

**Government Programs Can Improve Local Labor Markets: Evidence
from State Enterprise Zones, Federal Empowerment Zones and Federal
Enterprise Communities¹**

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ABSTRACT

Federal and state governments spend well over a billion dollars a year on programs that encourage employment development in disadvantaged labor markets through the use of subsidies and tax credits. In this paper we use an estimation approach that is valid under relatively weak assumptions to measure the impact of State Enterprise Zones (ENTZs), Federal Empowerment Zones (EMPZs), and Federal Enterprise Community (ENTC) programs on local labor markets. We find that all three programs have positive, statistically significant, impacts on local labor markets in terms of the unemployment rate, the poverty rate, the fraction with wage and salary income, and employment. Further, the effects of EMPZ and ENTC designation are considerably larger than the impact of ENTZ designation. We find that our estimates are robust to allowing for a regression to the mean effect. We also find that there are positive, but statistically insignificant, spillover effects to neighboring Census tracts of each of these programs. Thus our positive estimates of these program impacts do not simply represent a transfer from the nearest non-treated Census tract to the treated Census tract.

Our results are noteworthy for several reasons. First, our study is the first to jointly look at these three programs, thus allowing policy makers to compare the impacts of these programs. Second, our paper, along with a concurrent study by Neumark and Kolko (2008), is the first to carry out the estimation accounting for overlap between the programs. Third, our estimation strategy is valid under weaker assumptions than those made in many previous studies; we consider three comparison groups and let the data determine the appropriate group. Fourth, in spite of our conservative estimation strategy, by looking at national effects with disaggregated data, we show that ENTZ designation generally has a positive effect on the local labor market, while most previous research on ENTZs, much of which used more geographically aggregated data to look at state-specific effects, did not find any significant impacts. Fifth, we note that there is little or no previous work on ENTCs. Overall, our results strongly support the efficacy of these labor market interventions.

1. Introduction

Governments often intervene in an attempt to improve the labor market conditions of disadvantaged areas. One example of this intervention is state Enterprise Zones (ENTZs). States have been creating these zones in distressed areas since the 1980s, although the programs differ widely across states. Enterprise Zone programs often involve substantial expenditures -- for example California reports an estimate of \$290 million in tax credits in 2008 for such activities in economically depressed areas.² Further, the Federal government introduced its Empowerment Zone (EMPZ) and Enterprise Community (ENTC) programs in the mid 1990s; again these were aimed at improving conditions in disadvantaged neighborhoods.³ The resources involved in these federal programs are quite substantial too, as it is estimated that the EMPZ and ENTC programs had a combined cost of \$1.21 billion in 2006.⁴ In this paper we use a common methodology to evaluate the labor market impact of each of these programs.

There is substantial interest in the efficacy of these programs, both because of the resources involved, and because they offer an alternative to programs aimed at low -income labor markets such as Job Corps, which are estimated to have had modest success at best (LaLonde, 1995). Of course, the crucial issue in the evaluation of ENTZ, EMPZ and ENTC programs is the need to assess how the affected labor markets would have performed in the absence of these programs; i.e. one must construct the appropriate counter-factual. However, this is difficult for at least two reasons. First, the areas affected tend to be among the poorest areas, and so it can be challenging to find appropriate comparison areas.⁵ Second, one faces a tradeoff between the level of geographic aggregation and the frequency of data collection. Labor market data is freely available annually for counties or Zip codes, but an ENTZ often only covers a small portion of a county or Zip code, which makes defining impacts problematic. This suggests the need to work at a finer level of geographical aggregation, which in turn generally requires using Census data.⁶

Much of the literature suggests that ENTZ designation does *not* have a positive impact on the affected labor market. While Papke (1994) finds a positive impact of ENTZs in Indiana when she looks at labor markets at the level of an unemployment insurance office, she could not find a positive

² See the California Legislative Analyst's Report at http://www.lao.ca.gov/handouts/Econ/2008/Tax_Expend_04_07_08.pdf.

³ Our analysis ignores a third Federal program, Renewal Communities, that were established after 2000 and thus are outside of the scope of our study

⁴ *Projected Tax Expenditures Budget, 2004-2010*. Tax Policy Center, 2004.

⁵ This is also true of participants in many manpower training programs, and twenty years after LaLonde's (1986) seminal paper, there is still substantial debate on the efficacy of nonexperimental evaluation of such programs.

⁶ As noted below, Neumark and Kolko (2008) provide a method for measuring employment (one of the five labor market measures we analyze) at the ENTZ level on an annual basis, albeit with potentially serious measurement error.

impact on labor markets using Census block data in her 1993 paper. Further, Bondonio and Greenbaum (2005, 2007), Engberg, and Greenbaum (1999) and Greenbaum and Engberg (2000, 2004) use Zip code data on state-specific ENTZ programs and find little or no positive labor market effects.⁷ Interestingly, in a paper written concurrently with an earlier draft of this paper, Neumark and Kolko (2008) use firm level data on employment (available in interval form) to study the impact of ENTZs in California on employment, but find no significant effect.⁸

Two papers on EMPZs introduced in the mid-1990s, by Oakley and Tsao (2006) and Busso and Kline (2007) draw opposite conclusions from their research, in spite of the fact that both studies use propensity score matching and Census tract data. Specifically, Oakley and Tsao find no significant effect of EMPZ designation, while Busso and Kline find, as we do, a significantly positive effect of EMPZs on local labor markets. However we argue below that there may be an identification issue that significantly reduces the appropriateness of using propensity score matching here, since it requires relatively precise estimates of a propensity score specification rich enough to achieve the Conditional Independence Assumption, but their estimation is based only on the eight urban EMPZs introduced in 1994.

In this paper we extend the literature on these important programs in several ways. First, we evaluate the impacts of all three programs: ENTZ designation, as well as EMPZ designation and ENTC designation in the mid 1990s, using a common methodology and level of geographical aggregation, which greatly aids comparing the effects of the programs. Second, we account for the fact that there is overlap between ENTZs and EMPZs, and between ENTZs and ENTCs, by estimating the model with and without the tracts involved in two programs. Note that one would expect that analyzing one program in isolation would lead to biased estimates of its effect if all three programs have positive effects, as we expect to be the case. Third, we avoid problems of geographic aggregation by using data at the Census tract level.

Fourth, when measuring the effects of ENTZ impacts we estimate an average effect at the national level, as well as state specific estimates of the impacts of the individual state ENTZ programs. We consider the average national effect because estimated state specific effects from previous research often had wide confidence intervals, and thus the test of the null hypothesis that the state specific impact of ENTZ designation is zero often has little power. An average national effect has a well defined interpretation and allows us to obtain much more precise estimates.

⁷ Engberg and Greenbaum (1999) found in a national study on moderate/small cities that enterprise zones helped distressed cities as long as they were not severely depressed. Some of these papers use data on enterprises and find disaggregated effects – see the discussion below.

⁸ As noted below, we also find that ENTZ designation in California has no significant effect on employment, but we do find that it improves local labor markets by having a significant effect of the unemployment rate, the poverty rate and the fraction of individuals with wage and salary income.

Fifth, by using data from all the 1980, 1990 and 2000 Censuses, we are able to use a quite flexible estimation strategy. Consider the case of measuring the impact of being designated as an ENTZ. Any program evaluation of the ENTZ program will use tracts that are not ENTZs (NENTZs) at the time of ENTZ assignment to answer the counter-factual of what would have happened to the ENTZs in the absence of the program. The most conservative (flexible) of our estimators takes the average difference between i) the double difference of the outcome measure at the Census tract level for the ENTZ⁹ and ii) the double difference of the outcome variable for the nearest NENTZ Census tract in the same state. We then consider a less flexible estimator which compares the average double difference between the outcome variable for an affected Census tract and the average in the outcome variable for the contiguous NENTZs in the same state.¹⁰ Finally, our least flexible estimator is the random growth estimator of Heckman and Hotz (1989) used in several previous studies, where we essentially compare double differences in all of the affected Census tracts to the double differences in all of the NENTZ tracts in a state. We then test the less flexible models against the more flexible models using tests from Hausman (1978). We consistently find significant (and substantial) beneficial (in the sense of improving the labor market) national average ENTZ effects on the unemployment rate, the poverty rate, average wage and salary income for those with positive earnings, and employment; we do not find a significant effect of ENTZ designation on the fraction of households with wage and salary income. These results stand in sharp contrast to the standard finding of ‘zero’ ENTZ effects, although the latter are for individual states. Interestingly, with our approach we often find significant state-specific beneficial ENTZ effects.

Since the EMPZ and ENTC programs are Federal programs, we only estimate average national effects for these programs.¹¹ We again use the three estimation methods and model selection approach described above. We find significant and substantial effects of the EMPZ and ENTC programs that generally are larger in absolute value than the average national effects of the state ENTZs.

We find that our estimates are robust to using an instrumental variable approach that avoids bias in the estimated treatment effect arising from the treated Census tracts exhibiting a regression to the mean phenomenon. To measure potential spillovers, we apply our approach to estimate treatment effects for the nearest NENTZs, NEMPZs, and NENTCs. We find that there are positive, but statistically insignificant, spillover effects to neighboring Census tracts of each of these programs. Thus our positive estimates of these program impacts do not simply represent a transfer the nearest

⁹ Let Y_{i2000} represent the outcome of interest in 2000. Then we define the double difference as $DD = (Y_{i2000} - Y_{i1990}) - (Y_{i1990} - Y_{i1980})$.

¹⁰ We construct the nearest and contiguous NENTZs based on the distance between the centroids (geographic center) of tracts surrounding each ENTZ.

¹¹ Note, however, that the programs are not uniformly implemented across states – see Oakley and Tsao (2006).

non-treated Census tract to the treated Census tract; indeed our estimates are conservative in the sense that they do not incorporate these positive (but statistically insignificant) spillover effects.

The outline of the paper is as follows: In Section 2.1 we describe the state ENTZ programs, while in Section 2.2 we give a brief overview of the Federal EMPZ and ENTC programs. In Section 3 we describe our econometric approach and compare it to previous approaches. In Section 4 we describe our data. In Section 5 we present our summary statistics, test results and estimates of the impact of each program. Section 6 concludes the paper.

2. A Brief Description of Enterprise Zones, Empowerment Zones, and Enterprise Communities

2.1 Enterprise Zones (ENTZs)

Connecticut created the first Enterprise Zone program in 1982, and a number of states quickly followed suit. By 2008, 40 states had ENTZ-type programs. Although the tax benefits and business qualifications vary across states, the common themes are: i) areas selected as zones typically lag behind the rest of the state in economic development; and ii) generally increased hiring of the local labor force is required. The number of such zones per state, and the geographic areas they cover, vary widely. For example, Ohio (as of 2008) had 482 zones, many of them smaller than a Census tract. In contrast, California's state constitution limits it to 42 zones, but some of the zones cover the majority of a particular city (such as San Francisco). Within a state, any local area's decision to participate in a state's ENTZ program is voluntary, but the area must also be approved by the state.

Tax benefits can be in the form of income tax, property tax, and/or sales tax benefits. Some states offer mostly property tax breaks, while others feature sales tax benefits (e.g. New Jersey exempts purchases made in urban ENTZs from sales tax), and a number of other states offer combinations of all three tax breaks (New York's Empire Zone program, and Pennsylvania's Keystone Opportunity Zone program, for example). Even for states which offer only income tax benefits, the magnitudes vary widely.¹² There is also wide variation in industry exclusions. Finally, some states require pre-qualification by the state for a firm to participate in an ENTZ program (i.e. approval must be obtained before breaking ground or moving into the ENTZ).¹³ It should be noted that these tax benefits can represent substantial expenditure (i.e. foregone tax revenue); as noted

¹² See Swenson (2010) for a detailed overview of the different programs by state. This paper is available at <http://www.marshall.usc.edu/leventhal/research/working-papers.htm>.

¹³ There are no "anti-churning" rules in any state. "Anti-churning" rules prevent an employer from firing a worker after receiving a credit, then hiring another employee in an attempt to get additional credits. However, many states obviate this problem by allowing credits for new employees only if total employment (or "headcount") at that firm also increases.

above, California reports an estimate of \$290 million in tax credits in 2008 for activities in economically depressed areas, while New York State, with a somewhat less generous but still substantial program, reports spending \$45 million in 2008 on its ENTZ programs.¹⁴

We restrict our analysis to estimating the impacts of ENTZs created during the 1990s.¹⁵ Thus we eliminate states where all zones were created in the 1980s: Alabama, Delaware, Indiana, Iowa, Kentucky, Louisiana, and Oklahoma. We also eliminated individual ENTZs not created in the 1990s for the other states. Similarly, we exclude ENTZs created after 2000 since we do not have 2010 Census data to obtain post-treatment outcomes. The latter include all ENTZs for Texas (created in 2001), all Keystone Opportunity Zones for Pennsylvania (created in 2002), Maine's Pine Tree Development Zones (created in 2004), and New Hampshire's CROP zones (created in 2005). Next, we eliminated "tier" states, where the entire state is an ENTZ. These states include Arkansas, Georgia, Mississippi, North Carolina, and South Carolina. Finally, we eliminated North Dakota (only 2 small Renaissance Zones), and Washington State (very tiny sales tax benefits given by county, where the qualifying counties vary every year). Finally we exclude Utah, Connecticut, Missouri and Maryland since we had less than ten observations on ENTZs for each of these states.

This left us with thirteen states in which to study ENTZs. Some states had enough Census tracts that belong to ENTZs that we could also analyze state-specific effects of ENTZ designation: California (99); Florida (66); Massachusetts (563); New York (116); Ohio (230) and Oregon (62).¹⁶ We collapsed the following states into an 'other states' category when considering state average effects: Colorado (14); Hawaii (10); Illinois (13); Nebraska (19); Rhode Island (31); Virginia (35); and Wisconsin (29).¹⁷ These states offer a rich variation in benefits and requirements for qualification, and since we are focusing on labor market effects, variations in tax benefits for hiring may be particularly important. One of the most generous states is California, which in the 1990s offered up to \$35,000 per employee hired in an ENTZ area, given over a five year period. Florida's and Wisconsin's support are also substantial, as they offer hiring credits of up to 30% and 15.8% of new payroll, respectively. Hawaii provides overall credits that are based on increased employment so long as other tests are met. (A general credit equal to 100% of the total Hawaii income tax paid by the

¹⁴ See <http://publications.budget.state.ny.us/eBudget0809/fy0809ter/taxExpenditure.pdf> for the NY state figure. Unfortunately most other states do not report a tax expenditures budget, and thus the expenditure magnitudes are not known for these states.

¹⁵ To analyze the ENTZs introduced in the 1980s we would need to use 1970 Census data, but as we note below, this data is not comparable to Census data from 1980-2000.

¹⁶ We exclude the California Targeted Employment Areas (TEAs) that are not in an ENTZ, EMPC or ENTC from our analysis. The TEAs not in ENTZs consist of census tracts of largely residential areas contiguous to an associated ENTZ. To qualify for hiring credits, a firm in an ENTZ must hire individuals meeting one of thirteen criteria, including one where the employee is a resident of a TEA.

¹⁷ These are the maximum number of zones we use. Missing data is more prevalent for some outcomes than others, and thus we have less data for these outcomes.

business in the ENTZ is given in the first year.) New York offers a \$3000 per new employee credit, and has other credits that are tied to increased employment. Benefits in several other states are as follows: Arizona (\$1500 per new employee); Colorado (up to \$2000 per new employee); Ohio (\$300 per new employee); Illinois (\$500 per new employee); Nebraska (up to \$4500 per new employee); Rhode Island (\$5000 per new employee); and Virginia (\$1000 per new employee). Finally, Oregon offers no hiring tax incentives, but does offer property tax incentives. In terms of timing, in January 2000 the median number of months that an ENTZ had been in existence in a given state are: California (90); Florida (54); Massachusetts (81); New York (66); Ohio (84); Oregon (78) and Other States (102).

2.2 Empowerment Zones (EMPZs) and Enterprise Communities (ENTCs)

Starting in the 1990s, the Federal government designated its own special tax zones in the form of EMPZs and ENTCs. They were established in two phases. In Round 1 in 1994, the government established 11 EMPZs, and 66 Enterprise Communities.¹⁸ In Round 2 in 1999 they designated 20 EMPZs and 20 ENTCs. Since our data will range between 1980 and 2000, we focus on evaluation of Round 1 zones. Our summary statistics in Section 5 below show that EMPZs are more disadvantaged than ENTCs, which in turn tend to be more disadvantaged than ENTZs. For example, in 1990 the average unemployment rates (poverty rates) were: ENTZs 9.2% (26.3%); ENTCs 15% (55.6%); and EMPZs 23.5% (61.3%).

The most prevalent incentives given in these federal programs are hiring tax credits (on firms' federal income tax returns) for hiring residents of the Zones. Both ENTCs and EMPZs provide employers a work opportunity tax credit of up to \$2400 for hiring 18-24 year olds who live in the areas. They also allow states to issue tax exempt bonds to finance certain investments in these areas. In addition, EMPZs have a credit of \$3,000 per EMPZ resident per year, and also have increased Sec. 179 expensing.¹⁹ In contrast, ENTCs do not feature the latter two tax benefits enjoyed by EMPZs. As noted above, the annual cost of these programs combined was estimated to be \$1.21 billion in 2006.²⁰ Since the programs have different features, we separately analyze EMPZs and ENTCs.

¹⁸ We analyze the effect of the eight urban EMPZs and the three rural EMPZs jointly, while Busso and Kline (2007) consider only the urban zones.

¹⁹ Section 179 expensing is a provision which allows a firm to write off (a portion of) the cost of assets in the year of acquisition, rather than depreciating them over a longer period.

²⁰ Tax Expenditures Budget, 2004-2010. Tax Policy Center, 2004.

3. Econometric Approach

3.1. Overview

In this section we describe our econometric approach for ENTZs, since our approach for EMPZs and ENTCs is essentially the same (except that we do not estimate state-specific effects for these two Federal Programs). As noted above, we estimate the labor market impact of being designated as a state ENTZ during the 1990s. We consider the effects of being designated an ENTZ at the Census tract level, where a tract is considered to be in an ENTZ if fifty per cent or more of it is covered by the ENTZ; this is a much lower level of aggregation than has been considered in most previous studies, which have focused on the county or Zip code level.²¹ To compare the two approaches, consider first Figure 1a for the Los Angeles ENTZ; the ENTZ covers several Zip codes, but only a relatively small fraction of each Zip code is in the ENTZ. Next, consider Figure 1b, where we now show the Census Tracts in and near the Los Angeles ENTZ; it is clear that one can more closely capture the ENTZ by working at a lower level of geographic aggregation.

Readers may be concerned that using Census tract data will artificially increase the precision of our estimates since there may be substantial correlation across tracts; however we address this issue by allowing for within-county correlation in our estimation procedures and/or calculation of the standard errors. As noted above, the major cost of using Census tracts is that we can only use data from Census years. Further, we chose not to use 1970 data for two reasons. First, matching Census tracts from 1980 and 1970 is a difficult and somewhat imprecise task. Secondly, the definition of the labor force changed between 1970 (individuals aged 14 and above) and 1980 (individuals aged 16 and above). The upshot is that we only use data from 1980, 1990 and 2000.

Specifically we consider both i) the average *national* effect of ENTZ designation on a Census tract and ii) the average effect *by state*; again most previous work has looked at average effects at the state level. As is well known from the random coefficients literature (e.g. Hsiao 2003), coefficients measuring national and state average effects have well defined interpretations that are clearly different.²² However they are also likely to be estimated with different degrees of precision. At the national level we are estimating a (weighted) average of state effects, which will be much more precisely estimated than the individual state effects. As a result, one has much more power when testing the standard null hypothesis that being designated an ENTZ has no effect. To look at this another way, many (but not all) studies at the state level have failed to reject this null hypothesis, but the confidence intervals around the estimated ENTZ effects are often quite large. Given this, one does not know whether one fails to reject the null hypothesis of no ENTZ effect because it really is

²¹ As noted above Papke (1993) uses Census blocks, which are smaller than Census tracts, while Neumark and Kolko (2008) aggregate firm level data. We first used tract data, and the nearest NENTZ, to evaluate ENTZ designation in Imrohoroglu and Swenson (2006).

²² Note that we are not claiming that ENTZ impacts are constant across states.

zero, or because these tests have little power. Estimating an average national effect significantly reduces this problem.

We consider three different estimators for these ENTZ effects at the national and state level. We start with a conservative version of difference in difference in difference (hereafter DDD) estimation. In this specification we allow for Census tract heterogeneity at the level of quadratic and higher trends, and assume that the coefficients on quadratic and higher order trends for an ENTZ are shared with only the nearest NENTZ Census tract in the same state. We then consider a slightly more restrictive DDD estimator where the coefficients on quadratic and higher order terms are shared between the ENTZ and all of the NENTZs in the same state that are contiguous to the ENTZ. Finally we consider the significantly more restrictive assumption made in the Heckman and Hotz (1989) random growth model, that all ENTZs and NENTZs within a state share the same quadratic and higher order trends. We assess the validity of the two latter (stronger) assumptions for each labor market outcome using Hausman (1978) tests. Finally we use ENTZs, EMPZs, and ENTCS that are affected by only one of the programs, although the results did change much for any program when we ignored this overlap.²³

3.2 Our Base Specification; Using the Nearest NENTZ as a Comparison for an ENTZ

3.2.1 Estimating an Average National Effect

Consider a pair j consisting of an ENTZ Census tract i and its nearest (in the same state) NENTZ tract i' ; in what follows we use the notation $i, i' \in j$. Our maintained assumption throughout what follows is that while i and i' share the same coefficients η_{jl} on quadratic and higher order trends, they are allowed to have tract-specific fixed effects α_k and linear trends γ_k . The labor market outcome of interest in tract k ($k = i, i'$) in year t ($t=1980, 1990, 2000$), W_{kt} , is determined by

$$W_{kt} = X_{kt}\beta + \delta EZ_{kt} + \alpha_k + \gamma_k T_t + \sum_{l=2}^L \eta_{jl} (T_t)^l + \varepsilon_{kt}. \quad (1)$$

In (1) X_{kt} is a vector of pre-treatment explanatory variables, EZ_{kt} equals 1 if $t=2000$ and $k=i$ and zero otherwise, and T_t denotes time. We have exploited the fact that i and i' share the same second and higher order trends so that η_{jl} captures the effect of second and higher order time trends on outcome variable, W_{kt} , for both an ENTZ Census tract i and the nearest NENTZ tract i' in the same state.

²³ We also exclude ENTZs and NENTZs that overlap with the EMPZs and ENTZs introduced in 1999. Our results where we do not exclude overlapping tracts are contained in our Online Appendix B available at <http://www.marshall.usc.edu/leventhal/research/working-papers.htm>.

Next we take the double differences for $k=i, i'$ respectively

$$Z_k = [(W_{k2000} - W_{k1990}) - (W_{k1990} - W_{k1980})] = [(X_{k2000} - 2X_{k1990} + X_{k1980})]\beta + \delta EZ_{k2000} + \sum_{l=2}^r \eta_{jl} [(T_{2000})^l - 2(T_{1990})^l + (T_{1980})^l] + (\varepsilon_{k2000} - 2\varepsilon_{k1990} + \varepsilon_{k1980}). \quad (2)$$

Note the tract specific intercepts α_k , and the tract specific trends $\gamma_k, k (k=i, i')$, drop out of (2).

Finally, we assume that²⁴

$$[(X_{i2000} - 2X_{i1990} + X_{i1980})] = [(X_{i'2000} - 2X_{i'1990} + X_{i'1980})] = \phi_j \text{ for } i, i' \in j, \quad (3)$$

i.e., tracts i and i' share the same double difference in the X variables.²⁵ Taking the triple difference yields the DDD estimator.

$$Y_j = Z_i - Z_{i'} = \delta EZ_{i2000} + e_j, \quad (4)$$

where $e_j = (\varepsilon_{i2000} - 2\varepsilon_{i1990} + \varepsilon_{i1980}) - (\varepsilon_{i'2000} - 2\varepsilon_{i'1990} + \varepsilon_{i'1980})$.²⁶ We allow the e_j to be correlated within the same county.²⁷

3.2.2 Estimating State-Specific Average Effects

We can allow treatment effect to differ by states. In this case we write

$$\delta = \sum_{s=1}^S \delta_s D_{si}, \quad (5)$$

where $D_{si}=1$ if i is in state s and 0 otherwise. We would expect these effects to differ due to differences in the state programs and the state economies. Given (5) we would then estimate

$$Y_j = \sum_{s=1}^S \delta_s D_{si} EZ_{i2000} + e_j. \quad (6)$$

In (6) the δ_s terms are the state-specific treatment effects; note that we would obtain essentially the same estimates if we ran state-specific regressions.²⁸ As noted above estimation of (6) has the advantage that it provides estimates for the effects of the individual state programs, but has the

²⁴ Of course this is a sufficient condition for consistent estimates of the treatment effects, since we really only need the sum of quadratic and higher order trends and the double difference to be equal for i and i' . Since there is no reason to think that this necessary condition would hold if the sufficient condition did not, we ignore this weaker condition in the remainder of the paper.

²⁵ Note that this assumption would be considerably less tenable if i and i' are not in the same state.

²⁶ Following Papke (1993), we attempted to let the impact of ENTZ designation depend on the length of time the tract had been an ENTZ. However, we generally could not reject the null hypothesis that the impact did not depend on time, although this generally reflected that our estimates of this extended model were quite imprecise.

²⁷ If i and i' are in different counties we use the county for i .

²⁸ The only caveat to this is that in joint RE estimation, we would assume that correlation across counties was not state-specific.

disadvantage that confidence intervals for these effects may be quite large and relatively uninformative.

3.3 A More Restrictive, but Potentially More Efficient, Estimator

The approach in Section 3.2 only requires that an ENTZ and the nearest NENTZ share the same quadratic (and higher order) trends, as well as the same double differences in the explanatory variables. This is a conservative strategy that could lead to large standard errors, especially when estimating state average effects. Given this, we next consider estimates based on a (slightly) stronger assumption that quadratic and higher order trends, as well as double differences in the explanatory variables, are *on average*, the same between the ENTZ and the contiguous NENTZs. In fact, Table 1 below shows that the contiguous NENTZs are more prosperous in every period than the ENTZs, so in fact we would not expect less prosperous contiguous NENTZs to average out more prosperous contiguous NENTZs, and thus this assumption is essentially equivalent to the ENTZs and NENTZs having the same trends.²⁹ Below we will test whether it is consistent with our data.

Define the set S'_i consisting of the NENTZs contiguous to i , and assume without loss of generality that S'_i contains I'_i elements. Now assume that the Census tracts in S'_i and the ENTZ Census tract i share the same coefficients on the tract specific quadratic and higher order trends and the same double difference in the explanatory variables. Next, let

$$\bar{Z}_i = \sum_{m \in S'_i} / I'_i, \quad \text{where} \quad (8)$$

$$Z_m = [(W_{m2000} - W_{m1990}) - (W_{m1990} - W_{m1980})] = [(X_{m2000} - 2X_{m1990} + X_{m1980})]\beta + \sum_{l=2}^{\tau} \eta_{jl} [(T_{2000})^l - 2(T_{1990})^l + (T_{1980})^l] + (\varepsilon_{m2000} - 2\varepsilon_{m1990} + \varepsilon_{m1980}). \quad (9)$$

The DDD estimator is now

$$Y_j = Z_i - \bar{Z}_i = \delta EZ_{2000i} + v_j. \quad (10)$$

To obtain a test of whether the data is consistent with the more restrictive model (10), denote the estimators of δ based on (4) and (10) by $\hat{\delta}$ and $\tilde{\delta}$ respectively. If (10) is valid, $\tilde{\delta}$ and $\hat{\delta}$ will be consistent, but $\tilde{\delta}$ will be more efficient. On the other hand, if only (4) is valid, $\tilde{\delta}$ will be inconsistent while $\hat{\delta}$ will still be consistent. Thus we can use Hausman (1978) to test the null

²⁹A referee noted that i) the nearest or contiguous NENTZs may be more residential than the ENTZs and ii) in states for which ENTZs tend to be large, the nearest or contiguous NENTZs may be further away from the ENTZs than in states where the ENTZs are relatively small. In either case we would expect the congruent or contiguous NENTZ tracts to have different fixed effects and trends than ENTZ tracts. However, note that our model allows NENTZ tracts to have different fixed effects and linear trends than ENTZs, mitigating this issue.

hypothesis that (10) is an appropriate specification.³⁰ The extension to the case where we estimate state-specific treatment effects is straight-forward; here we use a joint test on the state treatment effects rather than testing the state treatment estimates one by one.

3.4 The Heckman-Hotz Random Growth Model

Finally we consider the assumption introduced in Heckman and Hotz (1989) and used in much previous research using double difference estimators: all NENTZs and ENTZs in the same state share the same quadratic, higher order trends and the double difference in the explanatory variables.³¹ Under this assumption, we obtain our DDD estimator of the average national effect by running the regression

$$Z_i = \sum_s \varphi_s D_{is} + \delta EZ_{2000k} + \varepsilon_i$$

$$Z_{i'} = \sum_s \varphi_s D_{i's} + \varepsilon_{i'} \tag{11}$$

for all ENTZs i and NENTZs i' in the same state

We can again test this assumption using a Hausman test, comparing: i) the estimates from (11) to those from (4) or ii) the estimates from (11) to those from (10). This ability to test our models is important given that data limitations prevent us from carrying out a natural diagnostic. Following Imbens and Wooldridge (2008) and the previous literature, a natural test of our model would be to calculate the DDD between ENTZs (designated in the 1990s) and their nearest NENTZs over the period 1990-1970. Given that the treatment did not take place until after 1990, any significant ‘treatment’ effect under our (weakest) assumption that the ENTZ and the nearest NENTZ share quadratic and higher order trends and double difference in the explanatory variables would imply that this assumption is invalid. Unfortunately as noted earlier, using 1970 Census tract labor market data is problematic since matching Census tracts from 1980 and 1970 is an imprecise task. In addition, the definition of the labor force changed between 1970 (individuals aged 14 and above) and 1980 (individuals aged 16 and above). Thus we do not/cannot perform a specification test using the 1990-1970 DDD estimators.

³⁰ Note that even in the presence of heterogeneous treatment effects, the use of the Hausman test is valid since we are only changing the comparison group.

³¹ As in the case of the contiguous NENTZs, we really only need this be true on average. However Table 1 shows that the noncontiguous NENTZs are much more prosperous than the ENTZs, so that assuming that the averages are equal is basically equivalent to assuming equal trends between the ENTZs and all the NENTZs.

3.5 Issues that Arise in Using Hausman Tests in our Application

Earlier we raised the possibility that using the standard errors generated by least squares (OLS) may be misleading due to the fact that there are unobserved county specific effects in the error terms. A natural way of dealing with this problem is to use OLS and ‘cluster’ the standard errors by county, and we report the results of doing this using the nearest NENTZ as a control for our main specification for ENTZs, EMPZs and ENICs in Tables C1-C3 of our Online Appendix C.³² However OLS estimates for (10) or (11) are not efficient, so that one cannot use the simple form of the variance in the difference of the estimates from Hausman (1978). Instead we would have to construct the (complicated) variance-covariance matrix of the difference in the estimates using the appropriate formulae or the bootstrap. However, we can allow for these unobserved county effects and exploit the simplification from Hausman (1978) by using Random Effects (RE) estimation, where the random effect is at the county level.³³ Thus we use RE estimation to distinguish between the different assumptions and obtain our preferred estimates.

A second issue arises in the use of the Hausman tests in all applications: the estimated variance of the estimator that is efficient under the null hypothesis can be larger than the variance for the inefficient estimator in finite samples. In this case one again cannot use the simplification in Hausman (1978) when testing the equality of the estimates. Here there are two basic approaches one can take. First, one can construct the variance of the difference in the estimators using the appropriate formulae or the bootstrap. Alternatively, if one is willing to live with pre-test bias, one can simply reject the ‘more efficient’ estimator in this case, since the intuition behind the Hausman test is that the efficient estimator (under the null) should produce the ‘same’ coefficients but with smaller standard errors than the inefficient estimator. If the ‘efficient’ estimator produces a larger standard error, then the researcher is implicitly risking a chance of inconsistent estimates (if the null hypothesis is not valid) while not obtaining any benefit in terms of better precision in the estimate of the parameter of interest.³⁴ We use this second approach.

³² These tables are available at <http://www.marshall.usc.edu/leventhal/research/working-papers.htm> .

³³ Another possibility would be to use a GLS (or GMM) model where the covariance between tracts in the same county is a function of the distance between counties (Conley 1999). This could be considered as an intermediate position in between our RE models by county and our OLS estimates with clustered standard errors at the county level. Since the latter two estimation approaches produce very similar results, we do not pursue the Conley approach.

³⁴ Another issue is that the estimates based on all the NENTZs may have higher variance than the estimates based on the contiguous NENTZs since the contiguous NENTZs may be more homogeneous. In this case it would seem appropriate to go with the estimates based on the contiguous NENTZs since they are both more precise and based on weaker assumptions.

3.6 Regression Towards the Mean and Spillover Effects

3.6.1 IV Estimation to Allow for Regression Towards the Mean

To this point, in our most conservative approach we have assumed that ENTZs and the respective nearest NENTZs are comparable as long as we control for fixed effects and linear trends. However, it may be the case that treatment tracts are chosen on the basis of having a bad transitory shock in 1990, ε_{i1990} . (Recall that i denotes an ENTZ and i' denotes its nearest NENTZ.) In this case our estimates of the effect of ENTZ designation will be biased towards finding a positive (to the community), since we would expect ε_{k2000} to regress to its mean value of zero.³⁵

To see this more clearly, consider equation (2) when the outcome measure is the unemployment rate

$$Z_k = (W_{kt} - W_{kt-1}) - (W_{kt-1} - W_{kt-2}) = \delta EZ_{kt} + \theta_j + (\varepsilon_{kt} - 2\varepsilon_{kt-1} + \varepsilon_{kt-2}), \quad k = i, i' \quad (2)'$$

where effective treatment implies $\delta < 0$. If ENTZ designation depends on a bad (large positive) 1990 shock, then EZ_{k2000} will be positively correlated with ε_{k1990} , and negatively correlated with $-2\varepsilon_{k1990}$ in (2)', biasing $\hat{\delta}$ in a *negative* direction, i.e. the program effect will be overstated.

We address the problem of regression towards the mean by using an instrumental variables procedure. To describe this procedure, we assume for ease of exposition we have only two labor market outcomes of interest, $W_{1k\tau}$ and $W_{2k\tau}$ for Census tract k in year τ . Recall that we have for tract pair j ($i, i' \in j$)³⁶

$$W_{mk\tau} = X_{k\tau}\beta + \alpha_k + \gamma_k T_\tau + \sum_{l=2}^L \eta_{lj}(T_\tau)^l + \varepsilon_{mk\tau} \quad \text{for } k = i, i', m = 1, 2 \text{ and } \tau = t-2, t-1, \quad (12)$$

since the enterprise zones that we examine are formed in $\tau = t$ (between 1990 and 2000) and thus do not exist in $\tau = t-1$ and $\tau = t-2$, i.e. $EZ_{kt-2} = EZ_{kt-1} = 0$ for $k = i, i'$. On the other hand, in $\tau = t$ (2000) we have

$$W_{mk\tau} = \delta EZ_{kt} + X_{k\tau}\beta + \alpha_k + \gamma_k T_\tau + \sum_{l=2}^L \eta_{lj}(T_\tau)^l + \varepsilon_{mk\tau} \quad k = i, i', m = 1, 2, \quad (13)$$

³⁵ A similar problem, known as Ashenfelter's dip, occurs in the nonexperimental evaluation of many manpower training programs, since individuals tend to volunteer for training after experiencing a negative transitory shock. As a result, the estimated treatment effect is overstated simply because the trainees' transitory shocks in later periods will regress towards the mean. For example, in the JIPA training program, controls who have volunteered for training go from an essentially zero employment rate at the time that training is assigned to an employment rate of 0.3 eighteen months later without any receiving any intervention. Ignoring this phenomenon would bias the estimated (intent-to-treat on the treated) treatment effect from its experimental estimate of 0.1 to 0.4. (See, e.g., Figure 1 in Eberwein, Ham and LaLonde 1997.)

³⁶ In other words, i and i' are an ENTZ in 2000 and the nearest NENTZ to it in 2000 respectively.

where the enterprise zone dummy in year $\tau = t$ is defined as $EZ_{kt} = 1$ for $k = i$ and $EZ_{kt} = 0$ for $k = i'$. Using (3), (12), (13) and double differencing, our estimating equation is

$$Z_{mk} = (W_{mkt} - W_{mkt-1}) - (W_{mkt-1} - W_{mkt-2}) = \delta EZ_{kt} + \theta_j + (\varepsilon_{mkt} - 2\varepsilon_{mkt-1} + \varepsilon_{mkt-2}), \quad m = 1, 2 \text{ and } k = i, i'. \quad (14)$$

Here we focus on IV estimation of (14) for the first outcome, $m = 1$. Again, for ease of exposition we describe our first stage equation as

$$EZ_{kt} = \varphi W_{2kt-2} + u_{kt}, \quad k = i, i'. \quad (15)$$

In other words we use the $t-2$ (1980) value of outcome 2, W_{2kt-2} , as the instrument for EZ_{kt} ($k = i, i'$) when analyzing outcome 1.³⁷

For W_{2kt-2} to be a valid IV instrument for use in (14) we need³⁸

$$i) \quad \text{cov}(W_{2kt-2}, EZ_{kt}) \neq 0 \quad \text{and} \quad (16a)$$

$$ii) \quad \text{cov}(W_{2kt-2}, [\varepsilon_{1kt} - 2\varepsilon_{1kt-1} + \varepsilon_{1kt-2}]) = 0. \quad (16b)$$

Condition *i)* is simply a non-weak IV assumption which can be assessed by examining the first stage equation, but condition *ii)* is an exactly identifying assumption which, of course, cannot be tested.³⁹

To investigate the conditions under which *ii)* holds, for simplicity assume that ε_{mkt} is uncorrelated over time.⁴⁰ Given this assumption *ii)* reduces to

$$\text{cov}(W_{2kt-2}, \varepsilon_{1kt-2}) = 0. \quad (17)$$

Using (12) to substitute for W_{2kt-2} in (17), we need

$$\text{cov}([\mathbf{X}_{kt-2}\boldsymbol{\beta} + \alpha_k + \gamma_k \mathbf{T}_{t-2} + \sum_{l=2}^L \eta_{lj} (\mathbf{T}_{t-2})^l + \varepsilon_{2kt-2}], \varepsilon_{1kt-2}) = 0. \quad (18)$$

We believe it is reasonable to assume

$$\text{cov}([\mathbf{X}_{kt-2}\boldsymbol{\beta} + \alpha_k + \gamma_k \mathbf{T}_{t-2} + \sum_{l=2}^L \eta_{lj} (\mathbf{T}_{t-2})^l], \varepsilon_{1kt-2}) = 0. \quad (19)$$

In this case (17) will hold if (and only if)

$$\text{cov}(\varepsilon_{2kt-2}, \varepsilon_{1kt-2}) = 0. \quad (20)$$

³⁷ In fact in our empirical work we actually use $W_{2kt-2}, \dots, W_{5kt-2}$ as IV for EZ_{kt} in the equation for outcome 1, but considering this more complicated case does not add anything to our discussion.

³⁸ See Wooldridge (2002, section 5.1)

³⁹ Our analysis will not change if there is a structural equation determining EZ status of the form $EZ_{kt} = \tilde{\varphi}_1 W_{2kt-1} + \tilde{\varphi}_2 W_{2kt-2} + u_{kt}$, $k = i, i'$, or if EZ status depends on the $(t-1)$ and $(t-2)$ values of many outcome variables, as long as (16a) and (16b) hold, since in IV estimation we are not concerned with consistent estimation of the reduced form equation.

⁴⁰ The assumption that ε_{mkt} is uncorrelated over time makes things easier and will produce the largest possible bias from regression to the mean if we allow ε_{mkt} to be an AR(1) with $\rho \geq 0$.

Thus we need the transitory shock to outcome 2 in 1980 to be orthogonal to the transitory shock in outcome 1 in 1980 (after allowing for a tract specific intercept, a tract specific trend, and pair specific higher order trends). While this is a non-trivial assumption, it is difficult to think of a non-weak instrument that is unrelated to economic conditions in (t-2), so there is always the chance that the (t-2) transitory shock in the instrument will be correlated with the (t-2) transitory shock in outcome 1.

We use a similar approach when allowing for this type of potential endogeneity when using the contiguous comparison group. Finally we use a standard IV approach on (11) when using the Heckman-Hotz model where all NENTZs in the state act as the comparison group, i.e. the pair fixed effect becomes a state dummy. Finally the extension to estimating state specific treatment effects is straight forward and omitted to save space.⁴¹

Of course, there is still the issue of which IV model we should choose. One possibility would be to repeat the model selection process for the IV estimates. However, in order to maximize comparability with the OLS estimates, we use the same comparison group for the respective IV regression that was chosen for the OLS estimates. Finally, there is the question of whether we should test for endogeneity using a Hausman test to compare the OLS and TSLS results. However, we suspect that there are heterogeneous treatment effects of ENTZ designation, especially across state borders because of the difference in the programs. In this case it is now well known that the IV estimates will estimate treatment effects for marginal ENTZs,⁴² i.e. those tracts whose ENTZ designation is sensitive to changes in \tilde{Y}_{k1980} , and the OLS and IV estimates will differ even in a properly specified model. Thus instead we simply ask whether the IV and OLS procedures produce *qualitatively* similar treatment effects.

3.6.2 Allowing for Spillover Effects

If there are positive spillovers from an ENTZ to the nearest NENTZ, then estimated treatment effects will be understated. However if the ENTZ ‘steals’ business from the nearest NENTZ; and in this case δ will be upward biased. Thus it is important to account for the possibility of spillovers in estimation. One possibility is to follow other authors and use as a comparison an NENTZ i'' in the same state that is further away from ENTZ i . We believe there are two problems with this approach. First, it requires that the ENTZ i and the (further away) NENTZ i'' share common quadratic and higher order trends, as well as the same double difference in the explanatory variables, which we argue is substantially less plausible than making this assumption for i and the

⁴¹ Note that allowing the program effect to depend on the 1990 (or 1980) value of the dependent variable would raise even more difficult identification and estimation problems.

⁴² See, e.g., Imbens and Wooldridge (2008, lecture 5) for an accessible discussion of local average treatment effects.

nearest NENTZ i' . Second, for the chosen NENTZ to be comparable to the ENTZ, it is likely to be in a relatively disadvantaged area and thus likely to experience spillovers from other ENTZs in the state. Another possible path for avoiding the problem of spillovers would be to choose a comparison NENTZ from a state without an ENTZ program. However, this would accentuate the first problem since now assuming common quadratic and higher order trends and double differences in the explanatory variables for an ENTZ in one state and NENTZ in another is much less plausible.

We believe a much more useful approach is to calculate treatment effects for the nearest NENTZ directly using the methodology described above, where the second nearest NENTZ is the most conservative comparison group for the nearest NENTZ.⁴³ If these spillovers are statistically significant, one could then test for spillovers in the second nearest NENTZ and if these are significant, go the third nearest NENTZ and so on, until one obtains a statistically insignificant spillover effect for the l th nearest NENTZ. If all of the spillovers in the preceding $l-1$ nearest NENTZ are positive, then one could obtain a conservative treatment effect by replacing the nearest NENTZ by the l th nearest NENTZ. If, on the other hand, the spillovers were found to be negative, one would need to net these out in calculating a treatment effect.⁴⁴ In fact we find that the spillovers are generally beneficial but insignificant, but for completeness we re-estimate the treatment effects for ENTZs with the second nearest NENTZ replacing the nearest NENTZ. (We carry out the analogous estimation described above for the EMPZs and ENTICs.)

3.7 Comparison of Our Econometric Approach to that used in Previous Work

3.7.1 Comparison to Previous Work Studying ENTZS

There is a large and growing literature on ENTZ programs, and here we focus on the important econometric issues without claiming to provide an exhaustive review.⁴⁵ Generally, previous studies used either a double difference approach like random growth framework (11) or propensity score matching based on the first difference of the outcome variable. As noted above we work at a level of aggregation lower than many previous papers, so here we generally focus on whether the assumptions in previous studies would be appropriate given our level of aggregation.

Two of the early papers in this area are by Papke. Papke (1994) examines the impact of ENTZs in Indiana on two types of capital and on unemployment insurance claims; we focus on her work on unemployment insurance claims since it is much more closely related to our empirical work

⁴³ Busso and Kline (2007) suggest estimating treatment effects for nearby NEMPZs as a placebo test for their model and a test for spillovers.

⁴⁴ Calculating such a net effect would not be difficult but calculating its standard error would be quite cumbersome using the appropriate regression formulae. Instead it would be substantially easier to use the bootstrap. Note that like any model selection procedure this approach could introduce pre-test bias.

⁴⁵ See also the excellent surveys in Papke (1993) and Engberg and Greenbaum (2004).

below. Papke uses a series of estimation strategies, where the most general one is a DDD random growth estimator for 46 unemployment insurance offices containing enterprise zones and 152 unemployment insurance offices that do not include an enterprise zone. She finds significant negative effects of ENTZ designation on unemployment insurance claims in Indiana, indicating that ENTZ designation has a positive effect on the labor market.

Papke (1993) looks at the effect of the implementation of ENTZs in Indiana between 1980 and 1990 on Census blocks (which are smaller than the tracts that we use). Using the blocks that were designated as ENTZs and a random sample of NENTZ blocks, she compares the first difference in unemployment, per capita income and the fraction with wage and salary income between 1980 and 1990 for the ENTZ blocks and the NENTZ blocks. As she notes, this estimator imposes stronger assumptions than Papke (1994), since it assumes that the *linear*, as well as quadratic and higher order trends, are shared by all ENTZs and NENTZs in the same state-- an assumption that is rejected in our data. Her results show little or no effect of ENTZ designation, in contrast to her results in Papke (1994).

Bondonio and Engberg (2000) use data at the Zip code level in California, Kentucky, New York, Pennsylvania and Virginia to examine the effect on employment of ENTZ designation over the period 1981-1994. The advantage of using Zip code data is that labor market data are available for every year, while the disadvantage is that a Zip code is designated as an ENTZ even if only a small part of it is actually an ENTZ. They use two approaches to estimating the impact of ENTZ designation. The first is the DDD approach of Papke (1994) with a version (11) above where they estimate separate effects for each state while not considering a national average effect. They find no effects on employment in any of these states. Their second approach is based on propensity score matching for the first difference in employment, where the propensity score is based on the characteristics used to designate an ENTZ.⁴⁶ Since they include in the propensity score the variables used to determine eligibility for being designated as an ENTZ, they argue that it is reasonable to invoke the Conditional Independence Assumption (CIA)/ Ignorable Treatment Assignment Assumption (ITAA) underlying matching. However, since there can be substantial costs of applying for designation, and political factors can affect whether an application is successful, other variables that affect whether an application is made, or approval conditional on application, also could affect employment growth.⁴⁷ In this case the CIA would be violated. Of course, every study will have to make an exactly identifying assumption, and their assumption seems at least as reasonable as most made in the matching literature. Again they do not find an effect of ENTZ designation.

⁴⁶ They focus on the estimation of being designated an ENTZ on employment growth in the ENTZs in the sample (i.e. the effect of treatment on the treated).

⁴⁷ One might be able to control for this possibility by conditioning on other lagged variables that are not used to determine eligibility for ENTZ designation.

Greenbaum and Engberg (2000) also use propensity score matching to measure the impact of ENTZ designation on housing and labor market outcomes using Zip code data from 1990 and 1980 for six states. They match on a number of labor market and production data from 1980 and 1981. They find very few program impacts on labor market variables for the states they consider.⁴⁸ Greenbaum and Engberg (2004) use the U.S. Bureau of the Census longitudinal research database on manufacturing establishments along with first difference matching at the Zip code level for six states. They consider the effect of ENTZ designation on employment, establishment, shipments and capital spending. Their use of this data allows them to consider the effect of ENTZ designation on firm births, as well as economic activity at new and existing firms. They find little overall effect of ENTZ designation but do find that such designation has positive effects on births and employment, payroll, and shipments in new establishments, but a negative effect on these variables in previously existing establishments. Interestingly they argue that propensity score matching does better than geographical matching in their data; however their result is not applicable to our approach, since they investigate first differences in outcome variables at the Zip code level, while we use DDD estimation at the Census tract level. Bondonio and Greenbaum (2007) also use establishment data and propensity score matching to examine the impact of ENTZ designation in four states on gross and net flows of new firms, existing firms, and vanishing firms at the Zip code level. They continue to find a zero overall impact of ENTZ designation that arises from significant positive impacts in some disaggregated measures and negative effects on others. Further, Elvery (2009) uses propensity score matching at the neighborhood data in conjunction with outcome data at the individual level to study the effects of ENTZs in Florida and California, and again finds no significant effects.

Lynch and Zax (2008) use establishment data for Census Blocks in 2000 and 1990 to look at the impact of ENTZs in Colorado. They discuss the issue of selection bias due to sorting, and argue that they can minimize this bias by omitting from their analysis all establishments that moved from an ENTZ to a non-zone location or from a non-zone location to an ENTZ between 1990 and 2000. This argument is in turn based on the assertion that establishment locations which were stable with respect to ENTZ membership over the period are more likely to be exogenous for the purposes here; however it is not clear, *a priori*, why stable firms are not a select sample.

Finally, in a paper written concurrently with an earlier draft of this paper, Neumark and Kolko (2008) use an interesting and complex process to construct *annual* employment data for each ENTZ in California. They look at employment growth (i.e. a first difference model in log employment) and their preferred comparison group, similar to that of Busso and Kline (2007) discussed below, consists of areas that have been designated in the past, or that will be designated in

⁴⁸ They also consider the effect of ENTZ on housing market variables, as do Engberg and Greenbaum (1999), using propensity score matching. Since our focus is on the labor market, we do not discuss these results.

the future, as ENTZs. However, these are not ideal comparisons, since they may be stronger or weaker than the tracts in the treatment group. For example, if government officials want the program to succeed they will reject the weaker tracts or defer their designation; this ‘creaming’ is widely thought to be a problem in the manpower training literature. Alternatively if authorities designate as ENTZs first those tracts that need help most, the comparison group will be stronger than the treatment group. Further, if ENTZ assignment is based on an area receiving a recent negative shock, their estimates will be subject to the ‘regression towards the mean’ problem that our instrumental variables estimates address below. Of course, since every study must make an identifying assumption, the crucial (and open) question is whether their assumption is more or less reasonable than that made in other studies. In comparison with our approach, theirs has the advantage of directly measuring employment by ENTZ while we measure tracts as ENTZs if over half their area is covered by an ENTZ; if half or less of the tract is covered by an ENTZ we delete the tract from our data, suggesting that we may understate the treatment effect. Secondly, they obtain annual data, while we only observe data in 1980, 1990 and 2000. On the other hand, a potential issue in Neumark and Kolko’s analysis is that employment by firm is available only in interval form, and this may introduce substantial measurement error into their analysis. Moreover, because they use a first difference model, they also must assume that their treatment and comparison groups share i) *common linear trends*, as well as ii) common quadratic and higher order trends, while we must only assume ii) and indeed find that i) does not hold in our data. Also, as noted above, their estimates may be affected by a ‘regression towards the mean’ problem, although we should note that we do not find that this problem biases our estimates below. Finally, they can only look at the effect of ENTZ designation on employment growth, while we can examine the effect on employment and four other labor market variables. This latter distinction is important since both our study and theirs finds insignificant, but imprecise, estimates of the employment effect in California – in other words, neither estimate is very informative. However, we find much more precise estimates for other labor market variables.

3.7.2 Comparison to Previous Work on the Effect of EMPZ and ENTC Designation

As noted above, Oakley and Tsao (2006) and Busso and Kline (2007) both use first difference in Census tract labor market data and propensity score matching to estimate the effect of being designated as an EMPZ in the first round of the program. However, Oakley and Tsao use 1990 and 1980 variables in the propensity score, while Busso and Kline use only 1990 variables. Interestingly, the former study finds no effect while the latter finds a substantial positive effect. It is beyond the scope of our paper to isolate which set of conditioning variables is more likely to achieve the CIA, although in general conditioning on both 1980 and 1990 variables would seem preferable. Here, we simply would note that when changes in specification lead to dramatically different results,

this is often an indication that the effect being measured is not well identified in the data. Such an identification problem could arise since the results are based on only eight EMPZs introduced in the mid 1990s, which may make it difficult to estimate precisely a rich enough propensity score to achieve the CIA. Of course, each EMPZ designation affects a number of zones, so there is clearly not a negative degrees-of-freedom problem here. On the other hand, the zones within an EMPZ may be highly correlated, so the empirical identification may be weaker than that suggested by the number of observations.⁴⁹ Finally, Busso and Kline run into a perfect prediction problem when they try to include population in the propensity score, which again can be indicative of the model not being well identified.⁵⁰ Note that our approach does not require us to estimate the probability of a tract being in an EMPZ and thus is unaffected by this problem.

Busso and Kline, in an attempt to avoid spillover effects, use comparisons in different cities not affected by EMPZ designation; however as noted above the use of this data will make the treatment and comparison groups less similar and thus make it harder to achieve the CIA. Finally, also as noted above we should note that Busso and Kline conduct tests based on placebo Census tracts. They use nearest neighbor matching within the city to find the ‘nearest’ NEMPZ to each EMPZ in a given city, and use this NEMPZ as a placebo tract. They then compare the placebo tracts to the comparison tracts (from other cities) used in their estimates for 2000 minus 1990 values, and find no placebo effect. To the best of our knowledge this approach is new to the literature, but there are some unresolved issues here. First, it is not obvious how to calculate standard errors for the placebo treatment effects when using this approach, since matching is essentially carried out twice and the bootstrap generally cannot be used for nearest neighbor matching (Abadie and Imbens 2006). Secondly, there is implicitly a Conditional Independence Assumption made concerning the differences between the placebo zones and the EMPZs, so one is essentially testing one CIA by invoking another. However, they also use this as a test for spillovers, and we adopt their test below, but because we calculate spillover effects for the nearest NENTZ, we do not encounter the statistical issues inherent in using nearest neighbor matching to choose the ‘treatment’ group.

The only studies of ENTCS that we are aware of are the HUD (2001) study based on a HUD survey of businesses located in the ENTCS and a GAO (2006) study. The HUD survey covered the first 5 years of the program, from 1995-2000, and found that businesses were indeed utilizing the benefits of being in an ENTCS. However, the study made no attempt to assess the economic impacts of the ENTCS designation.

⁴⁹ Busso and Kline do allow for this correlation in calculating standard errors.

⁵⁰ As a result, they must assume that one does not need to condition on population to achieve the CIA, which does not seem very reasonable *a priori*.

4. Data

4.1 Data for the Analysis of ENTZs

Our data, based on 2000 Census tract definitions, consists of tracts that were designated as i) an ENTZ in the 1990s but not as an EMPZ or ENTC in either the mid 1990s or 1999, resulting in approximately 1200 ENTZ Census tracts⁵¹ and ii) tracts that were not designated as an ENTZ, EMPZ or ENTC through 2000, i.e. the NENTZs. Avoiding overlap with tracts affected by the EMPZ and ENTC programs eliminated about 40 ENTZ tracts and 40 NENTZ tracts.⁵² Census tracts are designed to be relatively homogeneous units with respect to population characteristics, economic status and living conditions at the time of establishment. They average about 4,000 inhabitants. Because ENTZ locations are typically not publicly disclosed (e.g., website information on locations) we contacted individual ENTZ coordinators and requested data that would enable us to geocode ENTZ locations. Most states designate ENTZ status based on Census tracts. We translated all data into Census tracts through geographic information systems (GIS). After we digitized ENTZ boundaries, we coded every 2000 Census tract nationally based on whether it fell entirely within an ENTZ, partially within an ENTZ, or did not fall within an ENTZ – we call this later group all the NENTZs. We deleted any tracts that were less than 50% covered by an ENTZ from the analysis entirely, and treated a tract as an ENTZ tract if at least half of it was in an ENTZ. Further, as Talanker, Davis and Leroy (2003) note, ENTZs have a tendency to grow over time. Thus if an ENTZ that started in the 1980s grew in the 1990s, the tracts covered in the 1980s would be deleted from our analysis, and the tracts first designated as an ENTZs in the 1990s are part of the treatment group.

We then matched this database of ENTZ tracks to Bureau of Census data for 1980, 1990, and 2000 and obtained the labor market variables that are our outcome measures in the empirical analysis.⁵³ Next, we created an analogous database of all NENTZ tracts. As noted above, we formed three comparisons in each of the 13 states that we studied. Specifically, for a given ENTZ we collected: i) the NENTZ tract nearest to the ENTZ in the same state, again resulting in approximately 1200 tracts being used; ii) the average of the outcome variable for 2,900 NENTZ tracts contiguous to the ENTZ (and in the same state) which resulted in about 4,100 tracts being used; iii) all NENTZs in the same state as the ENTZ, resulting in approximately 22,000 tracts being used and iv) the second closest NENTZ to the ENTZ – these are used as a comparison when we

⁵¹ We say ‘approximate’ or ‘about’ since the actual number of tracts used depends on the specific outcome variable because of missing values.

⁵² For comparison to previous work that did not account for overlap in the programs, it is interesting to note that we found that deleting overlapping tracts had little effect on our estimates of treatment effects.

⁵³ Additional details of this process are reported in Appendix A located at <http://www.marshall.usc.edu/leventhal/research/working-papers.htm>.

investigate spillover effects on the nearest NENTZ and when we delete the nearest NENTZ to get a treatment effect independent of spillovers.

4.2 Data for the Analysis of EMPZ and ENTC Programs

We have approximately 260 EMPZs, and we constructed the NEMPZs as tracts in the same states as the EMPZs that we not affected by an ENTZ program through 2000, an ENTC program through 2000, or the 1999 EMPZ program. We constructed the comparison groups for the EMPZ tracts in the same way as for the ENTZ tracts: i) the nearest NEMPZ in the same state, resulting in about 240 tracts again being used; ii) average of the outcome variable for 960 contiguous NEMPZs; iii) all NEMPZs in the same state, resulting in about 15,000 tracts being used iv) the second closest NEMPZ to the EMPZ – these are used as a comparison when we investigate spillover effects on the nearest NEMPZ and when we delete the nearest NEMPZ to get a treatment effect independent of spillovers.

We have approximately 370 ENTCs, and we constructed the NENTCs as tracts in the same states as the ENTCs that we not affected by an ENTZ program through 2000, an EMPZ program through 2000, or the 1999 ENTC program. We constructed the comparison groups for the ENTC as: i) the nearest NENTC in the same state, resulting in about 350 tracts again being used; ii) average of the outcome variable for 1,300 contiguous NENTCs; and iii) all NENTCs in the same state, resulting in about 29,000 tracts being used. We also collected data on the second closest NENTC for the same reasons described above for the second closest NEMPZ.

5. Summary Statistics and Empirical Results

5.1 Summary Statistics for the ENTZ Analysis

National means and standard errors for the means for our ENTZ analysis are given in Table 1 for our five labor market variables: the unemployment rate, the poverty rate, the fraction of households with working age population that have positive wage and salary income, real average household wage and salary income (in 2000 \$), for those with positive income and total employment. Here and in the tables that follow on ENTZs we have dropped all tracts covered by an EMPZ or ENTC from the analysis – see Online Appendix B for the results with these tracts included. In each case the standard errors of the mean values have been adjusted to allow for arbitrary heteroskedasticity and correlation across Census tracts in the same county. Lines 1 through 3 give the averages for the ENTZs in 1980, 1990 and 2000 respectively across the five labor market outcomes, while lines 4-6, 7-9 and 10-12 give the respective figures for the nearest, contiguous and all NENTZs respectively. Note first that the ENTZs have more disadvantaged labor markets than any

of the comparison groups, while conditions in the nearest NENTZs are worse than those in the other two comparison groups.

Lines 13, 15, and 17 of Table 1 gives our national treatment effect for the three comparison groups if we assume that an ENTZ and the relevant comparison groups share the same *linear* and higher order trends, so that first differencing is a valid means of estimating the treatment effect for the group. However, first differencing can only be considered valid for a given comparison group if the 1990-1980 (placebo) first differences in lines 14, 16, 18 are zero, which is clearly not the case. The intuition here is that the only way ENTZ designation in the 1990s can show a significant ‘placebo’ treatment effect between 1990 and 1980 is if the comparison group is inappropriate in first differences. Indeed lines 14, 16, 18 indicate that all three comparison groups had more beneficial trends than the ENTZs, indicating that the first difference treatment effects in lines 13, 15 and 17 are downward biased and that it is indeed important for us to use DDD estimation to measure the treatment effects of ENTZ designation.⁵⁴

5.2 Estimates of the Average National and State Effects of Being Designated an Enterprise Zone

As noted in Section 3, we consider estimators based on the following assumptions: A1) ENTZs share quadratic and higher order trends with their nearest NENTZs in the same state; A2) ENTZs share quadratic and higher order trends with their contiguous NENTZs in the same state and A3) all ENTZs share quadratic and higher order trends with all NENTZs in the same state. We use Hausman tests (with a 5% significance level) to choose our preferred model. Specifically, we test assumption A2 versus assumption A1, and assumption A3 versus assumption A1 when we use RE estimation. If both A2 and A3 pass, we choose our preferred estimates by testing A3 versus A2 for the RE estimates.

The RE estimation results for the case when we eliminate program overlap and estimate average *national* impacts of ENTZ designation on our five labor market outcomes are given in columns 1 through 5 of Table 2 respectively. The comparison group row shows which comparison group was chosen by the Hausman tests. For example the Hausman test chose the contiguous comparison group when the outcome variable is the unemployment rate and the closest comparison group for the poverty rate. We see that ENTZ designation significantly affects all outcome measures but the fraction of households with wage and salary income; the estimates for this later outcome are essentially uninformative. The effects on the other outcomes are substantial: the unemployment rate falls by about 1.6 percentage points, the poverty rate falls by about 6.1

⁵⁴ Note that this is an alternative explanation of why first difference studies such as Neumark and Kolko many find no significant effect of ENTZ designation.

percentage points, average wage and salary income rises by about \$700 (in \$2000), and employment rises by about 69 people. As noted above, to obtain the most conservative estimates of the effect of ENTZ designation possible we also estimated the program effects by OLS with standard errors clustered at the county level using the nearest NENTZ as the comparison group. It is worth noting that the OLS and RE results are not directly comparable in the presence of heterogeneous treatment effects, but we would expect the results to be qualitatively similar. From the results in Table C1 of Online Appendix C, it is clear that the OLS results are indeed qualitatively similar to those in Table 2 but that there is a clear efficiency gain to using RE estimation. (Below we find that there is less of an efficiency gain to using RE as compared to OLS when measuring the impact of EMPZs and ENTZs.) Specifically, the treatment effects for the unemployment rate and poverty rate are still quite strong in Table C1.

Table 3 contains the ENTZ effects at the state level. As expected many of these effects are imprecisely estimated and thus statistically insignificant, and thus we do not discuss them in detail. It is perhaps worth noting that all significant effects are in the expected direction, and we are able to estimate several impacts for California, Massachusetts and New York State relatively precisely.⁵⁵ It is useful to compare our results to those in the literature. We do not formally test whether our results differ significantly from those in other papers since we cannot allow for the correlation between our estimates and others; rather we simply present our 95% confidence intervals and those from other research for comparable estimates; surprisingly we were not able to find that many comparable estimates. The 95% confidence interval for our California employment estimate is [-230.91, 183.37]. Neumark and Kolko measure the effect of ENTZ designation on annual employment growth, and thus we use the procedure described in Section 2 of Appendix A to obtain the implied effect on Census Tract employment.⁵⁶ When we do this for their estimates in Table 6, Row A, Column 1, we obtain an approximate 95% confidence interval of [-332.61, 411.01]. (This effect and those reported below are for Census tract of 2000 employed individuals.) When we use this for their estimates in Table 6, Row A, Column 3, we obtain an approximate 95% confidence interval of [-456.49, 303.76] on a Census tract of 2000 employed individuals. We can also transform Bondonio and Engberg's (2000) Table 7, Column 1 estimates (for the 1994-1980 period) using the second approach in Section 2 of Appendix A. Here we obtain the 95% confidence interval for California employment of [-80.78, 102.52]. Also, our results imply a 95% confidence interval for the effect of ENTZ designation on New York State employment of [-123.38, 252.08], while Bondonio and Engberg's (2000) estimates imply a 95% confidence interval for this effect of [-68.76, 75.99].

⁵⁵ California, Massachusetts and New York all have relatively generous and aggressive ENTZ programs.

⁵⁶ As noted above, Appendix A is available at <http://www.marshall.usc.edu/leventhal/research/working-papers.htm>.

Note that in contrast, our confidence interval for the average national effect of ENTZ designation on employment is a much more precise [2.99, 133.39].

We can also compare our confidence interval for the effect of ENTZ designation on the unemployment rate for California, [-4.587, -1.327], to that implied by Elvery (2009) (for the period 1980-1990) of [-1.399, 1.715]. If we repeat the same exercise for Florida, the confidence interval of our unemployment rate effect is [-2.815, 1.137] versus his confidence interval is [-0.907, 3.04]. In contrast our confidence interval for the average national effect is again more precise at [-2.105, -1.178]. If we compare our confidence intervals for the effect of ENTZ designation on the California poverty rate we get [-14.367, 0.667], while his estimates imply [-0.101, 2.883]; for the Florida poverty rate effect we obtain [-14.463, -0.031], while Elvery's results imply [-0.907, 3.043]. Finally we estimate the confidence interval for the average national effect on the poverty rate as [-8.521, -3.685].

To summarize, the Neumark-Kolko, Bondonio-Engberg and Elvery studies produce confidence intervals for the state effects that are basically uninformative,⁵⁷ while among the effects discussed immediately above, our confidence intervals for state effects are informative only for the California unemployment and poverty rates, and perhaps the Florida poverty rate effect. Overall these results indicate the difficulty of obtaining useful state estimates of the effect of ENTZ designation, and why we believe it is inappropriate to argue that previous work has shown the effect of ENTZ designation to be zero.

Table 4 contains our IV estimates at the national level, and before discussing the estimates it is appropriate to consider whether we have a weak IV issue. Researchers often address this issue by using the rule-of-thumb from Staiger and Stock (1997) that the F-test on the excluded (from the second stage equation) instruments \tilde{Y}_{i1980} in the first stage equation (15) be greater than 10, or by considering the refinements of this rule-of-thumb in Stock and Yogo (2005). However, we cannot use the results in these papers since the F-test is not appropriate if the observations are dependent across the same county, as is assumed in our model. Instead we use the rule-of-thumb from Hansen, Hausman, and Newey (2008) that the Wald statistic for the null hypothesis that coefficients on the excluded instruments are zero in the first stage equation should be greater than 36 (for four excluded instruments). The appropriate Chi-Square statistics are presented in the last row of Table 4 and are much larger than 36; thus we conclude that weak IV is not a problem here. Focusing on the treatment effects in the first row of Table 4, we continue to see significant effects in the expected direction for the unemployment rate, the poverty rate, and average household wage and salary

⁵⁷ One might consider Elvery's confidence interval for the California poverty rate informative; our concern here is that the vast majority of the confidence interval is positive, while we would expect the ENTZ effect on poverty to be zero or negative.

income. Moreover, the employment effect is very close to the OLS estimates, but its standard error becomes much larger when we use IV estimation.

The state IV estimates are in Table 5. When considering the weak IV issue, the literature is of less help, since we know of no rule-of-thumb for the case of several endogenous variables and correlated residuals. However, we note that our random effects estimation has a natural interpretation as a seemingly unrelated regression for a model where state treatment effects are estimated state by state. Thus it is natural to perform a weak IV test state by state (and dependent variable by dependent variable); again the respective Chi-Square statistic should be greater than 36 to reject the null hypothesis of weak IV. The Chi-Square statistics in Table 5 generally allow us to reject the null of weak IV, except for all the Ohio first stage regressions, two of the Oregon first stage regressions, and one of Other States first stage regressions. Given the presence of weak IV in these states, we focus on the results for the other states.⁵⁸ Interestingly we continue to see several significant treatment effects for California and Massachusetts. The Florida coefficients are less significant now, while the New York State impacts have become more statistically significant.

In Table 6 we consider the spillover treatment effect on the nearest NENTZ at the national level, but find no significant spillover effects. (Recall that we used exactly the same methodology here as in Tables 2 and 3, except that the second nearest NENTZ now acts as the most conservative comparison group for the nearest NENTZ). Indeed the only estimates with an asymptotic t-statistic greater than one are the approximately 2% reduction in the poverty rate and the increase in wage and salary income of almost \$1000. Thus to the extent we find any evidence of spillovers, they are positive (beneficial) spillovers. In Table 7 we carry out the same analysis at the state level. Now eight of the thirty-five estimated treatment effects are statistically significant, with five of them indicating beneficial spillovers and three of them indicating negative spillovers. Thus there is somewhat more evidence of spillovers at the state level, but it seems reasonable to conclude that they are not in any particular direction.

For completeness, in Table 8 we re-estimate our model with the nearest NENTZ dropped and the second nearest NENTZ taking its role. The treatment effects on the unemployment rate, the poverty rate and average wage and salary income are statistically significant, in the expected direction, and of the same magnitude as in Table 2. However, now the effect on the fraction of households with positive wage and salary income is positive and significant, while the estimated effect for employment is still positive but insignificant. In Table 9 we repeat this procedure to control for spillovers at the state level. Thus exactly which outcomes are statistically significant is affected by controlling for spillovers in this way, but the overall result that ENTZ designation is beneficial is not.

⁵⁸ If our primary interest was in these states, we would next use the 1980 value of the unemployment rate as an instrument for these states.

At the state level, all significant effects are in the expected direction, and we are able to estimate several significant beneficial impacts for California, Massachusetts, New York State and Oregon.

5.3 Summary Statistics for Federal EMPZs and ENTC Impacts

Table 10 contains the summary statistics for the EMPZs while Table 11 has the statistics for the ENTCs. Here and in the tables that follow on EMPZs and ENTCs, we have dropped all tracts covered by an ENTZ from the analysis – see Online Appendix B for the results with these tracts included. These tables indicate that EMPZs are more disadvantaged than ENTCs, which in turn are more disadvantaged than ENTZs. Considering EMPZs specifically, the average nearest NEMPZ and the average contiguous NEMPZ are better off than the average EMPZ in all years. Further, the average member of the all NEMPZ is much better off than the average nearest NEMPZ and the average contiguous NEMPZ for all years. Lines 13, 15, and 17 of Table 10 gives our national treatment effect for the three comparison groups if we assume that an EMPZ and the relevant comparison groups share the same *linear* and higher order trends, so that first differencing is a valid means of estimating the treatment effect for the group. However, as noted for the ENTZs, first differencing can only be considered valid for a given comparison group if the 1990-1980 (placebo) first differences in lines 14, 16, 18 are zero, which is clearly not the case.⁵⁹ As in the case of the ENTZs, lines 14, 16, 18 indicate that all three comparison groups had more beneficial trends than the EMPZs, indicating that the first difference treatment effects in lines 13, 15 and 17 are downward biased, and that DDD estimation also should be used to measure the treatment effects of EMPZ designation.

Table 11 indicates very similar patterns for the ENTCs. With regard to the NENTCs, from Table 11 we see a similar picture as found in Table 10 – on average, the nearest and contiguous NENTCs are better off than the ENTCs in all years, while the average member of the all NENTC comparison group has much better economic conditions than the other comparison groups. Finally, lines 14, 16, and 18 indicate significant ‘placebo’ effects from 1990-1980 for all three comparison groups, again indicating that all three comparison groups have more favorable trends and that first difference estimates of the effect of ENTC designation will also be biased downward.

5.4 Estimated Treatment Effects of EMPZ and ENTC Designation

We again consider the three comparison groups used for the ENTZs when analyzing the effect of EMPZ and ENTC designation, and then choose the most appropriate group using Hausman tests. Since both of these are Federal programs we consider only the national effects. Table

⁵⁹ Again the intuition here is that the only way EMPZ designation in the 1990s can show a significant (placebo) ‘treatment effect’ between 1990 and 1980 is if the comparison group is inappropriate in first differences.

12 presents the RE estimates of the treatment effects. We see that EMPZ designation significantly reduces tract unemployment by about 8.7%, the poverty rate by about 8.8%, and significantly raises average wage and salary income by about \$6000 and employment by about 238 people. It has a positive but insignificant effect on the fraction with positive wage and salary income. Note that these effects are much larger than those for ENTZ designation; however, it is also important to recall that EMPZ tracts are starting from a much worse base than the ENTZ tracts. Finally, to again obtain the most conservative estimates of the effect of EMPZ designation possible, we also estimated the program effects by OLS with standard errors clustered at the county level using the nearest NEMPZ as the comparison group. From the results in Table C2 of Online Appendix C, it is clear that these results are very similar to those in Table 12.

Table 13 contains the IV results for the EMPZ designation treatment effect. From the last line of the table we see that the Chi-Square statistics are much larger than the critical value of 36, again indicating that weak instruments is not an issue. The IV results are treatment effects on one outcome are for tracts whose EMPZ designation are sensitive to marginal changes in the 1980 values of the other outcomes, and thus not directly comparable to the results in Table 12, but certainly are at least as strong as the results in Table 12 of these programs on the unemployment rate. Table 14 gives the spillover treatment effects on the nearest NEMPZ. Interestingly none of the spillovers have a t-statistic greater than 0.5. In Table 15 for completeness we repeat the analysis in Table 12 with the nearest NEMPZ dropped, and the results are basically unchanged from Table 12, except that the treatment effect on average wage and salary income is no longer statistically significant.

The results for the ENTICs are in Tables 16-19. Table 16 contains our base results, and we see that ENTIC designation significantly affects all five labor market indicators in a beneficial direction. Specifically, ENTIC designation lowers the unemployment rate by about 2.6 percentage points, the poverty rate by approximately 20 percentage points, raises the fraction with positive employment earnings by 1.36 percentage points, average wage and salary income by \$3209 and employment by about 154 jobs. Finally, we also estimated the program effects by OLS with standard errors clustered at the county level using the nearest NEMPZ as the comparison group to again obtain the most conservative estimates of the effect of ENTIC designation possible. From the results in Table C3 of Online Appendix C, it is clear that these results are very similar to those in Table 16.

The IV local average treatment effect estimates are in Table 17. While these are not directly comparable to the results in Table 16 for the reasons discussed above, it is important to note that the significant beneficial effects continue on all five labor market indicators. The (spillover) treatment effects are reported in Table 18. Note that the point estimates indicate positive spillover effects on all five labor market variables, although only the poverty rate effect is different from zero. Given this it

is not surprising that the estimates in Table 19 for the case where we drop the nearest NENTZ are quite similar to our base results in Table 16.

In summary, EMPZ designation significantly improves the labor market in terms of every measure except, the fraction with wage and salary income, while ENTC designation significantly improves all five labor market measures. Moreover, while there is no clear picture in terms of the relative magnitudes of EMPZ and ENTC designation, both are considerably bigger than the impact of ENTZ designation, perhaps because the tracts affected by EMPZ and ENTC designation are considerably worse off than the tracts affect by ENTZ designation.

6. Conclusion

In this paper we use a conservative double difference estimation approach and disaggregated labor market data to measure the impact of state Enterprise Zones, federal Empowerment Zones, and federal Enterprise Community programs. We find that all of these programs significantly improve local labor markets, although the effects of EMPZ and ENTC designation are considerably larger in absolute value, perhaps because they are implemented in much more disadvantaged labor markets. We consider the possibility that treatment is assigned on the basis of a negative shock in 1990 which will cause an overstatement of beneficial treatment effects by using an IV approach, but our qualitative results are not affected by doing so. Finally, we find very little evidence of spillovers to the nearest non-treatment tract, and not surprisingly, dropping this nearest tract does not affect our results.

These results are noteworthy for several reasons. Our study is the first to jointly look at these three programs, allowing policy makers to compare the relative impacts of these programs estimated by a common research strategy. We show that about 5 percent of ENTZ tracts are also EMPZs or ENTCs, and that about 10 percent of EMPZs and 20 percent of ENTCs are also ENTZs.

Our paper is the first to carry out our estimation without the overlapping tracts, and we find that the results do not change in meaningful way if this overlap is ignored. Second, in spite of our conservative estimation strategy, by looking at national effects with disaggregated data we demonstrate that, on average, ENTZ designation has a significantly beneficial effect on local labor markets, while most previous research did not find any significant impact. In addition, we find strong and significant beneficial effects of EMPZ and ENTC designation. The EMPZ program has received less attention in the literature, and the studies that do consider this program produce conflicting results, perhaps because of an identification problem that arises with propensity score matching in this case. Using a common methodology, we find that all of these programs significantly improve local labor markets.

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Figure 1a: Zip Codes Intersecting Los Angeles Central ENTZ

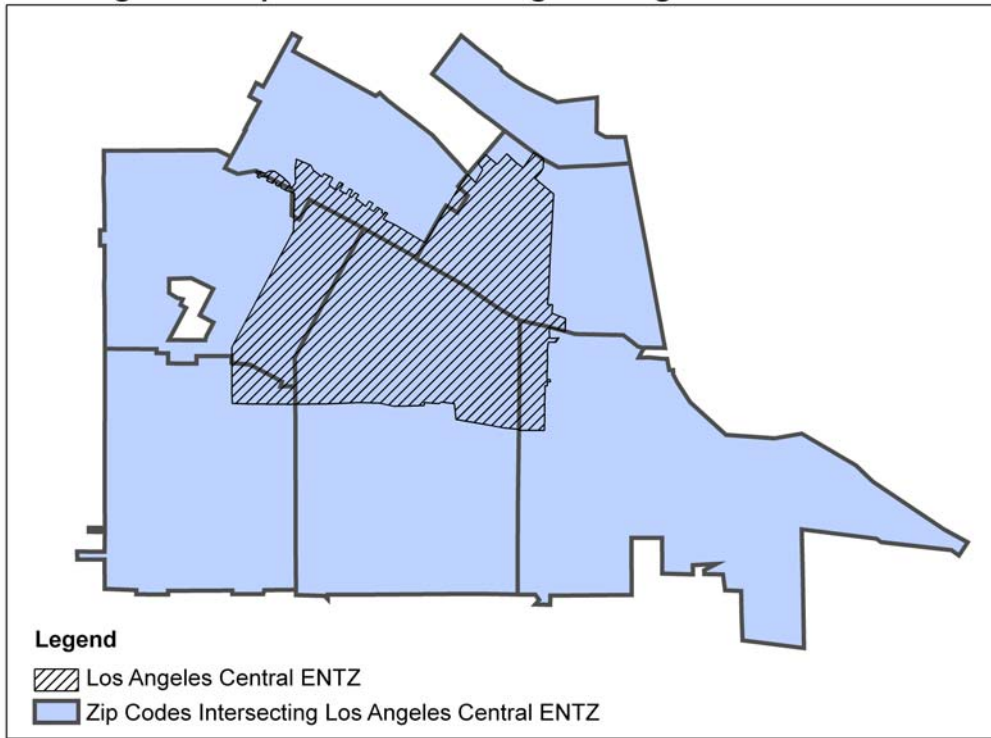


Figure 1b: Census Tracts Intersecting Los Angeles Central ENTZ

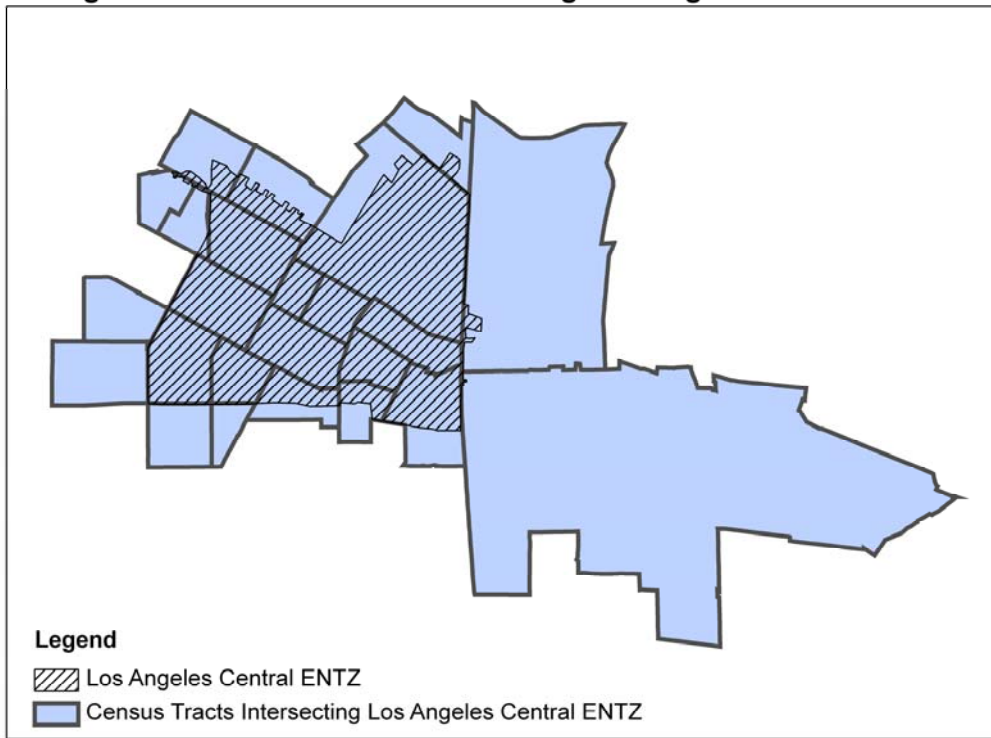


Table 1: Summary Statistics for Enterprise Zones Analysis

		Unemployment Rate (%)	Poverty Rate (%)	Fraction of Households with Wage and Salary Income (%)	Average Wage and Salary Income (\$2000)	Employment
1	ENTZ 1980	7.631*** (0.37)	16.41*** (1.44)	74.39*** (0.96)	35626*** (855)	1671*** (64.84)
2	ENTZ 1990	8.874*** (0.43)	25.67*** (1.77)	74.29*** (0.83)	43306*** (1297)	1866*** (65.65)
3	ENTZ 2000	7.723*** (0.51)	17.95*** (1.34)	75.08*** (0.75)	45820*** (1565)	1933*** (70.69)
4	Nearest NENTZ 1980	7.095*** (0.44)	12.90*** (1.70)	77.44*** (1.18)	40012*** (1311)	1626*** (84.07)
5	Nearest NENTZ 1990	7.381*** (0.39)	19.15*** (2.32)	77.10*** (0.81)	48542*** (2188)	1902*** (84.93)
6	Nearest NENTZ 2000	6.761*** (0.65)	13.92*** (1.69)	77.17*** (0.60)	52672*** (2668)	2004*** (90.72)
7	Contiguous NENTZ 1980	6.291*** (0.47)	11.46*** (1.20)	77.45*** (0.86)	40896*** (990)	1734*** (74.16)
8	Contiguous NENTZ 1990	6.455*** (0.34)	15.40*** (2.14)	76.98*** (0.61)	52314*** (2690)	2013*** (66.68)
9	Contiguous NENTZ 2000	5.957*** (0.54)	11.52*** (1.17)	76.89*** (0.44)	57279*** (3443)	2154*** (76.93)
10	All NENTZ 1980	6.594*** (0.21)	10.77*** (0.50)	78.56*** (0.61)	43567*** (683)	1538*** (40.51)
11	All NENTZ 1990	6.501*** (0.27)	15.77*** (0.71)	78.26*** (0.53)	53163*** (1146)	1895*** (42.95)
12	All NENTZ 2000	6.466*** (0.29)	12.13*** (0.54)	77.95*** (0.42)	57689*** (1206)	2073*** (47.18)
13	E{ENTZ(Δ 00) - Nearest NENTZ(Δ 00)}	-0.639** (0.30)	-3.802*** (1.16)	0.844*** (0.29)	-1882** (929)	-59.12 (45.68)
14	E{ENTZ(Δ 90) - Nearest NENTZ(Δ 90)}	0.937*** (0.24)	4.941*** (1.13)	0.62 (0.50)	-2585*** (698)	-71.37** (32.05)
15	E{ENTZ(Δ 00) - Contiguous NENTZ(Δ 00)}	-0.650** (0.26)	-3.813*** (1.14)	0.891*** (0.30)	-1967*** (591)	-74.84* (40.79)
16	E{ENTZ(Δ 90) - Contiguous NENTZ(Δ 90)}	1.070*** (0.23)	5.299*** (1.06)	0.37 (0.50)	-3408*** (881)	-83.94*** (30.44)
17	E{ENTZ(Δ 00)} - E{All NENTZ(Δ 00)}	-0.15 (0.26)	-4.595*** (0.76)	1.474*** (0.43)	-2129*** (760)	-86.67*** (33.29)
18	E{ENTZ(Δ 90)} - E{All NENTZ(Δ 90)}	0.782*** (0.25)	5.366*** (0.80)	0.700 (0.47)	-4192*** (578)	-112.2*** (36.89)

Notes:

1. Standard Errors in parentheses are adjusted for correlation across tracts in the same county. *** p<0.01, ** p<0.05.
2. Δ 00: 2000-1990, Δ 90: 1990-1980
3. Rows 17 and 18 are obtained by regression of outcomes on ENTZ dummy and state dummies.

Table 2. Estimates of the Average National Effects of Enterprise Zone Designation

	Unemployment Rate (%)	Poverty Rate (%)	Fraction of Households with Wage and Salary Income (%)	Average Wage and Salary Income (\$2000)	Employment
ENTZ	-1.641*** (0.232)	-6.103*** (1.209)	0.454 (0.298)	702.5* (389.6)	68.91** (32.6)
Comparison Group	Contiguous	Nearest	Contiguous	Nearest	Contiguous
Observations	1227	1245	1241	1219	1264
Number of ENTZs	1227	1245	1241	1219	1264
Number of Counties	112	112	112	112	112

Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3. Estimates of the Average State Effects of Enterprise Zone Designation

	Unemployment Rate (%)	Poverty Rate (%)	Fraction of Households with Wage and Salary Income (%)	Average Wage and Salary Income (\$2000)	Employment
ENTZ*California	-2.957*** (0.815)	-7.135** (3.608)	0.766 (1.050)	3056** (1393)	-23.77 (103.57)
ENTZ*Florida	-0.839 (0.988)	-7.247 (4.504)	2.197* (1.276)	1016 (1658)	187.14 (128.78)
ENTZ*Massachusetts	-2.494*** (0.342)	-13.954*** (2.219)	-0.507 (0.444)	87.98 (590)	-30.28 (76.48)
ENTZ*New York	-3.222*** (0.752)	-8.810*** (3.363)	1.553 (0.974)	1059 (1266)	64.14 (93.97)
ENTZ*Ohio	-0.088 (0.548)	1.911 (2.335)	-0.008 (0.711)	2203** (911)	124.81* (64.20)
ENTZ*Oregon	0.624 (1.113)	-10.290** (4.499)	4.150*** (1.348)	-2758 (1752)	185.89 (124.06)
ENTZ*Other states	0.449 (0.702)	-1.411 (2.895)	1.490 (0.912)	46.42 (1186)	60.99 (77.21)
Comparison Group	Contiguous	Nearest	Contiguous	Nearest	Contiguous
Observations	1226	1245	1241	1219	1264
Number of ENTZs	1226	1245	1241	1219	1264
Number of Counties	112	112	112	112	112

See Notes to Table 2.

Table 4. IV Estimates of the Average National Effects of Enterprise Zone Designation

	Unemployment Rate (%)	Poverty Rate (%)	Fraction of Households with Wage and Salary Income (%)	Average Wage and Salary Income (\$2000)	Employment
ENTZ	-3.123*** (0.787)	-26.13*** (2.737)	0.196 (1.297)	2714** (1056.0)	-51.17 (83.8)
Comparison Group	Contiguous	Nearest	Contiguous	Nearest	Contiguous
Observations	1226	1189	1238	1191	1239
Number of ENTZs	1226	1189	1238	1191	1239
Number of Counties	115	114	115	114	115
First Stage Chi-Square Statistic	457.95	317.05	453.38	272.95	443.29

Notes:

Each outcome is instrumented by other outcomes in 1980. For example, in column (1) ENTZ is instrumented by poverty rate, fraction of households with wage and salary income, average wage and salary income, and employment in 1980. *** p<0.01, ** p<0.05, * p<0.1

Table 5. IV Estimates of the Average State Effects of Enterprise Zone Designation

	Unemployment Rate (%)	Poverty Rate (%)	Fraction of Households with Wage and Salary Income (%)	Average Wage and Salary Income (\$2000)	Employment
ENTZ*California	-3.399*** (0.721)	-8.566 (8.499)	3.621*** (1.061)	3136** (1395)	7.14 (118.40)
ENTZ*Florida	-1.968** (0.905)	-13.91** (5.726)	3.709** (1.426)	3648 (2628)	240.7*** (84.23)
ENTZ*Massachusetts	-4.069*** (1.030)	-22.11*** (3.080)	-1.449 (1.457)	770.90 (1185)	-132.10 (132.80)
ENTZ*New York	-3.494 (2.485)	-17.81* (9.796)	-3.486 (3.089)	4705 (5362)	152.90 (130.10)
ENTZ*Ohio	6.164 (6.275)	22.47** (9.839)	-7.941 (5.603)	9740* (5663)	198.30 (442.30)
ENTZ*Oregon	-0.259 (3.061)	-15.61*** (4.786)	6.983*** (1.777)	-4312*** (1309)	558.3*** (104.70)
ENTZ*Other states	0.769 (1.235)	-11.670 (7.576)	1.379 (0.924)	-678.50 (2476)	101.60 (156.20)
Comparison Group	Contiguous	Nearest	Contiguous	Nearest	Contiguous
Observations	1227	1195	1241	1197	1242
Number of ENTZs	1227	1195	1241	1197	1242
Number of Counties	115	114	115	114	115
First Stage Chi-Square Statistic					
California	112.52	91.13	115.05	92.03	113.15
Florida	98.19	33.65	94.80	44.44	72.50
Massachusetts	447.64	471.95	429.41	270.22	437.53
New York	31.43	16.17	31.83	11.94	32.35
Ohio	3.41	3.13	2.17	4.63	3.47
Oregon	27.32	33.95	33.37	37.67	19.41
Other states	50.22	33.65	39.78	28.81	37.46

See Notes to Table 4.

Table 6. Estimates of the Average National Spillover Effects on the Nearest Non-Enterprise Zone

	Unemployment Rate (%)	Poverty Rate (%)	Fraction of Households with Wage and Salary Income (%)	Average Wage and Salary Income (\$2000)	Employment
NENTZ	-0.257 (0.325)	-2.075 (1.284)	-0.205 (0.502)	987.9 (697.9)	-31.55 (38.3)
Comparison Group	All	Contiguous	Contiguous	Contiguous	Contiguous
Observations	21758	412	409	414	414
Number of NENTZs	429	412	409	414	414
Number of Counties	488	100	100	100	100

See Notes to Table 2.

Table 7. Estimates of the Average State Spillover Effects on the Nearest Non-Enterprise Zone

	Unemployment Rate (%)	Poverty Rate (%)	Fraction of Households with Wage and Salary Income (%)	Average Wage and Salary Income (\$2000)	Employment
NENTZ*California	-0.848 (0.941)	-0.353 (3.563)	-1.393 (1.497)	162 (2093)	-126.66 (114.16)
NENTZ*Florida	-2.341** (1.059)	-6.171 (3.921)	1.271 (1.657)	1378 (2303)	-74.32 (125.61)
NENTZ*Massachusetts	-0.327 (0.674)	-0.986 (2.304)	-0.305 (0.988)	2974** (1347)	125.95* (73.49)
NENTZ*New York	1.795** (0.851)	1.142 (3.173)	-2.306* (1.372)	1377 (1864)	-34.16 (101.67)
NENTZ*Ohio	0.418 (0.646)	0.425 (2.429)	0.089 (1.021)	-1706 (1419)	-191.28** (77.43)
NENTZ*Oregon	-0.638 (1.521)	-7.493 (5.862)	1.337 (2.436)	2895 (3438)	-47.38 (187.79)
NENTZ*Other states	-1.813* (0.980)	-9.990*** (3.643)	1.332 (1.524)	1376 (2140)	80.05 (116.73)
Comparison Group	All	Contiguous	Contiguous	Contiguous	Contiguous
Observations	21758	412	409	414	414
Number of NENTZs	429	412	409	414	414
Number of Counties	488	100	100	100	100

See Notes to Table 2.

Table 8. Average National Effects of Enterprise Zone Designation After Excluding the Nearest Non-Enterprise Zone

	Unemployment Rate (%)	Poverty Rate (%)	Fraction of Households with Wage and Salary Income (%)	Average Wage and Salary Income (\$2000)	Employment
ENTZ	-1.594*** (0.240)	-7.388*** (1.604)	1.025*** (0.338)	1334*** (398.6)	35.83 (33.0)
Comparison Group	All	Contiguous	All	Contiguous	All
Observations	22278	854	22445	855	22643
Number of ENTZs	949	854	954	855	960
Number of Counties	488	100	494	100	512

See Notes to Table 2.

Table 9. Average State Effects of Enterprise Zone Designation After Excluding the Nearest Non-Enterprise Zone

	Unemployment Rate (%)	Poverty Rate (%)	Fraction of Households with Wage and Salary Income (%)	Average Wage and Salary Income (\$2000)	Employment
ENTZ*California	-3.345*** (0.542)	-2.927 (4.043)	-0.105 (1.218)	1937* (1166)	-51.11 (99.09)
ENTZ*Florida	-3.277*** (0.906)	-18.933*** (5.447)	4.897*** (1.662)	883 (1570)	160.04 (134.14)
ENTZ*Massachusetts	-2.779*** (0.480)	-12.906*** (3.508)	-0.264 (0.887)	3389*** (643)	129.02 (82.76)
ENTZ*New York	-1.260* (0.694)	-6.674* (3.945)	-0.098 (1.237)	388 (1166)	-95.26 (98.26)
ENTZ*Ohio	0.825 (0.530)	1.588 (2.929)	0.737 (0.931)	-1182 (912)	-44.56 (73.03)
ENTZ*Oregon	-1.860* (1.004)	-13.143** (5.470)	7.177*** (1.765)	1880 (1780)	142.55 (136.91)
ENTZ*Other states	0.756 (0.755)	-13.706 (3.998)	2.090 (1.289)	-1434 (1282)	-18.25 (100.19)
Comparison Group	All	Contiguous	Contiguous	Contiguous	Contiguous
Observations	22278	854	853	855	857
Number of ENTZs	949	854	853	855	857
Number of Counties	488	100	100	100	100

See Notes to Table 2.

Table 10: Summary Statistics for Empowerment Zones Analysis

		Unemployment Rate (%)	Poverty Rate (%)	Fraction of Households with Wage and Salary Income (%)	Average Wage and Salary Income (\$2000)	Employment
1	EMPZ 1980	18.60*** (1.47)	41.76*** (1.11)	56.42*** (0.95)	27319*** (868)	832.6*** (64.21)
2	EMPZ 1990	24.34*** (2.21)	62.51*** (2.22)	55.56*** (1.59)	29156*** (1012)	697.3*** (98)
3	EMPZ 2000	20.84*** (0.95)	39.15*** (0.88)	63.61*** (1.31)	34430*** (1587)	700.0*** (111)
4	Nearest NEMPZ 1980	16.63*** (1.10)	35.69*** (1.44)	62.36*** (1.14)	30410*** (1446)	1013*** (100)
5	Nearest NEMPZ 1990	19.35*** (1.65)	53.60*** (2.13)	62.08*** (1.79)	33887*** (1267)	987.9*** (152)
6	Nearest NEMPZ 2000	19.47*** (1.19)	34.84*** (1.33)	67.99*** (1.54)	36714*** (1805)	919.8*** (145)
7	Contiguous NEMPZ 1980	16.09*** (0.92)	35.65*** (1.36)	61.49*** (1.22)	30620*** (1630)	1095*** (113)
8	Contiguous NEMPZ 1990	18.40*** (1.17)	52.85*** (1.51)	60.78*** (2.06)	34686*** (1032)	1036*** (159)
9	Contiguous NEMPZ 2000	18.33*** (0.94)	34.90*** (1.07)	67.26*** (1.40)	37329*** (1949)	972.8*** (153)
10	All NEMPZ 1980	7.976*** (0.33)	11.06*** (0.78)	78.85*** (0.74)	45361*** (919)	1591*** (47.7)
11	All NEMPZ 1990	7.335*** (0.42)	16.64*** (1.12)	77.93*** (0.60)	54661*** (1274)	1811*** (52.9)
12	All NEMPZ 2000	6.884*** (0.41)	12.21*** (0.81)	77.90*** (0.49)	58849*** (1539)	1902*** (58.8)
13	E{EMPZ(Δ 00) - Nearest NEMPZ(Δ 00)}	-3.475*** 1.107	-5.272*** 1.518	1.268 1.459	2444*** 801.7	68.38* 33.9
14	E{EMPZ(Δ 90) - Nearest NEMPZ(Δ 90)}	3.431*** 0.549	3.941*** 1.123	-0.312 0.567	-2024*** 583.1	-106.2*** 32.6
15	E{EMPZ(Δ 00) - Contiguous NEMPZ(Δ 00)}	-3.569** (1.34)	-5.417*** (1.65)	1.57 (1.40)	2697*** (856)	65.74** (26.8)
16	E{EMPZ(Δ 90) - Contiguous NEMPZ(Δ 90)}	3.561*** (0.51)	3.509** (1.21)	-0.16 (0.61)	-2273*** (541)	-76.44*** (22.0)
17	E{EMPZ(Δ 00)} - E{All NEMPZ(Δ 00)}	-3.199* (1.75)	-18.83*** (2.08)	8.054*** (1.51)	1,243 (1123)	-87.95** (34.2)
18	E{EMPZ(Δ 90)} - E{All NEMPZ(Δ 90)}	6.394*** (1.30)	15.00*** (2.13)	0.09 (1.44)	-7458*** (1355)	-353.3*** (38.4)

Notes:

- Standard Errors in parentheses are adjusted for correlation across tracts in the same county. *** p<0.01, ** p<0.05, *p<0.1
- Δ 00: 2000-1990, Δ 90: 1990-1980
- Rows 17 and 18 are obtained by regression of outcomes on EMPZ dummy.

Table 11: Summary Statistics for Enterprise Communities Analysis

		Unemployment Rate (%)	Poverty Rate (%)	Fraction of Households with Wage and Salary Income (%)	Average Wage and Salary Income (\$2000)	Employment
1	ENTC 1980	12.65*** (0.54)	32.13*** (1.19)	67.65*** (1.11)	26967*** (498)	1088*** (50.2)
2	ENTC 1990	15.51*** (0.58)	55.69*** (1.70)	65.47*** (1.02)	27705*** (657)	1015*** (55.2)
3	ENTC 2000	15.33*** (0.61)	35.04*** (1.16)	69.59*** (1.11)	31930*** (701)	983.7*** (62.9)
4	Nearest NENTC 1980	9.243*** (0.39)	21.43*** (1.07)	71.74*** (0.99)	31676*** (620)	1278*** (59.2)
5	Nearest NENTC 1990	9.824*** (0.53)	34.51*** (1.96)	71.66*** (0.82)	34721*** (805)	1305*** (68.6)
6	Nearest NENTC 2000	11.12*** (0.72)	24.09*** (1.29)	74.20*** (0.61)	38378*** (934)	1332*** (83.1)
7	Contiguous NENTC 1980	9.049*** (0.43)	20.92*** (0.82)	72.06*** (1.05)	31927*** (483)	1359*** (62.1)
8	Contiguous NENTC 1990	9.535*** (0.43)	33.73*** (1.74)	72.51*** (0.85)	35557*** (738)	1383*** (67.7)
9	Contiguous NENTC 2000	10.34*** (0.61)	23.11*** (1.11)	74.36*** (0.81)	39071*** (890)	1401*** (80.5)
10	All NENTC 1980	6.340*** (0.18)	9.897*** (0.27)	80.12*** (0.42)	44286*** (563)	1472*** (33.7)
11	All NENTC 1990	6.184*** (0.19)	15.45*** (0.46)	79.23*** (0.40)	51528*** (964)	1869*** (29.5)
12	All NENTC 2000	5.872*** (0.22)	11.11*** (0.35)	78.96*** (0.32)	56640*** (956)	2132*** (29.8)
13	$E\{\text{ENTC}(\Delta 00) - \text{Nearest NENTC}(\Delta 00)\}$	-1.37 (0.83)	-9.887*** (1.61)	1.688*** (0.54)	849.6* (468)	-41.96*** (15.3)
14	$E\{\text{ENTC}(\Delta 90) - \text{Nearest NENTC}(\Delta 90)\}$	2.325*** (0.47)	10.15*** (1.62)	-2.364*** (0.58)	-2673*** (425)	-101.7*** (24.2)
15	$E\{\text{ENTC}(\Delta 00) - \text{Contiguous NENTC}(\Delta 00)\}$	-0.96 (0.79)	-10.18*** (1.50)	2.215*** (0.51)	794.0* (416)	-50.19*** (15.5)
16	$E\{\text{ENTC}(\Delta 90) - \text{Contiguous NENTC}(\Delta 90)\}$	2.366*** (0.45)	10.70*** (1.41)	-2.612*** (0.50)	-2726*** (331)	-96.25*** (24.5)
17	$E\{\text{ENTC}(\Delta 00)\} - E\{\text{All NENTC}(\Delta 00)\}$	-0.01 (0.66)	-16.70*** (1.40)	4.258*** (0.68)	-786 (675)	-289.4*** (38.1)
18	$E\{\text{ENTC}(\Delta 90)\} - E\{\text{All NENTC}(\Delta 90)\}$	3.122*** (0.48)	18.07*** (1.52)	-1.02 (0.63)	-6094*** (683)	-457.0*** (32.5)

Notes:

- Standard Errors in parentheses are adjusted for correlation across tracts in the same county. *** p<0.01, ** p<0.05, *p<0.1
- $\Delta 00$: 2000-1990, $\Delta 90$: 1990-1980
- Rows 17 and 18 are obtained by regression of outcomes on ENTC dummy.

Table 12. Estimates of the Average National Effects of Empowerment Zone Designation

	Unemployment Rate (%)	Poverty Rate (%)	Fraction of Households with Wage and Salary Income (%)	Average Wage and Salary Income (\$2000)	Employment
EMPZ	-8.694*** (0.47)	-8.810*** (2.78)	1.743 (1.25)	6011*** (926)	238.2*** (43.0)
Comparison Group	All	Contiguous	Contiguous	All	All
Observations	14850	268	264	15043	15065
Number of EMPZs	264	268	264	272	251
Number of Counties	279	14	14	284	271

See Notes to Table 2.

Table 13. IV Estimates of the Average National Effects of Empowerment Zone Designation

	Unemployment Rate (%)	Poverty Rate (%)	Fraction of Households with Wage and Salary Income (%)	Average Wage and Salary Income (\$2000)	Employment
EMPZ	-36.64*** (8.32)	-10.73 (6.205)	6.148* (3.291)	38328*** (9232)	656.6** (301.8)
Comparison Group	All	Contiguous	Contiguous	All	All
Observations	14839	262	261	14953	14951
Number of EMPZs	264	262	261	270	264
Number of Counties	279	13	13	279	279
First Stage Chi-Square Statistic	15.24	85.56	87.86	18.22	19.84

See Notes to Table 4.

Table 14. Estimates of the Average National Spillover Effect on the Nearest Non- Empowerment Zone

	Unemployment Rate (%)	Poverty Rate (%)	Fraction of Households with Wage and Salary Income (%)	Average Wage and Salary Income (\$2000)	Employment
NEMPZ	0.474 (0.92)	0.874 (2.45)	-0.399 (1.02)	-218.8 (1012)	21.3 (59.49)
Comparison Group	Contiguous	Contiguous	Contiguous	Contiguous	All
Observations	212	208	214	213	29080
Number of NEMPZs	212	208	214	213	232
Number of Counties	28	27	28	28	777

See Notes to Table 2.

Table 15. Average National Effects of Empowerment Zone After Excluding the Nearest Non-Empowerment Zone

	Unemployment Rate (%)	Poverty Rate (%)	Fraction of Households with Wage and Salary Income (%)	Average Wage and Salary Income (\$2000)	Employment
EMPZ	-4.104** (1.94)	-9.966*** (3.60)	1.358 (2.24)	3209 (2108)	154.4** (61.0)
Comparison Group	Contiguous	Contiguous	Contiguous	2nd Nearest	2nd Nearest
Observations	118	123	120	120	125
Number of EMPZs	118	123	120	120	125
Number of Counties	12	12	12	12	12

See Notes to Table 2.

Table 16. Estimates of the Average National Effects of Enterprise Community Designation

	Unemployment Rate (%)	Poverty Rate (%)	Fraction of Households with Wage and Salary Income (%)	Average Wage and Salary Income (\$2000)	Employment
ENTC	-2.585*** (0.34)	-20.28*** (2.29)	4.897*** (0.49)	3520*** (500)	109.0** (51.26)
Comparison Group	All	Contiguous	All	Contiguous	All
Observations	28958	346	29287	374	29599
Number of ENTCS	401	346	900	374	412
Number of Counties	881	57	59	59	954

See Notes to Table 2.

Table 17. IV Estimates of the Average National Effects of Enterprise Community Designation

	Unemployment Rate (%)	Poverty Rate (%)	Fraction of Households with Wage and Salary Income (%)	Average Wage and Salary Income (\$2000)	Employment
ENTC	-23.65*** (8.939)	-19.57*** (4.246)	22.99*** (7.299)	3009*** (1052)	790.9** (333.0)
Comparison Group	All	Contiguous	All	Contiguous	All
Observations	28934	342	29047	371	29147
Number of ENTCS	401	342	407	371	412
Number of Counties	881	58	881	60	881
First Stage Chi-Square Statistic	40.00	334.58	20.19	330.41	41.53

See Notes to Table 4.

Table 18. Estimates of the Average National Spillover Effect on the Nearest Non- Enterprise Community

	Unemployment Rate (%)	Poverty Rate (%)	Fraction of Households with Wage and Salary Income (%)	Average Wage and Salary Income (\$2000)	Employment
NENTC	-0.035 (0.41)	-4.231* (2.21)	1.475 (1.06)	1329 (1130)	58.07 (86.63)
Comparison Group	All	Contiguous	Nearest	Contiguous	Nearest
Observations	28604	229	236	241	242
Number of NENTCs	245	229	236	241	242
Number of Counties	889	75	74	77	77

See Notes to Table 2.

Table 19. Average National Effects of Enterprise Community After Excluding the Nearest Non-Enterprise Community

	Unemployment Rate (%)	Poverty Rate (%)	Fraction of Households with Wage and Salary Income (%)	Average Wage and Salary Income (\$2000)	Employment
ENTC	-2.563*** (0.34)	-22.15*** (3.17)	4.733*** (1.19)	3217*** (775)	112.1** (51.43)
Comparison Group	All	2nd Nearest	2nd Nearest	Contiguous	All
Observations	28760	200	211	227	29399
Number of ENTCS	401	200	211	227	412
Number of Counties	880	55	56	57	953

See Notes to Table 2.