

Incidence and substitution in Enterprise Zone Programs:

The case of Colorado

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Abstract

GIS techniques distinguish between Colorado establishments that are and are not in enterprise zones in a sample of 73,008. Holding constant industry, initial establishment size, initial payroll per worker and county characteristics, rural enterprise zones increase establishment employment but do not affect payroll per worker, as is expected of local subsidies to mobile factors. Urban enterprise zones reduce employment, implying that substitution effects of subsidies for capital overwhelm scale effects on labor demand. Establishments in industries which receive most zone subsidies perform no better within zones than without. Equilibrium incidence of most enterprise zone subsidies is probably on immobile factors such as commercial real estate.

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Enterprise Zones (EZs) are economically depressed geographic areas where capital and/or labor receive tax preferences with the intent of inducing investment and enhancing employment opportunities. The EZ concept has been attributed to British politicians and academics who were inspired by the performance of some East Asian economies in the 1970s (Peters and Fisher; 2002 pg. 24). The intent of Enterprise Zone Programs (EZPs) was to invigorate the private sector through public subsidies. The British government implemented a ten-year EZP in 1981.

EZPs have been employed in the U.S. since the early 1980s. They have been adopted by the Federal government, approximately forty states and the District of Columbia (Peters and Fisher, 2002). Beck (2001) identified more than 3,500 individual zones. The widespread use of EZs and the costs of their subsidies provide compelling reasons to evaluate their effectiveness. Increased wages and employment would seem to be prerequisites to stimulating economic growth in depressed communities. Accordingly, this paper analyses the impact of the Colorado EZP on establishment-level payroll per worker and employment per establishment.

This paper makes five contributions. Section I reconsiders the question of EZ subsidy incidence. Section IV introduces the other four: First, this paper uses GIS technology to identify EZ boundaries precisely. Second, it reconsiders enterprise zone endogeneity and suggests a new control strategy. Third, it explores establishment behaviour in all industries, rather than only manufacturing. Fourth, it differentiates between the effects of different types of EZs.

Of the remaining sections in the paper, section II reviews previous literature. Section III describes the Colorado EZP. Section V analyzes payroll per worker. Section VI estimates the effects of EZs on employment per establishment. Section VII explores whether these effects are industry-specific. Section VIII distinguishes the employment effects of urban and rural EZs. Section IX concludes.

I. Incidence and scale

Typical EZ subsidies can include investment tax credits, credits for new jobs and for property taxes.¹ If subsidies initially increase the returns to any mobile factor, supplies of that factor within the EZ will increase. This will continue until returns to mobile factors within EZs are identical to those without.

Therefore, in equilibrium, wages for subsidized factors should be no different than wages for the same factors in areas without subsidy. However, employers outside EZs will hire factors only until their marginal products equal their wages, while employers within will hire until marginal products equal the differences between factor wages and factor subsidies. Consequently, factor quantities and ratios may differ for employers within and outside EZs.

Allen (1938, 341-343; reiterated in Hamermesh, 1993, 22-29) suggests the conditions which determine these differences. With two factors and constant returns to scale, the elasticity of demand for labor with respect to changes in the net wage paid by the employer, η_w , is

$$\eta_w = - (1-s)\sigma - s\eta$$

where s represents labor's share in costs, σ represents the elasticity of substitution between labor and capital and η represents the elasticity of output demand.

Changes in the net wage induce a substitution and a scale effect in labor demand. The first, $-(1-s)\sigma$, represents the change in employment as a consequence of the change in optimal factor ratios, holding constant output. The second, $-s\eta$, represents the change in labor demand as a consequence of the change in optimal output levels. If EZ subsidies reduce labor costs to EZ employers, both effects increase labor demand. Employment increases both because it substitutes

¹ Engberg and Greenbaum (1999, table 1) present a tabulation of available subsidy types by state.

for capital and because output increases.

Analogously, the elasticity of demand for labor with respect to capital's net price, η_r , is

$$\eta_r = -(1-s)(\sigma - \eta)$$

If EZ subsidies also reduce the cost of capital to employers, labor demand would again increase as a consequence of the scale effect, $-(1-s)\eta$. However, it would simultaneously decline by $-(1-s)\sigma$, which represents the amount of labor that would be replaced by capital.

If, as is common, EZs subsidize both labor and capital, three effects encourage employment: the substitution effect of the labor subsidy and the scale effects of both subsidies. Only one effect, the substitution effect of the capital subsidy, discourages employment. The net effect depends on the relative sizes of the subsidies and the extent to which labor and capital are substitutable. However, the clear intent of EZ policies is to encourage employment. Therefore, the intent must be that the positive effects dominate.

Nevertheless, any increases in quantities of either factor should not increase equilibrium firm returns. Increases in the employment of mobile factors within EZs will engender concomitant increases in the demand for complementary immobile factors. The price of immobile factors will, consequently, increase. Therefore, the equilibrium incidence of zone incentives should be entirely on immobile factors of production.

In the short run, labor, capital and entrepreneurs may be partially immobile. These factors may benefit initially from the introduction of EZ incentives. However, in the long run, land is probably the only truly immobile factor. Consequently, the long-run equilibrium incidence of EZ subsidies should be on real estate.

Commercial and industrial real estate will benefit directly. In contrast, EZ subsidies have

ambiguous consequences for residential property. If it can be converted to commercial and industrial use, its values should increase, as well. If, however, zoning or other restrictions prevent this conversion, increased commercial and industrial activity may generate externalities that reduce the value of residential property.² Moreover, any increases in EZ employment or wages are unlikely to yield substantial increases in local housing demand because optimal residential and workplace locations rarely coincide and are likely to diverge as wages increase.³

II. Previous evidence

Previous research is, at least, not inconsistent with incidence predictions. Papke (1994) demonstrates that factor-specific EZ subsidies alter factor proportions within enterprise zones in favor of subsidized factors. Erickson and Syms (1986) report dramatic, if descriptive, evidence of price increases for commercial and industrial property within zones and substantial price reductions for similar properties outside of these zones, but on their borders. Although the predicted long-run effects of EZs on factor proportions and the prices of non-residential properties are unambiguous, these appear to be the only papers which address them.

Observed effects of enterprise zones on residential property values are also consistent with incidence predictions, if only because those predictions are ambiguous. Boarnet and Bogart (1996) and Engberg and Greenbaum (1999) find no effect. Greenbaum and Engberg (2000) estimate that housing values and rents grow more slowly within EZs. However, Busso and Kline

² These effects are paradoxical. One motivation for EZ programs is to arrest population decline in targeted areas. If residential property can be converted to commercial and industrial use, successful EZs will reduce the supply of residential property, and quite possibly the resident population. If conversion is prevented, the supply of residential property will be maintained, but at the cost of denying capital gains to incumbent homeowners.

³ Gravelle (1992) anticipates and amplifies some of the discussion in this section. She notes (1992, 11) "(t)he ideal place for business activity to expand would not necessarily be in a residential area".

(2007) and Hanson (2008) find large increases in the values of owner-occupied housing. The differences in these results are probably attributable, at least in part, to differences in the ease with which residential property can be converted to commercial and industrial use and to differences in the externalities from these uses across the different samples.

Lastly, estimates of EZ wage effects do not contradict the presumption that mobility will equate labor returns within and without EZs. O’Keefe (2004) reports insignificant EZ wage effects. Bondonio and Greenbaum (2007) estimate significant negative effects, which suggest that the EZs they examine encourage substitution of lower-skilled for higher-skilled labor.

However, previous research has failed, for the most part, to identify the intended scale effects of EZ subsidies. These effects appear to be positive, as expected, for aggregate zone employment in Papke (1994) and Busso and Kline (2007), and in some specifications for establishments in Billings (2007). O’Keefe (2004) finds initial employment increases in enterprise zones which vanish as zone subsidies near expiry and, presumably, as these increases reduce zone labor returns to levels outside of zones. Bromley and Rees (1988) offer descriptive evidence of employment increases, though they either identify these as relocations from non-zone locations or characterize them as “disappointing”.

These results, though plausible, are anomalous. In contrast, Bromley and Morgan (1985), Boarnet and Bogart (1996), Dowall (1996), Bondonio and Engberg (2000), Greenbaum and Engberg (2000), Lambert and Coomes (2001), Greenbaum and Engberg (2004), Couch, Atkinson and Smith (2005) and Bondonio and Greenbaum (2007) all report that employment levels are no greater within EZs than without.⁴

These results suggest that the geographic distribution of employment is insensitive to EZ

⁴ In addition, Elvery (2007) and Hanson (2008) estimate that subsidies targeted specifically at the employment of enterprise zone residents have little or no effect on resident employment rates.

subsidies. They are clearly inconsistent with explicit EZ objectives. They may be attributable to subsidies which are effectively trivial. Worse, they may be caused by subsidies which so heavily favor capital as to overwhelm the scale effects encouraging additional employment.

However, these results may also be attributable to imprecise identification of EZ beneficiaries. In general, EZs are not confined to the boundaries of pre-existing jurisdictions. Nevertheless, most previous work has relied on these jurisdictions as units of analysis.

Observations in O'Keefe (2004), Rogers and Tao (2004) and Elvery (2007) are census tracts. Papke (1994) examines "taxing districts". Bondonio and Engberg (2000), Greenbaum and Engberg (2000), Moore (2003), Greenbaum and Engberg (2004) and Bondonio and Greenbaum (2007) base their empirical work on zip codes. Boarnet and Bogart (1996) study municipalities and Engberg and Greenbaum (1999) analyze Census places. In all of these examples, the units of observation do not coincide with actual EZs. Consequently, analyses based on these units cannot rely on a clear distinction between observations that are and are not eligible for EZ subsidies.⁵

This ambiguity probably creates a bias in the direction of underestimating the effects of EZs. EZ subsidies may encourage firms originally located near, but outside these zones to relocate within them. In this case, EZs would appear to have negative effects on adjacent areas. If units of observation combine EZ and adjacent territory, any beneficial EZ effects within their borders would be obscured by negative effects outside.⁶

Aggregation may also be responsible for the paucity of positive EZ employment effects.

⁵ Elvery (2007, 12) illustrates the potential for mismeasurement: "on average, less than half of the 1990 population of tracts that contain enterprise zones actually lives in the zones." Units of observations and EZs coincide only in Lambert and Coomes (2001), Busso and Kline (2007) and Hanson (2008), where census tracts are the basis for zone definitions, and Couch, Atkinson and Smith (2005), where these definitions appear to be based on counties.

⁶ These redistributive border effects may be important, themselves, but they cannot be identified unless the research design accurately distinguishes between areas that are and are not within EZs. Billings (2007) explores them using the EZ boundary digitization introduced here.

Firms whose production functions are intensive in subsidized factors should be attracted to zones and should be larger than otherwise similar firms in non-zone locations. However, firms which are intensive in immobile factors should avoid zones and be smaller within them than without. EZ aggregates may dilute positive zone effects on the numbers and scales of the former firms with negative effects on the latter.

Only Bromley and Rees (1988), O’Keefe (2004) and Billings (2007) examine EZ employment effects at the establishment level. As discussed above, all three find these effects to be positive. The analysis below recapitulates the establishment perspective of these papers, but in a context where EZ beneficiaries are identified precisely.

III. Colorado’s Enterprise Zone Program

Colorado’s EZP was introduced in 1986. The number of zones is limited to sixteen. Eight were designated in 1986. The last zone, the Larimer County EZ, was designated in 1993.

Colorado was comprised of 63 counties during the period under analysis. The sixteen EZs contained all of the territory in 35 counties. Eight counties were entirely outside of all Colorado EZs. The remaining 20 counties lay partially within EZs (Colorado State Auditor, 1998). EZs encompassed approximately 70% of the total land area of Colorado.⁷

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” (Colorado State Auditor, 1998). The original statute stipulated that areas designated as EZs must have populations of less than 50,000 and at least one of the

⁷ This proportion is unusually large. Bondonio and Greenbaum (2007, 124) report that 14.6% of the District of Columbia is in an EZ, with lower proportions in the ten states which they analyze in the same paper. However, all of Arkansas is a designated EZ (Greenbaum, 2004, 70).

following conditions: an unemployment rate at least 25 percent above the state average, a population growth rate less than 25 percent of the state average or per capita income less than 75 percent of the state average.⁸ As of 1996, designation for thirteen of the sixteen zones depended on elevated unemployment rates.

Ten different tax credits and incentives are available to EZ firms.⁹ The most valuable is a 3% investment tax credit, offered to businesses investing in machinery and equipment used exclusively in an EZ. Between Fiscal Year 1989 and 2001, this credit represented about 70% of the total subsidies claimed by EZ participants (Colorado State Auditor, 1998).¹⁰

Two of Colorado's EZ tax incentives are directly related to employment. First, firms 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 firms which add value to agriculture commodities through manufacturing or processing.

Between the implementation of the program in 1986 and 2000, accumulated EZ tax credits amounted to approximately \$300 million. The effects of these credits are uncertain. The Colorado Legislative Council (1996), the research office of the Colorado State Legislature, finds evidence that personal income and employment accelerated in the rural EZs relative to the state as a whole. However, they are unable to attribute this acceleration to the presence of the EZP.

Similarly, the Colorado State Auditor (1998) concludes, from EZP audits in 1995 and 1998, that "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

⁸ The population maximum was increased to 80,000 in 1996.

⁹ The authors can provide an Appendix detailing all tax credits and incentives.

¹⁰ The importance of this subsidy suggests that firms within EZs should employ relatively more capital. Unfortunately, data on capital quantities are unavailable.

changes in the zones.” The Colorado State Auditor (2002) observes that EZ areas have shown improvements in both employment and per capita income growth, but, again, cannot conclusively attribute these improvements to the EZ program.

In contrast, Alm and Hart (1998) examine the effects of the Colorado EZP on economic development from 1980 through 1990 using data from the 1970, 1980 and 1990 Censuses of Population and Housing. They conclude that the program had positive and significant impacts on both employment growth and per capita income.

The following sections re-examine the effects of the Colorado EZ program for the succeeding decade, from 1990 through 2000. Following both the programmatic emphasis on improved employment outcomes and the practical importance of unemployment levels in zone designation, they analyze the effects of EZs on changes across this decade in establishment-level earnings per worker and employment.

IV. The empirical context

The empirical analyses of sections V through VIII take advantage of several unique features of the data set constructed here. The first is the precision of EZ identification.

According to the State of Colorado Department of Local Affairs and administrators of the sixteen individual EZs, nine are not coterminous with the boundaries of other jurisdictions. Moreover, all nine contain non-contiguous sites. The data examined below are based on original GIS digitizations of the reported boundaries for Colorado EZs. This innovation identifies treatment areas exactly, rather than as approximations based on existing jurisdictions.¹¹

¹¹ Apparently, only Dowall (1996) has previously taken this approach. He digitized EZ boundaries for the purpose of creating accurate aggregate observations. Engberg and Greenbaum (1999, footnote 2) report that eleven states were not able to accurately describe the boundaries of

EZ subsidies accrue to establishments within EZs, rather than to EZs as aggregates. Consequently, establishments are, arguably, more appropriate units of observation than EZs. The analyses which follow rely on establishment-level data from the ES-202 databases of 1990 and 2000, provided by the Department of the State of Colorado.¹²

The ES-202 databases identify industry, location, quarterly employment and payroll for all establishments with employees subject to Social Security taxation. The comprehensive coverage of these databases allows for analyses of establishments in all industries, in contrast to, for example, the exclusive focus on manufacturing in Engberg and Greenbaum (1999) and Greenbaum and Engberg (2000, 2004). Establishment location, in conjunction with the GIS maps of EZ boundaries, distinguishes unambiguously between those establishments that are and are not eligible for EZ subsidies.

The use of establishment data avoids the risk of endogeneity inherent in EZ analyses at aggregate levels. EZ designation is typically based on some combination of high unemployment, low population growth, low per capita income and low per capita income growth.¹³ These would also be natural outcome variables in any attempt to analyse EZ effectiveness. If these characteristics are persistent, then EZ identification would not be exogenous to the dependent variables of interest. Analysis at the establishment level avoids the consequent bias because EZ designation does not depend on the prior status of any individual establishment.¹⁴

their own EZs. This is indicative of the challenge posed to digitization, to say nothing of monitoring compliance.

¹² Boarnet and Bogart (1996) and O'Keefe (2004) also employ ES-202 data.

¹³ Political considerations may also affect designations (Couch and Barrett, 2004; Hanson, 2008).

¹⁴ In aggregate-level analyses, strategies to control for endogeneity bias include before and after comparisons (Papke, 1994; Greenbaum and Engberg, 2004), control groups comprised of areas eligible for EZ designation (Boarnet and Bogart, 1996; Hanson, 2008), or were later designated as EZs (Moore, 2003; Busso and Kline, 2007) and matching via propensity scores (Greenbaum and Engberg, 2000; O'Keefe, 2004; Billings, 2007; Elvery, 2007).

However, it presents the alternate challenge of selection bias. Firms that locate within an EZ may have unobserved characteristics which predispose them to better performance there than in locations which do not receive EZ subsidies. If so, the measured effects of EZs on member firms may overstate their effects on firms randomly subjected to the treatment of EZ subsidies.

Though plausible, the empirical consequences of this possibility are more ambiguous than previously recognized. By the same logic, establishments which locate outside EZs would also have unobserved characteristics which endow them with better prospects there than within these zones. In this case, the performance of establishments outside of EZs would also overstate that of an establishment placed there randomly.

The parameter of interest is the true difference between the effects of EZs on firms randomly located within and without. In the absence of explicit corrections for selectivity, the estimated difference between establishments in and outside of EZs would depend on this true effect and the difference between the biases in estimated performance within and outside. The sign on this difference is not guaranteed. However, the estimated difference would overstate the unconditional effect of EZs only if selection into EZs was more powerful than selection out of them. Section VI discusses evidence in this regard.

Available data contain no exogenous variables that could plausibly affect establishment location but not establishment performance. Consequently, analyses below cannot identify selection processes explicitly. Instead, they minimize the effects of these processes by omitting all establishments that moved from an EZ to a non-zone location or from a non-zone location to an EZ between 1990 and 2000. Establishment locations which were stable with respect to EZ membership over the period are more likely to be exogenous for the purposes here.

Consequently, the contrasts estimated below are based on comparisons between

establishments whose EZ status did not change between 1990 and 2000. EZ status for all establishments in the Larimer County EZ changed in 1993, when this EZ was established. Therefore, the analyses below omit them, as well.

The ES-202 file for 1990 identifies 110,962 establishments. Of these, the sample examined here omits 20,137 establishments because their 1990 physical addresses are uncertain, 10,522 because their 2000 physical addresses are uncertain¹⁵, 4,835 because they reported zero employment in 1990, 1,241 because their 1990 and 2000 locations differed in EZ status and 1,219 because they were located in the Larimer County EZ. Consequently, the available sample consists of 73,008 establishments. Of these, 17,331 survived from 1990 to 2000 and 55,677 dissolved at some point during the decade.¹⁶

Table 1

Average monthly employment, all establishments

	State		Enterprise Zone		non-zone	
	1990	2000	1990	2000	1990	2000
Average	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
Observations	73,008	73,008	23,200	23,200	49,808	49,808

Tables 1 through 4 present summary statistics for this sample regarding the number of establishments within and outside of EZs and the outcome variables, employment per establishment and monthly payroll per worker. Table 1 reports that, in 1990, this sample contains approximately two non-zone establishments for every establishment located in an EZ.

According to table 1, average employment per establishment in 1990 was approximately the same in both EZ and non-zone areas, though more variable in the latter. In contrast, table 2

¹⁵ The ES-202 files identify mailing addresses rather than physical location for these establishments. Analyses, available from the authors, which include these establishments at their mailing addresses yield results that are very similar to those below.

¹⁶ Lynch and Zax (2008) analyze the determinants of these establishment deaths.

demonstrates that average 1990 nominal monthly payroll per worker was higher among establishments outside of enterprise zones.

Table 2

Average monthly payroll per worker, all establishments

	State		Enterprise Zone		non-zone	
	1990	2000	1990	2000	1990	2000
Average	\$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
Observations	73,008	73,008	23,200	23,200	49,808	49,808

Summary statistics for 2000 in tables 1 and 2 include zeros for establishments that did not survive to that year. Consequently, their interpretation is uncertain. Tables 3 and 4 reproduce the statistics of tables 1 and 2 for the subsample of establishments that survived from 1990 to 2000.

Table 3

Average monthly employment, surviving establishments

	State		Enterprise Zone		non-zone	
	1990	2000	1990	2000	1990	2000
Average	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
Observations	17,331	17,331	6,041	6,041	11,290	11,290

These tables demonstrate, implicitly, that of the 55,677 establishments that died between 1990 and 2000, 17,159 were located in EZs and 38,518 in non-zones. The ratio of deaths within and outside of zones was similar to the ratio of establishments within and without EZs in 1990. Table 3 reports that, in the subsample of surviving establishments, those outside of EZs were larger in both years. Moreover, average establishment size increased by more, both relatively and absolutely, for non-zone than for zone establishments. This is inconsistent with the intended and expected effects of zone subsidies.

According to table 4, the same comparison holds for monthly payroll per worker. Among establishments that existed in both 1990 and 2000, 1990 nominal payroll per worker was 49.4%

greater in those located outside EZs. In 2000, nominal payroll per worker in these establishments was 57.4% greater than in zone establishments.¹⁷

Table 4

Average monthly payroll per worker, surviving establishments

	State		Enterprise Zone		non-zone	
	1990	2000	1990	2000	1990	2000
Average	\$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

V. EZ effects on payroll per worker

The incidence analysis of section I predicts that, in equilibrium, worker compensation should be unaffected by EZ status because EZ employment subsidies pass on to immobile factors. This prediction deserves examination in its own right. Moreover, it is the foundation for the section I predictions regarding EZ employment levels. If, somehow, some of these subsidies actually accrued to workers, their employment effects would be reduced.

At the same time, any examination of this prediction is necessarily limited. First, in the present context, evidence supportive of this prediction would consist of insignificant coefficients on EZ variables in equations predicting payroll per worker. The confirmation provided by acceptance of the null hypothesis is inevitably weak.

Second, payroll per worker is not observable for firms which did not survive until 2000. Survival must be modelled explicitly in order to derive reliable estimates of EZ effects on payroll per worker, unconditional on survival. Unfortunately, no available variables would plausibly

¹⁷ All comparisons in tables 1 through 4 are similar in the sample of footnote 15.

affect firm survival, but not the firm's ability to compensate its employees.¹⁸

Consequently, Heckit models for payroll per worker in this section employ the same explanatory variables in the selection and payroll equations. They are identified solely by functional form. Therefore, they are illustrative only.¹⁹

Table 5 presents three Heckit estimations for 2000 nominal payroll per worker. Each holds constant 1990 nominal payroll per worker. As additional control variables, all three include nine dummy variables which distinguish industries at the two-digit SIC level from agriculture, the reference industry, and a dummy which distinguishes single-establishment firms from establishments which belong to multi-establishment firms.

Models 2 and 3 of table 5 augment this basic specification with controls for characteristics of establishment location. Model 2 includes seven county-level economic and demographic variables from the 1990 Census of Population and Housing: per capita income in ten thousands of dollars, unemployment rate, county population in hundred thousands, the proportion of county adults who are high school and college graduates, the proportion of the entire county population that is white and the proportion that is black. Model 3 replaces these variables with fixed effects for all but one of the sixty-three counties represented in the sample. The additional variables are jointly significant at better than 1% in their respective models.²⁰

All three models control for initial size variations across establishments with dummy variables for 1990 employment size class. This representation is derived from the employment

¹⁸ Conceptually, it is difficult to identify any variables which would have these properties unless the firm is a price-taker in the labor market. However, this cannot be assumed because it is precisely the point at issue here.

¹⁹ Lynch and Zax (2008) discuss the survival equations.

²⁰ The likelihood ratio test which compares models 2 and 1 yields a value of 454.00 with seven degrees of freedom. The test which compares models 3 and 1 yields a value of 754.00 with 62 degrees of freedom.

Table 5

Heckit regressions for 2000 payroll per worker

Dependent Variable: Average monthly payroll per worker 2000

Variables	Model 1		Model 2 Control for population characteristics		Model 3 Control for location using County dummy	
	Coefficient	t-stats	Coefficient	t-stats	Coefficient	t-stats
3 ≥ Avg. monthly employment (1990)*EZ	-2.01	0.02	-235.11	1.69*	-552.69	3.70**
3 < Avg. monthly employment (1990) ≤ 6*EZ	371.85	2.19**	182.77	1.03	-52.25	0.28
6 < Avg. monthly employment (1990) ≤ 10*EZ	551.48	2.62**	408.13	1.89*	204.60	0.92
10 < Avg. monthly employment (1990) ≤ 20*EZ	308.21	1.36	253.75	1.10	36.76	0.16
20 < Avg. monthly employment (1990) ≤ 30*EZ	-130.12	0.37	-138.21	0.39	-364.76	1.03
30 < Avg. monthly employment (1990) ≤ 40*EZ	264.14	0.55	248.26	0.51	-24.72	0.05
40 < Avg. monthly employment (1990) ≤ 50*EZ	370.37	0.62	302.39	0.50	36.06	0.06
50 < Avg. monthly employment (1990)*EZ	-229.78	0.70	-229.78	0.70	-464.36	1.41
3 ≥ Avg. monthly employment (1990)	-4409.80	21.18**	-4472.97	21.51**	-4503.85	21.68**
3 < Avg. monthly employment (1990) ≤ 6	-2682.38	12.36**	-2755.07	12.71**	-2792.03	12.89**
6 < Avg. monthly employment (1990) ≤ 10	-2017.53	8.82**	-2095.12	9.17**	-2130.91	9.34**
10 < Avg. monthly employment (1990) ≤ 20	-1441.69	6.15**	-1553.53	6.64**	-1568.06	6.71**
20 < Avg. monthly employment (1990) ≤ 30	-982.24	3.44**	-1088.49	3.82**	-1124.55	3.95**
30 < Avg. monthly employment (1990) ≤ 40	-897.96	2.62**	-955.67	2.79**	-944.60	2.77**
40 < Avg. monthly employment (1990) ≤ 50	-866.36	2.09**	-868.81	2.10**	-879.94	2.13**
Mineral Industries	-295.58	0.70	74.98	0.18	121.11	0.29
Construction Industries	-164.03	0.57	-67.53	0.24	28.87	0.10
Manufacturing	-855.71	2.83**	-575.30	1.90*	-463.54	1.52
Transportation, Communication and Utilities	96.02	0.31	213.47	0.69	283.80	0.91
Wholesale Trade	231.91	0.82	606.16	2.12**	740.59	2.58**
Retail Trade	-1019.96	3.83**	-867.40	3.25**	-811.52	3.02**
Finance, Insurance and Real Estate	458.47	1.65*	717.79	2.57**	776.47	2.76**
Service Industry	-119.42	0.46	137.58	0.53	219.61	0.83
Other	3418.20	7.98**	3245.27	7.56**	3140.64	7.30**
Single Establishment	482.77	4.57**	386.62	3.65**	369.89	3.49**
Avg. monthly earnings per worker (1990) in \$10,000	2024.44	28.18**	2048.86	28.46**	2046.44	28.51**
Per Capita Income in \$10,000			219.86	4.18**		
Unemployment rate			45.44	1.26		
Population in 100,000			-433.36	11.56**		
% High School Graduates			-2.57	0.16		
% College Graduates			3.77	0.34		
% White Population			-21.56	1.12		
% Black Population			-33.47	1.55		
Constant	-3337.65	10.21**	-1979.84	1.41	-1637.36	0.51
County Fixed Effects		No		No		Yes
Sigma		7288.31		7266.67		7253.78
Log Likelihood		-199563.00		-199336.00		-199186.00

* significant at 10% level; ** significant at 5% level Observation = 78,008

estimations in the next section, which demonstrate that the employment effects of enterprise zones are non-linear in initial employment. Consequently, the estimations here and below represent EZ effects, the effects of interest, through complete interactions between the dummies

for 1990 employment size class and enterprise zone status.

In all three models, monthly payroll per worker in 2000 depends on many establishment characteristics. Not surprisingly, it is significantly higher in establishments with higher 1990 nominal monthly payrolls per worker. The well-known firm-size wage effect (Oi and Idson, 1999, for example) also reappears in these data: Monthly payrolls per worker are significantly and systematically higher in establishments with more employees, with the largest establishments as the reference category. Monthly payrolls per worker are also significantly higher in single-establishment firms. Lastly, they vary significantly by industry.

However, EZs have no significant effects on monthly payroll per worker in establishments with more than ten employees. In model 1 they appear to have positive effects in establishments with four to six and with seven to ten employees. With the introduction of population characteristics in model 2, the first of these effects becomes insignificant, the second weakens in both magnitude and significance and establishments with one to three employees display a negative effect which is marginally significant. With the county fixed effects of model 3, only this last effect remains, increased in both magnitude and significance.²¹

In sum, these results are generally consistent with the expectation that workers do not enjoy the incidence of EZ subsidies. Positive EZ effects on payroll per worker are limited to two of the eight establishment size classes and are not robust to the introduction of controls for local

²¹ Regressions for 2000 payroll per worker on the subsample of surviving establishments also estimate significant negative EZ effects for the smallest establishment size class using the specifications of models 2 and 3. With the specification of model 1, these regressions estimate significant negative payroll effects for establishments with one to three, four to six and fifty or more employees. Heckit models which replace EZ-establishment size interactions with a single EZ dummy estimate EZ effects on monthly payroll per worker which are significantly positive in model 1, insignificant in model two and negative with marginal significance in model 3. These models, available from the authors, confirm that, at least with controls for location characteristics, there is no evidence that the incidence of EZ subsidies is on workers.

area characteristics. The only EZ effect which is robust to these controls is the reduction in EZ payrolls per worker in the smallest establishments.

Superficially, this last effect suggests that workers in the smallest establishments actually suffer, at least relatively, within EZs. In the absence of controls for worker quality, it may indicate instead that the smallest EZ firms substitute lower- for higher-quality workers.

EZ employment tax credits encourage this substitution because they are invariant to wage, and therefore represent a reduction in the relative price of low-wage to high-wage labor. However, this effect is present for establishments of all size classes. The differential response in the smallest establishments might therefore be attributable to differences in production functions across establishment size. The capital receiving EZ subsidies in the smallest establishments may be more substitutable for high-wage labor and more complementary with low-wage labor than is the subsidized capital required by larger establishments.

VI. EZ effects on employment per establishment in 2000

Section V demonstrates that EZ subsidies do not increase, and in the smallest establishments may even decrease, payrolls per worker. This certainly does not contradict the incidence analysis of section I. Moreover, it supports and reinforces the employment predictions there: employment in otherwise identical establishments should be greater within EZs than without.

This section examines those predictions. As in the previous section, this examination must incorporate the information embodied in the majority of establishments which did not survive to 2000. For this purpose, the statistical explanations for 2000 establishment employment levels take the form of tobit models.

These models assume that the true variable of interest is the latent “net position with

respect to the labor market”. Establishments with positive employment in 2000 are purchasers of labor in that year. For them, observed employment is the true value of this variable.

In contrast, establishments with zero employment in 2000 have either zero labor demand or would prefer, notionally, to supply labor. In the first case, the data again report the exact value of the latent variable. In the second case, the data report the censored value of zero when the true value of the latent variable is negative. The tobit model estimates the determinants of this latent variable, based on the censored dependent variable measuring observed employment in 2000.²²

Table 6 presents the first three of these models. The explanatory variables in these three equations are identical to those of the parallel equations in table 5.

Model 4, the first model of table 6, demonstrates that initial employment has non-linear effects on final employment. The largest establishments, those with more than 50 employees in 1990, comprise the omitted category. Estimated coefficients for all other 1990 employment categories are negative, significant, and increasing in algebraic value with 1990 establishment employment. This implies, not surprisingly, that establishments in smaller 1990 employment size-classes had systematically lower employment in 2000. This implication is present in all subsequent models.

Model 4 demonstrates that other control variables also have significant influence over employment per establishment 2000. It was higher in establishments with higher 1990 payrolls per worker, perhaps because they were initially more successful. It was also higher in single-establishment firms, perhaps because the presence of additional establishments within the firm indicates that individual establishments had already reached optimal size. Both of these effects persist through all subsequent models.

²² The firm-level regressions of O’Keefe (2004) also control for community characteristics and fixed effects. However, they contain fewer firm characteristics than those here.

Table 6

Tobits for 2000 employment with nonlinear employment effects

Dependent Variable: Average monthly employment in 2000

Variables	Model 4		Model 5 Control for population characteristics		Model 6 Control for location using County dummy	
	Coefficient	t-stats	Coefficient	t-stats	Coefficient	t-stats
3 ≥ Avg. monthly employment (1990)*EZ	9.86	3.18**	-0.51	0.15	-8.23	2.27**
3 < Avg. monthly employment (1990) ≤ 6*EZ	14.56	3.56**	5.31	1.24	-1.24	0.28
6 < Avg. monthly employment (1990) ≤ 10*EZ	15.69	3.08**	7.84	1.50	2.30	0.43
10 < Avg. monthly employment (1990) ≤ 20*EZ	8.34	1.53	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	7.94	0.71	4.03	0.35	-2.33	0.20
40 < Avg. monthly employment (1990) ≤ 50*EZ	4.75	0.33	-0.37	0.03	-7.01	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**	-176.27	35.62**	-177.09	35.74**
6 < Avg. monthly employment (1990) ≤ 10	-159.41	30.41**	-161.63	30.79**	-162.32	30.91**
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	-121.97	15.11**	-122.64	15.29**	-122.82	15.31**
40 < Avg. monthly employment (1990) ≤ 50	-110.05	11.33**	-110.38	11.37**	-110.82	11.41**
Mineral Industries	-44.72	4.34**	-35.03	3.39**	-33.16	3.19**
Construction Industries	-13.24	1.94*	-9.84	1.43	-7.22	1.04
Manufacturing	-23.89	3.32**	-15.75	2.17**	-12.41	1.69*
Transportation, Communication and Utilities	-8.88	1.19	-5.43	0.73	-3.15	0.42
Wholesale Trade	-14.65	2.14**	-4.35	0.63	-0.69	0.10
Retail Trade	-20.92	3.29**	-16.05	2.51**	-13.75	2.13**
Finance, Insurance and Real Estate	-2.71	0.41	5.11	0.76	7.29	1.08
Service Industry	-5.11	0.82	2.32	0.37	4.88	0.77
Other	142.89	14.17**	140.03	13.84**	139.14	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			1.08	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**	-8.06	0.94
County Fixed Effects	No		No		Yes	
Sigma	173.37		173.30		173.27	
Log Likelihood	-132540.97		-132364.30		-132269.77	

* significant at 10% level; ** significant at 5% level Observation = 78,008

Most importantly, EZ effects on employment per establishment vary by employer size-class. Estimated coefficients for the three smallest establishment size-classes are positive and significant. This suggests that the smallest establishments in 1990 had significantly higher levels

of employment in 2000 if they were located in EZs than if they were not. At the same time, the negative significant coefficient for the largest establishment size-class indicates that the largest establishments in 1990 were larger in 2000 if they were located outside of enterprise zones.²³

Models 5 and 6 of table 6 demonstrate that county characteristics and fixed effects, themselves, contribute significantly to the explanation of 2000 employment.²⁴ They do not appreciably alter the magnitude or the significance of the estimated effects of 1990 payroll, single-establishment status or 1990 establishment size-class. In addition, they impose some stability on the industry effects. In their presence, 2000 employment is significantly lower in the mineral, manufacturing and retail industries, both in table 6 and in all subsequent tables.

The most important consequence of the augmented specifications in models 5 and 6 is the disappearance of the positive EZ effect on employment in the three smallest establishment size-classes. The effect for the smallest size-class in model 5 is negligible in magnitude and significance. Both the effects and the t-statistics for establishments with four to six and with seven to ten 1990 employees are less than one-half of their values in model 4.

²³ Preliminary estimations with alternative techniques are generally consistent with these results. Heckits analogous to those of table 5 estimate a significant positive EZ effect on establishment survival. This is presumably most important for the small 1990 establishments which exhibit positive EZ 2000 employment effects in model 4. They also reproduce the significant negative EZ effect on 2000 employment in the largest 1990 establishments. Tobit is preferable to Heckit here both because Heckit identification relies on functional form and because, in contrast to the Heckits of table 5, the survival dependent variable in the selection equation is simply a censored version of the dependent variable of interest, 2000 employment. Regressions for 2000 employment per establishment among surviving establishments also exhibit the significant negative EZ effect among the largest 1990 establishments. They do not reproduce the negative EZ employment effects on the smallest 1990 establishments, presumably because these were at greatest risk of exiting the sample through failure to survive. Regression is less appealing than tobit precisely because it ignores the information about EZ effects on changes in employment levels embodied in those establishments whose 2000 employment declined to zero.

²⁴ The likelihood ratio test which compares models 5 and 4 yields a value of 353.34 with seven degrees of freedom. The test which compares models 6 and 4 yields a value of 542.40 with 62 degrees of freedom. Both tests are significant at better than 1%.

The county fixed effects of model 6 reduce these effects even further. There, the coefficients for establishments with four to six and with seven to ten 1990 employees are small in magnitude and less than half the size of their standard errors. The coefficient for the smallest establishments is now statistically significant and of approximately the same magnitude as in model 4, but of opposite sign.

These results demonstrate that the apparent positive effects of EZs on the smallest establishments in model 4 are spurious. The smallest establishments appear to enjoy greater success in EZs because these zones are located in communities which are favorable for small establishments. EZs, themselves, have no independent positive effects on these establishments. At the same time, models 5 and 6 reproduce the significantly negative EZ effect on employment in establishments with more than 50 1990 employees of model 4.

These models suggest that EZ subsidies must encourage substitution of capital for labor. In establishments of most sizes, this substitution is sufficient to counteract any scale effects on labor demand. Among the largest employers, it actually dominates these scale effects. The effects in these establishments may be distinct because the production functions associated with their size permit greater substitution.

Table 6 also provides informative bounds on the potential role of selection bias. As discussed in section IV, the typical concern is that positive selection bias into EZs will exaggerate their estimated effects. The results in Table 6 and its successors below demonstrate that there is little scope for positive selection bias in the data examined here. Positive estimated EZ effects are rare in the three models and probably spurious. If even these are exaggerated by

selection, then the true effects must be truly negligible.²⁵

VII. Employment by Industry

EZ incentives in most states are targeted at particular industries, especially manufacturing.²⁶ Manufacturing establishments received 36% of the subsidies awarded under the Colorado EZP, retail establishments received 11% of these subsidies and establishments in agriculture received 10% (Colorado State Auditor, 1998). This raises the question of whether EZs have sectoral employment impacts which are commensurate with the distribution of EZ subsidies.

Table 7 explores this question by augmenting the models of table 6 with interactions between the EZ dummy variable and industry dummies. These interactions do not change any of the substantive results in table 6. The effects of single-establishment status, 1990 payroll, 1990 establishment size-class and the interactions between enterprise zones and 1990 establishment size-class are almost all of similar magnitude and significance in each model of table 6 and in the corresponding model of table 7.²⁷

Nevertheless, the nine interactions between industry and enterprise zones contribute significantly to explanatory power.²⁸ However, among the three industries receiving the largest shares of these subsidies, the preponderance of the evidence in table 7 indicates that EZs have no employment effects.

²⁵ The results here would understate the true effects of EZs only if selection out of these zones was more important than selection into them. This would require the unlikely condition that unobserved benefits of locations outside of EZs to establishments choosing them exceed the unobserved benefits of EZ locations to member establishments.

²⁶ Erickson and Friedman (1990), analyzing a survey of 357 EZs between 1982 and 1987, claim that manufacturing accounted for 73% of new jobs created.

²⁷ Table 7 omits population variables for brevity. Their effects are very similar to those of table 6.

²⁸ The likelihood ratio tests which compare models 7 and 4, models 8 and 5 and models 9 and 6 yield, respectively, chi-square values of 25.48, 27.70 and 28.94, with nine degrees of freedom. Each of these tests is significant at better than 1%.

Table 7

Tobits for 2000 employment with industry-EZ interactions

Dependent Variable: Average monthly employment in 2000

Variables	Model 7		Model 8 Control for population characteristics		Model 9 Control for location using County dummy	
	Coefficient	t-stats	Coefficient	t-stats	Coefficient	t-stats
3 ≥ Avg. monthly employment (1990)*EZ	23.61	1.85*	0.07	0.01	-16.36	1.23
3 < Avg. monthly employment (1990) ≤ 6*EZ	27.33	2.11**	4.85	0.37	-10.32	0.77
6 < Avg. monthly employment (1990) ≤ 10*EZ	27.99	2.11**	6.97	0.52	-7.13	0.52
10 < Avg. monthly employment (1990) ≤ 20*EZ	20.11	1.50	0.76	0.06	-13.55	0.98
20 < Avg. monthly employment (1990) ≤ 30*EZ	11.11	0.74	-6.52	0.43	-20.79	1.36
30 < Avg. monthly employment (1990) ≤ 40*EZ	19.67	1.17	2.70	0.16	-12.06	0.71
40 < Avg. monthly employment (1990) ≤ 50*EZ	17.51	0.93	-0.91	0.05	-16.08	0.84
50 < Avg. monthly employment (1990)*EZ	-15.95	1.10	-34.09	2.33**	-48.35	3.26**
Mineral*EZ	-6.30	0.30	-3.90	0.19	3.61	0.17
Construction*EZ	-25.45	1.78*	-10.12	0.70	-1.28	0.09
Manufacturing*EZ	-4.80	0.33	17.02	1.15	28.50	1.91*
Transportation, Communication and Utilities*EZ	16.79	1.11	27.86	1.83*	32.30	2.10**
Wholesale*EZ	-12.43	0.89	4.58	0.33	13.19	0.93
Retail*EZ	-11.27	0.86	0.68	0.05	8.97	0.68
Finance, Insurance and Real Estate*EZ	-9.02	0.65	1.35	0.10	9.82	0.69
Service*EZ	-17.65	1.37	-4.08	0.31	4.94	0.38
Other*EZ	-51.17	2.52**	-46.42	2.28**	-44.67	2.18**
3 ≥ Avg. monthly employment (1990)	-214.80	45.32**	-216.46	45.62**	-217.32	45.74**
3 < Avg. monthly employment (1990) ≤ 6	-174.71	35.29**	-176.78	35.66**	-177.57	35.78**
6 < Avg. monthly employment (1990) ≤ 10	-159.67	30.42**	-162.02	30.82**	-162.70	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	-121.01	15.09**	-122.52	15.30**	-122.97	15.32**
40 < Avg. monthly employment (1990) ≤ 50	-110.17	11.35**	-110.70	11.40**	-111.19	11.44**
Mineral Industries	-42.45	3.19**	-33.29	2.50**	-34.23	2.57**
Construction Industries	-4.88	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	-9.86	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	167.48	12.10**	163.95	11.82**	163.23	11.76**
Single Establishment	14.77	5.85**	12.78	5.05**	12.59	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**	-3.70	0.38
County Fixed Effects	No		No		Yes	
Sigma	173.28		173.19		173.15	
Log Likelihood	-132528.23		-132350.45		-132255.30	

* significant at 10% level; ** significant at 5% level Observation = 73,008

In all three models of table 7, the effects of EZs on establishment employment in retail trade are insignificant. Therefore, they are not distinguishable from those for agriculture, the

reference industry. Moreover, the effects for mineral; wholesale; finance, insurance and real estate and service industries are also insignificant in all three models. They are similarly indistinguishable from those in agriculture and retail trade, even though these latter two industries receive much larger shares of EZ subsidies.

EZ-manufacturing interactions are insignificant in models 7 and 8. That in model 9, positive and significant at 10%, offers the only suggestion that industries receiving the bulk of EZ subsidies might benefit commensurately. This is also the only support here for the assertion (Colorado State Auditor, 2002) that manufacturing was the only Colorado industry in which EZ establishments were more successful than establishments in non-zone areas.

However, positive employment effects, significant at 10% in model 8 and at 5% in model 9, also appear for the transportation, communication and utilities industry. At the very least, these results demonstrate that large EZ subsidies are not prerequisites for the few positive industry-specific EZ employment effects. Moreover, the joint significance of industry-EZ interactions depends heavily on the significant negative effects for the residual “Other” industry in all three models. Taken together, these results suggest that EZ incentives to substitute capital for labor, although perhaps inadvertent, are effective across many, if not all industries.²⁹

VIII. Heterogeneous EZ effects

As in prior literature, the preceding analyses assume that all EZs exhibit common effects. However, as discussed in section III, employers who add value to agricultural commodities may be able to claim an additional employment credit. These employers are, presumably, largely rural. Therefore, the effects of rural and urban zones may differ.

²⁹ Further estimations, available from the authors, demonstrate that differential EZ effects by employment-size class are not concentrated in particular industries.

Table 8 examines this proposition. It replaces the interaction between the EZ dummy and the smallest employment size-class in the models of table 6 with separate dummies for urban and rural EZs.

Once again, this elaboration of the specification in table 6 does not alter the magnitudes or significance levels of the principal results in that table. In all three models of table 8, the effects of single-establishment firms and 1990 payroll per worker are significant and positive. The effects of all establishment size-class dummies are negative, significant and increasing in size-class. EZ effects on employment in the largest establishments are negative and significant. Lastly, variables for county characteristics and county fixed effects make statistically significant contributions to the explanatory power of models 11 and 12, respectively.³⁰

However, the models in table 8 demonstrate conclusively that the employment effects of rural and urban EZs are distinct. Those for urban EZs are significant and negative in all three models. The effects for rural EZs are positive and significant in models 10 and 11. They are positive but insignificant in model 12, presumably because the rural EZ effect there is identified only by the eleven rural counties which were split into EZ and non-zone areas.³¹ These results demonstrate that the absence of EZ employment effects in tables 6 and 7 is an artefact of specifications which constrain the urban and rural effects to be equal.

Moreover, these effects are relatively large. For example, models 11 and 12 imply that

³⁰ The likelihood ratio tests which compare models 11 and 12 to model 10 yield chi-square values of 164.30 and 313.96 with seven and 62 degrees of freedom, respectively. Both are significant at better than 1%.

³¹ The effects of the 35 remaining counties which are entirely in rural EZs are absorbed by county dummies. The likelihood-ratio test comparisons between models 4 and 10 and models 5 and 11 yield chi-square statistics of 230.94 and 41.90, respectively, each with one degree of freedom and better than 1% significance. The chi-square statistic for the comparison between models 6 and 12 is insignificant at 2.50. However, this is solely attributable to the insignificance of the rural EZ dummy. The urban EZ effect is -9.72, significant at better than 1%, in the absence of the dummy for rural EZs.

Table 8

Tobits for 2000 employment with urban and rural EZ effects

Dependent Variable: Average monthly employment in 2000

Variables	Model 10		Model 11 Control for population characteristics		Model 12 Control for location using County dummy	
	Coefficient	t-stats	Coefficient	t-stats	Coefficient	t-stats
Urban EZ	-14.52	4.11**	-8.89	2.45**	-9.31	2.52**
Rural EZ	33.25	9.62**	17.58	4.01**	8.36	0.76
3 < Avg. monthly employment (1990) ≤ 6*EZ	5.96	1.16	6.15	1.20	7.06	1.37
6 < Avg. monthly employment (1990) ≤ 10*EZ	9.04	1.52	9.19	1.54	10.60	1.77*
10 < Avg. monthly employment (1990) ≤ 20*EZ	3.29	0.52	3.90	0.62	4.46	0.71
20 < Avg. monthly employment (1990) ≤ 30*EZ	-4.22	0.47	-2.86	0.32	-2.28	0.26
30 < Avg. monthly employment (1990) ≤ 40*EZ	6.78	0.57	7.34	0.62	6.26	0.53
40 < Avg. monthly employment (1990) ≤ 50*EZ	2.39	0.16	2.33	0.16	1.53	0.11
50 < Avg. monthly employment (1990)*EZ	-28.85	3.61**	-29.63	3.70**	-30.43	3.79**
3 ≥ Avg. monthly employment (1990)	-213.97	45.33**	-215.29	45.56**	-216.46	45.74**
3 < Avg. monthly employment (1990) ≤ 6	-174.09	35.23**	-175.79	35.53**	-177.01	35.73**
6 < Avg. monthly employment (1990) ≤ 10	-159.23	30.38**	-161.09	30.69**	-162.25	30.89**
10 < Avg. monthly employment (1990) ≤ 20	-143.94	26.76**	-146.10	27.13**	-146.95	27.27**
20 < Avg. monthly employment (1990) ≤ 30	-122.80	18.54**	-124.92	18.84**	-126.14	19.01**
30 < Avg. monthly employment (1990) ≤ 40	-120.90	15.09**	-122.35	15.26**	-122.79	15.30**
40 < Avg. monthly employment (1990) ≤ 50	-109.76	11.31**	-110.14	11.34**	-110.74	11.40**
Mineral Industries	-40.32	3.90**	-34.82	3.36**	-33.12	3.18**
Construction Industries	-7.29	1.06	-8.06	1.17	-7.14	1.03
Manufacturing	-14.00	1.93*	-13.12	1.81*	-12.21	1.67*
Transportation, Communication and Utilities	-4.69	0.63	-4.18	0.56	-3.07	0.41
Wholesale Trade	-5.28	0.77	-2.32	0.34	-0.55	0.08
Retail Trade	-15.04	2.35**	-14.59	2.28**	-13.67	2.12**
Finance, Insurance and Real Estate	3.58	0.53	6.12	0.91	7.34	1.08
Service Industry	1.73	0.28	3.69	0.59	4.95	0.78
Other	140.95	13.94**	139.97	13.82**	138.94	13.67**
Single Establishment	13.92	5.52**	12.44	4.91**	12.37	4.88**
Avg. monthly earnings per worker (1990) in \$10,000	9.90	5.60**	10.13	5.73**	10.22	5.78**
Constant	1.96	0.26	87.74	2.61**	-8.06	0.94
County Fixed Effects	No		No		Yes	
Sigma	173.36		173.31		173.27	
Log Likelihood	-132425.50		-132343.35		-132268.52	

* significant at 10% level; ** significant at 5% level Observation = 73,008

urban EZs reduce the probability of establishment survival until 2000 by approximately 1.3 percentage points. Among those which survive, urban EZs reduce their employment levels by approximately 1.8 workers.

From table 3, this reduction represents nearly 10% of average 1990 employment among EZ establishments which survived to 2000. On average, these establishments grew by 4.03 employees. Average growth among surviving non-EZ establishments was 5.67 employees, or

1.64 employees greater. Therefore, the difference in average growth rates between EZ and non-EZ establishments is entirely explained by the employment reduction associated with urban EZs.

In contrast, the model 11 effect for rural EZs implies that survivor rates among member establishments were 2.7 percentage points higher than among other establishments. Employment in surviving establishments increased by 3.7 workers, or nearly as much again as the average employment gain among all surviving EZ establishments.

These results suggest that, for firms in urban enterprise zones, the large investment tax credit and small employment tax credit yield net substitution effects against labor. These substitution effects are so large as to overwhelm the scale effects of EZ benefits which would otherwise encourage employment.

If the price of capital is equal in rural and urban areas but rural wages are lower than urban wages, employment tax credits will constitute a larger reduction in the price of rural labor relative to that of capital than in the price of urban labor. This effect will be augmented if rural establishments benefit disproportionately from the employment tax credit for agricultural processing. Both of these considerations would discourage the substitution of capital for labor in rural firms. The models of table 8 indicate that whatever substitution remains is dominated by scale effects, to yield net positive rural EZ employment effects.³²

EZ effects may differ by individual EZ, as well as by EZ category, because each of the Colorado EZs was encouraged to adopt distinctive economic development objectives (Colorado State Auditor, 2002, 27). Tobits, available from the authors, which replace the dummies for urban and rural EZs with individual dummy variables for each of the fifteen EZs represented in

³² It is also possible that labor and capital are less substitutable in production functions for rural activities than in those of urban activities. However, any such distinction would have to occur within the two-digit industries represented in the models of table 8.

the sample examine this proposition. These models yield similar estimates of the effects of control variables to those of their counterparts in table 8, but are statistically superior.³³

Eight of the sample EZs lie within municipal boundaries. Four have negative significant effects on 2000 employment without population controls but these effects persist for only two, Arapahoe and Denver, in their presence. The negative effect for Denver, alone, survives with county fixed effects. This EZ apparently bears principal responsibility for the consistent negative urban EZ effect in table 8.

In contrast, six of the seven rural EZs have significant positive employment effects without and with controls for county population characteristics. Even with county fixed effects, the Southeast, South Central, San Luis/Upper Arkansas and Region 10 EZs have significantly positive employment effects. These results indicate that the positive rural EZ effects of table 8 appear in many individual rural EZs.³⁴

IX. Conclusion

The empirical analyses in sections V, VI, VII and VIII demonstrate, first, that returns to labor are essentially identical within and without EZs. The incidence of EZ subsidies is therefore elsewhere, and presumably on real estate alone.

Second, any common EZ effects are negligible. In tables 5, 6 and 7, where all EZs are represented by a single dummy variable and its interactions, almost all EZ effects are small and statistically insignificant. The remaining effects are anomalous.

³³ The likelihood–ratio test comparisons between models 10, 11 and 12 and their counterparts with separate dummy variables for all 15 EZs yield chi-square statistics of 73.62, 62.30 and 23.72, respectively. Each test has thirteen degrees of freedom. The first two are significant at better than 1%. The last is significant at better than 5%.

³⁴ The models of table 5, expanded to identify separate effects for urban and rural EZs, and for each EZ, are available from the authors. They indicate that payrolls per worker in urban and rural EZs were not significantly different from each other or from payroll per worker outside of EZs.

Third, these analyses demonstrate conclusively that the absence of common EZ effects conceals strong effects by zone type. The employment tobits of table 8, which distinguish between urban and rural EZs, are statistically superior to those which allow only a single EZ effect.

The effects of rural enterprise zones in table 8 are also consistent with simple incidence expectations. In at least four of the seven rural enterprise zones, employment per establishment is significantly greater than outside of these zones. This suggests that subsidies in these EZs yield substitution effects in favor of capital which are dominated by substitution and scale effects in favor of labor.

However, urban EZs have either no or negative effects on workers per firm. This suggests that on net, EZ subsidies favor capital rather than labor in the context of urban production functions. This suggestion requires further validation, however, because EZ subsidies are common to rural and urban areas and different EZ effects across these areas occur within two-digit industry.

In sum, the Colorado Enterprise Zone program has achieved some “success” in expanding the scale of employment in rural enterprises. This success is qualified because, as Gravelle (1992, 10) states, "(w)ith very mobile labor, the overall result of enterprise zones would probably be to move production locations, with little effect on the relative incomes. In this case, the primary effect will be inefficiency in the location of investment." Even this degree of “success” is absent in urban areas, where, if anything, Colorado EZs encourage the replacement of labor with capital.

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