DO ENTERPRISE ZONES CREATE JOBS? EVIDENCE FROM CALIFORNIA'S
ENTERPRISE ZONE PROGRAM

David Neumark
Jed Kolko

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David Neumark and Jed Kolko
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<u>ABSTRACT</u>

We use new establishment-level data and geographic mapping methods to improve upon evaluations of the effectiveness of state enterprise zones, focusing on California's program. Because zone boundaries do not follow census tracts or zip codes, we created digitized maps of original zone boundaries and later expansions. We combine these maps with geocoded observations on most businesses located in California. The evidence indicates that enterprise zones do not increase employment. We also find no shift of employment toward the lower-wage workers or manufacturing sector targeted by enterprise zone incentives. We conclude that the program is ineffective in achieving its primary goals.

David Neumark
Department of Economics
3151 Social Science Plaza
University of California, Irvine
Irvine, CA 92697-5100
and NBER
dneumark@uci.edu

Jed Kolko
Public Policy Institute of California
500 Washington St., Suite 600
San Francisco, CA 94111
kolko@ppic.org

1. Introduction

State and federal enterprise zone programs are the principal means by which governments try to directly promote economic development in specific locations, typically economically-distressed urban areas. There is considerable debate over the effectiveness of enterprise zones in spurring job creation (e.g., Peters and Fisher, 2002). Numerous problems, however, have plagued past evaluations of enterprise zone programs, including precisely identifying the targeted areas, selecting appropriate comparison or control areas, distinguishing the effects of enterprise zones from other geographically-targeted policies, and choosing outcomes in line with program incentives and goals (e.g., Boarnet, 2001).

In this study, we evaluate the effectiveness of California's enterprise zone program, using new data sources that permit us to address these shortcomings of previous research. The first data source is detailed GIS maps we have constructed of the precise boundaries of enterprise zones and their evolution over time. The second is the National Establishment Time-Series (NETS) database, which includes employment and location information on nearly all business establishments in California in the period 1992-2004. By constructing precisely geocoded location information and combining this information with GIS maps, we can measure employment, the number of establishments, and other characteristics of these establishments in each year and in each enterprise zone, as well as in appropriate control areas. In addition, we incorporate information on other geographically-targeted policies to avoid confounding their effects with those of the state enterprise zone program. Our data and methods permit us to accurately assess the effects of enterprise zones on their principal goal – job creation.

2. Limitations of Previous Research on Enterprise Zones

Most existing research evaluating the effects of enterprise zones assesses their effects on jobs, businesses, or zone residents. Typically, these studies compare outcomes like employment (e.g., Bondonio and Engberg, 2000; O'Keefe, 2004) or number of establishments (Dabney, 1991) across enterprise zones and comparable regions where zone incentives do not apply. The results vary across studies. Many studies fail to find employment effects of enterprise zones, although some of the work (e.g., O'Keefe, 2004; and research reviewed in Wilder and Rubin, 1996) concludes that there are positive

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employment effects, at least in the short-run. Other recent work (Busso and Kline, 2007; Elvery, 2009) provides thorough overviews of the literature. In this section, instead, we highlight the limitations of the existing research on which we try to improve in the present study. In the concluding section of the paper we provide some comparisons between our findings and the existing literature.

The first challenge in estimating the effects of enterprise zones is to identify geographic areas that precisely reflect enterprise zone boundaries for which outcomes of interest – such as employment – can be measured. In California and many other places, the boundaries of enterprise zones do not follow boundaries of census tracts, zip codes, or other standard geographic designations. Instead, studies have used aggregate data on zip codes (e.g., Dowall, 1996; Bondonio and Greenbaum, 2007) or census tracts (e.g., O'Keefe, 2004), the boundaries of which correspond only approximately to those of enterprise zones. These approximations, however, introduce measurement error by incorrectly assigning areas (and the workers or businesses in them) as inside or outside enterprise zones (Papke, 1993). For example, Elvery (2008) notes that for the two states he studies, if enterprise zones are defined as the areas encompassing all zip codes that overlap with enterprise zones, then the resulting enterprise zone definitions are six times larger than the actual zones. Similarly, he shows that, based on 1990 Census data and tracts, less than half of the population residing in census tracts that include enterprise zones actually live in enterprise zones. If locations are incorrectly classified as to whether or not they are in enterprise zones or control areas, there is likely a bias towards finding no effect of enterprise zones.

The second challenge is selecting appropriate control groups for enterprise zones. The ideal control group consists of areas economically similar to enterprise zones but lacking enterprise zone designation. Some studies, however, have used broad control groups that may preclude meaningful comparison with the enterprise zones. For instance, Peters and Fisher (2002) estimate the effects of enterprise zones in a number of states relative to the areas of states outside the enterprise zone; similarly, Lynch and Zax (2008) use all regions of Colorado that are not in enterprise zones.[1] Others have constructed control groups differently. O'Keefe (2004) matches census tracts that approximate enterprise zone boundaries to

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[1] By ignoring births and relocations, this study may miss an important role played by births in job growth.

other census tracts using propensity score matching based on residential and employment characteristics. However, propensity score matching does not account for unobservable sources of differences in job growth that may be the basis for assignment to zones. In addition, for many of the zones the matching is on post-treatment observations, which implies that her matching may mask the effects of enterprise zones by conditioning them out.[2] Elvery (2009) improves on this propensity score strategy by matching on the employment variation across neighborhoods that is not accounted for by residents' characteristics, and by matching on pre-treatment observations. None of these studies makes use of before and after comparisons of areas observed both before and after enterprise zones were established.[3]

More recent research has addressed the comparison group problem differently. Billings (2009) uses a spatial discontinuity model, looking at employment growth in Colorado's enterprise zones within ¼ mile of the zone boundary and using the area outside the zones within ¼ mile of the zone boundary as the control group.[4] Busso and Kline (2007) compare residential employment outcomes in census tracts that became part of federal empowerment zones with outcomes in census tracts that submitted unsuccessful applications to be designated empowerment zones; they also in some cases do comparisons with areas that become parts of zones in the future. Boarnet and Bogart (1996) study a set of municipalities in New Jersey, all of which qualified for enterprise zones. They compare those that received zones to all that qualified, and, paralleling Busso and Line, also compare those that received zones to those that qualified *and applied* for zones but did not receive them; however, this study suffers from poor delineation of enterprise zones by using entire municipalities.

The third challenge is that an enterprise zone program may cover areas that are also affected by other geographically-targeted policies, including other local or state policies or federal enterprise zone programs. If another program has strong effects and in some areas targets both the treatment and control

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[2] Moreover, O'Keefe matches on employment levels, whereas we would like to hold employment growth rates (in the pre-treatment period) constant between treatment and control groups. Bondonio and Greenbaum (2007) also use propensity score methods, but their study is limited to manufacturing establishments.
[3] In contrast, see Papke (1994) and Greenbaum and Engberg (2004).
[4] Billings uses geographic methods most similar to ours, with digitized maps of enterprise zone boundaries and geocoded establishment locations.

areas used to estimate the effects of enterprise zones, then ignoring the effects of the other program will lead to biased estimates of the effects of enterprise zones. We are not aware of studies that have simultaneously considered the effects of programs that apply to overlapping areas.

Finally, the fourth challenge is to study outcomes that are appropriate – and appropriately measured – in light of the enterprise zone program's goals and design. If the program's incentives are geared toward certain sectors (for example, manufacturing) or lower-wage industries, then we should look at outcome variables – like growth in particular industries – that reflect the program's aims. It is also essential to identify which businesses and households qualify for program incentives. For example, in California's program businesses in an enterprise zone can claim hiring credits for employees living in a targeted employment area (TEA), which need not be coincident with the enterprise zone. Hence, evaluating the program in terms of employment of zone residents would be inappropriate, as the effects of California's enterprise zones on household outcomes, like employment or poverty, should be more apparent in TEA's. In contrast, for asking whether the state's enterprise program boosted employment – when measured by the *location* of jobs – it is appropriate to look at enterprise zones.

3. California's Enterprise Zone Program

California's enterprise zone program has multiple goals: attracting jobs and businesses and raising employment is one goal, while others include reducing poverty and unemployment and raising incomes in target areas.[5] These multiple goals – job creation and improving residents' circumstances – stem from the 1996 merger of two precursor programs that gave rise to the current enterprise zone program: the Enterprise Zone Act, which provided incentives to businesses located in specific areas (and which led to the creation of the original enterprise zones); and the Employment and Economic Incentive Act, which provided incentives to businesses that hired employees living in distressed residential areas.

The program seeks to accomplish these goals by providing a variety of tax incentives to businesses located in designated areas to encourage the hiring of economically disadvantaged workers and to spur the creation of businesses. The largest incentive is state tax credits for hiring a "disadvantaged" employee.

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[5] See Assembly Jobs, Economic Development, and the Economy Committee (2006, p. 5).

The state calculates the allowable hiring credit as a share of wages up to 150% of the minimum wage; the allowable credit is 50% of qualified wages in the first year, falling by 10 percentage points each year until reaching zero after five years. Workers qualify as "disadvantaged" if they are unemployed for a sufficient duration or for certain other reasons − for example, if they have sufficiently low income, if they belong to one of several "eligibility groups" (veteran, enrolled in welfare-to-work, etc.), or if they live in a targeted employment area.[6] Given that disadvantaged workers are likely to earn low wages, the tax credit can result in a substantial reduction in the cost of hiring low-skill labor. For example, at a $6 minimum wage, the credit would reduce the cost of a full-time worker earning $9 per hour by $9,000 in the first year, $7,200 in the second year, etc. Another way to think about this is that the cost of hiring new low-skilled workers who are likely to turn over within a year is reduced by 50%, which is much more than the labor cost change associated with other policies about which there is lively debate regarding employment effects (e.g., health insurance mandates, discussed in Burkhauser and Simon, 2008).[7]

The program offers four other incentives: (1) an income tax credit for sales or use taxes for machinery or parts for use within the zone; (2) a longer period (15 years versus 10 years) in which businesses can carry forward net operating losses into future years to reduce tax liabilities; (3) accelerated depreciation of depreciable property; (4) a tax credit of 5% of qualified wages that low-income employees can claim, up to a maximum and subject to restrictions on work for the business in the zone and services performed within the zone.[8] Each of these incentives is intended to reduce the tax burden or costs for businesses located in enterprise zones, which might be expected to spur the creation of new businesses or the expansion of existing ones. In addition, businesses in enterprise zones can sometimes receive

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[6] The eligibility of residents of targeted employment areas (TEA's) for the hiring credit began in 1997. Enterprise zones are defined by individual street addresses. TEA's are defined by census tracts. TEA's often include parts of an enterprise zone itself along with other lower-income neighborhoods, but they are defined independently of enterprise zones and do not necessarily overlap with them. A worker living in a TEA qualifies for the hiring credit regardless of the worker's characteristics. TEA's include census tracts where more than half the population earns less than 80 percent of median area income, according to the 1980 Census.

[7] See Engberg and Greenbaum (1999) for a description of subsidies in other states.

[8] Although technically this credit is given to the worker, the incidence of the tax credit is independent of who claims it. As long as labor supply is not completely inelastic, market wages should fall (although wages plus the credit will rise), and employment increase.

preferential treatment on state contracts. Finally, financial lenders may deduct from their income net interest received from loans made to businesses in enterprise zones.

Localities apply to the Department of Housing and Community Development (HCD) to have a geographic area designated as an enterprise zone. Eligibility criteria include job-generating capacity as well as the level of economic distress measured along a number of dimensions. New zones are selected by HCD from the eligible areas based on these and other factors, including the local applicant's plan for bundling other local incentives, administering the program, and evaluating the outcome. There are some minimum criteria for enterprise zone designation – such as, for example, whether an area has recently experienced a particularly sharp decline, or conversely exhibited promise for growth – but beyond that the process is not formulaic and appears to rely on subjective assessments.

As of the period covered by this paper, the enterprise zone program allowed for up to 42 zones in the state. HCD can conduct an application process when the number of zones falls below the maximum, whether due to zones expiring, zones being de-designated, or the legislature increasing the maximum number of zones. Ten enterprise zones were created at the program's inception in 1986; since then, legislation has increased the number to 42. Zones are designated for an initial 15-year term, after which 5-year extensions can be granted. After the 15- or 20-year period, the enterprise zone expires, and a new application must be submitted. In practice, there is very little turnover in enterprise zones. All of the zones designated before 1990 were granted 5-year extensions when they reached the end of their original 15-year terms. Zones have expanded periodically, and in 1998 many enterprise zones were allowed a one-time expansion of their boundaries by up to 20% (which they could undertake later). No enterprise zone in the state has ever been de-designated for poor audit results or any other reason.[9]

Our study focuses on the effects of enterprise zones on jobs and businesses located inside the zones, emphasizing the question of whether enterprise zones spur job growth. Job creation is an explicit goal of the program, and is also presumably a prerequisite for improving the economic circumstances of the disadvantaged workers the program is intended to help. In addition, in a survey of local zone

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[9] See Assembly Jobs, Economic Development, and the Economy Committee (2006, p. 10).

managers, nearly all respondents cited job or business creation when asked an open-ended question about the purpose of the enterprise zone program; far fewer cited improving residents' outcomes such as unemployment or poverty (Kolko and Neumark, in progress).

Our study does *not* directly assess evidence of the effects on residents of the enterprise zones or of targeted employment areas (or on other individuals meeting eligibility for the hiring tax credit). The effects on zone residents per se are not relevant, since the zones do not target these residents. As already noted, although the effects on residents of targeted employment areas are relevant (since 1997), the first-order question, it seems to us, is whether enterprise zones lead to job creation. If they do, then an important next step would be to ask whether the gains accrue to those who are targeted by the policy. We do, though, indirectly assess the question of whether enterprise zones likely help these disadvantaged workers, by asking whether the enterprise zone incentives affect the composition of employment in a manner that is more likely to be consistent with helping those who are socioeconomically disadvantaged.

4. Data, Mapping, and Methods

The challenges faced by research on enterprise zones played a central role in shaping the methods and approaches we use in this paper. With regard to the first challenge – precise identification of zone boundaries – we digitally map California's enterprise zones street-by-street rather than approximating by using census tracts, zip codes, etc. Of course the precise geographic contours of enterprise zones that we create are only useful if we can map business establishments or employment into them. The NETS data are uniquely suited to this task, as they include exact street addresses that we have geocoded to precise geographic locations.

The second challenge concerns the selection of appropriate control groups. We use two approaches. One approach we use is similar to Billings (2007) in that we consider a narrow buffer just outside the enterprise zone as a control group. Our second approach is to use areas that are later added to enterprise zones as control groups for areas original to (or added earlier to) the same enterprise zone, taking advantage of the ability of California's enterprise zones to expand numerous times; this has parallels with some of the analyses of federal zones in Busso and Kline (2007). In our view, this latter approach

provides the most reliable estimates. In addition, we estimate heavily-saturated regression models to account for remaining possible differences between treatment and control areas.

The third challenge is accounting for other geographically-targeted policies, which we address by also digitizing maps of the areas affected by two such policies that are particularly important. The most extensive is redevelopment areas, which are designed to encourage property development that removes urban blight.[10] Cities and counties in California administer hundreds of redevelopment agencies (Dardia, 1998). Many of these redevelopment areas partially overlap with or are adjacent to enterprise zones. In addition, there are three federal programs – Renewal Communities, Enterprise Communities, and Empowerment Zones – with a variety of benefits similar to those in state enterprise zones.[11] These can also overlap with enterprise zones or our control areas.

And finally, with regard to the fourth challenge – using appropriate outcome variables defined for the right areas – we focus on the effects of enterprise zones on job growth in the zones compared to control areas. We also examine their effects on establishment counts and the composition of employment. In particular, because the hiring credit is capped per worker, firms in industries that hire lower-wage workers would see their labor costs reduced by a higher percentage than firms in high-wage industries, and the program's tax incentives that target machinery and property are most likely to benefit manufacturing enterprises. We therefore estimate the effects of enterprise zones on the shares of employment in low-wage industries or manufacturing. The next two subsections explain the mapping procedures and the statistical models we use.

4.1. Data and Geographic Methods

We use the National Establishment Time-Series (NETS) database and GIS software to address

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[10] Activities qualifying for redevelopment area benefits include the "rehabilitation/reconstruction of existing structures, the redesign/replanning of areas with inefficient site layout, the demolition and clearance of existing structures, the construction/rehabilitation of affordable housing and the construction of public facilities including, but not limited to, public buildings, streets, sidewalks, sewers, storm drains, water systems and street lights" (California Redevelopment Association, 2008). Redevelopment is typically financed via tax-increment revenue.
[11] For discussion of federal benefits, see Busso and Kline (2007), U.S. Department of Housing and Urban Development (2003), and http://www.hud.gov/offices/cpd/economicdevelopment/programs/rc/about/timeline.cfm (viewed October 10, 2006).

many of the difficulties and complications that arise in delineating the boundaries of areas affected by

enterprise zone incentives, and measuring the effects of these incentives on affected businesses. The

NETS is a national, longitudinal file of the universe of business establishments, created by Walls &

Associates using establishment-level data from Dun & Bradstreet. Our extract of the NETS covers all of

California over the period 1992-2004. The NETS has a couple of central features that make it well-suited

to studying the effects of California's enterprise zones. First, it provides exact street addresses for

establishments in every year, allowing us to identify location precisely rather than having to aggregate to

the tract or zip code level, once the enterprise zones are mapped. Second, it includes detailed industry

information, allowing us to look not only at changes in the level of employment and number of

establishments but also in the composition of employment.[12]

Preparing the data for analysis involved two processes: digitizing enterprise zone maps (as well as

maps for areas covered by other policies), and geocoding establishments in the NETS so that they can be

mapped. The geocoding is fairly standard and is explained in Appendix A. The mapping, however, is

central to this research, and so we explain it in some detail.

4.1.1. Mapping Enterprise Zones and Businesses

Mapping establishments to enterprise zones requires GIS maps ("shapefiles") of the zones, and our

identification strategy requires historical as well as current maps in order to distinguish original zone

definitions from expansion areas. As these shapefiles are not available, we had to create historical and

current enterprise zone maps from official lists of street address ranges and the years they were included

in the zone; these lists are provided by local zone administrators to HCD.[13] The method for selection of

street address ranges in GIS is described in Appendix B. Table 1 lists the enterprise zones in the state, the

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[12] Neumark et al. (2007) conducted a detailed investigation of the quality of the NETS data along numerous dimensions, including issues raised in earlier criticism of the Dun & Bradstreet data from which the NETS is constructed (Davis et al., 1996). They concluded that the NETS by and large provides reliable measurement of employment levels, births and deaths, business relocations, etc.

[13] Available at http://www.hcd.ca.gov/fa/cdbg/ez/enterprise/ (viewed November 1, 2006). These lists are used by California's Franchise Tax Board to determine whether establishments qualify for benefits. In some cases date ranges were missing or ambiguous in the files listed on the HCD webpage, in which case we contacted zone administrators directly to obtain the requisite information. In the majority of cases zone administrators were able to provide us with clarifying information.

years when they were initially designated, and the number of expansions (if any). The table also shows a handful of enterprise zones – mainly smaller ones – for which the street list information was either unavailable or inconsistent and which were therefore dropped from the analysis.

Because the date each address range was added to the zone is contained in the underlying data for each hypothetical address, we can select street ranges for the year in which the street range would have entered the zone. Figure 1 displays the results for the San Diego Barrio Logan zone. The red streets represent the original zone (1987), the light blue streets represent the first expansion to the zone (1991), and the two purple streets are the only streets added in the final expansion for this zone (1998). This map also illustrates that a polygon of the outer boundary by year would miss much detail. There are streets that were not included in the original zone that are internal – i.e., surrounded by zone streets. If we simply used outer boundaries we would have some misclassification of areas. In fact these internal streets in some cases constitute significant parts of the control areas that we use in our analysis. There are a handful of cases where we are unable to determine if a street belongs in a zone. This can occur if a street is not listed as belonging to a zone but appears to be completely surrounded by streets in the zone, which happens, for example, when a street has been developed subsequent to zone designation or expansion but the street lists from HCD do not yet reflect this information. For the main analysis we exclude these questionable streets, but we also verify that our analysis is not affected by including them in the zones.

After creating the GIS shapefile with all zone streets, we display the zone streets and the geocoded businesses in the same map and then select businesses based on their location, by year, in the enterprise zone treatment or control areas. Because geocoded longitude and latitude assigned to establishments corresponds to the center of the street on which they are located, some modifications had to be implemented for the correct classification of whether a business was inside an enterprise zone for streets on the boundaries of zones, by determining on which side of a street a business was located; these modifications are described in Appendix C.

Overall, our approach to determining whether businesses are in or out of a zone in each year was successful. We checked the error rate by comparing the final variable created for the enterprise data

indicating zone status in various years against the original zone ranges from the street address lists for San

Diego (a city zone) and Yuba Sutter (a rural zone), for random samples of observations, finding both to

have error rates of less than 1%. However, our approach was more problematic for the zones in Los

Angeles, for which the mapping of enterprise zones was much more complicated because of the large

numbers of street ranges (covering 103 pages) and the four separate zones in the city. Appendix D

describes modifications to our mapping procedures to handle this complexity. For Los Angeles, we end

up with a classification error rate in the 5-6% range – higher than for other enterprise zones because of the

unavoidable complexity of the Los Angeles zones.

4.1.2. Mapping Redevelopment Areas and Federal Zones

Redevelopment areas are included in the analysis if they are located within one mile of an

enterprise zone boundary. For each enterprise zone, the overlapping and surrounding redevelopment

agencies were found by combining information from the California State Controller's Office's

Redevelopment Agencies Annual Report[14] with information from Google Maps, to determine which

redevelopment agencies within that enterprise zone's county were located near that enterprise zone. We

then contacted the agencies online or by phone to obtain maps of the redevelopment areas administered

by that agency. These maps could take a few forms, including GIS files, PDF files, or paper maps. We

used information from the agencies and the Controller's report to determine when areas had been created,

when they would expire, and where the area boundaries changed during the study period. We then used

the maps and this information to create the final files for use in the analysis. If GIS maps were available,

we edited these as necessary if there were boundary changes not reflected in the most current map. This

might involve cutting existing polygons or creating new polygons using GIS software. If PDF or paper

maps were available, we used these to draw polygons that corresponded to the maps.[15] There were some

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[14] Table 2, http://www.sco.ca.gov/ard/local/locrep/index.shtml (viewed August 28, 2008).

[15] Drawing and editing polygons was done utilizing StreetMap, following the boundaries in the redevelopment area maps. Streets were followed down the center if the area boundary followed the center of the street or about 30 feet to either side if the area included both or neither sides of the street. This ensured that establishments, which were geocoded to be located 10 feet from the center of the street, were properly included in or excluded from the area. The points that connect the edges of the polygons were placed along the streets as closely as was required to ensure

areas for which it was impossible to tell from the map, from the boundary description, or from talking with people at the redevelopment agency whether the boundary followed the center of the street or included both sides of the street. For these areas the boundary was drawn down the center of the street. Then, as for the maps of enterprise zones, we added dates to each polygon.[16]

Information on the locations of federal designated zones comes from the U.S. Department of Housing and Urban Development.[17] These zones are listed in Table 2. We added beginning and ending dates for each area to the resulting polygon for each federal designated community. There are some designated communities that changed status during the period of analysis. However, because we treat these federal programs identically in terms of their potential economic impact, the beginning date assigned to each zone is the first year when they were designated federally.[18] As an example of the combination of all of the information on geographically-targeted incentives, Figure 2 displays the redevelopment areas, the federal zones, and the state enterprise zone streets for Santa Ana.

4.2. Statistical Approach

For any enterprise zone, we define a set of subzones consisting of the original zone plus each expansion. An observation, then, is a subzone-year pair. For example, suppose that a zone is designated in year 1, and expands only once, in year 5, and that there are 10 years of data. Then this zone contributes 20 observations – 10 years of observations on the originally-designated area, and 10 years of observations on the expansion area.

Our estimates of the effects of enterprise zones come from comparing changes in outcomes

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that the boundary was less than 10 feet from the center of the street if the boundary included one side or more than 30 feet from the center of the street if the boundary included both or neither sides. This placement depended on how much the streets curved.

[16] For one redevelopment area, overlapping with the Lindsay enterprise zone, we were unable to obtain maps or descriptions of its original 1986 or its amended 1993 boundary and only obtained those for its amended 1995 boundary. We use the 1995 boundary for all years of the analysis.

[17] Specifically, we use GIS boundary files that were available from the HUD website (http://www.hud.gov/offices/cpd/economicdevelopment/programs/rc/tour/ca/index.cfm, viewed July 20, 2007). The files have since been removed and replaced with tables containing the 1990 and 2000 census tracts that make up the Renewal Communities, Empowerment Zones and Enterprise Communities (http://www.hud.gov/offices/cpd/systems/mapping/rcezec/boundaryfiles/metadata.htm, viewed September 30, 2008). This information still allows creating the RC/EZ/EC boundaries and incorporating them into GIS.

[18] For the same reason, although Los Angeles has both a Renewal Community and an Empowerment Zone, we have appended the two together.

associated with an area becoming designated as an enterprise zone to changes in areas for which

enterprise zone status does not change. Because economic conditions vary across areas, it is important to

identify an appropriate control group. One approach we use is to restrict attention to a very narrow control

ring. In particular, based on our GIS maps of enterprise zones, we choose an area of fixed, relatively small

distance from the outer boundary of an enterprise zone – 1,000 feet – on the presumption that economic

conditions, aside from the effects of the enterprise zone, are likely to be very similar in the treated area

that became an enterprise zone and the surrounding, nearby control area.[19] To illustrate, Figure 3 shows

the map for the Santa Ana enterprise zone, displaying the initially-designated streets, the expansion

streets, and the 1,000-foot control ring. When we include control rings, we generate an additional

observation for each year's data on each control ring; we also refer to the control ring as a subzone.

A second and potentially more reliable source of identifying information comes from variation

strictly within the zone. In particular, we can compare what happens when an areas of a zone is

designated relative to changes in areas that were designated earlier or will be designated later. This

identifying information is likely even more reliable because the control areas consist *only* of areas that

were included in the zone at some point during the sample period. That is, it has been *demonstrated*

through the policy process that the areas in the groups used for this analysis were appropriate for

enterprise zone designation.[20] However, we need to include the control rings to identify the effects of

enterprise zones in some of the richer specifications we estimate.

We index the geographic locations corresponding to each enterprise zone indexed by $j = 1, \ldots, J$,

which include the zone itself and can include the control ring. We have observations over time, indexed

by $t = 1, \ldots, T$. We define subzones within j, indexed by k, with $k = 0, \ldots, K_j$; $k = 0$ for the part of j that is

never in a zone, and $k = 1, \ldots, K_j$ for the parts that become a zone initially and with each expansion. The

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[19] In some sensitivity analyses, we also report results using a 2,500-foot control ring. Because the results are insensitive we did not explore using different dimensions for this control ring.

[20] Perhaps the area outside the zone was already conducive to job growth, and that is why it was not included. In that case, comparing what happened in designated areas to what happened in the control ring would suggest that enterprise zones reduced employment, even if this was not their actual effect. Alternatively, perhaps the area outside the zone was *not* conducive to job growth, and that is why the area was omitted. In this case we would have the opposite bias – overstating the positive impact of enterprise zones.

dependent variable Y_{jkt} is, for example, the number of jobs in a subzone. We denote by EZ_{jkt} a dummy variable for whether a location k in area j is in an enterprise zone in year t. So for the part of area of j that is never in the zone, $EZ_{jkt} = 0$ for all t; in a sub-area that becomes a zone in t', $EZ_{jkt} = 0$ for all $t < t'$, and $EZ_{jkt} = 1$ for all $t \geq t'$; and for the part that is always a zone in our sample period, $EZ_{jkt} = 1$ for all t.

We estimate the models:

$$(1) \quad Y_{jkt} = \alpha + \beta EZ_{jkt} \cdot t + \sum_{j'=1}^{J} \sum_{k'=0 or 1}^{K_j} D_{j'k'} \lambda_{j'k'} + \sum_{t'=1}^{T} D_{t'} \theta_{t'} + \sum_{j'=1}^{J} \sum_{t'=1}^{T} \{D_{j'} \cdot D_{t'}\} \tau_{j't'} + \varepsilon_{jkt}$$

$$(2) \quad Y_{jkt} = \alpha + \beta EZ_{jkt} + \sum_{j'=1}^{J} \sum_{k'=0 or 1}^{K_j} D_{j'k'} \lambda_{j'k'} + \sum_{t'=1}^{T} D_{t'} \theta_{t'} + \sum_{j'=1}^{J} \sum_{t'=1}^{T} \{D_{j'} \cdot D_{t'}\} \tau_{j't'} + \varepsilon_{jkt}$$

$$(3) \quad Y_{jkt} = \alpha + \beta EZ_{jkt} \cdot t + \beta' EZ_{jkt} + \sum_{j'=1}^{J} \sum_{k'=0 or 1}^{K_j} D_{j'k'} \lambda_{j'k'} + \sum_{t'=1}^{T} D_{t'} \theta_{t'} + \sum_{j'=1}^{J} \sum_{t'=1}^{T} \{D_{j'} \cdot D_{t'}\} \tau_{j't'} + \varepsilon_{jkt} .^{21}$$

The parameter of interest is β (as well as β' in model (3)), which measures the effect of enterprise zones on the outcome Y. In model (1), enterprise zone designation shifts the growth in Y; in model (2) it shifts the level, and in model (3) it shifts both. D_{jk}, D_t, and D_j are dummy variables for each subzone, year, and enterprise zone, respectively. The dummy variables D_{jk} capture differences common to each subzone,[22] while D_t captures aggregate changes. The term $D_j \cdot D_t$ allows for enterprise zone-specific changes over time in the outcome Y. Given that we identify effects off of subzone-level variation, we can allow arbitrary changes over time for each enterprise zone j and still identify β.

Given that we allow separate dummy variables for each subzone, an area jk that is in an enterprise zone for the entire sample period contributes nothing to the identification of β, as D_{jk} and EZ_{jkt} are identical for all t. In this case, we have a pure difference-in-differences estimator that identifies β only from subzones that change status, relative to those that do not. Because the data begin in 1992, whereas most zones were originally designated prior to that year, much of our identifying information comes from

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[21] Note that the sum over k' begins with zero if the control ring is included, and one if it is not.
[22] For example, in the estimations in which we include the control rings, the dummy variables for each control ring will account for differences in zoning, job density, etc., in areas that were never part of enterprise zones.

expansions.[23] Thus, interpreting our results as estimating "the" effects of enterprise zones hinges on the assumption that the effects of original designations and expansions are the same.[24] We present some results that seek to separately identify the effects of initial zone designations and expansions, and find no significant evidence of differences.

We can also add subzone-specific linear time trends, or some other parameterized function of time interacted with the subzone-specific dummy variables D_{jk}. For the most part, we rely on a fairly simple form of this type of specification that simply allows different trends in all areas designated as enterprise zones and the control areas. In particular, define a dummy variable $EZever_{jk}$ to equal one for subzones that are ever part of a zone, and zero for the control areas. We then augment model (1), for example, to be

$$(4) \qquad Y_{jkt} = \alpha + \beta EZ_{jkt} \cdot t + \gamma EZever_{jk} + \sum_{j'=1}^{J} \sum_{k'=0 or 1}^{K_j} D_{j'k'} \lambda_{j'k'} + \sum_{t'=1}^{T} D_{t'} \theta_{t'} + \sum_{j'=1}^{J} \sum_{t'=1}^{T} \{D_{j'} \cdot D_{t'}\} \tau_{j't'} + \varepsilon_{jkt}.$$

Note that we cannot estimate this specification for the subsample of zones excluding the control rings, since *EZever* is always equal to one for this subsample. We also report estimates from specifications in which every subzone has its own linear trend.

We account for other geographically-targeted policies in two steps. First, we redefine subzone-year pairs to represent status with regard not only to whether and when they became part of an enterprise zone but also whether and when they became part of a redevelopment area or federal zone. As a result, there are far more subzones. Second, we modify the above specifications to include dummy variables indicating whether each subzone k is in a redevelopment area (or federal zone) in year t. We also include the enterprise zone dummy variables as well as interactions between these. Thus, we do not restrict the effects of the different kinds of zones to be additive, but rather allow for the possibility, for example, that state

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[23] For three of the 26 zones, there is no expansion and the original zone was created before the first year for which NETS data are available, implying that only 23 zones contribute identifying information.

[24] In our survey of local enterprise zone administrators, we asked why zones expanded when and where they did. Two main reasons emerged. First, zones often expanded to benefit businesses that were moving to or growing in areas just outside the enterprise zone. Second, zones sometimes expanded to incorporate areas newly designated as commercial or industrial by the local planning process. To the extent that zones expanded where businesses planned to relocate or grow, zone expansions were sometimes the effect rather than the cause of employment growth; thus, our estimates of the effect of the enterprise zone program on employment would be biased upward, strengthening our findings of no positive employment effects of enterprise zones.

enterprise zone benefits have different effects if the state enterprise zone overlaps with a federal zone.

In all of the estimations, to allow for arbitrary correlations over time within areas and across observations on the subzones of each zone, we use standard errors that cluster on the enterprise zone only; this also allows for different error variances across zones. Our tables report the standard cluster-robust standard errors. However, as noted above, we do not have data on a large number of zones, so the usual asymptotics under which these standard errors are consistent, and confidence intervals therefore provide the correct coverage, may not apply. Cameron et al. (2008) have shown that using the wild bootstrap, modified to account for clustering,[25] provides confidence intervals for the *t*-statistics based on the standard cluster-robust standard errors with coverage probabilities that are approximately correct even when the number of groups (zones, in our case) is quite small.[26] In addition to the standard cluster-robust standard errors, therefore, we have also calculated these bootstrapped confidence intervals, and in each table report whether the estimated effects of enterprise zones are statistically significant at various significance levels based on the bootstrap results. As it turns out, some differences emerge, but virtually never for the employment results.

5. The Effects of Enterprise Zones on Employment and Business Establishments

5.1. Enterprise Zone Employment and Establishments in the Context of the State's Economy

Table 3 presents descriptive information on the enterprise zones we study. Column (1) reports employment in each enterprise zone in our sample as of 2004. The zones are sorted from highest to lowest employment levels. As reported at the bottom of column (1), overall employment statewide in these enterprise zones is about 1.38 million, and employment in the control rings used in our empirical analysis (extending 1,000 feet from the zone boundaries) is about 580,000. Overall, employment in the counties in which the zones we study are located is 12.6 million, so that enterprise zone employment is about 11% of the total. Statewide employment in 2004, based on the NETS data, was 16.4 million, and employment in

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[25] In particular, the bootstrapping is on the clusters rather than the individual observations.
[26] In their Monte Carlo simulations, when the number of groups is in the 20's, confidence intervals based on the standard cluster-robust standard errors are fairly accurate; but this result need not carry over to our particular specification.

all counties with enterprise zones – whether or not we could construct maps for those zones – was about 14.2 million. Thus, if we assume that the share of county employment represented by enterprise zones is the same in the counties for which we do not have zone maps as for the counties for which we could construct these maps, then our enterprise zones represent 89% (12.7/14.2) of enterprise zone employment in the state. Columns (2)-(4) provide information on enterprise zone employment relative to county and statewide employment. The shares of enterprise zones in county employment vary a good deal across counties, varying from a high of 52.8% in Shasta Metro to a low of 0.7% in Altadena/Pasadena. Column (4) indicates that the large zones (Los Angeles, San Francisco, Santa Ana, and Oakland) each account, *on their own*, for 1% or more of total statewide employment.[27]

5.2. Enterprise Zones: Initially-Designated Areas, Expansions, and Control Rings

Table 4 reports some figures for the sample as a whole (i.e., covering the enterprise zones and the control rings), as well as the enterprise zone separately – including the originally-designated areas as well as the expansions separately – and the 1,000-foot control ring. These are reported for 1992, the first year of the sample.[28] As indicated in the first row, enterprise zone employment constitutes about 69% of total employment in the zones and the control rings, and of this, about 72% is in the areas originally designated as part of zones. The last three rows report establishment size and composition. Establishment size in the zones and the control rings is quite similar (around 14.5), although slightly higher in the enterprise zone expansion areas than in the originally-designated areas. The same is true of the share of employees in low-wage industries.[29] The share in manufacturing is somewhat higher in the zone expansion areas, and somewhat lower in the control rings. It is certainly the case that the three types of areas are not identical

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[27] Similar figures for the number of establishments indicate that the establishments in the enterprise zones we study are 6.5% of the statewide total, and 8.7% of establishments in the counties in which they are located. In total, we have data on about 124,000 enterprise zone establishments, about 58,000 in control rings, relative to a statewide total of 1.6 million establishments, 1.4 million of which are in the counties with enterprise zones in our study. As for employment, the largest zones (in this case Los Angeles and San Francisco) each account for 1% of more of the total statewide number of establishments.

[28] Ideally, we would like pre-treatment comparisons. However, many of the areas in the original zone designations were so designated before 1992, and there is no pre-treatment year for the control rings.

[29] We ranked industries by average pay based on 2004 data from the Quarterly Census of Employment and Wages, dividing NAICS industry subsectors into three groups, each containing approximately one-third of the workforce.

in terms of these measures; but it would be quite surprising if they were. Nonetheless, there is plenty of employment in the expansion areas and the control rings, and the types of establishments do not appear inordinately different across them. In the empirical analysis, of course, we control for initial or time-invariant differences between the areas by including subzone fixed effects, and also examine evidence on any differences in things such as prior trends in employment or job growth in the periods leading up to enterprise zone designations or expansions.

5.3. The Effects of Enterprise Zones on Jobs and Businesses

We begin our analysis of the economic effects of enterprise zones by looking at their effects on employment and the number of business establishments. We have already argued why the employment effect is central. Information on effects on the number of establishments, coupled with information on employment effects, is informative about whether enterprise zones lead to larger establishments (for example, fewer establishments coupled with no effect on employment) or smaller establishments, which is likely related to the question of whether enterprise zones lead to the creation of more new businesses, although we do not explore the latter question directly.

5.3.1. Descriptive Statistics

Table 5 presents a simple descriptive analysis. This table treats each subzone-year pair as an observation, as we do in the regression analysis, so that a subzone is classified by year as to whether or not it is in the zone. Reflecting what we regard as the cleanest way to measure the effects of enterprise zones, the control rings are not used in this table, and instead comparisons are only between subzones currently in enterprise zones, and subzones not currently in enterprise zones but which are in enterprise zones in later years. Columns (1) and (2) report unweighted averages of annual percentage changes in employment and the number of establishments, and suggest that for both employment and establishments – but especially employment – the rate of growth was faster after areas were included in enterprise zones.

However, given that there is tremendous variation in size across zones (see Table 4), small zones may contribute large percentage changes but small absolute changes. In columns (3)-(6) we report the same descriptive statistics for below-median and above-median size subzones (in terms of 1992

employment or number of establishments). These columns show that, for employment, the small

subzones contribute larger percentage changes in both directions. Moreover, for the larger subzones,

which obviously represent a far larger number of jobs or establishments, growth in jobs and the number of

establishments was *lower* in the enterprise zones; the difference is much sharper for job growth.

Weighting the observations by either employment or number of establishments thus gives us

estimates that are more representative of what actually happens to jobs or establishments statewide. As

reported in columns (7) and (8), when we weight by base-year levels, the weighted estimates are much

closer to those using the large subzones. The evidence now suggests that enterprise zones slightly reduced

the growth of jobs, with a fairly small relative difference of 0.5% slower growth in enterprise zones. In

contrast, enterprise zones appear to have slightly increased the growth rate in the number of

establishments.

5.3.2. Basic Regression Estimates

The descriptive statistics reported in Table 5 do not account for other influences on employment

and the number of establishments that we want to disentangle from the actual effects of enterprise zones.

Regression estimates ignoring a number of complications, including the overlap of enterprise zones with

areas affected by other geographically-targeted polices, are reported in Table 6. The dependent variable is

the log of employment or the number of establishments.[30] The model includes year dummy variables to

account for the possibility that enterprise zones tended to be established in periods of either particularly

high or low employment (or establishment) growth across all of the regions included in our sample. The

model also includes dummy variables for each subzone to control for any characteristics (education

levels, industry mix, infrastructure, size, etc.) that are time-invariant; alternatively, these can be thought of

as controlling for baseline values of these characteristics. Finally, the model includes a full set of

enterprise zone-year interactions, to allow for differences in growth rates over time across the broad area

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[30] As indicated in the notes to the table, in the handful of cases where employment (or the number of
establishments) was zero (26 observations), we substituted one for zero before taking logs. This can be viewed as
perhaps introducing the slightest measurement error, or presuming that the data are not sufficiently accurate to
distinguish between zero and one job or establishment in a cell. Regardless, we verified that simply dropping these
cases had no impact on the estimates.

covered by a zone, its expansions, and the associated control ring (when included). All estimates are weighted by 1992 employment levels or numbers of establishments. The control rings are included in columns (1) and (2) but excluded in columns (3) and (4); we view the latter specifications as preferable, when they can be estimated.

In Panel A, the key independent variable is an interaction between a dummy variable for enterprise zones and a linear time trend. The estimated coefficient of this interaction represents the approximate change in the growth rate of jobs or establishments attributable to enterprise zones. The estimates provide no evidence that enterprise zones boost the rate of job growth. The estimates (in columns (1) and (3)) are small and statistically insignificant, and in the preferred specification in column (3) the estimate is negative rather than positive. With regard to establishments, both estimates indicate that enterprise zones slow the rate of growth of the number of businesses, although the estimated effect is not significant, based on the bootstrap results, in the preferred specification (column (4)).

The model specification in Panel B is augmented to allow for a different underlying trend in the treatment (enterprise zone) and control ring areas (taken as a whole), irrespective of when the various subzones actually were designated as part of the enterprise zone. This allows for the possibility, for example, that enterprise zones were established in areas that had particularly slow job growth relative to the control rings, which could mask positive effects of enterprise zones. Since this specification utilizes the difference between enterprise zone and control ring areas, we cannot estimate it excluding the control rings. As Panel B of Table 6 shows, the trends in areas that became parts of enterprise zones are if anything positive (although not significant), so that the estimated effects of enterprise zones on growth in jobs or establishments becomes slightly more negative, although remaining statistically insignificant.

As a more flexible way of asking whether enterprise zone designation shifts the rate of employment growth (on which we focus), we estimated a model for first differences of log employment as a function of many leads and lags of the enterprise zone dummy variable. The first difference transformation, applied to equation (1), means that the interaction between the enterprise zone dummy variable and the time trend becomes simply an enterprise zone dummy variable, the coefficient of which now measures the

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shift in the growth rate of employment. The estimated coefficients of many leads and lags reveals changes in areas before they were designated as enterprise zones, in a much more flexible fashion than in the specification in Panel B. For example, the leads reveal whether enterprise zones have tended to be established in areas that had transitory downturns in employment growth relative to other areas, in which case our finding of no effect would be strengthened (because the mean reversion would look like a positive treatment effect). Alternatively, if zones are established in areas doing particularly well just before designation, perhaps because such areas have better organized constituents for capturing an enterprise zone, then the estimated effects from the simple model might fail to detect longer-run positive effects of enterprise zone designation on the rate of job growth. Similarly, the many lags allow the data to tell us whether over the longer-term the effects of enterprise zones look different from what is implied by the one-time contemporaneous shift in the growth rate implied by equation (1).[31]

Figure 4 displays the results for the specification both with and without the control ring. The figure reports the leading (to the left) and lagged (to the right) coefficient estimates, as well as the upper and lower limits of the standard cluster-based 95-percent confidence intervals for each estimate.[32] The figures – whether with or without the control ring – do not exhibit any evidence indicating that the basic specification obscures more interesting results. For example, there is no evidence of leading effects of enterprise zones because they are either established in places doing particularly well or particularly badly; similarly, there is no evidence that employment growth rates increase more further from the date of enterprise zone designation. Rather, the results in Figure 4 cement the view that enterprise zones do not affect job growth.

We regard the specifications in Panels A and B as most natural, since the lowering of costs associated with enterprise zone designation should imply – at least until the supply of land becomes constrained – that enterprise zones should get a larger share of businesses and jobs stemming from the steady upward growth in population and output. An alternative possibility, however, is that there is a

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[31] This is an "event study" form of the analysis; see Jacobson et al. (1993) for a nice exposition.
[32] The larger confidence intervals for the relatively long leads reflect the fact that we can identify these long leads for relatively few subzones – mainly those designated late in the sample period.

relatively rapid increase in either jobs or establishments, after which rents adjust to offset the cost advantages, so that there is more of a one-time shift in the dependent variables. We therefore augment the specification, in Panels C and D, to allow enterprise zone designation to shift both the levels and the growth rates of jobs and businesses. We again find no significant effect of enterprise zones. And finally, we restrict the specification to allow only shifts in levels, because it may be hard to precisely estimate both effects simultaneously. These estimates, reported in Panels E and F, again reveal no significant effect on jobs; the negative effect on the number of establishments in the estimation without control rings is again not significant based on the bootstrap results.[33]

Figure 5 reports results similar specification to Figure 4, but now for the specification that directly adds leads and lags to the corresponding equation (2) that simply detects shifts in the level of (log) employment associated with enterprise zone designation. As before, the figure gives no indication that the simple dummy variable specification reported in Table 6 masks any greater richness that might suggest different effects of enterprise zones, whether positive or negative.

If the enterprise zone program has positive spillovers, encouraging employment growth not only within zone boundaries but just outside zone boundaries as well, then we might find no effect of enterprise zones on employment because we are comparing enterprise zones to immediately neighboring areas.[34] By using two different control groups – future expansion areas and control rings – we can assess whether spillovers color our results. Future expansion areas are closer to current enterprise zone areas geographically than control rings are, so any spillover effects should be greater in future expansion areas than in control rings. Evidence of positive employment effects when using the control rings but not when using only the future expansion areas would suggest that zones create positive spillover effects in neighboring areas. However, since our results with and without control rings are similar, we discount the

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[33] The specifications in Panels E and F might be more likely to detect short-run shifts in outcomes associated with enterprise zones, while the specifications in Panels A and B would more likely capture longer-run effects. The failure to find evidence of employment effects of enterprise zone effects in either type of specification is consistent with findings – reported in Figure 4 – that adding explicit lagged (or leading) enterprise zone variables similarly led to no evidence of employment effects.

[34] Spillovers could stem from a number of sources, including increased retail "traffic," rising incomes of nearby residents, and changes in infrastructure.

possibility of positive spillover effects.[35]

Perhaps a more likely scenario, especially given our research design, is that there are negative spillover effects, with enterprise zones pulling jobs and businesses away from nearby areas. Given that our control areas are geographically close to our treatment areas, it might be fairly easy for businesses to move to take advantage of enterprise zone incentives (without, for example, inconveniencing their workforce). The similarity of results with and without control rings also undermines this possibility. Moreover, such negative spillovers would tend to produce evidence that enterprise zones *do* encourage job growth relative to control areas. Thus, if there were negative spillovers, our conclusion that there are not positive employment effects would only be reinforced.

Although the evidence on number of establishments is weak, one possible interpretation of a decline in the establishment count coupled with no change in employment – which implies that establishments are becoming larger – is that there are fixed costs to taking advantage of enterprise zone benefits, and large establishments (or firms) are therefore more likely to find enterprise zone benefits attractive. If this interpretation is valid, it suggests that enterprise zone policies do not particularly favor and may even adversely affect entrepreneurship in the form of small business creation.[36]

5.3.3. Accounting for Other Local Economic Policies

We next turn to the analysis where we account for the overlap between state enterprise zones and redevelopment areas or federal zones. Table 7 reports the share of enterprise zone employment that is in either redevelopment areas or federal zones, in the last year of our sample. Clearly redevelopment areas cover a much wider swath of enterprise zones and generally a larger area of the zones than do the federal zones, with a couple of exceptions.

The regression models are now expanded to include a dummy variable for redevelopment areas or

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[35] Similarly, had we found evidence of a positive effect using the larger (2,500-foot) control ring, we might have reached this conclusion. However, as discussed below, the results are essentially unchanged using the larger control ring.

[36] There is a long-standing debate on whether small businesses create more jobs, on net, than large businesses. Neumark et al. (2008) provide recent evidence on this question using the NETS data pointing to a higher net job creation rate from small businesses.

federal zones, and an interaction for regions that are in both enterprise zones *and* one of these other areas. We also include separate trends in subzones that were ever in enterprise zones, and that were ever in redevelopment areas or the federal zones (depending on the specification). Although the cost of this specification is that we have to include the control rings, the benefit is that we are able to control for the possibility that the different kinds of polices were adapted in areas that had differences in underlying job growth (or establishment growth).

As reported in Table 8, in all of the estimations the estimated effects of enterprise zones in areas that do not overlap with redevelopment areas (columns (1) and (2)) or federal zones (columns (3) and (4)) – which are reported in the first row of each panel – are small and statistically insignificant. The effects of enterprise zones that overlap with these other areas comes from the sum of these estimates plus the estimated interactions between enterprise zone and either redevelopment areas or federal zones. As reported in the fourth row of each panel, these estimates are almost always negative; based on the bootstrap results, these estimates are never statistically significant.[37] There is also no evidence of significant differences between the effects of enterprise zones that are or are not part of redevelopment areas or federal zones (the interactions, reported in the third row of each panel). The main conclusion, therefore, is that there is no evidence that enterprise zones have positive effects, whether or not they are combined with these other local policies.[38,39]

5.3.4. Effects on the Composition of Employment

The results to this point suggest that enterprise zones do not affect employment growth. However,

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[37] We actually did the bootstrap inference for these effects by respecifying the model so that the effect of enterprise zones in either redevelopment areas or federal zones was captured in a single coefficient.

[38] There is some evidence of positive effects of redevelopment areas and federal zones. We do not emphasize these findings, however, as our research was not designed to assess the effects of these areas in the most definitive way, but instead simply to distinguish between different "parts" of enterprise zone areas – that do and do not overlap with redevelopment areas or federal zones. In particular, the comparison groups are either other part of enterprise zones or the rings around them, which are not necessarily the best comparison groups for estimating the effects of redevelopment areas or federal zones. In addition, the mapping of redevelopment areas is not as accurate as the mapping of enterprise zones.

[39] There are also a few other state programs focused on specific areas, including: the Los Angeles Revitalization Zone (LARZ), Local Agency Military Base Recovery Areas, the Tulare Targeted Tax Area, and Manufacturing Enhancement Areas (in Imperial County). We address potentially problems from overlap between the LARZ and the Los Angeles and Long Beach zones below.

using a criterion of overall job growth may be inappropriate. After all, one goal of enterprise zones is to help create jobs among those who are economically-disadvantaged and likely to be low-skilled. In addition, some of the enterprise zone benefits targeted on machinery and property are most likely to benefit manufacturing enterprises. Thus, it is possible that enterprise zones do not affect *overall* employment growth, but nonetheless affect the *composition* of employment growth.

The NETS data do not permit us to say anything about the workers employed by business establishments. Nonetheless, we can ask whether there is a shift toward lower-paying industries. We might not normally think of this as a good outcome, but in this case it could reflect increased hiring of less-skilled workers. On the other hand, it could be that the shift to less-skilled workers occurs *within* industries, which we would not observe. It is straightforward in the NETS to ask whether enterprise zones are associated with shifts in the share of employment in manufacturing.

The results for employment in low-wage industries, reported in the first two columns of Table 9, do not provide any indication that enterprise zones shift employment towards (or away from) low-wage industries. All of the estimated compositional effects are small and statistically insignificant. The estimates in columns (3) and (4) suggest that there is a positive and statistically significant effect on the growth rate of the share of employment in manufacturing in Panels A and C (when the control rings are excluded). However, this appears to be offset by a negative (albeit insignificant) effect on the level, as reported in Panel C, and the effect on the growth rate in Panel A is not significant based on the bootstrap results. Thus, there is not a consistent indication that enterprise zones boost manufacturing employment.

5.3.5. Sensitivity Analyses

Finally, we report on a number of sensitivity analyses. The first set of these focuses on whether our conclusions are sensitive to issues regarding the mapping, or "measurement," of the enterprise zones or the control rings. In Table 10, row 1 reports the baseline estimates from Table 6. Then rows 2-4 present estimates for the variations in how we define the enterprise zones or control rings. First, we use a 2,500-foot control ring instead of a 1,000-foot control ring. This results in little change. Second, we revert to the 1,000-foot control ring, but include questionable streets that are in the interior of the zones but are not

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explicitly listed as belonging to them. This has virtually no impact on the estimates. And third, we revert to the 1,000-foot control ring and exclude questionable streets, but we also exclude a 100-foot buffer (in any direction) from the enterprise zone boundary, to exclude observations that might be more likely to be incorrectly classified as in or out of the zone. This, too, has no substantive effect on the estimates.

Next, we consider alternative weighting schemes. In row 5 we report estimates in which we do not weight the observations. These estimates similarly point to no gains from enterprise zones, and if anything weaker evidence of declines in the number of establishments.[40] A somewhat different weighting issue arises because our unit of observation is the subzone-year pair, which implies that if a particular zone had a lot of little expansions as opposed to a smaller number of relatively larger expansions, that zone contributes more observations. However, we may not want the estimates to be weighted towards zones with more expansions.[41] One way to make the estimates representative of zones rather than subzones is to weight the observations inversely by the number of subzones. Estimates with this weighting are reported in row 6 of Table 10. This turns out to have little impact on the estimates.

Because Los Angeles is so large (and perhaps because it has so many expansions), it may have a large influence on the estimates. We therefore, in the row 7 of Table 10, report results excluding Los Angeles.[42] For the specifications with the control rings, in this case we find positive employment effects. However, for the specification without the control rings, which we regard as more reliable, we again find no effects of enterprise zones. More substantively, as we noted earlier, there is the potential for overlap between the Los Angeles Revitalization Zone (LARZ) and the Los Angeles and Long Beach enterprise zones. The LARZ offers benefits that are very similar to those of the state enterprise zone program (Assembly Jobs, Economic Development, and the Economy Committee, 2006). Because of the potential overlap and similar benefits, and given that the LARZ started in 1992, failure to account for overlap

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[40] Recall that we first raised the issue of weighting with respect to Table 5. There, we saw that, in the raw data, the percentage growth in subzones after they became part of the enterprise zone was higher. Here, in contrast, we are reporting the estimates of highly-saturated regression models.

[41] The weighting by base-year employment or establishment levels offsets this to some extent, since when a zone is divided into more subzones because of a greater number of expansions, each subzone gets a lower base-year weight.

[42] In addition, recall that mapping enterprise zone boundaries for Los Angeles was more difficult.

between the LARZ and subzones into which the Los Angeles or Long Beach zones expanded *after* 1992

can lead to misclassification of the treatment and control groups. Consequently, we excluded the Census

tracts and cities covered by the LARZ,[43] and re-estimated our models. The results, reported in row 8 of

Table 10, are very robust to this change, indicating that the overlap between the LARZ and the Los

Angeles and Long Beach enterprise zones does not affect our results.[44]

Our empirical strategy is predicated on having valid comparison groups. We have already

addressed this issue in a number of ways, but in row 9 we take this one step further and report results

from a specification that lets every subzone have its own linear trend. This allows not only for differential

linear trends in the treatment and control groups as a whole, but also in each of the different treatment and

control groups. Thus, this specification allows for the possibility – in an unrestricted fashion – that each

subzone had different underlying rates of growth of either employment or the number of businesses. The

conclusions regarding employment growth are unchanged. There is now, however, a hint of evidence of a

positive effect on the number of establishments, in one specification.

Finally, we consider two issues related to the timing of enterprise zone designation. First, we ask

whether the effects of initial enterprise zone designation differ from the effects of subsequent zone

expansions.[45] Since initial designation results from a different process than subsequent expansions, the

effects could differ. The specification in row 10 of Table 10 shows that the effect of initial designations is

not significantly different from the overall enterprise zone effect.[46] Second, we ask whether enterprise

zones became more effective at creating jobs in 1997, when the pool of workers eligible for the hiring

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[43] See http://www.hcd.ca.gov/fa/cdbg/ez/larz/ (viewed November 10, 2008).

[44] This may reflect the fact that the overlap is not extensive. Using the census tracts that include the LARZ, which encompass *more than* the actual streets covered, as of 2004 only 5% of Los Angeles enterprise zone employment and 7.7% of Long Beach enterprise zone employment was in the LARZ. These percentages are considerably lower than those covered by redevelopment areas or federal zones, as reported in Table 7.

[45] The specification includes the enterprise zone dummy variable as well as an interaction of this dummy variable with a corresponding dummy variable for the initially-designated areas only; the coefficient of the interaction measures the difference between the effect in initially-designated areas and expansion areas.

[46] We report only the model with control rings. The initial-designation estimates are identified from five zones that were designated after 1992 (and before 2004). Only two of these – Oakland and Santa Ana – had subsequent expansions (which serve as the control group when control rings are excluded), and Oakland's expansion was very small in terms of employment and therefore quite imprecise as a control group for the initial Oakland designation. Omitting control rings would mean identifying the initial designation effect essentially only from Santa Ana.

credit areas expanded to include those in TEA's. As shown in row 11 of the table, there is no evidence of this. And although not shown in the table, the differences between the effects in the two sub-periods were never statistically significant.

Overall, then, the earlier analysis plus all of our sensitivity analyses establish that our estimates indicating that state enterprise zones in California do not boost employment growth is very robust.[47] The estimates for the effects of enterprise zones on the number of establishments is perhaps less robust, with some indication that enterprise zones may reduce the number of establishments.

6. Discussion and Conclusions

Our analysis of California's enterprise zone program fails to find that the program has increased job growth. We arrived at this conclusion after drawing precise enterprise zone boundaries digitally, mapping nearly all businesses in the state, accounting for other geographically-targeted policies, and comparing employment growth in enterprise zones with carefully considered control areas. We do not assess the effect of the program on unemployment or poverty, but it is hard to see how these outcomes could improve in the absence of a positive effect on employment.[48]

At the same time, we find some evidence that enterprise zones reduce the number of establishments, which coupled with lack of an employment effect suggests that establishments are growing in size. Increasing establishment size is consistent with survey respondents' comments that smaller businesses find it less worthwhile than larger businesses do to claim enterprise zone benefits because of the administrative burden.[49] Another possibility is that increased prices for land relative to other inputs lead employers to substitute towards other inputs including labor.

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[47] Although not shown in the table, the estimates for the other specifications from Table 6 were also robust to the variations shown in Table 10.

[48] As noted earlier, if enterprise zones have beneficial spillover effects onto other nearby areas, then benefits of the program estimated from comparisons of areas inside the zone to areas outside the zone may be understated. Nonetheless, our results would still imply that enterprise zones do not *differentially* benefit the areas they target. Although we cannot rule out this possibility, we regard it as unlikely because our estimated effects were not sensitive to using control groups that extended increasingly further from the enterprise zone itself.

[49] See Kolko and Neumark (in progress). This is also consistent with evidence from a HUD survey indicating that large firms used federal enterprise zone tax credits, wage subsidies, and capital write-offs much more intensively than small firms (Hebert et al., 2001).

The lack of an effect on employment may seem surprising in light of the program's incentives. In fact, however, economic theory provides some possible explanations for the absence of an employment effect or at least a weak effect. First, as noted earlier, the strongest incentive offered by the enterprise zone program is for hiring disadvantaged workers. If there are opportunities to substitute low-skilled for higher-skilled labor, however, this incentive may induce a fair amount of "labor-labor" substitution, with weaker effects on employment overall. At the same time, this kind of substitution might still lead to beneficial shifts in the composition of employment to the extent that the program is intended to improve job opportunities for the disadvantaged. A related issue is that some of the enterprise zone benefits targeting machinery and property could lead to substitution away from labor and towards other inputs; depending on the magnitude of scale and substitution effects of lower prices for other inputs, the overall employment effect could be positive or negative. Our evidence suggests that there is no overall increase in employment. Our ability to assess whether there is labor-labor substitution is limited by the available data. All we can do is ask whether enterprise zones shift employment toward low-wage industries that might be more likely to employ these workers, and we find no such effect.[50] We find it less plausible that the enterprise zone program leads to substitution away from labor and towards other non-labor inputs, given the generosity of the hiring credit – although admittedly we do not know the magnitudes of all of the relevant elasticities.[51]

One argument that we do *not* think is the right explanation is that the incentive effect of the program is weak. If we simply divide the cost of the program by the number of jobs in enterprise zones,

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[50] A related possible beneficial effect of the program is increasing incomes of affected workers, owing to the tax credit. In the absence of employment effects, however, the gross wage (the market wage plus the tax credit) increases only if labor supply is perfectly inelastic, which does not seem plausible. The relevant elasticity is for the extensive margin of labor supply – that is, entry into the labor market in response to a higher wage. There is ample evidence of elastic labor supply on this dimension. See, for example, Juhn (1992). At the same time, Bostic and Prohofsky (2006) find positive wage effects in the very short-run (1-2 years) for low-wage affected workers (a non-representative sample of workers for whom employers claimed an enterprise-zone-specific hiring tax credit).

[51] Lynch and Zax (2008) discuss substitution between labor and other inputs. They suggest that one interpretation of their findings that Colorado's enterprise zone benefits do not boost employment at existing establishments is that there is substitution towards capital that offsets any effect of reduced labor costs. It is the case that the Colorado enterprise program has relatively strong capital subsidies and weak labor subsidies, in contrast to California; Lynch and Zax argue that the most valuable enterprise zone incentive in Colorado is a 3% investment tax credit, while the hiring tax credit is simply $500 for each new employee.

one might get this impression; using the 2005 spending figure of about $330 million (California Franchise Tax Board, 2006), and dividing by enterprise zone employment of roughly 1.4 million, yields an estimate of about $240 per worker. However, what is relevant is the effect of program subsidies at the margin; clearly a fairly large share of enterprise zone employment would occur regardless of the program. At any rate, if the program incentives are too weak to affect behavior, then it remains difficult to justify the program's costs. A second argument that also does not explain the absence of an employment effect is that capitalization of enterprise zone benefits into land values eliminates the incentive effects of the enterprise zone benefits. Even if land values rise, reductions in the relative cost of labor owing to the hiring credit still imply that employers will substitute toward labor. And indeed increased land values may reflect an outward shift in the demand for land against an upward-sloping supply of land available for businesses.[52]

We have argued that the data and strategies we use to estimate the effects of enterprise zones improve upon the existing literature along a number of dimensions. As it turns out, though, our findings are consistent with much of the literature, in particular with more the recent and what we regard as the more compelling studies. We already noted that many studies conclude that enterprise zones are ineffective at creating jobs, consistent with our conclusions. If we use the confidence intervals for our estimates to ask whether our approach "rules out" estimated magnitudes in the existing literature, again we find broad consistency, although the results for other states or federal programs may vary because of differences in incentives or other interventions. For example, the specification in Panel F of Table 6, column (1), yields an estimate of a 2.9 percent reduction in employment, with the 95-percent confidence interval ranging from an effect of −8.6 percent to 2.8 percent; the 95-percent confidence interval for the effect on the annual growth rate (Panel B) is −3.3 percent to 1.1 percent. Busso and Kline (2007, Table 10) report effects on the employment rate in a five-year interval, from their preferred specification, implying an annual growth rate that is higher by 0.6 percent, within this confidence interval. O'Keefe

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[52] Landers (2006) suggests that this capitalization occurs to some extent in industrial and commercial property values, with the magnitude of the effect depending on other factors such as the supply elasticity of land.

(2004) estimates that enterprise zone designation boosts employment growth by 3 percent per year for the first six years of enterprise zone designation – an estimate that is well outside of our confidence interval; on the other hand, she finds that this effect does not persist after six years, with the results pointing to negative but insignificant effects in years eight through 13. Elvery (2009) finds insignificant negative employment effects; for California his estimates for men and women combined are in the −0.1 to −0.6 range, quite consistent with ours. The results in Billings (2007) are not easily translatable, but if we use his estimates for existing establishments (Table 3, row 1) they suggest effects on the level of employment of approximately 1.3 to 2.1 percent,[53] which are within our confidence interval. Moreover, even researchers who find some positive and significant point estimates of the effects of state programs tend to downplay the strength of the results. As already noted, O'Keefe (2004) points out that the positive job creation effects do not last; and Billings (2007) concludes that his results "weakly support the idea that … job creation tax credits positively influence … job creation" (p. 25).

We cannot necessarily generalize conclusions regarding the absence of job creation effects of California's enterprise zone program to state enterprise programs elsewhere, because programs are not the same everywhere, although they often have a similar set of incentives.[54] The generalization is probably least valid with respect to the federal programs, which also entail other interventions including large block grants aimed at poverty reduction, as well as loans for improvements in local infrastructure, and for which the existing evidence seems to point more strongly to beneficial effects (Busso and Kline, 2007). Indeed, if the beneficial effects of federal programs hold up in further research, then the contrast with apparently ineffective state programs raises two questions. First, do these other interventions included in the federal programs account for the apparent differences in the effects of federal versus state programs, suggesting that states seeking to improve the effectiveness of their programs should incorporate these elements of federal programs? And second, aside from these other interventions, are the federal tax incentives that parallel those offered by state enterprise programs ineffective, or do they perhaps interact to enhance the

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[53] We use the sample means for all establishments from his Table 1, for the corresponding columns.
[54] See, e.g., National Conference of State Legislatures (2005).

effects of the federal block grants and infrastructure loans?

Acknowledgements

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Figure 1: Enterprise Zone Streets for San Diego (Barrio Logan), by Year

Figure 2: Santa Ana Redevelopment Areas (Magenta), Federal Zones (Purple), and State Enterprise Zone Streets as of 2004
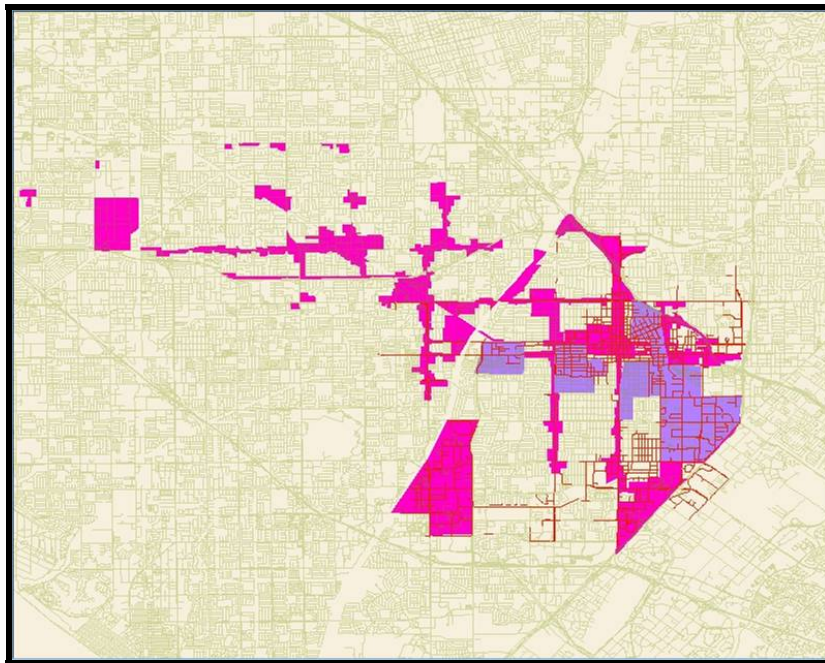
Figure 3: Santa Ana Enterprise Zone, Initial 1993 Designation (Red), 1994 Expansion (Blue), and 1,000-Foot Control Ring (Green)
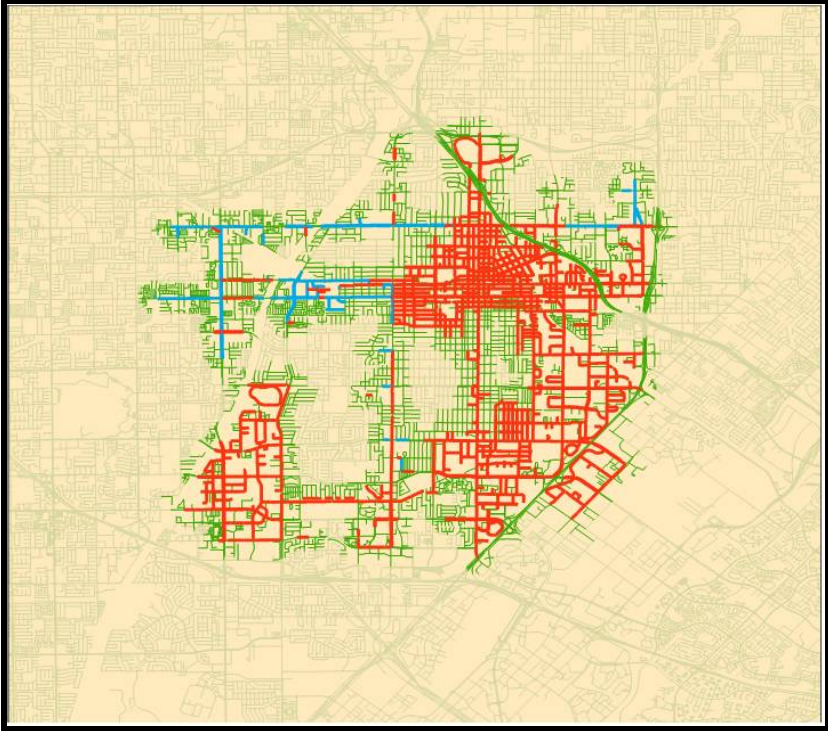
Figure 4: Event Study Estimates of Effects of Enterprise Zone Designation on Rate of Job Growth[a]

A. *With Control Ring*

B. *Without Control Ring*
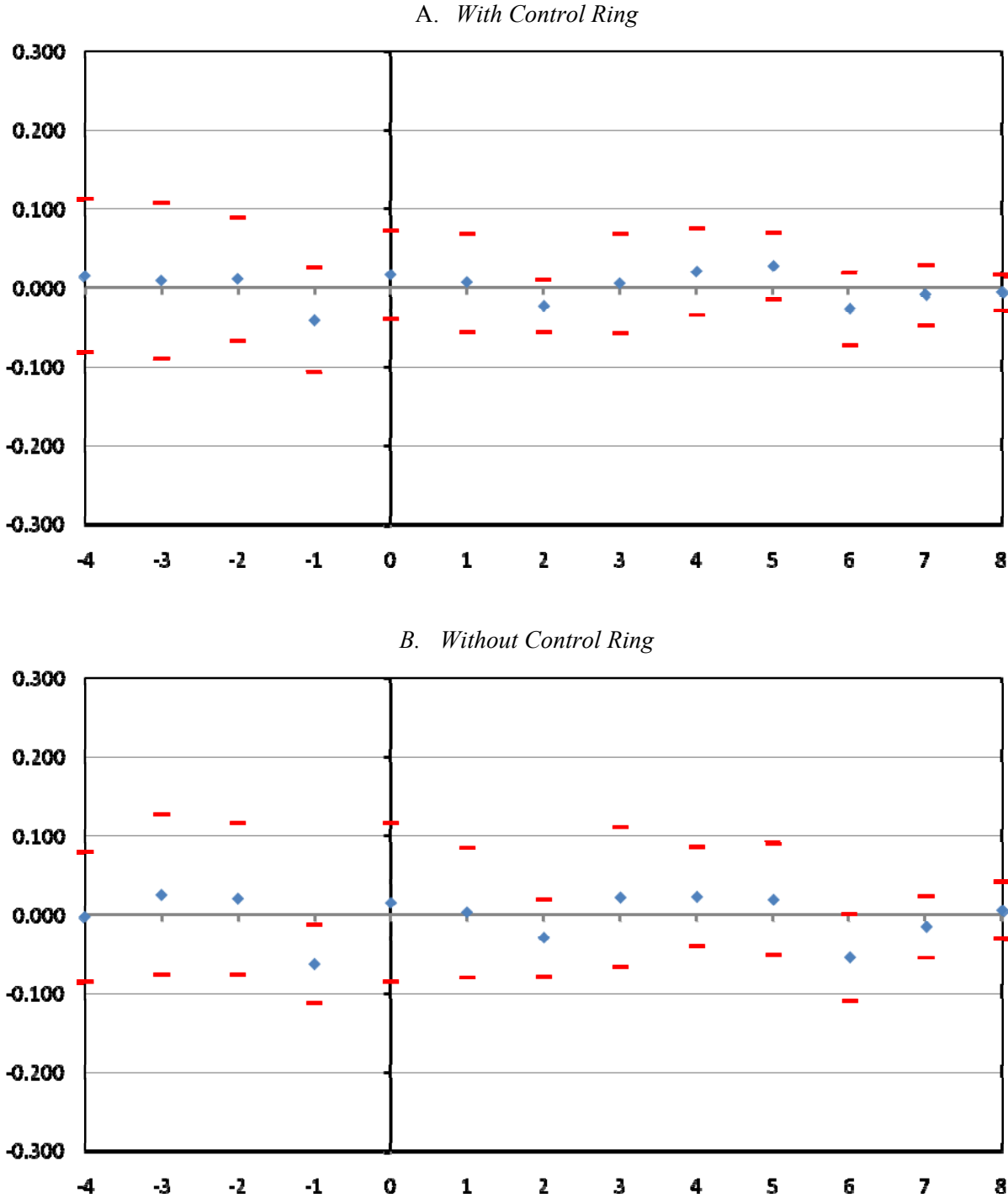
[a] Estimates are of first-differenced form of equation (1), with four leads and eight lags of enterprise zone dummy variable added. The estimated lead effects are displayed to the left of zero – i.e., prior to enterprise zone designation at time zero – and the estimated lagged effects to the right.

Figure 5: Event Study Estimates of Effects of Enterprise Zone Designation on Log Employment[a]

A. *With Control Ring*
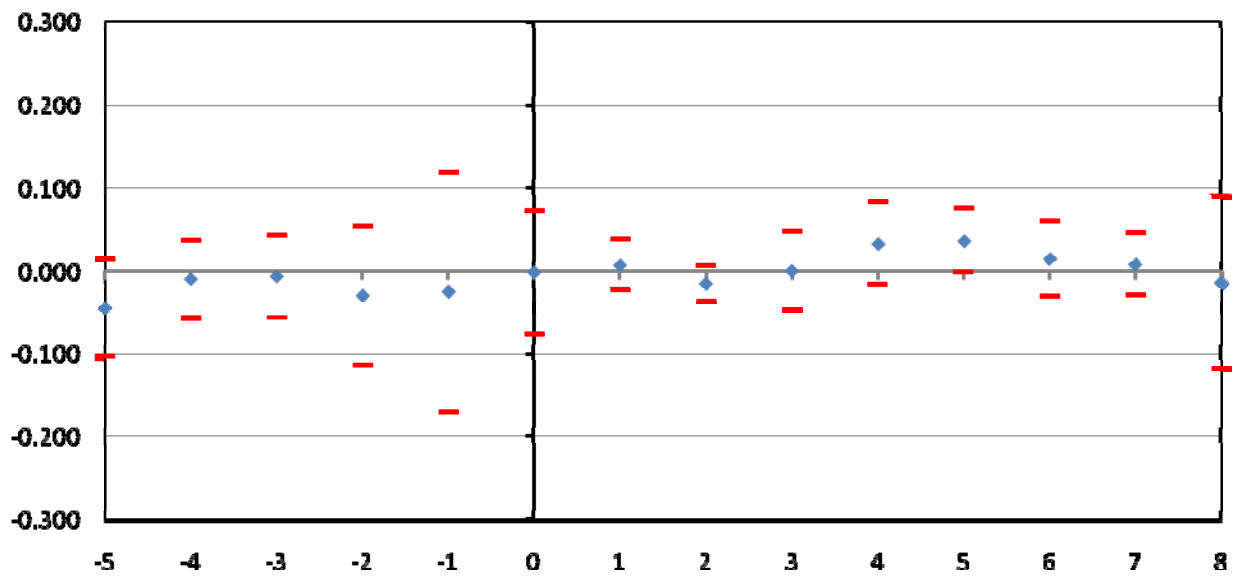
B. *Without Control Ring*
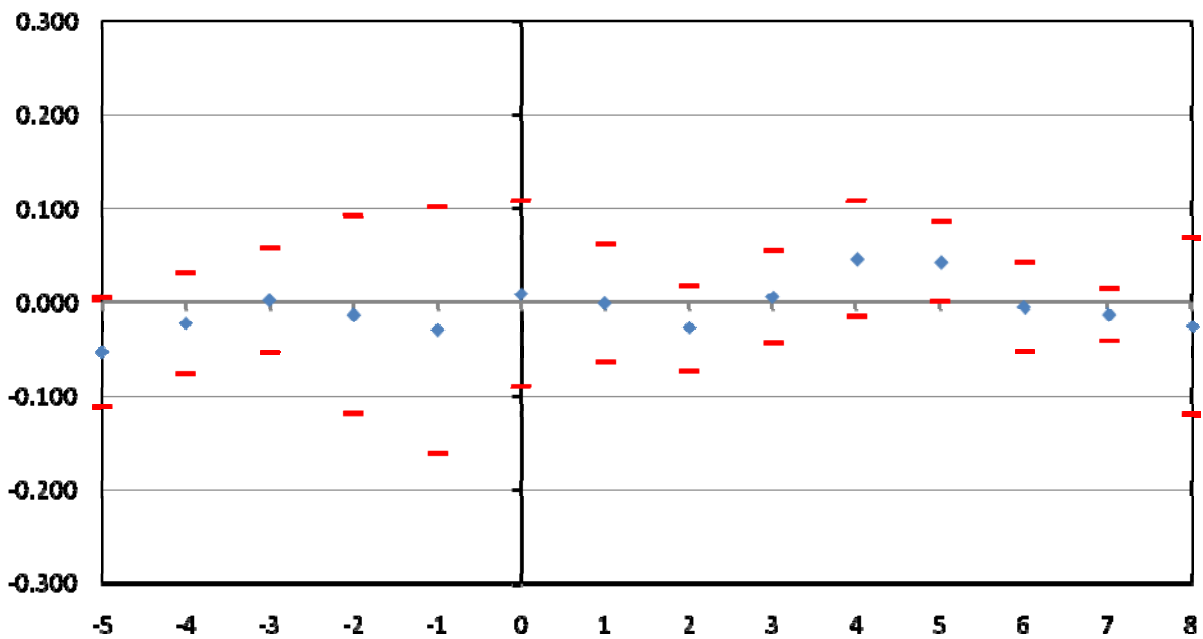
[a] Estimates are of same specification as equation (2), with five leads and eight lags of enterprise zone dummy variable added. The estimated lead effects are displayed to the left of zero – i.e., prior to enterprise zone designation at time zero – and the estimated lagged effects to the right.

Table 1: Current California Enterprise Zones and Year of Designation[a]

Enterprise zones included in the study	Year of designation	Number of expansions in the zone	Enterprise zones not included in the study	Year of designation
Altadena/Pasadena	1992	1	Agua Mansa	1986
Bakersfield	1986	3	Antelope Valley	1997
Coachella Valley	1992	2	Calexico	1986
Delano	1991	1	Fresno	1986
Eureka	1986	1	Kings County	1993
Lindsay	1997	0	Pittsburg	1988
Long Beach	1992	1	Stockton	1993
Los Angeles	...	14	Watsonville	1997
Los Angeles, Central City	1986		Barstow	2005
Los Angeles, East Side	1988		Imperial Valley	2005
Los Angeles, Harbor Area	1989		Stanislaus	2005
Los Angeles, Mid-Alameda Corridor	1986			
Los Angeles, Northeast Valley	1986			
Madera	1989	0		
Merced	1991	1		
Oakland	1993	1		
Oroville	1991	1		
Porterville	1985	0		
Richmond	1992	1		
Sacramento, Florin Perkins and Army Depot	1989	2		
Sacramento, Northgate / Norwood	1989	2		
San Diego, Barrio Logan	1987	2		
San Diego, Ysidro/Otay Mesa	1991	3		
San Francisco	1992	4		
San Jose	1986	1		
Santa Ana	1993	1		
Shafter	1995	0		
Shasta Metro	1991	2		
Shasta Valley	1993	0		
West Sacramento	1988	0		
Yuba/Sutter	1986	4		

[a] The five Los Angeles zones are treated as one large zone for the analysis. In some cases the sources listed below provided different start dates. In the cases of such discrepancies, we checked with zone administrators to verify the start date. For Coachella, because the zone started in late 1991 (November 10), we use 1992 as the first year.
Sources: http://www.caez.org/Programs/Map_of_CA_Zones.html (viewed September 19, 2008); street address changes taken from street files, found at http://www.hcd.ca.gov/fa/cdbg/ez/enterprise (viewed November 1, 2006); Assembly Jobs, Economic Development, and the Economy Committee (2006).

Table 2: Federal Designated Communities in California[a]

Date of designation	Date of expiration	Program	Overlapping state enterprise zone
12/21/1994	12/31/2009	**Round I Urban Enterprise Communities (65)**	
		Los Angeles	Los Angeles
		Oakland	Oakland
		San Diego	San Diego, Barrio Logan
		San Francisco	San Francisco
12/21/1994	12/31/2009	**Round I Supplemental Empowerment Zones (2)**	
		Los Angeles	Los Angeles
12/21/1994	12/31/2009	**Round I Enhanced Enterprise Communities (4)**	
		Oakland	Oakland
12/21/1994	12/31/2004	**Round I Rural Enterprise Communities (30)**	
		City of Watsonville	Watsonville
		Imperial County	Calexico
12/31/1998	12/31/2009	**Round II Urban Empowerment Zones (15)**	
		Santa Ana	Santa Ana
12/24/1998	12/31/2009	**Round II Rural Empowerment Zones (5)**	
		Desert Communities	Coachela Valley
12/24/1998	12/24/2009	**Round II Rural Enterprise Communities (20)**	
		Huron-Tule	Fresno
1/1/2002	12/31/2009	**Round III Urban Empowerment Zones (8)**	
		Fresno	Fresno
1/1/2002	12/31/2009	**Urban Renewal Communities (28)**	
		Orange Cove	Fresno
		Parlier	Fresno

[a] The shaded regions correspond to federal zones that overlap the state enterprise zones included in our study. Although some zones changed status during the sample period, we treat the different federal zones as homogeneous, assigning to each zone the starting year for the first year they were designated federally. Los Angeles has both a Renewal Community and an Empowerment Zone. For our analysis we have appended these two together. The numbers in parentheses in the third column are the total number of federal zones designated in each round in the entire country. The Huron-Tule, Los Angeles, San Diego, and San Francisco Enterprise Communities became Renewal Communities in 2002.
Source: U.S. General Accounting Office (2004).

Table 3: Descriptive Information on Zone Employment, 2004[a]

	Employment in enterprise zone	Employment in county	Col. 1 share of county employment	Col. 1 share of state employment
	(1)	(2)	(3)	(4)
Los Angeles	274,434	4,677,221	5.9	1.7
San Francisco	215,329	600,488	35.9	1.3
Santa Ana	175,018	1,733,164	10.1	1.1
Oakland	163,181	775,214	21.0	1.0
Long Beach	121,754	4,677,221	2.6	0.7
San Jose	98,162	984,246	10.0	0.6
Sacramento, Florin Perkins and Army Depot	40,832	624,638	6.5	0.2
Shasta Metro	40,178	76,069	52.8	0.2
Altadena/Pasadena	33,956	4,677,221	0.7	0.2
San Diego, Barrio Logan	28,624	1,440,987	2.0	0.2
West Sacramento	24,779	85,538	29.0	0.2
San Diego, Ysidro/Otay Mesa	24,196	1,440,987	1.7	0.1
Yuba/Sutter	21,853	47,581	45.9	0.1
Richmond	20,567	389,983	5.3	0.1
Eureka	18,065	50,442	35.8	0.1
Sacramento, Northgate/Norwood	15,279	624,638	2.4	0.1
Coachella Valley	11,050	586,101	1.9	0.1
Madera	9,765	38,635	25.3	0.1
Oroville	8,954	81,353	11.0	0.1
Bakersfield	8,829	242,303	3.6	0.1
Delano	6,212	242,303	2.6	0.0
Shasta Valley	5,818	18,777	31.0	0.0
Shafter	3,695	242,303	1.5	0.0
Lindsay	2,758	123,101	2.2	0.0
Porterville	2,633	123,101	2.1	0.0
Merced	641	68,050	0.9	0.0
Employment in all zones	**1,376,562**			**8.4**
Employment in control rings	**579,845**			**3.5**
Employment in all counties with zones in our sample	**12,643,891**			
Employment in all counties with enterprise zones	**14,186,945**			
Statewide employment	**16,441,979**			

[a] Figures are reported for the complete area of each zone as of 2004. In cases where a zone is mainly in one county but also extends into another county, in this table the zone is assigned to the county in which most of the zone is located. Note that some numbers repeat in column (2). This occurs when there are multiple zones in the same county.

Table 4: Descriptive Statistics for Enterprise Zones and Control Ring, 1992

	Whole sample (zones plus control rings)	Areas ever included in EZ's	Areas included in the original EZ designations	Areas included in EZ expansions	1,000-foot control ring
Employment (total)	1,953,220	1,349,629	976,119	373,510	603,591
Establishments (total)	140,969	96,752	71,006	25,746	44,217
Employees per establishment (weighted mean)	14.6	14.7	14.2	16.2	14.3
Share of employees in low-wage industries (weighted mean)	11.6	11.8	11.6	12.4	10.9
Share of employees in manufacturing (weighted mean)	8.0	8.6	7.8	10.8	6.7

Table 5: Descriptive Statistics on Employment and Establishment Growth, Full Enterprise Subzones, Within Zone Comparisons[a]

	Unweighted		Unweighted small subzones		Unweighted large subzones		Weighted by levels in 1992	
	Empl. (1)	Estab.'s (2)	Empl. (3)	Estab.'s (4)	Empl. (5)	Estab.'s (6)	Empl. (7)	Estab.'s (8)
Enterprise subzones after zone designation								
Average annual percent change, 2004-year of designation	2.3	1.8	4.4	1.6	0.2	2.1	0.3	2.1
Enterprise subzones prior to zone designation								
Average annual percent change, year of designation-1992	-0.6	1.4	-3.3	0.7	2.2	2.2	0.8	1.8

[a] There is one observation for each year for each subzone. Only areas ever included in enterprise zones in the sample period are included in this table. In columns (3)-(6), "small" and "large" refer to below or above median employment (or number of establishments) in 1992. In columns (7) and (8) annual percentage changes are weighted using employment level/number of establishments in 1992.

Table 6: Regression Estimates of Effects of Enterprise Zones[a]

	Including control rings		No control rings	
	Employment (1)	**Establishments** (2)	**Employment** (3)	**Establishments** (4)
A. Shift in growth rate				
Enterprise zone × linear trend	0.002	-0.003	-0.007	-0.008
	(0.011)	(0.002)†	(0.012)	(0.003)***
B. Model A, different trends in zone and control areas				
Enterprise zone × linear trend	-0.009	-0.005	…	…
	(0.011)	(0.004)		
Ever in enterprise zone × linear trend	0.012	0.002	…	…
	(0.012)	(0.005)		
C. Shift in growth rate and level				
Enterprise zone × linear trend	0.003	-0.003	-0.010	-0.002
	(0.011)	(0.003)	(0.013)	(0.003)
Enterprise zone	-0.037	-0.004	0.016	-0.037
	(0.042)	(0.032)	(0.026)	(0.028)
D. Model C, different trends in zone and control areas				
Enterprise zone × linear trend	-0.007	-0.005	…	…
	(0.014)	(0.004)		
Enterprise zone	-0.011	0.002	…	…
	(0.036)	(0.036)		
Ever in enterprise zone × linear trend	0.010	0.002	…	…
	(0.011)	(0.004)		
E. Shift in level				
Enterprise zone	-0.017	-0.022	-0.012	-0.042
	(0.047)	(0.024)	(0.035)	(0.021)*
F. Model E, different trends in zone and control areas				
Enterprise zone	-0.029	-0.012	…	…
	(0.029)	(0.028)		
Ever in enterprise zone × linear trend	0.004	-0.003	…	…
	(0.011)	(0.003)		
N	1,300	1,300	962	962

[a] Each column and panel reports estimates of the enterprise zone effects from a separate regression. The differences in the specification are explained in the panel headings. The dependent variables are in logs, substituting ones for zeros in levels prior to taking logs. The models all include subzone and year dummy variables, and zone-year interactions. There are 26 zones, with the total number of initial zone designations and expansions adding to 74. Thus, because we have 13 years of data, when we do the analysis without control rings we have 962 observations (74 × 13). When we include a control ring for each zone, we have 1,300 observations ({74 + 26} × 13). Standard cluster-robust standard errors (clustering on enterprise zones) are in parentheses; ***, ** and * indicate that the estimated coefficient is significant at the 1-, 5- or 10-percent level based on these standard errors. Cluster-robust wild bootstrapped confidence intervals for the *t*-statistic were also computed for the enterprise zone treatment variables ("Enterprise zone" and "Enterprise zone × linear trend"), based on 1,000 replications; †††, †† and † indicate that the estimated coefficient is significant at the 1-, 5- or 10-percent level based on these standard errors. All estimates are weighted by 1992 employment levels (columns (1) and (3)) or number of establishments (columns (2) and (4)) in each subzone.

Table 7: Employment in Enterprise Zones, Redevelopment Areas and Federal Designated Zones[a]

	% of enterprise zone employment in redevelopment areas	% of enterprise zone employment in federal zones
Altadena/Pasadena	11.6	...
Bakersfield	60.2	...
Coachella Valley	79.6	18.4
Delano	70.4	...
Eureka	58.1	...
Lindsay	...	...
Long Beach	63.4	...
Los Angeles	44.8	30.5
Madera	70.4	...
Merced	28.5	...
Oakland	82.8	...
Oroville	88.4	...
Porterville	37.1	...
Richmond	55.5	...
Sacramento, Florin Perkins and Army Depot	34.1	...
Sacramento, Northgate/Norwood	13.8	...
San Diego, Barrio Logan	52.1	74.9
San Diego, Ysidro/Otay Mesa	17.0	...
San Francisco	15.3	25.5
San Jose	59.5	...
Santa Ana	68.1	17.6
Shafter	88.3	...
Shasta Metro	67.5	...
Shasta Valley	...	...
West Sacramento	92.5	...
Yuba/Sutter	93.3	...

[a] It is possible for a redevelopment area or federal zone to overlap *only* with an enterprise zone's control ring, in which case none of the enterprise zone's employment would be in the redevelopment area or federal zone.

Table 8: Regression Estimates of Enterprise Zones Accounting for Redevelopment Areas or Federal Zones, Including Control Rings[a]

	Redevelopment areas		Federal zones	
	Employment	Establishments	Employment	Establishments
	(1)	(2)	(3)	(4)
A. Shift in growth rate				
Enterprise zone × linear trend	-0.006	-0.004	-0.011	-0.005
	(0.008)	(0.003)	(0.012)	(0.004)
Redevelopment area/federal zone × linear trend	0.008	0.003	-0.012	-0.001
	(0.004)*	(0.002)*,†	(0.011)	(0.003)
Enterprise zone × redevelopment area/ federal zone × linear trend	-0.011	-0.002	0.013	0.002
	(0.008)	(0.003)	(0.013)	(0.004)
Effect of enterprise zone in redevelopment area/federal zone	-0.017	-0.006	0.002	-0.003
	(0.008)**	(0.003)**	(0.018)	(0.004)
Ever in enterprise zone × linear trend	0.014	0.002	0.010	0.001
	(0.010)	(0.003)	(0.010)	(0.005)
Ever in redevelopment area/federal zone × linear trend	-0.010	-0.008	-0.007	-0.005
	(0.006)	(0.002)***	(0.011)	(0.003)*
B. Shift in level				
Enterprise zone	-0.024	-0.012	-0.020	-0.009
	(0.025)	(0.021)	(0.026)	(0.027)
Redevelopment area/federal zone	-0.033	-0.002	0.033	0.037
	(0.034)	(0.020)	(0.024)	(0.006)***,††
Enterprise zone × redevelopment area/ federal zone	-0.021	0.010	-0.015	-0.015
	(0.030)	(0.016)	(0.052)	(0.018)
Effect of enterprise zone in redevelopment area/federal zone	-0.044	-0.003	-0.035	-0.024
	(0.038)	(0.030)	(0.043)	(0.021)
Ever in enterprise zone × linear trend	0.006	-0.002	0.003	-0.003
	(0.010)	(0.002)	(0.009)	(0.002)
Ever in redevelopment area/federal zone × linear trend	-0.009	-0.007	-0.013	-0.006
	(0.005)*	(0.002)***	(0.008)	(0.002)***
N	4,667	4,667	1,664	1,664

[a] Each column and panel reports estimates of the enterprise zone effects from a separate regression. See notes to Table 6 for additional details. The fourth row (labeled "Effect of enterprise zone in redevelopment area/federal zone") reports the estimated sum of the coefficients in the first and third rows of each panel. When we expand the analysis to account for redevelopment areas, we have a total of 255 distinct enterprise zone-redevelopment area designations or expansions, and 78 designations or expansions of redevelopment areas in the enterprise zone control rings. Thus, we have 4,667 observations ({255 + 26 + 78} × 13) when the enterprise zone control rings are included. When we expand the analysis to account for federal zones, we have a total of 96 distinct enterprise zone-federal zone designations or expansions, and 6 designations or expansions in control rings, for a total of 1,664 observations ({96 + 26 + 6} × 13) when the control rings are included. Standard cluster-robust standard errors (clustering on enterprise zones) are in parentheses; ***, ** and * indicate that the estimated coefficient is significant at the 1-, 5- or 10-percent level based on these standard errors. Cluster-robust wild bootstrapped confidence intervals for the *t*-statistic were also computed for the enterprise zone variables, based on 1,000 replications; †††, †† and † indicate that the estimated coefficient is significant at the 1-, 5- or 10-percent level based on these standard errors. All estimates are weighted by 1992 employment levels or number of establishments in each subzone.

Table 9: Regression Estimates of Effects of Enterprise Zones on Share of Employment in Low-Wage Industries and in Manufacturing[a]

	Low-wage industries		Manufacturing	
	Including control rings	No control rings	Including control rings	No control rings
	(1)	(2)	(3)	(4)
A. Shift in growth rate				
Enterprise zone × linear trend	-0.0003	0.001	0.0002	0.004
	(0.002)	(0.003)	(0.001)	(0.001)**
B. Model A, different trends in zone and control areas				
Enterprise zone × linear trend	0.002	…	0.003	
	(0.003)		(0.002)	…
Ever in enterprise zone	-0.003	…	-0.003	
× linear trend	(0.004)		(0.002)	…
C. Shift in growth rate and level				
Enterprise zone × linear trend	-0.001	0.002	0.0002	0.007
	(0.002)	(0.005)	(0.001)	(0.002)***, †††
Enterprise zone	0.009	-0.005	0.0005	-0.021
	(0.016)	(0.027)	(0.012)	(0.014)
D. Model C, different trends in zone and control areas				
Enterprise zone × linear trend	0.002	…	0.005	…
	(0.003)		(0.002)**,†	
Enterprise zone	0.003	…	-0.011	…
	(0.018)		(0.013)	
Ever in enterprise zone	-0.002	…	-0.004	…
× linear trend	(0.005)		(0.002)**	
E. Shift in level				
Enterprise zone	0.005	0.001	0.002	0.001
	(0.014)	(0.018)	(0.010)	(0.011)
F. Model E, different trends in zone and control areas				
Enterprise zone	0.008	…	0.002	…
	(0.014)		(0.010)	
Ever in enterprise zone	-0.001	…	0.00001	…
× linear trend	(0.002)		(0.001)	
N	1,300	962	1,300	962

[a] Each column and panel reports estimates of the enterprise zone effects from a separate regression. See notes to Table 6 for additional details. The dependent variables are the shares of employment in low-wage industries or manufacturing. Standard cluster-robust standard errors (clustering on enterprise zones) are in parentheses; ***, ** and * indicate that the estimated coefficient is significant at the 1-, 5- or 10-percent level based on these standard errors. Cluster-robust wild bootstrapped confidence intervals for the *t*-statistic were also computed for the enterprise zone treatment variables ("Enterprise zone" and "Enterprise zone × linear trend"), based on 1,000 replications; †††, †† and † indicate that the estimated coefficient is significant at the 1-, 5- or 10-percent level based on these standard errors. All estimates are weighted by 1992 employment levels or number of establishments in each subzone.

Table 10: Regression Estimates of Effects of Enterprise Zones, Sensitivity Analysis[a]

	Including control rings		No control rings		Including control rings		No control rings	
	Empl.	Estab.'s	Empl.	Estab.'s	Empl.	Estab.'s	Empl.	Estab.'s
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Model A from Table 6				Model E from Table 6			
	Estimated coefficient of enterprise zone × linear trend				*Estimated coefficient of enterprise zone*			
1. Baseline (Table 6)	0.002	-0.003	-0.007	-0.008	-0.017	-0.022	-0.012	-0.042
	(0.011)	(0.002)[†]	(0.012)	(0.003)[***]	(0.047)	(0.024)	(0.035)	(0.021)[*]
2. 2,500-foot control ring	0.0002	-0.005	-0.0068	-0.008	-0.022	-0.027	-0.012	-0.042
	(0.006)	(0.002)[**, †††]	(0.012)	(0.003)[***]	(0.034)	(0.018)	(0.035)	(0.021)[*]
3. 1,000-foot control ring, include questionable streets	0.003	-0.003	-0.006	-0.008	-0.011	-0.022	-0.003	-0.043
	(0.011)	(0.002)[†]	(0.012)	(0.003)[***]	(0.050)	(0.024)	(0.040)	(0.021)[*]
4. 1,000-foot control ring, excluding 100-foot buffer on either side of boundary	0.006	-0.003	-0.007	-0.011	0.010	-0.024	0.008	-0.061
	(0.013)	(0.003)	(0.020)	(0.008)	(0.035)	(0.030)	(0.041)	(0.040)
5. No weighting	-0.003	-0.010	0.0004	-0.005	-0.033	-0.048	-0.047	-0.038
	(0.005)	(0.004)[**, †††]	(0.011)	(0.007)	(0.067)	(0.056)	(0.073)	(0.066)
6. Weighting by adjusting for number of subzones	-0.003	-0.010	0.009	-0.002	-0.008	-0.027	-0.017	-0.011
	(0.006)	(0.004)[**, †††]	(0.014)	(0.009)	(0.078)	(0.064)	(0.098)	(0.080)
7. Estimates dropping LA	0.013	-0.004	0.009	-0.007	0.046	-0.002	0.033	-0.028
	(0.007)[*, ††]	(0.003)	(0.008)	(0.007)	(0.018)[**, ††]	(0.032)	(0.040)	(0.044)
8. Estimates dropping LARZ	0.003	-0.002	-0.007	-0.008[**]	-0.015	-0.019	-0.009	-0.039[*]
	(0.011)	(0.002)	(0.011)	(0.003)	(0.047)	(0.023)	(0.034)	(0.021)
9. Including subzone-specific linear trends	-0.002	-0.001	-0.003	-0.002	0.012	0.029	0.018	0.015
	(.012)	(0.003)	(0.013)	(0.002)	(0.051)	(0.022)[†]	(0.077)	(0.020)
10. EZ initial designations or expansions	-0.002	-0.004	…	…	-0.019	-0.025	…	…
	(0.009)	(0.003)[†]			(0.054)	(0.027)		
EZ initial designations only	0.008	0.002	…	…	0.018	0.021	…	…
	(0.006)	(0.003)			(0.085)	(0.045)		
11. EZ status pre-1997	-0.004	-0.002	-0.009	-0.014	-0.026	-0.015	-0.007	-0.042
	(0.005)	(0.003)	(0.010)	(0.006)**	(0.027)	(0.027)	(0.026)	(0.023)*
EZ status 1997 and after	0.001	-0.003	-0.006	-0.006	-0.007	-0.029	-0.035	-0.040
	(0.009)	(0.002)[†]	(0.0130	(0.002)***	(0.086)	(0.023)	(0.088)	(0.014)***

[a] Each column and panel reports estimates of the enterprise zone effects from a separate regression. See notes to Table 6 for additional details. The sample sizes are as in Table 6, except that in Panel 7 the sample sizes fall to 1,092 and 767 (with and without control rings). The differences in the specification are explained in the panel headings. Standard cluster-robust standard errors (clustering on enterprise zones) are in parentheses; ***, ** and * indicate that the estimated coefficient is significant at the 1-, 5- or 10-percent level based on these standard errors. Cluster-robust wild bootstrapped confidence intervals for the *t*-statistic were also computed for the enterprise zone variables, based on 1,000 replications; †††, †† and † indicate that the estimated coefficient is significant at the 1-, 5- or 10-percent level based on these standard errors. All estimates are weighted by 1992 employment levels or number of establishments in each subzone.

Appendices: Details on Geographic Mapping

A. Geocoding the NETS

      Although the NETS contains the street address of each business establishment, to be able to use our GIS maps of enterprise zones (and other targeted areas) to identify whether establishments are inside or outside the zones, we need to geocode the exact locations of these business establishments.[55] "Geocoding" is the conversion of street addresses or other designators to latitude-longitude coordinates, which is common language that allows geographic information from different sources to be combined.

      To geocode the NETS establishments, we use the U.S. StreetMap Premium data, published by TeleAtlas. We matched NETS addresses with the street map using street names, street numbers, and zip codes, using ArcGIS. We performed this matching process twice, once at a high "spelling sensitivity" option of 80 and once at a low option of 40. The spelling sensitivity controls how much variation in spelling the software allows when it searches for likely match candidates; the higher the value, the more restricted the number of candidates. For each round of matching, the geocoding process returns a "match score" for each address on a 0-100 scale that reflects the confidence that the NETS address matched its correct analog in the street map file. With a high spelling sensitivity, more addresses fail to have a reasonable match (a match score above 60), but more have a very high match score. Thus, we used both rounds of matching; we chose the result with the higher match score and considered match scores below 60 to be a failed match and excluded them from subsequent analysis. With these procedures and a few other refinements that added a modest number of matches, of the 21 million establishment-year observations we attempted to geocode, 95.3% were successfully geocoded.[56] Among these, 96% had a

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[55] The NETS data set includes latitudes and longitudes, but not to a sufficient degree of precision to identify side of street, which, as explained in the main text, is important in determining whether businesses are inside or outside an enterprise zone. In addition, it does this only for the last observation on each establishment, and often to the centroid of the zip code rather than to the exact street address. Thus, we entirely redid the geocoding.

[56] We pursued a number of refinements. First, because some establishments have non-standard addresses, like retail centers or landmark names (rather than street addresses), we did a second round of geocoding with an address locator consisting of these named features instead of street names. We were able to geocode (or improve the match for) an additional 2,000 or so establishments this way. Second, we examined cases where establishments were not successfully geocoded for up to a maximum of four consecutive years but were successfully geocoded to the *same* address (i.e., they had not relocated) both before and after the year(s) they were not geocoded. We replaced the ungeocoded establishment-years with the latitude and longitude from the successfully geocoded years, on the

match score of 80 or higher. Establishments were less likely to be geocoded if they had over 1,000

employees, were in agriculture, utilities, or public administration, or were located in rural counties.

B. Street Selection in GIS

Mapping of the enterprise zones required automating the selection of an entire list of street ranges

using GIS. Simply geocoding the beginning and ending address for a street range does not select the

entire range of a street, because the range often covers many segments in ArcGIS. Instead, we had to

create a file of hypothetical addresses that finely divide up each range contained in the list, and then

geocode these created addresses using ArcMap. Selecting the segments associated with the geocoded

hypothetical addresses then selects the appropriate streets and ranges. Table B1 provides an example,

showing a small segment of the PDF file for the San Diego zones. The table shows, for example, that

3950 B Street to 4099 B Street are included in the enterprise zone.[57] If we geocode only the range end

points (3950 and 4099) we would not necessarily select the entire street between those endpoints, because

the U.S. StreetMap Premium database divides streets into segments of varying length. Thus, for example,

the endpoints might give us the segments 3950-3999 and 4050-4099, but miss 4000-4049. Selecting the

middle segment requires an additional address such as 4025.

In many cases, the original lists contained only street ranges and names, but excluded city and zip

code information. Without city and zip code information, geocoding can be less precise because there

may be two streets with the same name in two different cities, and the address locator used for geocoding

cannot distinguish between them. To mitigate this problem, we limited the streets that the address locator

uses to geocode by clipping a subset of streets that should contain the zone, such as the county within

which a city lies. Still, the geocoding can lead to faulty matches, and so we hand-checked the selected

streets against the original lists. When a street was falsely selected we removed it manually, and when

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assumption that the ungeocoded years were due to errors or misspellings rather than establishments moving from an identifiable location to an ungeocodable location and then back to the same identifiable location. This "filling-in" process geocoded an additional 15,000 or so observations. Finally, we manually geocoded a few hundred observations, primarily establishments in airports and military bases.

[57] We had to first convert PDF files listing street names, ranges, dates, and so on into DBF files using a PDF converter, in some cases doing additional manual editing or completely entering the data by hand. The original PDF lists varied in length from a few pages to around 100 pages.

streets were missed we created additional files of hypothetical addresses to correctly select the desired street.

C. Classifying Geocoded Business Locations on Zone Boundaries as Inside or Outside Enterprise Zones

Classifying geocoded business locations as inside or outside enterprise zones was complicated for streets on zone boundaries. To be able to distinguish between establishments inside and outside a zone when a street is on the zone boundary, we modify the geocoding to offset establishments 10 feet from the center of the street in one direction or the other, corresponding to odd and even addresses. We then select businesses by using ArcMap's automated function to select all points lying within a certain distance from the streets.

But because there is no automated function in ArcMap for selecting points along one side of a street and not the other (e.g., within 15 feet of a street in one direction but not another), we created polygons for streets that are enterprise zone boundaries, which include the correct side of the street but exclude the other side. Much like a cookie cutter, we use these polygons to cut away the selected points that we do not actually want in our selection of enterprise zone businesses.

To see the process using polygons to select establishments only on one side of a street where needed, Figure C1 provides a view of a small portion of the San Diego Barrio Logan zone, focusing on a location where all three expansions can be shown at once. The red lines and the red polygon correspond to the original 1987 San Diego zone. For a business to be selected as being in the 1987 zone, the point corresponding to the business would need to be selected as being along a red street, as well as lying within the red polygon boundary. Likewise, businesses that were part of the 1991 (1998) expansion would need to lie along the blue (purple) streets as well as be contained in the blue (purple) polygon. The green streets were not part of the zone in any year. At marker 1 in the figure, points to the left of the street would be excluded from the zone in 1991 (shown in blue) because, although they lie along the enterprise zone street, they are not within the polygon, while points to the right would be included. At marker 2, points on both sides of the street would be included in the zone in 1987 (shown in red) since they would both be selected as being along the 1987 enterprise zone streets and fall within the 1987 polygon. At

marker 3, points to the right of the leftmost street and to the left of the rightmost street will be selected. Lastly, marker 4 points to a slightly different issue – internal streets that are not part of the zone. At this marker, points along the green street will not be selected.

D. Mapping the Los Angeles Zone

The list of street ranges for Los Angeles is the largest of all of the zones, consisting of 103 pages of street ranges for four separate zones in the city of Los Angeles: Eastside, Central, Harbor, and Northeast. Since the Los Angeles area is quite large, ideally we would limit the streets to a subset of potential streets on which the hypothetical addresses could geocode, separately for each of the four zones. Initial attempts at proceeding in this way, creating separate clips by referencing maps for each of the four Los Angeles zones,[58] indicated that we could not treat the four parts of the Los Angeles as separate zones, but instead had to treat them as constituting one large zone. In particular, treating each zone separately resulted in relatively small numbers of points geocoding to the correct zone, and many coding incorrectly. In contrast, when we geocoded the four zones together, the geocoded points filled the general shape of the zones combined according to the maps we were referencing, yet the points were located randomly with respect to the zone name.[59] We determined that this was due to errors in the zone variable (that is, which zone the street range belonged to) and not errors in the address information. We also hand-checked the geocoded points to confirm the correct geocoding of addresses. Moreover, the selection of points by year of expansion was not random, but rather led to the selection of groups of streets as would be expected and occurred in the other zones.

Figure D1 illustrates how the general shape of the zones overall matched what we were seeing with the maps provided on the Los Angeles city website. The left panel contains polygons showing the general shape of each of the zones in Los Angeles, drawn from the maps on the Los Angeles website; the inset shows one of these maps for the Central zone. The right panel shows the initial geocoding when all four zones were geocoded as if they were one large zone, and it is apparent that the five zones are being traced

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[58] See http://www.lacity.org/Cdd/bus_statecred.html (viewed February 1, 2007).
[59] The fifth area, Mid-Alameda, had streets listed separately and therefore could be geocoded in isolation.

out by the selected streets.

The procedures we had to adopt for Los Angeles led to a couple of other complications. First, treating the four Los Angeles zones as if they were one large zone naturally resulted in more error in the initial geocoding process and made thorough hand-checking of streets infeasible. We therefore relied more heavily on polygons that we created to refine the selection of points. Second, the extensive overlap among areas covered by different years of expansion, coupled with a large number of expansions, led to many cases where streets intersect but only one cross-street is included in the zone in a particular year, resulting in more error in the initial selection of businesses along street ranges. When we did an initial check of the assignment of establishments to zone expansions, we found error rates much higher than for other zones – typically in the range of 5 to 10%. We found, however, that a large share of these errors was coming from intersections of zone streets, with streets were correctly selected as in the zone but in the wrong year due to neighboring zone streets from different expansions. To deal with this type of errant selection, we drew detailed polygons to avoid intersections, following directly along each street of each zone year and cutting at a 45-degree angle at intersections. These polygons are used to cut out those businesses that are selected as being along a street but do not fall within the specified polygon. We estimated that once the zone expansions are adjusted using these polygons, the remaining error rates for Los Angeles were in the 5-6% range.

Table B1: Example of Street Names, Ranges, and Dates for San Diego Enterprise Zones[a]

Street name	Direction	Lower address	Upper address	Sides of street	Date	Zone
Avenida del Mexico		1900	3099	Both	12/91	OM
Avenita Costa Brava		2400	2499	Both	12/91	OM
Averil Road		100	399	Both	12/91	OM
Aviator Road		8600	8899	Both	12/91	OM
B STREET		3950	4099	Both	11/87	BL
Balchen Way		1500	1599	Both	12/91	OM
BANCROFT STREET	NORTH	1	599	Both	11/87	BL
BANCROFT STREET	South	1	899	Both	11/87	BL
Bandolier Lane		2000	2099	Both	12/91	OM
Barsanti Court		1900	1999	Both	12/91	OM

[a] This is a sample of a small number of lines from the description of enterprise zones in San Diego. "OM" refers to Otay Mesa, and "BL" to Barrio Logan. Source: http://www.hcd.ca.gov/fa/cdbg/ez/enterprise/ (viewed November 1, 2006).

Figure C1: Illustration of Use of Polygons to Choose Sides of Streets
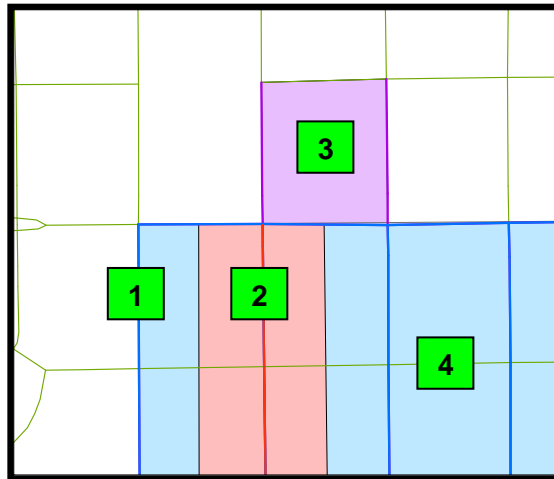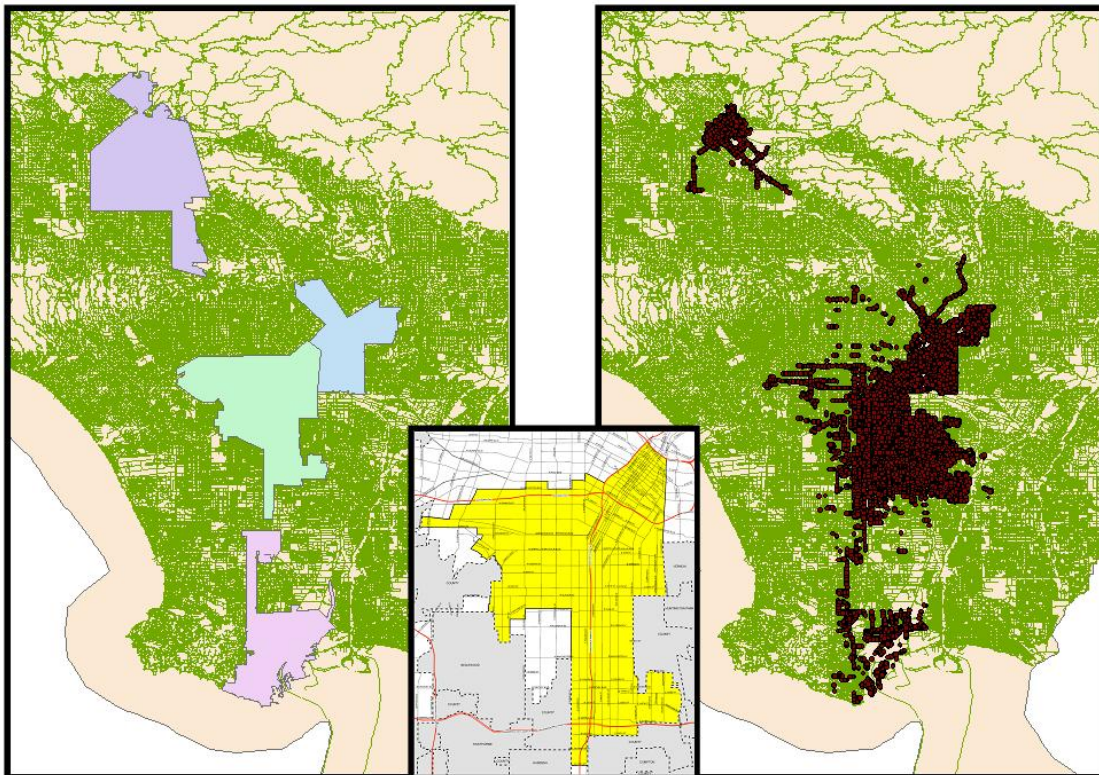

Figure D1: Polygons and Street Selections for Los Angeles[a]

[a] The polygons are drawn using the maps provided on the Los Angeles city website (http://www.lacity.org/Cdd/bus_statecred.html, viewed October 1, 2007), which are drawn for multiple years combined, some of which are more recent and therefore not in our data. Because of this, the shapes can differ some from the street selections.