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Government Programs Can Improve Local Labor Markets: Evidence from State Enterprise Zones, Federal Empowerment Zones and Federal Enterprise Communities[1]

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November 2008

Revised February 2009

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[1] This paper was previously circulated under the title "Do Enterprise Zones Work" (mimeo 2006, 2007). Ham's work was supported by NSF grant SBS0627934. We are grateful for helpful comments from Fernando Alvarez, Tony Braun, Duke Bristow, Tom Holmes, Douglas Joines, Selahattin Imrohoroglu, Antonio Merlo, Shirley Maxey, Serkan Ozbelik, Vincenzo Quadrini, Geert Ridder, Jacqueline Smith, Heonjae Song, Martin Weidner and participants at the USC Applied Micro Workshop. Huseyin Gunay and Heonjae Song provided able research assistance. Any opinions, findings, and conclusions or recommendations in this material are those of the authors and do not necessarily reflect the views of the National Science Foundation, the Federal Reserve Bank of San Francisco or the Federal Reserve System. We are responsible for any errors.

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.

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 our 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 1980's, 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 1990's; 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) found a positive impact of ENTZs in Indiana when she looked at labor markets at the level of an unemployment insurance office, she could

[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 (RCs). Since RCs were established after 2000, they 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 nonexpermental evaluation of such programs.
[6] As noted below, Neumark and Kolko (2008) provide an ingenious method for measuring one of the five labor market measures, employment, at the Census tract level on an annual basis.

not find a positive 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 ours, Neumark and Kolko (2008) use Census tract data to study the impact of ENTZs in California on employment, but find no significant effect.[8]

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Two papers on EMPZs introduced in the mid-1990's, 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

[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 do find that it improves local labor markets by having a significant effect of the poverty rate and the fraction of individuals with wage and salary income.

national effect has a well defined interpretation and allows us to obtain much more precise estimates.

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 ENTZ[9] and ii) the double difference of the outcome variable for the nearest NENTZ Census tract in the same state.[10] 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. 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 generally find that the estimates from the random growth model are rejected when we evaluate ENTZs, However, we also consistently find significant positive (in the sense of improving the labor market) national average ENTZ effects; as well we often find significant state-specific positive effects.

Since the EMPZ and ENTC programs are Federal programs, we only estimate average national effects for these programs. We again use the three estimation methods described above, and in this case the Random Growth framework is rejected in about half of the specifications. Further, 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.

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.

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[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] Thus our measure of impact could be affected by spillovers to the NENTZ; we argue below that it is infeasible to obtain measures that do not include spillover effects and are credible econometrically.

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. Readers can gain an overview of the different programs by considering Appendix Table 1, where tax incentives, prequalification rules, and excluded industries by state, are presented. 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)[11] It should be noted that these tax benefits can represent substantial expenditure (i.e. foregone tax revenue); as noted 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 more modest program, reports spending $45 million in 2008 on its ENTZ programs.[12]

[11] 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.

[12] 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.

We restrict our analysis to estimating the impacts of ENTZs created during the 1990's.[13] Thus we eliminate states where all zones were created in the 1980's: Alabama, Delaware, Indiana, Iowa, Kentucky, Louisiana, and Oklahoma. We also eliminated individual ENTZs not created in the 1990's 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 (191); Florida (66); Massachusetts (563); New York (116); Ohio (230) and Oregon (62). 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).[14] 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 1990's 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 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 new per 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. Further information on these

[13] 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.
[14] 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.

programs is available from our detailed summaries of tax benefits and qualifications by state in our online Appendix A, Table A1.[15]

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

Starting in the 1990's, 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.[16] 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.[17] 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.[18] 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.[19] Since the programs have different features, we separately analyze EMPZs and ENTCs.

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 1990's. 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

[15] This is available at

[16] 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.

[17] We jointly estimate the effects of the 1994 and 1999 zones in our extra results appendix.

[18] 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.

[19] Tax Expenditures Budget, 2004-2010. Tax Policy Center, 2004.

considered in most previous studies, which have focused on the county or zip code level.[20] 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, the definition of the labor force changed between 1970 (individuals aged 14 and above) and 1980 (individuals aged 16 and above), so we can only use data from 1980, 1990 and 2000.[21]

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 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

[20] As noted above Papke (1993) uses Census blocks, which are smaller than Census tracts, while Neumark and Kolko (2008) use tract data. We first used tract data, and the closest NENTZ, to evaluate ENTZ designation in Imrohoroglu and Swenson (2006).

[21] As noted above an exception to this is provided by Neumark and Kolko (2008) who ingeniously use establishment data to construct annual employment data at the Census tract level. However, their procedure is very labor-intensive and involves difficult judgment calls, and thus would be extremely time consuming to implement for all states. Further, using it would also restrict us to consider only one of the five outcome variables we use below.

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

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. Our results below indicate that the tests have substantial power and that the random growth model is usually rejected for ENTZs. Finally we use ENTZs, EMPZs, and ENTCs that are affected by only one of the programs, although we also indicate how the results change for the program impacts when we ignore 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 doublet j of an ENTZ Census tract *i* and the nearest NENTZ tract *i'* in the same state for which we use the notation $i,i' \in j$. Our maintained assumption throughout what follows is that i and i' share the same coefficients on quadratic and higher order trends; they are allowed to have tract-specific fixed effects and linear trends. The labor market outcome of interest in tract k $(k=i,i')$ in year t (t=1980, 1990, 1980) is determined by

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

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. We have exploited the fact that i and i' share the same second and higher order trends. Next we take the double differences for k=i , i' respectively

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

Note the tract specific intercepts α_i and $\alpha_{i'}$, as well as the tract specific fixed effects and linear trends drop out of (2). Finally, we assume that[24]

[23] We also exclude ENTZs and NENTZs that overlap with the EMPZs and ENTZs introduced in 1999.
[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'.

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

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

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

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]

If there are spillovers from an ENTZ to the nearest NENTZ, then the impact measured by δ will be the net of these spillovers. To try to obtain an estimate which does not contain spillovers, one might use instead, as a comparison, a 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 nearest NENTZ *i'*.[28] (In fact, in our empirical work below we generally reject the null hypothesis that all ENTZs and NENTZs in a state share the same quadratic and higher order trends.) 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.

3.2.2 Estimating State- Specific Average Effects

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

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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] Note that in our empirical work below we generally reject the null hypothesis that all ENTZs and NENTZs in a state share the same quadratic and higher order trends.

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

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

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

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.[29] As noted above estimation of (6) has the advantage that it provides estimates for the effects of the individual state programs, but has the disadvantage that confidence intervals for these effects may be quite large and relatively uninformative.

3.2.3 Why One Cannot Allow the Program Effect to depend on the 1990 Value of the Outcome Variable

One possibility is to allow program effects to depend on the tracts' economic situation in 1990 by interacting the 1990 ENTZ outcome variable with the ENTZ dummy variable. However it is very difficult to estimate this effect consistently within our framework. To see this note, our model will become

$$\begin{aligned} Y_j &= \delta_0 EZ_{i2000} + \delta_1 EZ_{i2000} W_{i1990} + e_j \\ &= \delta_0 EZ_{i2000} + \delta_1 EZ_{i1990} W_{i1990} + (\varepsilon_{i2000} - 2\varepsilon_{i1990} + \varepsilon_{i1980}) - (\varepsilon_{i'2000} - 2\varepsilon_{i'1990} + \varepsilon_{i'1980}). \end{aligned} \tag{7}$$

The problem arises from the fact that W_{i1990} is potentially correlated with many terms in composite error term. For example, even if ε_{it} is uncorrelated over time and independent of $\varepsilon_{i'\tau}$, W_{i1990} will be correlated with ε_{i1990} and δ_1 will be biased in a negative direction. Since there are no obvious candidates to use as instrumental variables for W_{i1990}, we do not pursue this approach.

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

[29] The only caveat to this is that in joint Random Effects estimation, we would assume that correlation across counties was not state-specific.

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. 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

$$\bar{Z}_{i'} = \sum_{l \in S_i^{'}.} Z_l / I_i^{'}, \qquad \text{where} \tag{8}$$

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

The DDD estimator is now

$$Y_j = Z_i - \bar{Z}_{i'} = \delta E Z_{2000i} + v_j. \tag{10}$$

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 hypothesis that (10) is an appropriate specification. 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.[30] Under this assumption, we obtain our DDD estimator of the average national effect by running the regression

$$Z_i = \sum_s^s \alpha_s D_{is} + \delta EZ_{2000k} + u_l + \varepsilon_i$$

$$Z_{i'} = \sum_s^s \alpha_s D_{i's} + u_l + \varepsilon_{i'} \tag{11}$$

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 1990's) and their nearest NENTZ 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 above, 1970 Census tract labor market data is not comparable to that for 1980-2000, since the former is based on individuals 14 years and over, while the latter are based on individuals 16 years and over. Thus we cannot perform a specification test using the 1990-1970 DDD estimators.

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 Hausman tests and these estimates in the Extra Results Appendix B available online.[31] 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

[30] 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.

[31] **This is available at xxx.**

Hausman (1978) by using Random Effects (RE) estimation, where the random effect is at the county level. Thus we use RE estimation to distinguish between the different assumptions and obtain our preferred estimates. As one can see from the online Extra Results Appendix B, the RE and OLS estimates are relatively close.

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 basics 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.[32] We use this second approach.

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

3.6.1 Comparison to Previous Work Studying ENTZS

There is large and growing literature on ENTZ programs, and here we focus on the important econometric issues without claiming to provide an exhaustive review.[33] Generally, previous studies used either a double difference approach like random growth framework (15) 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 were 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

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[32] 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.

[33] See also the excellent surveys in Papke (1993) and Engberg and Greenbaum (2004).

empirical work 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) and (15) above and 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.[34] 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.[35] In this case the CIA would be violated. Of course, every study will have to make an exactly identifying

[34] 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).
[35] 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.

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.

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.[36] 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 in 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.

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 this draft of our paper, Neumark and Kolko (2008) use an ingenious and complex process (see their discussion on p.11-18) to

[36] 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.

construct *annual* Census tract data on employment for California to analyze the effect of ENTZ designation. Since we rely on the Census, data at the tract level are only available by decade, but on the other hand because we use Census data we can measure the effect of ENTZ designation on several other labor market variables. Neumark and Kolko use a first difference model and consider two comparison groups. First, analogous to our use of the closest NENTZ, they use a small area near, but not in, the ENTZ to form a comparison group. Their second, and preferred, comparison group, similar to that of Busso and Kline (2007) discussed below, consists of tracts in ENTZ that have been designated in the past or that will be designated in the future. These latter tracts 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 those tracts that need help most, the comparison group will be stronger than the treatment group. 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. Note that they also must assume that their treatment and control groups share *common linear trends*, as well as common quadratic and higher order trends. Neumark and Kolko find no effect of ENTZ designation on employment in California. Interestingly, while we also find no employment effect in California, we find that ENTZ designation significantly reduces the unemployment rate and significantly increases the fraction with wage and salary income in this state.

3.6.2 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;[37] 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 1990's, which may make it difficult to estimate

[37] However, it should be noted that Busso and Kline do not find significant treatment effects in a placebo exercise using 1990 outcomes minus 1980 outcomes..

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.[38] 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.[39] 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 the use of this data will make the treatment and comparison groups less similar and thus make it harder to achieve the CIA. Finally, 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 CIA assumption made concerning the differences between the placebo zones and the EMPZs, so one is essentially testing one CIA by invoking another.

The only study of ENTCs that we are aware of is the HUD (2001) study based on a HUD survey of businesses located in the ENTCs. The survey covered the first 5 years of the program, from 1995-2000, and found that businesses were in deed utilizing the benefits of being in an ENTC. However, the study made no attempt to assess the economic impacts of the ENTC designation.[40]

4. ***Data***

4.1 Data for the Analysis of ENTZs

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[38] Busso and Kline do allow for this correlation in calculating standard errors.

[39] 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*.

[40] See US Department of Housing and Urban Development (2001). "Interim Assessment of the Empowerment Zones and Enterprise Communities (EZ/EC) Program: a Progress Report." (November).

Our data, based on 2000 Census tract definitions, consists of tracts that were designated as i) an ENTZ in the 1990's but not as an EMPZ or ENTC in either the mid 1990s or 1999, resulting in approximately 1300 ENTZ Census tracts[41] 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. (We also present results using all of the overlapping tracts below.) 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 4000 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.

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.[42] 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 1300 tracts being used; ii) the average of the outcome variable for NENTZ tracts in the same state that border the ENTZ, which resulted in about 3100 Contiguous NENTZ tracts being used, resulting in about 3100 tracts being used; and iii) all NENTZs in the same state as the ENT, resulting in approximately 25,000 tracts being used. We use these comparison groups to proceed with the analysis described above in sections 3.2, 3.3 and 3.4 respectively.

4.2 Data for the Analysis of EMPZ and ENTC Programs

We have approximately 240 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

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[41] We say 'approximate' or 'about' since the actual number of tracts used depends on the specific outcome variable because of missing values.

[42] Additional details of this process are reported in the online Data Appendix A located at

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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 760 contiguous NEMPZs iii) all NEMPZs in the same state, resulting in about 27,000 tracts being used

We have approximately 400 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 400 tracts again being used ii) average of the outcome variable for 1,300 contiguous NENTCs iii) all NENTCs in the same state, resulting in about 45,000 tracts being used.

5. Summary Statistics and Empirical Results

5.1 Summary Statistics For the ENTZ Analysis

Our basic national summary statistics for the 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 wage and salary income, real average household wage and salary income for those with positive income (in 2000 $), and total employment. In each case the standard errors of the mean values have been adjusted to allow for arbitrary heteroskedacticty 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 as a general rule, for the unemployment rate, the poverty rate, the fraction with wage and salary income and mean wage and salary income in a given year, the all-NENTZ group usually has a more favorable outcome than the contiguous NENTZ group, which in turn usually has a more favorable outcome than the nearest NENTZ (although this difference is often quite small), which in turn tends to dominate the ENTZs. There is no informative pattern for employment between the ENTZs and the various comparison groups.

Line 13 gives our national treatment effects if we assume that an ENTZ and its nearest *NENTZ* share the same *linear* and higher order trends. Line 15 gives our national treatment effects if we assume that an ENTZ and its contiguous *NENTZs* share the same specific *linear* and higher order trends, while line 17 gives our national treatment effects if we assume that all

ENTZs and all *NENTZs* share the same specific *linear* and higher order trends.[43] It is intriguing that, given this assumption for all comparison groups, ENTZ designation has a positive effect on the unemployment rate, the poverty rate and the fraction with wage and salary income, but has a negative effect on mean wage and salary income and total employment. Moreover, the vast majority of these effects are statistically significant. Taken at face value these would seem to indicate that ENTZ designation hurts a tract. However, lines 14, 16, and 18 allow us to test the respective assumption on the common linear and higher order trends between the ENTZ and the nearest, contiguous and all NENTZs respectively. Specifically they present the pre-program 1990-1980 first differences between the ENTZs and the three comparison groups and each component should be zero if the common linear trend and higher assumptions are valid. The pre-program first differences are significantly different from zero for the unemployment rate, the poverty rate, mean wage and salary income, total income and employment, indicating that the common linear and higher order trends assumption is rejected for these variables, and explaining the negative program effects for these variables in lines 13, 15, and 17. On the other hand, none of the respective assumptions is rejected for these groups for the fraction with wage and salary income. Given this, we now move to our DDD estimates, which do not require this assumption for any comparison group.

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 results for the Hausman tests are provided in our online Extra Results Appendix B, Table B1.[44]

The RE estimation results for the case when we eliminate program overlap and estimate average national and state-specific impact of ENTZ designation on the unemployment

[43] The difference between the first difference in the ENTZ and NENTZ is based on a regression where we include state dummies and an ENTZ dummy variable.

[44] This table is available at xxx.

rate are in columns 1 and 2 of Table 2 respectively.[45] The comparison group row shows which comparison group was chosen by the Hausman tests, so for columns 1 and 2 we are using estimates based on a comparison group of contiguous NENTZs. Here we see that on average (across the country) ENTZ designation lowers the unemployment rate by a statistically significant amount of (approximately) 1.6 *percentage points*. For the model with separate state effects, we find statistically significant reductions in California, Massachusetts and New York of approximately 2, 2.5, and 3.2 percentage points respectively. Columns (3) and (4) give the respective estimates for the national average and state-specific models when we allow overlap between the ENTZs and the EMPZs ENTCs, and the results are quite similar to those in columns 1 and 2 except that the All NENTZs comparison group is chosen in Column 3. The bottom line of the Table gives the estimate of the correlation coefficient between tracts in the same county, and in every case in this and the Tables below, the estimated correlation is quite small.[46]

The poverty rate results are in Table 3, which follows the same format as Table 2. In other words, columns (1) and (2) present the results for the national average and state-specific models when program overlap is eliminated. We find a statistically significant average national reduction in the poverty rate by about 5.4 percentage points, as well as statistically significant reductions in the poverty rate of 7.2, 14.2, 8.2 and 9.9 percentage points for Florida, Massachusetts, New York and Oregon respectively. Again the sample including the ENTZs that overlap with other programs provides quite similar results. Now the closest NENTZ comparison group is chosen in each case.

Table 4 presents the results when our outcome variable is the fraction of households with wage and salary income. The results in column 1 suggest that ENTZ designation raises this fraction by .006 at the national level, while the results in column 2 indicate that there are significant positive effects in California, Florida and Oregon of over .002, .002 and .004 respectively. Yet again, adding the overlapping ENTZs has little effect on the estimates, and the contiguous comparison group is always chosen. Table 5 presents the results for real mean household wage and salary income for those with positive income. Interestingly, ENTZ designation has no significant effect on this variable at the national level, and only a significant positive effect in Ohio and a significant negative effect in Oregon.

One worry is that the improvements in the unemployment rate, poverty rate and fraction with positive earnings discussed above were achieved by driving out the least able

[45] The corresponding estimates for OLS estimation with clustered standard errors are in Table B2 - B6 of the online Appendix B.

[46] An estimate of rho equal to zero indicates that the least squares estimated variance of the random effect is less than or equal to zero

members of the tracts' labor markets. We investigate this by considering the effect of ENTZ designation on employment in Table 6; if the above concern is valid we should see a negative effect of ENTZ designation on total employment.[47] Instead we see an increase of about 67 individuals at the national level, but the effect appears to be too small to be seen at the state level, except in the case of Ohio where a significantly positive effect is found.

In summary, ENTZ designation is found to improve labor markets in terms of the unemployment rate, poverty rate and fraction with wage and salary income. More over this improvement is not obtained by driving out the least able individuals in a tract since ENTZ designation also raises employment, at least at the national level. Finally we find very little evidence of cross tract correlation in the same county, and that in all but one case, the All NENTZ comparison group is not the appropriate one.

5.3 Summary Statistics for Federal EMPZs and ENTC Impacts

Table 7 contains the summary statistics for the EMPZs while Table 8 has the statistics for the ENTCs. 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 somewhat better off than the average EMPZ in all years. Further, the average member of the All NEMPZ is much better off than the contiguous NEMPZs for all years. Finally, simply taking first differences to measure the treatment effect of EMPZ designation using any comparison group will lead to downward biased estimates of the effect on unemployment rates, poverty rates, average earnings, and employment, as the 'placebo' effects for 1990-1980 for all four comparison groups in lines 14, 16, and 18 are statistically significant.

Table 8 indicates very similar patterns for the ENTCs. With regard to the NENTCs, from Table 8 we see a similar picture as found in Table 7 – the nearest and contiguous NENTCs are somewhat 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 contiguous NENTZs. Finally, from lines 14, 16, and 18 the 'placebo' effects from 1990-1980 for all three comparison groups are statistically significant for all five outcomes, again indicating that using first differences, as opposed to double differences, is not appropriate.

5.4 Estimated Treatment Effects of EMPZ and ENTC Designation

[47] Note that one does not want to control for employment when calculating the other outcome effects because employment is post-treatment and potentially affected by ENTZ designation. In this case the conditions for consistent parameter estimation are extremely stringent (Flores-Lagunes and Lagunes, 2008) and are likely to be considered unrealistic by policy makers.

We 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.[48] Since these are both Federal programs we consider only national effects. Table 9 presents the RE estimates of the effects of these programs on the unemployment rate. From columns (1) and (3) we see that we use a sample that does not overlap with ENTZs, EMPZ and ENTC designation reduces the unemployment rate by about 8.2 and 2.8 percentage points respectively, and both estimates are very statistically significant. When we ignore overlap with ENTZs, the effect falls somewhat for the EMPZs but increases slightly for the ENTCs. Note that this can go either way, since the positive effect of ENTZ designation should raise the effect (in absolute value), but the fact that ENTZs are much off, on average, than EMPZ or ENTC tracts would tend to diminish the effect. Finally, while the estimates of cross tract correlation remain small, the data pick the All NEMPZ and all NENTC comparison groups in each case.

The results for the poverty rate are in Table 10. The estimate of the effect of EMPZ and ENTC designation for the no-overlap sample are contained in columns (1) and (3) respectively, and suggest that the programs reduce poverty by about 7 and 19.5 percentage points, respectively. Further, allowing for overlap again slightly decreases the EMPZ effect and slightly increases the ENTC effect. The ENTC effect on the poverty rate seems implausibly large until one considers that the average poverty rate in 1990 for the ENTC tracts reported in Table 8 was over 55 percent. However the 1990 poverty rate for EMPZs was over 60 percent, so there still remains a puzzle as to why ENTC designation is having a much bigger impact on the poverty rate than EMPZ designation, especially since the reverse was true for the unemployment rate in Table 9. Finally, the Hausman tests always choose the closest or congruent comparison groups.

The results for the fraction with wage and salary income are in Table 11. The results for the sample with no overlap in columns (1) and (3) suggest that EMPZ and ENTC designation raise the fraction with wage and salary income by about 1.6 and 4.6 percentage points, respectively. However, only the latter effect is statistically significant. Again the results are relatively similar when we allow overlap with ENTZS in columns (2) and (4). As in Table 10, it is unclear why the ENTC effects are bigger, since the ENTC tracts have a higher value of this outcome in 1990. The Hausman tests pick the congruent comparison NEMPZ groups for the EMPZ analysis but pick the all NENTC comparison groups for the ENTC evaluation.

[48] The Hausman tests for the EMPZs and ENTCs are in Tables B7 and B8 of the online Appendix B. The OLS estimates with clustered standard errors are in Tables B9-B13 of this Appendix. The results when we include the EMPZs and ENTCs established in 1999 are in Tables B14-B18 of this Appendix.

The estimated impacts of EMPZ and ENTC designation on real average wage and salary income for those with positive values are reported in Table 12. The estimated impacts from the no-overlap samples for EMPZ and ENTC designation are contained in columns (1) and (3) and suggest a positive impact of approximately $5900 and $3500 for EMPZ and ENTC designation on this variable. The results for the sample that includes tracts that overlap with ENTZs are in columns (2) and (4); the differences with column (1) and (3) seem large in absolute terms but not when compared to the standard errors of the estimates. Lastly, all estimates are again very statistically significant, and the Hausman tests always choose the contiguous comparison groups for this outcome variable, except in column (1) where the All-comparison group is found to be appropriate.

Finally, the estimated program effects on employment are presented in Table 13. We see that the estimated impact of EMPZ designation is increased employment of about 232 people, while the estimate outcome for ENTC designation is 90 jobs. When we include tracts that are also ENTZs, our EMPZ estimated impact falls slightly while the estimated ENTC impact rises increases. Again all estimates are statistically significant, and now the Hausman tests choose for comparison groups all tracts that are not affected by one of these three programs.

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. Finally, the estimates of cross tract correlation continue to be small, but the All comparison group is chosen much more frequently than in our analysis of 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 larger in absolute value, perhaps because they are implemented in much more disadvantaged labor markets. Especially for our analysis of ENTZs, we generally find that a less conservative approach, which uses all of the Census tracts in a state that do not have the respective designation,, is almost always rejected by the data.

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 20 percent of EMPZs and 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 positive effect on local labor markets, while most previous did not find any significant impact. In addition, the Federal EMPZ program has received less attention in the literature, and the studies that do consider these programs produce conflicting results, perhaps because of an identification problem that arises with propensity score matching in this case. Further, we know of no previous work that investigates the impact of the Federal ENTC program. Using a common methodology, we find that all of these programs significantly improve local labor markets.

7. References

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Figure 1a: Zip Codes Intersecting Los Angeles Central ENTZ

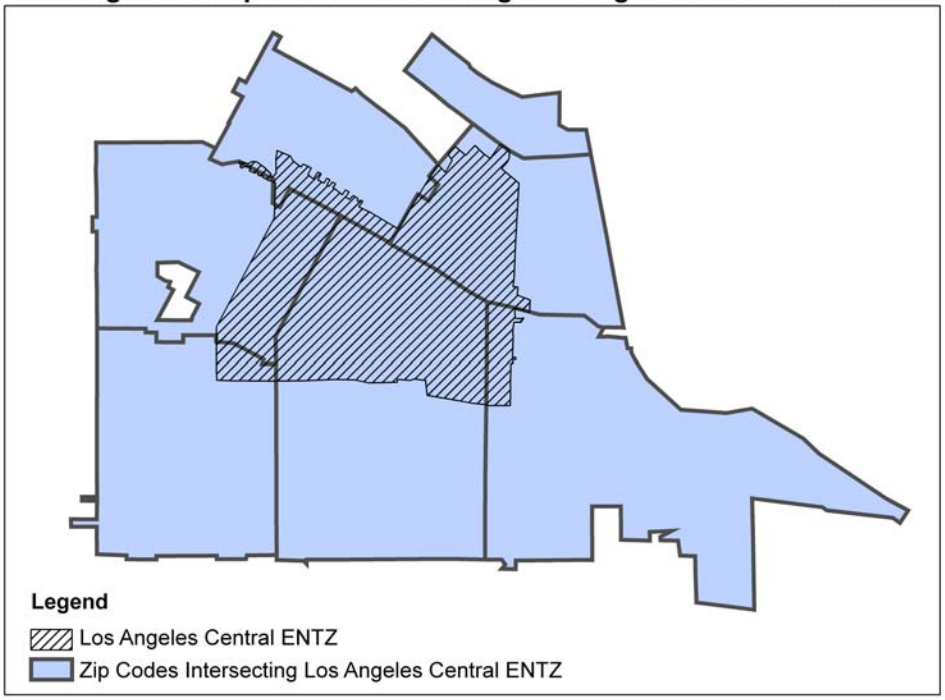

Figure 1b: Census Tracts Intersecting Los Angeles Central ENTZ