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An Evaluation of Colorado's Enterprise Zone Program: Measuring the Impact on Establishment-Level Employment and Earnings per Worker

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Abstract

Since the early 1980s, most states have implemented enterprise zones. This paper examines the impact of the Colorado enterprise zone program on employment and earnings per worker for 73,008 establishments, controlling for establishment and county characteristics. Tobits reveal that enterprise zone designation has positive effects on employment and earnings. Perhaps surprisingly, these effects occur only in rural areas. Consistent with previous research, enterprise zones appear to have no impact in the manufacturing industry. Moreover, the only significant effect observed in urban areas is a negative impact in the Denver County enterprise zone. This is of interest since the urban areas contain most of the target populations.

Key word: Enterprise Zone (EZ)

1. Introduction

An Enterprise Zone (EZ) may be defined as an economically depressed geographic area where certain tax preferences are allocated to capital and/or labour in an effort to induce investment and enhance employment opportunities. The idea of the EZ has been attributed to a few British politicians and academics who were inspired by the success of some East Asian economies in the 1970s. These economies, it was felt, were vibrant because of the heavy involvement of the private sector relative to that of government. (Peters and Fisher; 2002 pg. 24) The British government implemented a ten-year EZ program in 1981.

In the U.S., Enterprise Zone Programs (EZPs) have been used as an economic development tool since the early 1980s. To date, according to Peters and Fisher (2002), approximately forty states as well as the District of Columbia have implemented an EZP in one form or another. While these programs vary in specifics they are all aimed at stimulating economic development in depressed areas.

The criteria for EZ designation vary from state to state. However, designation is generally given to areas characterized by:

- high unemployment;
- low population growth and/or substantial population decline;
- low per capita income relative to the state's average.

Some of the tax incentives offered in the zones include investment tax credits, credits for new jobs and credit for property taxes. By offering these incentives, it is hoped that some businesses operating outside an EZ will relocate at least a part of their production to an EZ, or that existing firms will expand and new firms will begin

operating within a zone. It is hoped that with increasing investment, employment growth will be stimulated, factor income should increase, and overall, the economic blight evident in these areas will be removed.

The widespread use of EZs as an economic development tool and the diversity of EZ's policies across states provide a compelling reason to evaluate the effectiveness of these programs. As such, whether or not an EZ is achieving the intended goal of stimulating economic growth in depressed communities needs to be addressed. Thus, this paper analyses the impact of EZP on establishment-level employment and earnings per worker (a proxy for wage) for the case of Colorado.

2. Literature Review

Before Papke (1994), most of the early research evaluating EZPs in the U.S. consisted of case studies or surveys of zone administrators and participating businesses. While informative, these surveys were inadequate in addressing the question of whether or not EZ designation improved economic conditions in and around the zones. The surveys typically ask zone administrators about the incentives offered, the number and type of businesses taking advantage of these incentives, as well as the number and types of jobs created through investment made in an EZ.

The problem with surveys is that zone administrators and business people are not always objective in their analysis, especially if they have stakes in the outcome. Furthermore, surveys suffer from sample selection bias since only participating firms are included. Therefore, firms that leave the zones and firms that did not locate in a zone are not taken into account. Given the shortcomings of case studies and surveys, a number of

economists have taken on the task of analysing the effectiveness of EZPs using econometric techniques.

Papke (1994) analysed the effectiveness of Indiana's EZP on investment and employment. Using a panel of local jurisdictions in both zones and non-zones, she addressed the question of whether or not zone designation improved economic conditions in and around the zones. Indiana's investment tax credit targeted both labour and capital. However, the most valuable incentive from a tax-saving standpoint is a tax credit equal to 100 percent of the property tax imposed on all inventories located within an EZ. Therefore, Papke's analysis of investment was broken down into the effect on machinery and equipment, and the effect on inventories. She concluded that EZ designation resulted in an 8 percent increase in the value of inventories and that there was a 19 percent reduction in unemployment claims. However, the value of machinery and equipment fell by approximately 13 percent.

Using Papke's methodology, Boarnet and Bogart (1996) analysed the effects of EZ designation in the state of New Jersey. The variables evaluated were employment and property value. Using data at the municipal level from 1982 to 1990, Boarnet and Bogart used an experimental approach in which the municipalities designated EZs were the treatment group. They used two control groups; one group called Qualifiers and the other called Applicants. The applicants are a subset of the qualifiers. The qualifiers were all the municipalities that qualified for EZ designation but were never designated. Applicants were all municipalities that qualified and applied for EZ designation but were denied. Not all qualifiers applied for EZ status. They found that EZ designation had no significant effect on employment or property value.

Papke and Boarnet/Bogart represent the literature that used econometric techniques to evaluate the effectiveness of EZPs in an individual state. There is another set of literature in which the evaluation of EZPs was done in multiple states. Greenbaum and Engberg in two separate papers analysed the effects of urban EZs in six states: California, Florida, New Jersey, New York, Pennsylvania and Virginia.

In one paper they examined the effect of EZ designation on business outcomes and in the other, the effect on housing values. Using the difference-in-difference technique, they found that EZPs had a negative or insignificant effect on the measures of business outcomes at the ZIP code level. However, analysis at the establishment level showed that EZ designation had both positive and negative effects depending on the type of establishment. That is, EZ designation seemed to increase business outcomes through the birth of new firms. However, it appeared to be less effective in retaining existing activities.

A review of the literature in which econometric techniques were use to analyse EZPs reveals that the effects of these programs appear not to be uniform across the U.S. This indicates a need to evaluate each program and to examine reasons why some programs work and others do not.

3. Expected impact of Enterprise Zone Programs

EZ incentives are particularly geared toward businesses. Hence, the success of zone incentives can be measured by their impact on business decisions and on business outcomes. However, tax incentives are one of the many factors taken into consideration

in the decision process of businesses. Therefore, it is not clear what impact each EZP is likely to have on businesses.

EZ incentives are subsidies to firms. If these subsidies make it more profitable for a business to operate in one area as opposed to another, then the mobile factors of production will move to EZ areas in an effort to be more profitable. Factors of production will continue to move until the factor returns within an EZ are no greater than returns outside the zones. Given this phenomenon, it is expected that in equilibrium mobile factors of production will not experience higher returns in zones. It follows therefore, that the benefit from the zone incentives will be absorbed by the immobile factors of production.

In the short run, some factors of production, for example, capital may be immobile and may benefit from tax incentives in EZs. However, in the long run, since land cannot move, it is the only immobile factor. Therefore, the long-run incidence will be on the owners of properties subject to zone subsidies. The properties that are subject to EZ incentives are mostly commercial.

There are a few studies that have looked at the impact of EZPs on property value. Boarnet and Bogart (1996) found that the New Jersey program had no effect on property value. Greenbaum and Engberg (1999 and 2000) found no effect on housing values in the six state programs they evaluated. Greenbaum and Engberg focused on residential property values. However, if residential property cannot be readily re-zoned as commercial or industrial, then it would not receive the benefits of the EZP, so prices should not increase. Hence, the limited success of EZPs evaluated so far may be attributed to the outcome variables considered. The outcome variables analysed are the

mobile factors of production which are less likely to benefit from the presence of EZ incentives.

4. Contributions of this paper

One problem faced in evaluating the effects of EZPs is endogeneity bias. An area is designated EZ because it is characterised by high unemployment, low population growth and low per capita income. However, to analyse the effectiveness of the zone programs, the same characteristics that resulted in the area receiving EZ status are also the dependent variables.

Papke (1994) suggested that one reason for using panel data analysis was to address the possibility of EZ being endogenously selected. However, Boarnet and Bogart (1996) found that the panel data analysis used both by them and Papke did not completely correct the endogeneity bias. In light of this, this paper addresses the endogeneity bias by changing the unit of observation from jurisdiction, ZIP codes and municipalities to establishments. This addresses the endogeneity problem in that an area is not likely to be designated a zone on the basis of the performance of individual establishments. Also, using establishment-level data will shed additional light on the behavioural effects of EZPs at the micro level rather than the macro level, which is what is attained when jurisdictions or municipalities are the units of observation.

Greenbaum and Engberg (1998) showed the value of evaluating EZ effects using establishment-level data in the manufacturing industry. Analysing the effect of EZPs in six states, they found that the program was ineffective when examined at the ZIP code level. However, when examined using establishments as their units of observation they

observed that the programs had varying effects depending on the types of establishment. By using establishments as our unit of observation, this paper follows the work of Greenbaum and Engberg. However, rather than focusing on only establishments in the manufacturing industry we include establishments in all industries. The establishments are divided into ten major industries as determined from the first two digits of the Standard Industrial Classification (SIC) index.

Another problem faced in analysing EZPs is that zones do not follow established boundaries. As a result, all previous studies have not been able to isolate the influence of a specific zone area. For example, Boarnet and Bogart (1996) used municipalities as their unit of observation. Their treatment group consists of municipalities that have at least a part of their boundary designated as an EZ. The second contribution of this paper is to construct, as precisely as possible, with the aid of Geographic Information Systems (GIS), the exact areas of each EZ in the state of Colorado. This construction is of paramount importance since the area of the municipality that is not in the zone may confound the effect of the EZ.

EZs are so designated because of the economic blight evident in these areas. Therefore, to determine if there are any improvements in the economic conditions in these areas it is not enough to evaluate the entire municipality or zip code of which the EZ is a subset. If the zones are found to be ineffective, as evidenced from previous research, we cannot conclusively say that the programs are ineffective. Ostensibly, EZPs could have caused a shift of economic activities within the municipality from areas outside the zones to areas within. While it may not be desirable on efficiency grounds for

an economic development tool to result in the improvement of one area at the expense of another, it is certainly useful to determine if this is the case.

Data limitations and the boundary definition of EZs make it difficult to determine if EZs are cannibalising neighbouring communities. Notwithstanding, the analyses done so far were made using the data available at county, municipal or ZIP code level. As such, we return to our initial concern, that is, the difficulty in isolating the efficacy of the zone area.

This paper, will not test if EZs are cannibalising their neighbours. However, with the use of GIS we will clearly define each EZ in order to determine its impact on economic development. In future research one should be able to determine if EZs are displacing economic activity from nearby areas.

The final contribution of this paper is that we use dummy variables to determine if the effects of EZs differ by location, industries and the employment-size class of establishments. All previous studies either concentrated on a particular industry, for example manufacturing, or treat the effects of EZs as uniform across industries. Similarly, previous studies either concentrated on urban EZs as a whole, or treated the effects of the EZs as uniform across zones. None of the research done so far has considered the effects of EZPs based on the employment-size class of establishments.

The State Auditor's April 1998 report of Colorado's EZP breaks out the EZ incentives received by the various industries as follows; manufacturing (36%), retail (11%) and agriculture (10%). The other seven industries combined, received the other 43

per cent.[1] Hence, an evaluation of the EZP by industry is important to determine if the effects of the zones across industries are related to the level of incentives received. Further, evaluation at the industry level will inform about changes in the sectoral composition of employment. It could be the case that without controlling for industries we conclude that EZ designation has no effect on employment. However, this may not be since employment could be changing in the various industries though the total number of jobs remained constant. If the changes occurring in the different industries are such that it increases the chance of people finding jobs that are better matches for them, then it could be argued that the EZP is effective.

In most states, EZ incentives are aimed at traditional industries, in particular manufacturing. Further, it is argued that EZs are more competitive in attracting manufacturing facilities because of the presence of affordable labour. In fact, Erickson and Friedman (1990a) found that manufacturing accounted for 73 percent of new jobs created. This was based on a survey study of 357 EZs between 1982 and 1987. Given the concentration on manufacturing, we would expect more action in this industry as a result of zone programs. In fact, the 2002 State Auditor's Report of Colorado's EZP found that manufacturing was the only industry in which EZ outperformed non-zone areas. By controlling for industries, we will determine which sectors are impacted by the presence of EZ.

Related to the evaluation of the impact of EZPs at the industry level is an evaluation of its effect on establishments based on their employment size. Greenbaum and Engberg show that EZ effects vary according to the type of an establishment - that is,

[1] Based on the State Auditors report (1998). It is possible that one of the seven industries in the combined category receiving 43% of the tax incentive receives more in incentives than the manufacturing industry. However, that information is not available.

whether or not an establishment is new, dead, growing or shrinking. Given that tax incentives are one of the many things taken into consideration when making business decisions, its effect may vary based on the size of an establishment. A small establishment that is struggling to survive may breathe new life because of incentives available through the EZP.

Colorado's EZs are located both in rural and urban areas. Since each zone is expected to adopt specific economic development objectives, we will also evaluate the effects of the program in each individual zone. This will help us determine if the effects of the EZP vary between the rural and urban zones. This evaluation is particularly important because, as a single industry, agriculture receives the third highest amount of tax incentives through the EZP. Since most agricultural activities are concentrated in the rural areas, it is necessary to evaluate the effects EZ designation is having in the rural as well as the urban areas.

There are sixteen EZs in Colorado. Therefore, it is also important to analyse EZ effectiveness at the level of each individual zones. If Colorado's EZP is analysed as a whole, it is possible to conclude that the program is ineffective. However, while the overall results may indicate that the EZP is not effective, it is possible that when evaluated at the level of each individual zone we may see some impact. This is so because when evaluated as a whole, a positive effect in one zone may be negated by a negative effect in another.

4. A brief description of Colorado's Enterprise Zone Program

Colorado's EZP was introduced in 1986. State officials evaluated the program and zones were re-designated in 1996. By statute, the number of zones is limited to sixteen. Eight of the existing zones were designated in 1986. The last zone, the Larimer County EZ, was selected in 1993. Of the sixty-three counties in Colorado, thirty-five lie entirely within an EZ, while eight have no part of their boundaries within an EZ. The remaining counties are part EZ and part non-zones.[2] EZs account for approximately seventy percent of the total land area of Colorado.

Colorado's EZP was created "to provide incentives for private enterprise to expand, for new businesses to locate in economically depressed areas and to provide more job opportunities for residents of such areas."[3] According to statute, to be designated an EZ an area must have a population of less than 50,000[4] and meet at least one of the following criteria:

- An unemployment rate at least 25 percent above state average.
- A population growth rate less than 25 percent of the state average.
- A per capita income less than 75 percent of the state average.

Businesses located within an EZ may access any of the ten different tax credits and incentives offered as long as they qualify.[5] A three percent investment tax credit, offered to businesses making investment in machinery and equipment used exclusively in an EZ is the most valuable incentive from a tax-saving viewpoint. Between Fiscal Year 1989 and 2001 the investment tax credit represented about 70 percent of the total credit

[2] See Figure 1 in appendix for a map of Colorado's enterprise zones.
[3] State Auditor's report, April 1998.
[4] This was increased to 80,000 after the re-designation process in 1996.
[5] See appendix for a list of tax credits and incentives.

given to all participants. Given this phenomenon, an evaluation of the effects of EZ designation on investment should be undertaken. However, the data used in this research do not allow this, since they contain no measures of investment in machinery and equipment.

Two of Colorado's EZ tax incentives are directly related to employment. First, businesses hiring new employees in connection with a "new business facility" located in an EZ are eligible for a $500 tax credit against state income taxes for each new employee. Second, an additional credit of $500 per new business facility employee may be claimed by businesses that add value to agriculture commodities through manufacturing or processing. Furthermore, of the three criteria required for zone designation, an unemployment rate at least 25 percent above state average is the most frequent reason for zone eligibility. 13 of the 16 zones re-qualified for eligibility in 1996 based on the unemployment criteria. Against this background and the fact that one of the goals of the EZP is to provide greater job opportunities for residents in economically depressed areas, this paper provides an analysis of the effects of zone designation on employment.

The effectiveness of Colorado's EZP is of concern to many state officials. Therefore, the Office of the State Auditor as well as the Colorado Legislative Council has evaluated the program. Both departments have raised many questions but have still left unanswered the question of whether or not the program works. The Colorado Legislative Council (1996) found evidence that personal income and employment accelerated in the rural EZs relative to the state as a whole. However, they were unable to attribute this acceleration to the presence of the EZP.

The state auditors concluded - based on their 1995 and 1998 audits of the EZP - "because of serious data limitations and other problems we cannot determine whether the program has been effective or whether it has been responsible for any of the economic changes in the zones."[6] However, in the 2002 Report of the State Auditor, they concluded that since the implementation of the program, EZ areas have shown improvements in both employment and per capita income growth. What remains uncertain is whether any of these improvements can be directly attributed to the implementation of the program. What is known for certain is since the implementation of the program in 1986 to 2000, over $300 millions has been given in tax credits.

Given the cost to the government and the uncertainty of state officials about the effectiveness of Colorado's EZP; the attempt of this paper to evaluate its impact should be of some value. This paper is also of interest since it follows the econometric analysis of James Alm and Julie Ann Hart (1998). They used data from the 1980 and 1990 decennial census to evaluate the effects of Colorado's EZP on economic development. They concluded that the program has positive and significant impact on both employment growth and per capita income. Using a different data set and econometric methodology, we evaluate the impact of the program on establishment-level employment and earnings per worker.

5. Data

The data used in this paper are from a number of sources. Information on Colorado's EZP was obtained from the State of Colorado Department of Local Affairs.

[6] Auditor's Report 1998

The information consists of the geographic areas of the EZs and the date of designation for each individual zone. Nine of the sixteen EZs are multi-site and do not follow established boundary levels, for example, county or municipal designation. The geographic information obtained from the State of Colorado Department of Local Affairs and the individual zone coordinators was used to build GIS maps of each EZ.

The second data source is the 1990 Census of Population and Housing. The census data provides information on county characteristics that may impact both the number of employees employed to an establishment as well as wages paid. These include per-capita income, unemployment rate and the size of the population. The approach taken in this paper is to define as precisely as possible each EZ as well as non-zone areas. Using econometric techniques, we control for factors that are likely to influence the employment and wages of establishments other than EZ designation. The census data is used to control for factors influencing the performance of establishments other than EZ designation.

The final data source is the ES-202 data. The ES-202 database is a cooperative endeavour of the Bureau of Labour Statistics (BLS) and the employment security agency of the State. Comprising quarterly establishment-level data, the ES-202 program is a comprehensive and accurate source of employment and wage data, by industry, at the state, and county levels. The addresses of the establishments obtained from the ES-202 data were overlaid on the GIS map to determine whether or not an establishment is located in an EZ. The outcome variables are average monthly establishment employment and earnings per worker. We use the ES-202 data for the years 1990 and 2000 in this evaluation.

One of the concerns in compiling the data is selection bias. That is, why are establishments located where they are in the first place? The aim of the paper is to evaluate the effects of the EZP on establishment-level employment and wages. However, it is possible that establishments located within or without an EZ were different to begin with. That is, establishments selected their location based on whether or not they think being in a zone will be beneficial. If this is the case, then the location of an establishment is endogenous. This paper does not deal extensively with the selection problem. However, as an initial approach we deleted all the establishments that either moved from an EZ to a non-zone location or from a non-zone location to an EZ location between 1990 and 2000. Locations are more likely to be exogenous for establishments whose locations were stable over the period.

In compiling the data, we eliminated all the establishments that are located in the Larimer County EZ. The Larimer County EZ was designated as a zone in 1993. Therefore, establishments located in the Larimer County EZ in 2000 would have switched EZ status during the period.

A number of observations were also lost in the GIS process. The GIS was used to map the addresses of establishments to determine whether or not they are located in an EZ. A number of the reported addresses were not recognised by the GIS program and therefore eliminated from the final data set.

EZ incentives are given to establishments based on their physical location. For a number of the establishments in the original data we could not determine with certainty their physical locations. As a result, all of these establishments were also deleted. We deleted 62,309 establishments in 2000 compared to 6,278 in 1990 for this reason. This

however, was not a problem for this analysis since most of the establishments deleted from the 2000 sample had not been in existence in 1990. For this reason, they would not have qualified for the final sample.

We are analysing the effects on establishment-level employment and wages in 2000 as a result of EZ designation in 1990. Therefore, establishments that did not exist in 1990 do not have an EZ designation. Of the 129,781 establishments in the data in the year 2000, 100,017 did not exist in 1990 and hence were deleted from the final data set.[7] All establishments in the final data set were in existence in 1990.

The final data set contains 73,008 establishments. This was obtained from merging the 85,990 establishments in the 1990 sample with the 82,794 establishments in the 2000 sample and making some of the adjustments described above. Included in the 73,008 are 17,331 establishments that existed both in 1990 and 2000 as well as 55,677 establishments that existed in 1990 but not in 2000. Among the establishments that existed in both periods, only those that remained in the same EZ or were in a non-zone in both 1990 and 2000 are included in the final sample.[8]

The final data set, including establishments that died between 1990 and 2000 and the establishments that existed in both periods, was so compiled because it allows for a more thorough investigation of the effects of EZ designation on establishment-level employment and wages. The establishments that died have zero employment and wages in 2000. However, if we analysed only those establishments that survived from 1990 to

[7] We also omitted a further 10,522 establishments which existed in both 1990 and 2000, but their physical location in 2000 was uncertain.

[8] Analysis was also done using the data set with all the establishments whose physical location could not be determined with certainty. Taking the addresses reported for these establishments as their physical location we assign them EZ status based on their 1990 location. The number of establishments in this data is 83,539. The results obtained are basically the same as those reported in this paper.

2000 we would systematically throw away information about the establishments with the poorest performance. Further, the resulting estimates would not hold for the entire population since it is based on a non-randomly selected sample.

6. Econometric Methodology

The basic econometric model estimated is $y = \beta_0 + \beta_1 EZ + X\gamma + \varepsilon$ where y is establishment-level employment and earnings per worker in the year 2000. The explanatory variable of main interest is the dummy variable EZ that is equal to 1 if the firm is located in an enterprise zone and equal to 0 if not. X is a vector of independent variables, and ε is an independently distributed error assumed to be normal with mean zero and constant variance σ^2.

Included in the vector of independent variables X is a set of industry dummy variables used to capture the impact of each of the ten major industrial classifications as determined by the first two digits of the SIC index. A dummy variable called single establishment is also included as a measure of the size of the firm. This variable is equal to 1 if there is only one establishment under a particular ownership and 0 otherwise. Average establishment monthly employment and wage per worker in 1990 are also included. These are used as measures of the initial size of individual establishments. The variables described here are the ones included in the basic econometric specification.

The final sets of independent variables are a set of population characteristics for each county and a set of county dummies. The population characteristics include the size of the population, per capita income, the level of unemployment, and measures of the educational attainment of the population as well as measures of race. Alternatively, we

include a set of sixty-three dummy variables for each of the counties in Colorado. They control for all characteristics that are unique to each individual county and constant over the period under examination. These two alternative sets of controls test the robustness of the models estimated.

We estimate the models using Tobit since we observe that average monthly employment and earnings per worker is censored at zero. The 55,677 establishments that went out of business between 1990 and 2000 have zero employment and wages in 2000. Given the number of establishments that went out of business between 1990 and 2000 our two questions of interest are: (1) what is the effect of EZ designation on establishment-level employment and wages in 2000 and (2) what is the probability of an establishment surviving given that it is located in an EZ? These two questions are answered by estimating a Tobit model.

The Tobit model assumes an underlying latent variable which in this case is "net position with respect to labour market". Establishments with positive employment are purchasers of labour. Establishments with zero employment are not, but their optimum could be negative purchases, or rather, supply of labour. We observe the true value for establishments whose optimum is labour purchase, and zero for any establishment whose optimum is labour supply. Using ordinary least squares (OLS) to regress employment and wages on the explanatory variables will result in biased estimates since we do not observe the true value of employment and wages below zero. The Tobit model is designed to estimate censored data and is therefore appropriate in this case.

The Tobit model may be expressed as:

$$y^* = x'\beta + \varepsilon$$

where y* is the latent dependent variable which is unobservable for all values of y* less than zero. The relationship between the latent variable y^* and observed y is:

$$y = y^* \text{ if } y^* > 0$$

$$y = 0 \quad \text{if } y^* \leq 0$$

The model written in terms of observed y is:

$$y = x'\beta + \varepsilon \text{ if } y > 0$$

$$y = 0 \text{ otherwise}$$

The results from the Tobit estimations can be interpreted with reference to four types of expected values.

1. The expected value of the latent variable; that is,

$$E(y^* \mid x) = x'\beta$$

The marginal effect is given by $\frac{\partial E(y^*)}{\partial x} = \beta$, that is, the effects on the latent dependent variables as a result of being in an EZ.

2. The estimated probability of exceeding the censored value 0, in this case, the estimated probability of an establishment surviving:

$$P(y > 0) = \Phi\left(\frac{x'\beta}{\sigma}\right).$$

The marginal effect is given by $\frac{\partial P(y > 0)}{\partial x} = \phi(z)\frac{\beta}{\sigma}$; that is, the probability of an establishment surviving given it is located in an EZ.

Φ is the standard normal distribution function and ϕ is the corresponding standard normal density function. Here, z is a particular value of $\frac{x'\beta}{\sigma}$.

3. The expected, unconditional value of the realised or observed variable:

$$E(y \mid x) = \Phi\left(x'\beta + \sigma \frac{\phi}{\Phi} \right).$$

The marginal effect is given by $\frac{\partial E(y)}{\partial x} = \Phi(z)\beta$, that is, the effects on the observed dependent variables as a result of being in an EZ.

4. The expected value of the observed variables conditional on this being greater than the threshold value zero:

$$E(y \mid y > 0) = x'\beta + \sigma \frac{\phi}{\Phi}$$

The marginal effect is given by $\frac{\partial E(y / y > 0)}{\partial x} = \beta\left[1 - z\frac{\phi(z)}{\Phi(z)} - \left(\frac{\phi(z)}{\Phi(z)} \right)^2 \right]$ that is, the effects on the dependent variables for those establishments that existed both in 1990 and 2000 as a result of being in an EZ.

7. Descriptive Statistics[9]

Table I
Summary statistics for average monthly earnings per worker

	State		Enterprise Zone		Non-Ezone	
	1990	2000	1990	2000	1990	2000
Mean	$1,677.81	$663.49	$1,448.59	$586.84	$1,784.58	$699.19
Standard deviation	$4,282.71	$3,128.83	$2,869.47	$1,973.50	$4,797.30	$3,539.98
N	73,008	73,008	23,200	23,200	49,808	49,808

Summary statistics for average monthly employment

	State		Enterprise Zone		Non-Ezone	
	1990	2000	1990	2000	1990	2000
Mean	14.12	6.34	14.16	6.16	14.10	6.43
Standard deviation	98.18	82.66	60.93	44.28	111.36	95.04
N	73,008	73,008	23,200	23,200	49,808	49,808

Examination of the descriptive statistics in Table I above shows that although approximately 70 percent of the land area of Colorado is an EZ most of the economic activities, as far as the number of establishments operating, are taking place in non EZ areas. There are approximately two establishments in a non-zone area for every establishment located in an EZ. In both 1990 and 2000 average monthly employment per establishment is approximately the same in both EZ and non-zone areas. However, average monthly wage per worker is higher in the non-EZ areas.

Average wage per worker and average employment in 2000 is substantially smaller than in 1990. The difference is a result of the number of establishments that went out of business over the period. The 73,008 establishments in our sample includes

[9] The results presented here are similar to the results of the descriptive statistics for the sample with 83,539 firms referred to in footnote 8. While the magnitude of the numbers differs the qualitative findings are in general the same. The ratios of firms in zone to non-zone for all categories of firms are similar for both datasets.

55,677 with employment and wage equal to zero in 2000, indicating that they went out of business. All these zeros are included in the averages calculated for that year.

Of the 55,677 establishments that went out of business between 1990 and 2000, 17,159 were located in EZs and 38,518 in non-zones. This is consistent with the 2:1 ratio of establishments in zones to establishments in non-zones.

We now present the descriptive statistics of the 17,331 establishments that existed in both 1990 and 2000. The results are reported in Table II below.

Table II: **Summary statistics for establishments that survived**

Summary statistics for average monthly earnings per worker

	State		Enterprise Zone		Non-Ezone	
	1990	2000	1990	2000	1990	2000
Mean	$1,802.25	$2,794.99	$1,508.53	$2,253.70	$1,959.41	$3,084.62
Standard deviation	$2,902.91	$5,939.97	$1,682.71	$3,346.93	$3,369.04	$6,923.15
N	17,331	17,331	6,041	6,041	11,290	11,290

Summary statistics for average monthly employment

	State		Enterprise Zone		Non-Ezone	
	1990	2000	1990	2000	1990	2000
Mean	21.63	26.72	19.62	23.65	22.70	28.37
Standard deviation	149.27	168.05	70.51	84.36	177.60	198.84
N	17,331	17,331	6,041	6,041	11,290	11,290

Among the establishments that existed in both 1990 and 2000 we observe that their average monthly employment and wage per worker are higher than those reported in Table I. This indicates, as would be expected, that the establishments that survived are those which on average are out-performing other establishments. Establishments located in non-zones have a higher level of employment and wages than establishments located in EZs. The ratio of establishments in non-zones to establishments in zones among the

surviving establishments is lower than the 2:1 ratio observed in the rest of the sample. One possible implication here is that EZ designation is effective in improving the chances of an establishment surviving, thus increasing the number of establishments located in EZs relative to non-zones.

8. Impact on Employment

8.1 Impact on Employment by Employment Size Class of Establishments

Below, Table III reports the results of the first three models estimating the effect of Colorado's EZP on the latent dependent variable, average monthly employment in 2000.

Table III[10]
Dependent Variable: Average monthly employment in 2000

	Model 1 Tobit results		Model 2 Linear Regression for employement 2000>0		Model 3a Heckman selection corrected model		Model 3b sample selection probit model	
Variables	Coefficient	t-stats	Coefficient	t-stats	Coefficient	t-stats	Coefficient	t-stats
EZ	9.73	6.68**	-1.65	2.19**	-19.72	10.14**	0.0407	3.64**
Mineral Industries	-36.76	4.61**	-7.03	1.66*	33.01	3.19**	-0.3674	6.20**
Construction Industries	-14.13	2.67**	4.52	1.69*	20.99	3.09**	-0.2326	5.91**
Manufacturing	-16.58	2.97**	1.81	0.64	26.85	3.74**	-0.2315	5.57**
Transportation, Communication and Utilities	-7.01	1.22	1.76	0.61	6.41	0.87	-0.1538	3.58**
Wholesale Trade	-13.05	2.47**	2.77	1.04	18.32	2.70**	-0.2209	5.61**
Retail Trade	-10.37	2.11**	-1.02	0.41	8.50	1.35	-0.2200	6.01**
Finance, Insurance and Real Estate	-10.09	1.95*	1.31	0.50	11.37	1.72*	-0.1903	4.95**
Service Industry	-10.62	2.20**	2.67	1.10	2.32	0.37	-0.1987	5.53**
Other	105.37	13.50**	21.91	6.24**	-71.64	7.22**	0.5418	9.02**
Single Establishment	-2.37	1.29	3.33	3.43**	8.63	3.52**	-0.1424	9.93**
Avg. monthly earnings per worker (1990) in	6.19	4.46**	2.64	2.13**	13.91	6.13**	0	1.34
Avg. monthly employment (1990)	0.56	208.91**	1.08	453.56**	1.15	158.18**	0.0012	26.05**
Constant	-121.65	24.03**	-1.20	0.47	173.52	25.74**	-0.6171	14.40**
Correlation Coefficient							-0.9596	930.97**
Observations	73008		17331		73008			
* significant at 10% level; ** significant at 5% level								

[10] The results in the third and fourth sets of columns (Models 3a and 3b) were estimated using the Heckman sample selection procedure.

The three models reported in Table III are three different estimation techniques testing the robustness of the Tobit specification. Model 1 is the basic Tobit specification with the explanatory variable of main interest being the dummy variable controlling for the effect of EZ. Included are the ten major industrial classifications as well as average monthly employment and wages per worker in 1990. The dummy variable, single establishment is included to control for whether or not an establishment is part of a firm with one or more establishments.

The results in Model 1 suggest that EZ designation has a positive effect on latent average monthly establishment employment in 2000. Further results from the Tobit indicate that among the establishments that survived, that is, those with positive employment in 2000, EZ effect is positive and significant. However, when the simple linear regression is estimated using data with only the establishments that survived (Model 2), the effect of the EZs when compared to non-zones is negative. This raises the question as to whether or not the Tobit estimation of this particular model is correct.

The Tobit estimation (Model 1) assumes that the variables that impact employment in 2000 are the same variables that determine the probability of an establishment surviving. This may be considered restrictive. Generally, this is a criticism of the Tobit model, that is, it restricts the same set of variables to determine both the probability of truncation and the expected value of the realized dependent variable conditional on it being observed. In this case however, the characterization seems reasonable since an establishment that survives is one with employment greater than zero. Hence, it is logical to think that the same variables which increase the probability of an establishment surviving are also increasing its expected level of employment.

An alternate approach to the Tobit estimation is to estimate a probit for establishment survival from 1990 to 2000 and then a separate linear model for employment in 2000 conditional on survival. This alternate model is estimated using the Heckman selection model (Models 3a and 3b).

The Heckman selection equation relaxes the constraint of the Tobit model. It allows for each variable to have different effects on the probability of an establishment surviving and the level of employment. Ideally, at least one of the variables included in the equation estimating the probability of an establishment surviving should be excluded from the employment equation. If this is not the case, then the employment equation is identified only because of the nonlinearity of the probit equation. As a general rule, it is not advisable to depend on the nonlinearity of the probit equation for identification.

We do not have a variable that satisfies the exclusion restriction. Therefore, the results must be interpreted cautiously. The correlation coefficient between the error terms is highly negative, indicating that establishments that have unobservable characteristics that make them more likely to survive also have unobservable characteristics that make them smaller. This result is not intuitive. However, it suggests that the effect of EZ designation may vary by the employment size of each establishment. Hence, we re-estimate Model 1, changing the linear specification of average monthly employment in 1990 to include eight dummy variables accounting for different employment size of establishments. The results are presented in Table IV, below.

Table IV
Dependent Variable: Average monthly employment in 2000

	Model 4		Model 5 Control for population characteristics		Model 6 Control for location using County dummy	
Variables	Coefficient	t-stats	Coefficient	t-stats	Coeffcient	t-stats
3 ≥ Avg. monthly employment (1990)*EZ	9.88	3.19**	-0.51	0.15	-8.23	2.27**
3 < Avg. monthly employment (1990) ≤ 6*EZ	14.63	3.58**	5.39	1.26	-1.15	0.26
6 < Avg. monthly employment (1990) ≤ 10*EZ	15.50	3.06**	7.74	1.48	2.19	0.41
10 < Avg. monthly employment (1990) ≤ 20*EZ	8.34	1.54	2.07	0.37	-3.88	0.68
20 < Avg. monthly employment (1990) ≤ 30*EZ	-0.77	0.09	-5.07	0.60	-10.78	1.27
30 < Avg. monthly employment (1990) ≤ 40*EZ	8.11	0.71	4.14	0.36	-2.30	0.20
40 < Avg. monthly employment (1990) ≤ 50*EZ	4.54	0.32	-0.51	0.04	-7.04	0.49
50 < Avg. monthly employment (1990)*EZ	-28.95	3.95**	-33.40	4.51**	-39.08	5.23**
3 ≥ Avg. monthly employment (1990)	-214.09	45.35**	-215.66	45.64**	-216.57	45.77**
3 < Avg. monthly employment (1990) ≤ 6	-174.35	35.27**	-176.28	35.62**	-177.09	35.74**
6 < Avg. monthly employment (1990) ≤ 10	-159.41	30.41**	-161.61	30.79**	-162.32	30.90**
10 < Avg. monthly employment (1990) ≤ 20	-144.16	26.80**	-146.63	27.22**	-147.06	27.29**
20 < Avg. monthly employment (1990) ≤ 30	-122.97	18.56**	-125.40	18.91**	-126.23	19.03**
30 < Avg. monthly employment (1990) ≤ 40	-120.87	15.09**	-122.41	15.28**	-122.60	15.29**
40 < Avg. monthly employment (1990) ≤ 50	-110.44	11.36**	-110.74	11.39**	-111.16	11.43**
Mineral Industries	-44.72	4.34**	-35.03	3.39**	33.14	3.18**
Construction Industries	-13.24	1.94*	-9.84	1.43	-7.20	1.04
Manufacturing	-23.89	3.32**	-15.74	2.17**	-12.40	1.69*
Transportation, Communication and Utilities	-8.88	1.19	-5.43	0.73	-3.14	0.42
Wholesale Trade	-14.65	2.14**	-4.35	0.63	-0.68	0.10
Retail Trade	-20.92	3.29**	-16.05	2.51**	-13.74	2.13**
Finance, Insurance and Real Estate	-2.71	0.41	5.11	0.76	7.30	1.08
Service Industry	-5.11	0.82	2.33	0.37	4.91	0.77
Other	142.89	14.17**	140.03	13.84**	139.15	13.69**
Single Establishment	14.54	5.77**	12.49	4.93**	12.33	4.86**
Avg. monthly earnings per worker (1990) in $10,000	9.17	5.16**	10.09	5.70**	10.22	5.78**
Per Capita Income in $10,000			11.54	2.17**		
Unemployment rate			1.09	1.25		
Population in 100,000			-9.31	10.29**		
% High School Graduates			-0.39	1.06		
% College Graduates			0.12	0.48		
% White Population			-0.49	1.08		
% Black Population			-0.98	1.88*		
Constant	8.18	1.07	84.12	2.49**	11.58	1.24
Observations	73008					
Log Likelihood	-132541.22		-132364.48		-132269.89	
Likelihood ratio test[11]	$9353.52 > \chi^2_{(13)} = 22.36$		$353.48 > \chi^2_{(7)} = 14.07$		$542.66 > \chi^2_{(62)} = 81.37$	
* significant at 10% level; ** significant at 5% level						

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Model 4 in Table IV corresponds with Model 1 in Table III. We replace the linear specification of average monthly employment in 1990 with dummy variables representing eight different employment sizes of establishments. We also interact each employment-size category with the EZ dummy to determine if the effects of EZ designation vary by establishment size.

[11] Likelihood ratio test = 2(unrestricted log-likelihood ratio – restricted log-likelihood ratio). This value is compared with the chi-squared value. The degree of freedom for the chi-squared value is the difference between the number of regressors in the restricted and unrestricted models. The new variables added to the unrestricted models are retained if the likelihood ratio test is greater than the chi-squared value.

Models 5 and 6 augment the specification of Model 4 with controls for county characteristics. In Model 5, we add variables controlling for the population characteristics of the sixty-three counties in Colorado. In Model 6 we replace the population characteristics in Model 5 with county dummies. The coefficients on the county dummies are not reported here for economy of presentation.

In all three models presented in Table IV we drop the EZ dummy and include all eight employment-size/EZ interaction terms. The coefficients on average wage per worker in 1990, single establishment and all seven employment-size dummy variables are significant at the 5% level, in all three models. The coefficients on all seven employment-size dummies are negative. This indicates that when compared to establishments with over fifty employees in 1990, all other establishments have a lower level of latent average monthly employment in 2000. The coefficient on single establishment is positive, indicating that when compared to establishments belonging to multi-establishment firms, latent average monthly employment in 2000 is greater for single establishment firms.

The basic specification, Model 4, suggests that EZ designation is good for smaller establishments but bad for larger establishments. That is, when compared to similar establishments not located in EZs, establishments with ten or fewer employees that are located in an EZ have a greater level of latent employment in 2000. However, establishments with more than fifty employees that are located in EZs have fewer potential employees in 2000 than similar establishments not located in an EZ.

These initial results indicate that the Tobit specification estimated in Model 4 (Table IV) is preferred to that estimated in Model 1 (Table III). First, the likelihood ratio

test suggests that the additional variables included in Model 4 should be retained. Secondly, the results in Model 4 reconcile the results of all three models estimated in Table III.

Model 1 indicates that when compared to non-zone areas the level of latent employment in 2000 is higher in EZs. However, in Model 2 we see that the level of latent employment in 2000 is lower in EZs among the establishments that survived. Model 3b suggests that the probability of an establishment surviving is higher in EZs when compared to non-zone. However, the level of employment conditional on survival is lower for establishments located in EZs (Model 3a).

Model 4 confirms that the level of latent employment is higher in EZs. However, this is true for firms with, at most, ten employees. The negative effect observed on EZ, particularly among establishments that survived, is also explained in Model 4. We observed a negative effect on establishments with over fifty employees, indicating that among establishments with the largest number of employees, the level of latent employment is lower for those located within an EZ.

Finally, we can determine from the sample selection model whether or not selectivity is a problem. The general Heckman selection model estimating

Stage 1: $z^* = w'\gamma + \mu, \quad z = 1 \quad if \;\; z^* > 0 \quad and \; z = 0 \;\; if \;\; z^* \le 0$ and

Stage 2: $y^* = x'\beta + \varepsilon, \quad y = y^* \;\; if \;\; z = 1 \quad and \quad y = 0 \; if \;\; z = 0$

is a two-step procedure as follows:

Step 1. Selection Equation:

$$\Pr ob(y^* > 0) = \Pr ob(z = 1) = \Phi(\gamma' w)$$

Step 2. Regression estimating the expected value of y, conditional on z = 1:

$E(y \mid z = 1, x) = x'\beta + \rho\sigma \frac{\phi}{\Phi}$ where $\frac{\phi}{\Phi}$ is the selectivity regressor or inverse Mills ratio.

$\rho\sigma$ is the coefficient on the selectivity regressor. The hypothesis ρ = 0 can be tested. If this hypothesis cannot be rejected, then selectivity is not a problem. All the Tobit specifications estimated in Table VII and subsequent tables reveals that selectivity is not a problem.

In Models 5 and 6 the significantly negative effect on establishments with more than fifty employees is also present. However, the significantly positive effect on establishments with ten or fewer employees vanishes when we control for population characteristics or county fixed effects. For establishments with at most three employees the effect is significant and negative in Model 6; however, it is insignificant in Model 5. We can therefore conclude that the apparent positive effects of EZs on the smallest establishments are spurious. They appear in Model 4 because the communities in which EZs are located are also good places for small establishments to operate. However, there is strong evidence across all models that EZs are not good for the largest size establishments.

The literature on EZs has raised the possibility of selection bias. The primary concern is that positive selection bias into EZs will exaggerate their estimated effects. Establishments choosing to locate in EZs would have unobserved characteristics that make them especially profitable in EZs. Establishments with different unobserved characteristics would be less profitable in EZs. Their experiences, were they to locate in EZs, would not be as successful as are those of establishments that actually choose to so locate.

The results in Table IV and subsequent estimations demonstrate that there is little scope for positive selection bias in the data examined here. Positive EZ effects are rare in the three models and isolated in their impact. If even these are exaggerated by selection, then the true effects must be truly negligible.[12]

8.2 Impact on Employment by Industry

The models in Table IV focus on the effects of EZ designation on establishments of different size classes. However, an argument can be made that the effects of the EZP may also vary by industry as discussed in the section 4. To determine this effect we interact each industrial classification with the EZ dummy variable. The results are presented in Table V, below.

[12] At the same time, the theoretical consequences of selection bias are more ambiguous than previously recognized. Establishments that locate outside EZs could also have unobserved characteristics that endow them with better prospects there than inside such zones. In this case, the performance of establishments outside of EZs would overstate that of an establishment placed there randomly, just as the performance of establishments inside EZs would overstate that of an establishment randomly located within a zone. The estimated difference between establishments in and outside of EZs would depend on the true effects of EZs and the difference between the biases in estimated performance within and without them. The sign on this difference is not guaranteed. However, the estimated difference would understate the true effect of EZs only if selection out of EZs and their incentives was more powerful than selection into them. This seems unlikely.

Table V
Dependent Variable: Average monthly employment in 2000

	Model 7		Model 8 Control for population characteristics		Model 9 Control for location using County dummy	
Variables	Coefficient	t-stats	Coefficient	t-stats	Coeffcient	t-stats
3 ≥ Avg. monthly employment (1990)*EZ	23.63	1.85*	0.07	0.01	-16.40	1.24
3 < Avg. monthly employment (1990) ≤ 6*EZ	27.38	2.12**	4.91	0.37	-10.27	0.77
6 < Avg. monthly employment (1990) ≤ 10*EZ	27.88	2.10**	6.85	0.51	-7.27	0.53
10 < Avg. monthly employment (1990) ≤ 20*EZ	20.11	1.50	0.74	0.05	-13.59	0.98
20 < Avg. monthly employment (1990) ≤ 30*EZ	11.11	0.74	-6.54	0.43	-20.83	1.36
30 < Avg. monthly employment (1990) ≤ 40*EZ	19.85	1.18	2.81	0.17	-12.06	0.71
40 < Avg. monthly employment (1990) ≤ 50*EZ	17.28	0.91	-1.08	0.06	-16.16	0.84
50 < Avg. monthly employment (1990)*EZ	-15.96	1.10	-34.10	2.33**	-48.40	3.27**
Mineral*EZ	-6.30	0.30	-3.90	0.19	3.65	0.17
Construction*EZ	-25.45	1.78*	-10.12	0.70	-1.25	0.09
Manufacturing*EZ	-4.80	0.33	17.03	1.15	28.54	1.91*
Transportation, Communication and Utilities*EZ	16.79	1.11	27.86	1.83*	32.32	2.10**
Wholesale*EZ	-12.43	0.89	4.59	0.33	13.23	0.93
Retail*EZ	-11.27	0.86	0.68	0.05	8.99	0.68
Finance,Insurance and Real Estate*EZ	-9.02	0.65	1.35	0.10	9.86	0.69
Sevice*EZ	-17.63	1.37	-4.05	0.31	5.00	0.38
Other*EZ	-51.18	2.52**	-46.42	2.28**	-44.66	2.18**
3 ≥ Avg. monthly employment (1990)	-214.80	45.32**	-216.46	45.62**	-217.31	45.74**
3 < Avg. monthly employment (1990) ≤ 6	-174.72	35.29**	-176.79	35.67**	-177.58	35.78**
6 < Avg. monthly employment (1990) ≤ 10	-159.65	30.41**	-162.00	30.82**	-162.68	30.93**
10 < Avg. monthly employment (1990) ≤ 20	-144.24	26.79**	-146.87	27.24**	-147.30	27.31**
20 < Avg. monthly employment (1990) ≤ 30	-122.97	18.56**	-125.55	18.93**	-126.36	19.04**
30 < Avg. monthly employment (1990) ≤ 40	-120.75	15.07**	-122.52	15.28**	-122.75	15.30**
40 < Avg. monthly employment (1990) ≤ 50	-110.56	11.37**	-111.06	11.42**	-111.54	11.46**
Mineral Industries	-42.45	3.19**	-33.28	2.50**	-34.23	2.57**
Construction Industries	-4.87	0.57	-7.07	0.82	-7.66	0.89
Manufacturing	-22.93	2.43**	-23.85	2.52**	-25.10	2.64**
Transportation, Communication and Utilities	-16.83	1.74*	-17.46	1.80*	-16.63	1.72*
Wholesale Trade	-9.85	1.12	-6.17	0.70	-5.75	0.65
Retail Trade	-16.57	2.04**	-16.23	2.00**	-17.01	2.09**
Finance, Insurance and Real Estate	1.22	0.15	4.84	0.57	3.89	0.46
Service Industry	1.18	0.15	3.54	0.45	2.82	0.35
Other	167.49	12.10**	163.96	11.82**	163.25	11.76**
Single Establishment	14.76	5.85**	12.78	5.04**	12.58	4.95**
Avg. monthly earnings per worker (1990) in $10,000	9.16	5.15**	10.10	5.71**	10.24	5.80**
Constant	3.57	0.39	86.50	2.54**	15.48	1.47
Observations	73008					
Log Likelihood	-132528.49		-132350.73		-132255.42	
Likelihood ratio test	$25.46 > \chi^2_{(6)} = 16.92$		$355.52 > \chi^2_{(7)} = 14.07$		$546.14 > \chi^2_{(62)} = 81.37$	
* significant at 10% level; ** significant at 5% level						

Table V is an extension of Table IV with the addition of the industry/EZ interaction variables. We do not present the population variables here because the results are similar to those presented in Table IV. The results in Table V support our previous findings on the effect of EZ designation on the different size class of establishments. That is, with population or county controls EZ designation continues to

be bad for establishments with over fifty employees in 1990. However, it again has no significant effects on establishments that are smaller.

The results, in Table V, show that the effects of the EZP vary somewhat by industry. This effect is evident in the "other" and the transportation industries. When compared to the agricultural industry located in an EZ, latent average monthly employment in 2000 is lower for establishments in the "other" industry but larger for establishments in the transportation industry (Models 8 and 9). The effect in the transportation industry is marginally significant in Model 8.

The negative impact of EZ designation on latent employment in the "other" industry is particularly interesting when compared to the effect on latent employment for the industry irrespective of location. The positive coefficient on the dummy variable for all "other" establishments indicates that the mean level of employment for establishments in the industry increased between 1990 and 2000 relative to the agricultural industry. However, while the "other" industry as a whole had an increase in the number of employees between 1990 and 2000 the level of employment grew by significantly less for the subset of the industry that is located in an EZ. Hence, relative to the agricultural industry, EZ designation seems to reduce the advantage enjoyed by establishments in the "other" industry.

Conversely, the coefficient on the transportation industry is negative and marginally significant indicating that the mean level of latent employment for establishments in the industry is lower than the agricultural industry between the period 1990 and 2000. Therefore, while the transportation industry as a whole had fewer

employees between 1990 and 2000, that effect was negated or reversed for the subset of the industry that is located in an EZ.

The impact of EZ designation at the level of the industry is worthy of analysis because as a single industry, most of the EZ tax credits in the state of Colorado go to manufacturing followed by retail and agriculture. Since we find some evidence that the effect of EZ varies across industries, we would expect to see this effect reflected in the industries that benefited more. However, relative to agriculture, the effect of EZ designation on 2000 latent employment in the manufacturing and retail industries is insignificant. This should be of concern to policy makers since it indicates that investment made in the manufacturing and retail industries may not be reaping any benefits when compared with the agricultural industry which receives less EZ incentives than the manufacturing and retail industries. In fact, when compared to the agricultural industry as a whole, latent employment in 2000 was less for establishments in the manufacturing, retail, mineral and transportation industries. Transportation is the only industry where the EZP seems to have any effect in reversing this trend.

Further analysis was done to see if the differential effects of EZ designation on establishments in different employment-size classes is concentrated in particular industries. This was done by interacting the establishment-size dummies, the industry dummies and the EZ dummy. The results from these estimations do not provide any strong evidence that the differential effects of EZs on establishments in different employment-size classes are concentrated in particular industries.[13]

[13] These results are not presented in this paper but are available on request.

8.3 EZ Effect on Survival of Establishments

Given the large number of establishments that died between 1990 and 2000, we are interested in the effects of EZ designation on the probability that an establishment will survive. This probability, as well as the effect on average employment in 2000 for the establishments that survived, is provided in Table VI, below. Table VI is based on the coefficients of Table V. Table V reports the marginal effects on the latent dependent variable, while Table VI reports the other three marginal effects obtained from the Tobit estimations. (See section 6).

Table VI: Marginal effects 2 – 4 as discussed in the section on Econometric Methodology

		Model 7b			Model 8b			Model 9b	
Variable	E(y)	E(y|y>0)	P(y>0)	E(y)	E(y|y>0)	P(y>0)	E(y)	E(y|y>0)	P(y>0)
3 ≥ Avg. monthly employment (1990)*EZ	4.45	5.040	0.037	0.011	0.013	0	-2.69	-3.31	-0.023
	(1.99)	(1.91)	(1.94)	(0.01)	(0.01)	(0.01)	(1.18)	(1.21)	(1.20)
3 < Avg. monthly employment (1990) ≤ 6*EZ	5.31	5.91	0.044	0.87	1.02	0.007	-1.71	-2.08	-0.015
	(2.26)	(2.21)	(2.25)	(0.38)	(0.38)	(0.38)	(0.74)	(0.76)	(0.75)
6 < Avg. monthly employment (1990) ≤ 10*EZ	5.44	6.03	0.044	1.22	1.43	0.010	-1.22	-1.48	-0.011
	(2.34)	(2.20)	(2.25)	(0.52)	(0.52)	(0.52)	(0.53)	(0.53)	(0.52)
10 < Avg. monthly employment (1990) ≤ 20*EZ	3.82	4.30	0.031	0.13	0.154	0.001	-2.23	-2.74	-0.019
	(1.62)	(1.55)	(1.58)	(0.05)	(0.05)	(0.05)	(0.93)	(0.96)	(0.95)
20 < Avg. monthly employment (1990) ≤ 30*EZ	2.03	2.34	0.017	-1.11	-1.33	-0.010	-3.30	-4.15	-0.029
	(0.78)	(0.76)	(0.77)	(0.42)	(0.42)	(0.45)	(1.25)	(1.32)	(1.29)
30 < Avg. monthly employment (1990) ≤ 40*EZ	3.78	4.25	0.031	0.49	0.58	0.004	-1.98	-2.44	-0.017
	(1.29)	(1.22)	(1.25)	(0.17)	(0.17)	(0.17)	(0.67)	(0.69)	(0.68)
40 < Avg. monthly employment (1990) ≤ 50*EZ	3.26	3.68	0.027	-0.19	-0.22	-0.002	-2.61	-3.24	-0.023
	(0.98)	(0.94)	(0.96)	(0.06)	(0.06)	(0.06)	(0.78)	(0.82)	(0.80)
50 < Avg. monthly employment (1990)*EZ	-2.62	-3.22	-0.023	-5.14	-6.66	-0.046	-6.84	-9.22	-0.062
	(1.03)	(1.07)	(1.06)	(2.02)	(2.20)	(2.12)	(2.67)	(3.02)	(2.85)
Mineral*EZ	-1.08	-1.29	-0.009	-0.67	-0.80	-0.006	0.64	0.76	0.005
	(0.29)	(0.30)	(0.29)	(0.18)	(0.18)	(0.18)	(0.17)	(0.17)	(0.17)
Construction*EZ	-4.02	-5.06	-0.035	-1.69	-2.05	-0.015	-0.22	-0.26	-0.002
	(1.60)	(1.70)	(1.66)	(0.67)	(0.69)	(0.68)	(0.09)	(0.09)	(0.09)
Manufacturing*EZ	-0.83	-0.99	-0.007	3.17	3.61	0.026	5.54	6.16	0.045
	(0.32)	(0.32)	(-0.32)	(1.23)	(1.18)	(1.20)	(2.14)	(2.00)	(2.05)
Transportation, Communication and Utilities*EZ	3.15	3.58	0.026	5.43	6.02	0.044	6.38	7.03	0.052
	(1.18)	(1.14)	(1.15)	(2.05)	(1.91)	(1.96)	(2.40)	(2.22)	(2.28)
Wholesale*EZ	-2.07	-2.52	-0.018	0.81	0.95	0.007	2.41	2.78	0.020
	(0.85)	(0.87)	(0.86)	(0.33)	(0.33)	(0.33)	(0.98)	(0.95)	(0.96)
Retail*EZ	-1.90	-2.30	-0.016	0.12	0.14	0.001	1.61	1.88	0.014
	(0.83)	(0.85)	(0.84)	(0.05)	(0.05)	(0.05)	(0.70)	(0.69)	(0.69)
Finance,Insurance and Real Estate*EZ	-1.53	-1.84	-0.013	0.24	0.28	0.002	1.77	2.06	0.015
	(0.62)	(0.64)	(0.63)	(0.10)	(0.10)	(0.10)	(0.72)	(0.71)	(0.71)
Service*EZ	-2.91	-3.56	-0.025	-0.69	-0.83	-0.006	0.88	1.04	0.007
	(1.29)	(1.34)	(1.32)	(0.31)	(0.31)	(0.31)	(0.39)	(0.38)	(0.39)
Other*Ez	-7.21	-9.74	-0.066	-6.62	-8.87	-0.060	-6.38	-8.54	-0.058
	(2.03)	(2.32)	(2.18)	(1.87)	(2.11)	(2.00)	(1.80)	(2.03)	(1.92)
t-statistics are in parentheses									

E(y) is the marginal effect on observed average employment in 2000 as a result of being in an EZ. Observed employment is what is actually reported in the data which includes all the zeros for the establishments that went out of business. E(y|y>0) is the effect on average employment in 2000 for establishments that existed both in 1990 and 2000 and P(y>0) is the probability of the establishments surviving, given they are located in an EZ.

EZ designation has no effect on the probability of survival for establishments with less than fifty employees in 1990. However, when compared to similar establishments not located in an EZ, establishments with over fifty employees in 1990 have a lower probability of survival. That is, establishments located in an EZ with more than fifty employees are approximately five percentage points less likely to survive than similar establishments which are located in a non-EZ.

The probability of survival for a transportation establishment located in an EZ is approximately five percentage points (Models 8b and 9b) higher than an agricultural establishment located in an EZ. However, for establishments in the "other" industry the probability of survival is approximately six percentage points lower than establishments in the agricultural industry located in an EZ. For the transportation establishments that survived and are located in an EZ, they have approximately six more employees than their counterpart establishments that are not located in an EZ. This effect is substantially lower than the marginal effect on the latent employment variable.

The marginal effect on latent average employment in 2000 for transportation establishments located in an EZ is approximately twenty seven (27) to thirty two (32) (Table V) more employees when compared to agricultural establishments not located in

an EZ. The impact on the latent dependent variable is the level of employment that we would expect, were we able to observe all the values of the dependent variable. That is, we would observe those establishments who are "supplying" labour separate from those that are demanding labour. Since this effect is greater for the overall latent employment when compared to the establishments that survived, the implication here is that if the transportation industry were to take advantage of the EZ incentives their chance of survival would increase. This would likely result in an increase in the amount of labour demanded in the transportation industry relative to agriculture.

8.4 EZ effects at the level of each individual EZs

So far we have evaluated Colorado's EZP as a whole. However, there are sixteen EZs in Colorado, and since each zone is expected to adopt specific economic development objectives, it is useful to evaluate the effects of the program at the level of each individual zone. To do so, we replace the single dummy for all EZs with individual dummy variables for each EZ in the models estimated in Table IV. The results are presented in Table VII.

The results in Table VII support the previous findings that EZ designation is bad for establishments with more than fifty employees in 1990. However, there is again no evidence that it has any effect on establishments of smaller sizes. The positive effect of the EZP observed in the transportation industry and the negative effect in the "other" industry are also confirmed in these models.[14] The specifications estimated here suggest that, when compared to the agricultural industry located in an EZ, the effect on latent

[14] The specifications with the industry/EZ interaction terms are excluded for economy of presentation. Results are available on request.

employment in 2000 for establishments in the manufacturing industry that are located in an EZ is positive. This result is not supported by the previous Tobit estimations.

Table VII
Dependent Variable: Average monthly employment in 2000

	Model 10		Model 11 Control for population characteristics		Model 12 Control for location using County dummy	
Variables	Coefficient	t-stats	Coefficient	t-stats	Coeffcient	t-stats
Adams EZ	-12.94	1.40	-7.44	0.78	0.23	0.02
Arapahoe EZ	-24.15	3.30**	-18.50	2.47**	-7.55	0.96
Denver EZ	-25.34	5.58**	-14.56	2.86**	-17.72	3.45**
El Paso EZ	-12.08	2.14**	-3.97	0.62	-7.79	1.19
Jefferson EZ	-17.54	2.17**	-4.24	0.50	-10.65	1.24
Mesa EZ	5.40	0.54	6.36	0.61	16.12	1.32
Pueblo EZ	7.97	0.98	-1.86	0.21	-9.94	0.93
Greeley/Weld EZ	4.36	0.57	0.96	0.12	1.58	0.14
Southeast EZ	56.80	6.64**	48.22	5.00**	52.24	2.35**
South Central EZ	39.51	5.20**	27.16	3.07**	44.47	2.31**
San Luis/Upper Arkansas EZ	39.77	7.44**	30.09	4.65**	49.14	3.63**
East Central/Northeast EZ	41.59	7.74**	41.61	5.74**	73.91	1.26
Northwest EZ	13.66	1.74*	7.19	0.85	-9.66	0.59
Region 10 EZ	27.72	4.42**	25.52	3.32**	41.23	1.84*
Southwest EZ	2.58	0.35	-18.62	2.32**	0.21	0.01
3 < Avg. monthly employment (1990) ≤ 6*EZ	6.01	1.17	6.19	1.20	7.01	1.36
6 < Avg. monthly employment (1990) ≤ 10*EZ	9.69	1.62	9.86	1.65*	10.37	1.73*
10 < Avg. monthly employment (1990) ≤ 20*EZ	3.79	0.60	4.17	0.66	4.73	0.75
20 < Avg. monthly employment (1990) ≤ 30*EZ	-2.85	0.32	-2.12	0.24	-1.69	0.19
30 < Avg. monthly employment (1990) ≤ 40*EZ	7.21	0.61	7.15	0.60	6.60	0.55
40 < Avg. monthly employment (1990) ≤ 50*EZ	3.74	0.26	2.38	0.16	2.38	0.16
50 < Avg. monthly employment (1990)*EZ	-27.83	3.47**	-29.46	3.68**	-29.67	3.70**
3 ≥ Avg. monthly employment (1990)	-214.23	45.39**	-215.46	45.60**	-216.51	45.74**
3 < Avg. monthly employment (1990) ≤ 6	-174.24	35.26**	-175.88	35.55**	-176.99	35.72**
6 < Avg. monthly employment (1990) ≤ 10	-159.34	30.41**	-161.18	30.71**	-162.10	30.86**
10 < Avg. monthly employment (1990) ≤ 20	-144.02	26.78**	-146.18	27.14**	-146.86	27.25**
20 < Avg. monthly employment (1990) ≤ 30	-122.86	18.55**	-125.05	18.86**	-126.06	19.00**
30 < Avg. monthly employment (1990) ≤ 40	-120.87	15.09**	-122.45	15.27**	-122.65	15.29**
40 < Avg. monthly employment (1990) ≤ 50	-109.73	11.31**	-110.13	11.34**	-110.64	11.39**
Mineral Industries	-37.62	3.64**	-32.99	3.18**	-33.72	3.24**
Construction Industries	-5.30	0.77	-6.36	0.92	-6.92	1.00
Manufacturing	-11.36	1.56	-11.28	1.55	-11.94	1.63
Transportation, Communication and Utilities	-3.21	0.43	-2.76	0.37	-2.81	0.37
Wholesale Trade	-2.56	0.37	-0.57	0.08	-0.02	0.00
Retail Trade	-13.13	2.05**	-12.88	2.01**	-13.61	2.11**
Finance, Insurance and Real Estate	5.45	0.81	7.34	-1.09	7.06	1.04
Service Industry	3.84	0.61	5.23	0.83	4.90	0.77
Other	141.43	13.97**	141.17	13.92**	138.94	13.66**
Single Establishment	14.61	5.78**	12.83	5.06**	12.47	4.91**
Avg. monthly earnings per worker (1990) in $10,000	10.10	5.72**	10.13	5.73**	10.19	5.76**
Constant	-0.47	0.06	115.60	2.89**	5.70	0.52
Observations	73008					
Log Likelihood	-132388.69		-132312.22		-132260.93	
Likelihood ratio test			$152.94 > \chi^2_{(7)} = 14.07$		$255.52 > \chi^2_{(59)} = 79.08$	
* significant at 10% level; ** significant at 5% level						

When compared to non-EZs, the impact on latent average employment in 2000 for establishments located in each of the nine urban EZs, with the exception of Denver, is insignificant (Model 10).[15] The effect on latent employment in 2000 for establishments in the Denver County EZ is significantly negative. In contrast, four of the seven rural EZs (Southeast, South Central, San Luis/Upper Arkansas and Region 10) record positive and significant impacts on latent employment in 2000.

Our evaluation of the effects of Colorado's EZP on latent average monthly establishment employment in 2000 reveals the following. If the program is having any effect, it is bad for large establishments, particularly those with over fifty employees in 1990. At the level of the industries, there is no strong evidence that the EZP is having any major differential impact. The only industries in which we observe a consistent effect is in "other" where the influence is negative and in transportation where the effect is positive. At the level of individual EZs, we observe positive effects in four of the seven rural zones and a negative effect in the Denver County EZ.

Although we do not observe any effect in ten of the fifteen EZs evaluated, we cannot make a definite conclusion on the impact of EZ designation in these zones. We have evidence from the results presented above that the effect of EZ designation varies by employment-size and industry. Therefore, the absence of any effect on establishment-level employment in Adams County EZ, for example, (Table VII) does not mean that EZ designation in Adams County is ineffective. It could be that the effect of Adams County EZ varies by the size of establishments as well as by industry.

[15] Urban enterprise zones are Adams, Arapahoe, Denver, El Paso, Jefferson, Mesa, Pueblo, and Greeley/Weld. These are all the EZs within city limits.

As estimated in Table VII, each individual EZ gets the same differential effect on the latent employment of establishments in 2000 regardless of the size of the establishments. This however may not be the case. Hence, further estimations were done including interaction terms for all fifteen EZs interacted with the ten industries and similarly with the different employment-size class. These models suggest that the negative effects of EZs on larger establishments appear to be driven by significantly negative effects in two urban zones, Arapahoe and Denver, and two rural zones, the San Luis/ Upper Arkansas and South Central EZs.

9. Impact on Average Earnings per Worker

Table: VIII
Dependent Variable: Average monthly wage per worker in 2000

	Model 14		Model 15 Control for population characteristics		Model 16 Control for location using County dummy	
Variables	Coefficient	t-stats	Coefficient	t-stats	Coeffcient	t-stats
3 ≥ Avg. monthly employment (1990)*EZ	52.51	0.40	-258.36	1.80*	-610.44	3.97**
3 < Avg. monthly employment (1990) ≤ 6*EZ	471.98	2.70**	204.14	1.11	-78.66	0.41
6 < Avg. monthly employment (1990) ≤ 10*EZ	578.20	2.66**	364.84	1.64	122.60	0.54
10 < Avg. monthly employment (1990) ≤ 20*EZ	353.40	1.52	216.43	0.91	-45.11	0.19
20 < Avg. monthly employment (1990) ≤ 30*EZ	-105.03	0.29	-172.52	0.47	-424.84	1.16
30 < Avg. monthly employment (1990) ≤ 40*EZ	245.16	0.49	191.27	0.38	-103.97	0.21
40 < Avg. monthly employment (1990) ≤ 50*EZ	270.56	0.44	145.70	0.23	-139.27	0.22
50 < Avg. monthly employment (1990)*EZ	-191.17	0.57	-288.81	0.86	-554.77	1.63
3 ≥ Avg. monthly employment (1990)	-4397.05	20.51**	-4465.38	20.83**	-4495.35	20.97**
3 < Avg. monthly employment (1990) ≤ 6	-2701.39	12.09**	-2785.44	12.46**	-2823.16	12.64**
6 < Avg. monthly employment (1990) ≤ 10	-2031.13	8.62**	-2123.38	9.01**	-2155.92	9.15**
10 < Avg. monthly employment (1990) ≤ 20	-1444.58	5.98**	-1558.56	6.45**	-1575.02	6.52**
20 < Avg. monthly employment (1990) ≤ 30	-925.56	3.14**	-1039.85	3.53**	-1082.50	3.68**
30 < Avg. monthly employment (1990) ≤ 40	-862.92	2.44**	-932.36	2.64**	-927.42	2.63**
40 < Avg. monthly employment (1990) ≤ 50	-801.99	1.87*	-808.10	1.89*	-828.69	1.94*
Mineral Industries	-704.26	1.64	-299.59	0.69	-235.77	0.54
Construction Industries	-335.05	1.14	-211.50	0.72	-105.81	0.36
Manufacturing	-1058.64	3.40**	-734.77	2.35**	-592.34	1.88*
Transportation, Communication and Utilities	88.92	0.28	232.39	0.73	320.49	1.00
Wholesale Trade	99.02	0.34	519.67	1.76*	673.64	2.28**
Retail Trade	-957.38	3.49**	-770.55	2.80**	-691.92	2.50**
Finance, Insurance and Real Estate	454.24	1.58	765.79	2.65**	842.51	2.90**
Service Industry	-97.90	0.36	204.43	0.76	306.79	1.13
Other	3719.47	8.38**	3556.41	7.99**	3481.38	7.80**
Single Establishment	588.41	5.42**	481.26	4.43**	463.15	4.25**
Avg. monthly earnings per worker (1990) in $10,000	1811.51	153.84**	1859.86	154.58**	1855.81	153.83**
Per Capita Income in $10,000			888.71	3.92**		
Unemployment rate			46.63	1.25		
Population in 100,000			-455.85	11.79**		
% High School Graduates			-6.25	0.39		
% College Graduates			3.11	0.27		
% White Population			-21.01	1.06		
% Black Population			-36.22	1.62		
Constant	-3875.84	11.49**	-2127.67	1.47	-4140.85	10.12**
Observations	73008					
Log Likelihood	-200084.37		-199883.50		-199761.08	
Likelihood ratio test			$401.74 > \chi^2_{(7)} = 14.07$		$646.58 > \chi^2_{(62)} = 81.37$	
* significant at 10% level; ** significant at 5% level						

Table VIII reports the results of the models estimating the effect of EZ designation on the latent variable average monthly earnings per worker in 2000. There are a number of similarities in the results presented here and those estimating the effects of EZ designation on average monthly employment in 2000 as presented in Table IV. Similar to the employment results, the coefficients on average wage per worker in 1990,

single establishment and all seven employment-size dummy variables are significant in all three models. The direction of the effects is also the same for these variables as the impact observed on latent employment. Of interest is the significantly negative coefficient observed on all seven employment-size categories, indicating that bigger establishments pay more. The main difference in the results is that the EZ effects observed on latent employment are not the same for wages. If the EZP in Colorado is having any effect on wages based on the size of establishments, it is among the smallest establishments. That is, establishments with three or fewer employees. However, this effect is only marginally significant in Model 14.

Estimations to determine if the effects of EZ designation on wages vary by industries are reported in Table IX. The results in Table IX do not support the result from Table VIII that Colorado's EZP seems to be bad for establishments with at most three employees. We can therefore conclude that there is no evidence that the EZP in Colorado has any effect on latent wages in 2000 at the level of the different employment-size classes of establishments.

At the industry level, there is evidence (Model 16) that, when compared to the agricultural industry located in an EZ, latent wages in 2000 are lower for establishments in the construction, wholesale and retail industries. However, they are higher for establishments in the "other" industry. Controlling for population characteristics and county fixed effects, the apparent EZ effect in the construction, wholesale and retail industries disappears. However, the level of latent wages in 2000 for establishments in the "other" industry that are located in EZs is consistently higher than wages in the agricultural industry. This is true in all the specifications estimated. This result differs

from the impact we saw on latent employment, where the level of employment was lower for establishments in the "other" industry when compared to similar establishments in the agricultural industry.

Table: IX
Dependent Variable: Average monthly wage per worker in 2000

	Model 16		Model 17 Control for population characteristics		Model 18 Control for location using County dummy	
Variables	Coefficient	t-stats	Coefficient	t-stats	Coeffcient	t-stats
3 ≥ Avg. monthly employment (1990)*EZ	902.01	1.64	81.74	0.15	-672.48	1.18
3 < Avg. monthly employment (1990) ≤ 6*EZ	1289.43	2.31**	508.57	0.90	-173.42	0.30
6 < Avg. monthly employment (1990) ≤ 10*EZ	1392.60	2.44**	663.74	1.15	23.61	0.04
10 < Avg. monthly employment (1990) ≤ 20*EZ	1130.18	1.95*	483.53	0.83	-170.68	0.29
20 < Avg. monthly employment (1990) ≤ 30*EZ	676.95	1.05	88.56	0.14	-563.64	0.85
30 < Avg. monthly employment (1990) ≤ 40*EZ	1006.62	1.39	440.62	0.60	-250.11	0.34
40 < Avg. monthly employment (1990) ≤ 50*EZ	1034.74	1.26	387.10	0.47	-298.55	0.36
50 < Avg. monthly employment (1990)*EZ	518.94	0.82	-116.88	0.18	-787.96	1.22
Mineral*EZ	-1392.65	1.58	-1345.63	1.52	-1003.94	1.13
Construction*EZ	-1564.76	2.53**	-939.84	1.52	-521.34	0.83
Manufacturing*EZ	-762.45	1.20	110.35	0.17	640.65	1.00
Transportation, Communication and Utilities*EZ	413.61	0.63	842.82	1.29	1073.03	1.63
Wholesale*EZ	-1174.79	1.96**	-538.83	0.89	-123.67	0.20
Retail*EZ	-699.57	1.24	-210.58	0.37	194.88	0.34
Finance,Insurance and Real Estate*EZ	-889.84	1.48	-467.32	0.77	-61.21	0.10
Service*EZ	-915.27	1.65*	-371.99	0.67	65.82	0.12
Other*EZ	1488.48	1.65*	1736.57	1.93*	1831.53	2.02**
3 ≥ Avg. monthly employment (1990)	-4456.90	20.71**	-4529.40	21.05**	-4559.25	21.18**
3 < Avg. monthly employment (1990) ≤ 6	-2745.85	12.27**	-2835.60	12.67**	-2874.68	12.84**
6 < Avg. monthly employment (1990) ≤ 10	-2075.23	8.79**	-2172.92	9.20**	-2206.41	9.35**
10 < Avg. monthly employment (1990) ≤ 20	-1478.41	6.11**	-1598.74	6.61**	-1616.61	6.69**
20 < Avg. monthly employment (1990) ≤ 30	-954.02	3.24**	-1074.18	3.64**	-1117.49	3.79**
30 < Avg. monthly employment (1990) ≤ 40	-879.16	2.48**	-957.26	2.71**	-954.65	2.70**
40 < Avg. monthly employment (1990) ≤ 50	-799.72	1.87*	-812.61	1.90*	-836.56	1.95*
Mineral Industries	-168.28	0.31	227.68	0.41	160.44	0.29
Construction Industries	176.72	0.48	70.69	0.19	19.34	0.05
Manufacturing	-773.81	1.90*	-818.54	2.01**	-889.86	2.18**
Transportation, Communication and Utilities	-128.66	0.31	-144.83	0.35	-135.44	0.33
Wholesale Trade	566.99	1.51	737.40	1.96**	732.94	1.94*
Retail Trade	-691.29	1.98**	-694.99	1.99**	-769.64	2.20**
Finance, Insurance and Real Estate	782.57	2.17**	917.96	2.54**	842.64	2.33**
Service Industry	240.20	0.70	334.83	0.98	275.73	0.81
Other	2804.04	4.49**	2589.00	4.13**	2528.29	4.04**
Single Establishment	591.80	5.45**	485.47	4.46**	465.82	4.27**
Avg. monthly earnings per worker (1990) in $10,000	1804.77	153.20**	1839.98	153.63**	1831.15	152.69**
Constant	-4138.85	10.43**	-2114.10	1.44	-4047.82	8.84**
Observations	73008					
Log Likelihood	-200067.79		-199869.68		-199749.48	
Likelihood ratio test	$33.16 > \chi^2_{(9)} = 16.92$		$396.22 > \chi^2_{(71)} = 14.07$		$240.40 > \chi^2_{(62)} = 81.37$	
* significant at 10% level; ** significant at 5% level						

Evaluation to determine if the effect of EZ designation on establishment level wages varies by zone reveals some similarities with the effects observed on employment. When compared with non-zone areas, EZ designation is effective in four of the seven rural EZs (Table X), namely Southeast, South Central, San Luis/Upper Arkansas and Region 10. The effect in Region 10 is only marginally significant in Model 21. These are the same EZs in which we observe positive effects on latent employment. However, when we introduced the industry/EZ interaction terms, the positive effects vanished for the South Central and Region 10 EZs. Therefore, the evidence suggests that EZ designation is good for two of the seven rural zones in terms of its effect on latent wage per worker in 2000.

Among the urban zones, the results in Table X suggest that when compared to non-zone areas, EZ designation is bad for the Arapahoe and Denver Counties EZs. The negative effect in Denver holds when the industry/EZ interactions are added. The only industry that seems to be affected by the presence of the EZP is "other". The positive effect on latent wages in the "other" industry when compared to the agricultural industry is a consistent result seen in all the specifications estimated.

Table X

Dependent Variable: Average monthly wage per worker in 2000

	Model 19		Model 20 Control for population characteristics		Model 21 Control for location using County dummy	
Variables	Coefficient	t-stats	Coefficient	t-stats	Coeffcient	t-stats
Adams EZ	-987.82	2.49**	-486.92	1.19	-215.54	0.49
Arapahoe EZ	-1405.07	4.48**	-1337.58	4.19**	-838.75	2.51**
Denver EZ	-1401.92	7.25**	-938.10	4.34**	-1108.83	5.09**
El Paso EZ	-702.72	2.93**	-67.88	0.25	-197.27	0.71
Jefferson EZ	-1132.40	3.26**	-533.11	1.48	-866.19	2.37**
Mesa EZ	-226.01	0.53	32.59	0.07	441.50	0.85
Pueblo EZ	-210.33	0.60	-263.60	0.70	-583.28	1.27
Greeley/Weld EZ	-521.11	1.57	-389.78	1.09	-130.36	0.27
Southeast EZ	2148.03	5.81**	1861.12	4.47**	2485.36	2.59**
South Central EZ	1273.18	3.87**	1182.35	3.09**	2048.82	2.47**
San Luis/Upper Arkansas EZ	1464.17	6.37**	1415.06	5.09**	2054.12	3.50**
East Central/Northeast EZ	1420.58	6.14**	1382.74	4.44**	2434.68	0.96
Northwest EZ	332.66	0.99	108.96	0.30	-916.25	1.31
Region 10 EZ	789.90	2.92**	1003.72	3.04**	1714.31	1.79*
Southwest EZ	-376.13	1.17	-1102.16	3.20**	-227.78	0.25
3 < Avg. monthly employment (1990) ≤ 6*EZ	476.11	2.17**	480.28	2.19**	532.59	2.42**
6 < Avg. monthly employment (1990) ≤ 10*EZ	698.30	2.74**	692.21	2.72**	729.62	2.86**
10 < Avg. monthly employment (1990) ≤ 20*EZ	527.57	1.96**	546.46	2.03**	581.52	2.16**
20 < Avg. monthly employment (1990) ≤ 30*EZ	164.47	0.43	198.05	0.51	230.00	0.60
30 < Avg. monthly employment (1990) ≤ 40*EZ	577.94	1.12	568.52	1.10	536.36	1.04
40 < Avg. monthly employment (1990) ≤ 50*EZ	581.00	0.91	505.94	0.79	525.36	0.82
50 < Avg. monthly employment (1990)*EZ	182.16	0.50	107.41	0.30	103.49	0.29
3 ≥ Avg. monthly employment (1990)	-4405.41	20.55**	-4457.33	20.81**	-4492.08	20.95**
3 < Avg. monthly employment (1990) ≤ 6	-2699.59	12.08**	-2767.69	12.39**	-2817.29	12.61**
6 < Avg. monthly employment (1990) ≤ 10	-2029.54	8.61**	-2101.64	8.92**	-2142.08	9.09**
10 < Avg. monthly employment (1990) ≤ 20	-1440.54	5.96**	-1537.90	6.37**	-1565.05	6.48**
20 < Avg. monthly employment (1990) ≤ 30	-922.69	3.13**	-1023.99	3.48**	-1073.94	3.65**
30 < Avg. monthly employment (1990) ≤ 40	-853.90	2.41**	-919.16	2.60**	-915.99	2.59**
40 < Avg. monthly employment (1990) ≤ 50	-789.86	1.84*	-796.96	1.86*	-819.68	1.92*
Mineral Industries	-387.92	0.90	-201.38	0.47	-263.07	0.61
Construction Industries	6.27	0.02	-54.49	0.18	-90.24	0.30
Manufacturing	-523.46	1.67*	-530.12	1.69*	-569.71	1.81*
Transportation, Communication and Utilities	331.76	1.03	345.70	1.08	327.38	1.02
Wholesale Trade	613.38	2.08**	690.69	2.34**	704.89	2.38**
Retail Trade	-624.18	2.26**	-628.99	2.28**	-686.45	2.48**
Finance, Insurance and Real Estate	806.09	2.79**	861.96	2.98**	826.73	2.85**
Service Industry	285.88	1.05	332.82	1.23	303.86	1.12
Other	3644.89	8.17**	3595.87	8.06**	3474.32	7.78**
Single Establishment	591.16	5.44**	494.98	4.55**	469.39	4.31**
Avg. monthly earnings per worker (1990) in $10,000	1830.08	154.37**	0.18	154.03**	0.18	152.96**
Constant	-4241.52	12.51**	-8.68	0.01	-4422.48	9.20**
Observations	73008					
Log Likelihood	-199932.18		-199818.48		-199748.59	
Likelihood ratio test			$227.40 > \chi^2_{(7)} = 14.07$		$139.78 > \chi^2_{(59)} = 79.08$	
* significant at 10% level; ** significant at 5% level						

10. Conclusion

This paper analyses the effect of Colorado's EZP on establishment-level employment and wages. We observe that, although approximately seventy percent of the land area of Colorado is an EZ, most of the economic activities in terms of the number of establishments operating are taking place in non-zone areas. Colorado's EZ incentives are geared toward attracting new businesses in an effort to provide more jobs and hence reduce the economic blight present in these areas.

We analysed the effect of the EZP at the level of, the employment-size classes of establishments, the ten major industries and the individual EZs. Our estimates indicate that if EZs are having any effect on employment, it is bad for establishments with over fifty employees. There is no evidence that the program has any significant effect on establishments that are smaller. The only industries in which we observed any effect is transportation and "other". The level of latent employment is higher in the transportation industry located in an EZ when compared to the agricultural industry located in an EZ. The opposite is true for the "other" industry.

The individual EZs in which we observe positive effects on employment are four of the seven rural zones, namely: Southeast, South Central, San Luis/Upper Arkansas and Region 10. In contrast, the only urban zone that seems to be impacted by the EZP is Denver; and the effect is negative when compared to non-zone areas.

The effects on wages at the level of the individual zones are similar to the effects observed on employment. When compared to non-zone areas, the level of wages is lower in the Denver EZ, while it is higher in the Southeast and San Luis/Upper Arkansas EZs. The only industry in which we observe any consistent and significant effect on wages is "other". When compared to non-zone areas the level of wages is higher in the "other" industry.

This paper does not evaluate all the possible variables that are likely affected by the EZP, for example, some measure of investment. However, our analysis of employment and wages indicates that the impact of the zone program is minimal. This result is not surprising since we anticipate that any impact the program is likely to have will be evident in immobile factors of production, particularly commercial properties. In this regard, further analysis of Colorado's EZP is encouraged, particularly evaluation of its impact on investment and the value of commercial properties.

Appendix

Enterprise Zone Tax Credits and Incentives

1. **Three percent investment tax credit.** Businesses making investments in equipment used exclusively in an enterprise zone may claim a credit against their Colorado income taxes equal to 3 percent of the amount of the investment, subject to limitations on the amount that can be claimed in any one year. Investment that results from an in-state relocation is not eligible for the credit unless the new location qualifies as an expansion. Excess credits may be carried back three years and forward twelve.

2. **$500 job tax credit.** Businesses hiring new employees in connection with a "new business facility" located in an enterprise zone may claim a tax credit against state income taxes of $500 for each such employee. An expansion of an existing facility may be considered a "new business facility" if the expansion adds at least 10 employees or a 10 percent increase over the previous annual average, if it is at least $1 million in investment, or, if less, at least doubles the original investment in the facility. The credit may be taken in subsequent years of the enterprise zone for each additional employee above the maximum number employed in any prior tax year. Excess credits may be carried forward five years (applies to 3 and 4 below).

3. **Double job tax credit for agricultural processing.** An additional credit of $500 per new business facility employee may be claimed by businesses that add value to agricultural commodities through manufacturing or processing.

4. **$200 job tax credit for employer health insurance.** In order to encourage employer-sponsored health insurance plans, a taxpayer with a qualifying new business facility is

allowed a two-year $200 tax credit for each new business facility employee who is insured under a qualifying employer-sponsored health insurance program.

5. **R&D tax credit**. Taxpayers who make private expenditures on research and experimental activities (as defined in federal tax law) conducted in an enterprise zone qualify for an income tax credit. This credit equals 3 percent of the amount of the increase in the taxpayer's R&D expenditures within the zone for the current tax year above the average of R&D expenditures within the zone area in the previous two years. No more than one fourth of the allowable credit may be taken in any one tax year.

6. **Credit to rehabilitate vacant buildings**. Owners or tenants of commercial buildings in an enterprise zone which are at least 20 years old and which have been vacant for at least 2 years may claim a credit of 25 percent of the cost of rehabilitating each building. The credit is limited to $50,000.

7. **Credit for contributions to zones**. A taxpayer who makes a contribution to certain eligible enterprise zone development projects including business assistance, job training, economic development marketing, community development and homeless organizations in zones may claim a tax credit. To be eligible, a proposed project must be approved by the local enterprise zone administrator and by the Colorado Economic Development Commission. The amount of the credit is 25 percent of the value of the contribution, up to $100,000 (12.5% for in-kind contributions).

8. **Ten Percent Job Training Credit.** Beginning with 1997 tax years, employers who carry out a qualified job training program for their enterprise zone employees may claim an income tax credit of 10 percent of their eligible training costs.

9. **Exemption from state sales and use tax for manufacturing and mining equipment.**

- Manufacturing Machinery
- Machine Tools and
- Machine Parts

  are exempt from the 3 percent state sales and use tax state-wide, regardless of where within the state the equipment is used.

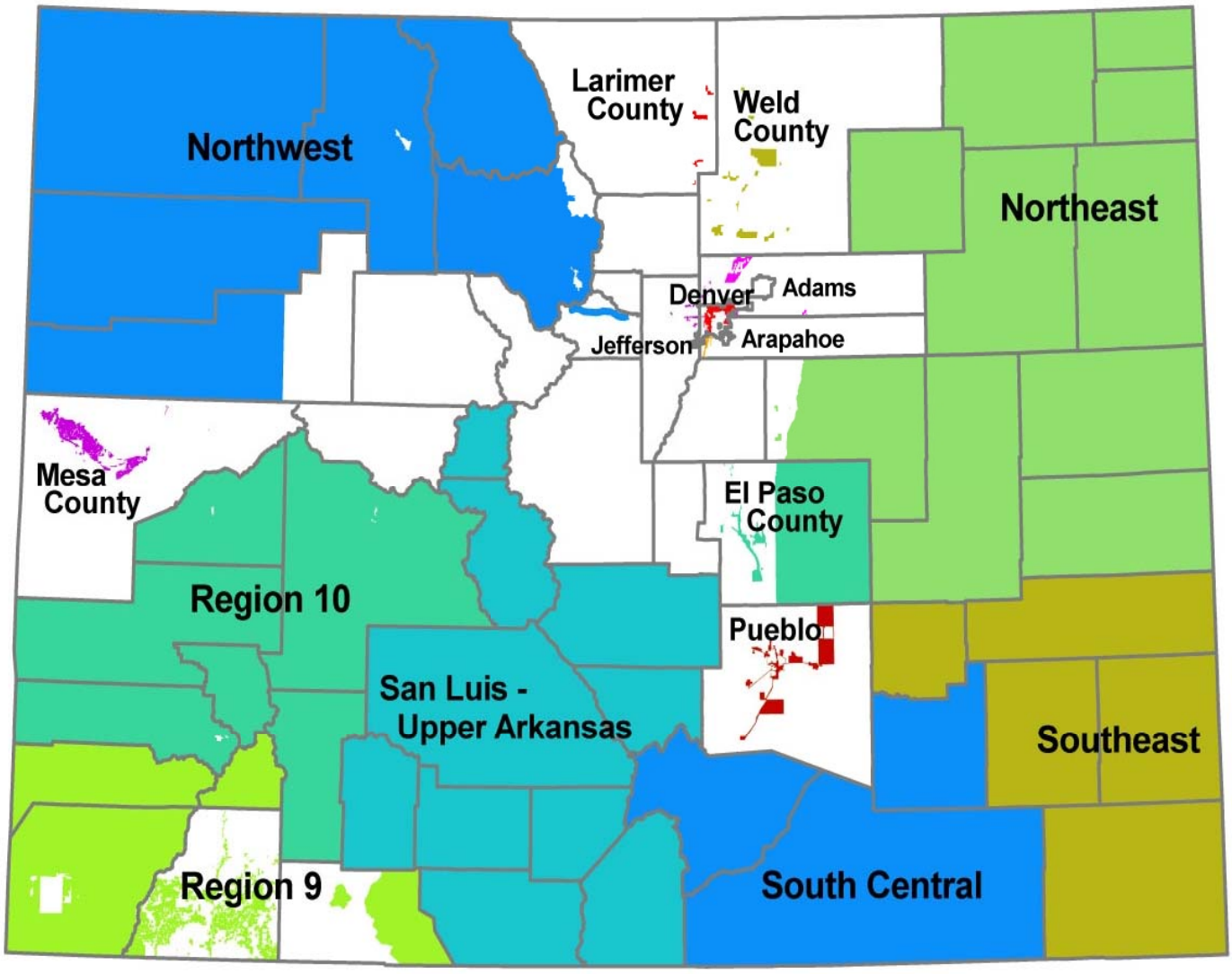
When used solely within an enterprise zone this exemption may also be claimed for purchases of:

- Mining Equipment
- Materials Used to Make Eligible Machinery

Eligible purchases used in an enterprise zone are exempt whether the purchases are capitalized or expensed for accounting purposes.

10. **Local government tax incentives.** Any city or county within an enterprise zone is authorized to negotiate with individual taxpayers who have qualifying new business facilities (a) an incentive payment equal to not more than the amount of the increase in property tax liability over pre-enterprise zone levels; and (b) a refund of local sales taxes on purchases of equipment, machinery, machine tools, or supplies used in the taxpayer's enterprise zone.

Figure 1: A Map of Colorado's Enterprise Zone



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