPA's figures not take into account the cost of benefits offered by localities, but much of that investment and job creation may have occurred in the absence of the program. The results of this paper speak to the extent of crowd-out of unsubsidized private-sector investment associated with Texas' EZ Program.[8]

B. EZ Designations

Since the program was amended in 2003, EZs in Texas have been designated according to a non-competitive, rule-based assignment scheme.[9] In particular, any census block group (as defined by the most recent decennial census) in which 20% or more of the residents have income

[6] Exceptions include Ham et al. (2011) and Busso et al. (2011).

[7] While they have no discretion over which areas are designated EZs, local officials have discretion over which projects they nominate and state officials can turn down applications. The state does not make information on failed applications available, but there is reason to believe that many are turned down because they were too late to apply during the biennium (i.e., all 105 designations had already been awarded).

[8] Notably, even if the program merely reshuffles employment without creating any new jobs, to the extent that unmeasured positive externalities associated with investment in poor communities are relatively large or that the reshuffling mitigates some current or past inequities across certain subpopulations, the program could be justified.

[9] Prior to 2003, EZs in Texas were required to be areas "of pervasive poverty, unemployment, and economic distress" and to be between one and ten square miles in size (see Texas State Government Code, Title 10, Subtitle G, Chapter 2303). Similar to most other states' current programs, areas also had to be nominated by an ordinance or order adopted by a locality.

below the federal poverty level is automatically designated an EZ. Further, any area designated by the federal government as an Empowerment Zone, Renewal Community, or Enterprise Community is also designated a state EZ. Another amendment in 2005 stipulated that any county in the state in which the poverty rate exceeds 15.4%, at least 25.4% of the adult population does not hold a high school diploma (or equivalent), and the unemployment rate has remained above 4.9% for five consecutive years is automatically designated an EZ.

The shaded areas in Figure 1 represent the Texas EZs as of 2010.[10] Of the 254 counties in the state, 34 were designated EZs based on county poverty rates, adult education rates, and unemployment rates. Meanwhile, 16 tracts covering parts of ten counties were designated state EZs by virtue of being federal Empowerment Zones or Renewal Communities (EZ/RCs).[11] Altogether, those 34 counties and 16 tracts encompass 1,465 (or 10.1%) of the 14,463 block groups in Texas. Of the remaining 12,998 block groups in the state, 3,598 (or 27.7%) of the block groups are EZs as a result of having a poverty rate of at least 20%. For the purposes of identification, I focus on this latter group of block groups in the main analysis, although in robustness tests I take into account the overlap in programs. Notably, while most distressed counties and federal EZ/RCs are concentrated along the border with Mexico, EZs designated on the basis of block group poverty rates are scattered throughout the state.[12]

In addition to showing EZs in Texas, Figure 1 plots the locations of the roughly 300 Enterprise Projects that received awards between September 2003 and September 2010.[13] Figure 2 zooms in on select urban areas in the state, including Dallas-Fort Worth and Houston.[14] These figures make clear not only how physically small EZs in Texas can be, but also how Enterprise Projects tend to be near, but not necessarily in, EZs. Indeed, as Figure 3 shows, only 45% of the Enterprise Projects that could be geocoded were located in an area designated as an EZ between 2003 and 2010; the remainder were located outside EZs but received awards by pledging to hire

[10] 2000 Decennial Census data were used to determine the eligibility of counties and block groups for EZ status from the start of the program through the present. The maps also reflect 2000 Census geographic boundaries.

[11] The tax benefits of federal Enterprise Communities expired in 2004.

[12] One challenge that arises in determining the effects of EZs in many states is that EZ boundaries do not conform to standard census or postal boundaries for which one can obtain accurate measures of outcomes of interest (Elvery 2009). If some areas are misclassified as EZs and others are misclassified as controls as a result of the aggregation or manipulation of data to construct consistent geographic regions, it will tend to bias one toward finding no effect of EZs. After 2003 in Texas, this does not pose a problem, as all EZs in the state conform to FIPS codes.

[13] 297 of the 320 business that received Enterprise Project designations between September 2003 and September 2010 could be precisely geocoded and appear on the map.

[14] Neither of these cities includes any distressed counties or federal EZ/RCs.

at least 35% of their workforce from a disadvantaged group or from an EZ. Likely as a result of this requirement, 85% of Enterprise Projects were located within at least five kilometers of an EZ. Over 99% of Enterprise Projects were located within 20 kilometers of an EZ.

III. Empirical Strategy

A. Model

This paper aims to identify the effects of EZ designation on local labor markets. I conduct the analysis at the level of census block group, which are geographies larger than a city block but smaller than a census tract. In Texas, block groups had a median population of 1,180 and a median area of 1.2 square kilometers in 2000.

Following Chay and Greenstone (2005), Baum-Snow and Marion (2009), and Freedman (2012), I am interested in estimating β_1 in the following equation:

$$\Delta y_i = \beta_0 + \beta_1 EZ_i + \mathbf{X}_i \Omega + \varepsilon_i \qquad (1)$$

In equation (1), y_i is the outcome of interest for block group i, EZ_i is a dummy for whether i is an enterprise zone, and $\mathbf{X}_i$ is a set of baseline (year 2000) characteristics of i. Simply estimating equation (1) on all block groups will likely yield inconsistent estimates of β_1 given that unobserved and unmeasured local characteristics might be correlated with EZ status and independently affect the outcome of interest. If localities must apply for EZ designation (as they must do in many states), those localities that apply are likely to be different in unmeasurable ways than localities that do not apply. This selection problem has hampered past research on EZs.

Since September 2003, EZs have been designated in Texas according to a non-competitive, rule-based process in which local economic conditions determine EZ status. While this overcomes the selection problem associated with differences across areas related to the propensity to apply for EZ designations, there are still potential omitted variables at the block group level that could bias estimates of the effects of EZ status on local labor market outcomes. The decision of a business to invest in or hire from a particular place is likely to be influenced by local characteristics as well as expectations about the future prospects of the area, each of which may not be fully captured in $\mathbf{X}_i$ and might also affect Δy_i. To the extent that we cannot control for such factors, the error term ε_i will be correlated with EZ_i, which in turn will bias estimates of β_1.

To address this problem, I adopt an RD design that takes advantage of the rule determining EZ status. The approach in this paper builds on recent work exploiting the formula structure of various placed-based programs on neighborhoods (Baum-Snow and Marion 2009, Freedman and Owens 2011). In an RD framework, whether an observed covariate (i.e., the forcing variable) lies on either side of a fixed cutoff value at least partly determines treatment.[15] In Texas, outside of distressed counties and federal EZ/RCs, a block group's poverty rate determines EZ status. Specifically, block groups with poverty rates of 20% or greater are EZs, while block groups with poverty rates less than 20% are not. Thus, for the subset of block groups that I consider in the main analysis, the discontinuity is "sharp" in the sense that no block groups with poverty rates below 20% qualify as EZs and all block groups with poverty rates above 20% qualify as EZs.[16]

The critical assumption behind my econometric approach is that block groups in a sufficiently narrow window around the 20% poverty rate threshold are similar along observable as well as unobservable dimensions. More specifically, covariates besides EZ status that might affect the outcomes of interest cannot change discontinuously at the poverty rate threshold for EZ designation. If this is true, and if there is no sorting around the threshold, then for a population of block groups near the 20% poverty rate threshold, EZ designation is assigned essentially at random. In the next section and in Section IV.C, I argue and provide evidence that the preconditions for an RD design hold, and that in turn, we can interpret any discontinuity in the conditional distribution of outcomes as a causal effect of EZ designation.

Following much of the literature on EZ programs, I examine the reduced form relationship between zone status and local labor market outcomes.[17] The equation of interest is therefore

$$\Delta y_i = \gamma_0 + \gamma_1 EZ_i + f(p_i) + \mathbf{X}_i \Psi + u_i , \tag{2}$$

[15] For a detailed discussion of RD designs, see Lee and Lemieux (2010).

[16] In principle, one could also exploit the cutoffs for designation as a distressed county. However, there are only 254 counties in Texas, and many fewer close to the poverty, education, or unemployment thresholds for distressed county designation. In Section V.D, I include distressed counties and federal EZ/RCs as a robustness check.

[17] If one considers an area treated if an Enterprise Project hires from or locates in that area, the coefficient on *EZ* reflects an intent to treat. While I observe location for most Enterprise Projects, I do not have information on where Enterprise Project employees reside. Also, the intent to treat is arguably the outcome of interest from a policy perspective. Firms may locate in or near EZs in hopes of benefitting from the program even if they ultimately do not. People may also move to EZs in hopes of benefitting from the incentives businesses have to hire from those areas. Thus, the overall impact on neighborhoods of EZ designation, over which officials also have more control than where Enterprise Projects invest, is of substantial policy import.

where p_i is the poverty rate of block group i and f is a control function. I use a variety of specifications for f, including various polynomial specifications (quadratic, cubic, and quartic) in which the polynomial coefficients are allowed to differ above and below the cutoff.[18] As I show in the results, the estimates vary only slightly with different specifications of the control function. Moreover, as I show in the results and as would be expected if the identification strategy outlined here is valid, including controls in **X** does not affect the results substantively.

B. Identification

The main identifying assumption underlying the RD design employed in this paper is that unobservable determinants of Δy_i do not differ among census block groups within a narrow window around the poverty rate cutoff. One possible threat to this assumption is that sorting occurred among block groups around the threshold. While self-selection into treatment is a problem in the analysis of state EZs in most of the over 40 states that currently have zone programs, it is highly unlikely that such sorting occurred in the case of the Texas EZ Program. Designations for block groups outside distressed counties and federal EZ/RCs are determined strictly according to the poverty rate published in the most recent decennial census. Even if communities anticipated the formula structure of the Texas EZ Program, it is unlikely that they would be able to manipulate the census results. Indeed, the sampling variability associated with the one-in-six sample drawn for the long-form 2000 Decennial Census, as well as imputation and confidentiality protection procedures conducted by the Census, add noise to the data that ensures local officials could not have perfect control over the value of the forcing variable for any given block group. As further checks on the assumption that no sorting occurred around the threshold that might invalidate the proposed RD design, in Section IV.C, I provide descriptive evidence and formally test that there is no discontinuity in the distribution of the forcing variable at the cutoff and show that observable baseline covariates evolve smoothly through the threshold.

Importantly, the RD estimates represent a weighted average of the effects of EZ designation for the subpopulation of neighborhoods near the cutoff, where the weights are proportional to the ex-ante likelihood of having a poverty rate near the threshold. The effects of EZ designation are

[18] Specifically, I use control functions that take the following general form:

$$f(p_i) = \sum_{k=1}^{d} [\varphi_{1k}(p_i - 0.2)^k + \varphi_{2k} EZ_i (p_i - 0.2)^k]$$

where d is the order of the polynomial.

unlikely to be the same in very high or very low income areas with poverty rates far from the cutoff. In other words, equation (2) identifies a local average treatment effect that may not be equal to the average treatment effect for the entire population of block groups. Nonetheless, estimates of the local average treatment effect are of substantial policy import. While an important dimension over which state and federal zone programs differ is the criteria used to determine whether areas are sufficiently "poor" to warrant designation, nearly all programs focus on low-income communities, and often communities with poverty rates not far from 20%.[19]

IV. Data

A. Sources

The data used in this analysis come from several sources. The first is the Economic Development and Tourism division of the Texas Office of the Governor, which publishes on its website a list of EZs in the state that qualify on the block group poverty rate criterion or as distressed counties.[20] I combined these data with information on the location of federal EZ/RCs, which is published on the Department of Housing and Urban Development's website. This left me with a dataset encompassing all EZs in Texas.

Baseline resident characteristics of block groups were extracted from the 2000 Decennial Census. These data include a host of demographic characteristics, including information on total population, racial and ethnic composition, gender composition, the age distribution, the use of languages other than English (mainly Spanish in Texas), shares with U.S. citizenship, educational attainment levels, household and family income, unemployment rates, labor force participation rates, household mobility, and poverty rates. The data also include a number of housing-related variables, including total housing units, share vacant, share occupied, share owner occupied, share rented, median age of units, and median house value.

In order to examine the effects of the EZ Program on local labor markets over the 2000s, I obtained administrative data on resident and workplace employment by block group and year between 2002 and 2009 from the Longitudinal Employer-Household Dynamics (LEHD)

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[19] The average poverty rate in tracts located in state EZs across the country in 2000 was 18% (Ham et al. 2011).

[20] The Economic Development and Tourism division of the Texas Office of the Governor also provided me with a list of Enterprise Projects, or businesses that have received incentives through the state's EZ program.

Program at the U.S. Census Bureau.[21] These data, which are derived from state unemployment insurance records, capture approximately 98% of private sector employment in the U.S. Resident employment in a block group represents the number of residents in that block group who hold jobs (regardless of where those jobs are located). Workplace employment in a block group represents all jobs that are located in that block group (regardless of where the workers who hold those jobs reside). For both resident and workplace employment, these data contain job counts as well as the number of jobs in each of three pay categories (jobs paying less than $15,000 annually, jobs paying between $15,000 and $39,999 annually, and jobs paying $40,000 or more annually). Additionally, the data contain the number of resident and workplace jobs in each major industry (at the two-digit NAICS level). As outcome variables, I use the average annual change in log employment (either resident or workplace) in a block group.[22] State sales and use tax refunds flow to Enterprise Projects on an annual basis. Also, average annual changes are more easily interpretable than are changes over seven years (between 2002 and 2009).[23]

Due to their comprehensiveness, fine geographic resolution, and information on both resident and workplace employment, the LEHD data are well suited to study how EZ designations affect local labor markets. However, they have several drawbacks. First, they do not contain information on hours worked, so calculating hourly wages is not possible. Second, the earnings thresholds are not adjusted for inflation each year, and hence there is a gradually declining fraction of jobs in the low-earnings bin and a gradually increasing fraction of jobs in the high-earnings bin. In the empirical analysis, I test whether EZs experienced differentially large or small changes in each type of job. Finally, for confidentiality reasons, LEHD infuses noise into the resident employment data by synthesizing it from the true data in a method similar to data

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[21] These data are derived from the LEHD Program's OnTheMap program, which provides annual cross-sectional information on jobs at detailed geographies. I include in the sample all jobs, regardless of whether they are primary or secondary jobs. Restricting the sample to primary jobs, or the job for each person that contributes the most to his/her earnings each year, has little effect on the results.

[22] I add one to both resident and workplace employment to avoid taking the log of zero. As less than 3% of observations have zero resident employment, workplace employment, or both, instead excluding observations with zero employment has little effect on the main results.

[23] For both resident and workplace employment, using the difference in log employment in 2009 and log employment in 2002 yields effect sizes almost exactly seven times the size (and with the same statistical significance) of those found using the average annual difference in log employment between those years. This implies that no information is lost by averaging across years.

swapping (Andersson et al. 2008).[24] This will tend to add measurement error to the dependent variables that should only be problematic to the extent that it is correlated with EZ designation.

Both as a robustness check on the resident employment results based on the LEHD data and in order to examine the impacts of Texas' EZ program on other neighborhood conditions, I take advantage of recently released small-area estimates from the 2005-2009 American Community Survey (ACS). These estimates reflect survey information collected between January 1, 2005 and December 31, 2009, and hence the data capture average neighborhood characteristics over the entire five-year period.[25] The neighborhood outcomes of interest, including resident employment, population, poverty rates, median household income, median house values, and vacancy rates, are measured as changes between the 2000 Decennial Census and the 2005-2009 ACS. Notably, to the extent that the impact of EZ designation took time to affect neighborhoods, the estimated effects using the ACS will be attenuated.

B. Sample

In order to cleanly identify the effect of EZ designation on local labor market outcomes, I limit attention to the subset of Texas block groups that were not designated a distressed county or a federal EZ/RC at any point during the sample period. Dropping these block groups left me with 11,943 block groups. Of these, 11,692 had valid information for all the socio-economic and housing variables in the 2000 Decennial Census.

For the RD assumptions to be valid, a sufficiently narrow window around the relevant threshold must be used to ensure that observations on either side of the cutoff are similar along both observable and unobservable dimensions. While the latter cannot be verified conclusively, the former can be. In the main analysis, I limit attention to census block groups with poverty rates (based on 2000 Decennial data) of between 18% and 22% (inclusive). The final sample that has all requisite variables available and that are outside distressed counties or federal EZ/RCs is

[24] Prior to 2006, LEHD suppressed information for blocks where the number of workers residing in the block was fewer than five or where the number of different blocks where resident workers were employed was fewer than three. In rural areas in particular, this tended to bias downward aggregate estimates of resident employment. This will affect my estimates only to the extent that the error generated by cell suppression is correlated with EZ designation. Estimates based on only densely populated areas are qualitatively and quantitatively similar to the main results.

[25] Some areas in Texas used preliminary 2010 Decennial Census boundaries in the 2005-2009 ACS. Using crosswalks provided by the Census Bureau in combination with commercial GIS software, I normalized all geography to 2000 boundaries.

995 block groups. As robustness checks, I consider alternative windows around the 20% poverty rate threshold as well as placebo cutoffs.

C. Descriptive Statistics

In Figure 4, I present a histogram showing the number of tracts in each half percentage point bin of the poverty rate in a twenty percentage point window around the 20% cutoff that determines EZ status for block groups in Texas. While fewer block groups have poverty rates close to 30% than poverty rates close to 10%, there is no indication of a discontinuity in the distribution of the forcing variable at the cutoff. A McCrary (2008) test confirms there is no statistically significant jump in the density function at 20% (see Appendix Table A2). This is consistent with a lack of any manipulation in the value of the poverty rate that might undermine the RD design.[26]

Table 1 presents descriptive statistics for census block groups in the main sample. In Panel A, average values for the baseline (year 2000) demographic and housing variables are presented in one percentage point bins of the forcing variable on either side of the 20% threshold. Panel B shows 2002 resident and workplace employment information from the LEHD data in bins on either side of the cutoff, including figures broken out by earnings and broad industry.

There is little evidence that the covariates are anything but smooth through the cutoff.[27] In fact, none of the differences in baseline demographic and housing characteristics among block groups in the [0.19, 0.20) bin and the [0.20, 0,21) bin are statistically significant at the 5% level.[28] Consistent with the density test of the forcing variable itself, tests of the null hypothesis of continuity of the density of each of the covariates, as well as that of the initial (i.e., year 2002) values of the outcome variables, against the alternative of a jump in the density suggest that there

[26] The lack of any discontinuity in the forcing variable or the other baseline characteristics also indicates that there were no pre-existing place-based programs that used the same cutoff whose impacts might obscure the effect of EZ designation on the outcomes of interest.

[27] Building 2000-boundary block groups from 1990 blocks, I also checked for differential trends between 1990 and 2000 on either side of the threshold in population, share black, share Hispanic, share under age 30, share age 65 or over, number of housing units, share vacant, share owner occupied, and median house values. There are no statistically significant differences across the threshold in these trends, and the main regression results are robust to their inclusion as additional controls. Results are available upon request.

[28] Differences across the threshold in only two covariates (the share of the population that is male and the share with some college) are significant at the 10% level.

is no sorting around the threshold.[29] Graphical evidence of the lack of discontinuities in a select set of baseline covariates appears in Appendix Figure A1.

V. Results

A. Main Results

In this section, I present results for my preferred sample, which consists of all block groups in Texas with poverty rates within a four percentage point window around the 20% threshold determining EZ status and excluding areas in distressed counties or federal EZ/RCs. In subsequent sections, I perform a variety of robustness tests to ensure that the results are not sensitive to the particular sample and specifications chosen.

First, I show graphical evidence of differences in resident and workplace employment growth across EZ and non-EZ block groups near the 20% threshold. Panel A of Figure 5 shows the average annual change in log resident employment within 0.1 percentage point bins in a four percentage point window around the 20% poverty rate threshold, while Panel B shows the average annual change in log workplace employment. In each case, there appears to be a discontinuity at the 20% cutoff, with higher employment growth above the threshold (in block groups that are EZs) than below the threshold (in block groups that are not EZs).

Corresponding regression estimates appear in Panels A and B of Table 2. Each entry in the table is from a separate regression, where for different control functions (quadratic, cubic, and quartic), I present estimates based on regressions that include the control function alone (columns (1), (4), and (7)); the control function plus the demographic and housing controls listed in Table 1 (columns (2), (5), and (8)); and the control function, demographic and housing controls, and county dummies (columns (3), (6), and (9)). The standard errors are adjusted for heteroskedasticity and clusters at the county level, which may overstate the standard errors somewhat by allowing for a fairly general covariance structure in which observations are assumed to be independent across counties but not necessarily within counties.[30]

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[29] A chi-squared test based on a seemingly unrelated regression in which each equation is for a separate baseline covariate cannot reject the null that all the discontinuity gaps are jointly equal to zero. Graphical evidence of the lack of any discontinuities in baseline covariates at the threshold is also available upon request.

[30] Of the 254 counties in Texas, 137 are represented in the regressions that include block groups with poverty rates between 0.18 and 0.22. Clustering at the level of census tract, of which there are 822 in the main sample, has little effect on the standard errors.

Panel A of Table 2 indicates that, regardless of the form chosen for the control function and which controls are included in the regression, the average annual difference in log resident employment is consistently 1-2% greater in areas just qualifying as EZs relative to similar areas that barely fail to qualify as EZs. The coefficient estimates are all statistically significant at least at the 10% level, and most are significant at the 5% level. Additional controls not only have little impact on the estimates, but also have little effect on their precision. The estimates imply an increase on average of 5-6 resident jobs per designated block group per year, or about 35-42 resident jobs over the time horizon under consideration.[31]

Turning to Panel B of Table 2, the estimates for the impact of EZ designation on workplace employment are slightly larger at 3-5%. However, the estimates are much less precise, likely a reflection of the fact that the Texas EZ Program, while making locating in EZs more desirable by lowering the share of employees that must be disadvantaged or from an EZ, does not require that businesses locate in EZs to receive EZ benefits. Hence, as Figures 1-3 hint, while some Enterprise Projects contribute directly to job creation in EZs, many contribute to job creation elsewhere (including in nearby non-EZ block groups). As a result, job creation spurred by the program is more geographically diffuse than are increases in resident jobholding, and the results are not definitive as to whether EZ status is associated with higher workplace employment.

B. Placebo Estimates

To verify that any observed jumps are in fact being driven by the EZ designation, I conduct a placebo exercise using a series of alternative thresholds. The results of this robustness check appear in Figure 6, in which I plot discontinuity estimates from separate regressions run using four percentage point windows around each percentage point of the poverty rate between 10% and 30%. I present results from regressions including a cubic polynomial in the poverty rate around the false threshold (with coefficients allowed to vary above and below the placebo cutoff), demographic and housing controls, and county dummies, although the graphs look nearly identical excluding controls or county dummies. Again, the results for resident employment growth appear in Panel A, while results for workplace employment growth appear in Panel B.

As expected, there is not a significant increase in resident employment growth at any poverty rate except 20%. This suggests that it is indeed EZ designation that is driving the resident

[31] By comparison, Ham et al. (2011) find using data for several states that EZs increased tract-level resident employment by about 69 on average between 1990 and 2000.

employment effects. However, while positive at the 20% threshold, the effect on workplace employment is less pronounced at the cutoff, and in fact the placebo discontinuity estimate at other points (e.g., 14% and 22%) are nearly as large as at the true cutoff. This again is likely a reflection of the fact that the program, which does not require locating in any particular area as a precondition for receiving state and local benefits, has a somewhat diffuse effect on the location of new jobs even as it has more concentrated impacts on resident employment.

C. Alternative Windows

I conduct the main analysis for a number of different bandwidths to determine whether the particular window around the threshold chosen affects the main results. The results appear in Table 3, where I present regression estimates using a cubic polynomial control function for the full sample (poverty rates of 0-1), a 20 percentage point window around the 20% cutoff (0.1-0.3), a ten percentage point window (0.15-0.25), an eight percentage point window (0.16-0.24), a six percentage point window (0.17-0.23), and a two percentage point window (0.19-0.21).

As is evident in the first three columns in Table 3, for windows of up to at least ten percentage points, the estimates for resident employment are very stable and are generally significant at the 5% level. The results suggest that, as long as a sufficiently narrow window is used such that unobserved characteristics do not differ substantially across treated and untreated block groups, EZ designation increases resident employment by 1-2% per year.

However, upon expanding the window beyond ten percentage points, the differences in resident employment outcomes across treated and control groups largely vanish. Widening the window introduces more census block groups to the sample that are farther from the cutoff and more likely to be different along both observed and unobserved dimensions. Using relatively affluent census block groups far from the 20% threshold, whose observable and unobservable characteristics could ensure robust resident job growth regardless of EZ designation, as controls for EZs attenuates the estimated impact of EZ designation on resident employment growth.

Turning to workplace employment (columns (4)-(6) in Table 3), the estimated effects of EZ designation for block groups in narrow windows around the cutoff (eight percentage points or fewer) are not significant at conventional levels, consistent with the findings in previous sections. In contrast to the results for resident employment growth, the coefficient estimates on EZ status for workplace employment growth gain statistical significance and tend not to diminish in magnitude with wider windows around the threshold. This is partly attributable to larger sample sizes. However, expanding the window may also introduce omitted variables that bias the estimated effects of EZ designation on workplace employment growth upward. Since Enterprise Projects may locate inside or outside EZs, they are likely to locate only in EZs that exhibit signs of economic improvement. To the extent that we cannot control for such differences in economic conditions and prospects among very dissimilar census block groups farther from the threshold, the estimated effects of EZ status on workplace employment growth could be biased upward in the expanded sample. In fact, the positive workplace employment effects for the wider windows are driven entirely by several very high poverty block groups that received influxes of investment by Enterprise Projects; changes in the control group were less important.[32]

D. Distressed Counties and Federal EZ/RCs

The previous results exclude areas designated as EZs on the basis of the distressed county criteria or because they were federal EZ/RCs. In this section, I test the robustness of the prior results to including all EZs in the state. While including distressed counties and federal EZ/RCs might be expected to reduce the precision of the estimated effects of EZ designation all else being equal, it increases the sample size and may alleviate concerns that excluding these other EZs introduces a selection problem.

I present the results for the full sample of block groups with poverty rates between 18% and 22% in Table 4. In general, the noise introduced by augmenting the sample with block groups designated EZs despite having poverty rates below 20% offsets the effects of having a larger sample size on the precision of the estimates. However, the resident employment estimates are all of nearly the same magnitude as for the main sample. Meanwhile, the effects on workplace employment are slightly larger and more significant if one includes distressed counties and federal EZ/RCs. While in part due to the larger sample sizes, this could also be driven by the additional federal incentives that come with locating in a federal EZ/RC. Businesses must be sited in an Empowerment Zone or Renewal Community in order to receive Empowerment Zone or Renewal Community tax credits, which amount to up to $3,000 and $1,500 per employee, respectively. A positive effect of federal EZ/RCs on local employment is consistent with Busso et al. (2011) and Ham et al. (2011). Overall, though, the fact that including distressed counties

[32] Limiting the sample to block groups with poverty rates between 0.18 and 1, for example, yields results similar to those from using block groups with poverty rates between 0 and 1 (i.e., the full sample).

and federal EZ/RCs in the sample does not substantially change the results suggests that exploiting the sharp discontinuity among areas that qualify on the basis of block group poverty rates alone does not introduce serious sample selection.[33]

VI. Employment Composition

In this section, I consider the effects of EZ designation on the composition of resident and workplace employment. I start by considering the effects of EZ designation on growth in employment in jobs in different earnings categories. The LEHD data break jobs out into those paying less than $15,000 per year ("low-wage"), between $15,000 and $39,999 per year ("mid-wage"), and $40,000 or more per year ("high-wage"). In Table 5, I present estimates of resident and workplace employment growth broken out by earnings level. As in Tables 2 and 4, I use a window of four percentage points around the threshold and include estimates from regressions with the control function alone (columns (1) and (4)); the control function plus the demographic and housing controls listed in Table 1 (columns (2) and (5)); and the control function, demographic and housing controls, and county dummies (columns (3) and (6)). My preferred specification, which includes the full set of demographic and housing controls as well as county dummies, implies that EZ designation increases both low-wage and mid-wage resident employment significantly, with the largest effect for mid-wage jobs (2.3%). The workplace employment estimates are all insignificant, although the largest effect is for low-wage jobs.

In Table 6, I break out the results by two-digit NAICS industry.[34] I find a statistically significant positive effect of EZ designation on resident employment in at least one specification for construction, manufacturing, wholesale trade, retail trade, transportation and warehousing, and health care industries. There is no discernible effect on employment in higher-paying and more skill-intensive industries such as information, finance and insurance, and professional services. Meanwhile, the only significant effects I find for workplace employment are in utilities, retail trade, real estate, and health care. Although one should not put too much weight on the few marginally significant and less stable estimates in the workplace regressions, job gains in these industries may be in response to improved economic conditions of EZ residents.

[33] These results are also consistent with Ham et al. (2011), who find that taking into account the overlap between state and federal zone programs has little qualitative effect on the estimated impact of these programs in general.

[34] I exclude agriculture, management, and public administration due to incomplete coverage and/or small cell sizes.

The resident employment results by industry are consistent with the composition of businesses that received awards since 2003. An attempt to classify Enterprise Projects by major industry revealed that over half were in retail trade (particularly big box stores) or manufacturing (e.g., food processing and packaging, aluminum and steel production, chemicals, medical devices, and consumer goods), with many of the remainder in wholesale trade, transportation, and warehousing industries (e.g., distribution centers and storage facilities).

VII. Spillovers

Any benefits associated with EZ status could spill over into nearby areas. It is also possible, though, that EZs divert hiring and cannibalize business investment from surrounding neighborhoods. In the former case, estimates of the effects of EZs that use nearby areas as control groups may underestimate the impact of designation. In the latter case, estimates that use surrounding areas will tend to overestimate the impact of designation. Spatial spillovers are of particular concern in evaluating Texas' EZ Program since census block groups can be very small.

The fact that the estimates in Tables 2-6 differ little with and without county dummies suggests that spillovers may not be large. In regressions excluding county dummies, outcomes in EZ block groups are compared to similar non-EZ block groups throughout the state. In regressions including county dummies, outcomes in EZ block groups are compared to outcomes in similar non-EZ block groups in the same county. The latter approach may help to control for unobserved characteristics of counties that could be correlated with EZ status and affect employment outcomes, but will tend to exacerbate any bias attributable to spillovers between nearby block groups. The similarity in estimated effects in specifications including and excluding county dummies suggests that any such bias may not be large.

However, poverty tends to be spatially concentrated, and regardless of whether county dummies are included, many of the block groups in the sample are located in the same general areas in the state. Therefore, as additional robustness tests, I run regressions in which I exclude from the controls block groups that are in successively larger rings around EZs. In particular, I exclude non-EZ block groups whose centroids fall within a half a kilometer, one kilometer, two kilometers, three kilometers, four kilometers, and five kilometers from an EZ's border. The results from these regressions appear in Table 7, where I present estimates using a cubic polynomial control function alone; the control function and demographic and housing controls; and the control function, demographic and housing controls, and county dummies.

The results for resident and workplace employment are qualitatively similar to those in Table 2 when we exclude non-EZs in very narrow rings around EZs. However, chi-squared tests for the joint significance of the discontinuity gaps estimated for all the covariates suggest that the covariates are not balanced for block groups on either side of the threshold once we exclude control block groups that are within just two kilometers from EZs. This highlights an important trade-off that arises in the context of testing for spatial spillovers. As we exclude more nearby control groups, we mitigate bias arising from spillovers. However, we also make the treated and control groups more dissimilar and potentially introduce more omitted variables bias. Consequently, as the results in Table 7 show, the estimated coefficient on EZ status is more sensitive to the inclusion of demographic and housing covariates as we introduce larger buffers.

Reassuringly, the effects for resident employment are similar to those in Table 2 when other controls are included for rings of up to five kilometers. The workplace employment results become larger in magnitude with larger buffer zones, which would hint that if anything, my main estimates for the effects on workplace employment are biased downward due to the contamination of control groups. This is not surprising given that, as Figure 3 shows, Enterprise Projects that do not locate in an EZ tend to locate near an EZ. However, one should use caution in interpreting these results, as all the coefficients for larger rings are very imprecisely estimated.

VIII. Neighborhood Conditions

EZ designation may have effects on neighborhood conditions above and beyond the impacts on employment.[35] For example, Ham et al. (2011) find that state and federal zone programs reduce local poverty rates, and Hanson (2009) documents positive effects of zone programs on home values in affected communities.

Using the same RD approach as in previous sections, I exploit 2000 Decennial Census and 2005-2009 ACS data to examine the impact of EZ designation in Texas on a variety of neighborhood characteristics. Because the ACS data reflect average neighborhood characteristics between 2005 and 2009, they may not capture the full impacts of EZ designation over the decade.

[35] See Glaeser and Gottlieb (2008), Moretti (2011), and Busso et al. (2011) for detailed discussions of the general equilibrium effects of local subsidies.

Nonetheless, they can provide both a check on the robustness of the resident employment results from the LEHD as well as a more complete picture of how EZ designation affects neighborhoods.

The results appear in Table 8. For the sake of space, I show only the results from specifications that include a cubic control function and the complete set of controls.[36] The results for the change in log resident employment between 2000 and 2005-2009 in column (1) corroborate the resident employment results in the previous section. In particular, we see a positive (albeit statistically insignificant) impact on resident employment. When annualized, the estimate implies a qualitatively and quantitatively similar effect to what was found using the LEHD (approximately 1.2% per year if we assume the difference is measured over seven years).

In the remaining columns of Table 8, I explore the impact of EZ designation on other neighborhood characteristics. There is some indication that EZ designation brings with it slight increases in the population and reductions in the fraction of the population that is black or that has income below the poverty line on average (although these estimates are statistically insignificant). Median household income was similar if not slightly lower in EZs than in other comparable communities on average, which taken together with the impact on poverty, would suggest that EZ designation mainly affects lower income households. Meanwhile, relative to neighborhoods just below the 20% poverty rate cutoff, neighborhoods just above the threshold experienced 10% greater house price appreciation and four percentage points lower home vacancy rates on average. These housing market impacts are both significant at the 10% level.

The RD results using survey data are broadly consistent with those using administrative data and suggest that Texas' EZ Program had a positive effect on communities, but one that was largely confined to households in the lower end of the income distribution. They also suggest that some of the benefits manifested themselves in the housing market. This is consistent with EZ designation rendering certain neighborhoods more attractive places to live given the improved employment prospects for residents.

IX. Conclusion

Despite mixed evidence on their effectiveness, EZs have proliferated in the U.S. as officials have attempted to stoke job creation and improve conditions in their states' most distressed

[36] Due to missing information in the 2005-2009 ACS, we lose one observation for median household income and 26 observations for median home value.

communities. This paper considers the effects of EZ designations on local labor markets in Texas, where since 2003, a non-competitive, rule-based system has been used to assign EZ status to census block groups. In particular, subject to a few exceptions, all block groups with poverty rates of 20% or greater are EZs, while block groups with poverty rates less than 20% are not.

Exploiting the discontinuity in the formula used to determine EZ status to identify the impact of designation among a subset of block groups near the 20% poverty rate threshold, I find using administrative data that EZ designation has positive effects on resident employment. Most of the gains are in jobs paying less than $40,000 a year and are in goods-producing and trade industries. Estimates based on resident survey data also point to positive impacts on communities, but impacts that are concentrated in the lower part of the income distribution. In part due to the particular incentive structure of Texas' program, which stipulates that businesses receiving rewards must hire from but not necessarily locate in EZs, the effects on workplace employment are less precisely estimated. The fact that Texas' program places restrictions more on whom firms must hire than on where firms must locate may help to explain why I find stronger effects on resident employment than do studies on EZ programs elsewhere, whose impacts on resident employment may be diluted by constraining firms to locate in particular areas but leaving unconstrained their choice of whom to hire and from where.

While I find some positive effects of the program, a number of caveats apply. First, the EZ program in Texas is different from those of most other states, where in general localities must apply for EZ status. While Texas' unusual program structure aids in the identification of its impacts on communities, it also limits the extent to which one can generalize the results. Second, the identification strategy used in this paper involves comparing outcomes for a narrow sliver of neighborhoods near the poverty rate threshold used to determine EZ status. Therefore, it would be misguided to assume that if one were to expand EZ coverage to include more affluent communities, it would have similar effects in those areas.

Finally, finding positive effects of the program does not immediately imply that it is cost effective. Unfortunately, because the state government and individual localities each offer incentives to Enterprise Projects, and because the incentives offered by localities vary in generosity, it is difficult to determine the total cost of Texas' program. However, assuming that localities bear the same financial burden as the state and given that the positive employment effects exist for windows around the poverty rate threshold of up to ten percentage points, the implied cost per job for residents of EZs comes to approximately $6500.[37] To the extent that localities bear more of the financial burden and that there are other unmeasured administrative costs, though, the actual cost per EZ job will be higher. Also important to keep in mind, many of the jobs created or preserved are lower-paying positions. This, combined with cost-of-living increases in EZs, would tend to erode any improvement in overall welfare owing to the program.

More generally, this paper contributes to a growing literature on place-based programs and their impacts on targeted communities. Using a quasi-experimental research design that exploits certain institutional details of Texas' EZ Program, I circumvent selection issues that have impeded past work on such programs. Yet Texas' current EZ Program is still in its infancy and differs in important ways from those of other states; any positive effects may not persist in the long run, and programs elsewhere may have very different impacts on neighborhoods. Whether place-based policies can have long-lasting impacts on communities and what features of these programs render them more or less successful in achieving their aims remain open questions.

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[37] In 2002, there were 526 resident jobs per block group on average in the 994 EZs in the ten percentage point window around the poverty rate threshold, where the resident employment effects are stable and positive (see Table 3). According to the Texas CPA, the state spent $17 million on the program in fiscal year 2009. Assuming 1% resident job gains per year (taking into account the slight attenuation of effects due to spillovers) and that localities bore an equal share of the financial burden of the program, the cost per EZ job comes to $6502. This is lower than estimates of the costs per job from many other place-based programs, which could be attributable to the unique structure of Texas' program. However, this estimate is very similar to Criscuolo et al. (2012)'s cost per job estimate for Britain's Regional Selective Assistance program.

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Table 1
Descriptive Statistics

	Poverty Rate Bin			
	[0.18, 0.19)	[0.19, 0.20)	[0.20 0.21)	[0.21 0.22)
A. Demographic and Housing Characteristics (2000 Decennial Census)				
Log Population	7.08	7.06	7.07	7.05
Share Black	0.15	0.18	0.16	0.17
Share Hispanic	0.32	0.37	0.37	0.42
Share Male	0.50	0.49	0.50	0.50
Share Under Age 30	0.46	0.47	0.47	0.48
Share Age 65+	0.12	0.12	0.11	0.11
Share Households Speak Spanish	0.28	0.30	0.32	0.36
Share Foreign Born	0.14	0.15	0.17	0.17
Share Same House as 5 Years Ago	0.51	0.52	0.50	0.50
Share Only HS Degree	0.29	0.28	0.28	0.28
Share Some College	0.21	0.20	0.19	0.19
Share College Degree	0.18	0.17	0.17	0.16
Unemployment Rate	0.07	0.08	0.08	0.08
Labor Force Participation Rate	0.61	0.60	0.61	0.59
Log Household Income	10.37	10.33	10.32	10.28
Log Number of Housing Units	6.16	6.15	6.15	6.09
Share of Homes Vacant	0.10	0.10	0.10	0.09
Share of Homes Owner Occupied	0.61	0.59	0.57	0.57
Log House Value	10.91	10.87	10.88	10.86
Median House Age	31.69	34.24	32.79	34.68
B. Employment Characteristics (2002 LEHD)				
Log Resident Employment	6.14	6.15	6.13	6.11
Low-Wage (<$15,000/year)	5.04	5.08	5.05	5.05
Mid-Wage ($15-$39,999/year)	5.36	5.38	5.36	5.35
High-Wage ($40,000+/year)	4.44	4.37	4.38	4.27
Goods-Producing Industries[1]	4.62	4.58	4.60	4.57
Trade Industries[2]	4.56	4.54	4.53	4.53
Services Industries[3]	5.48	5.52	5.48	5.47
Log Workplace Employment	5.16	5.09	5.28	4.94
Low-Wage (<$15,000/year)	4.10	4.12	4.31	4.02
Mid-Wage ($15-$39,999/year)	4.30	4.25	4.42	4.11
High-Wage ($40,000+/year)	3.35	3.24	3.37	3.06
Goods-Producing Industries[1]	3.14	3.03	3.18	2.90
Trade Industries[2]	3.14	3.22	3.40	3.09
Services Industries[3]	4.06	4.07	4.35	3.95
Block Groups (2000 Definitions)	264	288	248	195

Notes: Includes block groups in Texas that are not in distressed counties or federal EC/RZs and that are not missing 2000 Decennial Census information. [1] Goods-producing industries include agriculture, mining, utilities, construction, and manufacturing industries. [2] Trade industries include wholesale trade, retail trade, and transportation and warehousing. [3] Services industries include information, finance and insurance, real estate, professional services, management, administrative and support services, educational services, health care, arts and entertainment, accommodation and food services, and other services.

Table 2

RD Estimates for Resident and Workplace Employment – Average Annual Change in Log Employment, 2002-2009

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Panel A: Resident Employment								
Enterprise Zone Dummy	0.011**	0.009*	0.011**	0.014*	0.013*	0.019***	0.021**	0.019**	0.022***
	[0.005]	[0.005]	[0.005]	[0.008]	[0.008]	[0.007]	[0.010]	[0.010]	[0.008]
	Panel B: Workplace Employment								
Enterprise Zone Dummy	0.045*	0.040	0.045	0.032	0.030	0.047	0.051	0.050	0.078
	[0.025]	[0.028]	[0.033]	[0.033]	[0.033]	[0.042]	[0.046]	[0.045]	[0.058]
Quadratic in Poverty Rate	Y	Y	Y						
Cubic in Poverty Rate				Y	Y	Y			
Quartic in Poverty Rate							Y	Y	Y
Demographic and Housing Controls		Y	Y		Y	Y		Y	Y
County Dummies			Y			Y			Y
Observations	995	995	995	995	995	995	995	995	995

Notes: Includes block groups in Texas with poverty rates between 0.18 and 0.22 (inclusive) that are not in distressed counties or federal EC/RZs and that are not missing 2000 Decennial Census information. Demographic controls and housing controls are listed in Table 1. Standard errors are adjusted for heteroskedasticity and clusters at the county level. Significant at the * 10% level, ** 5% level, and *** 1% level.

Table 3

RD Estimates for Resident and Workplace Employment, Alternative Windows – Average Annual Change in Log Employment, 2002-2009

	(1)	(2)	(3)	(4)	(5)	(6)
	Enterprise Zone Dummy Coefficient					
	Resident Employment			Workplace Employment		
Window: 0-1 (Full Sample)	0.002	0.001	0.001	0.016**	0.015**	0.017**
Observations: 11,692	[0.002]	[0.002]	[0.002]	[0.008]	[0.007]	[0.008]
Window: 0.1-0.3	0.004	0.003	0.005*	0.036**	0.034**	0.037**
Observations: 5,047	[0.004]	[0.004]	[0.003]	[0.015]	[0.015]	[0.017]
Window: 0.15-0.25	0.011**	0.009**	0.009**	0.052**	0.044*	0.047*
Observations: 2,467	[0.005]	[0.004]	[0.004]	[0.023]	[0.023]	[0.024]
Window: 0.16-0.24	0.014**	0.011**	0.013***	0.038	0.031	0.030
Observations: 1,940	[0.006]	[0.005]	[0.005]	[0.026]	[0.027]	[0.029]
Window: 0.17-0.23	0.013**	0.011*	0.013**	0.033	0.025	0.029
Observations: 1,460	[0.006]	[0.006]	[0.006]	[0.028]	[0.029]	[0.034]
Window: 0.19-0.21	0.021*	0.021**	0.024**	0.034	0.035	0.043
Observations: 536	[0.011]	[0.010]	[0.010]	[0.046]	[0.043]	[0.055]
Cubic in Poverty Rate	Y	Y	Y	Y	Y	Y
Demographic and Housing Controls		Y	Y		Y	Y
County Dummies			Y			Y

Notes: Includes block groups in Texas with poverty rates in the listed windows that are not in distressed counties or federal EC/RZs and that are not missing 2000 Decennial Census information. Demographic controls and housing controls are listed in Table 1. Standard errors are adjusted for heteroskedasticity and clusters at the county level. Significant at the * 10% level, ** 5% level, and *** 1% level.

Table 4
RD Estimates for Resident and Workplace Employment, Including Distressed Counties and Federal EZ/RCs – Average Annual Change in Log Employment, 2002-2009

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Resident Employment									
Enterprise Zone Dummy	0.008	0.008	0.003	0.011	0.012	0.009	0.015	0.013	0.012
	[0.006]	[0.006]	[0.006]	[0.008]	[0.008]	[0.007]	[0.010]	[0.010]	[0.009]
Panel B: Workplace Employment									
Enterprise Zone Dummy	0.056**	0.057**	0.058*	0.041	0.040	0.046	0.065	0.063	0.083*
	[0.025]	[0.027]	[0.034]	[0.030]	[0.030]	[0.038]	[0.040]	[0.038]	[0.050]
Quadratic in Poverty Rate	Y	Y	Y						
Cubic in Poverty Rate				Y	Y	Y			
Quartic in Poverty Rate							Y	Y	Y
Demographic and Housing Controls		Y	Y		Y	Y		Y	Y
County Dummies			Y			Y			Y
Observations	1237	1237	1237	1237	1237	1237	1237	1237	1237

Notes: Includes block groups in Texas with poverty rates between 0.18 and 0.22 (inclusive) that are not missing 2000 Decennial Census information. Includes block groups that are in distressed counties and federal EC/RZs. Demographic controls and housing controls are listed in Table 1. Standard errors are adjusted for heteroskedasticity and clusters at the county level. Significant at the * 10% level, ** 5% level, and *** 1% level.

Table 5

RD Estimates for Resident and Workplace Employment, by Earnings – Average Annual Change in Log Employment, 2002-2009

	(1)	(2)	(3)	(4)	(5)	(6)
	Resident Employment			Workplace Employment		
Low-Wage (<$15,000/year)	0.005	0.006	0.014*	0.035	0.034	0.056
	[0.009]	[0.008]	[0.008]	[0.037]	[0.037]	[0.046]
Mid-Wage ($15,000-$39,999/year)	0.017*	0.015	0.023***	0.016	0.013	0.031
	[0.010]	[0.011]	[0.009]	[0.030]	[0.031]	[0.040]
High-Wage ($40,000/year)	0.022**	0.016	0.019	0.012	0.009	0.037
	[0.011]	[0.011]	[0.014]	[0.035]	[0.036]	[0.047]
Cubic in Poverty Rate	Y	Y	Y	Y	Y	Y
Demographic and Housing Controls		Y	Y		Y	Y
County Dummies			Y			Y
Observations	995	995	995	995	995	995

Notes: Includes block groups in Texas with poverty rates between 0.18 and 0.22 (inclusive) that are not in distressed counties or federal EC/RCs and that are not missing 2000 Decennial Census information. Demographic controls and housing controls are listed in Table 1. Standard errors are adjusted for heteroskedasticity and clusters at the county level. Significant at the * 10% level, ** 5% level, and *** 1% level.

Table 6
RD Estimates for Resident and Workplace Employment, by Industry – Average Annual Change in Log Employment, 2002-2009

	(1)	(2)	(3)	(4)	(5)	(6)
	Resident Employment			Workplace Employment		
NAICS 21: Mining	0.034	0.032	0.021	-0.034	-0.035	-0.056
	[0.034]	[0.032]	[0.036]	[0.044]	[0.044]	[0.046]
NAICS 22: Utilities	0.029	0.028	0.026	0.029	0.037*	0.036
	[0.026]	[0.024]	[0.028]	[0.022]	[0.022]	[0.025]
NAICS 23: Construction	0.027**	0.025*	0.035**	0.033	0.032	0.056
	[0.013]	[0.014]	[0.015]	[0.045]	[0.043]	[0.053]
NAICS 31-33: Manufacturing	0.014	0.012	0.021**	-0.029	-0.03	-0.029
	[0.016]	[0.015]	[0.010]	[0.070]	[0.069]	[0.081]
NAICS 42: Wholesale Trade	0.022	0.019	0.025*	-0.059	-0.052	-0.020
	[0.015]	[0.014]	[0.015]	[0.044]	[0.047]	[0.048]
NAICS 44-45: Retail Trade	0.012	0.01	0.018**	0.032	0.028	0.092**
	[0.011]	[0.011]	[0.009]	[0.041]	[0.043]	[0.043]
NAICS 48-49: Transp. & Warehousing	0.036*	0.032**	0.018	0.024	0.027	0.027
	[0.018]	[0.015]	[0.015]	[0.042]	[0.049]	[0.057]
NAICS 51: Information	-0.002	-0.005	-0.003	-0.039	-0.025	0.010
	[0.018]	[0.017]	[0.016]	[0.044]	[0.044]	[0.053]
NAICS 52: Finance and Insurance	0.008	0.006	0.016	0.007	0.014	0.045
	[0.014]	[0.014]	[0.016]	[0.059]	[0.061]	[0.070]
NAICS 53: Real Estate	-0.001	-0.005	0.009	0.058	0.051	0.088**
	[0.023]	[0.022]	[0.022]	[0.036]	[0.037]	[0.040]
NAICS 54: Professional Services	0.019	0.017	0.015	-0.018	-0.016	0.026
	[0.016]	[0.015]	[0.013]	[0.034]	[0.036]	[0.044]
NAICS 56: Admin. & Support Services	0.011	0.008	0.003	-0.013	-0.012	0.008
	[0.017]	[0.015]	[0.014]	[0.038]	[0.041]	[0.049]
NAICS 61: Educational Services	0.014	0.016	0.02	0.006	0.009	0.033
	[0.015]	[0.016]	[0.014]	[0.031]	[0.029]	[0.040]
NAICS 62: Health Care	0.007	0.009	0.020*	0.041	0.052	0.072*
	[0.011]	[0.012]	[0.010]	[0.037]	[0.035]	[0.043]
NAICS 71: Arts & Entertainment	0.025	0.026	0.031	0.018	0.027	0.023
	[0.025]	[0.024]	[0.027]	[0.040]	[0.038]	[0.042]
NAICS 72: Accom. & Food Services	0.004	0.002	0.017	-0.025	-0.03	-0.026
	[0.015]	[0.014]	[0.012]	[0.053]	[0.053]	[0.062]
NAICS 81: Other Services	-0.007	-0.009	0.005	-0.021	-0.013	0.012
	[0.016]	[0.015]	[0.015]	[0.048]	[0.048]	[0.055]
Cubic in Poverty Rate	Y	Y	Y	Y	Y	Y
Demographic and Housing Controls		Y	Y		Y	Y
County Dummies			Y			Y
Observations	995	995	995	995	995	995

Notes: Includes block groups in Texas with poverty rates between 0.18 and 0.22 (inclusive) that are not in distressed counties or federal EC/RCs and that are not missing 2000 Decennial Census information. Demographic controls and housing controls are listed in Table 1. Standard errors are adjusted for heteroskedasticity and clusters at the county level. Significant at the * 10% level, ** 5% level, and *** 1% level.

Table 7

RD Estimates for Resident and Workplace Employment, Excluding Non-EZ Block Groups Proximal to EZs – Average Annual Change in Log Employment, 2002-2009

	(1)	(2)	(3)	(4)	(5)	(6)
Excluding non-EZ Block Groups	Resident Employment			Workplace Employment		
Within 0.5km of an EZ	0.007	0.013	0.022*	-0.001	0.005	0.039
Observations: 722	[0.012]	[0.011]	[0.011]	[0.041]	[0.043]	[0.060]
Within 1km of an EZ	-0.004	0.004	0.017	0.009	0.022	0.074
Observations: 621	[0.015]	[0.014]	[0.017]	[0.051]	[0.053]	[0.076]
Within 2km of an EZ	-0.013	0.002	0.021	0.040	0.049	0.118
Observations: 552	[0.022]	[0.021]	[0.030]	[0.062]	[0.067]	[0.114]
Within 3km of an EZ	-0.005	0.010	0.020	0.024	0.039	0.132
Observations: 527	[0.022]	[0.021]	[0.029]	[0.065]	[0.069]	[0.126]
Within 4km of an EZ	0.003	0.022	0.032	0.060	0.078	0.179
Observations: 517	[0.027]	[0.026]	[0.028]	[0.078]	[0.083]	[0.134]
Within 5km of an EZ	-0.020	0.0004	0.009	0.091	0.110	0.194
Observations: 506	[0.019]	[0.020]	[0.019]	[0.094]	[0.102]	[0.176]
Cubic in Poverty Rate	Y	Y	Y	Y	Y	Y
Demographic and Housing Controls		Y	Y		Y	Y
County Dummies			Y			Y

Notes: Includes block groups in Texas with poverty rates between 0.18 and 0.22 (inclusive) that are not in distressed counties or federal EC/RCs and that are not missing 2000 Decennial Census information. Demographic controls and housing controls are listed in Table 1. Standard errors are adjusted for heteroskedasticity and clusters at the county level. Significant at the * 10% level, ** 5% level, and *** 1% level.

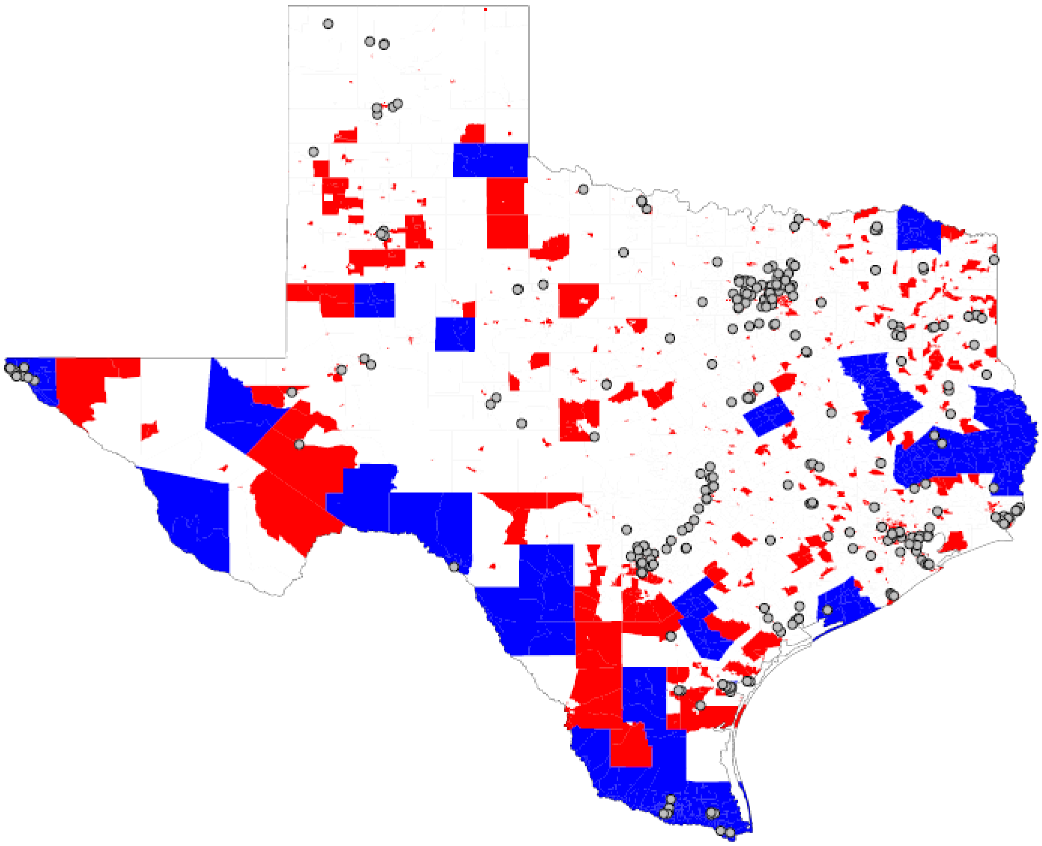
Table 8
RD Estimates for American Community Survey Outcomes, 2000 to 2005-2009

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Log Resident Employment	Log Population	Share Black	Poverty Rate	Log Median Household Income	Log Median House Value	Share Housing Units Vacant
Enterprise Zone Dummy	0.085	0.023	-0.033	-0.033	-0.009	0.107*	-0.040*
	[0.082]	[0.106]	[0.033]	[0.038]	[0.081]	[0.054]	[0.022]
Cubic in Poverty Rate	Y	Y	Y	Y	Y	Y	Y
Demographic and Housing Controls	Y	Y	Y	Y	Y	Y	Y
County Dummies	Y	Y	Y	Y	Y	Y	Y
Observations	995	995	995	995	994	969	995

Notes: Includes block groups in Texas with poverty rates between 0.18 and 0.22 (inclusive) that are not in distressed counties or federal EC/RCs and that are not missing 2000 Decennial Census information. Demographic controls and housing controls are listed in Table 1. Standard errors are adjusted for heteroskedasticity and clusters at the county level. Significant at the * 10% level, ** 5% level, and *** 1% level.

Figure 1

Texas Enterprise Zones and Enterprise Projects

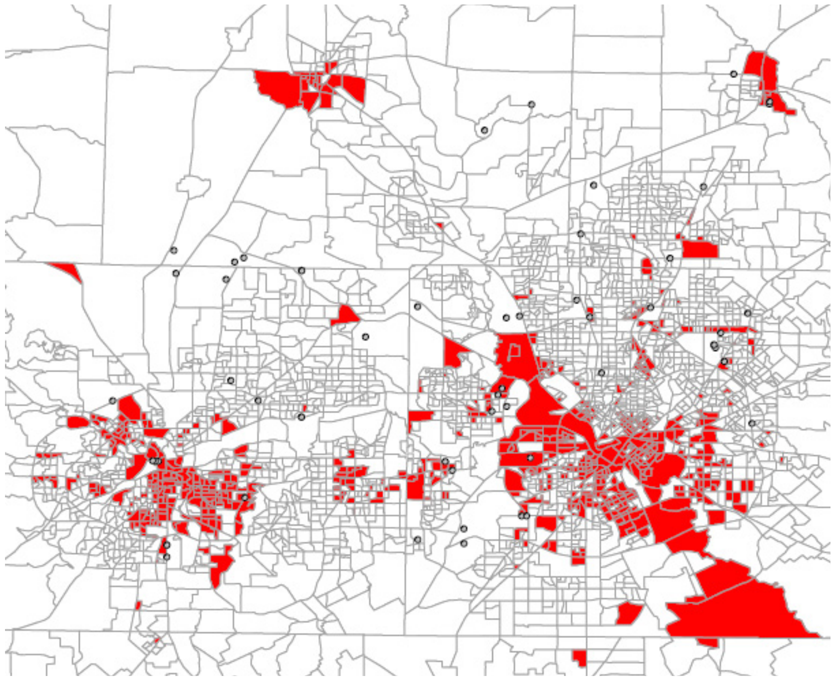


Notes: Colored areas are enterprise zones as of 2010. Red areas are EZ block groups that do not qualify as distressed counties or as federal EZ/RCs (3,598 block groups). Blue areas are either distressed counties or federal EZ/RCs (encompassing 1,465 block groups). The points represent the 297 Enterprise Projects that received awards between September 2003 and September 2010 that could be precisely geocoded. 23 Enterprise Projects could not be precisely geocoded and do not appear on the map.

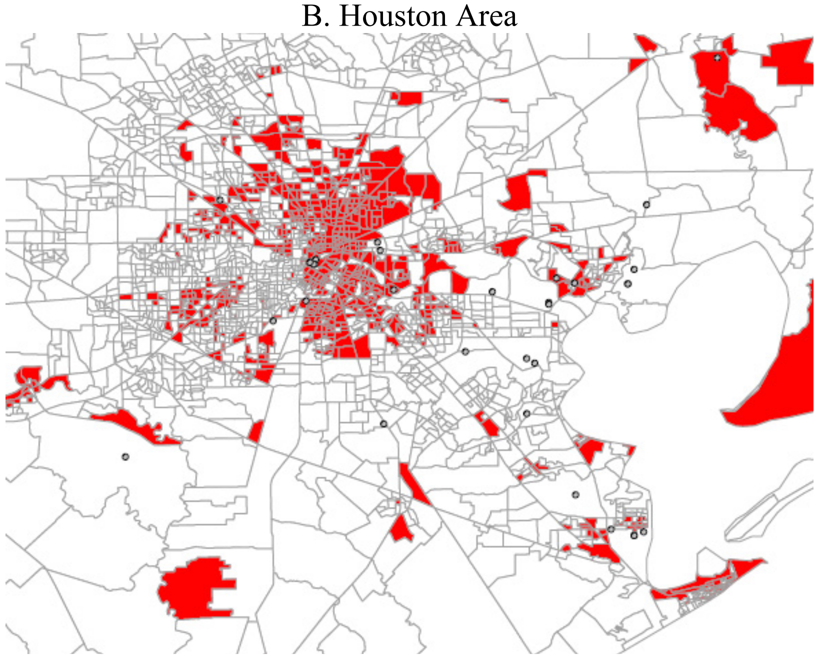
Figure 2

Enterprise Zones and Enterprise Projects in Selected Texas Cities

A. Dallas-Fort Worth Area



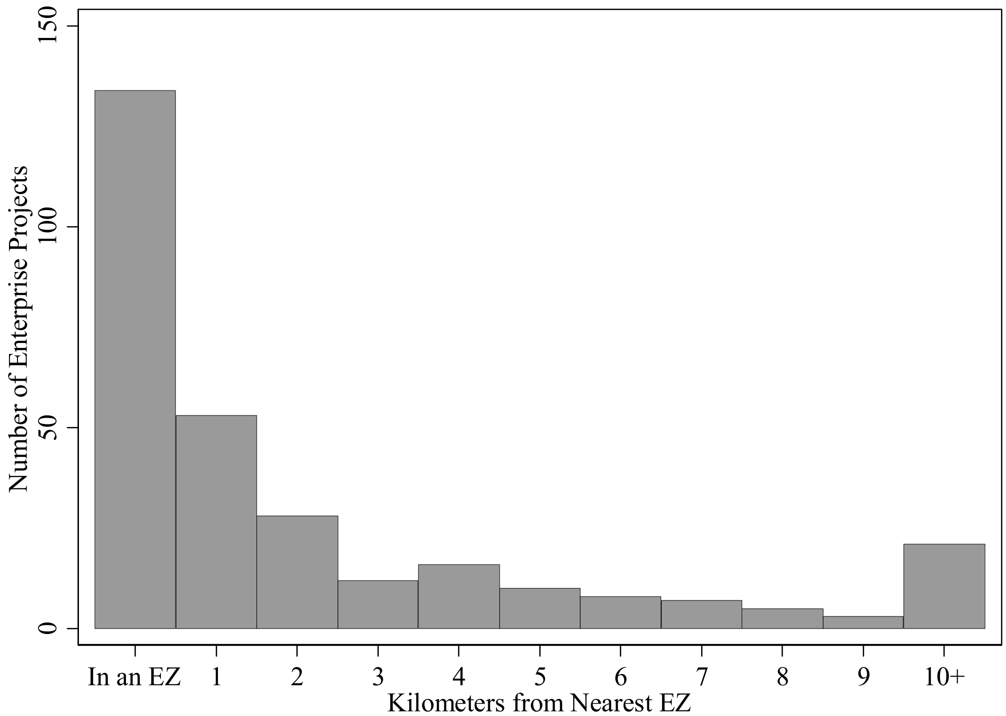
B. Houston Area



Notes: Colored areas are enterprise zones as of 2010. Red areas are EZ block groups that do not qualify as distressed counties or as federal EZ/RCs. The points represent Enterprise Projects that received awards between September 2003 and September 2010 that could be precisely geocoded.

Figure 3

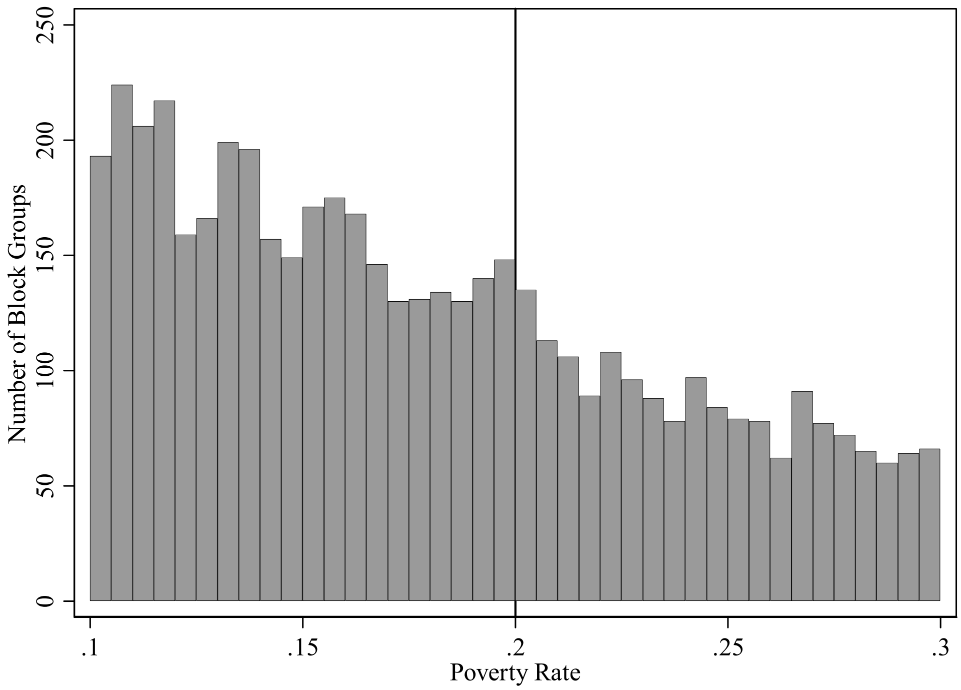
Spatial Distribution of Enterprise Projects



Notes: Includes 297 Enterprise Projects that received awards between September 2003 and September 2010 and that could be precisely geocoded. 23 Enterprise Projects could not be precisely geocoded.

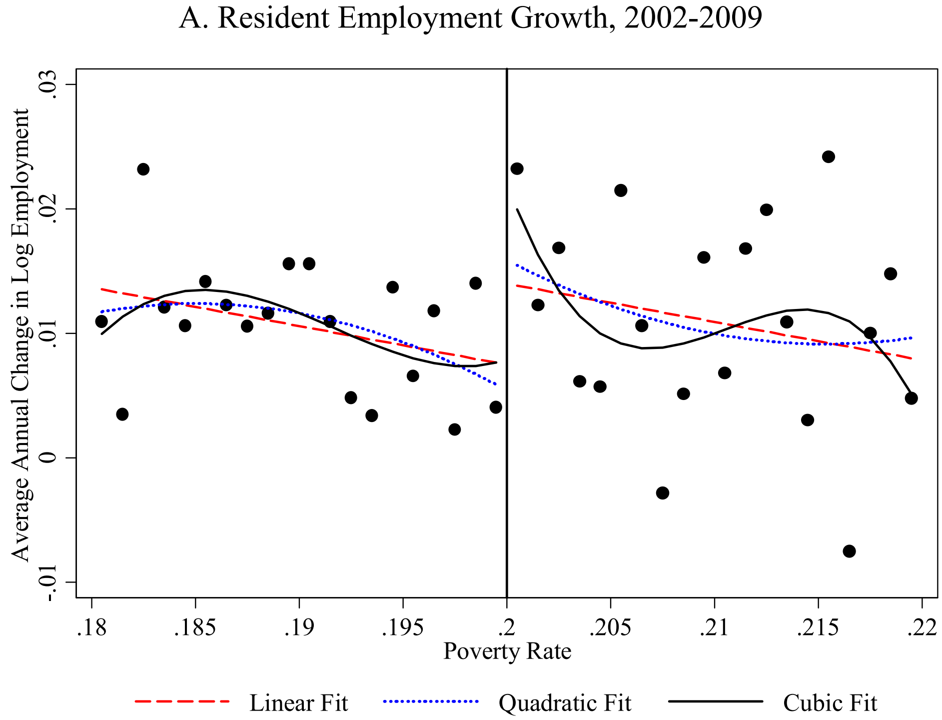
Figure 4

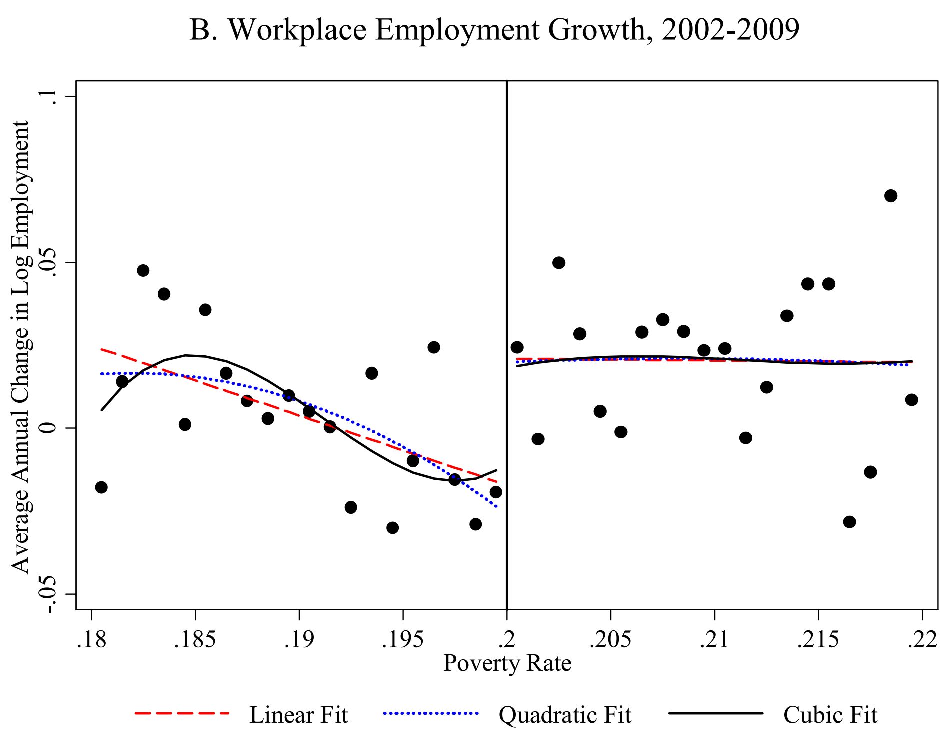
Density of the Forcing Variable



Notes: Count of census block groups within half percentage point bins of the poverty rate.

Figure 5
Resident and Workplace Employment Growth at the Poverty Rate Threshold for EZ Designation



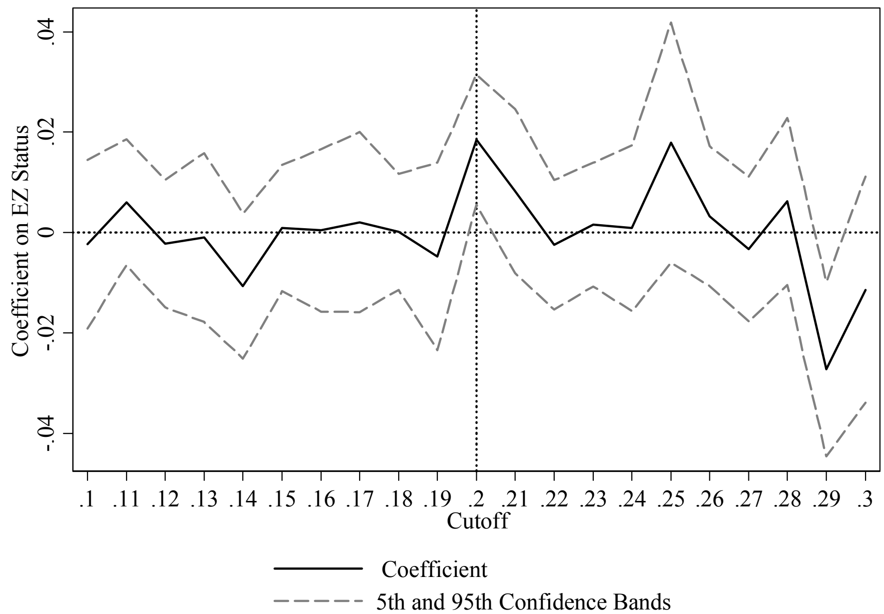


Notes: Sample includes 995 block groups. Bin size = 0.001.

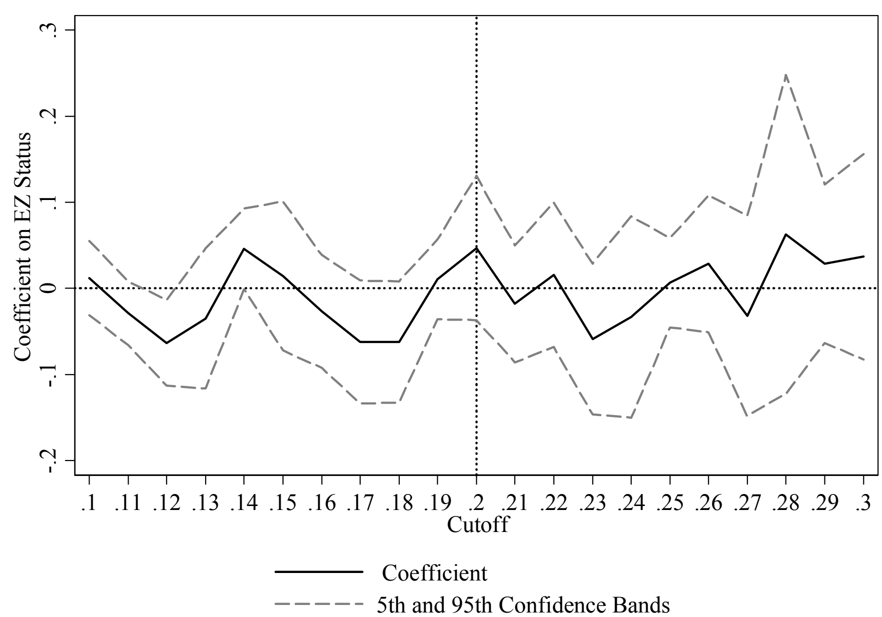
Figure 6

Resident and Workplace Employment Growth at the Placebo Poverty Rate Thresholds for EZ Designation

A. Resident Employment Growth, 2002-2009



B. Workplace Employment Growth, 2002-2009



Notes*:* The window around each placebo cutoff is four percentage points. Regressions include the full set of demographic and housing controls listed in Table 1 as well as county dummies.

Appendix

Table A1
Maximum Refunds by Enterprise Project Designation Type

Designation Type	Level of Capital Investment	Maximum Number of Jobs Allocated	Maximum Potential Refund	Maximum Refund per Job Allocation
Single Project	$40,000 to $399,999	10	$25,000	$2,500
Single Project	$400,000 to $999,999	25	$62,000	$2,500
Single Project	$1M to $4,999,999	125	$312,500	$2,500
Single Project	$5M to $149,999,999	500	$1,250,000	$2,500
Double Jumbo Project	$150M to $249,999,999	500	$2,500,000	$5,000
Triple Jumbo Project	$250M or more	500	$3,750,000	$7,500

Source: Texas State Office of the Governor, Economic Development and Tourism Division.

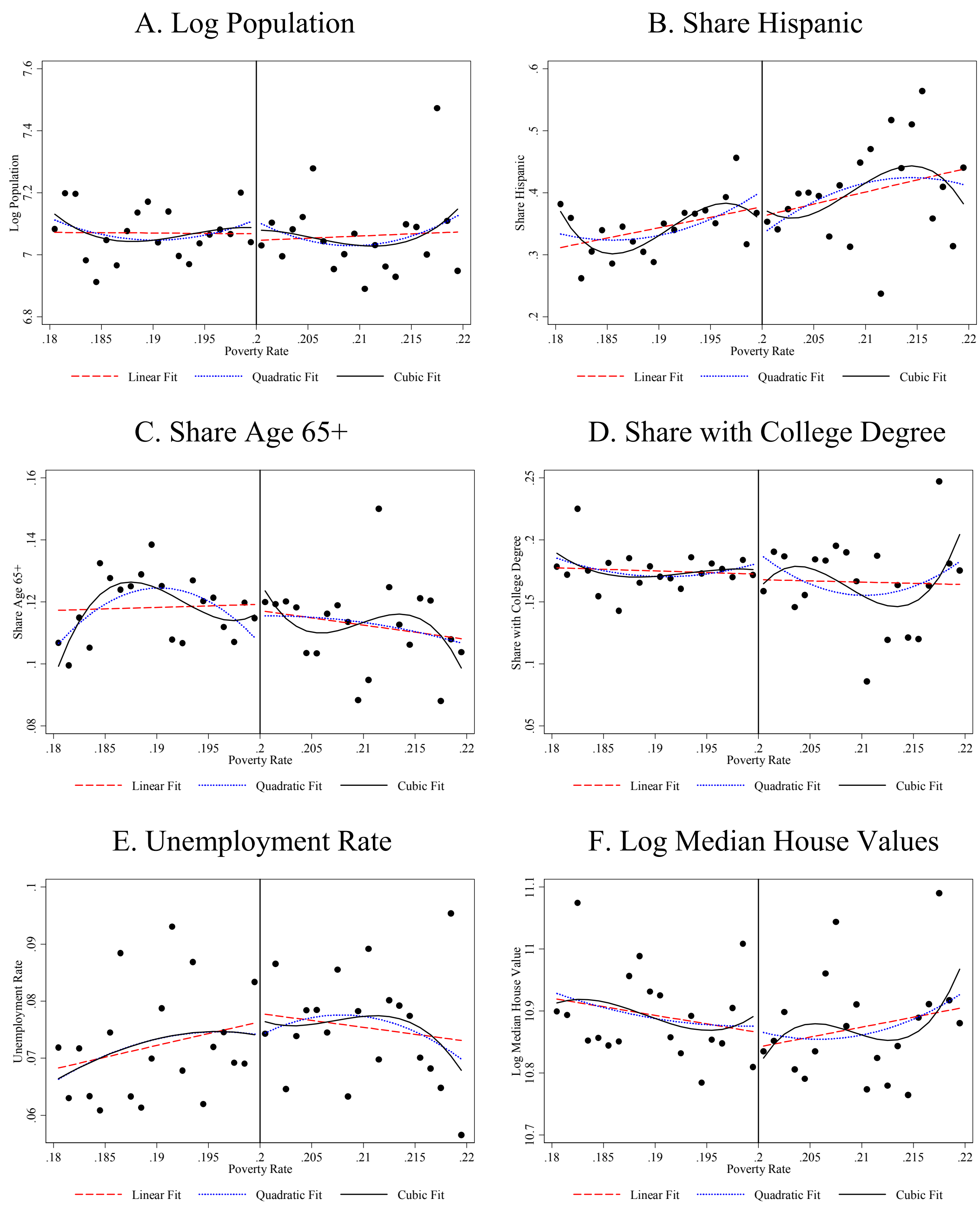
Table A2
McCrary (2008) Test for a Discontinuity in the Density of the Forcing Variable

	Bin Size	
	0.001	0.0001
Discontinuity Estimate	1.745	0.174
Window: 0-1 (Full Sample)	[3.354]	[0.292]
Observations (Bins)	1000	10000
Discontinuity Estimate	-0.445	-0.024
Window: 0-0.4	[6.312]	[0.708]
Observations (Bins)	400	4000
Discontinuity Estimate	-1.823	-0.179
Window: 0.1-0.3	[2.683]	[0.297]
Observations (Bins)	200	2000
Discontinuity Estimate	-3.086	-0.262
Window: 0.15-0.25	[3.396]	[0.415]
Observations (Bins)	100	1000
Discontinuity Estimate	0.498	0.065
Window: 0.16-0.24	[3.719]	[0.459]
Observations (Bins)	80	800
Discontinuity Estimate	-0.908	-0.066
Window: 0.18-0.22	[5.600]	[0.658]
Observations (Bins)	40	400

Notes: These represent the results of a McCrary (2008) test in which I counted the number of block groups in bins (0.1 percentage point or 0.01 percentage point) within a window (0-100%, 0-40%, 10-30%, 15-25%, 16-24%, or 18-22%) around the poverty rate cutoff., then regressed the frequencies on a dummy for EZ status and a cubic polynomials in the poverty rate bin (differenced from zero). The polynomial coefficients are allowed to differ above and below the cutoff. Standard errors are in brackets. Significant at the * 10% level, ** 5% level, and *** 1% level.

Figure A1

Selected Baseline Characteristics at the Poverty Rate Threshold for EZ Designation



Notes: Sample includes 995 block groups. Bin size = 0.001.