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Do *Some* Enterprise Zones Create Jobs?
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ABSTRACT

We study how the employment effects of enterprise zones vary with their location, implementation, and administration, based on evidence from California. We use new establishment-level data and geographic mapping methods, coupled with a survey of enterprise zone administrators. Overall, the evidence indicates that enterprise zones do not increase employment. However, the evidence also suggests that the enterprise zone program has a more favorable effect on employment in zones that have a lower share of manufacturing and in zones where managers report doing more marketing and outreach activities. On the other hand, devoting more effort to helping firms get hiring tax credits reduces or eliminates any positive employment effects, which may be attributable to idiosyncrasies of California's enterprise zone program during the period we study.

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1. Introduction

For over 20 years, national and state policymakers have targeted economic development efforts toward businesses in specific geographic areas. Often called “enterprise zones,” these programs have developed in the context of a long-standing debate on how best to combat poverty, unemployment, and other social ills, which tend to be geographically concentrated. “Place-based” policies, like urban redevelopment, target benefits at specific economically distressed neighborhoods or other geographic areas; “people-based” policies, such as the Earned Income Tax Credit, offer benefits to individuals based on individual criteria regardless of their location.¹

Enterprise zone programs are often a hybrid of place-based and people-based: targeting benefits to businesses located in a specified area is place-based, but many programs – including the California program we study in this paper – condition benefits on whether firms hire disadvantaged workers, which is people-based. Ladd (1994) refers to “using place-specific assistance to help the residents” as a “place-based people strategy.” Her review of enterprise zones and similar programs stresses their diversity: looking across programs at the federal level and in several states, some programs are more “pure place-based” while others are more hybrid. Accordingly, programs can vary in their effectiveness, and in a recent study of state and federal programs using a consistent methodology, Ham et al. (2009) find that some states’ programs reduce poverty or unemployment, and one (Ohio’s) raises employment, while other states’ programs do not have these effects. Enterprise zone programs vary in the level and nature of tax credits and other incentives, as well as in other forms of assistance available to zone businesses – some of which are difficult to quantify and evaluate. This heterogeneity across programs limits how much one can generalize from the study of a single program to enterprise zones as a category or, even more broadly, to place-based policies.

Likely reflecting this heterogeneity, the extensive research literature on the average employment effects of enterprise zones – where, by “average,” we mean across individual enterprise zones *within* a

¹ For arguments for and against place-based policies, see Glaeser (2005, 2007) and Crane and Manville (2008).

particular state or federal enterprise zone program – is not unanimous in the conclusions it reaches.² In general, though, the existing evidence does *not* find positive employment effects of enterprise zones. For example, although Lynch and Zax (2008) discuss a few studies that find positive employment effects (Papke, 1994; O’Keefe, 2004; Busso and Kline, 2007; and a limited amount of evidence in Billings, 2009), they describe these findings as “anomalous” (p. 5) relative to a much larger number of studies finding no employment effect.³

From a policy perspective, the absence of effects of enterprise zones, on average, is discouraging – at least for those who hope that geographically-targeted incentives such as enterprise zones can deliver benefits to economically-distressed areas. On the other hand, it is well-established that average treatment effects can mask important heterogeneity. If there is, in fact, variation in the effectiveness of enterprise zones, then there may be scope making enterprise zones more effective by replicating or encouraging the features of enterprise zone programs that are associated with increases in employment.

The goal of this paper is to provide an assessment of empirical evidence on sources of variation in the effectiveness of enterprise zones *within* California’s enterprise zone program. Specifically, we explore the associations between the job-creating effects of enterprise zones and (1) factors relating to the areas in which enterprise zones are established and (2) how enterprise zones are implemented – i.e., the activities that zone administrators engage in to try to achieve the program’s goals. We do this in the context of an empirical approach, based on new data sources and methods that we have developed, which is intended to provide rigorous evidence on the causal effects of enterprise zone programs, although meeting this standard is more difficult with respect to trying to explain what makes enterprise zones more (or less) effective.

² An earlier review is provided in Wilder and Rubin (1996). For a more recent reviews, see Elvery (2009), Landers (2006), and Lynch and Zax (2008). The literature is best viewed as estimating the average treatment effect on the treated (i.e., areas treated by designation as enterprise zones).

³ An earlier review by Wilder and Rubin (1996) is somewhat more positive in concluding that enterprise zones increased economic activity. However, much of the evidence in this earlier review is hardly definitive when it comes to establishing causal effects of enterprise zones – being based, for example, on perceptions of program participants (e.g., U.S. Department of Housing and Urban Development, 1986). See related criticisms in Boarnet (2001). In recent work (Neumark and Kolko, 2008), we re-examined evidence on the employment effects of enterprise zones, attempting to address many limitations of the existing research; our findings echo the conclusion that enterprise zones do not boost employment.

Our empirical analysis builds on previous research in which we estimate the average employment effect of California's enterprise zone program. Our approach to estimating this average effect used two data sources: detailed GIS maps we constructed of the precise boundaries of enterprise zones and their evolution over time; and the National Establishment Time-Series (NETS) database, which includes employment and location information on nearly all business establishments in California in the period 1992-2004.

The new dimension that we explore in this paper, however, is explaining *variation* in the employment effects of enterprise zones *within* California's program. To do this, we conducted interviews with local administrators of the enterprise zone program. In these interviews, we asked about the goals of the program, the activities of local zone administrators, the main challenges they face, and other questions. We use the responses to this survey, coupled with information from the NETS and other secondary data sources, to characterize differences across enterprise zones along numerous dimensions. We then estimate models of the effects of enterprise zones that allow the employment effects of zones to vary with these zone characteristics. These estimates allow us to assess how local zone activities as well as features of the areas in which zones are established influence the effect of the enterprise zone program on jobs. In light of the fairly overwhelming evidence that enterprise zones are ineffective, on average, it is important to assess whether there is evidence that some combinations of locational factors and variations in implementation or administration might increase (or perhaps decrease) the effectiveness of enterprise zones.

Focusing on within-program variation across enterprise zones means that, in effect, we hold constant the tax credits and other incentives that are uniform across California's enterprise zones and assess how local conditions and local program administration matter. Ladd's (1994) review suggests that "supply-side tax reductions" – which, in California's case, are uniform across individual zones – are ineffective, whereas "interventionist components" like technical assistance – which, in California's case, vary among zones – account for whatever success enterprise zone programs have (p. 202). Our approach of looking at within-program variation allows us to isolate the effect of interventionist components, as well as variation in local conditions.

To preview the results, we do find evidence of variation in program effectiveness among individual zones. Zones vary in their demographic and economic conditions. They also vary because local zone management is responsible for marketing and outreach, coordinating other incentives, and other economic development activities in the zones, and zone administrators make different choices about which of these activities they engage in and choose to emphasize. The evidence suggests that the enterprise zone program has a more favorable effect on employment in zones that have a lower share of manufacturing and in zones where managers report doing more marketing and outreach activities; as it turns out, this latter result has some parallels to findings from earlier literature on heterogeneity in the effects of enterprise zones (discussed below). On the other hand, somewhat surprisingly, a strong focus on helping firms pursue hiring credits made available by the enterprise zone program appears to run counter to job-creation efforts. These findings have potentially important policy implications for the targeting of areas to designate as enterprise zones, and for features of enterprise zone programs that policymakers and administrators encourage via both legislation and the selection of sites as enterprise zones. Moreover, the results suggest that the overall findings of the literature on enterprise zones may be too pessimistic, and that it may be possible to find ways to make enterprise zones more effective at creating jobs.

2. Relation to Previous Research

Although the recent research literature on enterprise zones has focused on average effects, earlier work provided suggestive evidence that there may be substantial heterogeneity in the effects of enterprise zones, both within and across state enterprise zone programs (e.g., Dowell, 1996; Elling and Sheldon, 1991; Erickson and Friedman, 1990). The earlier evidence on heterogeneity in the effects of enterprise zones is reviewed in Landers (2006) and Wilder and Rubin (1996). Those reviews conclude that there is substantial variation in the effects, and, among other conclusions, that enterprise zones were more effective when tax incentives were “complemented by more traditional supports for economic development (e.g., technical assistance, location/site analysis, special staffing)” (Wilder and Rubin, p. 478); similarly Elling and Sheldon conclude that administrative resources devoted to operating the enterprise zone, and services such as technical assistance, are associated with greater economic impact.

We are not sold on the empirical validity of this earlier research on heterogeneity in the effects of enterprise zones. First, this research is not based on comparisons with control groups, but instead only focuses on differences in outcomes across zones; without establishing that these differences exist relative to comparable control groups, however, there is no way to be sure that we are observing variation in the effectiveness of enterprise zones. Second, this research is not based on objective measures of outcomes. In the Elling and Sheldon analysis, the dependent variable on which they focus is responses of enterprise zone officials about the number of firms qualifying for zone benefits by investing in the zone; it is not obvious to us how this is objectively measured. Erickson and Friedman (1990) use a similar outcome, as well as the number of jobs created or saved per year as reported by local zone administrators; we have no idea how administrators would know these numbers, which require the type of estimation of causal effects of enterprise zones that poses such a challenge to researchers.⁴ Our strategy and our estimates address these two weaknesses of earlier research. First, they are based on comparisons of each enterprise zone to appropriate control groups. And second, they are based on objective measures of outcomes – specifically, measured employment at business establishments inside the zone (and in control areas).

3. California’s Enterprise Zone Program

California’s enterprise zone program has multiple goals: attracting jobs and businesses and raising employment is a primary goal, while others include reducing poverty and unemployment and raising incomes in target areas.⁵ The program seeks to accomplish these goals by providing a variety of tax incentives to businesses located in designated areas to encourage the hiring of economically-disadvantaged workers and to spur the creation of businesses. The largest incentive is state tax credits for hiring a “disadvantaged” employee. The state calculates the allowable hiring credit as a share of wages up to 150% of the minimum wage; the allowable credit is 50% of qualified wages in the first year, falling by 10

⁴ The Elling and Sheldon analysis also suffers from other problems. The regression models estimating the effects of zone characteristics on outcomes do not account for many characteristics of zones – such as their size.

⁵ See Assembly Jobs, Economic Development, and the Economy Committee (2006, p. 5). These multiple goals – job creation and improving residents’ circumstances – stem from the 1996 merger of two precursor programs that gave rise to the current enterprise zone program: the Enterprise Zone Act, which provided incentives to businesses located in specific areas (and which led to the creation of the original enterprise zones); and the Employment and Economic Incentive Act, which provided incentives to businesses that hired employees living in distressed residential areas.

percentage points each year until reaching zero after five years. Workers qualify as “disadvantaged” if they are unemployed for a sufficient duration or for certain other reasons – for example, if they have sufficiently low income, if they belong to one of several “eligibility groups” (veteran, enrolled in welfare-to-work, etc.). Most importantly, workers qualify if they live in a targeted employment area (TEA) – a Census tract with low median income.⁶ A worker living in a TEA qualifies for the hiring credit regardless of the worker’s characteristics, and many TEA residents – particularly in mixed-income neighborhoods – are not “disadvantaged.” However, given that disadvantaged workers are likely to earn low wages, the tax credit can result in a substantial reduction in the cost of hiring low-skill labor. For example, at an \$8 minimum wage (the current minimum in California), the credit would reduce the cost of a full-time worker earning \$12 per hour by \$12,000 in the first year, \$9,600 in the second year, etc.

The program offers four other incentives: (1) an income tax credit for sales or use taxes for machinery or parts for use within the zone; (2) a longer period (15 years versus 10 years) in which businesses can carry forward net operating losses into future years to reduce tax liabilities; (3) accelerated depreciation of depreciable property; and (4) a tax credit of 5% of qualified wages that low-income employees can claim, up to a maximum and subject to restrictions on work for the business in the zone and services performed within the zone. Each of these incentives is intended to reduce the tax burden or costs for businesses located in enterprise zones, which might be expected to spur the creation of new businesses or the expansion of existing ones. In addition, businesses in enterprise zones can sometimes receive preferential treatment on state contracts. Finally, financial lenders may deduct from their income net interest received from loans made to businesses in enterprise zones.⁷ In aggregate, these other incentives are small relative to the hiring credit. In 2005, the hiring credit accounted for roughly two-thirds of the program’s

⁶ It is estimated that between 80 and 90 percent of hiring credit vouchers use the TEA designation to qualify employees (Assembly Committee on Jobs, Economic Development and the Economy, 2009). TEA’s are defined by census tracts. TEA’s often include parts of an enterprise zone itself along with other lower-income neighborhoods, but they are defined independently of enterprise zones and do not necessarily overlap with them. Residents of TEA’s became eligible for the hiring tax credit beginning in 1997. Although this enlarged the pool of eligible workers, our analysis of the average effect of enterprise zones found no evidence that the effects of enterprise zones on job creation became any stronger after this policy change.

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

\$421 million cost; the sales and use tax credit accounted for another quarter.⁸ The relative importance of the hiring credit illustrates the program's goal of job creation and the appropriateness of employment growth as a measure of program success.

Localities apply to the Department of Housing and Community Development (HCD) to have a geographic area designated as an enterprise zone. Eligibility criteria include job-generating capacity as well as the level of economic distress measured along a number of dimensions. New zones are selected by HCD from the eligible areas based on these and other factors, including the local applicant's plan for bundling other local incentives, administering the program, and evaluating the outcome. In the 2006 application round, for instance, an area was eligible for consideration as an enterprise zone if it included a residential portion sufficiently "distressed" (as measured by income level, income growth, unemployment, and poverty) or petitioned for "distressed" status based on plant closures, gang violence, or other measures. The area also had to include an industrial or commercial area "contiguous or adjacent to" the distressed area. In addition, the application for enterprise zone status required the preparation of an economic development plan (including marketing, finance and administration of the plan, other local incentives, infrastructure development plans, and information management). The new application process weighted localities' economic development strategies more heavily than in the past and required localities to identify development objectives.⁹ This discussion, and in particular the recent emphasis on local strategies, highlights the fact that economic development administrators and policymakers may have a good deal of scope for decisions affecting the locations of enterprise zones and how they are implemented and administered – emphasizing why it is important to better understand the potential for these decisions to influence the effectiveness of enterprise zones.

⁸ See California Franchise Tax Board (2008). Additional information based on personal conversation with Allen Prohofsky, Franchise Tax Board, June 3, 2009.

⁹ In the 2006 round of zone designation, applicants were scored and ranked on their economic development plan; the bulk of the score was derived from HCD's assessments of the marketing strategy, plans for financing and administering the program, local incentives, infrastructure development plans, and information management. About one-quarter of the score was based on current conditions of the zone; this included the number of businesses, commercial and industrial vacancy rates, and available land as well as unemployment and income levels. HCD assigned an aggregate score as well as scores for individual components, all of which are publicly available. The application process is described in Department of Housing and Community Development (2006).

California's enterprise zone program is what Ladd (1994) calls a "place-based people strategy." Because the primary incentive – the hiring credit – is available to firms in distressed areas that hire disadvantaged workers, the program does attempt to target both people and places. As described above, not all workers that qualify for the hiring credit are "disadvantaged" since some middle- or upper-income workers live in TEA's. Furthermore, TEA's are not synonymous with enterprise zones, so the program is intended to help disadvantaged workers who might live outside the distressed area. Still "place-based people strategy" best characterizes the program. The program is also a mix of (Ladd's words again) "supply size tax reductions" and "more active governmental strategy." The recent 2006 reform, as described above, gave more responsibility for formulating economic development strategy and setting objectives to the local administrators, shifting the program's emphasis somewhat more toward the latter approach.¹⁰

As of the period covered by this paper, the enterprise zone program allowed for up to 42 zones in the state. Ten enterprise zones were created at the program's inception in 1986; since then, legislation has increased the number to 42. Zones are designated for an initial 15-year term, after which 5-year extensions can be granted. After the 15- or 20-year period, the enterprise zone expires, and a new application must be submitted. In practice, there is very little turnover in enterprise zones. All of the zones designated before 1990 were granted 5-year extensions when they reached the end of their original 15-year terms. Zones have expanded periodically, and in 1998 many enterprise zones were allowed a one-time expansion of their boundaries by up to 20% (which they could undertake later). In our survey of local enterprise zone administrators, we asked why zones expanded when and where they did. Two main reasons emerged. First, zones often expanded to benefit businesses that were moving to or growing in areas just outside the enterprise zone. Second, zones sometimes expanded to incorporate areas newly designated as commercial or industrial by the local planning process.¹¹

Table 1 lists the enterprise zones in the state, the years when they were initially designated, and the

¹⁰ This 2006 reform took place after the period of our study, 1992-2004. We cannot evaluate how this reform changed the program's effectiveness.

¹¹ To the extent that zones expanded where businesses planned to relocate or grow, zone expansions were sometimes the effect rather than the cause of employment growth; thus, our estimates of the average effect of the enterprise zone program on employment would be biased upward, strengthening our findings in earlier research of no positive employment effects of enterprise zones.

number of expansions (if any). The table also shows a handful of enterprise zones – mainly smaller ones – for which the information we needed to construct the maps for our analysis (discussed below) was either unavailable or inconsistent. Table 2 presents descriptive information on the enterprise zones we study. Column (1) reports employment in each enterprise zone in our sample as of 2004. The zones are sorted from highest to lowest employment levels. Columns (2)-(4) provide information on enterprise zone employment relative to county and statewide employment. The shares of enterprise zones in county employment vary a good deal across counties, varying from a high of 52.8% in Shasta Metro to a low of 0.7% in Altadena/Pasadena. Column (4) indicates that the large zones (Los Angeles, San Francisco, Santa Ana, and Oakland) each account, *on their own*, for 1% or more of total statewide employment.¹²

The 42 zones in California’s enterprise zone program include a wide variety of places and local economies. The zones range from dense urban centers to rural areas. Some are located in rich counties, while others are in counties that are among the state’s poorest. And the industry composition of zones differs. Columns (5)-(7) of Table 2 present descriptive statistics for a range of economic factors.¹³ As already noted, zones vary in size from employment under 1,000 (Merced) to over 200,000 (San Francisco and Los Angeles). These larger zones do not necessarily cover more land area, so the density of employment per square mile varies considerably. Zones covering downtown areas, like in San Francisco and Santa Ana, show densities of around 20,000 workers per square mile; at the other extreme, the Coachella Valley, Merced, Shafter, and Yuba/Sutter zones all have employment density of fewer than 1,000 workers per square mile.

The composition of employment varies, too. Manufacturing accounts for more than one-third of employment in the Madera, Porterville, and San Diego Ysidro/Otay Mesa zones but less than 10% of

¹² Overall employment statewide in the enterprise zones for which we have data is about 1.38 million, while overall employment in the counties in which the zones we study are located is 12.6 million, so that enterprise zone employment is about 11% of the total. Statewide employment in 2004, based on the NETS data, was 16.4 million, and employment in all counties with enterprise zones was about 14.2 million. Thus, if we assume that the share of county employment represented by enterprise zones is the same in the counties for which we do not have zone maps as for the counties for which we could construct these maps, then our enterprise zones represent 89% (12.6/14.2) of enterprise zone employment in the state.

¹³ These descriptive statistics are based on 1992 employment for the areas that are or will become part of an enterprise zone by 2004. Data come from the NETS. Ideally we would describe zone characteristics prior to zone designation, to characterize enterprise zones before they were affected by the policy. The best we can do on this score is to use 1992 data, the earliest year available in the NETS data we use.

employment in several zones. Nearly 90% of employees work in establishments with fewer than 50 employees in the Merced zone, though fewer than half of employees work in small establishments in many zones, especially larger zones.

Finally, the demographics of the labor markets that enterprise zones draw from may vary. We are unable to measure the demographic characteristics of the residents of zones, since the NETS does not include such data. But this may not be much of a limitation, because there is no reason to believe that enterprise zone employees are enterprise zone residents. Indeed, as we noted earlier, this is typically not the case, as the rationale for enterprise zones is their proximity to *other* areas of socioeconomic disadvantage; and employees can come from different areas as well. Looking at the education level of counties containing enterprise zones in column (8), the share of adults with college degrees in 1990 ranges from 35% in San Francisco county and 33% in Santa Clara county, where the San Jose zone is located, to 11.8% in Tulare, where the Lindsay and Porterville zones are located.¹⁴

4. Estimating the Effects of Enterprise Zones in California

We focus on the effects of enterprise zones on job growth. Job creation is an explicit goal of the program, and is also presumably a prerequisite for improving the economic circumstances of residents in targeted areas. In addition, in our survey of local zone administrators, nearly all respondents cited job or business creation when asked an open-ended question about the purpose of the enterprise zone program; far fewer cited improving residents' outcomes such as unemployment or poverty. In particular, we study changes in employment at businesses located in enterprise zones, relative to businesses located in appropriate control or comparison areas. Given the structure of the state's enterprise zone program, this is the correct metric for asking whether the state's enterprise program boosted employment. Businesses in an enterprise zone can claim hiring credits for employees living in TEA's (beginning in 1997), or meeting other eligibility criteria. In contrast, residence in the enterprise zone itself – which need not include the TEA – does not qualify a worker for the hiring credit. Hence, evaluating the program in terms of employment of zone residents would be inappropriate.

¹⁴ Demographic data for counties come from the 1990 U.S. Census.

Our approach addresses significant limitations of prior research estimating the effects of enterprise zones. First, a central 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 census tracts, zip codes, or other standard geographic designations. Instead, studies have used aggregate data on zip codes (e.g., Dowall, 1996; Bondonio and Greenbaum, 2007) or census tracts (e.g., O’Keefe, 2004) to approximate these boundaries. These methods, however, introduce measurement error by incorrectly assigning areas (and the workers or businesses in them) as inside or outside enterprise zones (Papke, 1993).¹⁵ In order to precisely identify zone boundaries, we digitally map California’s enterprise zones street-by-street rather than approximating with census tracts, zip codes, etc. Of course the precise geographic contours of enterprise zones that we create are only useful if we can map business establishments or employment into them. The NETS data are uniquely suited to this task, as they include exact street addresses that we have geocoded to precise geographic locations.

The second challenge concerns the selection of appropriate control groups, which ideally consist of areas economically similar to enterprise zones but lacking enterprise zone designation. Some studies have used broad control groups – such as the rest of the state – that may preclude meaningful comparisons with the enterprise zones (e.g., Peters and Fisher, 2002; Lynch and Zax, 2008). Others have used propensity score matching based on residential and employment characteristics (Greenberg and Engberg, 2004; O’Keefe, 2004; Elvery, 2009). However, propensity score matching may fail to account for unobservable sources of differences in job growth that may be the basis for assignment to zones. Also, many of these studies do not take advantage of before and after observations on enterprise zone and control areas.

We use two approaches to this problem. One is to consider a narrow buffer (or “control ring”) just outside the enterprise zone as a control group, as well as areas that are added to enterprise zones at different

¹⁵ For example, Elvery (2008) notes that for the two states he studies, if enterprise zones are defined as the areas encompassing all zip codes that overlap with enterprise zones, then the resulting enterprise zone definitions are six times larger than the actual zones. Similarly, he shows that, based on 1990 Census data and tracts, less than one-half of the population residing in census tracts that include enterprise zones actually live in enterprise zones.

times, taking advantage of the ability of California's enterprise zones to expand numerous times.¹⁶ The second is to exclude the control rings, and to focus exclusively on the areas added to the enterprise zone at different times. In our view, this latter approach provides the most reliable estimates, because it has been demonstrated through the policy process that the areas in the control groups used in this approach were appropriate for enterprise zone designation. In addition, we estimate heavily-saturated regression models to account for remaining possible differences between treatment and control areas.¹⁷

4.1. Data and Geographic Methods

We use the National Establishment Time-Series (NETS) database and GIS software to address many of the difficulties and complications that arise in delineating the boundaries of areas affected by enterprise zone incentives, and measuring the effects of these incentives on affected businesses. The NETS is a national, longitudinal file of the universe of business establishments, created by Walls & Associates using establishment-level data from Dun & Bradstreet. Our extract of the NETS covers all of California over the period 1992-2004. The NETS provides exact street addresses for establishments in every year, allowing us to identify location precisely rather than having to aggregate to the tract or zip code level, once the enterprise zones are mapped.¹⁸

Preparing the data for analysis involved two processes: digitizing enterprise zone maps, and geocoding establishments in the NETS so that they can be mapped.¹⁹ The geocoding is fairly standard. The mapping is more complex, however. Mapping establishments to enterprise zones requires GIS maps ("shapefiles") of the zones, and our identification strategy also requires historical 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

¹⁶ The use of a narrow control ring has some parallels to the regression discontinuity design that Billings (2009) uses to study Colorado enterprise zones. The use of areas added at different times has parallels with some of the analyses of federal zones in Busso and Kline (2007).

¹⁷ Our earlier study (Neumark and Kolko, 2008) also looked at the problem of other geographically-targeted policies that covered areas overlapping with enterprise zones. The results were not sensitive to accounting for these other policies, so we do not emphasize them here.

¹⁸ Neumark et al. (2007) conducted a detailed investigation of the quality of the NETS data along numerous dimensions.

¹⁹ See Neumark and Kolko (2008) for a more-detailed discussion of the mapping and geocoding – a labor-intensive process that occupied numerous research assistants over a two-year period.

were included in the zone; these lists are provided by local zone administrators to HCD.²⁰ Because the date each address range was added to the zone is contained in the underlying data for each hypothetical address, we can select street ranges for the year in which the street range would have entered the zone.

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. Geocoded longitude and latitude that are assigned to establishments correspond to the center of the street on which they are located, so 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.

Overall, our approach to determining whether businesses are in or out of a zone in each year was successful. We checked the error rate by comparing the final variable created for the enterprise data indicating zone status in various years against the original zone ranges from the street address lists for San Diego (a city zone) and Yuba Sutter (a rural zone), for random samples of observations, finding both to have error rates of less than 1%. However, our approach was more problematic for the zones in Los Angeles, for which the mapping of enterprise zones was much more complicated because of the large numbers of street ranges and the five separate zones in the city. Because of these complications and problems with the coding of the street ranges, we treat the separate zones in Los Angeles as one zone. For this zone we end up with a classification error rate in the 5-6% range – higher than for other enterprise zones because of the unavoidable complexity of the Los Angeles area.

The enterprise zone maps are also used to create the control rings discussed above. In particular, based on the GIS maps of enterprise zones as of the last year of the sample, we define these control rings as areas of a fixed, relatively small distance from the outer boundary of an enterprise zone – 1,000 feet – on the presumption that economic conditions in these narrow rings, aside from the effects of the enterprise

²⁰ Available at <http://www.hcd.ca.gov/fa/cdbg/ez/enterprise/> (viewed November 1, 2006). These lists are used by California's Franchise Tax Board to determine whether establishments qualify for benefits. In some cases date ranges were missing or ambiguous in the files listed on the HCD webpage, in which case we contacted zone administrators directly to obtain the requisite information. In most cases zone administrators were able to provide us with clarifying information.

zone, are likely to be very similar in the treated area that became an enterprise zone and the surrounding, nearby control area.

4.2. Statistical Approach

Our statistical analysis is based on observations on what we call “subzones.” For any enterprise zone, there is a set of subzones consisting of the original zone plus each expansion. An observation, then, is a subzone-year pair. 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. To illustrate, Figures 1A and 1B show maps for the Santa Ana enterprise zone. Figure 1A displays the city streets including and surrounding the zone, as well as the zone streets as of 2004; Figure 1B displays the initially designated streets, the expansion streets, and the 1,000-foot control ring.

We estimate models both including and excluding the control rings. When we exclude the control rings – generating our preferred estimates – the effects of enterprise zones are identified by comparing what happens when a subzone of a zone is designated to changes in subzones that were designated earlier or will be designated later. Thus, for example, the blue streets in Figure 1 would serve as the control group for the red streets in the original enterprise zone, and in later data the red streets would serve as the control group for the blue streets.²¹

Of course as discussed above, the validity of our empirical strategy depends on the validity of the comparison areas; that is, they have to provide counterfactual estimates of what would have happened in the treatment areas had enterprise designation not occurred. Although this is inherently untestable, a look at some of the data on treatment and comparison areas is informative, and is given in Table 3. The table reports some figures for the sample as a whole (i.e., covering the enterprise zones and the control rings), as well as the enterprise zone separately – including the originally-designated areas as well as the expansions separately – and the 1,000-foot control ring. These are reported for 1992, the first year of the sample.²²

²¹ That is, the expansion areas are, themselves, treatment areas in the year they were added to the enterprise zone; the control group when an expansion area is added consists of the initial designation area, any other expansion areas added at different times.

²² Ideally, we would like pre-treatment comparisons. However, many of the areas in the original zone designations were so designated before 1992, and there is no pre-treatment year for the control rings.

As indicated in the first row, enterprise zone employment constitutes about 69% of total employment in the zones and the control rings, and of this, about 72% is in the areas originally designated as part of zones. Clearly there is plenty of employment (and also plenty of establishments, as shown in the second row) in the control ring and, of course, in the expansion areas. Perhaps even more informative is the third row, which reports employment density. Although density is higher in the areas designated as enterprise zones than the control rings, density is still quite high in the latter. Moreover, density is actually higher in the expansion areas than in the initially-designated areas. The last three rows report some information on establishment size and composition. Establishment size in the zones and the control rings is quite similar (around 14.5), although slightly higher in the enterprise zone expansion areas than in the originally-designated areas. The same is true of the share of employees in low-wage industries.²³ The share in manufacturing is somewhat higher in the zone expansion areas, and somewhat lower in the control rings. It is certainly the case that the three types of areas are not identical in terms of these measures; but it would be quite surprising if they were. Nonetheless, there is extensive employment in the expansion areas and the control rings, and the types of establishments do not appear inordinately different across them. In the regression analysis described in the next sub-section, we take further steps to account for differences between the treatment and comparison areas.

4.3. Regression Models

The regression models we estimate require some notation. We index the geographic locations corresponding to each enterprise zone indexed by $j = 1, \dots, J$, which include the zone itself and can include the control ring. We have observations over time, indexed by $t = 1, \dots, T$. We define subzones within j , indexed by k , with $k = 0, \dots, K_j$; $k = 0$ for the part of j that is never in a zone (the control ring), and $k = 1, \dots, K_j$ for the parts that become a zone initially and with each expansion. The dependent variable Y_{jkt} is log of the number of jobs in a subzone.²⁴ We denote by EZ_{jkt} a dummy variable for whether subzone k in area j

²³ We ranked industries by average pay based on 2004 data from the Quarterly Census of Employment and Wages, dividing NAICS industry subsectors into three groups, each containing approximately one-third of the workforce.

²⁴ In the handful of cases where employment was zero (26 observations), we substituted one for zero before taking logs. This can be viewed as perhaps introducing the slightest measurement error, or presuming that the data are not sufficiently accurate to distinguish between zero and one job or establishment in a cell. Regardless, we verified that

is in an enterprise zone in year t . So for the part of area of j that is never in the zone, $EZ_{jkt} = 0$ for all t ; in a subzone 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 .

To estimate the effects of enterprise zones when heterogeneity in effects is ignored – the focus of our earlier work (Neumark and Kolko, 2008) – we estimate the models:

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

$$(2) \quad Y_{jkt} = \alpha + \beta EZ_{jkt} \cdot t + \sum_{j'=1}^J \left\{ \sum_{k'=0 \text{ or } 1}^{K_j} D_{j'k'} \lambda_{j'k'} \right\} + \sum_{t'=1}^T D_{t'} \theta_{t'} + \sum_{j'=2}^J \sum_{t'=1}^T \{D_{j'} \cdot D_{t'}\} \tau_{j't'} + \varepsilon_{jkt}.^{25}$$

In model (1), enterprise zone designation shifts the level of employment, and in model (2) it shifts the growth rate. We interpret specification (2) as testing whether the lowering of costs associated with enterprise zone designation implies that enterprise zones get a larger share of businesses and jobs stemming from the steady upward growth in population and output. Alternatively, there may be 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 variable; this is captured in specification (1).²⁶

The variables D_{jk} , D_t , and D_j are fixed effects for each subzone, year, and enterprise zone, respectively. These are added to the models in a highly flexible way so as to control for differences between each of the subzones in a zone (as well as the control ring), and differences in the economic shocks that affect each zone (treated as common across the subzones of a zone). In particular, the dummy variables D_{jk} capture fixed differences across subzones. These control for any differences in characteristics of each subzone and control ring, such as education levels, industry mix, infrastructure, size, etc., that are time-invariant. By construction, of course, these also control for any overall time-invariant differences between

simply dropping these cases had no impact on the estimates.

²⁵ The sum over k' begins with zero if the control ring is included, and one if it is not.

²⁶ Because the data begin in 1992, whereas most zones were originally designated prior to that year, much of our identifying information comes from expansions. 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 have estimated versions of equations (1) and (2) that separately identify the effects of initial zone designations and expansions, and find no evidence of differences. We also estimated model with long lags, for which the effects of initial designations are identified, and found no differences relative to models without lags.

areas initially designated as zones, zone expansion areas, and control rings. The D_t capture aggregate changes, to account for the possibility that enterprise zones tended to be established in periods of either particularly high or low employment growth across all of the regions included in our sample, owing to factors such as the business cycle. Finally, the term $D_j \cdot D_t$ allows for enterprise zone-specific changes over time in employment, 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).²⁷

To assess how enterprise zone program effects vary across zones – the focus of this paper – we augment the regression models with measures of local zone conditions and activities. Denoting by C_{jk} a vector of enterprise zone characteristics,²⁸ our models become:

$$(3) \quad Y_{jkt} = \alpha + \beta EZ_{jkt} + EZ_{jkt} \cdot (C_{jk} - \bar{C})\gamma + \sum_{j'=1}^J \left\{ \sum_{k'=0 \text{ or } 1}^{K_j} D_{j'k'} \lambda_{j'k'} \right\} + \sum_{t'=1}^T D_{t'} \theta_{t'} + \sum_{j'=2}^J \sum_{t'=1}^T \{D_{j'} \cdot D_{t'}\} \tau_{j't'} + \varepsilon_{jkt}$$

$$(4) \quad Y_{jkt} = \alpha + \beta EZ_{jkt} \cdot t + EZ_{jkt} \cdot t \cdot (C_{jk} - \bar{C})\gamma + \sum_{j'=1}^J \left\{ \sum_{k'=0 \text{ or } 1}^{K_j} D_{j'k'} \lambda_{j'k'} \right\} + \sum_{t'=1}^T D_{t'} \theta_{t'} + \sum_{j'=2}^J \sum_{t'=1}^T \{D_{j'} \cdot D_{t'}\} \tau_{j't'} + \varepsilon_{jkt}$$

The estimates of the vector of coefficients γ capture variation in the effects of enterprise zones – on the level of employment specification (3), and on the rate of job growth in specification (4) – associated with variation in enterprise zone characteristics.²⁹ Note that for specification (3), the main effects of the zone characteristics are subsumed in the subzone-specific dummy variables D_{jk} , given that the variables in C have only cross-sectional variation. For specification (4), the main effects would be interactions of C and the time trend t , which are subsumed in the enterprise zone-specific year effects $D_j \cdot D_t$.³⁰

²⁷ We can allow arbitrary changes over time for each enterprise zone j and still identify β , because we identify effects off of subzone-level variation.

²⁸ Note that our measures of variation in zone conditions and activities come from a survey conducted at a point in time, so there is no time variation in these characteristics (and hence no t subscript on C).

²⁹ Note that for each characteristic C , the interacted variable has the weighted mean subtracted off, so the estimated coefficient of the enterprise zone dummy measures the effect at the weighted sample means of the variables in C .

³⁰ There is a potentially subtle complication if we think of the zone characteristics from our survey as not applying to the control rings. In this case, it is still true that for the specifications excluding the control rings, and for all of the specifications focusing only on the zone characteristics based on the NETS or other data sources (employment density,

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.³¹ Estimates are always weighted by 1992 employment in the subzone.

A potentially important limitation of this analysis is that we do not have as rigorous an approach for estimating effects of program characteristics as we do for estimating the average effects of enterprise programs. In particular, as explained in the next section, the information from zone administrators comes from a single point in time, following the end of the sample period. As a consequence, 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 average 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.

4.4. Results on Average Effects

Before turning in detail to our analysis of variation in the effectiveness of enterprise zones in California, we provide a brief summary of our earlier findings on their average effects. Representative estimates of specifications (1) and (2) are reported in Table 4. The control rings are included in column (1) but excluded in column (2). In Panel A, which corresponds to specification (1) above, the key independent variable is the dummy variable for enterprise zones, so that enterprise zone designation shifts the level of employment. The estimated coefficient of this variable is negative, rather than positive, suggesting that, if anything, enterprise zones decrease employment slightly (by about 1.5 percent); but in both columns the

industry mix, etc.) – which apply to the control rings as well – the zone-specific year effects $D_j D_t$ subsume the main effects.

³¹ Because we do not have data on a large number of zones, 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 their Monte Carlo simulations, when the number of groups is in the 20's, confidence intervals based on the standard cluster-robust standard errors are fairly accurate; but this result need not carry over to our particular specification. In addition to the standard cluster-robust standard errors, therefore, we have also calculated these bootstrapped confidence intervals. As reported in our earlier paper, for the employment regressions for the average effects the statistical conclusions were nearly always the same.

estimated effect is statistically insignificant. Panel B, instead, models the effect of enterprise zones as shifting the growth rate of jobs. The estimates provide no evidence that enterprise zones boost the rate of job growth. The estimates are small and statistically insignificant, and in the preferred specification in column (2) the estimate is negative rather than positive.

We have subjected these specifications to an extensive battery of sensitivity analyses, and the finding of no employment effect is very robust (Neumark and Kolko, 2008).³² Among the sensitivity analyses were: accounting for the effects of other geographically-targeted policies; specifying the models to include large numbers of leads and lags of the enterprise zone variable; using 2,500-foot instead of 1,000-foot control rings; including in the zone streets that were difficult to classify and hence left out of the main analysis; excluding streets within a 100-foot buffer on either side of the enterprise zone boundary, to reduce classification errors; not weighting the estimates; including separate linear time trends for each subzone; dropping Los Angeles, which has a strong influence on the estimates by virtue of its size;³³ allowing differential effects of enterprise zones beginning in 1997, when residents of TEA's – a potentially larger pool – became eligible for the hiring credit; and estimating separate effects for the areas included in initial enterprise zone designations and areas brought in via expansions.

5. Evidence on Variation in the Effectiveness of Enterprise Zones

Although the tax and other incentives offered by the state are uniform across enterprise zones, local zone administrators oversee marketing strategies for making businesses aware of the tax benefits and for coordinating complementary local incentives.³⁴ To characterize the variation in local economic development strategies and local resources devoted to the program, we conducted a survey in 2007 of enterprise zone administrators. The effects of enterprise zones could also vary owing to their employment density, their industry mix, local demographics, and so on. Credits on sales tax for machinery purchases, for

³² In that paper, we also discuss possible reasons why enterprise zones may fail to create jobs.

³³ For the specifications including the control rings (only), we find a significant positive effect of enterprise zones on employment when we exclude Los Angeles. This is the only case where this occurs in all of our sensitivity analyses.

³⁴ The potential importance of local efforts is highlighted by a recent reform of the enterprise zone program, which puts more emphasis on local management and local commitment. In particular, California AB 1550, enacted in 2006, updates standards for evaluation of enterprise zone performance, among other reforms. Insufficient local commitment to supporting the program, defined at the time of zone designation, is potentially grounds for de-designation (Arambula, 2008).

instance, are likely to have a larger effect on manufacturing firms than on services firms, and certain areas may be more amenable to manufacturing development than others. The hiring credit, too, could have a differential effect if firms in some industries can more easily employ disadvantaged workers. In our analysis, therefore, we also examine a short list of characteristics of enterprise zone locations that could influence the effectiveness of the program at the zone level, some of which were highlighted in Table 2.

5.1. Survey of Enterprise Zone Administrators

Our survey of enterprise zone administrators asked open-ended qualitative questions about their views on the purpose of the enterprise zone program, if they evaluate the program’s effectiveness, and what their biggest successes and challenges have been. We also asked detailed questions about how the local zone tries to maximize the effectiveness of the program, and we received quantitative responses to the following questions:³⁵

On a 1-5 scale, where 1 means “not at all active” and 5 means “extremely active,” how active is your zone in doing each of the following?

1. **using marketing**, for instance informing businesses about the zone and what incentives it offers;
2. **amending zoning or other local regulations** to favor growth in your zone;
3. **training workers** to make qualified candidates **or operating hiring centers** to match them with businesses;
4. **facilitating earning tax credits**, for example by hosting informational sessions with business owners or employees;³⁶
5. **encouraging the building of additional infrastructure**, such as a bus line or freeway spur;

³⁵ We conducted phone interviews in the spring and summer of 2007, lasting typically 30-45 minutes. The survey was a mix of yes/no, 1-5 scales, and open-ended questions. We talked to 36 zone managers; since some are responsible for multiple zones located in the same county, these 36 interviews covered all zones listed in Table 1, including those we did not include in our quantitative analysis. We identified potential respondents from the list of enterprise zone contacts on the HCD website. We promised respondents confidentiality.

³⁶ We did not refer specifically to the hiring tax credit, but it is clear from the open-ended responses to the survey that the question was interpreted this way. A large share of respondents provided information about how they help firms get vouchers for hiring eligible workers, referring businesses to tax consultants, etc., and no respondents referred to any other kind of tax credits. Thus, in what follows we interpret this response as referring specifically to facilitating earning the hiring tax credit.

6. **offering other tax incentives, credits, or discounts on public services** at the local level.

It would have been ideal to be able to gather historical information about zone activities from the original dates of zone designation to the last year covered by the data, but that was not feasible. Some respondents have worked as zone administrators for as little as a few months, and their responses refer only to the very recent past and the present.³⁷ On the other hand, many respondents had several years experience with their zone, some up to 20 years. Thus, the responses represent a mix of recent and long-term views and behaviors. Nonetheless, these surveys do provide insight into local zone activities.

Among the six activities we asked about, local zone administrators said they were most active using marketing and facilitating earning hiring tax credits, which both received average scores of 4.0 on the 1-5 scale. The next highest scores were, in order, offering other tax incentives, credits, or discounts (2.9), training workers or operating hiring centers (2.8), encouraging infrastructure building (2.3), and amending zoning or other local regulations (1.6). Zones varied in their self-reported scores: the average score across all six activities ranged from 4.3 for an urban zone in southern California to 1.2 for a rural zone. In general, the larger and more urban zones are more likely to facilitate earning tax credits and to offer other tax incentives, credits, or discounts on public services, and less likely to encourage building additional infrastructure.³⁸

We noted earlier that we focus on the job-creating effects of enterprise zones both because this is an explicit policy goal, and it is cited by zone administrators. This latter conclusion came out of the open-ended questions included in the interview, the responses to which indicated that zone administrators widely shared the view that the purpose of the enterprise zone program is to create jobs, in part by attracting and retaining businesses. In particular, we asked respondents what they thought the “primary goal” of the

³⁷ Although the 2006 reforms to the program had mainly to do with criteria for zone selection and reporting requirements, they may have affected behavior of zone administrators, further emphasizing that the 2007 survey responses may not always give an accurate representation of what zone administrators were doing in earlier years.

³⁸ The correlation of facilitating tax credits is .34 ($p=.10$) with employment and .30 ($p=.15$) with zone employment density. The correlation of offering other tax incentives, credits, or discounts on public services is .33 ($p=.12$) with employment and is .33 ($p=.11$) with zone density. The correlation of encouraging building additional infrastructure is $-.35$ ($p=.09$) with employment and $-.44$ ($p=.03$) with density.

enterprise zone program was. Nearly all mentioned job growth; far fewer also explicitly mentioned reducing unemployment or poverty.³⁹

5.2. Results

Table 5 presents descriptive statistics on a number of zone characteristics that we incorporate in our regression models. The top panel reports statistics for baseline characteristics of the zone. Most of these were reported by zone in Table 2. Here we report means and standard deviations as well as the 25th and 75th percentiles of the distribution across zones (all unweighted).⁴⁰ The bottom panel presents information on the survey responses regarding zone activities. Aside from the mean differences just discussed, the fairly substantial variation in many of the measures is noteworthy.

The estimates of different versions of specifications (3) and (4) are presented in Tables 6 and 7. We report estimates for four different 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 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”).

In light of the fact that the average effect of enterprise zones is near zero, evidence of variation in the effects of enterprise zones could suggest that some enterprise zones increase employment, while others decrease it. While one might think that if an incentive is ineffective it can have, at worst, no effect, this is not the case in an environment in which regions compete for employers/jobs. In this case, if one region

³⁹ Representative answers included the following: “To stimulate jobs and investment in economically disadvantaged areas of the State.” “To help the local economy by giving local businesses a means to expand and to encourage new firms to enter their area instead of a competing area.” “The program is a business attraction and retention tool. It’s the only statewide program we have.” “To make California competitive against other states and regions in terms of manufacturing. Enterprise zones are the only incentive that they really have left.”

⁴⁰ While weighted estimates would give a more representative picture of the variation faced by workers, the unweighted estimates – in particular for the standard deviations – give a better idea of how much zones vary.

adopts effective methods and one adopts ineffective methods, the latter *can* lose out, generating fewer jobs than it would if there were no enterprise zones, as businesses are attracted to the first region.⁴¹

In the employment level model (Table 6), 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 when we include the control rings and the interactions with the survey responses are also included. And there is evidence that zones designated more recently have more positive employment effects, although this evidence is statistically significant only in the last two columns, and therefore is less robust than the other findings on which we focus.

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, 5-percent level, or 10-percent level with or without the control rings included, as long as the specifications also include the interactions with the other zone characteristics. Among other local activities, facilitating earning 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, 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 7, where we look at the effects of enterprise zones on employment growth, the evidence is generally a bit weaker, but qualitatively similar. In particular, we continue to find negative estimates of the interaction between 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

⁴¹ Of course the estimated coefficients of the enterprise zone indicator and its interaction with zone characteristics only tell us about the effects of enterprise zones relative to the counterfactual of no enterprise zones. A negative coefficient on an interaction between the enterprise zone indicator and a zone characteristic does not imply that in an enterprise zone *with* that characteristic employment declines in absolute terms.

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 earning hiring tax credits, employment growth is lower; these results are significant at the 5-percent or 10-percent level, although only in the specifications including the control rings. And finally, there is also some evidence of a negative interaction with offering other tax incentives.⁴²

Given that we have a fairly small number of enterprise zones from which we identify the effects of variation in zone characteristics, and given that the models in Tables 6 and 7 include many interactions with zone characteristics that may have a high degree of multicollinearity, we were interested in assessing how robust the results are, aside from what can be seen from the different specifications reported in those tables. We therefore re-estimated the models including only one interaction in each specification, and compared the results to those in Tables 6 and 7. For example, we estimated the model adding *only* the interaction with the share in manufacturing in 1992, and compared the estimated coefficient of that interaction with what we get when all of the interactions are included in the various specifications. We found evidence that is generally supportive of the conclusions we drew from Tables 6 and 7. There continues to be evidence that zones with a larger share of manufacturing employment at the beginning of the sample period are less effective at creating jobs, and the zones designated later are more effective. We continue to find some evidence that marketing activities boost the effectiveness of enterprise zones, but that zones that focus on facilitating earning the hiring tax credit are less effective at creating jobs. In contrast, the evidence regarding offering other tax incentives appears less robust.⁴³

One problem with the estimates reported thus far is that they do not easily convey the effects of enterprise zones with particular types of characteristics. Of course one can use the descriptive information in Table 5 coupled with the regression estimates to do these calculations; but that is cumbersome and would not be informative about statistical significance. To better convey this information, we use the regression

⁴² One other result to note is that, as reported in the first row of the table, 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 store.

⁴³ Tables with these results are available from the authors upon request.

estimates and the variance-covariance matrix to calculate the effects of enterprise zones for different values of the zone characteristics. Specifically, for a given characteristic, we hold the others at their weighted mean values, and calculate the implied effects (and standard errors) at the 25th and 75th percentiles of the distribution of the characteristic in question.⁴⁴

To clarify, suppose our model contained two enterprise zone characteristics C^1 and C^2 . Then specification (3) becomes

$$(5) \quad Y_{jkt} = \alpha + \beta EZ_{jkt} + EZ_{jkt} \cdot (C_{jk}^1 - \bar{C}^1) \gamma_1 + EZ_{jkt} \cdot (C_{jk}^2 - \bar{C}^2) \gamma_2 + \sum_{j'=1}^J \left\{ \sum_{k'=0 \text{ or } 1}^{K_j} D_{j'k'} \lambda_{j'k'} \right\} + \sum_{t'=1}^T D_{t'} \theta_{t'} + \sum_{j'=2}^J \sum_{t'=1}^T \{D_{j'} \cdot D_{t'}\} \tau_{j't'} + \varepsilon_{jkt}$$

and, for example, the estimated effect of enterprise zones at the 25th percent of C^1 , denoted $C^{1(25)}$, is

$$(6) \quad \hat{\beta} + (C^{1(25)} - \bar{C}^1) \hat{\gamma}_1 + (\bar{C}^2 - \bar{C}^2) \hat{\gamma}_2 = \hat{\beta} + (C^{1(25)} - \bar{C}^1).⁴⁵$$

The results are reported in Table 8, for the three zone characteristics for which we found relatively robust evidence of differential effects.⁴⁶ As the table shows, for the specifications with control rings, the estimated effect of enterprise zones is in fact positive and statistically significant in areas with a relatively low manufacturing base (the 25th percentile, or a 9.2 percent share), and the point estimates are qualitatively similar when the control rings are excluded. With regard to local marketing efforts, in three of the four cases we find a positive and statistically significant effect of enterprise zones for relatively high activity levels (the 75 percentile, or a 5 on the 1-5 scale on which responses are ranked). Finally, with respect to facilitating the earning of hiring credits, we find that the estimated effects – for the specifications including control rings – are positive when engagement in this activity is low (the 25th percentile, or a 3 on the 1-5 scale). The implied effects of moving from the 25th to the 75th percentile, holding other characteristics at their means, can be read off of the differences in effects at these two percentiles. For example, looking at columns (5) and (6), the effect of moving marketing activity from the 25th to the 75th percentile is a 1.5 percentage point increase in the employment growth rate, and the implied effect of moving from the 25th to

⁴⁴ For the standard error calculations, we treat these percentiles as known.

⁴⁵ Note that we use unweighted percentiles, since our goal is to report the implied effects for different types of enterprise zones.

⁴⁶ We do not do this for the year of designation, since that variable does not reflect a policy choice.

the 75th percentile of the share manufacturing is 0.8 percentage point slower employment growth. The implied effects from such calculations are quite large, but the standard errors are sizable, and of course in comparing across actual zones one would want to take account of differences in all characteristics.⁴⁷

5.3. Robustness Analyses

One potential problem with estimating the effects of enterprise zones is that the program could have positive spillovers, encouraging employment growth not only within zone boundaries but just outside zone boundaries as well. In this case, when we estimate the effects of enterprise zones by comparing the zones to immediately neighboring areas we may have a bias towards finding no effect.⁴⁸ To examine this issue, we have re-estimated the models using a considerably larger control ring that extends 2,500 feet from the boundaries of the enterprise zone. If positive spillover effects are important, then we should find larger effects when using the larger control ring, although of course the area in the larger control ring may be less comparable to the enterprise zone treatment areas. As reported in the top panel of Table 9, the estimates using the larger control ring are virtually the same as the corresponding estimates in Table 8.⁴⁹ Thus, we conclude that positive spillovers likely to do not influence our results.

A second robustness analysis we considered is intended to address possible differences between the treatment and comparison areas. In particular, although we have argued that the areas appear largely comparable, and that our regression models capture differences between the areas, these arguments referred to the differences in levels (or time-invariant characteristics) across areas initially designated as enterprise zones, expansion areas, and control rings. If enterprise zones were established or expanded into areas that had different trajectories – either faster or slower employment growth – then our estimates could still be biased. To address this in a quite flexible way, we augmented the regression models (3) and (4) to include a

⁴⁷ Note, of course, that many of the implied effects of zone characteristics are much smaller. We have applied a filter of emphasizing those that are significant and hence, in general, largest. Of course similar calculations can be done by combining any of the regression estimates with the descriptive statistics in Table 5.

⁴⁸ Spillovers could stem from a number of sources, including increased retail “traffic,” rising incomes of nearby residents, and changes in infrastructure. The discussion assumes positive effects. If enterprise zone have negative effects, then again positive spillovers (i.e., spillovers in the same direction) would generate a bias toward finding no effect.

⁴⁹ We show the results for the employment level specification; results were also insensitive to using the larger control ring for the employment growth rate specification. Only the results using the control ring are different in this case, which is why columns (3) and (4) are empty.

full set of interactions between the year fixed effects and two dummy variables: one for all areas that are in control rings, and one for all zone expansions. This specification allows for arbitrary differences in the economic shocks affecting these two types of areas, relative to the initial designation areas of enterprise zones, and hence goes a long way toward accounting for time-varying differences between the three types of areas.⁵⁰ The results are reported in the bottom panel of Table 9. Compared to the baseline estimates in Table 8, columns (1)-(4), the point estimates are very similar, although less precise, unsurprisingly. Thus, allowing for differences in underlying employment changes in treatment and control areas does not change the results or the conclusions.

5.4. Relocation

Although some of the estimates in Table 8 point to characteristics of enterprise zone locations or administration that can lead to positive employment effects, *any* evidence of positive effects of enterprise zones on employment must consider the possibility that enterprise zones boost employment not through the creation of new jobs, and not by simply encouraging existing establishments to move into an enterprise zone to take advantage of zone incentives.⁵¹ In contrast to the possibility of positive spillovers considered in the previous sub-section, the relocation story is one of negative spillovers. The possibility that enterprise zones simply create incentives for businesses to move from outside to inside the zone has been noted most forcefully, perhaps, by Ladd (1994), who argues that the principal effect of pure place-based incentives (that do not also focus on residents, community resources, etc.) is to generate this type of relocation.

The most extensive evidence on relocation in response to zone incentives comes from previous experience in the United Kingdom – in which, unlike the United States, zones were established in places with little or no industrial activity. Research on the U.K. program found that between 50 and 80 percent of enterprise zone businesses had relocated into the zones, prompting the British government to phase out the program (Papke, 1993). Earlier evidence for the United States suggested that relocation has played a much

⁵⁰ We of course cannot allow arbitrary year effects for each subzone separately, as this would fully subsume the effects of enterprise zone designation.

⁵¹ The concept of a “new job” is ambiguous. When we refer to employment growth attributable to the creation of new jobs, we mean increases in births of establishments or growth of existing establishments, or conversely decreases in establishment deaths or contractions.

smaller role (e.g., Erickson, et al., 1989; U.S. Department of Housing and Urban Development, 1986), although the data have not been available to estimate the relocation response to enterprise zones and to compare it to other responses. Reflecting the preference for boosting births, for example, rather than relocations, some states have inserted provisions that bar relocating businesses from obtaining enterprise zone benefits (Wilder and Rubin, 1996). On the other hand, as Papke (1993) notes, relocation is not necessarily all bad, as it may bring economic activity to a region that previously lacked it, which may still generate some of the externalities or redistribution that policymakers intended.

As it turns out, the NETS data are ideally suited to studying the question of relocation, since they report the exact address (which we have geocoded) of each establishment in each year. Our strategy is to identify types of establishment moves – and their associated effects on employment – that we would want to exclude from the effects of enterprise zones, and then to re-estimate the models in Table 8 ignoring the employment effects of these moves. We have chosen to focus on moves *within* a given enterprise zone (its boundaries as of 2004) and its associated control ring that result in a change in enterprise zone status. For this analysis we use the 2,500-foot control ring that was mentioned in the sensitivity analyses above, rather than the 1,000-foot control ring, to allow for a somewhat larger, although still small, area over which moves can occur. In contrast, however, we ignore moves that result in changes in enterprise zone status that cover longer distances – from one enterprise zone to another, or into or out of enterprise zones from non-enterprise zone regions of the state. The rationale is two-fold. First, what we want to avoid is attributing job creation to enterprise zones when the job creation stems from short distance moves that may not even affect the labor market in which a business operates. And second, given that we do not really know the motives for businesses move, it seems more plausible to assume that very short moves were in response to zone incentives, whereas longer moves were not.⁵²

The results of this analysis are reported in Tables 10 and 11. Table 10 reports the number and

⁵² To be clear, we think it unlikely that a business moves from Los Angeles to San Francisco *because of* enterprise zone incentives, in part because there is a zone in Los Angeles anyway, and in part because long-distance moves are almost surely attributable to other factors. However, *conditional on* making a long-distance move, the business might choose to locate in an enterprise zone in the destination city, and it seems like we want to count this type of job creation as a benefit of the zone.

distribution of the relevant types of relocations, of which there are four: intra-zone moves in which a business moves *into* an area with current enterprise zone designation, from either the control ring or a part of the zone not yet designated; and intra-zone moves in the opposite direction. The first thing to notice is that the numbers of moves in the two different directions are not very different. For example, there are 6,879 moves from control rings into designated zone areas, but only about 250 fewer moves in the opposite direction. In addition, the numbers of establishment relocations captured in Table 10 are very small, together accounting for just over 0.5 percent (i.e., one-half of one percent) of establishment-year observations. Of course these are establishment counts, and the implications for employment may differ depending on the size of establishments. Thus, Table 11 reports estimates of the same models as in Table 8, but excluding from the calculation any employment changes associated with establishments making the relocations tabulated in Table 10. The estimates in Table 11 are virtually identical to those in Table 8, indicating that relocation of establishments within the geographic areas defined by the enterprise zones and their control rings does not drive any of the results.

5.5. Interpretation

In one sense it is surprising that enterprise zones have a stronger positive effect on employment when the zone is less manufacturing-heavy, since some of the tax incentives that the enterprise zone program offers, like the sales tax credit for machinery, should benefit manufacturing firms more than firms in other industries. One reason why the enterprise zone program might be less effective in manufacturing areas could be that manufacturing firms are often the target of other economic development efforts: the site-location decisions of automobile plants, for instance, get considerable public attention. A couple of the zone managers we surveyed noted that their biggest challenge was that the enterprise zone benefits are small relative to the incentives offered by other states and localities. While their comments may not be representative of all of California's enterprise zones, they emphasize that the enterprise zone program is only one tool that economic developers use to attract and retain businesses. Even if the enterprise zone program offers incentives that should appeal to manufacturing more than to other sectors, the competition for manufacturing jobs may be stiffer than for other jobs, and enterprise zone benefits may therefore matter

less for manufacturing firms. Another possibility is that for manufacturing industries, some of the other enterprise zone benefits focused on property and machinery are more important; because these subsidize capital rather than labor, they could encourage a shift from labor to capital, offsetting the positive employment effects that the hiring credit might create.⁵³

The evidence that activities focused on earning the hiring tax credit reduce the job-creating effects of enterprise zones is unexpected. One possible interpretation is that these activities focus more on claiming the tax credits retroactively than on creating jobs currently. Tax credits can be claimed retroactively for up to four years, and a substantial share of enterprise zone tax credits are claimed retroactively (California Budget Project, 2006). It is also clear from perusing the internet that many tax-service companies advertise their ability to help businesses in California receive tax reductions for the credits retroactively.⁵⁴ Critics of enterprise zones point to this retroactive activity as evidence that the zone benefits do not help create jobs: “By definition, retroactive credits provide bonuses for past actions, but do not encourage businesses to increase or maintain employment in future years and thus do not further program goals” (California Budget Project, 2006, p. 13). A high level of retroactive claiming with no job creation effects could occur if many firms do not know about the hiring credit until after the fact, so that it does not affect their hiring behavior. However, another possibility is that firms may know about and respond to the credit, but file retroactively only once they earn profits, or once the “load” is large enough to justify the costs of filing that are likely partially fixed. The behavior surrounding retroactive hiring credits is difficult to pin down. However, our estimates indicating that zones less focused on these credits are more effective at creating jobs provides some evidence in favor of the more critical view of how the hiring tax credit gets used (i.e., that some of the activities surrounding the hiring credit focus more on retroactive credits than on creating jobs contemporaneously).⁵⁵

⁵³ Greenbaum and Engberg (2004) find no effect on enterprise zones on manufacturing employment overall, using data from six states.

⁵⁴ See, e.g., ntcgtax.blogspot.com and enterprisetaxcredits.com/enterprise-zone-hiring-credit/ (viewed July 7, 2009).

⁵⁵ There is also the possibility of “cross-vouchering,” whereby one zone helps businesses from other zones get vouchers for the hiring credit. According to the California Budget Project (2006), enterprise zone administrators charge for this, and some zones adopted lenient documentation standards; indeed, new regulations adopted in 2007 standardized the documentation requirements for vouchering (e.g., California CPA Magazine, 2008; Fine, 2007).

Finally, of the local activities we asked zone administrators about in our survey, only marketing and outreach efforts increase the job-creating effects of enterprise zones. Marketing and outreach efforts are among the activities that zone managers are most likely to say they do (see Table 5), and the current zone application process requires localities to lay out a marketing plan. We caution, however, that we are measuring zone managers' own perceptions of local marketing and outreach efforts. It is possible that managers view themselves as more active in marketing and outreach after observing positive employment effects in their zones, in which case the self-assessment of marketing activity could be the result of employment growth rather than a contributor. We also reiterate the earlier caution that we have asked current zone managers about their recent marketing efforts, whereas our analysis of employment effects looks at the period 1992-2004. Nonetheless, our finding supports 2006 reforms to the enterprise zone program that placed more emphasis on local activities and local commitment.

6. Conclusions

We explore sources of heterogeneity in the effectiveness of enterprise zones. This inquiry is motivated by the policy question of whether, despite the overall discouraging conclusions from existing estimates of the average effects of enterprise zones, there may be ways to make enterprise zones more effective. In particular, we estimate how the effects of enterprise zones vary with factors relating to the areas in which enterprise zones are established as well as how enterprise zones are implemented and administered. Both of these sources of variation in the effectiveness of enterprise zones are potentially exploitable by policymakers and administrators – replicating or encouraging the features of enterprise zone programs that are associated with increases in employment, and discouraging zone location decisions and policies and activities that lead to ineffective or counter-productive enterprise zones.

Our empirical analysis is based on data from California's enterprise zone program, including: detailed GIS maps of enterprise zone boundaries; longitudinal data on the universe of business establishments with which, in combination with the maps, we can measure employment changes inside and outside enterprise zones; data on the characteristics of areas in which enterprise zones are established; and

Again, this cross-vouchering activity might have detracted from other efforts to boost zone employment, especially if the cross-vouchering is focused on retroactive credits.

information from surveys we conducted of enterprise zone administrators on the activities of their zones.

Our approach depends on both the detailed data on employment change and results from our survey of local zone administrators. The detailed data on employment change, though costly to acquire and time-consuming to geocode, map, and analyze, are of course essential to the analysis. Our surveys of local administrators, though, required only about 100 hours of work; relative to this very modest investment, its contribution to our understanding of enterprise zone effectiveness is considerable.

The evidence points to some potentially significant sources of variation in the effectiveness of enterprise zones. In particular, the enterprise zone program appears to have favorable effects on employment for zones established in areas that have a relatively low share of manufacturing employment, suggesting that enterprise zone incentives do more to favor the creation of jobs outside the manufacturing sector than inside that sector. Second, enterprise zones are more likely to boost employment when local administrators devote relatively more effort to marketing and outreach activities. On the other hand, devoting more effort to helping firms get hiring tax credits reduces any positive employment effects, and only zones that focus less on these activities appear to increase employment. This latter result may stem from idiosyncrasies of California's enterprise zone program that may have encouraged efforts to help firms get hiring credits that were not in the service of boosting job growth. More generally, given that enterprise zone programs differ across states, one should be cautious in extrapolating our results from California to draw conclusions about enterprise zone programs in other states, particularly if their programs are quite different.

We have raised some cautions about the strength of this evidence – given that we do not have longitudinal evidence on zone activities, and that we are asking a lot of the data in trying to identify and estimate the sources of variation in enterprise zone effects. Nonetheless, the evidence does suggest some potential avenues by which policymakers might be able to make enterprise zones more effective. More than anything else, perhaps, the nature of the evidence we find points to the potential value of shifting research on enterprise zones towards a greater emphasis on understanding whether some types of enterprise zones

are more effective than others, and what policymakers can do to design programs that might deliver more than is suggested by the heretofore discouraging research on enterprise zones.⁵⁶

The scope for obtaining additional data on enterprise zone activities may be quite limited, unless such data come to be collected as part of the administration of zones. We think that regular surveys of zone administrators on their current activities, with a survey instrument consistent across zones and over time, would be an inexpensive way to assess how recent and future reforms to the program affect local activities and, in turn, affect employment change and other outcomes. Such surveys would be an improvement on ours, which necessarily asked administrators about their activities retrospectively. Furthermore, building on the ability to exploit GIS mapping that we have illustrated in this paper, there may be considerably more scope for more detailed analysis of variation in the types of areas in which enterprise zones are situated – including things such as developable land zoned for commercial or industrial use, business and transportation infrastructure, and educational and training institutions – and how this variation influences the employment effects of enterprise zones.

⁵⁶ In a related type of analysis, Wernstedt et al. (2006) survey private developers to attempt to understand what types of policies and incentives are more or less likely to spur brownfield cleanup and development.

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Figure 1A: Santa Ana Enterprise Zone, City Streets and Enterprise Zone as of 2004

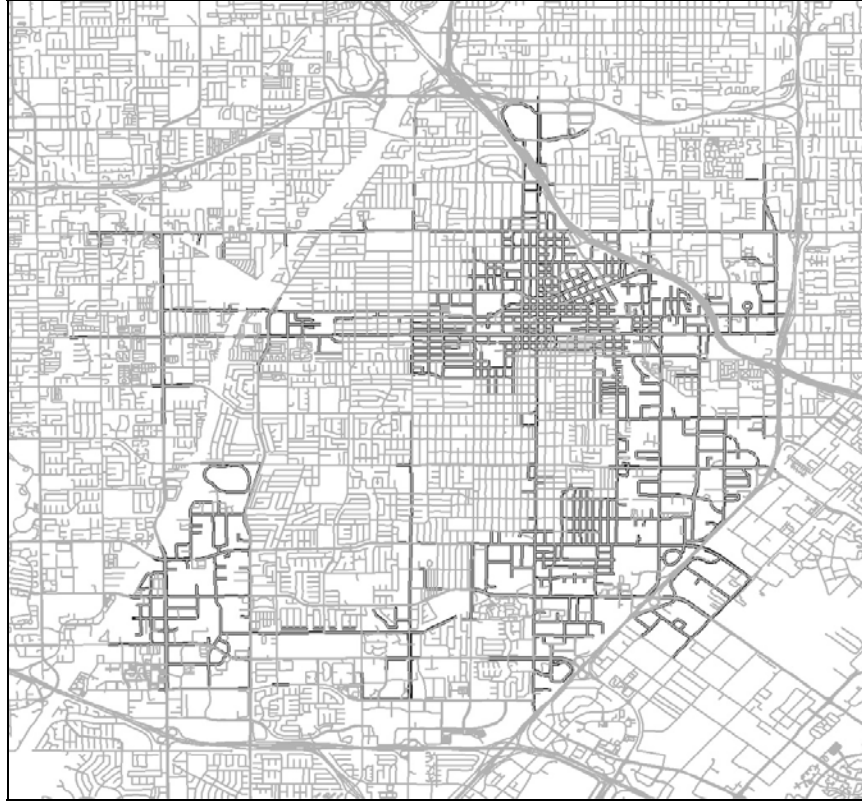


Figure 1B: Santa Ana Enterprise Zone, Initial 1993 Designation (thick black), 1994 Expansion (grey), and Control Ring (thin black)

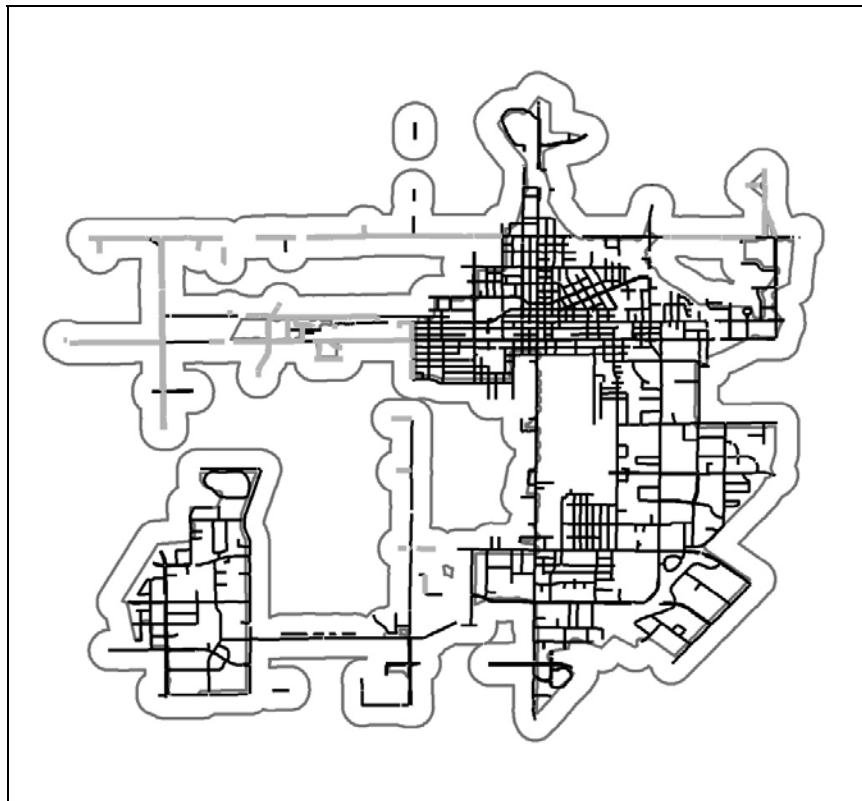


Table 1: Current California Enterprise Zones and Year of Designation^a

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

^a The five Los Angeles zones are treated as one large zone for the analysis. In some cases the sources listed below provided different start dates. In the cases of such discrepancies, we checked with zone administrators to verify the start date. For Coachella, because the zone started in late 1991 (November 10), we use 1992 as the first year.

Sources: http://www.caez.org/Programs/Map_of_CA_Zones.html (viewed September 19, 2008); street address changes taken from street files, found at <http://www.hcd.ca.gov/fa/cdbg/ez/enterprise> (viewed November 1, 2006); Assembly Jobs, Economic Development, and the Economy Committee (2006).

Table 2: Descriptive Information on Zones^a

	Empl. in enterprise zone (2004)	Empl. in county (2004)	Col. 1/ county empl.	Col. 1/ state empl.	Empl. density (1992)	Share of empl. in manufacturing (1992)	Share of empl. in establishments with < 50 employees (1992)	% college degree in county (1990)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Los Angeles	274,434	4,677,221	5.9	1.7	5,062	28.8	40.4	22.3
San Francisco	215,329	600,488	35.9	1.3	22,813	9.2	44.2	35.0
Santa Ana	175,018	1,733,164	10.1	1.1	19,919	20.9	39.3	27.8
Oakland	163,181	775,214	21.0	1.0	5,854	12.9	41.7	28.8
Long Beach	121,754	4,677,221	2.6	0.7	8,285	11.2	46.9	22.3
San Jose	98,162	984,246	10.0	0.6	8,787	12.5	46.1	32.6
Sacramento, Florin Perkins and Army Depot	40,832	624,638	6.5	0.2	4,285	15.7	42.4	23.0
Shasta Metro	40,178	76,069	52.8	0.2	1,059	9.4	51.3	13.7
Altadena/Pasadena	33,956	4,677,221	0.7	0.2	8,744	10.4	44.8	22.3
San Diego, Barrio Logan	28,624	1,440,987	2.0	0.2	5,085	22.0	51.3	25.3
West Sacramento	24,779	85,538	29.0	0.2	1,673	11.1	46.9	30.3
San Diego, Ysidro/Otay Mesa	24,196	1,440,987	1.7	0.1	1,658	34.8	46.5	25.3
Yuba/Sutter	21,853	47,581	45.9	0.1	487	16.2	52.0	12.5
Richmond	20,567	389,983	5.3	0.1	3,561	17.4	47.3	31.6
Eureka	18,065	50,442	35.8	0.1	4,843	3.2	56.2	20.0
Sacramento, Northgate/Norwood	15,279	624,638	2.4	0.1	7,790	11.2	48.9	23.0
Coachella Valley	11,050	586,101	1.9	0.1	298	6.0	48.7	14.6
Madera	9,765	38,635	25.3	0.1	2,176	34.4	47.3	11.7
Oroville	8,954	81,353	11.0	0.1	1,006	11.3	48.8	19.5
Bakersfield	8,829	242,303	3.6	0.1	1,516	9.8	50.8	13.3
Delano	6,212	242,303	2.6	0.0	1,239	2.6	58.7	13.3
Shasta Valley	5,818	18,777	31.0	0.0	1,395	6.3	62.3	14.2
Shafter	3,695	242,303	1.5	0.0	786	3.6	73.4	13.3
Lindsay	2,758	123,101	2.2	0.0	1,331	21.0	55.1	11.8
Porterville	2,633	123,101	2.1	0.0	2,014	40.6	34.6	11.8
Merced	641	68,050	0.9	0.0	119	1.9	89.9	12.0

^a Figures are reported for the complete area of each zone as of 2004. In cases where a zone is mainly in one county but also extends into another county, in this table the zone is assigned to the county in which most of the zone is located. Note that some numbers repeat in column (2). This occurs when there are multiple zones in the same county. For reasons explained in the text, the separate Los Angeles zones listed in Table 1 are treated as one zone.

Table 3: Descriptive Statistics for Enterprise Zones and Comparison Areas, 1992

	Whole sample (zones plus control rings)	Areas ever included in EZ's	Areas included in the original EZ designations	Areas included in EZ expansions	1,000-foot control ring
Employment (total)	1,953,220	1,349,629	976,119	373,510	603,591
Establishments (total)	140,969	96,752	71,006	25,746	44,217
Employees per square mile	9,974	11,531	10,778	13,500	8,124
Employees per establishment (weighted mean)	14.6	14.7	14.2	16.2	14.3
Share of employees in low-wage industries (weighted mean)	11.6	11.8	11.6	12.4	10.9
Share of employees in manufacturing (weighted mean)	8.0	8.6	7.8	10.8	6.7

Table 4: Regression Estimates of Effects of Enterprise Zones^a

	With control rings	Without control rings
	(1)	(2)
A. Shift in level		
Enterprise zone	-0.017 (0.047)	-0.012 (0.035)
B. Shift in growth rate		
Enterprise zone × linear trend	0.002 (0.011)	-0.007 (0.012)
N	1,300	962

^a Each column and panel reports estimates of the enterprise zone effects from a separate regression. These are estimates of specifications (1) and (2) in the text. The dependent variables are in logs, substituting ones for zeros in levels prior to taking logs. The models all include subzone and year dummy variables, and zone-year interactions. There are 26 zones, with the total number of initial zone designations and expansions adding to 74. Thus, because we have 13 years of data, when we do the analysis without control rings we have 962 observations (74×13). When we include a control ring for each zone, we have 1,300 observations ($\{74 + 26\} \times 13$). 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.

Table 5: Descriptive Statistics on Zone Characteristics^a

	Units	Mean	Standard deviation	25th percentile	75th percentile
Share of employment in establishments with fewer than 50 employees, 1992	Percentage	50.6	11.2	44.8	52.0
Share of employment in manufacturing, 1992	Percentage	14.8	10.3	9.2	20.9
Employment density, 1992	Empl. per square mile	4,684	5,639	1,239	5,854
Persons 25 + with bachelor's degree or higher, 1990, county	Percentage	20.4	7.5	13.3	25.3
Year of designation	Year	1990	3	1986	1992
Marketing	1-5 scale	4.0	0.9	3.0	5.0
Amending zoning	1-5 scale	1.7	1.1	1.0	2.0
Training workers	1-5 scale	2.8	1.3	1.0	4.0
Facilitating earning tax credits	1-5 scale	4.0	1.2	3.0	5.0
Encouraging the building of additional infrastructure	1-5 scale	2.3	1.5	1.0	3.0
Offering other tax incentives, credits or discounts	1-5 scale	2.8	1.6	1.0	4.0

^a All rows report unweighted statistics treating each enterprise zone as an observation. The figures in the top panel are based on zone boundaries as of 2004. The survey question in the bottom panel are based on a 1-5 scale regarding how active the zone is in doing each of the activities, 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 6: Regression Estimates of Effects of Enterprise Zones on Employment, Interacted with Zone Characteristics: Shift in Level^a

	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 (0.019)	0.013 (0.027)	0.038 (0.026)	0.031 (0.036)	0.029 (0.025)	0.047 (0.035)	0.001 (0.013)	0.027 (0.027)
Enterprise zone interacted with:								
Share of employment in establishments with < 50 employees, 1992	-0.509 (0.577)	-0.983 (1.343)			-0.911 (0.734)	-1.093 (1.966)	-0.946 (0.882)	-1.370 (2.337)
Share of employment in manufacturing, 1992	-0.782 (0.327)**	-0.712 (0.722)			-1.719 (0.820)**	-1.275 (1.126)	-0.972 (0.310)***	-0.804 (0.551)
Employment density, 1992 ('000)	0.001 (0.004)	-0.001 (0.012)			-0.014 (0.007)*	-0.008 (0.013)	-0.009 (0.003)***	-0.003 (0.010)
Percent adults 25 + with bachelor's degree, 1990, county	-0.065 (0.739)	-0.218 (1.296)			0.656 (0.392)	0.052 (0.650)	0.348 (0.210)	-0.360 (0.719)
Zone designation year (# years after 1986)	0.006 (0.009)	0.006 (0.014)			0.038 (0.023)	0.030 (0.034)	0.022 (0.006)***	0.022 (0.011)**
Marketing (from survey: 1-5 scale)			0.064 (0.042)	0.069 (0.053)	0.158 (0.065)**	0.148 (0.086)*	0.140 (0.032)***	0.167 (0.051)***
Amending zoning (from survey: 1-5 scale)			0.003 (0.034)	0.035 (0.045)	-0.039 (0.027)	-0.026 (0.039)	-0.032 (0.013)**	-0.049 (0.026)*
Training workers (from survey: 1-5 scale)			0.016 (0.025)	0.012 (0.023)	0.034 (0.036)	0.032 (0.045)	0.026 (0.019)	0.034 (0.029)
Facilitating earning tax credits (from survey: 1-5 scale)			-0.064 (0.027)**	-0.025 (0.044)	-0.123 (0.064)*	-0.061 (0.092)	-0.121 (0.063)*	-0.074 (0.090)
Encouraging the building of additional infrastructure (from survey: 1-5 scale)			-0.011 (0.027)	-0.062 (0.036)	-0.055 (0.035)	-0.046 (0.056)	-0.030 (0.020)	0.002 (0.047)
Offering other tax incentives, credits or discounts (from survey: 1-5 scale)			-0.014 (0.029)	-0.082 (0.047)*	-0.025 (0.023)	-0.058 (0.027)**		
N	1,300	962	1,092	767	1,092	767	1,300	962

^a 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. All of the variables interacted with the enterprise zone treatment are de-measured, 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. 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 7: Regression Estimates of Effects of Enterprise Zones on Employment, Interacted with Zone Characteristics:
Shift in Growth Rate^a**

	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 (0.002)*	0.001 (0.005)	0.011 (0.004)**	0.012 (0.016)	0.011 (0.002)***	0.023 (0.018)	0.004 (0.002)**	0.010 (0.016)
Enterprise zone × linear trend interacted with:								
Share of employment in establishments with < 50 employees, 1992	0.035 (0.063)	-0.069 (0.151)			-0.010 (0.054)	-0.363 (0.458)	0.005 (0.057)	-0.229 (0.352)
Share of employment in manufacturing, 1992	-0.106 (0.050)**	-0.106 (0.121)			-0.065 (0.054)	-0.178 (0.343)	-0.101 (0.060)	-0.351 (0.301)
Employment density, 1992 ('000)	0.001 (0.001)	0.000 (0.003)			-0.001 (0.001)	-0.001 (0.003)	-0.001 (0.001)	-0.003 (0.003)
Percent adults 25 + with bachelor's degree, 1990, county	0.024 (0.093)	-0.142 (0.305)			0.155 (0.114)	-0.194 (0.321)	0.121 (0.092)	-0.058 (0.376)
Zone designation year (# years after 1986)	0.0001 (0.001)	0.004 (0.002)*			0.001 (0.002)	0.002 (0.008)	0.002 (0.001)	0.005 (0.005)
Marketing (from survey: 1-5 scale)			0.007 (0.006)	0.014 (0.023)	0.007 (0.007)	0.024 (0.031)	0.010 (0.005)*	0.041 (0.024)
Amending zoning (from survey: 1-5 scale)			0.001 (0.003)	0.008 (0.012)	0.002 (0.005)	-0.003 (0.018)	-0.001 (0.002)	-0.016 (0.019)
Training workers (from survey: 1-5 scale)			-0.004 (0.002)	-0.002 (0.012)	-0.001 (0.002)	0.005 (0.019)	0.000 (0.002)	0.006 (0.017)
Facilitating earning tax credits (from survey: 1-5 scale)			-0.009 (0.005)**	0.002 (0.015)	-0.014 (0.006)**	-0.005 (0.021)	-0.013 (0.007)*	-0.012 (0.020)
Encouraging the building of additional infrastructure (from survey: 1-5 scale)			-0.005 (0.006)	-0.015 (0.006)**	-0.007 (0.005)	-0.017 (0.013)	-0.006 (0.003)*	-0.010 (0.012)
Offering other tax incentives, credits or discounts (from survey: 1-5 scale)			0.001 (0.004)	-0.018 (0.010)*	-0.002 (0.005)	-0.020 (0.012)		
N	1,300	962	1,092	767	1,092	767	1,300	962

^a See notes to Table 6. These are estimates of specification (4) in the text.

Table 8: Estimates of Variation in Effects of Enterprise Zones on Employment^a

	Shift in level				Shift in growth rate			
	With control rings		Without control rings		With control rings		Without control rings	
Evaluated at:	25th percentile	75th percentile	25th percentile	75th percentile	25th percentile	75th percentile	25th percentile	75th percentile
Corresponding specification:	Table 6, col. (5)	Table 6, col. (5)	Table 6, col. (6)	Table 6, col. (6)	Table 7, col. (5)	Table 7, col. (5)	Table 7, col. (6)	Table 7, col. (6)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effect of enterprise zone evaluated at 25th and 75th percentiles of:								
Share of employment in manufacturing, 1992	0.101 (0.029)***	-0.100 (0.078)	0.104 (0.063)	-0.046 (0.086)	0.014 (0.003)***	0.006 (0.005)	0.031 (0.029)	0.010 (0.021)
Marketing (from survey: 1-5 scale)	-0.150 (0.091)	0.166 (0.045)***	-0.125 (0.106)	0.170 (0.079)**	0.003 (0.008)	0.018 (0.006)**	-0.004 (0.030)	0.043 (0.040)
Facilitating earning tax credits (from survey: 1-5 scale)	0.202 (0.071)***	-0.044 (0.060)	0.135 (0.131)	0.013 (0.066)	0.030 (0.009)***	0.003 (0.004)	0.031 (0.033)	0.020 (0.023)
N	1,092	1,092	767	767	1,092	1,092	767	767

^a See notes to Table 6. The table reports estimated effects holding zone characteristics at their sample means, except for the variation in the characteristic indicated in each row. The 25th and 75th percentiles used are the unweighted percentiles across zones reported in Table 5. The column headings indicate the specification in Tables 6 and 7 to which the estimated effects correspond.

**Table 9: Estimates of Variation in Effects of Enterprise Zones on Employment,
Robustness Analyses^a**

	Shift in level			
	With control rings		Without control rings	
	25th percentile	75th percentile	25th percentile	75th percentile
Evaluated at:	Table 6, col. (5)	Table 6, col. (5)	Table 6, col. (6)	Table 6, col. (6)
Corresponding specification:	(1)	(2)	(3)	(4)
<i>With 2,500-foot control ring</i>				
Effect of enterprise zone evaluated at 25th and 75th percentiles of:				
Share of employment in manufacturing, 1992	0.076 (0.033)**	-0.128 (0.081)		
Marketing (from survey: 1-5 scale)	-0.137 (0.091)	0.111 (0.054)**		
Facilitating earning tax credits (from survey: 1-5 scale)	0.225 (0.077)***	-0.090 (0.060)		
<i>With separate year effects for control rings and expansion areas</i>				
Effect of enterprise zone evaluated at 25th and 75th percentiles of:				
Share of employment in manufacturing, 1992	0.074 (0.057)	-0.127 (0.111)	0.068 (0.082)	-0.042 (0.132)
Marketing (from survey: 1-5 scale)	-0.174 (0.115)	0.139 (0.066)**	-0.125 (0.130)	0.146 (0.107)
Facilitating earning tax credits (from survey: 1-5 scale)	0.188 (0.099)*	-0.076 (0.080)	0.095 (0.153)	0.001 (0.096)
N	1,092	1,092	767	767

^a See notes to Tables 6 and 8.

Table 10: Relocations

	Frequency	Percent
Intra-zone relocations		
<i>Into designated enterprise zone</i>		
Move from 2,500-foot control ring to subzone designated as enterprise zone	6,879	0.25
Move from subzone not designated as enterprise zone to subzone designated as enterprise zone	262	0.01
<i>Out of designated enterprise zone</i>		
Move from subzone designated as enterprise zone to 2,500-foot control ring	6,624	0.24
Move from subzone designated as enterprise zone to non-designated subzone	167	0.01
<i>Intra-zone moves that did not change enterprise zone status</i>	10,378	0.37
Total establishment-year observations	2,780,085	100

Table 11: Estimates of Variation in Effects of Enterprise Zones on Employment, Excluding Movers^a

	Shift in level				Shift in growth rate			
	With control rings		Without control rings		With control rings		Without control rings	
Evaluated at:	25th percentile	75th percentile	25th percentile	75th percentile	25th percentile	75th percentile	25th percentile	75th percentile
Corresponding specification:	Table 6, col. (5)	Table 6, col. (5)	Table 6, col. (6)	Table 6, col. (6)	Table 7, col. (5)	Table 7, col. (5)	Table 7, col. (6)	Table 7, col. (6)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effect of enterprise zone evaluated at 25th and 75th percentiles of:								
Share of employment in manufacturing, 1992	0.098 (0.028)***	-0.097 (0.074)	0.099 (0.060)	-0.046 (0.085)	0.014 (0.003)***	0.006 (0.005)	0.032 (0.029)	0.009 (0.022)
Marketing (from survey: 1-5 scale)	-0.160 (0.089)*	0.173 (0.045)***	-0.131 (0.102)	0.185 (0.080)**	0.002 (0.009)	0.018 (0.007)**	-0.009 (0.029)	0.049 (0.041)
Facilitating earning tax credits (from survey: 1-5 scale)	0.196 (0.070)***	-0.043 (0.058)	0.126 (0.125)	0.014 (0.068)	0.030 (0.009)***	0.003 (0.004)	0.027 (0.034)	0.022 (0.023)
N	1,092	1,092	767	767	1,092	1,092	767	767

^a See notes to Tables 6 and 8. The sample on which the calculations in this table are based differs from that in Table 8. Contributions to employment change from establishments that moved *within* an enterprise zone or its associated control ring, from an area that was not currently designated as a zone to an area that was so designated, or vice versa, are excluded. (Note that the numbers of subzones and years, and hence the sample size for the regressions, remain the same.) As in Table 8, the table reports estimated effects holding zone characteristics at their sample means, except for the variation in the characteristic indicated in each row. The 25th and 75th percentiles used are the unweighted percentiles across zones reported in Table 5. The column headings indicate the specification in Tables 6 and 7 to which the estimated effects correspond.