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30 April 2010

TO: Oregon Economic Development Association Executive Board
FROM: Bryce Ward
SUBJECT: AN EVALUATION OF THE EFFECTS OF OREGON'S ENTERPRISE ZONES – A GEOSPATIAL ANALYSIS USING THE NATIONAL ESTABLISHMENT TIME SERIES (NETS)

I. Assignment and Overview

The Oregon Economic Development Association (OEDA) hired ECONorthwest to help answer several pressing questions relating to enterprise zones including:

> *Do enterprise zone tax incentives substantially spur investment?*
>
> *What is the impact on direct, new employment and other associated benefits?*
>
> *What is the fiscal/unfunded imposition on services from enterprise zone developments?*
>
> *Do the benefits or return from these enterprise zone developments outweigh the costs?*
>
> *What does this and other research specific to Oregon's enterprise zones tell us about how well they are achieving the goals of ORS chapter 285C and of local zone sponsors, as well as program adjustments that might be made?*

The first two questions provide an important foundation for any attempt to address the others. That is, before one can assess the enterprise-zone program or how well it's meeting legislative goals, one must first determine if the program has significantly changed outcomes.

In this memo, we[1] describe the results of our efforts to serve this purpose. In particular, we describe quantitatively how eligibility for Oregon's EZ tax incentives might be shown to affect an area's employment growth, if at all.

II. Summary of Findings

In brief, this analysis using National Establishment Time Series (NETS) data does not yield conclusive results. Using a measure of employment during 1992–2005, we find no significant differences between Oregon's enterprise zones (OEZs) and Oregon's non-EZ areas.

[1] Throughout this memo, we use "we," "our," and "us" to refer to the authors, Bryce Ward and Ed Whitelaw. See descriptions of our training and experience at www.econw.com.

That said, the limitations of the data available to examine the existing OEZ program may render it impossible in this case to isolate and detect the program's economic effects given the significant other factors that impact economic growth in an area. The following contribute to this inability to detect the effects of the program:

- Areas are not randomly assigned OEZs. As a result, one can never rule out the possibility that unaccounted differences between OEZs and non-OEZ areas obscure the effects of the OEZs. I (This problem will always remain until enterprise zones are created as a scientific experiment)
- Historically, prior to the last few years, information about EZ boundary changes and program/OEZ geography is not precisely defined, area-by-area, in quantitative/geo-coded terms.
- The NETS data on establishment employment are insufficiently accurate relative to the small share of total employment associated with OEZ tax incentives, which themselves often generate only a modest benefit for many types of business investments.
- The NETS data appear to be overwhelmed by larger economic trends and factors affecting the much vaster economy, such that figuratively, there is too much "noise" or "static" in the data to effectively hear the type of signal, for which we are listening.

Moreover, direct comparisons between OEZ and non-OEZ areas (based on "selected observables") must address the rather insurmountable challenge of defining the appropriate non-OEZ area for an effective "control group." For example, should one look at areas that have had variously "similar" statistics or at areas that could have economically qualified for an OEZ originally but never become one? Or, how does one address stark differences among designated OEZs in terms of their activity and apparent performance?

The diverse and discrete areas associated with OEZs, many of which represent rather small communities, do not lend themselves to the identification of a method that renders a robustly quantifiable baseline for what would probably have occurred if the average OEZ had never been designated.

We also employ a "differences-in-differences" method that includes before/after measures for OEZ areas to resolve some of the deficiencies of analyses based on selected observables. These efforts rendered interesting if still mixed results. Even this method must employ techniques to control for baseline effects in the overall economy, and these techniques may have significant shortcomings.

III. Oregon's Enterprise Zone Program

Our analysis focuses on the standard, 3- to 5-year, property-tax exemption on new, qualified property—buildings, improvements or machinery & equipment by traded-sector, or primarily industrial businesses—in any Oregon enterprise zone. Oregon started its enterprise zone program in 1986. Since then, Oregon has added, dropped, or changed a number of zones. Currently, there are 59 designations that the state has made under statutory authority, at the request of local governments, in addition to one tribally based designation. These tax abatements are available to eligible types of business operations that are using an OEZ, and for which the business satisfies employment and other requirements under state law, as well as in some cases, special local policies of the governments that sponsor the particular OEZ.

In theory, the typical enterprise zone program (not only in Oregon) benefits the local economy by encouraging business investment, job creation or other outcomes or changes that would not otherwise occur. Specifically, as the theory posits, the enterprise zone incentives (which take all varieties of forms around the world) could change private business firm decisions (and impact the economy, somewhat simplistically) in one of two ways:

(1) For a business firm that is outside an enterprise zone or that has operations both inside the zone and in other, non-EZ communities, the program could induce it to locate new or expanded investments inside the EZ rather than at alternative locations, or in some cases to move, relocate or concentrate investments or operations into the enterprise zone;

(2) For a firm that anyway finds itself already operating inside an EZ or looking to start up in that area, the program could induce it to invest and hire more, or more rapidly, than it would have if the EZ did not exist.

Consequently, a local economy could benefit from the availability of enterprise zone incentives if those incentives are large enough to otherwise influence business firm decisions, at least at the margin, in that they made (further) investment either sufficiently more attractive in the EZ than the alternatives, or sufficiently more desirable there than would otherwise have been the case.

Quantitatively identifying these decisions points and influences in financial or nonfinancial terms represents another avenue of very challenging research—*i.e.*, the immediate "but–for" question—which is distinct from our effort here to compare measures of the local economy to a proxy of then supposed counterfactual baseline. If there is a statistically significant difference among the compared measures, then one might reasonably infer about how the OEZ affects local economic conditions through altered business decisions.

The theoretical framework above would indicate that we should look in the first instance at changes or differences in the amount of business investment. Unfortunately, NETS data does not include measures of business or industrial capital, so we use establishment employment, which is less directly linked to the OEZ incentive, but it does relates to program requirements and objectives.

IV. Estimation Challenges and Existing Studies

Demonstrating the efficacy of enterprise zone programs in general is difficult based on local economic outcomes. Specifically, there are four hurdles that must be cleared for an evaluation to produce meaningful, interpretable results:

(1) identifying the appropriate area or unit for analysis,

(2) selecting appropriate control or comparison areas and methods to capture the local, economic baseline,

(3) distinguishing the effects of the enterprise zone from other geographically targeted policies,

(4) choosing outcomes in line with program incentives and goals.

To date, a few relevant studies of enterprise zone have tried to deal with all of these issues, though none has been completely satisfactory. As such, the quantitative evidence of EZ effectiveness is mixed and inconclusive. Among the most recent and notable studies that find improvements (to an extent) in employment or other outcomes in enterprise zone areas:[2]

- Busso and Kline (2007) compare residential employment outcomes in census tracts that became part of federal empowerment zones with outcomes in census tracts that submitted unsuccessful applications to be designated empowerment zones; they also in some cases do comparisons with areas that become parts of zones in the future. These zones were in Atlanta, Baltimore, Chicago, Detroit, New York City, Philadelphia/ Camden, Los Angeles, and Cleveland. They found that neighborhoods receiving empowerment zone designation experienced substantial improvements in labor market conditions and moderate increases in rents relative to rejected and future zones. These effects were accompanied by small changes in the demographic composition of the neighborhoods, though, they found that evidence from disaggregate Census tabulations suggests that these changes account for little of the observed improvements.

- Ham et. al. (2009) examine the effects of enterprise zones in California on employment. In California, the boundaries of enterprise zones do not follow boundaries of census tracts, zip codes, or other standard geographic designations. Instead, this study used aggregate data on census tracts, the boundaries of which correspond only approximately to those of enterprise zones. They find that all three programs have positive, statistically significant, impacts on local labor markets in terms of the unemployment rate, the poverty rate, the fraction with wage and salary income, and employment.

- O'Keefe (2004) examines the impact of California's enterprise zone program on employment growth at the census tract level and at the establishment level. EZ census tracts are matched to non-EZ census tracts using a propensity score matching model. Annual establishment level employment data are used to estimate the impact of the program on growth in employment and wages. Estimates suggest that the enterprise zone designation raises employment growth about 3 percent each year during the first six years after designation, but this effect does not persist in later years. The number of employees at each business in an enterprise zone also rises more than employment at businesses that do not have the same tax incentives.

- Papke (1993) examines the effect of enterprise zones in Indiana, one of the oldest state programs that has been evaluated with a variety of types of data. Papke uses econometric analysis to determine whether the enterprise zones have had an effect on annual level of inventories, machinery and equipment, or unemployment claims. He finds that enterprise zones appear to have a positive effect on employment and inventories, however, data from the Census indicate that the well-being of Indiana zone

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[2] Busso, M. and P. Kline (2008) "Do Local Economic Development Programs Work? Evidence from the Federal Empowerment Zone Program" Yale Economics Department Working Paper No. 36., O'Keefe (2004) "Job Creation in California's Enterprise Zones: A Comparison Using a Propensity Score Matching Model" *Journal of Urban Economics* 55(1): 131-150. Papke, L. (1994) "Tax Policy and Urban Development: Evidence from the Indiana Enterpise Zone Program." *Journal of Public Economics* 54: 37-49

residents may not have been appreciably affected.

Other studies, find no change in local economic outcomes:[3]

- Kolko and Neumark (2010) use new establishment-level data and geographic mapping methods to improve upon evaluations of the effectiveness of state enterprise zones, focusing on California's program. Because zone boundaries do not follow census tracts or zip codes, they created digitized maps of original zone boundaries and later expansions. They combine these maps with geocoded observations on most businesses located in California. The evidence indicates that enterprise zones do not increase employment. They also find no shift of employment toward the lower-wage workers targeted by enterprise zone incentives. They conclude that the program is ineffective in achieving its primary goals.

- Boarnet and Bogart (1996) study a set of municipalities in New Jersey, all of which qualified for enterprise zones. They compare those that received zones to all that qualified, and, also compare those that received zones to those that qualified and applied for zones but did not receive them. They find no evidence that the urban enterprise zone program in New Jersey had a positive effect on total municipal employment, on employment in various sectors, or on municipal property values. They conclude that the program was ineffective in achieving its goal of improving the economic conditions in and around the zones.

Still others find mixed results where one outcome is positively affected, but another is negatively affected:[4]

- Billings (2009) examines the effects of enterprise zones in Colorado on employment growth. Billings identifies the effects of Colorado's enterprise zone program by comparing employment growth just inside (within 1/4 mile) the enterprise zone boundary to employment growth just outside the boundary (again, within 1/4 mile). Billings uses digitized maps of enterprise zone boundaries and geocoded establishment locations. He finds that while EZ fiscal incentives have no effect on where new establishments locate in Colorado, they do increase the number of employees hired. Industry results highlight the heterogeneity of tax credit impacts within the EZ Program. Results are robust to a variety of specifications for land-use controls and in comparison to a propensity score matching model.

- Greenbaum and Engberg (2004) analyze urban zones in six states (California, Florida, New Jersey, New York, Pennsylvania, and Virginia), examining the factors that states use to choose zone locations and the subsequent effect of the zones on business activity and employment. Matched sample and geographic comparison groups are created to

[3] Kolko, J. and D. Neumark (2010) "Do Enterprise Zones Create Jobs? Evidence from California's Enterprise Zone Program" *Journal of Urban Economics, forthcoming*, Boarnet M and W. Bogart (1996) "Enterprise Zones and Employment: Evidence from New Jersey," *Journal of Urban Economics* 15(3): 198-215.

[4] Billings, S. (2007) "Do Enterprise Zones Work? An Analysis at the Borders" http://papers.ssrn.com/sol3/papers.cfm?abstract_id=1028810, Greenbaum, R. and J. Engberg (2004) "The Impact of State Enterprise Zones on Urban Manufacturing Establishments," *Journal of Policy Analysis and Management* 23(2): 315-339.

measure the impact of zone policy on employment, establishment, shipment, payroll, and capital spending outcomes. The difference-in-difference estimates indicate that zones have little effect, on average. However, by exploiting the establishment-level data to examine gross as well as net changes, the analysis finds that zones have a positive effect on the outcomes of new establishments and a negative effect on the outcomes of previously existing establishments.

Nearly every study of enterprise zones has advanced our understanding of data sources and methodologies in the pursuit of more clear-cut results. However, all fail to meet at least one of the challenges described above, and thus they are all relatively easy to criticize.

With the exception of Ham et al. (2009), the above studies do not include Oregon enterprise zones (OEZs) in their analyses. The profound differences in structure, geography and business incentives among enterprise zone programs make it impossible to infer or extrapolate directly about OEZ from the above investigations.

Existing work on OEZs is limited and does not provide reliable evidence about the effects of Oregon's program. Aside from our analysis, two reports have attempted to describe the effects of OEZs—Hall, et al. (2007) was prepared for the Special Districts Association of Oregon (SDAO), and the Legislative Revenue Office (LRO) produced a study earlier this year (without special funding).[5]

The LRO study comes close to meeting some of the challenges described above. It employs four years of geo-coded establishment data on employment, as well as socioeconomic census tract data for 1980-2000. As for OEZ outcomes relative to comparison areas, it demonstrates a mixture of both positive and inconclusive findings, but it draws from a limited sample of OEZ areas and years, and lacks any tests for the statistical significance of its findings. As such, it provides some useful insights into the particulars of OEZ effects and baseline identification, but it does not firmly establish benefits to the local economy.

The SDAO report fails to rigorously address the effects of the OEZ program. The authors do compare the overall change in total employment (not rates of growth) between 1990 and 2000 in cities with OEZs to cities without OEZs. They find larger employment changes in cities without OEZs during the 1990s than in the few places that had an OEZ throughout the 1990s.[6] As the authors themselves note, the disparity in total employment among cities may reflect factors other than OEZs. The authors do not attempt to statistically account for any of these factors, besides not even controlling for differences in urban annexations or the sheer magnitude of population differences, but even if they had, it is unlikely that a cross-city comparison would yield meaningful estimates of the effects of the incentives.

[5] Hall, J., A. Rufalo Rufolo, and H. Dong (2007) "Oregon Enterprise Zone Tax Abatement Economic Development Study and Urban Renewal Study" and Oregon Legislative Revenue Office (2009) "Enterprise Zones Study" Number 4-09.

[6] They exclude omitted several cities that added or lost enterprise zones during the 1990s.

V. Strategy for Identifying the Effects of Oregon's Enterprise Zones

As stated in section IV, estimating the effects of Oregon's EZ tax incentives is challenging. Researchers attempt to identify the effect of enterprise zone incentives on economic activity by comparing economic activity in enterprise zone areas to the economic activity in similar non-enterprise zone areas. If one believes that the non-enterprise zone areas and non-enterprise zone firms used in this comparison reflect the outcomes that would have occurred within the enterprise zone areas and enterprise zone firms *but for* the availability of the incentives, then the observed differences describe the effect of these incentives.

Two key components of any empirical study are the data examined and the strategy used to identify the effects of interest. In this section, we discuss each of these components in more detail.

A. Data

Executing the identification strategy described below requires two important pieces of data. First, we need accurate and reliable information for the outcomes of interest. Second, we need accurate measurement of the economic activity inside and outside of enterprise zone areas.

In this analysis, we focus on growth in employment. While this outcome does not directly capture the capital investments that the EZ program directly incents, it does reflect the key objectives of the program—business recruitment and expansion to boost employment (and other local economic outcomes). We measure it using data from the National Establishment Time Series (NETS).[7]

The data in NETS come from Dun & Bradstreet. They track several key variables like employment, total sales, and industry for every establishment (any standalone entity of a particular firm) in the country annually since the early 1990s. It is important to note that while Dun & Bradstreet try and obtain accurate data for all establishments, this is clearly very difficult. Employment for as much as 25 percent of the establishments, therefore, is based on estimates from the dataset creators, to make up for non-responses.

In general, the employment estimates in the NETS are highly correlated with other employment estimates. For instance, the correlation between total employment by industry (2-digit SIC) at the county level in the NETS to the same measure in ES-202 (covered wage) data exceeds 0.9 and the median difference is fewer than 30 employees. In general, the NETS employment numbers are higher than ES-202 numbers. This is likely because the NETS data includes non-wage and salary employees who are not included in covered employment data of ES-202 (although it is possible that the differences reflect other factors as well).

It should be noted, though, that there is a fair amount of imprecision in the NETS employment reports. When responding to the Dun and Bradstreet survey many (although certainly not all) employers round their employment totals to the nearest 5 or 0. Furthermore, some employers

[7] A full description of the data – including comparison to several of the other establishment level panel datasets -- is available in Neumark, D, J. Zhang, and B. Wall (2005) "Employment Dynamics and Business Relocation: New Evidence from the National Establishment Time Series" *NBER Working Paper 11647*.

fail to respond to the survey in some years. When this occurs, Dun and Bradstreet and Wall Associates (the compilers of the NETS database) impute establishments' employment.

The imprecision of the NETS employment data may affect the results this analysis. The small size of Oregon enterprise zone activity relative to the sheer size of the overall economy may make it difficult to observe enterprise zone effects. If enterprise zones have effects that are small relative to the measurement error in the data, then whatever effects they produce may be swamped by the imprecision of the data.

The advantages of the NETS data for use in undertaking this evaluation, despite the potentially fatal flaws described above, are as follows:

(1) The data was readily available at no extra cost through the Oregon Business Development Department (OBDD), which had already acquired them.

(2) The data are geo-coded – that is they contain a generally accurate estimate for the latitude and longitude for each establishment. This addresses the second major data requirement for this type of evaluation – accurate descriptions of activity within OEZ areas and control areas. Since OEZs do not follow existing geographic boundaries like city boundaries or census tracts, we use GIS software to reconstruct economic activity within the OEZ areas.

(3) The data are in annual series, going back a number of years.

OBDD provided GIS maps of the current boundaries for Oregon's EZs, as well as raw maps of the boundaries in 1994. We used these raw maps to create GIS maps for the earlier boundaries. We placed all establishments within a grid that divided the entire state into one-square-mile cells. We identified all grid cells within the 1994 EZ boundaries, the 2009 boundaries, or a half-mile of these boundaries. Cells only partially contained within these groups were split so that only the part of the grid in each group was included with the group. Combined with information on the timing of EZ designations and modifications, these data allow us to execute the identification strategy we describe above.

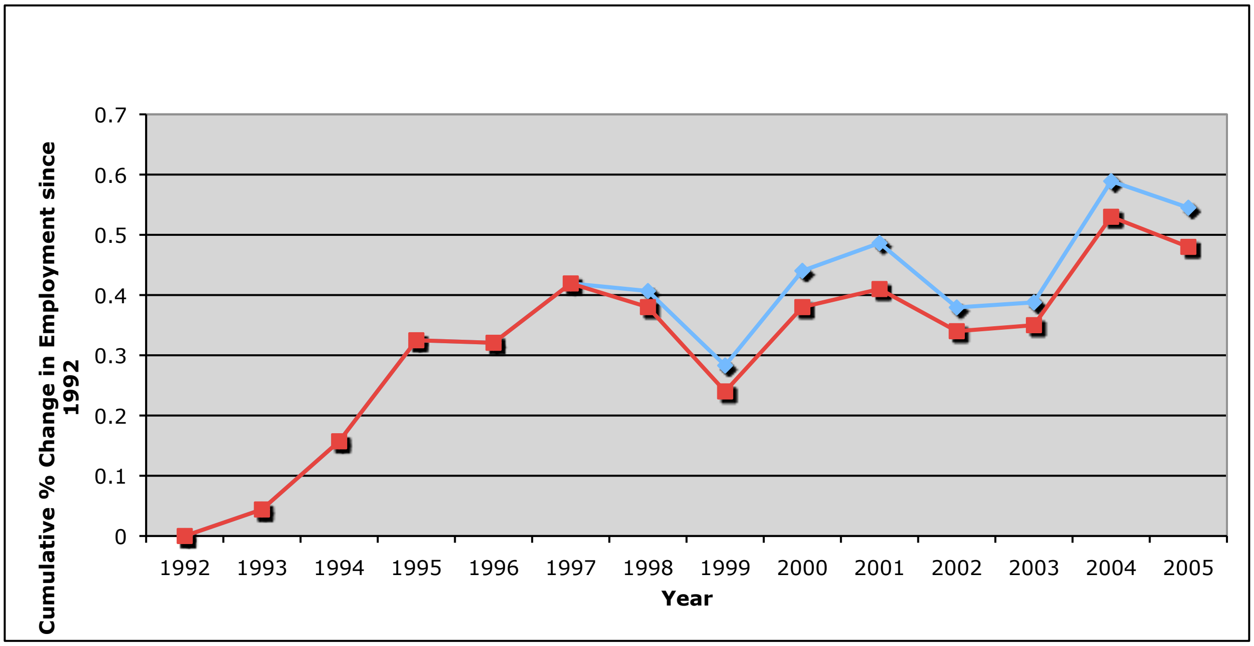
As with the outcomes, there is some imprecision in these data and this process. First, we observe only two sets of boundaries (1994 and 2009). Since EZ boundaries change overtime, we cannot place each establishment precisely within the zone in each year. For zones that significantly change boundaries between 1994 and 2005, we switch between the 1994 boundary and the 2009 boundary based on a description of the timing of major boundary changes provided by OBDD. When a zone had several large changes our colleagues and we selected the year that appeared to make the most significant change in the borders from 1994 to 2009. Second, the latitudinal and longitudinal measures in the NETS are not perfect. For example, they are not precise enough to determine which side of a street an establishment is on, so there may be some imprecision around OEZ borders. Also, for some establishments, the latitudinal and longitudinal measures are pegged to the zipcode centroid and not the actual physical location of the establishment.

Finally, we linked each gridcell to its primary census tract and added information about the characteristics of each area from the 1990 and 2000 Censuses.

B. Identification Strategy

In theory, estimating enterprise zones effects entails a simple subtraction problem. One identifies the effects of an enterprise zone by subtracting the employment growth that would have occurred in an area in the absence of enterprise zone incentives from the actual growth observed in the area. Figure 1 provides a visualization of this process. The blue line depicts what happens in an enterprise zone area, while the red line represents what would have happened in this area without an enterprise zone. The gap between these two lines represents the effect of the enterprise zone.

Figure 1. Cumulative Percentage Change in Employment in an Area With and Without Being Designated as an OEZ.



In practice, though, figuring out the red line for each enterprise zone area is extremely challenging. Researchers investigating the effects of enterprise zone incentives typically have relied on one or more of the following three methods to identify an appropriate comparison group:

(1) Compare enterprise zones to non-enterprise zones deemed most similar using observable data

(2) Compare enterprise zones to themselves or adjacent areas

(3) Compare enterprise zones to areas that applied for enterprise zone designation but did not receive it

Each of these methods might yield comparison groups that are the same except for the enterprise zone designation, but they might not. As such, any evaluations of enterprise zones based on these methods may produce flawed or inaccurate results.

We employ variants of the first two methods to investigate the effects of OEZs on employment. To our knowledge, method (3) is not feasible in Oregon; very few communities have ever sought an OEZ, been denied, and not ever been later designated as an OEZ or amended into another designation.

Within the broad categories discussed above, a number of methods might yield reasonable estimates. The simplest approach assumes that in the absence of the enterprise zone program, OEZ areas would have grown at the same rate as the state. The simplicity of this approach has some appeal, and it might work well for some areas. However, areas across the state differ wildly in their economic characteristics and local economic policies. For instance, there are many economic characteristics that vary across the state such as type of industries available, access to various modes of shipping/transportation, population, local economic policy including tax policies and public services, the skills/characteristics of a particular area's workforce, or access to natural resources. As such, many areas grow at rates that differ from the state for reasons that go beyond enterprise zones.

Figure 2. Cumulative Employment Growth Since 1992 for Ten Randomly Selected Areas

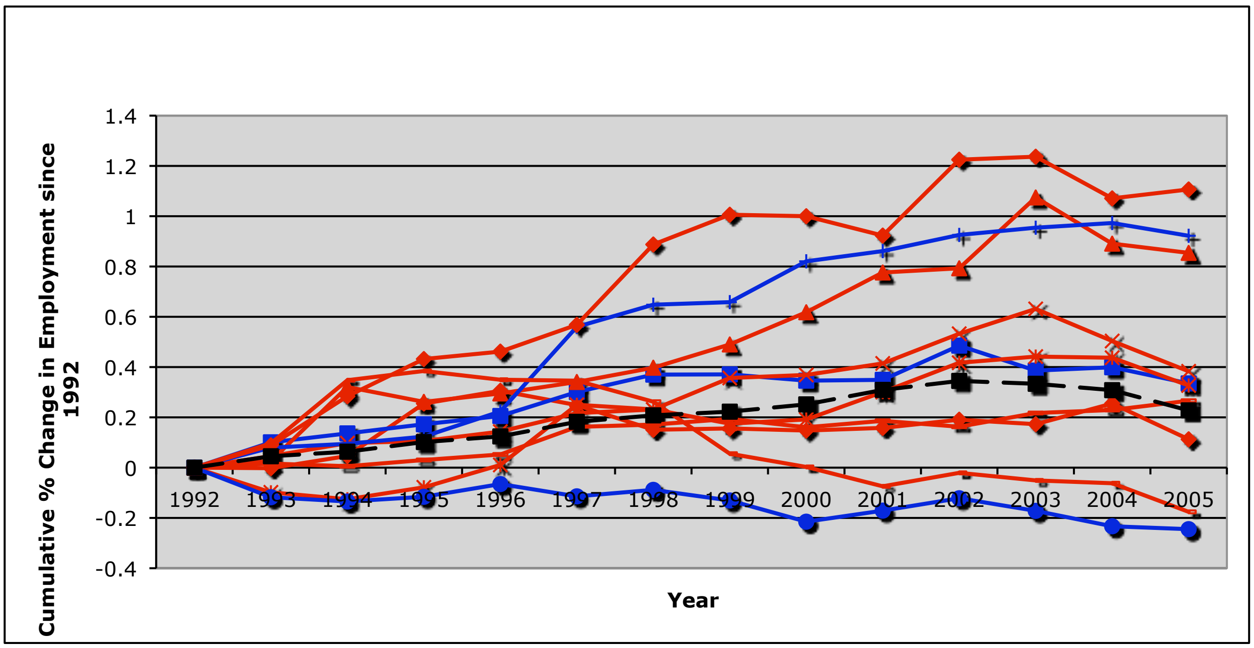
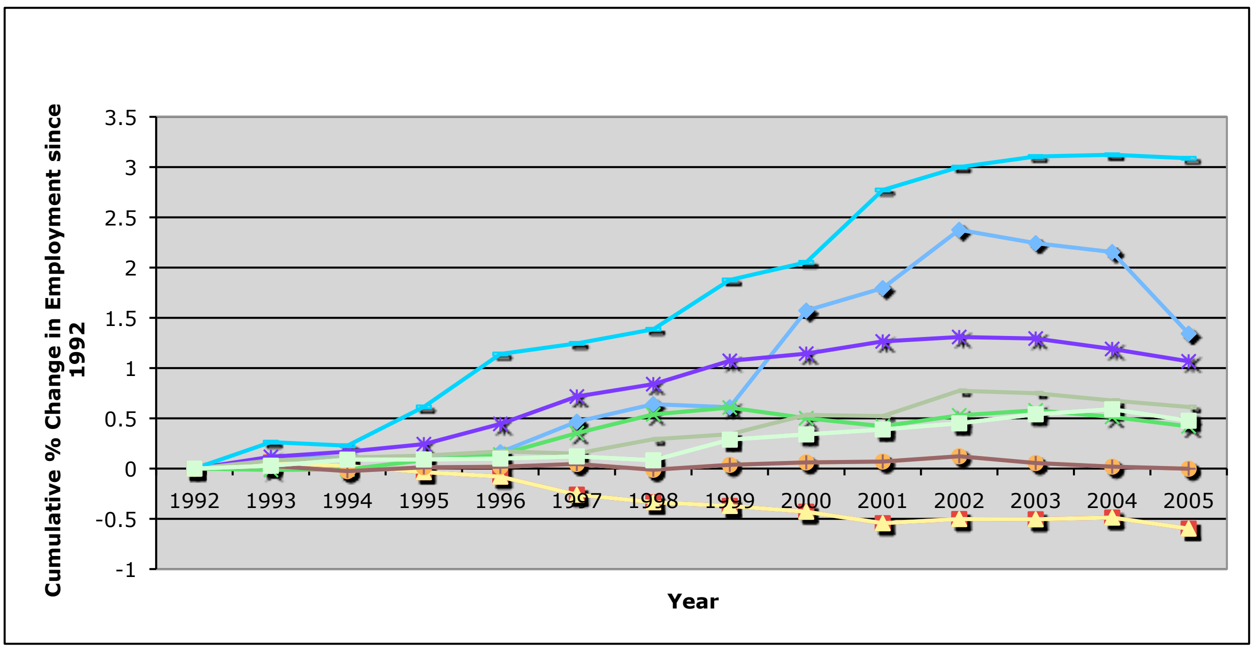


Figure 2 helps illustrate the variation in growth patterns across the state. This figure depicts the cumulative employment growth from the base year of 1992 until the end of our data in 2005 for 10 randomly selected areas. The blue lines depict enterprise zone areas, and the red lines depict census tracts that do not contain enterprise zones. The black dashed line depicts the overall state trend. As the figure makes clear, growth rates vary widely across areas and years. Thus, we believe it is unwise to assume that OEZ areas would have grown at the same rate as the state in the absence of the enterprise zone designation. An analysis based on this approach would attribute all differences between state trends and the OEZ trends to the enterprise zone program. Given that growth rates in many OEZ areas lag the state growth trend, this simple approach would support a conclusion that, for some areas, OEZs actually depress employment growth. We cannot think of an explanation for why the OEZ program might substantially reduce employment growth. Thus, this approach is unlikely to yield highly accurate estimates of OEZ effects.

A more complicated approach entails comparing OEZ areas to other areas with similar economic characteristics and local policies. In theory, this approach should yield a suitable comparison. In practice, though, the set of potentially important variables is enormous, and the data available describe only a small subset. Even if data existed that accurately described a greater number of local characteristics, the number of potential combinations of characteristics is so large that it can be difficult to identify reasonably similar areas.

Figure 3 helps to illustrate these problems. Similar to Figure 2, Figure 3 presents the employment growth trajectories for 10 statistically similar areas. That is, for these areas the values of various variables in the NETS and the Census that we included are fairly close. As with Figure 2, in this figure, we see that growth trends vary wildly. Based on this variation, we believe it may be unwise to assume that any observed differences between OEZ employment growth and employment growth in "statistically similar" non-OEZ areas reflect the effects of the enterprise zone incentives. Other factors, not accounted for in the data assembled for this analysis, likely play a significant role in determining these economic trends. For instance, the differences in growth between these "observably similar" areas may reflect a boom or bust in an industry that is important to a particular region, may be due to local economic policy, due to access to certain forms of capital, or a difference in public services.

Figure 3. Employment Growth Trajectories for 10 Statistically Similar Areas

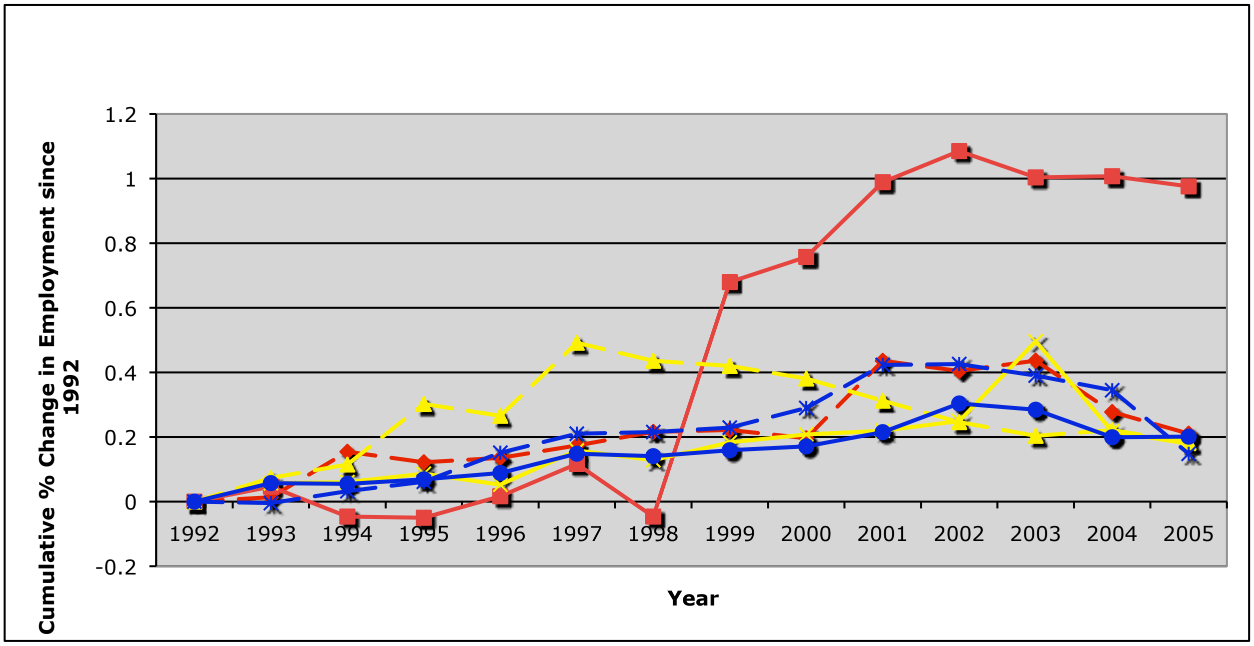


One approach commonly used to try and mitigate the effects of such unobserved factors involves comparing enterprise zone areas to areas very close to the enterprise zone. This approach is based on the assumption that many of the characteristics that are not included in the data are held constant across short distances. For instance, areas within the same community have the same local public goods and services, face the same local taxes, and draw from the same labor pool. As such, observed differences between enterprise zone areas and neighboring comparison areas are unlikely to reflect differences that remain fairly constant across space.

While this approach may account for some of the relevant unobserved characteristics, it may not account for them all. It is possible that economic conditions vary significantly across relatively short distances. If OEZ boundaries are drawn so that they encompass all of the areas with certain characteristics within a community, then the remaining nearby areas may be substantially different and thus provide an imperfect comparison.

Figure 4 helps illustrate these concerns. Similar to the previous figures, Figure 4 presents employment growth in selected OEZs (solid lines) and the areas adjacent to them (dashed lines). The figure shows that while areas adjacent to OEZs may have many similar characteristics, there can still be stark differences in their growth patterns.

Figure 4. Employment Growth in Selected OEZs and Adjacent Areas



In theory, it is possible to mitigate some of these concerns by further limiting the area examined to include only areas that are part of an enterprise zone at some point.[8] The intuition for this approach is that areas that were ever included in an enterprise zone should be fairly similar to the other parts of the zone (because it had to satisfy the criteria for inclusion). As such, areas that are added to or subtracted from an enterprise zone may offer a reasonable approximation for what would have happened in the rest of the enterprise zone during the times when they were not included. The major assumption underlying this approach is that the areas that are added or subtracted from a zone are, in fact, comparable to the rest of the zone throughout the entire period. This assumption may be invalid. The fact that these areas are not always included suggests that they may be different than the "core" enterprise zone area. It is also possible that some event (e.g., new development or rezoning) triggers or coincides with the change in the enterprise zone. If there are important differences between these marginal areas and the core enterprise zone area, then this type of comparison may not yield accurate results.

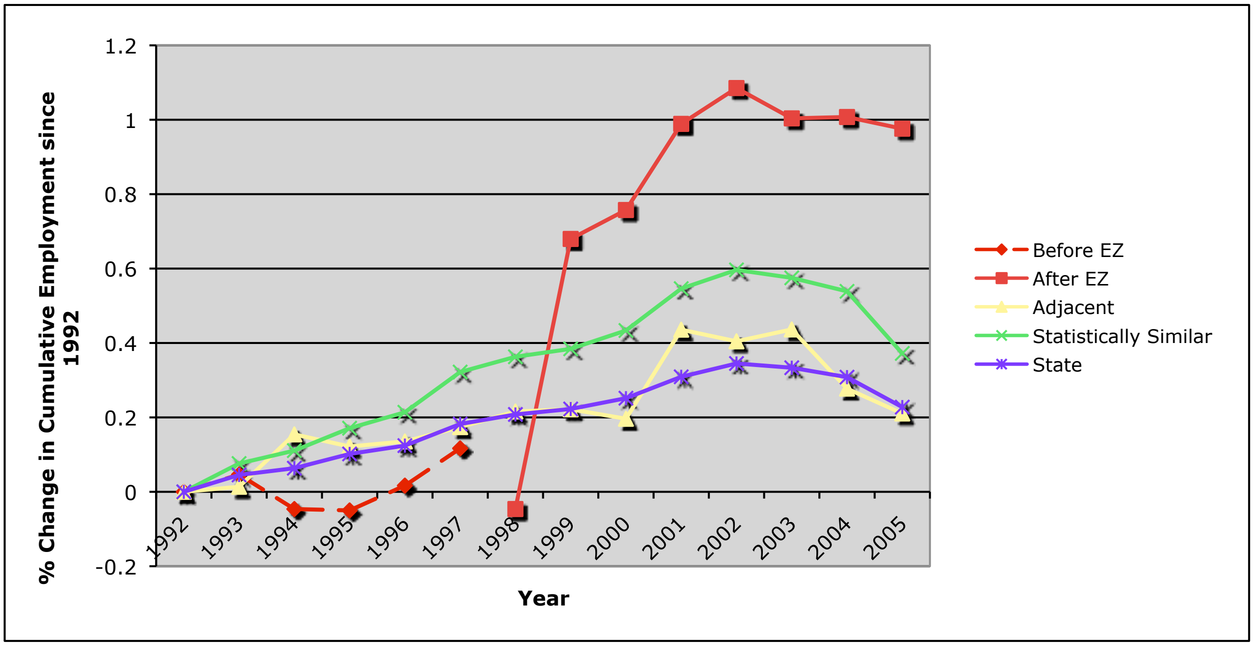
We do include this type of comparison in our analysis, but it may be further compromised by the data available. First, it is relatively uncommon for zones to make significant changes to their boundaries. Second, as noted in the data section, we do not have precise OEZ boundaries

[8] This is one of the methods used in Kolko & Neumark (2010)

for all zones in all years. We have only two years of zone shape files and a list of when zones experienced major additions or subtractions. Thus our ability to identify zone effects by comparing employment growth in zone areas to growth in areas that, at some point, are part of the enterprise zone is limited.

Figure 5 puts all of these different comparisons (with the exception of the last one) on a single graph for one enterprise zone – the Grant's Pass zone. The Grant's Pass zone was designated in 1997, during the period examined. As such, it helps to illustrate the final two aspects of our identification strategy – before and after comparisons and differences-in-differences.

Figure 5. Scenario Comparison for the Grant's Pass Enterprise Zone



The before-and-after approach to identifying the effects of enterprise zones entails simply assuming that, in the absence of the enterprise zone designation, the zone area would have grown at the same rate it grew in the years prior to designation. As Figure 5 and the figures above indicate, areas seldom grow along simple paths. They frequently bounce up and down due to both larger macroeconomic shocks and idiosyncratic economic shocks to this particular area or type of area. Thus, it is unlikely that a simple before and after comparison will be highly informative.

The differences-in-differences approach is designed to help factor out the effects of these shocks (or events) that cause employment growth to vary around its trend. The approach rests on the assumption that the comparison area (whichever one is used) and the enterprise zone area would follow the same trend in the absence of the enterprise zone designation. As such, they should respond to the same economic events in roughly the same way at the same time. The key addition of the differences-in-differences approach is the ability to compare the enterprise zone area to the comparison area prior to the enterprise zone intervention, and factor out any pre-existing differences (on the assumption that differences that existed prior to the intervention will persist after the intervention).

Figure 5 shows an example that is as close to ideal as we might expect to find. But even then, it is far from perfect. The trendlines for the various comparison areas move roughly together.

There are still differences, though, between the comparison trend lines and the "Before EZ" trend lines. The closest approximation is the adjacent area, which, while having different levels, shows the same increase and dip pattern. After designation we see a large increase although it is still possible that this increase is not entirely attributable to the designation of the enterprise zone. It is also possible that this increase will not be sustained over the long run. Depictions of the same lines for other areas show wildly different growth patterns for the comparison areas and enterprise zone (as one might infer from the previous figures). It is possibly, perhaps even likely, that these comparison areas may not provide a reasonable estimate of what would have happened to employment growth in the enterprise zone area in the absence of the enterprise zone intervention – at least for this one example.

VI. RESULTS

In this section, we describe selected results from regression analyses based on the intuition of the various analyses described above. Perhaps unsurprisingly, given the long list of both data and identification problems described in the previous section, the analyses we perform do not yield convincing results.

Table 1 presents results based on regression analyses that compare annual employment growth in areas with enterprise zones to observationally similar areas without enterprise zones. These analyses are similar that described by Figure 3. We report the results for 6 separate analyses. The first analysis (top left corner) describes the results of a regression that compares areas with enterprise zones to areas without enterprise zones controlling for area characteristics in the early 1990s.[9] This analysis finds a small (0.3%), positive, but highly statistically insignificant effect of enterprise zones. The second column performs a similar analysis (i.e., one that controls for observable characteristics), but restricts the set of areas examined to include only areas that could be enterprise zones with a reasonably high probability.[10] This analysis finds a small (-0.2%), negative and highly statistically insignificant result.

[9] Specifically, we control for population characteristics (the log of otal population, share white, share foreign born, share using public transportation, unemployment rate, poverty rate, the log of average household income, population density, and the share of owner occupied housing) using 1990 census tract data. We also control for initial economic conditions using the NETS (including controls for the share of employment in manufacturing and the share of employment at small firms). Finally, we include controls for the year and for whether or not the area is within an urban growth boundary.

[10] We identify the set of areas included by predicting the probability that an area could be an enterprise zone. We then examined the predicted probabilities for areas that actually are enterprise zones, and limited the analysis to include only areas whose predicted probabilities were above the median for actual enterprise zone areas.

Table 1. Results from cross-area (OLS) analysis

	All Areas, with Controls	EZ Areas compared to likely EZ eligible areas, with controls
All OEZs compared to similar areas	0.3%	-0.2%
High Implementation EZs Only	0.3%	-0.3%
Eligible Industries Only	2.4%	0.4%

Notes:
Results from separate OLS regressions (see footnote 11 for list of control variables.)
Bold = statistically significant at the 10% level (or higher)
Combining restrictions (e.g., high implementation and eligible industries) does not yield different results

The second row of Table 1 presents results for two analyses similar to the first row, but in these analyses we limit the set of enterprise zone areas to include only areas that were deemed to have a high level of implementation and use. The results of these analyses are nearly identical to the first analysis.

Finally, the third row of table two presents results for analyses that are very similar to the first two, but we examine employment growth only in the industries that are eligible to receive enterprise zone incentives. Here we find a more noticeable enterprise zone effect (2.4%) in the first column, but it is still statistically insignificant. Furthermore, the effect nearly disappears when we restrict the analysis to the areas most likely to qualify for OEZ incentives.

These results are quite muddled. The results are typically fairly small, but they are highly statistically insignificant and thus the measured effects could be positive or negative. As such, these results do not show evidence of enterprise zone effects. This may indicate that enterprise zone incentives do not measurably affect employment growth, but, perhaps more likely, these results reflect the limitations of the data and identification strategy described in the previous section. That is, the combination of potentially imprecise employment data and the failure to account for a potentially long list of other factors that affect employment growth that vary across places likely swamped our ability to detect any OEZ effects.

As such, it is necessary to try and account for some of these currently unaccounted for differences across space. Without extensive additional data that describes all of the potential differences across space, the only hope for overcoming the problems caused by these differences entails making more narrow geographic comparisons. One simple approach would entail adding geographic control variables (e.g., dummy variables for each county or otherwise defined small geographic area) to the analysis described above. Adding such control variables would mean that each enterprise zone would be compared to observably similar areas within the same county. Adding county variables to the above analysis slightly increases the coefficients in Table 1, but does not meaningfully change the results.

This leaves comparing EZ areas to themselves, before and after enterprise zone designation as our last hope for finding evidence of enterprise zone effects using these data. Many of Oregon's EZs were created or modified during the period examined. This creates the opportunity to conduct before and after analyses for a non-trivial number of EZs. Nevertheless, all but four of the newly designated zones saw little or no activity, and few only very briefly existed during the 1992–2005 period, before or after designation.

Comparing an area before and after designation eliminates some of the problems that plague the analysis above. When conducting a before and after analysis, researchers do not have to worry that unobserved differences in area characteristics (*e.g.*, local amenities, core economic conditions, basic demographics, etc) are affecting the results because these characteristics remain the constant throughout the period examined. However, it is still possible that over very short distances that conditions that effect employment growth change, and, as such, narrowing the geographic focus may not eliminate the effects of local conditions.

Even if all geographic concerns are eliminated, researchers still have to worry about determinants of employment growth that may change over time. If local economic conditions change over time and if the changes in enterprise zones differ from those elsewhere, it becomes difficult to separate the effects of the introduction of EZ incentives from the effects of other economic changes.

As such, when conducting before and after comparisons, researchers try and identify and account for important events that affect employment growth trends. This leads back to the need to find some set of areas outside the enterprise zone that are likely to be affected by the same events (with roughly the same magnitudes). We try four different approaches. These results are described in Table 2.

Table 2 – Differences-in-differences estimates of EZone effects

	Annual Employment Growth		
All	**0.7%**	-0.4%	-0.2%
High Implementation Enterprise Zones Only	**0.9%**	-0.1%	0.2%
Eligible Industries Only	**1.3%**	-0.4%	-1.5%
High Propensity Control Areas Only	-0.2%	-0.2%	-0.2%
Specification of Year-to-Year Shock Effects of Trend Line	All areas assumed to follow statewide trend	All areas assumed to follow same trend as average of all OEZ areas	Each EZ area has own specific shocks beased on adjacent areas

Notes:
Results from separate OLS regressions (see footnote 4 for list of control variables.)
Results expressed in percentage terms (i.e. the dependent variables in the regressions are in logs)
Bold = statistically significant at the 10% level (or higher)
Combining restrictions (e.g., high implementation and eligible industries) does not change the results

First, we assume that each OEZ area is affected by the same events or shocks as the entire state economy. Applying this approach to the NETS data, we find positive and significant EZ effects (described in the first column of results in Table 2). When we assume that statewide shocks adequately reflect the shocks occurring overtime in all EZ areas, we find that EZs increase employment growth by approximately 1 percentage point per year. Focusing only on high implementation areas and eligible industries, we observe the same result.

Unfortunately, these results depend on using all (or at least most) of the state to estimate local economic shocks. Restricting the data to include only enterprise zone areas (so the shocks or events accounted for reflect those occurring across all Oregon enterprise zones) eliminates the positive and statistically significant results. Finally, assuming that the shocks or events that affect employment growth within each enterprise zone are best captured by examining the areas within ½ mile of the enterprise zone boundary or by the areas that switch into or out of the zone (which amounts to allowing for separate shocks to affect each enterprise zone or local area) also eliminates the positive effects described above.

The lack of robust results – that is, a consistent pattern of results across reasonable specifications – suggests that these data and this approach do not show that Oregon's EZs significantly increase employment growth.

VII. Interpretation and Limitations

In this section, we explore some of the potential weaknesses with our analysis. Specifically, we start with the hypothesis that Oregon's EZ incentives do, in fact, affect employment growth, and we assess the likelihood that some weakness in our analysis is preventing us from finding this result.

If positive effects are being obscured, the most likely explanations for why they are not observed fall into the two categories we describe in section IV – data and identification. As mentioned in the data section, there is some measurement error in both the employment measures and identification of EZ eligible establishments. Typically, measurement error does not bias results (because there is no reason to expect that establishments entering EZs are more likely to be affected by these problems than the other establishments analyzed). Furthermore, analyses that restrict the included establishments to eliminate establishments with less reliable employment estimates, does not yield substantially different results.[11]

If total OEZ activity represented a substantial slice of the overall economy, then this measurement error may not matter. However, direct hiring in the OEZs are on the order of less that 1 percent of overall employment, affecting a couple hundred establishments among tens of thousand of businesses statewide at any given time. As such, the OEZ program is not only targeted geographically at communities, many of which face remoteness and other challenges that are not easily overcome, but it is also only meaningful for particular kinds of business investments in terms of program criteria, as well as the significance of temporary property tax relief new plant & equipment. Such investments may, however, have the advantage of not directly impacting the cost of public services relative to the temporary period of tax exemption, in comparison to other forms of development and the longer-run taxation of the property.

Regarding identification, there are two key questions: (1) Are existing EZ areas or control areas inappropriate comparison areas because they are fundamentally different than the areas that become EZs; and (2) Is it likely that concurrent with areas entering EZs negative shocks occur (or that positive shocks strike only control areas) that obscure the effects of the EZ incentives?

The analyses described above attempt to account for such differences by restricting the sample to areas to places that are more obviously comparable. However, unless we had a large sample of areas that were randomly assigned EZ status, we cannot rule out the possibility that some unobserved changes in the trajectory of growth are obscuring potentially positive effects of EZs. For instance, perhaps some other local development policy is consistently implemented in the comparison areas at roughly the same time as EZ changes. Alternatively, perhaps EZs are granted to areas enjoying slightly above average growth, but after implementation these areas revert toward their long-term averages making it appear that zones have little (or even negative) effects. A third possibility is that the effects on EZs on employment spillover into nearby areas. If the EZ increases employment outside the zone, then some of the potential effects of the EZ will be obscured.

Finally, it is also possible that the size of the program is too small relative to the rest of the economy to generate effects that can be detected using this type of aggregate analysis. Most

[11] Specifically, we exclude establishments where Dun and Bradstreet or Walls & Associates estimated employment.

firms do not qualify for and do not use EZ incentives. Thus, the magnitude of direct impacts on employment is likely small relative to the entire economy. As such, it is perhaps not surprising that larger economic trends would simply obscure the effects of the program.

VIII. Conclusions and Other Geo-Coded Data Sources

Our analysis of Oregon's EZ tax incentives using NETS data does not find evidence that the program significantly affects the growth rates of employment or establishments. However, the lack of complete and precise data on employment and EZ implementations, the fact that areas are not randomly assigned to EZs, and the small size of the program relative to the economy precludes us from concluding that these results suggest that Oregon's EZs do not have any effects on local employment growth. We simply cannot ascertain the true effect of the program given the way the program was implemented and the data available.

Why NETS?

The choice was made in attempting the analysis here to use National Establishment Time Series (NETS), which arises from collaboration between Walls & Associates in Oakland, Calif., and Dun and Bradstreet (D&B). The NETS longitudinal database offered several advantages:

- Analyzable relative to other Geographic Information System (GIS) files—*i.e.*, data are geo-coded—like what are newly available for most current OEZ boundaries.
- The data are annual, which improves prospects for meaningful evaluation by substantially increasing the number of observations for each area.
- The data-set covers a substantial number of years: 14, 1992–2005, even if this excludes more than half the OEZs that are older or more recent, and most of the active OEZs.
- The data are readily available to nongovernmental researchers, because there are no confidentiality restrictions.
- The data was free of charge through OBDD.
- The tracking and coverage of business establishments is superior in terms of comprehensiveness and specificity by industry and location, because NETS data are collected primarily for marketing purposes.

For research endeavors, however, such as this one to evaluate OEZs, the employment data in NETS database was not up to the task due to its imprecision, as concluded above.

For future research & analysis along these lines, the following are other sources of geo-coded, longitudinal employment data:

Longitudinal Business Database (LBD)

This census dataset is confidential and can only be accessed at a CES Research Center (closest is Berkeley) after approval of a given project is received from the Bureau, which can take 6 to 9 months and may include fees for use of the research center. Provides data for all employer establishments in the Census Bureau's business register, the Standard Statistical Establishment List. Currently, the LBD contains the universe of all U.S. business establishments with paid employees from 1976 to 1999. Includes all employer establishments that are in scope for the Economic Census as well as a large number of public administration and other out of scope entities. The LBD contains basic information on establishment size, industry, location and ownership that permit researchers to analyze establishment demographics. Since the data includes Invaluable to researchers interested in entry and exit, gross job flows and changes in the structure of the U.S. economy. There is a 5-year cycle of quality related to the Census conducting an Economic Census. Limited data on the over 23 million establishments that can be easily linked to firm and establishment data from the Census and other sources

U.S. Census Bureau's Statistics of U.S. Businesses and Business Information Tracking Series

This source improves on the decennial statistics, in that like NETS the data: (1) Focus on the specific establishment level, and (2) Are updated annually.

The problem is that to protect the confidentiality of the businesses responding to census surveys, access is restricted to persons with special government certification, who must travel to a secure site, of which the nearest one is in Berkeley, California. The travel and time needed to work effectively with these data makes this source all but unavailable.

State Employment Department Covered Payroll File

Derived from unemployment insurance forms, annual employment data from this source would again be quite precise. This source produced interesting results in LRO (2009), but it has two drawbacks: (1) Access must be arranged with employment department staff, which were heavily taxed upon for the LRO (2009) study, and (2) The number of years for which the data are in a geo-coded format is very limited, going from 2003 to about 2007 at this point.

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APPENDIX

Attached is a spreadsheet containing various characteristics of the enterprise zones (EZs). The file, entitled "EZone Description.xls," contains annual information from 1990-2005 for each EZ.

Employment and establishment counts from the NETS data were aggregated using the 2009 EZ boundaries. As such, the values for each year reflect the employment, etc. at all establishments within the 2009 EZ boundary regardless of whether or not these areas were in the EZ during that year

Share of employment in EZ eligible industries captures the share of total EZ area employment in industries eligible for EZ incentives (manufacturing with a bit of warehousing, wholesale trade, and a few other small industries).

Columns M-V include census tract data from the 1990 and 2000 Censuses. If an EZ includes more than one tract, the results reflect the average across the included tracts (weighted by the number of establishments in each tract).

We use the 2009 boundaries to determine the establishments and census tracts that are part of each zone.