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Government programs can improve local labor markets: Evidence from State Enterprise Zones, Federal Empowerment Zones and Federal Enterprise Community ☆

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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.[1] 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.[2] 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.[3] In this paper we use a common methodology to evaluate the labor market impact of each of these programs.

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

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

[3] *Projected Tax Expenditures Budget, 2004–2010*. Tax Policy Center, 2004.

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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.[4] 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.[5]

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 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.[6] 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.[7,8]

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 an 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.

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. 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 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.

[4] 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.

[5] 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.

[6] 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.

[7] 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.

[8] For other ENTZ studies, the reader is referred to Bartik (2004), Boarnet (2001), Boarnet and Bogart (1996), Bondonio (2002), Brunori (1997), Erickson and Friedman (1990), Jones and Manson (1982), Lambert and Coomes (2001), O'Keefe (2004), and Peters and Fisher (2002).

[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).