Do California's Enterprise Zones Create Jobs?
Technical Appendix

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Description

In our accompanying report, we assess the degree to which California's enterprise zone program has met its most important goal: creating employment. We use a unique set of data and methods to measure employment in enterprise zones in each year from 1992 through 2004, construct appropriate control groups for comparison, and estimate the effect of the program on employment. Our main finding is that, on average, enterprise zones have no effect on business creation or job growth. However, our report also includes several findings and recommendations that may be useful in making enterprise zones more effective.

This technical appendix reviews the academic literature, details our mapping and econometric methodologies, and presents our regression results.

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Appendix A
Academic Research on Enterprise Zone Programs

Most existing research evaluating the effects of enterprise zones assesses their effects on jobs, businesses, or zone residents. Typically, these studies compare such outcomes as 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 differ across studies. Many studies fail to find any employment effects from 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 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 appendix, we instead highlight the limitations of the existing research on which we try to improve in the present study.

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 the boundaries of Census tracts, zip codes, or other standard geographic designations. As a result, studies have used aggregate data on zip codes (Dowall, 1996; Bondonio and Greenbaum, 2007) or Census tracts (O'Keefe, 2004), the boundaries of which correspond only approximately to those of enterprise zones. These approximations introduce measurement error, however, by incorrectly assigning areas (and the workers or businesses in them) as inside or outside enterprise zones (Papke, 1993). For example, Elvery (2009) 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, using 1990 Census data and tracts, he shows that 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 a bias toward finding that enterprise zones have no effect.

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. However, some studies 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 zones; 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 other Census tracts using propensity score matching based on residential and employment characteristics. However, propensity score matching does not account for the 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

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

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 area observations made 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 a quarter mile of the zone boundary and using the area outside the zones within a quarter 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 as empowerment zones; they also, in some cases, make comparisons with areas that became parts of zones after the analysis period. Boarnet and Bogart (1996) study a set of municipalities in New Jersey, all of which qualified to be enterprise zones. They compare those that received zone designation to all that qualified and, paralleling Busso and Kline, also compare those that received zone designation to those that qualified *and applied* for designation but did not receive it; however, this study suffers from poor delineation of enterprise zones by using entire municipalities.

The third challenge is to study outcomes that are appropriate, and appropriately measured, in light of the enterprise zone program's goals and design. It is essential to evaluate the effects of the program on the businesses and households that qualify for program incentives. In addition, if the program's tax incentives are geared more toward certain sectors (for example, manufacturing) or lower-wage industries, then evaluations should look at outcome variables, such as growth in particular industries, that reflect the program's hypothesized effects. For example, in California's program, businesses in an enterprise zone can claim hiring credits for employees living in a targeted employment area, 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 such household outcomes as employment or poverty, should be more apparent in TEAs. In contrast, when asking whether the state's enterprise program boosted employment, as measured by the *location* of jobs, it is appropriate to look at enterprise zones.

Finally, the fourth 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 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.

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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's geographic methods are the most similar to ours, with digitized maps of enterprise zone boundaries and geocoded establishment locations.

Appendix B
Mapping Enterprise Zone and Establishment Locations

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.[1] "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 StreetMap data 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 matches; 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 StreetMap 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 percent were successfully geocoded.[2] Among these, 96 percent had a 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 in rural counties.

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[1] The NETS dataset includes latitudes and longitudes but not to a sufficient degree of precision to identify side of street, which 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.

[2] We pursued a number of refinements. First, because some establishments have nonstandard addresses, such as 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 assumption that the ungeocoded years were due to errors or misspellings rather than to 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.

Mapping Enterprise Zones

Mapping establishments to enterprise zones requires GIS maps ("shapefiles") of the zones, and our identification strategy requires historical as well as current maps 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.[3]

Mapping 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 A.1 provides an example, showing a small segment of the PDF file for the San Diego zones. The table shows, for example, that the addresses from 3950 B Street to 4099 B Street are included in the enterprise zone.[4] 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 lengths. 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 and 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. The geocoding can still lead to faulty matches, so we hand-checked the selected streets against the original lists. When a street was falsely selected, we removed it manually, and when streets were missed, we created additional files of hypothetical addresses to correctly select the desired street.

Because the date that 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 A.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

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[3] These lists are used by the California 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 website, 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.

[4] We had to first convert Portable Document Format (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 differed in length from a few pages to around 100.

boundary by year would miss much detail. Some streets that were not included in the original zone 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. In a handful of cases, 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 after 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 longitudes and latitudes assigned to establishments correspond 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.

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 ten 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. We use these polygons much like a cookie cutter, to cut away the selected points that we do not actually want in our selection of enterprise zone businesses.

To show the process using polygons to select establishments only on one side of a street where needed, Figure A.2 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, whereas 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. Last, 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.

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 percent.

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: 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,[5] 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.[6] We determined that this was due to errors in the zone variable (that is, which zone the street range belonged to) and not to 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 as occurred in the other zones.

Figure A.3 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 zone 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 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 in which 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 percent. However, we found that a large share of these errors

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[5] See Los Angeles Community Development Department (undated).

[6] The fifth area, Mid-Alameda, had streets listed separately and therefore could be geocoded in isolation.

was coming from intersections of zone streets, with streets correctly selected as being in the zone but in the wrong year because different expansions changed neighboring zone streets. 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 that 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 percent range.

Mapping Redevelopment Areas and Federal Zones

Redevelopment areas are included in the analysis if they are 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 Reports (multiple years, 2000-2006) with information from Google Maps, to determine which redevelopment agencies within that enterprise zone's county were 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.[7] For some areas, 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.[8]

Information on the locations of federal designated zones comes from the U.S. Department of Housing and Urban Development.[9] These zones are listed in Table A.2. We added beginning

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[7] Drawing and editing polygons was done using 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 10 feet from the center of the street, were properly included 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 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.

[8] 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 obtained those only for its amended 1995 boundary. We use the 1995 boundary for all years of the analysis.

[9] Specifically, we use GIS boundary files that were available from HUD (2008). 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 (HUD, undated). This information still allows creating the

and ending dates for each area to the resulting polygon for each federal designated community. Some designated communities changed status during the period of analysis. However, because we treat these federal programs identically in terms of their potential economic effects, the beginning date assigned to each zone is the first year when they were designated federally.[10] As an example of the combination of all of the information on geographically targeted incentives, Figure A.4 displays the redevelopment areas, the federal zones, and the state enterprise zone streets for Santa Ana.

Table A.1
Example of street names, ranges, and dates for San Diego enterprise zones

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

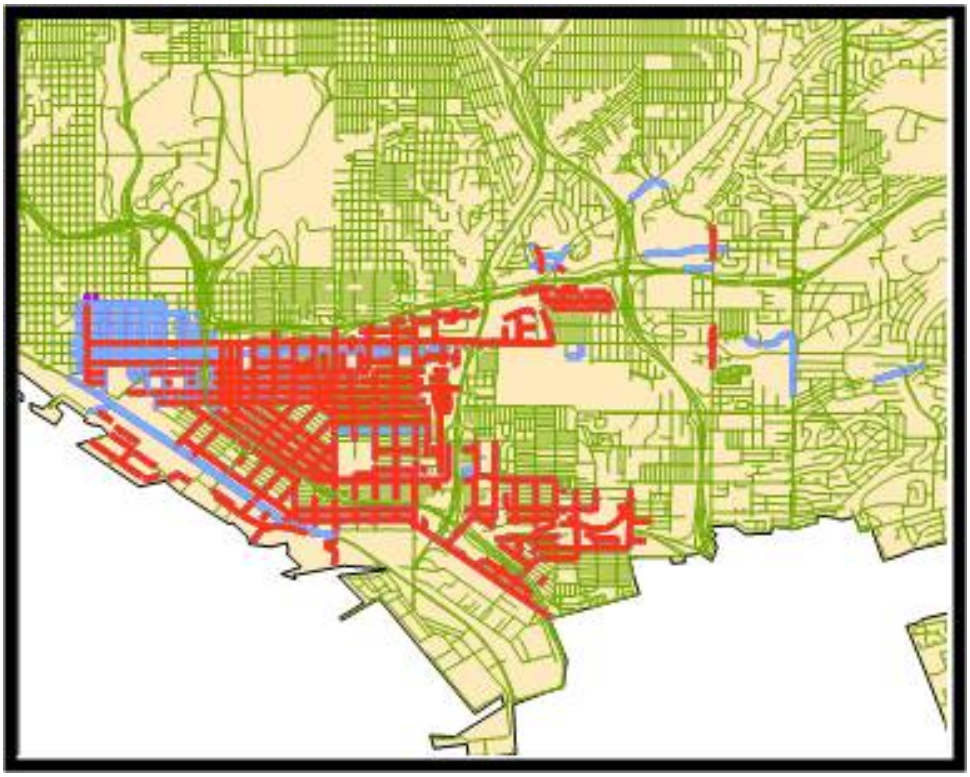
NOTES: This is a sample of a small number of lines from the description of enterprise zones in San Diego. "OM" refers to Otay Mesa, and "BL" to Barrio Logan.

SOURCE: California Department of Housing and Urban Development (undated-a).

RC/EZ/EC boundaries and incorporating them into GIS.

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

Figure A.1
Enterprise zone streets for San Diego (Barrio Logan), by year



LEGEND: Red streets: the original zone (1987). Light blue streets: first expansion (1991). Purple streets: final expansion (1998).

Figure A.2
Illustration of use of polygons to choose sides of streets

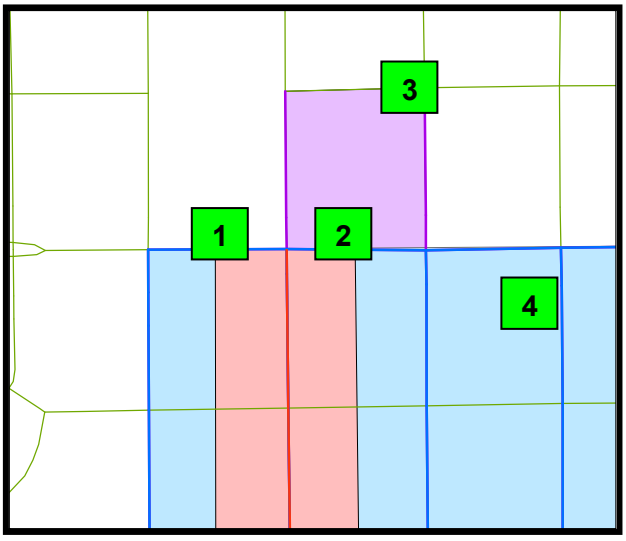
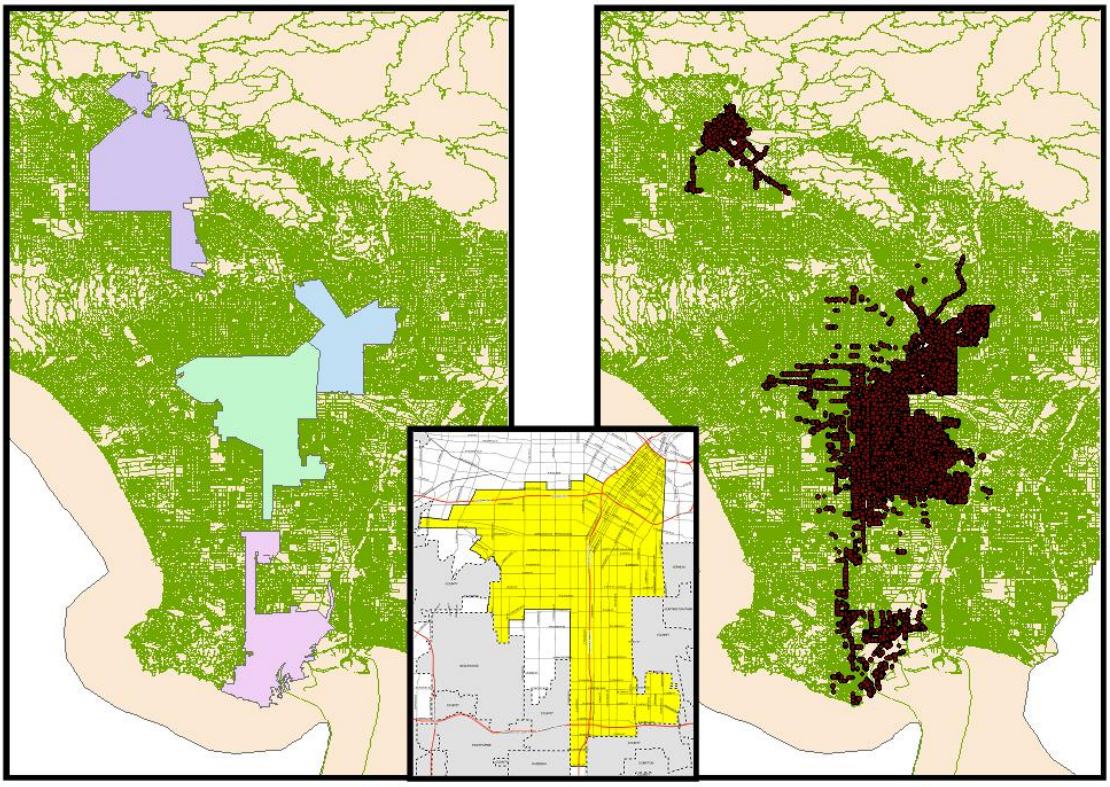


Figure A.3
Polygons and street selections for Los Angeles



NOTES: The polygons are drawn using the maps provided on the Los Angeles city website (Los Angeles Community Development Department, undated), 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 a bit from the street selections.

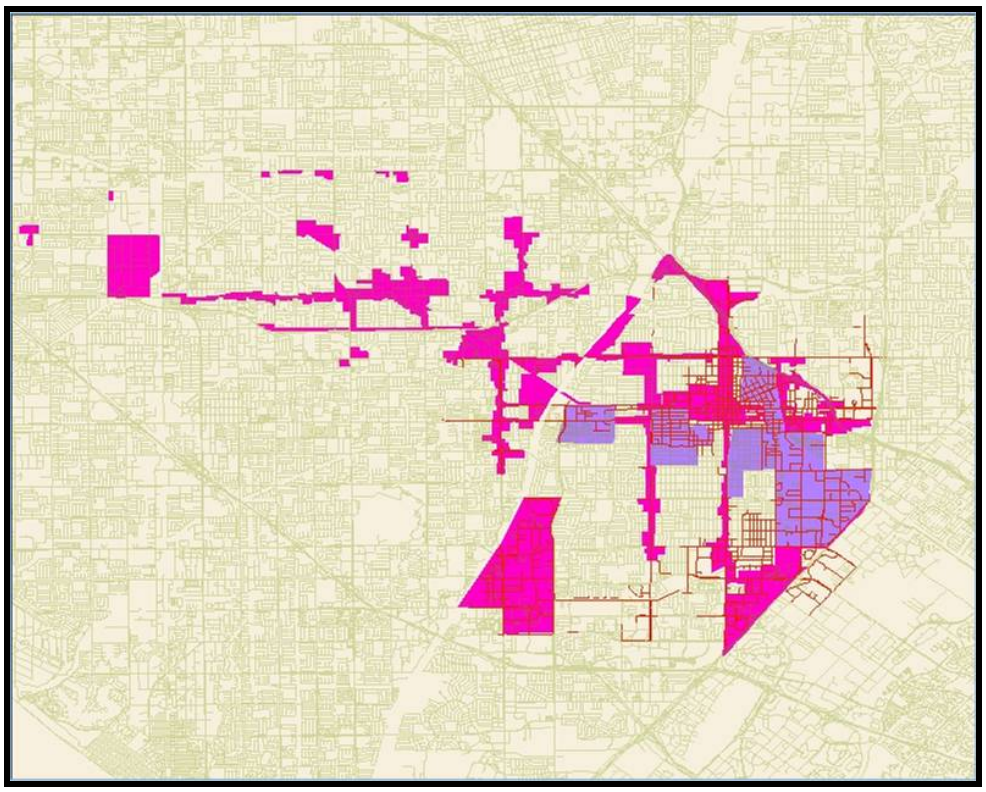
Table A.2
Federal designated communities in California

Designation date	Expiration date	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

SOURCE: U.S. General Accounting Office (2004).

NOTES: The shaded rows identify 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 show 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.

Figure A.4
Santa Ana redevelopment areas, federal zones, and state enterprise zone streets, as of 2004



LEGEND: Redevelopment areas: magenta. Federal zones: purple.
State enterprise zone streets: lines.

Appendix C
Statistical Methods

Introduction

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. This zone, then, 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 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 differ 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, using 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 nearby control area.[1] To illustrate, Figure A.5 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 area 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 used for this analysis were appropriate for enterprise zone designation.[2] However, we need to include the control rings to identify the effects of enterprise zones in some of the richer specifications we estimate.

Regression Specifications

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

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1 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.

2 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 effect of enterprise zones.

expansion. The 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 the 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:

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

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

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

[3]

The parameter of interest is β (as well as β' in equation (3)), which measures the effect of enterprise zones on the outcome Y. In equation (1), enterprise zone designation shifts the growth in Y; in equation (2), it shifts the level, and in equation (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,[4] and D_t captures aggregate changes. The term $D_j \ D_t$ allows for enterprise zone–specific changes over time in the outcome Y. Given that we identify the 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 before that year, much of our identifying information comes from expansions.[5] 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. 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, we 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 equation (1), for example, to be:

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[3] Note that the sum over k' begins with zero if the control ring is included and one if it is not.

[4] 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.

[5] 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.

$$Y_{jkt} = \alpha + \beta EZ_{jkt} \cdot t + \gamma EZever_{jk} + \sum_{j'=1}^{J} \sum_{k'=0or1}^{K_j} D_{j'k'} \lambda_{j'k'} + \sum_{t'=1}^{T} D_{t'} \theta_{t'} + \sum_{j'=1}^{J} \sum_{t'=1}^{T} \{D_{j'} \cdot D_{t'}\} \tau_{j't'} + \varepsilon_{jkt} \quad (4)$$

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 to 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 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.[6]

Finally, we have collected an array of information on enterprise zones, which we will code as a set of variables C_{jk}. Using the equations described above, we will develop a parsimonious model that we can then use to add interactions between the effects of enterprise zones and these characteristics *C*. These characteristics include local economic conditions and local economic development strategies—the latter from our survey of zone administrators. For illustrative purposes, equations (1) and (2) will be augmented as:

$$\begin{aligned} Y_{jkt} &= \alpha + \beta EZ_{jkt} + EZ_{jkt} \cdot (C_{jk} - \overline{C})\gamma \\ &+ \sum_{j'=1}^{J} \sum_{k'=0or1}^{K_j} D_{j'k'} \lambda_{j'k'} + \sum_{t'=1}^{T} D_{t'} \theta_{t'} + \sum_{j'=1}^{J} \sum_{t'=1}^{T} \{D_{j'} \cdot D_{t'}\} \tau_{j't'} + \varepsilon_{jkt} \end{aligned} \quad (5)$$

$$\begin{aligned} Y_{jkt} &= \alpha + \beta EZ_{jkt} \cdot t + EZ_{jkt} \cdot t \cdot (C_{jk} - \overline{C})\gamma \\ &+ \sum_{j'=1}^{J} \sum_{k'=0or1}^{K_j} D_{j'k'} \lambda_{j'k'} + \sum_{t'=1}^{T} D_{t'} \theta_{t'} + \sum_{j'=1}^{J} \sum_{t'=1}^{T} \{D_{j'} \cdot D_{t'}\} \tau_{j't'} + \varepsilon_{jkt}. \end{aligned} \quad (6)$$

In these models, the estimates of γ will identify characteristics of enterprise zones associated with stronger or weaker effects of zones on the various outcomes *Y*. Equation (5) shifts employment

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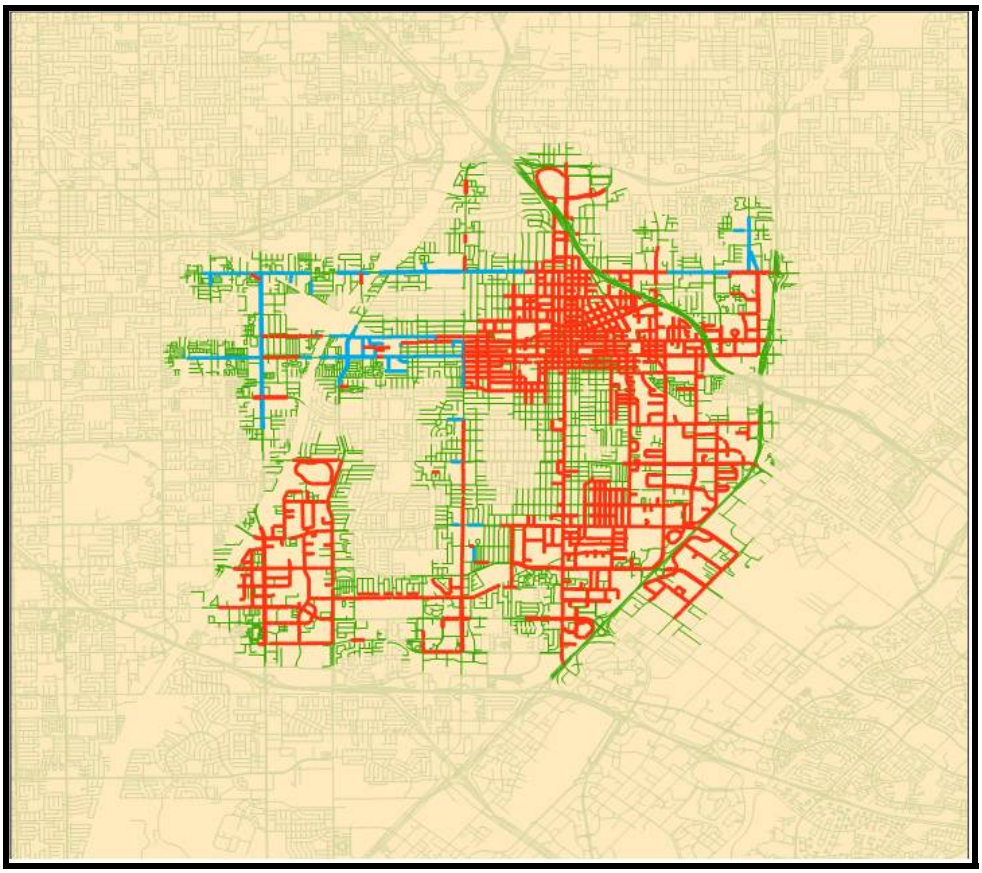
[6] However, as noted above, we do not have data on a large number of zones, so the usual asymptotics under which these standard errors are consistent, and confidence intervals therefore provide the correct coverage, may not apply. Cameron et al. (2008) have shown that using the wild bootstrap, modified to account for clustering, 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. In addition to the standard cluster-robust standard errors, therefore, we have also calculated these bootstrapped confidence intervals. As it turns out, some differences emerge but virtually never for the employment results. See Neumark and Kolko (2009) for details.

growth, and equation (6) shifts the level of employment. Given that we include subzone-specific dummy variables D_{jk}, and that the variables C that we use have only cross-sectional variation, the main effects of C do not appear in equations (5) and (6).

A potentially important limitation of this analysis is that we do not have as rigorous an approach for estimating the effects of program characteristics as we do for estimating the effects of enterprise programs more generally. In particular, because the information from zone administrators comes from a single point in time, and even more so because it comes from a period following the end of the sample period (rather than fairly early in the sample period), this information will not capture changes over time that may have occurred in zone activities. In addition, it is possible that the reported activities reflect responses to past economic developments in the zone. Thus, the inferences we draw from this analysis have to be viewed more cautiously than those regarding the overall effects of enterprise zones. Nonetheless, absent the availability of data on enterprise zone activities over time—and we are not aware of any such data, although they could in principle be collected—this is the best we can do.[7]

[7] And even if we had longitudinal data collected on enterprise zone activities, we would be skeptical about their reliability in tracking changes in what are, to a fairly large extent, subjective assessments of zone activities.

Figure A.5
Santa Ana enterprise zone



LEGEND: Red: initial 1993 designation. Blue: 1994 expansion. Green: 1,000-foot control ring.

Appendix D
Detailed Regression Results

Effects on Employment and Businesses

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 A.3. The dependent variable is the log of employment or the number of establishments.[1] The regression 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 the 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 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 in column 2.[2]

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 the positive effects of enterprise zones. Since this specification uses the difference between enterprise zone and control ring areas, we cannot estimate it excluding the control rings. As Panel B of Table A.3 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.

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[1] 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 a slight 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 instead had no effect on the estimates.

[2] And, as it turns out, based on the bootstrap results, the estimate in column (4) is not statistically significant.

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 shift in the growth rate of employment. The estimated coefficients of many leads and lags reveal 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 the 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).[3]

Figure 1 (in the main text) 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.[4] The figures—either 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 established in places doing either 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 1 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. However, an alternative possibility is that there is a 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 difficult 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

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[3] This is an "event study" form of the analysis; see Jacobson, Lalonde, and Sullivan (1993) for a nice exposition.

[4] 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.

rings is again not significant, judging by the bootstrap results.[5]

Accounting for Other Local Policies

We next turn to the analysis where we account for the overlap between state enterprise zones and redevelopment areas or federal zones. Table A.4 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 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 A.5, 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 come from the sum of these estimates plus the estimated interactions between the enterprise zone and either redevelopment areas or federal zones. As reported in the fourth row of each panel, these estimates are almost always negative; judging by the bootstrap results, these estimates are never statistically significant.[6] 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). Therefore, the main conclusion is that there is no evidence that enterprise zones have positive effects, whether or not they are combined with these other local policies.[7,8]

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[5] The specifications in Panels E and F might be more likely to detect short-run shifts in outcomes associated with enterprise zones, whereas the specifications in Panels A and B would more likely capture longer-run effects. The failure to find evidence of enterprise zone effects on employment in either type of specification is consistent with findings—reported in Figure 1 in the main text—that adding explicit lagged (or leading) enterprise zone variables similarly led to no evidence of employment effects.

[6] 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.

[7] 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 a part of enterprise zones or of 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.

[8] A few other state programs are 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). Below, we address potential problems from overlap between the LARZ and the Los Angeles

Effects on the Composition of Employment

The results to this point suggest that enterprise zones do not affect employment growth. However, 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 at 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. Using the NETS, it is straightforward 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 A.6, do not provide any indication that enterprise zones shift employment toward (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 judging by the bootstrap results. Thus, there is not a consistent indication that enterprise zones boost manufacturing employment.

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 A.7, row 1 reports the baseline estimates from Table A.3. 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 explicitly listed as belonging to them. This has virtually no effect 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 being 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. A somewhat

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and Long Beach zones.

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 toward zones with more expansions.[9] 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 A.7. This turns out to have little effect 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. Therefore, we report, in row 7 of Table A.7, results excluding Los Angeles.[10] 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 above, there is the potential for overlap between 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 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[11] and re-estimated our models. The results, reported in row 8 of Table A.7, 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.[12,]

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 for differential linear trends not only 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 are unchanged.

Finally, we ask whether the effects of initial enterprise zone designation differ from the effects

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9 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.

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

11 California Department of Housing and Community Development (undated-b).

12 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 percent of Los Angeles enterprise zone employment and 7.7 percent 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 A.4.

In yet another sensitivity analysis, although not reported in the table, we confirmed that there was no difference in the effects of enterprise zones beginning in 1997, when residents of TEAs—a potentially larger pool—became eligible for the hiring credit.

of subsequent zone expansions.[13] Since initial designation results from a different process than subsequent expansions, the effects could differ. The specification in row 10 of Table A.7 shows that the effect of initial designations is not significantly different from the overall enterprise zone effect.[14]

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 are very robust.[15] The estimates for the effects of enterprise zones on the number of establishments are perhaps less robust, with some indication that enterprise zones may reduce the number of establishments.

Variation in Program Effects across Zones

Table A.8 presents descriptive statistics on a number of zone characteristics that we incorporate in our regression models to study factors that might influence the effectiveness of enterprise zones. We present our regression results in Tables A.9 and A.10. As before, the dependent variables we use are the log of employment level and the rate of job growth. We add interaction terms between the enterprise zone variable and several zone-level characteristics. The estimated coefficients of these interaction terms capture differences in the effects of enterprise zones associated with these characteristics.

The zone-level characteristics we study include the composition of businesses in the zone, employment density, the education level in the county containing the zone, the year of zone designation, and local zone activities as reported in our survey of zone managers. For each dependent variable—employment level and rate of job growth—we report two models: first with only the zone-level characteristics related to employment, demographics, and year of designation, and then with the addition of the survey responses. As before, we report results with and without the 1,000-foot control ring around the zone's boundary in 2004.[16]

The regression estimates with the sets of interactions are presented in Tables A.9 and A.10. As with the baseline estimates, the dependent variable is the log of employment. Here, though, we look at how variation in enterprise zone characteristics influences the effect of enterprise zones on jobs. For each dependent variable—employment level and rate of job growth, in Tables A.9 and A.10, respectively—we report estimates for four models or samples and for each of these with and without the control groups. First, we include only the zone-level characteristics related to employment, demographics, and year of designation. Second, we instead include only the

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[13] 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 the effect in expansion areas.

[14] 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.

[15] Although not shown in the table, the estimates for the other specifications from Table A.3 were also robust to the variations shown in Table A.7.

[16] All results are reported for the mean values of the zone characteristics and survey responses.

information based on the survey responses. Third, we include both sets of interactions simultaneously. And, finally, because one of the survey responses is unavailable for Los Angeles, we report this same full specification for the full sample including Los Angeles, but dropping the missing variable ("Offering other tax incentives").[17]

In the employment-level model (Table A.9), several zone characteristics interact with the enterprise zone variable to yield statistically significant effects. Focusing first on the characteristics of areas where zones are established, the estimated coefficient of the interaction between the enterprise zone dummy variable and the share of zone employment in manufacturing (at the beginning of the sample period) is negative in all six specifications and statistically significant at the 1-percent or 5-percent level when the control rings are included. There is weaker evidence of a negative interaction with zone density; this is significant in two cases—when we include the control rings and when the interactions with the survey responses are also included. And in the last two specifications, there is statistically significant evidence, at the 1-percent or 5-percent level, that zones designated more recently have more positive employment effects.

Turning to the survey responses, there is consistent evidence that local zone marketing activity increases the job-creating effects of enterprise zones; the estimated interaction is significant at the 1-percent or 5-percent level with the control rings included and at the 10-percent level without the control rings, as long as the specifications also include the interactions with the other zone characteristics. Among other local activities, facilitating the earning of hiring tax credits appears to reduce the effect of enterprise zones on employment, although this estimate is significant (at the 5-percent or 10-percent level) only when the control rings are included. Finally, we also find a somewhat counterintuitive result that offering other tax incentives, credits, or discounts is associated with a weaker employment effect, significant at the 5-percent or 10-percent level in the specification without control rings.

In Table A.10, where we look at the effects of enterprise zones on employment growth, the evidence is generally a little bit weaker but qualitatively similar. In particular, we continue to find negative estimates of the interaction of the enterprise zone treatment variable and the baseline manufacturing share, although the estimate is statistically significant in only one case. There is, again, some weak evidence that zones designated in later years have more positive employment effects. Similarly, in every case, we again find a positive interaction with marketing activities, although the estimated interaction is statistically significant in only one case. We continue to find some evidence that when zone administrators concentrate on facilitating the earning of hiring tax credits, employment growth is lower; these results are significant at the 5-percent or 10-percent levels, although in the specifications including the control rings. And, finally, there is also some evidence of a negative interaction with offering other tax incentives.[18] Finally, we find some evidence that enterprise zones designated later

[17] As explained in the footnotes to the table, all of the interactions are with de-meaned variables, so that the main enterprise zone effect reported in the first row of the table is the effect evaluated at the sample means of the zone characteristics and survey responses.

[18] One other result to note is that, as reported in the first row of Table 6, the effects of enterprise zones at the sample means are significant and positive. However, this holds only for the specifications with control rings, in which we place less faith.

have more positive effects on employment growth. Although the estimated coefficient is significant (at the 10-percent level) in only one case, it is positive in all six specifications across table A.10).

Table A.3
Regression estimates of the effects of enterprise zones

	With control rings		Without control rings	
	Employment	**Establishments**	**Employment**	**Establishments**
	(1)	(2)	(3)	(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)		
No.	1,300	1,300	962	962

NOTES: 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 before taking logs. The models all include subzone and year dummy variables and zone-year interactions. There are 26 zones, with the number of initial zone designations and expansions summing 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. All estimates are weighted by 1992 employment levels (columns 1 and 3) or the number of establishments (columns 2 and 4) in each subzone.

Table A.4
Employment in enterprise zones, redevelopment areas, and federal designated zones

	% of zone employment in redevelopment areas	% of 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	...

NOTE: 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 A.5
Regression estimates of enterprise zones accounting for redevelopment areas or federal zones, including control rings

	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)***
No.	4,667	4,667	1,664	1,664

NOTES: Each column and panel reports estimates of the enterprise zone effects from a separate regression. See the 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 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 96 distinct enterprise zone-federal zone designations or expansions, and six designations or expansions in control rings, summing to 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. All estimates are weighted by 1992 employment levels or the number of establishments in each subzone.

Table A.6
Regression estimates of enterprise zone effects on the share of employment in low-wage industries and in manufacturing

	Low-wage industries		Manufacturing	
	With control rings	Without control rings	With control rings	Without 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)	
No.	1,300	962	1,300	962

NOTES: Each column and panel reports estimates of the enterprise zone effects from a separate regression. See the 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. All estimates are weighted by 1992 employment levels or the number of establishments in each subzone.

Table A.7
Regression estimates of the effects of enterprise zones, sensitivity analysis

	Including control rings		No control rings		Including control rings		No control rings	
	Empl. (1)	**Estabs.** (2)	**Empl.** (3)	**Estabs.** (4)	**Empl.** (5)	**Estabs.** (6)	**Empl.** (7)	**Estabs.** (8)
	Model A from Table A.6 *Estimated coefficient of enterprise zone × linear trend*				**Model E from Table A.6** *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, including 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 Los Angeles	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. Zone initial designations or expansions	–0.002	–0.004	…	…	–0.019	–0.025	…	…
	(0.009)	(0.003)			(0.054)	(0.027)		
Zone initial designations only	0.008	0.002	…	…	0.018	0.021	…	…
	(0.006)	(0.003)			(0.085)	(0.045)		

NOTE: Each column and panel reports estimates of the enterprise zone effects from a separate regression. See the notes to Table A.6 for additional details. The sample sizes are as in Table A.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. All estimates are weighted by 1992 employment levels or the number of establishments in each subzone.

Table A.8
Descriptive statistics on zone characteristics

	Units	Mean	Standard deviation
Share of employment in establishments with fewer than 50 employees, 1992	Percentage	50.6	11.2
Share of employment in manufacturing, 1992	Percentage	14.8	10.3
Area	Square miles	12.3	14.8
Employment density, 1992	Employment per square mile	4,684	5,639
Median household income, 1989, county	Dollars	31,441	7,139
Persons age 25 or over with a bachelor's degree or higher, 1990, county	Percentage	20.4	7.5
Year of designation	Year	1990	3
Average of six following questions (see note):	1–5 scale	2.9	0.6
Marketing (question a)	1–5 scale	4.0	0.9
Amending zoning (question b)	1–5 scale	1.7	1.1
Training workers (question c)	1–5 scale	2.8	1.3
Facilitating the earning of tax credits (question d)	1–5 scale	4.0	1.2
Encouraging the building of additional infrastructure (question e)	1–5 scale	2.3	1.5
Offering other tax incentives, credits, or discounts question f)	1–5 scale	2.8	1.6

SOURCES: Authors' computations based on NETS data, enterprise zone maps, U.S. Census data, and a survey of local zone administrators (described in the text).

NOTES: All rows report the total of the variable described in the column heading based on 26 enterprise zones, using zone boundaries as of 2004. Survey questions are based on a 1–5 scale: How active the zone is in doing each of the activities listed from a to f, where 1 is not at all active, 2 is not very active, 3 is somewhat active, 4 is very active, and 5 is extremely active. All figures reported in the table are for the full set of enterprise zones with the exception of the last item ("Offering other tax incentives"), for which we did not get a usable response for Los Angeles.

Table A.9
Regression estimates of the effects of enterprise zones on employment, interacted with zone characteristics: shift in level

	With control rings (1)	Without control rings (2)	With control rings (3)	Without control rings (4)	With control rings (5)	Without control rings (6)	With control rings (7)	Without control rings (8)
Enterprise zone	0.001	0.013	0.038	0.031	0.029	0.047	0.001	0.027
	(0.019)	(0.027)	(0.026)	(0.036)	(0.025)	(0.035)	(0.013)	(0.027)
Enterprise zone interacted with:								
Share of employment in establishments	–0.509	–0.983			–0.911	–1.093	–0.946	–1.370
with < 50 employees, 1992	(0.577)	(1.343)			(0.734)	(1.966)	(0.882)	(2.337)
Share of employment in manufacturing, 1992	–0.782	–0.712			–1.719	–1.275	–0.972	–0.804
	(0.327)**	(0.722)			(0.820)**	(1.126)	(0.310)***	(0.551)
Employment density, 1992 (000s)	0.001	–0.001			–0.014	–0.008	–0.009	–0.003
	(0.004)	(0.012)			(0.007)*	(0.013)	(0.003)***	(0.010)
Percentage of adults age 25 or over with a	–0.065	–0.218			0.656	0.052	0.348	–0.360
bachelor's degree, 1990, county	(0.739)	(1.296)			(0.392)	(0.650)	(0.210)	(0.719)
Zone designation year (no. of years after 1986)	0.006	0.006			0.038	0.030	0.022	0.022
	(0.009)	(0.014)			(0.023)	(0.034)	(0.006)***	(0.011)**
Marketing (from survey: 1–5 scale)			0.064	0.069	0.158	0.148	0.140	0.167
			(0.042)	(0.053)	(0.065)**	(0.086)*	(0.032)***	(0.051)***
Amending zoning (from survey: 1–5 scale)			0.003	0.035	–0.039	–0.026	–0.032	–0.049
			(0.034)	(0.045)	(0.027)	(0.039)	(0.013)**	(0.026)*
Training workers (from survey: 1–5 scale)			0.016	0.012	0.034	0.032	0.026	0.034
			(0.025)	(0.023)	(0.036)	(0.045)	(0.019)	(0.029)
Facilitating the earning of tax credits			–0.064	–0.025	–0.123	–0.061	–0.121	–0.074
(from survey: 1–5 scale)			(0.027)**	(0.044)	(0.064)*	(0.092)	(0.063)*	(0.090)
Encouraging the building of additional infrastructure			–0.011	–0.062	–0.055	–0.046	–0.030	0.002
(from survey: 1–5 scale)			(0.027)	(0.036)	(0.035)	(0.056)	(0.020)	(0.047)
Offering other tax incentives, credits or discounts			–0.014	–0.082	–0.025	–0.058		
(from survey: 1–5 scale)			(0.029)	(0.047)*	(0.023)	(0.027)**		
No.	1,300	962	1,092	767	1,092	767	1,300	962

NOTES: The dependent variable is in logs. These are estimates of specification (3) in the text. Note that the share variables are used in units ranging from zero to one, rather than zero to 100. Standard errors, clustered by zone, are in parentheses. ***, **, and * indicate significance at the 1-, 5-, or 10-percent level. All estimates are weighted by 1992 employment levels. In columns 7 and 8, the model is estimated including the Los Angeles zone, dropping the one survey variable with an unusable response.

Table A.10
Regression estimates of the effects of enterprise zones on employment, interacted with zone characteristics: shift in growth rate

	With control rings (1)	Without control rings (2)	With control rings (3)	Without control rings (4)	With control rings (5)	Without control rings (6)	With control rings (7)	Without control rings (8)
Enterprise zone × linear trend	0.004	0.001	0.011	0.012	0.011	0.023	0.004	0.010
	(0.002)*	(0.005)	(0.004)**	(0.016)	(0.002)***	(0.018)	(0.002)**	(0.016)
Enterprise zone × linear trend interacted with:	…	…	…	…	…	…	…	…
Share of employment in establishments	0.035	–0.069	…	…	–0.010	–0.363	0.005	–0.229
with < 50 employees, 1992	(0.063)	(0.151)	…	…	(0.054)	(0.458)	(0.057)	(0.352)
Share of employment in manufacturing, 1992	–0.106	–0.106	…	…	–0.065	–0.178	–0.101	–0.351
	(0.050)**	(0.121)	…	…	(0.054)	(0.343)	(0.060)	(0.301)
Employment density, 1992 (000s)	0.001	0.000	…	…	–0.001	–0.001	–0.001	–0.003
	(0.001)	(0.003)	…	…	(0.001)	(0.003)	(0.001)	(0.003)
Percentage of adults age 25 or over with a	0.024	–0.142	…	…	0.155	–0.194	0.121	–0.058
bachelor's degree,1990, county	(0.093)	(0.305)	…	…	(0.114)	(0.321)	(0.092)	(0.376)
Zone designation year (no. of years after 1986)	0.0001	0.004	…	…	0.001	0.002	0.002	0.005
	(0.001)	(0.002)*	…	…	(0.002)	(0.008)	(0.001)	(0.005)
Marketing (from survey: 1–5 scale)	…	…	0.007	0.014	0.007	0.024	0.010	0.041
	…	…	(0.006)	(0.023)	(0.007)	(0.031)	(0.005)*	(0.024)
Amending zoning (from survey: 1–5 scale)	…	…	0.001	0.008	0.002	–0.003	–0.001	–0.016
	…	…	(0.003)	(0.012)	(0.005)	(0.018)	(0.002)	(0.019)
Training workers (from survey: 1–5 scale)	…	…	–0.004	–0.002	–0.001	0.005	0.000	0.006
	…	…	(0.002)	(0.012)	(0.002)	(0.019)	(0.002)	(0.017)
Facilitating the earning of tax credits	…	…	–0.009	0.002	–0.014	–0.005	–0.013	–0.012
(from survey: 1–5 scale)	…	…	(0.005)**	(0.015)	(0.006)**	(0.021)	(0.007)*	(0.020)
Encouraging the building of additional infrastructure	…	…	–0.005	–0.015	–0.007	–0.017	–0.006	–0.010
(from survey: 1–5 scale)	…	…	(0.006)	(0.006)**	(0.005)	(0.013)	(0.003)*	(0.012)
Offering other tax incentives, credits, or discounts	…	…	0.001	–0.018	–0.002	–0.020	…	…
(from survey: 1–5 scale)	…	…	(0.004)	(0.010)*	(0.005)	(0.012)	…	…
No.	1,300	962	1,092	767	1,092	767	1,300	962

NOTE: See the notes to Table A.8.

Appendix E
Enterprise Zones for Which Information on Street Ranges and Dates Was Unavailable or Unreliable

The process used to create the enterprise zone maps by date relies on the information that the zone administrators have filed with the state delineating the streets within the zone by the address range and the date the range was incorporated into the zone. However, the state does not require that zone administrators submit a zone description in this manner. The empirical methods we use require information on the dates of incorporation. Date ranges were sometimes missing or ambiguous in the files listed on the website of the California Housing and Community Development, Division of Financial Assistance. In such cases, we contacted zone administrators directly to obtain the requisite information. In the majority of cases, zone administrators were able to clarify the information.

However, this information could not be obtained for seven zones, including Agua Mansa, Antelope Valley, Calexico, Fresno, Kings County, Pittsburg, Stockton, and Watsonville. For four of these—Fresno, Pittsburg, Stockton, and Watsonville—the zone administrator did not have documentation that attaches street names and ranges to dates. The Agua Mansa administrator could describe the final zone by street ranges and the boundaries for the second expansion but was unable to provide information on the original zone or the first expansion. The Antelope Valley administrator was able to describe the current zone by street range but could not distinguish street ranges that were part of the first expansion from those in the original zone. Finally, despite repeated requests, we could not obtain the required information from the Kings County administrator.

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