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Dissertation Awards

2005

# The Impact of Enterprise Zones on Resident Employment: An Evaluation of the Enterprise Zone Programs of California and Florida: Dissertation Summary

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# The Impact of Enterprise Zones on Resident Employment: An Evaluation of the Enterprise Zone Programs in California and Florida

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In the last two decades, the prevalence of enterprise zone programs has grown substantially as local, state, and federal policymakers look for ways to bring economic development to disadvantaged areas. One of the motivations for these costly and geographically targeted programs is to enhance job opportunities for people living in the zones. However, little is known about how enterprise zones affect resident employment. It is difficult to estimate the impact of these programs on the employment of people living in the zones for a number of reasons. In most program evaluation problems, the process that determines the outcome of interest (e.g., wages) and the process that determines selection for treatment (e.g., receive training) happen at the same level of aggregation. In evaluating the effect of enterprise zones on resident employment, an individual-level process determines the outcome of interest (resident employment) while selection for treatment occurs at the neighborhood level. Therefore, standard program evaluation techniques have to be modified to address this issue. A further problem is that enterprise zones are designated at very detailed levels of geography, which makes it difficult to define who is a zone resident and to measure the characteristics of zones across time.

I address these issues in my study of the effects of California's and Florida's enterprise zone programs on resident employment. I develop and implement a methodology to address the unusual selection process of these programs. The first step is to create a neighborhood-level measure of the component of residents' employment probability that is explained by the neighborhood when controlling for the characteristics of area residents. To do this, I estimate the component of employment probability correlated with residential neighborhood, which I call the conditional employment probability. The next step is to estimate the effect of enterprise zones on resident employment by comparing the conditional employment probabilities of neighborhoods containing enterprise zones with those of comparable areas. This is accomplished with tract-level propensity score matching.

I find that a substantial portion of the variation across neighborhoods in employment rates can be explained by controlling for the attributes of residents. This indicates that it is important to control for resident characteristics when making cross-neighborhood comparisons. Using propensity score matching, I find a pool of non-zone tracts that are observationally similar to tracts containing enterprise zones. I use these non-zone tracts to create an estimate of what the conditional employment probabilities in zone tracts would have been in the absence of the programs. Even though I focus on two very targeted and generous enterprise zone programs, I find no evidence that the programs impacted the employment of zone residents.

My dissertation has three interrelated chapters. The first chapter provides background information regarding enterprise zones, the enterprise zone programs in California and Florida, and the prior literature on enterprise zones. This chapter also describes the methodology I develop to estimate the effect of enterprise zones on resident employment and the data I use to implement that methodology. The estimation of the tract-level conditional employment probabilities is discussed in the second chapter. In the third and final chapter, I implement tract-level propensity score matching, provide estimates of the effect of enterprise zones on resident employment, and draw conclusions. In the following pages, I summarize each chapter in turn, which also provides an effective summary of the dissertation as a whole.

## Chapter 1: Framework for Evaluating the Impact of Enterprise Zones on Resident Employment

Enterprise zones are programs where governments provide incentives for businesses to grow in targeted geographically defined areas, typically ones that have had below-average economic growth and have a mixture of residential and business land use. The incentives provided are most often a combination of property and income tax abatements, advantageous permitting and regulation, some infrastructure improvements, and tax credits for job creation. By the year 2000, at least 40 states had enterprise zone programs, and the federal government had implemented similar programs. One of the motivations for these geographically targeted economic development programs is to improve the employment outcomes of people living in the targeted areas.

This dissertation estimates whether two enterprise zone programs achieved that goal and increased resident employment. I focus exclusively on the enterprise zone programs in California and Florida from the period 1986–1990. These programs share a number of features that make them interesting and feasible to study. First, both programs had generous hiring tax credits for hiring chronically unemployed workers; Florida had a similar tax credit for hiring zone residents. Both states designated enterprise zones in areas with high poverty

and unemployment rates. There were a sufficient number of enterprise zones in each state to allow me to provide separate estimates by state. Finally, detailed maps that show enterprise zone locations are available for both states.

According to economic theory, enterprise zone programs can affect resident employment by changing demand for zone residents' labor and by reducing zone residents' cost of working. Demand can change for several reasons. First, if zones lead to growth in the number of jobs located in the community that match the skills of zone residents, this would generally increase demand for local labor and then benefit zone residents. In zones where tax credits are offered for hiring zone residents, the cost of hiring zone residents would fall relative to other workers, which could affect both supply and demand for zone resident labor. For zone programs that include capital subsidies, businesses in zones may be induced to substitute capital for labor, which would reduce demand for labor generally and thereby reduce the employment of zone residents. I estimate the net effect of all these factors.

While there has been a substantial literature on the effect of enterprise zone programs on economic development (discussed in the dissertation), very little research has focused on the effect of enterprise zones on zone resident employment. In their recent book, Peters and Fisher (2002) look at commuting patterns data and find that only a fifth of people who work in zones live in the zones. In an early evaluation of Indiana's enterprise zones, Papke (1993) finds that enterprise zone residents are slightly more likely to be employed than others. Looking at six states, Greenbaum and Engberg (2000) find mixed results that suggest that enterprise zones have no significant effect on the growth rate of resident employment and that zones increase the growth rate of unemployment.

To evaluate the effect of enterprise zones on resident employment, it is necessary to do cross-neighborhood comparisons of resident employment measures. Comparing unconditional employment rates across neighborhoods may be misleading for several reasons. First, compared to characteristics like race, marital status, or education, neighborhood is a weak predictor of employment. Therefore, small differences in the demographics of neighborhoods could lead to large differences in neighborhood employment rates. Another issue is that even the best data available for neighborhood-level employment measures, the full long-form sample of the decennial censuses, can suffer from small sample distortions for very detailed definitions of neighborhood. Conditioning on the characteristics of residents can reduce these distortions. Therefore, generating neighborhood-level employment estimates that are conditional on resident characteristics facilitates cross-neighborhood comparisons. Because enterprise zones are very different from most other areas, it also is important to compare enterprise zones to observationally similar areas in order to generate estimates of what would have happened without the zone programs.

To address these issues, I develop and implement a three-stage estimation strategy that blends employment probability models with neighborhood-level propensity score matching to get estimated effects that condition on both resident characteristics and selection into containing an enterprise zone. The first stage uses employment probability models in order to calculate the component of employment probability that is correlated with neighborhood conditional on the characteristics of the people who live in the neighborhood. The second stage estimates the propensity for an area to be designated an enterprise zone. The third stage estimates the effect of enterprise zone policies on resident employment by matching on the estimated propensity scores. A more complete presentation of the estimation strategy follows.

The parameter of interest in this study is the average effect of containing an enterprise zone on resident employment probability for areas containing a zone, conditional on the traits of residents. This is also called the treatment effect on the treated, where the treatment for a neighborhood is containing an enterprise zone. More formally, the parameter of interest is

$$\Delta = E[Y_1 - Y_0 \mid T = 1, X = x],$$

where $T = 1$ if the area contains an enterprise zone, $Y_0$ is the employment rate in an area in the absence of an enterprise zone, $Y_1$ is the same with an enterprise zone, and $X$ is a vector of the demographic characteristics of the people who live in the area. What makes this nontrivial is that 1) it is not possible to observe $Y_1$ and $Y_0$ for the same area, and 2) it is necessary to condition on $X$. The first stage conditions on $X$ by estimating the probability that an individual is employed as a function of their own characteristics as well as area fixed effects. The model estimated is

$$y_{ij} = f(\beta X_{ij} + \alpha_j + \varepsilon_i),$$

where $i$ indexes individuals and $j$ indexes areas, if $y_{ij} = 1$ individual $i$ in $j$ is employed and 0 otherwise, $X_{ij}$ is a set of characteristics of individual $i$ in $j$, $\alpha_j$ is an area fixed effect, and $\varepsilon_i$ is an error term. Because $\alpha_j$ is conditional on $X_{ij}$, the parameter of interest becomes:

$$\Delta^c = E[g(\alpha_1) - g(\alpha_0) | T = 1] = E[g(\alpha_1) | T = 1] - E[g(\alpha_0) | T = 1],$$

where $\alpha_1$ is the area effect if $T = 1$ and $\alpha_0$ is the area effect if $T = 0$, and g(.) is a function that maps the coefficient estimate to a marginal effect.

By estimating the fixed effect, the estimation problem becomes like other program evaluation problems, where the difficulty is in estimating the counterfactual,

$$E[g(\alpha_0)|T = 1].$$

I use propensity score matching to estimate the counterfactual rather than regressions because enterprise zones were designated in a small number of distressed areas. The vast majority of areas are not similar to enterprise zones, so most nontreated areas provide little information about what would have happened to enterprise zone areas in the absence of the programs. Propensity score matching resolves this problem by systematically selecting relevant comparison areas from a large pool of mostly irrelevant areas. Also, because they are so disadvantaged, enterprise zones would be outliers in most regressions of area traits on employment outcomes. Therefore, models that fit most areas are likely to fit poorly for enterprise zones. Matching estimates do not suffer from this problem because matching does not impose a specific functional form on the relationship between observable characteristics and the outcome of interest.

Of the assumptions necessary to use propensity score matching, the assumption that selection is strictly on observable characteristics usually raises the most concern. It is possible that unobservable characteristics influenced which of the areas that met the states' criteria were designated enterprise zones. However, as Greenbaum and Engberg (2000) note, enterprise zones were designated by state governments in accordance with policies that outline specific levels of poverty, unemployment, or other observable characteristics. In California and Florida, much of the legislated selection process depended on data from the 1980 Census of Population and Housing similar to that which I use to estimate the propensity scores. Therefore, the concern about selection on unobservable characteristics is less problematic than in many other contexts.

My methodology is data intensive. It requires individual-level data from some period after the designation of the zones, neighborhood-level demographic and economic data from prior to the designation of zones, and data on the location of zones. Since earlier estimates of the effect of enterprise zones on employment suggest that any effects are small, it is very important to minimize measurement error by using the same detailed geographic definition in both the pre- and postdesignation periods. For this reason, I use 1980 census tract–census place combinations (which I will call tracts) as my definition of neighborhood. The postdesignation data that I use are the restricted access individual-level microdata from the 1990 Decennial Census of Population and Housing. Because these data have geographic units that are finer than 1980 census tract, I can use 1980 census geographic definitions for the postdesignation period as well as the predesignation period. The predesignation neighborhood-level data come from the tract-level tables from the 1980 Census of Population and Housing. To measure economic growth immediately before the period when zones were designated, I make census-place-level tabulations of the number of jobs and establishments for each year from the 1982 through 1986 Standard Statistical Establishment Lists. These are restricted access establishment-level databases maintained by the Census Bureau that are used as the sample frame for census establishment surveys and as the source data for the County Business Patterns series.

Without access to the restricted access microdata, I would have to use less precise geographic definitions, such as zip codes, and would induce measurement error by converting data from one unit of geography to another. I find that on average less than 50 percent of the 1990 population of tracts that contain enterprise zones actually live in the zones. To further reduce measurement error, I drop tracts from my sample that contain a zone but where less than 25 percent of the population lives in a zone. For the remaining tracts that contain a zone, over 70 percent of the 1990 population lives in the zones.

## Chapter 2: Neighborhood and Employment: Separating Who You Are from Where You Live

This chapter describes the estimation of the component of employment probability explained by tract and explores the resulting tract effect estimates. I estimate these tract effects using several methods and then compare the resulting estimates to see if they are sensitive to estimator or sample. The discussion of the estimation of these tract effects is very detailed for two reasons. First, there is no prior literature about directly estimating the component of employment probability explained by neighborhood, which makes it important to explore a variety of ways to estimate the effects and to evaluate the resulting estimates. Second, the tract effects are central to my study. They are the tract-level outcomes that I use in the propensity score matching that produces the final estimates of the effects of enterprise zones on resident employment.

I use three estimators to estimate different sets of tract effects: individual-level probit and linear probability (OLS) models with tract fixed effects and tract-level weighted least-squares (WLS) models. The dataset used to estimate these models is the restricted access long form sample of 1990 decennial census. This individual-level dataset is from the full 1-in-6 sample of households from which the Public Use Micro Samples are drawn. The method for calculating the tract effects differs across the

different estimators. In the probit model, the tract effects are the marginal effects derived from the coefficients on the tract fixed effects. In the linear probability models, the tract effects are simply the fixed effect coefficients. The tract effect estimates from the WLS models are the predicted residuals for the tracts. The employment probability models are estimated for three samples of people aged 18–55 for each state: men and women in the labor force, men in the labor force, and men regardless of labor force participation.

I find that the three methods for estimating the tract effects generate very similar results. To compare the tract effects across samples and estimators, I present density graphs, scatter plots, means, and tables of correlations. The distributions of the tract effects from all three estimators are similar and have smaller tails than the distribution of the unconditional employment rates. The tract effects are highly correlated across estimators for both California and Florida. The most highly correlated tract effects are the probit and OLS estimates and the least correlated effects are the probit and the WLS effects, for which the coefficient of correlations is still above 0.9. The various tract effect estimates are more correlated with each other than with the unconditional employment rate, which shows that all of the tract effect estimators control for the influence of resident characteristics on neighborhood employment rates.

One interesting result from this chapter is that the tract-level WLS estimates of tract effects are very similar to those estimated with individual-level employment probability models. The WLS models have two practical advantages over the individual-level models: ease of computation and potential of using publicly available data. Using a powerful computer, the individual-level probit models for the California sample take at least 11 hours to run per sample. This implies that bootstrapping the estimation procedure with 300 replications would take months. The WLS models can be estimated in less than a minute, which makes bootstrapping the estimation procedure feasible. The other advantage is that the WLS estimator uses data comparable to that which is publicly released at the tract-level by the Census Bureau. The similarity of the tract effect estimates from the tract-level and individual-level models suggests that the estimation strategy I use to evaluate enterprise zones could be applied using publicly released data. This implies that researchers interested in using this methodology can take advantage of publicly available data for their research.

## Chapter 3: The Impact of Enterprise Zones on Resident Employment

In California and Florida, enterprise zones were designated in disadvantaged areas where one would expect low resident employment rates. For this reason, it is necessary to control for the selection of enterprise zones and generate an estimate of what would have happened in enterprise zone tracts in the absence of the program. I use tract-level propensity score matching to do this. Propensity score matching generates an index of observable characteristics correlated with containing a zone. The index is called a propensity score. Tracts that contain a zone are then matched to tracts that do not contain a zone but have nearly equal propensity scores and, therefore, similar observable characteristics. The average treatment effect estimate is the average difference in the outcome of interest between zone tracts and matching non-zone tracts.

To implement propensity score matching, I first estimate the probability that a tract contains part of an enterprise zone as a function of observable characteristics. I do this using separate probit models for each state to capture differences in the designation process. Variables from the 1980 census included in these models are log median household income, poverty rate, unemployment rate, share of households receiving public assistance, share of households headed by a single mother, share of population that is nonwhite, share of adults with more than high school degree, share of workers employed in manufacturing, housing vacancy rate, and share of housing units built in the prior 10 years. To control for differences in business climate prior to the existence of the zones, I use the city-level job growth rate from 1983 to 1986 calculated from the Standard Statistical Establishment List. The estimated probabilities of containing an enterprise zone are called propensity scores. Enterprise zone tracts are matched to non-zone tracts with similar propensity scores. The outcomes of these comparable non-zone tracts provide an estimate of what would have happened in the zone tracts without the program.

In order to have estimates comparable to the prior literature, I look at the effect of enterprise zones on both the unconditional resident employment rate and the conditional employment probability. In both states, the difference in the average conditional employment probabilities between zone tracts and all non-zone tracts is less than 30 percent of the difference in the average employment rate. This shows that much of the difference between the employment rates in zone tracts and non-zone tracts can be explained by who lives in the tracts. The propensity score matching estimates discussed below show that a large portion of the difference in resident employment between zone tracts and comparable tracts is also explained by resident characteristics.

The average treatment effect estimates show that ignoring resident characteristics can lead to an overly pessimistic estimate of the effect of enterprise zones on

resident employment. If I do not condition on resident characteristics, the estimated effects of enterprise zones on resident employment is consistently negative and close to a 1-percentage-point reduction in employment. When conditioning on resident characteristics, the estimated effects are close to zero. In California, when pooling men and women the estimated effect of enterprise zones on the employment rate is –1.5 percentage points, and the estimated effect on the conditional employment probability is –0.5 percentage points. For the equivalent samples from Florida, the estimated effect on the unconditional employment rate is –0.7 percentage points, and the effect on the conditional employment probability is –0.1 percentage points. The effect of California zones on men's employment is roughly the same: –1.3 percentage points (unconditional) and –0.4 percentage points (conditional). In Florida, the estimated effects on men's unconditional and conditional employment are close, respectively –1.4 percentage points and –1.2 percentage points. These point estimates come from kernel matching; I find similar results with nearest neighbor matching. Due to the small point estimates of the effects and the semiparametric estimators, the estimates discussed are not significantly different from zero or each other.

While the effects of enterprise zones on the employment of residents in the labor force are the primary focus of my dissertation, I also estimate the effect of enterprise zones on the employment of men when including men out of the labor force and on the employment of people living near but not in the zones. The effect on the joint probability that men are in the labor force and employed conditional on resident characteristics is –1.4 in California and 0.1 in Florida. In both states, the estimates that condition on men's characteristics are less negative than the unconditional estimates. The estimated effect of enterprise zones on the employment of people living outside the zones but within five miles of the center of the nearest zone is small in both states, ranging from 0.2 to 0.5 percentage points.

A critique of my methodology is that the zone programs may have led to selective migration into or out of enterprise zones, in which case resident characteristics should not be treated as exogenous to the programs. To address this, I show that zone tracts had migration patterns similar to comparable non-zone tracts. I also perform several sensitivity analyses to see if my estimates are sensitive to what variables are included in the propensity score models, how the tract-level employment probability models are specified, and whether the conditional employment probabilities are estimated with an individual-level probit. While the magnitudes of the point estimates are affected in some cases, the main results hold: conditioning on resident characteristics makes the estimated effect of enterprise zones on resident employment less negative (or more positive) and close to zero. One caveat that might explain the lack of measurable effects is that the programs studied had been in effect for only three years prior to the year I measure resident employment, 1990.

The enterprise zone programs in California and Florida were atypical in that they were carefully targeted and provided relatively large incentives for hiring people with a history of unemployment and, in Florida, zone residents. The majority of the zone program expenditures in these states were spent on hiring tax credits. If one were to expect a positive impact of enterprise zones on resident employment, it would be in these two states. I carefully measure zone location, control for the characteristics of people who lived in the zones in 1990, and systematically choose observationally similar non-zone tracts to use as comparison samples. In the end, I find that the enterprise zones in California and Florida had no measurable impact on the employment of residents. This provides further evidence that, at least at the historical level of expenditures and over a short horizon, enterprise zones were not an effective way of increasing the employment probability of people living in distressed communities.

## References


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## Note

1. The author is a research economist at the Bureau of Labor Statistics. This summary reports the results of research and analysis undertaken while the author was an employee of the Center for Economic Studies at the U.S. Census Bureau. It has undergone a Census Bureau review more limited in scope than that given to official Census Bureau publications. Results have been screened to insure that no confidential information is revealed. Research results and conclusions reported are those of the author and do not reflect the views or policies of the Census Bureau or the Bureau of Labor Statistics.