Enterprise Zones, Poverty, and Labor Market Outcomes: Resolving Conflicting Evidence

David Neumark, Timothy Young

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Abstract

This paper revisits an important analysis of enterprise zones (EZs) by Ham, Swenson, Imrohoroğlu, and Song (2011), who report substantial poverty reductions from state and federal EZs, as well as improvements in other labor market outcomes. In our re-analysis, we find that a data error accounts for a large share of the estimated impact of state EZs in reducing poverty. More generally, we find that both state and federal EZs appear to be endogenously selected based on prior changes in poverty and other labor market outcomes. Once we account for this selection, much of the evidence that state and federal EZs reduce poverty largely evaporates, as does most of the evidence for other beneficial effects of enterprise zones, with the main exception of some limited evidence for federal Empowerment Zones. Thus, we confirm the more widely-prevailing view that EZs – and especially state EZs – have for the most part been ineffective at reducing urban poverty or improving labor market outcomes in the United States.

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I. Introduction

Research on the effects of enterprise zones in the United States has often failed to find evidence of beneficial effects on labor market outcomes – including reducing poverty. Neumark and Simpson (2015) review a number of recent studies reaching this conclusion, including Freedman (2013), Hanson (2009), and Reynolds and Rohlin (2015). Two exceptions are Busso et al. (2013), who find very large impacts of federal Empowerment Zones (EMPZs) on job growth and employment, and Ham, Swenson, Imrohoroğlu, and Song (2011, hereafter HSIS) who estimate the effects of both state and federal enterprise zones (EZs), and conclude that both types of EZs (and two different types of federal EZs) significantly improved labor market outcomes. Summary estimates from a number of these studies are reported in Table 1.[1] For example, HSIS estimate that state enterprise zones (ENTZs) reduced poverty by 6.1 percentage points (their Table 2), and federal EMPZs and Enterprise Communities (ENTCs) reduced poverty by 8.8 and 20.3 percentage points, respectively (their Tables 12 and 16).[2]

The large effects reported by HSIS, for a broad set of outcomes and multiple types of EZs, prompted us to re-analyze the data and methods they used. One issue we uncovered, which we mention only briefly in this paper, is a serious error in the construction of tract-level poverty rates in their data. This error accounts for a large share of the estimated impact of state EZs in reducing poverty that HSIS find. The last row of Panel B of Table 1 reports corrected estimates, from Ham et al. (2018).[3]

The second issue, which is more substantive and has potentially important implications for the broad research literature on enterprise zones, is that the data on state and federal zones suggest

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[1] A potential advantage of the state-level estimates in HSIS is that they pool across many state enterprise zones, which can provide improved precision relative to some of the studies of individual states.

[2] Given that poverty rates in areas that states designated as EZs averaged around 18%, and poverty rates in areas designated as federal EMPZs or ENTCs averaged around 48% and 40%, respectively, these estimated poverty reductions are extraordinarily large. The same is true for other outcomes HSIS study; for example, they estimate that federal EMPZs reduced unemployment rates by 8.7 percentage points (their Table 12).

[3] The original documentation and analysis of this error is presented is Neumark and Young (2017).

strong selection of areas that experienced negative changes prior to ENTZ, EMPZ, or ENTC designation, the recovery from which could explain the large poverty-reduction and other beneficial effects that HSIS estimate – especially the large poverty reductions for the federal EMPZs and ENTCs. This issue is our focus. Given that the estimates in HSIS are based on a triple-difference strategy comparing 1990 to 2000 changes to 1980 to 1990 changes, between tracts designated as enterprise zones between 1990 and 2000 and those that were not designated, the 1980 to 1990 trends are critical, and differences in these trends between treatment and control tracts could seriously bias their estimates.

When we limit the control tracts used in HSIS's EMPZ or ENTC analysis to more-comparable areas that applied for and were rejected as zones, or became zones in the future, we find smaller estimates than those in HSIS – although the estimates still indicate that EMPZs, and to a lesser extent ENTCs, reduced poverty and had other beneficial effects.[4] These limited controls tracts better match the pre-treatment trends in treated tracts compared to any of the alternative sets of control tracts that HSIS use.

In our preferred specifications, however, we use a propensity score matching estimator to match on levels in both pre-treatment years (and hence also implicitly on changes between 1980 and 1990) in labor market indicators. This estimator selects controls with pre-treatment trends that are even more similar to treated tracts than the rejected and future zones. Our re-analysis accounting for differences in pre-trends results in estimated effects of enterprise zones in the United States that are much more consistent with most of the findings from past research. For example, if we focus on the poverty rate, then the triple-difference estimated effect of state EZs using contiguous tracts (the estimator selected by HSIS's approach) is a significant 2.0 percentage

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[4] The poverty-reduction and other estimated effects of EMPZs, although considerably more modest than HSIS's original estimates, are consistent with the employment findings of Busso et al. (2013) for these same types of federal zones.

point decline in the poverty rate, versus an insignificant 0.6 percentage point increase using the matching estimator. For federal EMPZs, the triple-difference contiguous-tract estimate (again, the selected one) indicates a significant 8.2 percentage point decline in the poverty rate, versus an insignificant 1.4 percentage point decline using the matching estimator. And for federal ENTCs, the triple-difference estimate using all tracts (the selected estimator in this case) indicates a significant 11.9 percentage point decline in the poverty rate, versus an insignificant 1.6 percentage point decline using the matching estimator. The results differ about as sharply for the effects on the unemployment rate. The triple-difference estimates are negative and significant for all three programs; a 2.1 percentage point decline for state EZs, a 10.2 percentage point decline for federal EMPZs, and a 2.8 percentage point decline for federal ENTCs. In contrast, the matching estimator yields estimates of −1.1 percentage points for state EZs (significant), −2.6 percentage points for federal EMPZs (significant), and −0.3 percentage point for federal ENTCs (insignificant).

Based on these estimates and those for other outcomes we study, there is at best some evidence of modest beneficial effects. Our research has implications for the broader literature on enterprise zones, implying that we should be wary of estimates that do not account for differences in prior trends between treated areas designated as enterprise zones, and control areas.

II. HSIS's Data, Methods, and Error in Measuring Poverty

Data

HSIS, in their original (2011) paper, use Census tract-level data from different sources.[5] Specifically, their data for 1980 come from the historical data archive at the Center for International Earth Science Information Network (CIESIN) at Columbia University. Their 1990 data are from Applied Geographic Solutions (AGS) in Thousand Oaks, CA.[6] And their 2000

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[5] This information comes from on-line Appendix A, an unpublished appendix to their paper, referenced in HSIS (2011), for which the url cited in the published paper no longer exists.
[6] AGS was subsequently changed to CIESIN.

Census data are from the SF-3 file from the U.S. Census Bureau.[7]

Methods

The key results in HSIS are based on a triple-difference (DDD) estimator. HSIS study

zones created between 1990 and 2000. They compute the difference in outcomes between 1990

and 2000 for tracts where zones were created, which we denote $\Delta Y_{cs1}{}^{T}$, where Y is the dependent

variable of interest, the c subscript denotes Census tracts, the s subscript denotes states, the 1

subscript denotes that the difference is for the post-treatment period, and the T superscript denotes

that this difference is computed for treated tracts. They subtract from this the pre-treatment

difference – between 1980 and 1990 – for the same tracts, $\Delta Y_{cs0}{}^{T}$.

HSIS use three different control tracts: the nearest tract, the average over all contiguous

tracts, and then simply all potential control tracts in the state that are also not designated as federal

EMPZs or ENTCs. The estimation strategy diverges depending on which control tracts are used.

For the nearest and contiguous tracts, there is a control "tract" matched to each treated tract – a

single tract in the case of the nearest tract controls, and an average tract in the case of the

contiguous tract controls (averaging across the set of contiguous tracts); the nearest or contiguous

tracts are in the same state as the treated tracts. In these two approaches, they construct a

difference-in-difference for the control tract matched to each treated tract c, $\Delta Y_{cs1}{}^{C} - \Delta Y_{cs0}{}^{C}$. To

estimate a common effect of state EZs using these two types of controls (and similarly when they

estimate the effects of federal zones), they form the triple-difference (DDD) as the difference

between the two double-differences, and estimate a simple regression of this DDD on an intercept

(using random county effects), as in:

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[7] Ayse Imrohoroğlu provides data on her website for the purposes of replicating HSIS's analysis. See
http://www-bcf.usc.edu/~aimrohor/links.htm (viewed October 19, 2017). All of our subsequent analysis in
this paper is performed using the NCDB data, since this is clearly the data set of choice for studying Census
tracts over time, as discussed below. In results available from the authors upon request, we show that the
estimates using their corrected data or the NCDB data are very similar.

$$\{\Delta Y_{cs1}{}^T - \Delta Y_{cs0}{}^T\} - \{\Delta Y_{cs1}{}^C - \Delta Y_{cs0}{}^C\} = \beta + \varepsilon_{cs} \ .^8 \tag{1}$$

This DDD estimator identifies the effects of EZ designation from the change in the dependent variable in treated tracts from 1990 to 2000 minus the change from 1980 to 1990, relative to the same difference-in-difference in control tracts.

The third approach, using all non-EZ tracts in the state as controls, is slightly different, but conceptually the same. Here, they simply form the double-difference for every tract and estimate a regression with an EZ dummy variable equal to one for the designated tracts. Letting EZ_{cs} denote a dummy variable for tracts designated as zones between 1990 and 2000, the model is just:

$$\{\Delta Y_{cs1} - \Delta Y_{cs0}\} = \alpha + \beta EZ_{cs} + \varepsilon_{cs} \ . \tag{2}$$

This is still a DDD estimator, but now the number of observations is the number of tracts, rather than the number of designated EZs.[9] HSIS include state dummy variables in equation (2), which essentially treats all treated and control tracts in a state as a matched pair.[10]

Using all tracts in the state as controls would be expected to provide more precise estimates, although with the potential for more bias if these controls are less similar to the treated tracts than are nearby tracts. Under the assumption that the most restrictive set of control Census tracts (nearest neighbors) provides unbiased estimates, then a Hausman test (1978) can be used to determine whether the model using a broader set of control tracts can be presumed to also produce consistent estimates. If so, then the broader set of control tracts can be viewed as valid, but if not, the differences can be attributable to an invalid set of controls and the authors then retain the more

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[8] The notation is different from that in HSIS.
[9] HSIS also consider using the second-nearest tract to each zone designated as an EZ, to avoid bias from spillovers (see Hanson and Rohlin, 2013). However, they find little evidence of spillovers or of substantial differences in results, so we ignore this alternative strategy.
[10] Note that HSIS could have implemented the estimators with the nearest or the (average of) contiguous tracts as controls using equation (2). There would then be double the number of observations, and one could introduce a fixed effect for each pair of treated and matched control tracts. However, these fixed effects drop out of the triple-difference in equation (1).

restricted set of control tracts.[11]

Impacts of their error

We illustrate the data error in HSIS and how it affects their results by comparing their

estimates to replications using the Neighborhood Change Database (NCDB). A difficulty in

constructing tract-level observations over time using decennial Census data is that many Census

tract boundaries change over time, depending on population change. The NCDB provides

consistent tract definitions over time.[12,13]

HSIS's data have a substantial error for 1990 poverty rates.[14] As explained above, their

estimators are based on comparisons of 1990-2000 changes to 1980-1990 changes. Serious

mismeasurement of the poverty rate in 1990 influences both changes and can lead to substantial

bias in the estimates – and indeed it does, especially for the effects of state enterprise zones

(ENTZs).

Panel 1 of Table 2 documents the key issue – that the original HSIS data exhibit

pronounced spikes in the 1990 poverty rate. In the "Treated" column in Panel 1, the poverty rate

in the NCDB across 1980, 1990, and 2000 is 16.78%, 17.79%, and 18.33% – i.e., quite stable –

while in their data it is 16.41%, 25.67%, and 17.95%. The poverty estimates using the NCDB and

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[11] Formally, the assumption is that the treatment and control tracts share the same quadratic and higher-order trends, and the same double-difference in any explanatory variables. HSIS use the random effects estimator in computing the Hausman tests to select the preferred estimator. In addition, when the estimator that is supposed to be more efficient (but is potentially more biased) has a larger standard error (which can happen in finite samples, even under the null hypothesis), they simply reject that estimator, since it has a greater risk of introducing bias.

[12] See http://www.geolytics.com/pdf/NCDB-LF-Data-Users-Guide.pdf (viewed October 27, 2017). As already noted, HSIS did not use the NCDB data, and matched tracts over time using other procedures. They provide some explanation in their on-line Appendix A.

[13] The perception of researchers that the NCDB matches tracts well is reflected in the extensive – indeed, nearly exclusive – use of the NCDB data in research that matches tracts over time to study enterprise zones, other local programs, and different questions entirely. See, e.g., Oakley and Taso (2006, 2007) and Meltzer (2012) on enterprise zones, and Cameron and McConnaha (2006), Card et al. (2008), Easterly (2009), and Logan and Zhang (2010) on a range of other topics.

[14] This was originally documented extensively in Neumark and Young (2017), where we also attempt to uncover the source of the error. But we show that the error is clear, as even the population counts for 1990 Census tracts are off by an order of magnitude.

HSIS's original data are very similar for 1980 and 2000, but not for 1990. Moreover, this spike is particularly pronounced in the tracts that were designated as state enterprise zones between 1990 and 2000, relative to the three alternative control groups shown in the remaining columns of the table.[15] The fact that the error differentially affected poverty estimates in treated and control tracts is important because if the spike had affected both treated and controls equally, the errors in 1990 poverty might be approximately differenced out and still produce unbiased triple-difference estimates.

Panel 2 of Table 2 shows that HSIS's data error affected their estimates of the effects of ENTZs on poverty rates.[16] For example, HSIS's triple-difference specification estimating the effects of ENTZs on poverty uses the nearest control tract (selected, in their paper, using a Hausman test), and yields an estimated effect of ENTZ designation of a 6.1 percentage point reduction in poverty, which is roughly four times larger than the 1.65 percentage point reduction using the NCDB. As we show in the other columns of the table, the triple-difference estimates using the HSIS and NCDB data are highly statistically different, regardless of which control group is used.[17,18]

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[15] Table 2 compares HSIS's data with NCDB data. We also find the same issue when we go back to the original data sources HSIS used and extract the data, and the same error has been confirmed, after we pointed it out, in HSIS (2018).

[16] In some cases, we were not able to exactly replicate their published estimates using their data. These cases are identified by shading, with their published estimates in brackets. As noted above, the last row of Table 1 shows the corrected estimates that Ham et al. (2018) reported in response to our pointing out their data error.

[17] Because we do not know the covariance between the estimates using the two data sources, we test for equality between the triple-difference estimates from the NCDB and HSIS data using the Cauchy-Schwarz inequality to assign the covariance between the two estimates that maximizes the variance of the difference between the estimates – i.e., the most conservative test statistic for testing the null of equal estimates. The resulting *t*-statistics for the estimates using the nearest, contiguous, and all control tracts are 6.81, 17.65, and 25.15, respectively.

[18] In Neumark and Young (2017), we show that there is a similar problem (although perhaps not as extreme) for the estimation of the effects on ENTCs on poverty, while the data error has little impact on the estimated effects of EMPZs on poverty. There is no reason to expect the same bias for each kind of enterprise zone, as it depends both on what actually happened to poverty in the treated and untreated zones, and the pattern of errors relative to the correct data. Indeed, the error in HSIS's data is simply one of

III. Evidence of Ashenfelter Dip in Enterprise Zones Designation

The NCDB data in Table 2 indicate that in the tracts designated as state enterprise zones (ENTZs), there is some evidence of increases in poverty rates from 1980 to 1990. In contrast, among the potential control tracts, poverty fell from 1980 to 1990 in the nearest and contiguous control tracts, although it rose (albeit by less than in the treated tracts) when we look at all untreated tracts in the state, from 10.72% to 11.41%. These differences in the correct data are less dramatic than what appeared in the data with 1990 poverty measured incorrectly, but still point to the possibility of different pre-treatment trends in treated versus control tracts.

From this point, we switch to using exclusively the NCDB data. In Panels 1-4 of Table 3 we explore the issue of pre-treatment changes for treatment tracts, and the control tracts discussed thus far, for all five outcomes. This table covers state enterprise zones (ENTZs). Panel 1 shows that there were large increases from 1980 to 1990 in the unemployment rate, the poverty rate, average wage and salary income, and employment in the tracts designated as ENTZs between 1990 and 2000. In Panels 2-4, we show the difference between these 1980 to 1990 changes between the alternative control tracts and the treated tracts. The changes in the treated tracts were often quite different. In Panels 2-4, we also indicate the statistical significance of the differences, between the treated and control tracts, in the changes from 1980 to 1990. For example, looking at the estimates for poverty in Panel 2, the difference of −1.444 reflects the fact that there was a decrease in poverty in the nearest control tracts from 1980 to 1990 of 0.36 percentage points, which is significantly less (at the 1% level) than the 1.09 percentage point increase in the treated

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extracting the incorrect tract-level poverty rates for 1990, rather than an error mechanically linked to treatment status. The reason there is less bias from the erroneous measurement of poverty in the 1990 HSIS data for federal EZs is twofold. First, for the nearest and contiguous comparison tracts, the spike in poverty rates in 1990 in the HSIS data is almost as large as for the treated EMPZ tracts, so there is similar bias in the DD estimates for the designated and control tracts, much of which nets out in the DDD estimate. Second, and importantly for the remainder of this paper, unlike for the state EZ tracts, there is a spike (albeit smaller) in the NCDB data in 1990 poverty rates for the federally-designated tracts.

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tracts. Panels 2-4 show consistent differences between the treatment tracts and the different sets of potential control tracts for the unemployment rate, which increased significantly less in the nearest and contiguous control tracts than in the treated tracts, and declined slightly in the "all" control tracts. (We return to Panel 5 later.)

Tables 4 and 5 show that there is a similar, and in some cases more severe, issue with federal EZs – both EMPZs and ENTCs. For example, in Table 4, the unemployment rate increased 6 percentage points in the EMPZ treated tracts from 1980 to 1990, but far less in any of the potential sets of treated tracts in Panels 2-4 (and the differences are strongly statistically significant). And in Table 5, the 1980 to 1990 increase in the unemployment rate and the poverty rate was significantly larger in the ENTC treated tracts than in any of HSIS's potential sets of control tracts.

For the other outcomes, the differences in the pre-treatment trends are also often different, although not always in the same direction. However, we have indicated in boldface in Tables 3, 4, and 5 the control tracts that HSIS used based on their Hausman tests. Here, there is often a clear indication that they chose control tracts with less evidence of worsening outcomes (or stronger evidence of improving outcomes) from 1980 to 1990 – the pre-treatment period – than in the treated tracts. In particular, their choice of control tracts with less evidence of worsening outcomes is apparent for the unemployment rate and the poverty rate for ENTZs, the unemployment rate, the poverty rate, average wage and salary income, and employment, for EMPZs, and the unemployment rate, the poverty rate, the fraction of households with income, average wage and salary income, and employment (i.e., all five outcomes), for ENTCs.[19]

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[19] There is an interesting implication of this with regard to the Hausman tests HSIS use to select among the three alternative control groups – nearest tracts, contiguous tracts, and all non-treated tracts in the state. The idea behind their Hausman test is that, under the null, the nearest-tract estimator is consistent but potentially inefficient, while the other estimators are consistent and more efficient. But under the alternative, the contiguous-tract and all-tract estimators are inconsistent. Thus, they essentially see whether

The differential changes in outcomes in the pre-treatment period in the treated tracts – for unemployment and poverty, in the direction of worsening outcomes in the treated tracts – suggest that the state and federal zones designated between 1990 and 2000 were selected based on particularly bad outcomes in 1990. In other words, there appears to be an "Ashenfelter dip" (Ashenfelter, 1978) with regard to EZ treatment, with EZ designation in the 1990s based in part on negative economic shocks between 1980 and 1990 that did not affect surrounding tracts – even their closest neighbors – as strongly.[20] These differences in pre-treatment changes – or violations of the parallel-trends assumption – imply that DDD estimates of the effects of enterprise zones, using the kinds of estimators HSIS employ, may lead to biased estimates of program effects, and assuming there is regression to the mean from negative shocks, the bias would be towards finding beneficial effects of EZs.

The institutional setting of enterprise zone designation could well be responsible for the kind of negative selection/Ashenfelter dip we have documented. Typically, tracts (or other geographic areas in some states, like California) are proposed as enterprise zones, and the criteria for eligibility are associated with worse economic outcomes and the likelihood of generating improvements in these outcomes. For example, the California program, while not formulaic, designated EZs based on criteria that included multiple measures of economic distress, evidence of recent sharp local economic deterioration, or a promise of economic growth (see Neumark and Kolko, 2010). Eligibility for the federal programs required poverty rates above 20 percent, 90 percent of zone tracts with poverty rates of at least 25 percent and 50 percent with poverty rates of at least 35 percent, and tract unemployment rates above 6.3 percent (Busso et al., 2013). And,

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they can get to a larger set of control tracts by expanding the set of control tracts and doing Hausman tests. However, Tables 3-5 show that the nearest tracts often match badly on pre-trends, which invalidates the Hausman tests. This is one reason we show results for all of the sets of control tracts that HSIS consider (with some reported in our on-line appendix tables referenced below).
[20] We thank Stephen Ross for highlighting this interpretation of the evidence.

some of the other states had criteria related to lagging economic development, such as Connecticut (Ham et al., 2011). These same kinds of selection criteria also imply that it is important to match treated areas to controls with similar trends prior to EZ designation, which we turn to next.

IV. Correcting for the Ashenfelter Dip in EZ Selection

The sharp pre-treatment changes in tracts designated as EZs is a reason to be skeptical of DDD estimates of the effects of state and federal EZs, suggesting that another estimation strategy may be needed to obtain reliable estimates.

HSIS solution to Ashenfelter dip: IV

Although they did not document its importance, HSIS were concerned with the same potential for negative shocks and regression to the mean, which they address using an instrumental variables (IV) estimator.[21] For each outcome and program, they instrument *EZ* in equation (2) using long lags – i.e., 1980 values – of the other outcomes. (Recall that *EZ* is defined as one or zero for the double-differenced observations in equation (2), but designation occurs between 1990 and 2000.)[22]

The exclusion restriction is that these IVs are orthogonal to the residual of equation (2), so, for example, in the model for the poverty rate, the assumption is that the unemployment rate, fraction of households with income, average income, and employment in 1980 are uncorrelated with the transitory shock to poverty (transitory, because the model includes fixed effects). This is not a very compelling assumption, given that the 1980 outcome enters the residual (since the dependent variable is the double-difference defined over 1980, 1990, and 2000). Why, for example, would transitory shocks to any of these five outcomes not be correlated with transitory

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[21] They argue that their IV approach "avoids bias in the estimated treatment effect arising from the treated Census tracts exhibiting a regression to the mean phenomenon" (p. 780).

[22] In their IV approach, HSIS always use the framework in equation (2), creating a data set including both treated and untreated tracts, with an EZ dummy variable that equals one for the treated tracts. When they use the nearest or contiguous tracts as controls, they include dummy variables for matched treatment and control pairs, and when they use all tracts as controls they include state dummy variables.

shocks to the others?[23] In their subsequent paper correcting their poverty data and estimates, HSIS

do not present IV estimates using their corrected data because, as they write, "…we now expect

that estimates based on IV estimation combined with triple difference estimation are likely to have

quite poor finite sample properties" (HSIS, 2018).

Rejected and future federal zones as controls for federal EZ designation

For estimating the effects of federal zones (EMPZs and ENTCs), there is another set of

potentially better controls, at least on a priori grounds. Busso et al. (2013), in their paper assessing

the effectiveness of Empowerment Zones (EMPZs), assign tracts as controls if they submitted

applications in Round I of the process of applying for zones but were not granted EMPZ status

(which often resulted in designation as an ENTC), or if they submitted applications in future

rounds. The idea is that these control tracts are more likely to have shared common outcomes with

tracts ultimately awarded EMPZ status, and hence are more likely to provide valid

counterfactuals.[24]

Furthermore, Busso et al. (2013) note that in the round of enterprise zone applications

during which these federal zones were created, eight cities received Empowerment Zone

designation (became EMPZs), while "49 rejected cities were awarded smaller enterprise

communities … [became ENTCs] as consolation prizes" (p. 900, bracketed comment added).

ENTCs did not have hiring tax credits; they only received $3 million in Social Services Block

Grant (SSBG) funds and were eligible for tax-exempt bond financing. In sharp contrast, EMPZs

received $100 million in SSBG funds instead of $3 million, and had 20% hiring credits for the

first $15,000 in wages earned by each employee who lived and worked in the community, for up

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[23] Put differently, in thinking about the IV estimator for the employment equation, for example, it seems
hard to rationalize why awarding an EZ to an area depends on the past unemployment rate but does not
depend on the past level of employment; the same argument casts serious doubt on the exclusion restriction
for IV estimation of their model for each of the five outcomes.
[24] Hanson (2009) also uses rejected EMPZs as controls, albeit coupled with an IV strategy.

to 10 years (declining). The difference in benefits between EMPZs and ENTCs is so stark that

Busso et al. include the rejected zones that became ENTCs in their *controls*.[25]

Following the reasoning that rejected and future zone tracts share characteristics with

treated tracts, we explore using the rejected and future zone tracts as controls.[26] We limit these

control tracts to the intersection of the rejected and future zone tracts in Busso et al. (2013) and the

tracts that HSIS use as controls, to ensure that our control tracts were not treated by any of the

three EZ programs; hence, we exclude ENTCs as controls when we examine EMPZs.[27] Unlike for

the HSIS triple-difference estimators, we do not restrict these controls to be in the same state as

the treated zones, given that they are matched on a different feature (having been including in

zones for which applications were made).

Examining the evidence on pre-trends for these alternative control tracts, Panel 5 of Table

4 indicates that in most cases these controls match the treated tracts much better on pre-treatment

changes in outcomes (from 1980 to 1990), compared to the controls HSIS used. The one

exception is for the unemployment rate. Similarly, the estimates for ENTCs, in Panel 5 of Table

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[25] The use of ENTCs as controls is consistent with Oakley and Tsao (2006)'s description of ENTCs, which notes that "the primary benefit … was new tax-exempt bond financing to encourage business investment in distressed areas by offering lower interest rates than conventional financing" (p. 446). Hanson and Rohlin (n.d.) also suggest that ENTCs are natural control tracts for EMPZs. Ham et al. (2018) point out that both EMPZs and ENTCs had $2,400 in Work Opportunity Tax Credits (WOTC) for employed zone residents between ages 18-24. However, WOTCs were far from limited to enterprise zones; the WOTC was available to *any* employer (including those in tracts that HSIS coded as controls) who hired members of targeted groups, such as TANF recipients, previously unemployed veterans, and ex-felons. In fact, the WOTC benefits were such a minor consideration for the evaluation of EMPZs and ENTCS that they are not even mentioned as a program benefit in a 2006 report by the Government Accountability Office (U.S. GAO, 2006, p. 8). Nonetheless, in correspondence with us regarding our earlier comment (Neumark and Young, 2017), HSIS have argued that ENTCs could have an effect and therefore that ENTC designation should be considered a treatment. We thus consider the effects of ENTCs in what follows, and exclude them as controls.

[26] We have no information on rejected or future state enterprise zones, and therefore can only use these controls for our federal analysis.

[27] HSIS only assign tracts as controls if they are untreated by all three programs. To quote them: "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, … and ii) tracts that were not designated as an ENTZ, EMPZ or ENTC through 2000…" (Ham et al., 2011, p. 788).

5, indicate that the rejected and future zone tracts generally have pre-trends more similar to the treated tracts than do the HSIS controls.

Propensity score matched controls

To better match the pre-treatment trends in outcomes between the treated and control tracts, we employ a data-driven approach to select control tracts, using propensity score methods. Using this approach, we match treatment and control tracts so as to minimize the differences in pre-treatment outcomes in terms of levels and changes. We implement this procedure separately for each of the three programs using the dataset containing the full set of potential control tracts for each program.[28] For each program, we match a single control tract to each treated tract by matching on the 1980 and 1990 levels for all five outcomes (i.e., we match on a total of 10 covariates); matching on levels in both years effectively matches on changes as well.[29] Our estimator is based on a comparison of the double-difference (the 1990 to 2000 change minus the 1980 to 1990 change) between each treated tract and its matched control tract, and hence we estimate the average treatment effect on the treated (ATT).[30]

The issue of matching on pre-trends is motivated clearly by the data on treatment tracts and potential control tracts that HSIS use, but it is of course potentially relevant to other estimators that do not use differences relative to changes in a prior period. Interestingly, there is virtually no attention to pre-trends in the U.S. enterprise zone literature. O'Keefe (2004), Greenbaum and

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[28] We do not restrict potential control tracts to be in the same state as the treated tract, for the same reason as in our analysis using rejected and future zone tracts.

[29] Matching on the 1980 to 1990 change and the level for one year would yield identical results. We reasoned that matching on levels and not only changes would provide better controls than just matching on changes.

[30] Propensity score matching was performed using "teffects psmatch" command in Stata 13. Controls used for matching were the 1980 levels and 1990 levels for the unemployment rate, poverty rate, average wage and salary income, fraction with wage and salary income, and count of employed persons. The propensity score is estimated using a probit model, and we selected the option "atet" to estimate the average treatment effect on the treated.

Engberg (2004), Elvery (2009), and Busso et al. (2013) all match on levels at a single point in time (as do, e.g., Gobillon et al. (2012) in research on French enterprise zones). We have only found one study, by Bondonio and Engberg (2000), that studies effects of enterprise zones on employment growth matching on employment growth in prior periods, paralleling what we do here.[31]

A standard procedure to verify the effectiveness of the propensity score matching procedure is to do what we have already done for the other potential control groups – comparing pre-treatment means between treated tracts and controls selected using matching, with the most relevant means in our case being pre-treatment changes in the outcomes. Panel 5 of Table 3 shows that the propensity score matching procedure does a good job of matching 1980 to 1990 changes. For example, the change in unemployment from 1980 to 1990 in ENTZ tracts is 1.4 percentage points (Panel 1), compared with about 1.3 percentage points in the matched controls, resulting in the small and insignificant difference of −0.146 reported in the table. In contrast to the findings for the nearest, contiguous, and "all" control tracts, we never reject the equality of pre-treatment parallel trends using propensity score matched controls in Panel 5 at the five-percent level (we do in one case at the 10-percent level). In contrast, the bolded entries in Table 3 indicate that the parallel trends assumption is rejected for the control groups HSIS use for all five outcomes.

We present the corresponding analysis for federal EMPZs in Panel 6 of Table 4. As with ENTZs, the control tracts selected using propensity score generally match the pre-trends of the treated tracts much better. The differences are smaller than for the other potential controls (including the rejected and future zone tracts) and are never statistically significant. Again, in contrast, the bolded entries in Table 4 indicate that the parallel trends assumption is rejected for

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[31] They find a negative but insignificant effect of past growth on zone designation. The sign of their estimate is consistent with much of the evidence we report below. (This paper concludes that enterprise zones did not increase employment.)

the control groups HSIS use for four of the five outcomes, the only exception being fraction of households with wage and salary income.

Finally, in Panel 6 of Table 5, we present similar analyses for federal ENTCs. In this case, again, the control tracts selected using propensity score match the pre-trends of the treated tracts much better than the nearest, contiguous, or "all" tracts used by HSIS. The bolded entries indicate that for their analysis of ENTCs the parallel trends assumption is rejected for all five of their analyses, whereas for our analysis using propensity score matching, the assumption is never rejected.

The evidence for matching on pre-trends indicates that for all three types of zones we will obtain more reliable results using the propensity score matching than the types of control tracts HSIS used. And for EMPZs and ENTCs, we might also expect more reliable results from using rejected and future zone tracts as controls compared to their controls, although the propensity score matching yields control tracts that better match the pre-trends in treated tracts (compare Panel 5 to Panel 6 in Tables 4 and 5).

V. New Estimates of the Effects of State and Federal Enterprise Zones

Based on the preceding analysis, we present estimates for the effects of each of the three types of enterprise zones, on each of the outcomes. In the tables, we present estimates based on the preferred/selected control groups from HSIS, the rejected and future zone controls for estimating the effects of federal programs, and propensity score matching for all three types of zones.[32] In each case, we first present graphical evidence, and we then present the estimates. In the tables for the latter, we highlight the estimates for which the results in Tables 3-5 indicate that the parallel trends assumption is not rejected.

Effects of state Enterprise Zones (ENTZs)

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[32] On-line Appendix Tables A1-A3 present the triple-difference estimates for all of the potential control groups HSIS considered.

We first turn to evidence on state enterprise zones (ENTZs). As a preliminary, Figure 1 plots the average values for each outcome in 1980, 1990, and 2000, for the treated zones and for the alternative controls. For ENTZs, these are the nearest tracts, the contiguous tracts, all tracts in the states, and (for this figure), the matched tracts from the propensity score estimation. In all five panels, it is clear that the matched tracts mirror the pre-trends (and levels, although that is less important) quite well, whereas the alternative control tracts (i.e., HSIS's nearest, contiguous, and all) often do not. In some cases, the difference is notable, like for the nearest and contiguous control tracts for which the poverty rate declines from 1980 to 1990, rather than increasing. This visual evidence is consistent with the statistical evidence in Table 3 showing that the pre-trends in the poverty rate were significantly different for these two sets of controls.

Looking at the 1990 to 2000 changes in Figure 1, it is quite clear that the estimated effect of ENTZs can depend on the controls. For example, looking at poverty, the comparison between the treated tracts and the matched controls provide no apparent evidence of a reduction in poverty, but some evidence of a decline in the unemployment rate, in contrast to comparisons between the treated tracts and the nearest or contiguous controls.

The actual model estimates are reported in Table 6. To help map between the comparisons for which parallel trends hold and the estimates in this table, we have shaded in grey the estimates for which, in Table 3, we did not reject the parallel trends assumption. In this table (and Tables 7 and 8 that follow) we report the HSIS estimators chosen based on their Hausman tests.[33] A number of conclusions are clear.

First, almost all of the matched estimates (four out of five) are based on specifications for which the parallel trends assumption is not rejected. In these estimates, there is evidence that state ENTZs reduced the unemployment rate – by about 1.1 percentage points. There is no other clear

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[33] These are our replications of their estimates using the NCDB, which are very close to their (corrected, in the case of poverty) published estimates.

evidence of beneficial effects; one specification indicates an increase of 0.6 percentage point in the poverty rate, while another specification points to decline in average income by about $627, but both estimates are statistically insignificant. There is, however, a negative and significant (at the 10-percent level) estimated effect on employment.

Second, the only estimate that HSIS produce that does not fail the parallel trends assumption is for employment using nearest control tracts (see on-line Appendix Table A1). In this case, there is evidence of a slight positive effect of ENTZs (an increase of about 10), but the estimate is statistically insignificant, and HSIS's Hausman selection method does not select this estimator.

Third, of the estimated effects selected by HSIS, three show statistically significant beneficial effects of ENTZs – reducing the unemployment rate and the poverty rate, and raising average incomes. However, the parallel trends assumption is rejected for all three of these estimates. Thus, the Ashenfelter dip in selection into state enterprise zone designation appears to have led HSIS to overstate the benefits of state enterprise zones.[34] Referring back to Figure 1, the problem is particularly clear in Panel B, for poverty, where poverty is trending down prior to ENTZ designation in the nearest and contiguous tracts, but not in the matched control tracts.

Fourth, as shown in the last panel of Table 6, the matched estimates are statistically significantly different from HSIS's estimates at the 10-percent level (or less) for four out of the five outcomes.[35] In fact, for the three outcomes for which HSIS's estimator generates statistically significant beneficial effects, we strongly reject equality of the HSIS and matched estimates.

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[34] Note also that the differences in estimates are sizable relative to their standard errors. For example, the propensity score matched estimate of the effect on the unemployment rate, in Panel 2, is sufficiently precise that the estimate using contiguous controls (used by HSIS) lies outside of its 95% confidence interval (−0.47 to −1.63). The same is true for their estimated statistically significant effects on poverty and on average wage and salary income.
[35] See the notes to Table 6 for a discussion of how the standard error for the difference between the estimates is calculated.

Effects of federal Empowerment Zones (EMPZs)

We next turn to evidence on federal Empowerment Zones (EMPZs). Figure 2 shows that

for some of the outcomes, especially the unemployment rate and the poverty rate, the propensity

score matched tracts mirror the pre-trends far better than the other potential control tracts –

including the rejected and future zone tracts for the unemployment rate.[36] This is consistent with

the numbers and tests reported in Table 4. And, we can see why in some cases this will bias the

estimated effect; for example, for poverty there is little or no improvement apparent from

comparing the treated and matched control tracts, but the comparisons to the other potential

control tracts suggest poverty declined as a result of EMPZs.

These differences are reflected in the estimates in Table 7. For poverty rates, the estimate

for the contiguous control groups that HSIS use indicate a very large reduction in poverty (an 8.2

percentage point decline).[37] The comparison to rejected and future zone tracts is still large but

more modest – a 4.4 percentage point decline. However, the propensity score matched estimate is

about one-third as large as this estimate and is not statistically significant. Note that the erosion of

statistical significance is principally due to different magnitudes of the estimated effects. The

standard error of the matched propensity score estimate is about the same as the estimate using the

contiguous control tracts.

The differences are also pronounced for the unemployment rate. The estimate using all

control tracts (per HSIS) indicates a very large reduction in the unemployment rate (10.2

percentage points).[38] The comparison to rejected and future zone tracts is a bit smaller – a 6.5

percentage point decline. However, the propensity score matched estimate is much smaller – only

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[36] Again, as an example of the problems with the estimates in HSIS, their selection procedure chose the all tracts control group for the unemployment rate, but Panel A of Figure 2 shows that the pre-trend for these controls was in the opposite direction from that for the treated tracts.
[37] The estimates are similar using the nearest tracts or all tracts; see on-line Appendix Table A2.
[38] The conclusion is similar using the nearest or contiguous tracts; see on-line Appendix Table A2.

2.6 percentage points.

For the other three outcomes, the propensity score matched estimates reveal no significant effects, while the estimates using other control groups generally do. (See Table 7, and on-line Appendix Table A2 for alternative control tracts.) For all outcomes except for the fraction of households with wage and salary income, we find strongly statistically significant differences between HSIS's preferred estimates and the propensity score matched estimates.

Again, comparing the shaded and unshaded estimates in Table 7 indicates that the evidence of beneficial effects of EMPZs using the HSIS estimates is based on controls that fail the parallel trends assumption. This is true for all four of the beneficial effects estimated using their control groups – for the unemployment rate, the poverty rate, average wage and salary income, and employment. However, for the fraction of households with wage and salary income and for employment, there are two cases where the control groups they considered (but did not select) satisfy the parallel trends assumption and yield positive effects of EMPZs. (See Appendix Table A2.) Regardless, most of the evidence of beneficial effects of EMPZs that HSIS estimated – especially the very large estimated effects on the unemployment rate and the poverty rate – appear to be spurious. In contrast, there is some evidence of beneficial effects of EMPZs using rejected and future zone control tracts for outcomes for which the pre-trends are not significantly different from those in the treated EMPZ tracts.

Effects of federal Enterprise Communities (ENTCs)

Finally, we turn to the evidence on federal Enterprise Communities (ENTCs), for which one might suspect any evidence of strong beneficial effects is particularly likely to be spurious because ENTC incentives were weak – as we noted, prompting Busso et al. (2013) to include them as controls. Figure 3 shows that for all five outcomes the nearest, contiguous, and all tract controls do not track pre-trends in the treatment group well. This is particularly notable for the unemployment rate, the poverty rate, and the fraction of households with wage and salary income.

In contrast, both the propensity score matched controls and the rejected and future zone tract controls track pre-trends quite well in all five cases – although the matched controls generally do better.

This evidence on the parallel trends assumption is reflected in the shading in Table 8. For the control tracts chosen by HSIS, the parallel trends assumption is rejected (and hence the estimate is not shaded) in every case;[39] in contrast, for the matched controls it is never rejected.

These differences between treatment and control group pre-trends influence the estimates in striking fashion. In Table 8, using the estimators HSIS select, the evidence points to strong and statistically significant beneficial effects of ENTCs – such as the 2.8 percentage point reduction in the unemployment rate and the 11.9 percentage point reduction in poverty.[40] However, recall that for all of these estimates the parallel trends assumption is rejected. In sharp contrast, none of the estimates using the matched controls yield statistically significant evidence of beneficial effects at the five-percent level, and the estimated effect on employment is negative and significant (at the 10-percent level). Only one of the three estimates based on rejected/future zones that satisfy the parallel trends assumption provides evidence of a beneficial effect (a reduction in poverty, significant at the 10-percent level).[41] Moreover, all of the propensity score matched estimates are statistically significantly different from the preferred HSIS estimates.

Summary

All told, we draw five conclusions from the estimates in Tables 6-8 (and Figures 1-3). First, our propensity score matched control tracts mirror the pre-trends in tracts enacted as enterprise zones rather well – substantially better than the nearest, contiguous, and "all tract"

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[39] As shown in on-line Appendix Table A3, it is rejected for every estimator – using nearest, contiguous, or all tracts as controls.
[40] The same is true for the other control tracts; see Appendix Table A3.
[41] Again, the different conclusions are due to different estimated magnitudes of the effects, not changes in precision of the estimates.

control groups that HSIS used, and also generally better than the rejected and future zone tracts.

Second, for every type of enterprise zone – state enterprise zones (ENTZs), federal Empowerment

Zones (EMPZs), and federal Enterprise Communities (ENTCs) – the evidence of beneficial effects

is much weaker when we focus on control tracts that track pre-trends in the treated tracts well.

Third, a little evidence of beneficial effects of ENTZs survives our analysis, with the evidence

pointing to a decline in the unemployment rate (by about one percentage point). Notably, we do

not find any evidence that ENTZs reduce poverty when we use controls that better match the pre-

trends of ENTZ treated tracts. Fourth, for EMPZs (but not ENTCs), the rejected/future tract

controls match the pre-trends for some outcomes and yield evidence of beneficial effects (but also

yield evidence of beneficial effects for outcomes for which the parallel trends assumption fails).

And finally, using propensity score matching on the pre-trends, the evidence of beneficial effects

of federal EMPZs and ENTCs becomes quite weak – with the propensity score matched estimates

providing no evidence of a beneficial effect on any outcome with the exception of evidence of an

unemployment rate reduction in EMPZs of about 2.5 percentage points.[42]

VI. Summary and Conclusions

Our analysis buttresses the conclusion of much (but not all) of the broader literature that

generally fails to find beneficial effects of enterprise zones in the United States, especially for

poverty and employment rates. This general conclusion is contradicted by recent research by Ham

et al. (2011, HSIS) claiming that state and federal enterprise zones generated large labor market

benefits, including poverty reductions. Aside from their evidence on the effects of state enterprise

zones in reducing poverty being driven by a data error, their more general evidence of beneficial

effects of both state and federal enterprise zones is largely driven by using control groups that

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[42] In on-line Appendix Tables A4-A6, we show that the propensity score estimates are robust to excluding nearby tracts as controls and dissimilar tracts as controls, to using inverse probability weighting, and to using two (instead of one) neighboring tracts as controls.

often strongly violate the parallel trends assumption. Given that their estimates are based on a triple-difference strategy comparing 1990 to 2000 changes to 1980 to 1990 changes, between tracts designated as enterprise zones between 1990 and 2000 and those that were not designated, the 1980 to 1990 trends are critical.

We show that, in many cases, the pre-trends from 1980 to 1990 are not even in the same direction in the treated and control tracts. We also show that when we use control tracts matched via propensity score to 1980 and 1990 values (and hence 1980 to 1990 changes), we are able to construct control tracts that much better track the pre-trends in treatment tracts. Finally, using these more satisfactory control tracts, most of the evidence of beneficial effects of state and federal enterprise zones evaporates. We find some evidence – but it is not always consistent – of declines in unemployment rates and poverty, but of much smaller magnitudes than HSIS found. And we would argue that if one wants to emphasize our significant findings of beneficial effects (ignoring other estimates using valid controls that find smaller and insignificant effects), the estimated magnitudes are much more plausible than those HSIS reported – a roughly one percentage point decline in the unemployment rate from state enterprise zones, and a 2.6 percentage point decline in from federal EMPZs.

Our estimates are less directly comparable to the findings of Busso et al. (2013), who also report substantial positive effects of federal Empowerment Zones. For the outcomes we study, our analysis of control tracts similar to those they used suggests that these control tracts are in some cases invalid for estimating the effects of these zones.[43] However, they are valid in some cases, and for all five outcomes, estimates based on rejected and future EMPZs point to positive effects. However, using the control tracts matching on pre-trends yields much less evidence of beneficial effects of EMPZs. We find weak or no evidence that Enterprise Communities improved labor

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[43] Their methods are a bit different. Whereas we simply use a subset of rejected and future zone tracts as controls, they restrict to a similar set of tracts, but reweight them based on a propensity score estimation.

market outcomes when we use rejected or future zone tracts as controls, and no evidence of beneficial effects using the matched controls. This is consistent with Empowerment Zones having much stronger incentives than Enterprise Communities – if either program has a beneficial effect on local labor markets, we would expect it to be Empowerment Zones. However, we caution that our results – the outcomes studied, the data used, and the methods – are less comparable to the Busso et al. analysis.

All told, our re-analysis accounting for differences in pre-trends – or an "Ashenfelter dip" prior to the designation of Census tracts as enterprise zones – leads to evidence on the effects of enterprise zones in the United States that is much more consistent with most of the findings from past research. There is at best some evidence of beneficial effects on unemployment rates, but there is some countervailing (albeit weaker) evidence of negative effects on other labor market outcomes. More generally, our analysis implies that future evaluations of place-based policies need to consider evidence on prior changes in treated and control areas that may drive selection of which areas get treated.

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Figure 1. Comparing Trends for State Enterprise Zones (ENTZs) and Potential Controls

A. Unemployment rate

B. Poverty rate

C. Fraction of h'holds with wage & salary income

D. Avg. wage & salary income

E. Employment
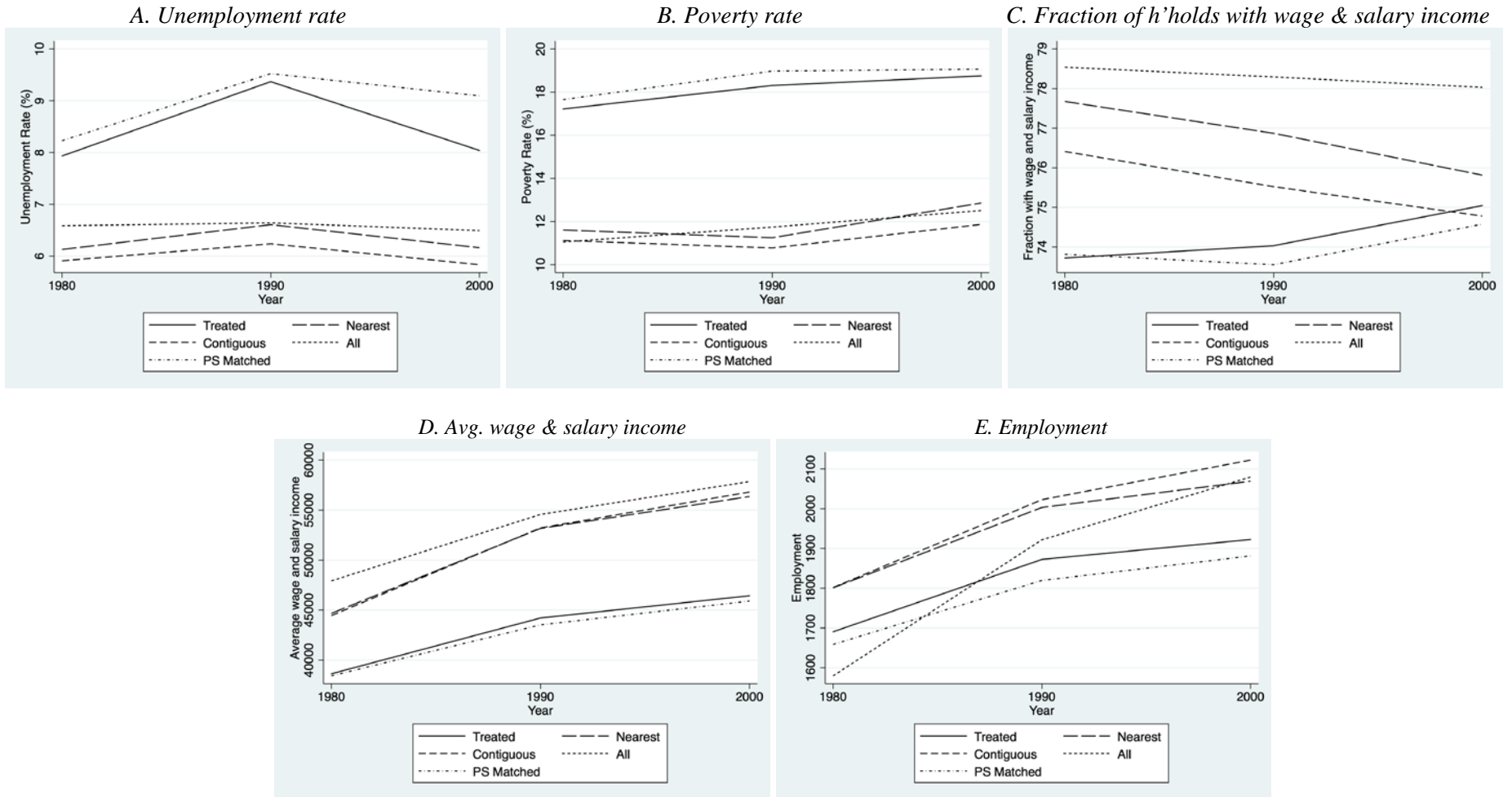
Notes: Figures are based on NCDB data. Treated, nearest, contiguous, and all control tracts are identified in the HSIS data. The "PS Matched" control tracts are identified using a propensity score matching estimator that matches on ten covariates: 1980 levels and 1990 levels for all five outcomes.

Figure 2. Comparing Trends in for Federal Empowerment Zones (EMPZs) and Potential Controls

A. *Unemployment rate*　　　　B. *Poverty rate*　　　　C. *Fraction of h'holds with wage & salary income*

D. *Avg. wage & salary income*　　　　E. *Employment*
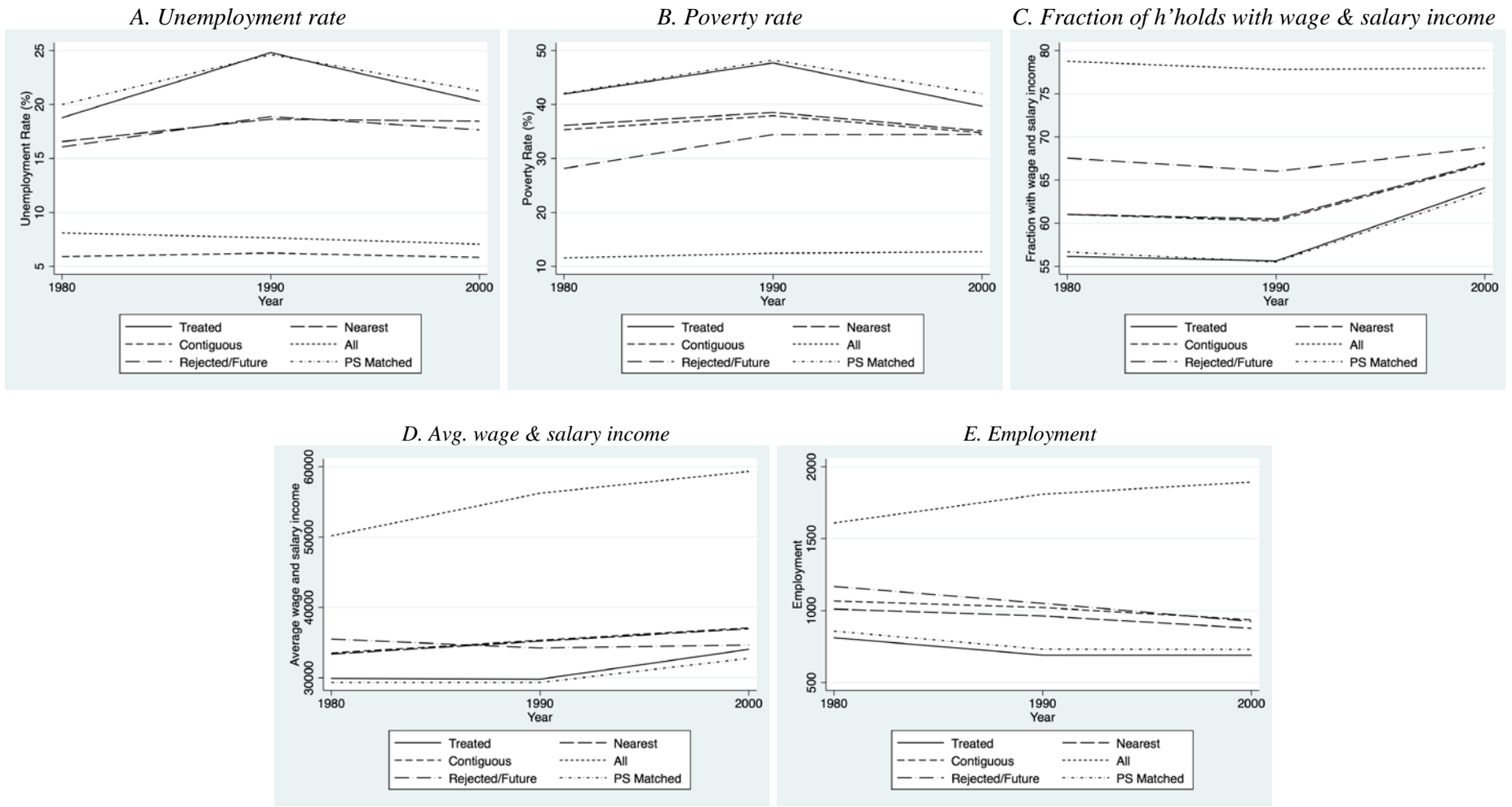
Notes: See notes to Figure 1. The rejected and future federal zone control tracts are the intersection of the control tracts identified in Busso et al. (2013) and the EMPZ controls identified in the HSIS data.

Figure 3. Comparing Trends in for Federal Enterprise Communities (ENTCs) and Potential Controls

A. Unemployment rate

B. Poverty rate

C. Fraction of h'holds with wage & salary income

D. Avg. wage & salary income

E. Employment
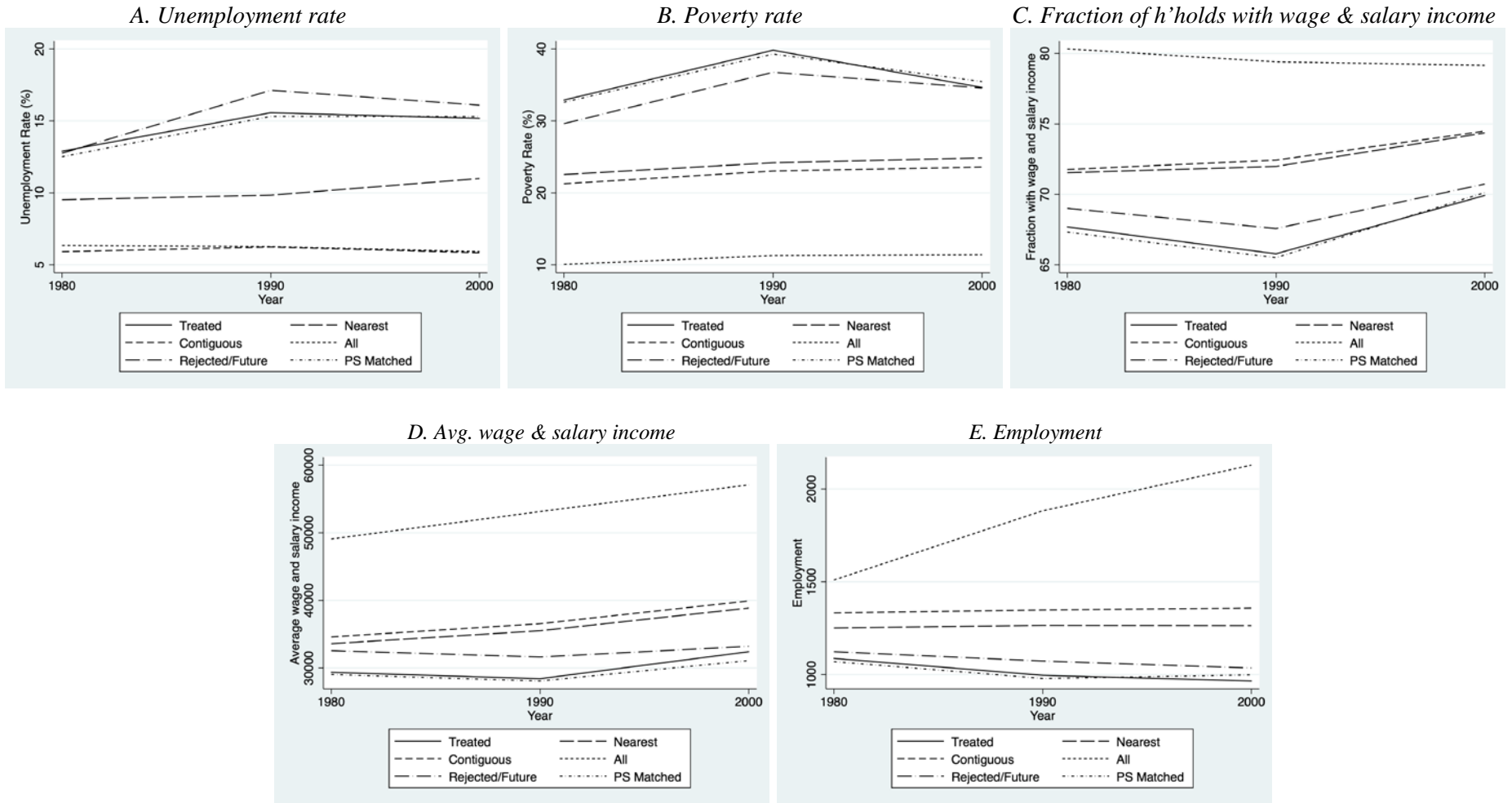
Notes: See notes to Figures 1 and 2.

Table 1: Summary of Recent Evidence on Enterprise Zones in the United States

Study	Program	Results
A. Summary of Enterprise Zone Estimates		
O'Keefe (2004)	California	**Employment growth** (annual) first six years after designation 3.1 percent; standard error 0.9 7-13 years after designation −3.2 percent; standard error 1.2
Greenbaum and Engberg (2004)	California, Florida, New Jersey, New York, Pennsylvania, and Virginia (urban)	**Employment** (annual) −0.4 percent; not significant (standard error not reported)
Couch et al. (2005)	Mississippi (counties)	**Job growth** (annual, in manufacturing) 1.4 percent; standard error ≈ 0.4
Elvery (2009)	California and Florida enterprise zones	**Employment rate** effects on zone residents: estimates for California range from −0.4 to −2.6 percent; standard errors ≈ 0.9 to 2.3 estimated for Florida from −1 to −4 percent; standard errors 1.0 to 2.8
Hanson (2009)	Federal Empowerment Zones	OLS estimates: **Employment rate** 2 percentage points; standard error 1 **Poverty rate** −2 percentage points; standard error 1 IV estimates: **Employment rate** 0 percentage points; standard error 2 **Poverty rate** 2 percentage points; standard error 3
Billings (2009)	Colorado	**Employment** estimates for existing establishments range from 0 to 0.3 employment change (≈ 0 to 2.1 percent); standard errors 0.1 to 0.2 estimates for new establishments range from 1.5 to 1.8 (≈ 10.3 to 12.3 percent); standard errors 0.7 to 0.9
Neumark and Kolko (2010)	California enterprise zones	**Employment** effects measured at establishments in zones: estimates range from −1.7 to +1.8 percent (levels); standard errors 3.5 to 7.7
Freedman (2013)	Texas enterprise zones	**Employment growth** among zone residents 1-2.2 percent per year (sometimes significant); standard errors ≈ 0.05 to 0.10 **Share with income below the poverty line**, −3.3 percentage points; standard error 3.8
Busso et al. (2013)	Federal Empowerment Zones	**Job growth** (in LBD) 12-21 percent; standard errors 4.8 to 7.2 **Employment** (in Census data) 12-19 percent; standard errors 6.2 to 8.5
Reynolds and Rohlin (2015)	Federal Empowerment Zones	**Poverty rate** −1 percentage point; standard error ≈ 0.6
B. Summary of Ham et al. Enterprise Zone Estimates		
Ham et al. (2011)	State enterprise zones, federal Empowerment Zones, federal Enterprise Communities	State programs: **Unemployment rate** −1.6 percentage points; standard error 0.2 **Poverty rate** −6.1 percentage points; standard error 1.2 Empowerment Zones: **Unemployment rate** −8.7 percentage points; standard error 0.5 **Poverty rate** −8.8 percentage points; standard error 2.8 Enterprise Communities: **Unemployment rate** −2.6 percentage points; standard error 0.3 **Poverty rate** −20.3 percentage points; standard error 2.3
Ham et al. (2018)	State enterprise zones, federal Empowerment Zones, federal Enterprise Communities	State programs: **Poverty rate** −1.7 percentage points; standard error 0.4 Empowerment Zones: **Poverty rate** −8.2 percentage points; standard error 1.5 Enterprise Communities: **Poverty rate** −11.7 percentage points; standard error 0.5

Table 2: Summary Statistics for Poverty Outcomes in State Enterprise Zone Analysis: Comparing Estimates Using NCDB Data and HSIS Data

Year	Treated NCDB	Treated HSIS	Nearest NCDB	Nearest HSIS	Contiguous NCDB	Contiguous HSIS	All NCDB	All HSIS
Panel I: Means								
1980	16.78	16.41	11.72	11.81	11.14	11.46	10.72	10.77
	(1.51)	(1.44)	(1.33)	(1.21)	(1.31)	(1.20)	(0.54)	(0.50)
				[12.90]				
N	1,176	1,245	1,176	1,245	1,193	1,247	21,931	23,420
1990	17.79	25.67	11.38	16.13	10.80	15.40	11.41	15.77
	(1.45)	(1.77)	(1.52)	(2.24)	(1.52)	(2.14)	(0.56)	(0.71)
				[19.15]				
N	1,176	1,245	1,176	1,245	1,193	1,247	21,931	23,420
2000	18.33	17.95	12.91	12.22	11.90	11.52	12.18	12.13
	(1.40)	(1.34)	(1.42)	(1.31)	(1.24)	(1.17)	(0.58)	(0.54)
				[13.92]				
N	1,176	1,245	1,176	1,245	1,193	1,247	21,931	23,420
Panel 2: Triple-difference estimates								
E{ENTZ(Δ00) – NENTZ(Δ00)}] – [E{ENTZ(Δ90) – NENTZ(Δ90)}]	-		-1.65***	-6.10***†††	-1.58***	-9.05***†††	-1.25***	-10.53***†††
			(0.55)	(1.21)	(0.36)	(0.79)	(0.25)	(0.69)
N			1,156	1,245	1,175	1,247	23,151	24,804
Number of EZs	1,176	1,245	1,156	1,245	1,175	1,247	1,290	1,384
Number of counties	90	112	90	112	90	112	317	562

Notes: Panel 1 replicates the mean poverty estimates in HSIS, Table 1. The triple-difference estimates in Panel 2 replicate Table 2 in HSIS, for each of the control groups they consider. (NENTZ denotes non-EZ tracts.) Columns labeled "NCDB data" attempt to replicate HSIS's estimates using the Neighborhood Change Database (NCDB). The sample in the "Treated" column is from those treated tracts identified in the HSIS data (in their file that matches treated tracts to nearest control tracts). Columns labeled "HSIS" are computed from their original (uncorrected) data. In a few instances, individual estimates reported in the paper differ from those we estimate using their original data; these are highlighted in the shaded boxes, and the published estimates are reported in square brackets. Each outcome mean is conditioned on not having missing observations for other years for that variable. For example, if there are data for 1990 employment, but there are some missing observations for 1980 employment, the estimate for 1990 will not include those tracts for which 1980 data were missing. Additionally, for the NCDB data, tracts are dropped if they have zero population in 1980 or 1990 (this explains many of the differences in the number of observations between HSIS data and the NCDB data). In other instances, the NCDB may have non-missing data for an outcome that is missing in the HSIS data. Standard errors are in parentheses. Asterisks represent the significance level of the estimates (for a null hypothesis that the differences equal zero): *** 1%, ** 5%, and * 10%. Daggers represent the significance level for the test of equality of the NCDB and HSIS estimates based on the maximum variance of the estimated difference based on the Cauchy-Schwarz inequality: ††† 1%, †† 5%, and † 10%.

Table 3. Testing for Pre-trends in State Enterprise Zones (ENTZs)

	Unemployment rate (%)	Poverty rate (%)	Fraction of households with wage and salary income	Average wage and salary income ($ 2000)	Employment
Panel 1: Treated ENTZs					
1980 – 1990 level change	1.436	1.087	0.313	5,576	182.0
N	1,296	1,296	1,296	1,296	1,296
Panel 2: Nearest					
Difference between 1980 – 1990 level change for controls and treated ENTZs (Panel 1)	-0.957*** [5.295]	-1.444 *** [5.695]	-1.116*** [3.953]	**-2,921*** [8.023]**	-20.0 [1.467]
N	1,174	1,174	1,174	1,174	1,174
Panel 3: Contiguous					
Difference between 1980 – 1990 level change for controls and treated ENTZs (Panel 1)	**-1.108*** [6.309]**	**-1.431*** [6.036]**	**-1.200*** [4.391]**	3,188*** [8.880]	**39.00*** [3.015]**
N	1,193	1,193	1,193	1,193	1,193
Panel 4: All					
Difference between 1980 – 1990 level change for controls and treated ENTZs (Panel 1)	-1.462*** [12.87]	-0.417** [2.403]	0.592*** [2.856]	1,123*** [3.286]	169.6*** [11.32]
N	21,894	21,894	21,894	21,894	21,894
Panel 5: Propensity score matched on 1980 and 1990 levels					
Difference between 1980 – 1990 level change for controls and treated ENTZs (Panel 1)	-0.146 [0.752]	0.228 [0.780]	-0.561* [1.791]	-475 [1.238]	-19.80 [1.314]
N	1,296	1,296	1,296	1,296	1,296
Number of unique NENTZs	1,179	1,179	1,179	1,179	1,179

Notes: The data used for these estimates come from the Neighborhood Change Database (NCDB). *t*-statistics, in brackets, are from a two-sample mean comparison test comparing the 1980-1990 changes in the control tracts to the treated tracts, and the asterisks identify whether the changes are statistically different (*** 1%, ** 5%, and * 10%). Bolded estimates represent the control group that HSIS used in their paper, which they selected using Hausman tests (for poverty, we bold the estimates selected in Ham et al., 2018, which uses their corrected data). The treated, nearest, contiguous, and all tracts are identified using HSIS's data. The propensity score matched controls are identified by matching treated EZ tracts to control tracts using ten covariates: 1980 levels and 1990 levels for all five outcomes. The propensity score matched control tracts are the same controls used to produce the estimates in Table 6. The HSIS data for all state enterprise zone controls are used for the propensity score matching. For each dataset, we restrict the sample to tracts with non-missing information for all outcomes and non-zero population counts in 1980 and 1990 according to the NCDB. The sample sizes are sometimes a bit larger than in Table 2 because a small number of tracts were not included in the HSIS data we consider in Table 2. One issue is that HSIS, when using nearest or contiguous control tracts, dropped a small number of treated tracts, likely because HSIS could not always define one of these types of control tracts for every treated tract. For the analysis here, we retain all treated tracts.

Table 4. Testing for Pre-trends in Federal Empowerment Zones (EMPZs)

	Unemployment rate (%)	Poverty rate (%)	Fraction of households with wage and salary income	Average wage and salary income ($ 2000)	Employment
	Panel 1: Treated EMPZs				
1980 – 1990 level change	6.040	5.766	-0.541	-129.3	-121.1
N	262	262	262	262	262
	Panel 2: Nearest				
Difference between 1980 – 1990 level change for controls and treated EMPZs (Panel 1)	-3.956*** [4.399]	-3.378*** [3.449]	0.004 [0.0041]	-1,957** [2.333]	72.88*** [3.185]
N	259	259	259	259	259
	Panel 3: Contiguous				
Difference between 1980 – 1990 level change for controls and treated EMPZs (Panel 1)	-4.004*** [4.747]	**-3.160*** [3.617]**	**-0.194 [0.217]**	1,944** [2.469]	74.69 [3.498]
N	259	259	259	259	259
	Panel 4: All				
Difference between 1980 – 1990 level change for controls and treated EMPZs (Panel 1)	**-6.604*** [22.69]**	-4.991*** [11.67]	-0.407 [0.884]	**-6,294*** [8.051]**	**327.0*** [12.50]**
N	13,881	13,881	13,881	13,881	13,881
	Panel 5: Rejected or future federal enterprise zones				
Difference between 1980 – 1990 level change for controls and treated EMPZs (Panel 1)	-3.225*** [4.594]	0.520 [-0.544]	-0.979 [1.183]	-1,128.7* [1.659]	4.30 [0.226]
N	442	442	442	442	442
	Panel 6: Propensity score matched on 1980 and 1990 levels				
Difference between 1980 – 1990 level change for controls and treated EMPZs (Panel 1)	-1.412 [1.565]	0.377 [0.319]	-0.611 [0.565]	134.4 [0.146]	-4.10 [0.189]
N	262	262	262	262	262
Number of unique NENTZs	225	225	225	225	225

Notes: See notes to Table 3. *t*-statistics, in brackets, are from a two-sample mean comparison test comparing the 1980-1990 level changes in the control tracts to the treated tracts, and the asterisks identify whether the changes are statistically different (*** 1%, ** 5%, and * 10%). In Panel 5, the rejected and future federal zone control tracts are the intersection of the control tracts identified in Busso et al. (2013) and the EMPZ controls identified in HSIS's data. The treated tracts in Panel 5 are those identified as treated EMPZs by HSIS. The propensity score matched control tracts are the same controls used to produce the estimates in Table 7.

Table 5. Testing for Pre-trends in Federal Enterprise Communities (ENTCs)

	Unemployment rate (%)	Poverty rate (%)	Fraction of households with wage and salary income	Average wage and salary income ($ 2000)	Employment
		Panel 1: Treated ENTCs			
1980 – 1990 level change	2.676	6.921	-1.910	-944.9	-90.98
N	373	373	373	373	373
		Panel 2: Nearest			
Difference between 1980 – 1990 level change for controls and treated ENTCs (Panel 1)	-2.357*** [5.074]	-5.280*** [6.429]	2.358*** [-3.421]	-2,860*** [4.392]	-105.0*** [5.425]
N	341	341	341	341	341
		Panel 3: Contiguous			
Difference between 1980 – 1990 level change for controls and treated ENTCs (Panel 1)	-2.180*** [5.107]	-5.162*** [7.267]	-2.579*** [4.187]	**-2,899*** [4.814]**	-106.7*** [6.119]
N	342	342	342	342	342
		Panel 4: All			
Difference between 1980 – 1990 level change for controls and treated ENTCs (Panel 1)	**-2.797*** [14.50]**	**-5.773*** [19.29]**	**1.009*** [2.824]**	-5,069*** [8.282]	**-470.3*** [16.07]**
N	27,145	27,145	27,145	27,145	27,145
		Panel 5: Rejected or future federal enterprise zones			
Difference between 1980 – 1990 level change for controls and treated ENTCs (Panel 1)	1.698*** [3.209]	0.214 [0.269]	0.477 [0.701]	13.90 [0.024	40.67* [1.899]
N	360	360	360	360	360
		Panel 6: Propensity score matched on 1980 and 1990 levels only			
Difference between 1980 – 1990 level change for controls and treated ENTCs (Panel 1)	0.129 [0.249]	-0.228 [0.273]	0.099 [-0.126]	-20.0 [0.033]	-0.550 [0.028]
N	373	373	373	373	373
Number of unique NENTZs	332	332	332	332	332

Notes: See notes to Tables 3 and 4. *t*-statistics, in brackets, are from a two-sample mean comparison test comparing the 1980-1990 level changes in the control tracts to the treated tracts, and the asterisks identify whether the changes are statistically different (*** 1%, ** 5%, and * 10%). The propensity score matched control tracts are the same controls used to produce the estimates in Table 8.

Table 6. Estimates of the Effects of State Enterprise Zones (ENTZs)

	Unemployment rate (%)	Poverty rate (%)	Fraction of households with wage and salary income	Average wage and salary income ($ 2000)	Employment
Panel 1: HSIS preferred estimator					
ENTZ	-2.090***	-2.003***	0.393	1,530***	-0.987
	(0.270)	(0.390)	(0.460)	(455.0)	(19.54)
N	1,193	1,193	1,193	1,174	1,193
Number of ENTZs	1,193	1,193	1,193	1,174	1,193
Number of NENTZs	1,193	1,193	1,193	1,174	1,193
Number of counties	90	90	90	90	90
Comparison group (Hausman selected)	Contiguous	Contiguous	Contiguous	Nearest	Contiguous
Panel 2: Propensity score matched on 1980 and 1990 levels					
ENTZ	-1.050***	0.593	-0.573	-626.8	-32.37*
	(0.296)	(0.458)	(0.379)	(525.5)	(19.51)
N	23,190	23,190	23,190	23,190	23,190
Number of ENTZs	1,296	1,296	1,296	1,296	1,296
Number of matched controls	1,296	1,296	1,296	1,296	1,296
Number of unique NENTZs	1,179	1,179	1,179	1,179	1,179
Number of counties	94	94	94	94	94
Standard error for the difference between PSM and HSIS estimates[†]	0.362	0.571	0.546	614.0	24.61
t-statistic for the difference between PSM and HSIS estimates	2.87	4.55	-1.77	-3.45	-1.28

[†] Standard error for the difference is calculated using the standard errors from this table and the correlation between HSIS and the propensity score model estimates from bootstrapping samples 1,000 times. Specifically, we would

estimate the standard error of the difference as $SE_{\beta_{ps}-\beta_{HSIS}} = \sqrt{var(\widehat{\beta_{ps}}) + var(\widehat{\beta_{HSIS}}) - 2 \cdot cov(\widehat{\beta_{ps}}, \widehat{\beta_{HSIS}})}$.

However, because $cov(\widehat{\beta_{ps}}, \widehat{\beta_{HSIS}})$ is not directly estimable from joint estimation of the two models, we bootstrap the correlation between both estimators using 1,000 iterations, running both models on the identical bootstrapped sample. That is, we estimate $cov(\widehat{\beta_{ps}}, \widehat{\beta_{HSIS}}) = corr(\widehat{\beta_{ps}^{BS}}, \widehat{\beta_{HSIS}^{BS}}) \cdot \widehat{\sigma_{ps}} \cdot \widehat{\sigma_{HSIS}}$, with the correlation computed from the bootstrapped estimates. In each bootstrapped sample, we run each estimator using the same treated tracts.

Notes: The estimators are described in the text. Estimates shaded in grey are those for which parallel trends are not rejected in Table 3. PSM denotes propensity score matching.

Table 7. Estimates of the Effects of Federal Empowerment Zones (EMPZs)

	Unemployment rate (%)	Poverty rate (%)	Fraction of households with wage and salary income	Average wage and salary income ($ 2000)	Employment
Panel 1: HSIS preferred estimator					
EMPZ	-10.21***	-8.160***	1.761	5,247***	231.4***
	(0.524)	(1.656)	(1.417)	(1,007)	(32.68)
N	14,143	259	259	14,143	14,143
Number of EMPZs	262	259	259	262	262
Number of NEMPZs	13,881	259	259	13,881	13,881
Number of counties	178	9	9	178	178
Comparison group (Hausman selected)	All	Contiguous	Contiguous	All	All
Panel 2: Rejected (in Round 1) and future federal zones					
EMPZ	-6.501***	-4.427**	4.563**	2,694**	92.01***
	(1.326)	(2.088)	(1.867)	(1,232)	(34.18)
N	704	704	704	704	704
Number of EMPZs	262	262	262	262	262
Number of NEMPZs	442	442	442	442	442
Number of unique NEMPZs	442	442	442	442	442
Number of counties	31	31	31	31	31
Standard error for the difference between PSM and rejected/future zone estimates[†]	2.254	2.854	2.635	2,263	46.63
t-statistic for the difference between PSM and rejected/future zone estimates	1.742	1.043	-1.817	-0.771	-2.029
Panel 3: Propensity score matched on 1980 and 1990 levels					
EMPZ	-2.575***	-1.449	-0.224	949.9	-2.595
	(0.953)	(1.835)	(1.570)	(1,197)	(24.32)
N	14,143	14,143	14,143	14,143	14,143
Number of EMPZs	262	262	262	262	262
Number of matched controls	13,881	13,881	13,881	13,881	13,881
Number of unique NEMPZs	225	225	225	225	225
Number of counties	178	178	178	178	178
Standard error for the difference between PSM and HSIS estimates[†]	0.915	2.126	1.790	1,324	36.11
t-statistic for the difference between PSM and HSIS estimates	8.344	3.157	-1.109	-3.246	-6.479

[†] See Table 6.

Notes: See notes to Table 6. Models using rejected/future zone tracts as controls do not include the fixed state effects, more consistent with a matching estimator; but estimates were very similar including the state fixed effects.

Table 8. Estimates of the Effects of Federal Enterprise Community Zones (ENTCs)

	Unemployment rate (%)	Poverty rate (%)	Fraction of households with wage and salary income	Average wage and salary income ($ 2000)	Employment
	Panel 1: HSIS preferred estimator				
ENTC	-2.771***	-11.86***	5.327***	3,827***	123.7***
	(0.340)	(0.583)	(0.623)	(851.3)	(44.13)
N	27,518	27,518	27,518	342	27,518
Number of ENTCs	373	373	373	342	373
Number of NENTCs	27,145	27,145	27,145	342	27,145
Number of counties	533	533	533	41	533
Comparison group (Hausman selected)	All	All	All	Contiguous	All
	Panel 2: Rejected (in Round 1) and future federal zones				
ENTC	1.938*	-4.205*	1.801	1,390	71.81
	(1.121)	(2.354)	(1.618)	(1,662)	(48.62)
N	733	733	733	733	733
Number of ENTCs	373	373	373	373	373
Number of controls	360	360	360	360	360
Number of unique NENTCs	360	360	360	360	360
Number of counties	72	72	72	72	72
Standard error for the difference between PSM and rejected/future zone estimates[†]	1.727	2.459	2.07	1,697	53.86
t-statistic for the difference between PSM and rejected/future zone estimates	-1.269	1.068	-1.054	-0.256	-2.251
	Panel 3: Propensity score matched on 1980 and 1990 levels				
ENTC	-0.253	-1.577	-0.379	956.1	-49.42*
	(0.908)	(1.221)	(1.151)	(965.9)	(25.50)
N	27,518	27,518	27,518	27,518	27,518
Number of ENTCs	373	373	373	373	373
Number of matched controls	27,145	27,145	27,145	27,145	27,145
Number of unique NENTCs	332	332	332	332	332
Number of counties	44	44	44	44	44
Standard error for the difference between PSM and HSIS estimates[†]	0.871	1.199	1.15	1,177	44.83
t-statistic for the difference between PSM and HSIS estimates	2.891	8.575	-4.963	-2.439	-3.862

[†] See Table 6.

Notes: See notes to Table 6. Models using rejected/future zone tracts as controls do not include the fixed state effects, more consistent with a matching estimator; but estimates were very similar including the state fixed effects.

On-Line Appendix Tables

Appendix Table A1. Estimates of the Effects of State Enterprise Zones (ENTZs)

	Unemployment rate (%)	Poverty rate (%)	Fraction of households with wage and salary income	Average wage and salary income ($ 2000)	Employment
			Panel 1: Nearest		
ENTZ	-1.471***	-2.416***	0.712	**1,530*****	10.10
	(0.555)	(0.421)	(0.466)	**(455.0)**	(17.90)
N	1,174	1,174	1,174	1,174	1,174
Number of ENTZs	1,174	1,174	1,174	1,174	1,174
Number of NENTZs	1,174	1,174	1,174	1,174	1,174
Number of counties	90	90	90	90	90
Standard error for the difference between PSM (Panel 5) and HSIS estimates[†]	0.561	0.545	0.520	626.6	24.76
t-statistic for the difference between PSM (Panel 5) and HSIS estimates	0.749	5.520	-2.468	-3.442	-1.715
			Panel 2: Contiguous		
ENTZ	**-2.090*****	**-2.003*****	**0.393**	1,490***	**-0.987**
	(0.270)	**(0.390)**	**(0.460)**	(436.2)	**(19.54)**
N	1,193	1,193	1,193	1,193	1,193
Number of ENTZs	1,193	1,193	1,193	1,193	1,193
Number of NENTZs	1,193	1,193	1,193	1,193	1,193
Number of counties	90	90	90	90	90
Standard error for the difference between PSM (Panel 5) and HSIS estimates[†]	0.346	0.518	0.508	617.0	25.45
t-statistic for the difference between PSM (Panel 5) and HSIS estimates	3.008	5.012	-1.902	-3.432	-1.233
			Panel 3: All		
ENTZ	-1.503***	-1.298***	0.682**	2,138***	32.38
	(0.227)	(0.314)	(0.337)	(493.1)	(21.08)
N	23,190	23,190	23,190	23,190	23,190
Number of ENTZs	1,296	1,296	1,296	1,296	1,296
Number of NENTZs	21,894	21,894	21,894	21,894	21,894
Number of counties	317	317	317	317	317
Standard error for the difference between PSM (Panel 5) and HSIS estimates[†]	0.302	0.474	0.428	602.3	25.33
t-statistic for the difference between PSM (Panel 5) and HSIS estimates	1.500	3.986	-2.934	-4.591	-2.557

[†] See Table 6.

Notes: See notes to Table 6. Estimates shaded in grey are those for which parallel trends are not rejected in Table 3. Bolded estimates represent the control group that HSIS used in their paper, which they selected using Hausman tests; see notes to Table 3.

Appendix Table A2: Estimates of the Effects of Federal Empowerment Zones (EMPZs)

	Unemployme nt rate (%)	Poverty rate (%)	Fraction of households with wage and salary income	Average wage and salary income ($ 2000)	Employment
			Panel 1: Nearest		
EMPZ	-8.392***	-8.132***	2.019	4,574***	159.8***
	(1.603)	(1.853)	(1.568)	(1,494)	(36.59)
N	259	259	259	259	259
Number of EMPZs	259	259	259	259	259
Number of NEMPZs	259	259	259	259	259
Number of counties	9	9	9	9	9
Standard error for the difference between PSM (Panel 5) and HSIS estimates[†]	1.655	2.286	1.909	1,739	41.29
t-statistic for the difference between PSM (Panel 5) and HSIS estimates	3.514	2.923	-1.175	-2.085	-3.934
			Panel 2: Contiguous		
EMPZ	-8.135***	**-8.160***	**1.761**	4,086*	**166.9***
	(1.754)	**(1.656)**	**(1.417)**	(1,927)	**(49.51)**
N	259	259	259	259	259
Number of EMPZs	259	259	259	259	259
Number of NEMPZs	259	259	259	259	259
Number of counties	9	**9**	**9**	9	9
Standard error for the difference between PSM (Panel 5) and HSIS estimates[†]	1.772	2.126	**1.790**	2,078	51.75
t-statistic for the difference between PSM (Panel 5) and HSIS estimates	3.137	**3.157**	**-1.109**	-1.510	-3.275
			Panel 3: All		
EMPZ	**-10.21***	-11.74***	7.218***	**5,247***	**231.4***
	(0.524)	(0.713)	(0.722)	**(1,007)**	**(32.68)**
N	14,143	14,143	14,143	14,143	14,143
Number of EMPZs	262	262	262	262	262
Number of NEMPZs	13,881	13,881	13,881	13,881	13,881
Number of counties	178	178	178	178	178
Standard error for the difference between PSM (Panel 5) and HSIS estimates[†]	0.915	1.768	**1.515**	1,324	36.11
t-statistic for the difference between PSM (Panel 5) and HSIS estimates	8.344	**5.819**	**-4.912**	-3.246	-6.479

[†] See Table 6.

Notes: See notes to Table 7 and Appendix Table A1.

Appendix Table A3: Estimates of the Effects of Federal Enterprise Communities (ENTCs)

	Unemployment rate (%)	Poverty rate (%)	Fraction of households with wage and salary income	Average wage and salary income ($ 2000)	Employment
			Panel 1: Nearest		
ENTC	-3.910***	-11.51***	4.655***	3,841***	87.84**
	(1.121)	(1.408)	(1.057)	(1,043)	(25.79)
N	341	341	341	341	341
Number of ENTCs	341	341	341	341	341
Number of NENTCs	341	341	341	341	341
Number of counties	41	41	41	41	41
Standard error for the difference between PSM (Panel 5) and HSIS estimates[†]	1.211	1.665	1.415	1,324	32.65
t-statistic for the difference between PSM (Panel 5) and HSIS estimates	3.020	5.962	-3.559	-2.179	-4.204
			Panel 2: Contiguous		
ENTC	-3.474***	-11.09**	5.130***	**3,827*****	79.80***
	(0.970)	(1.176)	(0.867)	**(851.3)**	(19.02)
N	342	342	342	342	342
Number of ENTCs	342	342	342	342	342
Number of NENTCs	342	342	342	342	342
Number of counties	41	41	41	41	41
Standard error for the difference between PSM (Panel 5) and HSIS estimates[†]	1.079	1.463	1.264	1,177	28.15
t-statistic for the difference between PSM (Panel 5) and HSIS estimates	2.984	6.507	-4.357	-2.439	-4.591
			Panel 3: All		
ENTC	**-2.771*****	**-11.86*****	**5.327*****	3,627***	**123.7*****
	(0.398)	**(0.583)**	**(0.623)**	(979.0)	**(44.13)**
N	27,518	27,518	27,518	27,518	27,518
Number of ENTCs	373	373	373	373	373
Number of NENTCs	27,145	27,145	27,145	27,145	27,145
Number of counties	533	533	533	533	533
Standard error for the difference between PSM (Panel 5) and HSIS estimates[†]	0.871	1.199	1.150	1,250	44.83
t-statistic for the difference between PSM (Panel 5) and HSIS estimates	2.891	8.575	-4.963	-2.136	-3.862

[†] See Table 6.

Notes: See notes to Table 8 and Appendix Table A1.

Appendix Table A4. Testing Robustness of Propensity Score Matched Estimates of the Effects of ENTZs

	Unemployment rate (%)	Poverty rate (%)	Fraction of households with wage and salary income	Average wage and salary income ($ 2000)	Employment
Panel 1: Propensity score estimates excluding nearest neighbors as controls					
ENTZ	-1.067***	0.178	-791.1	-0.688*	-14.76
	(0.316)	(0.442)	(485.5)	(0.352)	(18.13)
N	22,668	22,668	22,668	22,668	22,668
Number of ENTZs	1,296	1,296	1,296	1,296	1,296
Number of matched controls	1,296	1,296	1,296	1,296	1,296
Number of unique NENTZs	1,246	1,246	1,246	1,246	1,246
Number of dropped NENTZ tracts	522	522	522	522	522
Number of counties	317	317	317	317	317
Panel 2: Propensity score estimates excluding contiguous neighbors as controls					
ENTZ	-1.210***	0.307	-1,119**	-0.345	-33.51
	(0.311)	(0.435)	(534.8)	(0.351)	(20.82)
N	21,947	21,947	21,947	21,947	21,947
Number of ENTZs	1,296	1,296	1,296	1,296	1,296
Number of matched controls	1,296	1,296	1,296	1,296	1,296
Number of unique NENTZs	1,230	1,230	1,230	1,230	1,230
Number of dropped NENTZ tracts	1,243	1,243	1,243	1,243	1,243
Number of counties	316	316	316	316	316
Panel 3: Propensity score estimates excluding dissimilar control tracts					
ENTZ	-1.609***	0.433	-1,196***	0.0610	-9.689
	(0.287)	(0.414)	(459.1)	(0.376)	(18.74)
N	23,177	23,177	23,177	23,177	23,177
Number of ENTZs	1,296	1,296	1,296	1,296	1,296
Number of matched controls	1,296	1,296	1,296	1,296	1,296
Number of unique NENTZs	1,246	1,246	1,246	1,246	1,246
Number of dropped NENTZ tracts	13	13	13	13	13
Number of counties	317	317	317	317	317
Panel 4: Inverse probability weight estimates					
ENTZ	-0.881***	-0.417	4.944	0.306	-32.61***
	(0.223)	(0.328)	(490.8)	(0.361)	(10.54)
N	23,190	23,190	23,190	23,190	23,190
Number of ENTZs	1,296	1,296	1,296	1,296	1,296
Number of matched controls	21,894	21,894	21,894	21,894	21,894
Number of unique NENTZs	21,894	21,894	21,894	21,894	21,894
Number of counties	317	317	317	317	317
Panel 5: Propensity score estimates matching on two nearest neighbors					
ENTZ	-0.810***	0.663*	-1,073**	-0.617**	-31.77**
	(0.237)	(0.379)	(423.7)	(0.303)	(15.80)
N	23,190	23,190	23,190	23,190	23,190
Number of ENTZs	1,296	1,296	1,296	1,296	1,296
Number of matched controls	2,592	2,592	2,592	2,592	2,592
Number of unique NENTZs	2,234	2,234	2,234	2,234	2,234
Number of counties	317	317	317	317	317

Notes: The estimates in Panels 1-3 are from a propensity score matched model. The estimates in Panel 3 have a caliper set at 0.0104, which is 0.2 multiplied by the standard deviation of the logit of the propensity score. That is, we run a logit model to predict the probability of a tract being enacted as an enterprise zone using the ten covariates: 1980 and 1990 levels for all five outcomes. The estimates in Panel 4 are estimated using inverse probability weights where weights are the propensity score estimates from a probit model that predicts enterprise zone designation based on ten covariates: 1980 and 1990 levels for all five outcomes. In all panels, the estimates are ATT. In Panel 4, all of the inverse probability weighted models converged except for average wage and salary income – for this outcome we estimate the effect using a two-stage estimator. In the first stage, we run a probit model as discussed above, and in the second stage we regress double-differenced average wage and salary income on an enterprise zone indicator, weighting by the inverse probability weights. (The two-step estimator results in the same coefficient estimates, but different standard errors – although in comparing them for the convergent cases, there was no clear bias towards larger or smaller standard errors.)

Appendix Table A5. Testing Robustness of Propensity Score Matched Estimates of the Effects of EMPZs

	Unemployment rate (%)	Poverty rate (%)	Fraction of households with wage and salary income	Average wage and salary income ($ 2000)	Employment
Panel 1: Propensity score estimates excluding nearest neighbors as controls					
EMPZ	-3.317**	-2.041	684.8	2.233	-17.37
	(1.367)	(1.646)	(1,529)	(2.043)	(33.75)
N	13,987	13,987	13,987	13,987	13,987
Number of EMPZs	262	262	262	262	262
Number of matched controls	262	262	262	262	262
Number of unique NEMPZs	226	226	226	226	226
Number of dropped NEMPZ tracts	156	156	156	156	156
Number of counties	178	178	178	178	178
Panel 2: Propensity score estimates excluding contiguous neighbors as controls					
EMPZ	-4.670**	-1.127	391.1	0.331	0.649
	(2.011)	(1.844)	(1,556)	(1.377)	(27.25)
N	13,808	13,808	13,808	13,808	13,808
Number of EMPZs	262	262	262	262	262
Number of matched controls	262	262	262	262	262
Number of unique NEMPZs	220	220	220	220	220
Number of dropped NEMPZ tracts	335	335	335	335	335
Number of counties	178	178	178	178	178
Panel 3: Propensity score estimates excluding dissimilar control tracts					
EMPZ	-0.0415	-0.594	358.3	-1.334	17.72
	(1.333)	(1.655)	(1,006)	(1.739)	(26.36)
N	14,132	14,132	14,132	14,132	14,132
Number of EMPZs	260	260	260	260	260
Number of matched controls	260	260	260	260	260
Number of unique NEMPZs	225	225	225	225	225
Number of dropped NEMPZ tracts	11	11	11	11	11
Number of counties	178	178	178	178	178
Panel 4: Inverse probability weight estimates					
EMPZ	2.462	2.942*	-2,096	-2.317	-23.12
	(2.101)	(1.508)	(1,825)	(1.811)	(25.97)
N	14,143	14,143	14,143	14,143	14,143
Number of EMPZs	262	262	262	262	262
Number of unique NEMPZs	13,881	13,881	13,881	13,881	13,881
Number of counties	178	178	178	178	178
Panel 5: Propensity score estimates matching on two nearest neighbors					
EMPZ	-1.549	0.684	-510.4	-0.433	-9.511
	(1.587)	(1.448)	(1,094)	(1.310)	(21.79)
N	14,143	14,143	14,143	14,143	14,143
Number of EMPZs	262	262	262	262	262
Number of matched controls	524	524	524	524	524
Number of unique NEMPZs	400	400	400	400	400
Number of counties	178	178	178	178	178

Notes: See notes to Appendix Table A4. The estimates in Panel 3 have a caliper set at 0.0133, which is 0.2 multiplied by the standard deviation of the logit of the propensity score. In Panel 4, all of the inverse probability weighted models converged except for average wage and salary income and employment – for this outcome we estimate the effect using a two-stage estimator. See notes to Appendix Table A4.

Appendix Table A6. Testing Robustness of Propensity Score Matched Estimates of the Effects of ENTCs

	Unemployment rate (%)	Poverty rate (%)	Fraction of households with wage and salary income	Average wage and salary income ($ 2000)	Employment
Panel 1: Propensity score estimates excluding nearest neighbors as controls					
ENTC	1.047	1.565	194.3	-0.376	-64.08***
	(0.871)	(1.318)	(883.4)	(0.990)	(22.91)
N	27,360	27,360	27,360	27,360	27,360
Number of ENTCs	373	373	373	373	373
Number of matched controls	373	373	373	373	373
Number of unique NENTCs	344	344	344	344	344
Number of dropped NENTC tracts	158	158	158	158	158
Number of counties	533	533	533	533	533
Panel 2: Propensity score estimates excluding contiguous neighbors as controls					
ENTC	2.213**	2.157	-329.4	-0.815	-37.33*
	(0.918)	(1.321)	(883.3)	(1.230)	(20.73)
N	27,096	27,096	27,096	27,096	27,096
Number of ENTCs	373	373	373	373	373
Number of matched controls	373	373	373	373	373
Number of unique NENTCs	344	344	344	344	344
Number of dropped NENTC tracts	422	422	422	422	422
Number of counties	533	533	533	533	533
Panel 3: Propensity score estimates excluding dissimilar control tracts					
ENTC	1.512	-0.611	863.6	-2.567**	-70.61***
	(1.037)	(1.334)	(1,356)	(1.176)	(24.42)
N	27,504	27,504	27,504	27,504	27,504
Number of ENTCs	373	373	373	373	373
Number of matched controls	373	373	373	373	373
Number of unique NENTCs	341	341	341	341	341
Number of dropped NENTC tracts	14	14	14	14	14
Number of counties	533	533	533	533	533
Panel 4: Inverse probability weight estimates					
ENTC	-17.35	18.74	-1,642	17.82	-51.93**
	(14.84)	(13.19)	(1,120)	(14.75)	(25.49)
N	27,518	27,518	27,518	27,518	27,518
Number of ENTCs	373	373	373	373	373
Number of unique NENTCs	27,145	27,145	27,145	27,145	27,145
Number of counties	533	533	533	533	533
Panel 5: Matching on two nearest neighbors					
ENTC	0.365	-0.672	493.6	-0.592	-38.84*
	(0.626)	(0.985)	(708.4)	(0.893)	(21.30)
N	27,518	27,518	27,518	27,518	27,518
Number of ENTCs	373	373	373	373	373
Number of matched controls	746	746	746	746	746
Number of unique NENTCs	605	605	605	605	605
Number of counties	533	533	533	533	533

Notes: See notes to Appendix Table A4. The estimates in Panel 3 have a caliper set at 0.0097, which is 0.2 multiplied by the standard deviation of the logit of the propensity score. In Panel 4, the inverse probability weighted model converged only for poverty – for the other outcomes we estimate the effect using a two-stage estimator. See notes to Appendix Table A4.