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Leslie E. Papke

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ABSTRACT

In the last decade, most states have targeted certain depressed areas for revitalization by providing a combination of labor and capital tax incentives to firms operating in the "enterprise zone" (EZ). Despite the large number of state initiatives, and the frequent re-introduction of federal EZ legislation, there have been few statistical analyses of the effect of EZs apart from surveys of plan administrators. This paper analyzes the effect of the Indiana EZ program on local employment and investment using a panel of local taxing jurisdictions.

In 1988, the direct budgetary costs of the Indiana program totalled over $11 million, averaging $13,933 per participating firm, $4,564 per new job, and $31,113 per new zone resident job. I estimate that zone designation initially reduces the value of depreciable personal property by about 13 percent, but also reduces unemployment claims in the zone and surrounding community by 19 percent. Both estimates are statistically significant. The value of inventories in Indiana zones is 8 percent higher than it otherwise would be, and the estimated effect is marginally statistically significant.

Leslie E. Papke
Department of Economics
Michigan State University
Marshall Hall
East Lansing, MI 48824
and NBER

1. Introduction

The concept of urban enterprise zones (EZs) returned to the national agenda with the introduction last year of the Bush Administration's EZ Jobs-Creation Act of 1990. It has been part of subnational economic development strategy, however, since the early 1980s. At last count, 37 states (and the District of Columbia) have established some form of EZ initiative. While they differ in specifics, all of the programs provide tax preferences to capital and/or labor and other development incentives in an attempt to induce investment expansion or location, and to enhance employment opportunities for residents in depressed urban areas.[1]

Our understanding to date of the effect of these programs has been gathered from surveys of zone administrators or participating firms.[2] These are primarily studies of the political economy of zones and their successful design, where success is measured by the number of participants.[3] While they provide valuable data on zone characteristics, these studies cannot address the first relevant question about EZs: Did zone designation improve economic conditions in and around the zone?

This paper pursues this question by analyzing the effects of the Indiana EZ program on local employment and investment with a panel of local taxing jurisdictions. The Indiana program includes employment incentives which are

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[1]Occasionally, EZs are designated in rural areas. See Erickson and Friedman (1990).

[2]Green (1990) summarizes the related literature.

[3]Most of these studies focus on the administrative design of the program, for example, the degree of state versus local involvement. See Sheldon and Elling (1989) for an example. One important and untested difference in programs is that some use marginal tax incentives (a tax credit on new investment, for example) and others do not (Indiana's inventory tax credit is an example).

typical of other state EZ programs, and has been operating long enough to generate several years of post-EZ data. Several of the survey studies conclude that Indiana's program is one of the most successful.[4] A second important issue which should be addressed -- the cost effectiveness of EZ programs -- is only introduced here, in a summary of the direct budgetary costs of the program.

Because of their location-specific nature, EZs can become valuable tools for evaluating the effectiveness of tax incentives as economic development policy, and can add to the longstanding debate on the effects of tax competition in the location of capital investment.[5] One criticism of tax competition between states is that any tax-induced investment may by only relocation from another state. Tax competition is then a zero sum game for the country as a whole. The issue of net capital investment is less pertinent to the effects of EZ incentives since redistribution even within the state may be an end in itself.[6] If investment is relocated from local labor markets with low unemployment to local labor markets with higher unemployment, the incentives may generate efficiency gains for the economy as under-utilized resources are tapped.

The next section reviews the survey-based literature, and describes the characteristics of the typical EZ in the United States. Section 3 summarizes

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[4]See Sheldon and Elling (1989).

[5]See L.E. Papke (1991) for a review of this debate.

[6]EZ investment may give individuals employment experience that enhance their long run employability. Then even a relatively short run economic development program may have longer run effects (See Bartik, 1991). EZ programs are not one time shocks, but rather continue for many years. Since my panel includes several years of post-EZ data, it may be possible to determine if the effects of designation persist.

characteristics of participating firms in the Indiana EZ program. Participants have been required to register annually with the Indiana Enterprise Zone Board since 1986, and report the value of the tax credit claimed and employment during the year. Section 4 presents econometric evidence on the effect of zone designation on investment and employment using a panel of local taxing jurisdictions. The final section summarizes and concludes.

2. What are the characteristics of the typical EZ?

The most comprehensive survey of local EZ coordinators is summarized by Erickson and Friedman (1990a). They analyze data from 17 states collected by the Department of Housing and Urban Development (HUD). I summarize some of their findings below to illustrate typical zone economic conditions.

Most zones are poor areas with high rates of unemployment. Erickson and Friedman report that most zones experienced negative population growth prior to designation (between the years 1970 and 1980). The median unemployment rate reported was 14.4 percent, with a mean of 16.0 percent. Unemployment in the surrounding community at designation was also well above the national rate, which ranged from 9.7 percent in 1982 to 7.0 percent in 1986. Median family income in the zones in 1979 was less than 60 percent of the comparable national figure, and the average proportion of families in poverty was over three times the national mean and 70 percent higher than that in the host community Minority residents composed 45 percent of the typical EZ population, about double the proportion in the host community and nearly three times the national average.

Zones are relatively small in area and population size. The sample mean

resident population was 14,500 but the median is 4,500 persons -- close to a census tract. The median zone size is 1.8 square miles, and 75 percent of zones are less than 5.6 square miles, although the mean is 25.6.

Zones have different land-use patterns than their surrounding communities. The average share of industrial land (18.1 percent) was over twice as large as in the host community, and the share of commercial land (15.3 percent) is about 70 percent higher than in their host communities.

Erickson and Friedman estimate that one-third of the zones in their survey achieved gross job growth rates that were higher than the national rate over comparable time periods. They conclude that this is a reasonable achievement for a severely economically depressed core surrounded by a depressed community.

Other studies measure the success of EZ programs by the number of participating firms. Sheldon and Elling (1989) compare the EZ programs in Illinois, Indiana, Ohio, and Kentucky. They analyze the effects of different levels of management involvement on the number of firms receiving benefits. They conclude that the number of administrative staffing hours is associated with the greatest number of participants, but that zone designation had a moderate impact at best. For Indiana, they report that 31 percent of firms receiving benefits were start-ups, 57 percent were expanding firms, and 12 percent were relocating. (They do not indicate whether these are local relocations.) They conclude that the Indiana program has been the most successful of the four in attracting participation.

Erickson and Friedman (1990b) use the HUD data to estimate the effect of state policy variables (such as marketing techniques by the state, number of incentives offered, number of zones designated, number of zone criteria) and

zone characteristics (unemployment rate, poverty rate, land area, population) on the reported number of jobs created or saved in the zone, and on the number of firms investing in the zone. While they find nothing conclusive about employment, they report that the number of incentives offered is positively related to the number of participating firms. The number of zones in a state, and the number of qualifying criteria are negatively related to participation. The policy variables are not statistically significant.

Reliance on the surveys of zone coordinators for measures of EZ success is problematic. As the authors above acknowledge, zone administrators may have an incentive to rationalize the existence of the zone program. Apart from that possibility, however, reported utilization rates are insufficient to distinguish economic activity due to zone designation from that which would have resulted in the absence of the program.

While the Indiana EZ tax credits began in 1984, participating firms have been required to report credits claimed and employment figures to the Indiana Enterprise Zone Board beginning in 1986. The next section describes the Indiana program and summarizes the three available years of registration data.

3. The Indiana Enterprise Zone Program

Indiana's EZ program was modeled and adopted in 1983 on the wave of enthusiasm and fanfare of the Reagan Administration's submission of federal EZ legislation in 1981. Initially, areas in six Indiana central cities were designated as zones; subsequently, others were added to bring the current total to 14. Each zone has a ten-year duration, subject to renewal.

To qualify for zone consideration, the area must have an unemployment rate at least 1.5 times the average statewide unemployment rate, and a

resident household poverty rate at least 25 percent above the U.S. poverty level. Its resident population must be between 2,000 and 8,000 persons and its geographic area is between three-quarter and three square miles, all with a continuous boundary.

Like EZ initiatives in other states, Indiana's incentives target both labor and capital. While there is no explicit statement of its goals in the original legislation, presumably the intention of the program is to increase both employment and investment. The employment tax credits provided are similar to those in other states, but the capital incentives are unusual, as explained below.

Several tax concessions are provided under the Indiana program.

1. Zone firms may claim a tax credit against local property tax liability equal to 100 percent of the property tax imposed on all inventories located in the zone.

Along with machinery and equipment, inventories are a component of the business tangible personal property tax base.[7] Eight states do not tax any tangible personal property, and another twenty-five states specifically exempt inventories. Thus, Indiana is one of a minority of states which impose a local property tax on inventories, although neighboring Ohio and Kentucky also tax inventories.

2. Zone firms may claim a total exemption from the corporate gross income (receipts) tax of all incremental income (receipts) derived from sources within the zone after the designation base year.

Indiana's gross income or receipts tax is a tax on instate receipts. Corporations pay the greater of this and the corporate net income tax (where

[7]Unlike machinery and equipment, inventories are not depreciable.

profits are allocated to the state based on the three-factor formula). Typically, a small firm will pay the gross income tax since no deductions are taken in computing tax liability. Eligibility for the exemption, then, requires that the enterprise be legally organized as a corporation; sole proprietorships, partnerships, and Subchapter S corporations do not qualify because they are not liable for the gross income tax. Further, if the sale giving rise to the incremental income is outside of the zone, it is ineligible for the exemption.

3. Lenders may claim a tax credit of 5 percent of interest income received from loans to zone businesses and residents for residential or business real property improvement. Existing loans qualify for the credit, as well as new loans. Lenders claiming the credit need not be located in the zone.

4. An income tax credit may be claimed by employers hiring zone residents of an amount equal to 10 percent of wages with a ceiling of $1,500 per qualified employee. This ceiling is not indexed and has not been raised since 1984.

5. Zone residents are allowed an income tax deduction equal to one-half of their adjusted gross income with a ceiling of $7,500.[8] This ceiling is also not indexed and has not been raised since 1984.[9]

These employment tax credits are typical of other EZ programs. They are designed to increase employment in the zone, and in particular the employment

[8]A sixth incentive, which has not yet been claimed on individual tax returns, is an income tax credit of up to 30 percent to individual investors for the purchase of stock in start-up or expanding zone businesses.

[9]The analysis in this paper is limited to the business tax incentives included in the EZ program, since the Indiana Department of Revenue does not presently collect separate data on this item from individual income tax returns.

and income of zone residents. Tax credits are valuable to both profitable and unprofitable (i.e., non-taxpaying) firms alike. However, since the dollar amounts of both the tax credit for firms, and the deduction for employees, are capped and not indexed for inflation, these incentives will lose much of their value over the life of the zone.

The capital incentives are more generous. The gross income tax exemption, since it is based on the increase in gross income, represents a partial barometer of the economic growth in the zone. It does not encourage sales outside of the zone, however.

The most lucrative capital incentive, the inventory tax credit, is unusual in several respects. First, zone programs in other states typically provide incentives for investment in machinery or equipment rather than inventories.[10] U.S. inventories are sharply procyclical, as is well known, and it is not clear what a large stock on inventories represents. Further, the stock of inventories held will vary with the production process of the firm. Thus, the value of the credit will vary by type of firm.[11]

In addition, the inventory tax credit applies to the stock of inventories in each year, not just to marginal investment. The local governments likely spent more than they needed to if they wanted to subsidize new investment.

The ITC does enhance cash flow for firms which hold inventories. Since the price of holding inventories has fallen, any zone firm will find it

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[10]Proposed federal legislation calls for tax credits for new investment in structures.

[11]For example, instrument manufacturers and retailers hold about 25 percent of total capital in inventories, compared to five percent for manufacturers of petroleum products and providers of business services.

optimal to hold a higher level of inventories than if it were a non-zone firm. The ITC will increase profits of zone firms, and may compensate them for noncapitalized profit-reducing characteristics of their EZ location. This may increase economic activity in the zone, and stimulate investment in machinery and equipment which would not have occurred in the absence of the program.[12]

An Overview of Zone Participants

Since 1986, zone businesses receiving tax savings are required to register annually with the State Enterprise Zone Board. The registration forms include questions on tax credits claimed, and on the hiring practices of zone firms. Table 1 shows the number of participating business by major industry group, organization type, and firm size for each of the three available years.[13] The number of firms averaged about 1000 each year, although there was an 11 percent drop in participation between 1987 and 1988. Retailers are the largest group, accounting for about one-third of participants. Business and professional service enterprises ranked second, followed by manufacturers and wholesale distributors.

There have been some changes in the industry mix over the three year period. The fraction of manufacturing firms rose from 17.9 to 21.3 percent, and wholesale distributors rose from 12.4 to 15.9 percent. Eighty percent of firms are corporations.

About two-thirds of participants have fewer than 20 employees. Firms reported 2,897 new jobs created in 1988, with 14.7 percent of those jobs going

[12]Some have argued that these areas are in such economic distress that maintaining the existing levels of employment and investment are desirable EZ goals.

[13]See J.A. Papke (1990) for more detailed participant characteristics.

to zone residents. On average, the zone residents were paid about half as much as the other employees. Most zone businesses are locally oriented, with 90 percent of total receipts derived from transactions within Indiana, and 74 percent derived from transactions within the zone.

The 1988 direct budgetary cost of the Indiana EZ tax preferences amounted to $13.6 million, of which 84 percent was attributable to the inventory property tax credit. The following lists the breakdown of direct budgetary costs (foregone tax revenue) in 1988 by program provision and level of government[14]:

	State	Local
	(in thousands)	
Inventory tax credit		11,459.1
Gross income tax exemption	480.4	
Employee expense credit	390.3	
Loan interest credit	892.7	
Personal income tax deduction	411.9	
	$2,175.3	$11,459.1

Table 2 details the dollar value of the EZ tax preferences for different size businesses. The average tax savings for all firms totaled $13,933 in 1988. The average tax preference rises with the size of the business. For example, the average tax savings for the smallest firm was $4,106, while the largest firms claimed $98,493 on average.

Table 3 presents tax savings by major industry. Manufacturing firms reported the largest number of tax savings each year, over 50 percent of the total. In 1988, manufacturers received 62 percent of the tax saving value of the total package of EZ subsidies, and their average of $40,335 was about three times the all-industry average.

[14]These direct budgetary costs are based on the credits and exemptions claimed on the 1989 registration forms filed by program participants in 1989, and on property tax data provided by the Indiana State Board of Tax Commissioners.

Since one purpose of the EZ program is to encourage job creation, particularly the hiring of zone residents, tax savings relative to the reported number of jobs created are also reported in Table 3. The average tax subsidy per new manufacturing job was $4,306. Expressed per new job going to the targeted group, zone residents, the tax savings were $29,203 per new zone resident job.

The inventory tax credit, the most generous incentive, is a credit against the local property tax. Since the property tax is a residual tax, when the credit reduces the assessed value of taxable property the amount is shifted onto other local sources. The amount of the shift will vary with the fraction of inventories of the firms located in the different zones. Table 4 presents estimates the shift in the property tax burden by zone, per new job and new zone resident job.[15] In 1983, the average cost per new job ranged from a high of $11,747 in Hammond to $815 in Madison.

4. The Impact of the EZ Program on Investment and Employment

This section presents econometric evidence on the effect of the EZ program on local employment and investment. At the time of this writing, a total of 10 zones have been in operation for at least four years, 6 of these zones for five years. Thus, it is possible to include several years of post-designation data. Since EZs are not a defined taxing jurisdiction or separate

[15]These property tax data were provided by the Division of Tax Review of the Indiana State Board of Tax Commissioners. For example, the 1987 assessed value of the exempt inventories in the Elkhart EZ was $4.2 million, or 2.7 percent of the total taxable property values in Concord township. To make up this loss in tax base, $331 thousand of taxes were shifted to the remaining non-exempt properties. Without the exemption, the gross tax rate in the district would have been $9.7807 (in dollars per $100 of assessed value); with the exemption, the actual tax rate was $9.8870, an increase of 1.1 percent.

governmental unit, conventional economic data are not available by zone. Instead, I use data from the smallest (in terms of geographic area) jurisdiction that surrounds the zone.

I estimate the effects of EZ designation on two types of capital. Inventories are examined since they are targeted by the investment incentives. In addition, if the program succeeds in increasing business activity in the zone, investment in machinery and equipment may increase.

The value of inventories and depreciable personal property (machinery and equipment) is obtained from the Division of Tax Review, Indiana State Board of Tax Commissioners. Annual data, 1981-1989, are provided for 16 taxing districts, 10 of which eventually included an EZ.[16] A taxing district is a geographic area within which property is taxed by the same taxing unit and at the same total rate (Indiana Code 6-1.1-1-20). It is generally smaller in area than a township and is close in size to an EZ.

Labor market effects of zone designation are estimated with data on annual unemployment claims.[17] Claims from January 1980 to December 1988 for 22 unemployment claims offices are included in the sample. Ten of these offices eventually served an EZ. Because each office serves the zone and surrounding city, a change in zone employment may not be reflected in these

[16]The six non-zone taxing districts are selected on the basis of similar industrial employment composition to the zones and data availability.

[17]Unemployment claims by office are provided by the Indiana Department of Employment and Training Services. Actual unemployment claims may better reflect local labor market conditions than city employment figures since (1) unemployment claims are by nearest office, and zone and near-zone workers would apply there; and (2) city employment estimates are based on a relationship between the county and city which is calculated only once every 10 years.

data.[18] However, any spillover effects from the zone into the community's labor market will be captured.

Tables 5 and 6 present summary statistics for machinery and equipment, inventories, and unemployment claims from 1981 to 1989. The jurisdictions averaged about $49 million in machinery and equipment, $38 million in inventories, and about 8,000 unemployment claims per year. The statistics are also reported separately for zones and non-zones, but comparisons between the two are not valid for assessing EZ effects (even if the zones were selected randomly) since zone designation occurs in later years.

The question of EZ effectiveness could be easily addressed if the programs were administered as traditional experiments. That is, areas would be randomly selected and the effect of the program would be measured by comparing the performance of the experimental and control groups. However, actual EZ designation is based on economic performance, so the data are nonexperimental in the usual sense. Nevertheless, by controlling for sample selection, the data can be used to address the counterfactual question: How did zones perform relative to what would have been their performance in the absence of zone designation?

The correct estimation technique for an experiment is determined by the assumptions about the nature of the data. As discussed above, if zones are selected randomly, the effects of the program are consistently estimated by a cross-section comparison of means between the control and experimental groups. No time series variation is necessary. Alternatively, if data are available only for the experimental group, but are available both before and after the

[18]In the sample, the average city size is 28.9 square miles, and ranged from 16.7 to 39.2. Zones (which can be no larger than three square miles) represent about 10 percent of land area on average.

experiment, then means can be compared across time. In this case, consistent estimation does not require random selection, but it does assume that all changes across time are attributable solely to the experiment -- there are no external influences.

I have panel data on zones before and after designation, as well as non-zone jurisdictions, so I can control for aggregate or external influences over time with aggregate time effects. Since EZs are selected on the basis of depressed economic conditions, I estimate three specifications which allow for different types of sample selection. In particular, the specifications allow for EZ designation to be correlated with unobservables affecting economic performance. All three models include jurisdiction fixed effects.[19] The fixed effects take account of permanent differences across zones which are likely to influence designation. For example, zones may vary with respect to industrial composition and characteristics of the labor force.

In the second specification, in addition to the fixed effects, I allow selection to be based on jurisdiction-specific growth rates. This allows for zones to grow at different rates and allows program designation to depend on these growth rates.[20] The third specification allows designation to be based

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[19]Estimates with and without fixed effects demonstrate that unobserved effects associated with capital investment are positively correlated with zone designation (these results are available upon request). This may occur, for example, if existing industrial composition was considered in the selection process (e.g., more manufacturing firms may have been included as survey literature indicates).

Estimates with and without fixed effects on the unemployment data indicate that unobserved factors associated with large unemployment claims are positively correlated with EZ designation. This is not surprising, given the selection criteria of the program.

Aside from sample selection issues, previous research has demonstrated the importance of controlling for jurisdictional fixed effects in measuring the influence of tax incentives on economic development (see L.E. Papke (1991)).

[20]Heckman and Hotz (1989) refer to this as the "random growth model."

on lagged values of the dependent variable as well as the zone-specific time invariant unobservables (fixed effects). For example, this allows zone designation to depend on the level of employment or investment in the previous period.

The most basic model is given by equation (1).

$$\log y_{it} = \alpha_i + \beta t + \delta EZ_{it} + u_{it} \qquad (1)$$

EZ_{it} equals 1 if jurisdiction i is a zone in year t, and 0 otherwise. The variable y_{it} is either the annual level of inventories, machinery and equipment, or unemployment claims. The coefficient on the EZ dummy, when multiplied by 100, measures the percentage change in inventories, for example, due to zone designation. This specification includes an linear time trend βt, and the α_is control for unobservables that are time-invariant over the sample period and may be correlated with zone designation.[21]

If the unobservables u_{it} are uncorrelated over time and are homoskedastic, equation (1) is efficiently estimated by fixed effects. However, the unobservables are unlikely to be uncorrelated. An alternative approach in the presence of likely persistent and positive serial correlation is to first difference equation and estimate the resulting equation (2) below by OLS.

$$\Delta \log y_{it} = \beta + \delta \Delta EZ_{it} + \Delta u_{it} \qquad (2)$$

Table 7 (first column of each pair) reports the estimated coefficients on the

[21]Since I am primarily interested in consistent estimation of the EZ effect, it is not necessary to include other covariates provided that they are not systematically related to zone designation, after controlling for fixed effects, random growth rates, and aggregate time effects in the specifications.

EZ dummies.

The estimates indicate that zone designation leads to a 9.8 percent decline in machinery and equipment, and an 8.3 percent increase in inventories. Both estimates are statistically significant at the five percent level.

The most valuable tax incentives apply to the stock of inventories so the positive coefficient in the inventory regression is not surprising. However, the approximately 10 percent drop in machinery and equipment suggests a substitution by zone firms away from machinery and equipment and toward inventories. Evaluated at the means of the sample, this is equivalent to about a $5 million drop in the value of depreciable personal property, which is not nearly offset by the $3.2 million increase in the value of the (now tax-free) inventories.

Using equation (2), EZs are estimated to reduce unemployment claims at the nearby claims office by 25 percent relative to what they would have been without the program. The coefficient is statistically significant at the five percent level. This is a striking effect, suggesting that there are substantial spillovers to the host community's labor market from EZ designation.

I test whether the unobservables in (1) are positively correlated by regressing the residuals from the differenced equation on lagged values. If there was strong positive correlation in the levels, then there should be little in remaining in the differenced equation. If, however, there was no positive correlation to begin with, then differencing induces negative correlation. The AR1 coefficient in the inventory regression is -0.178 (with a standard error of 0.104); in machinery and equipment, -0.261 (0.116), and in

unemployment claims, -0.317 (.007). These coefficients are statistically different from 0 which is the value expected in the case of a random walk. However, the estimate is also different from -0.5, which is expected if there were no correlation in the errors in (1). There is no strong case, then, for either the fixed effects technique or first difference estimation. However, first difference estimation is the conservative approach since any induced serial correlation would be negative, and thus the reported OLS standard errors would tend to be overestimates.

To get an idea about the effects of serial correlation on the standard errors, I calculate the robust standard errors in Wooldridge (1989), allowing for heteroskedasticity and first order serial correlation. As expected, these are all actually smaller than the OLS standard errors, reflecting the negative correlation induced by first differences. For machinery and equipment, the robust standard error on the EZ dummy is 0.02796, for inventories, 0.0239, and for unemployment claims, 0.0836.

The second specification, the random growth rates model given by

$$\log y_{it} = \alpha_i + \beta_{1i}t + \beta_{2t} + \delta EZ_{it} + u_{it}, \qquad (3)$$

allows zone selection to be based not only on the level of activity α_i, but on the growth rates β_{1i} as well. This generalization is desirable if, for example, fast-growing or slow-growing areas are more likely to be selected. This second model is more general than the first in another dimension in that aggregate time effects, β_{2t}, replace the linear time trend.

As with equation (1), equation (3) is first-differenced to obtain equation (4):

$$\Delta \log y_{it} = \beta_{1i} + \Delta\beta_{2t} + \delta \Delta EZ_{it} + \Delta u_{it}. \quad (4)$$

If the u_{it} show strong positive autocorrelation then equation (4) should have approximate white noise errors; it is estimated by standard fixed effects.

Results from estimating equation (4) are presented in the first columns of Table 8. The estimated effects of zone designation in this more general model are quite similar to those obtained with the first model. Zones are estimated to have 13 percent less machinery and equipment, 8 percent more inventories, and 19 percent fewer unemployment claims than they would have had as non-zones.

Specifications (1) and (3) control for varying degrees of sample selection, but they impose the restriction that zone designation has the same effect in each year after designation. That is, EZ designation causes a permanent shift in the level of activity in the zone, relative to its non-zone state. This may be too restrictive if the influences of the incentives change over time. This restriction is relaxed in

$$\log y_{it} = \alpha_i + \beta t + \delta_1 EZYR1_{it} + \delta_2 EZYR2_{it} + \ldots + \delta_5 EZYR5_{it} + u_{it}. \quad (5)$$

This is an extension of equation (1) which allows the effect of zone designation to vary over its life. The EZ dummy is replaced by a series of dummy variables for each year of zone designation; for example, $EZYR2_{it}$ takes on the value 1 if jurisdiction i has been a zone for two years in year t, and 0 otherwise. The same estimation technique is used; that is, equation (5) is first-differenced to obtain

$$\Delta \log y_{it} = \beta + \delta_1 \Delta EZYR1_{it} + \delta_2 \Delta EZYR2_{it} + \ldots + \delta_5 \Delta EZYR5_{it} + \Delta u_{it}, \qquad (6)$$

is which estimated by OLS.

Results are reported in the second columns of Table 7. Unfortunately, the data are not very informative about the time pattern of zone effects (partly due to the sparseness of the data in later periods). The point estimates suggest that the effect persists over the five years. For both inventories and unemployment claims, the hypothesis that all the coefficients are equal does not come close to being rejected (with p-value .78 and .73). That is, the permanent shift model (1) is supported for both inventories and unemployment claims.

Estimates of (6) can also be used to test whether zone designation has only an initial first-year effect. In the inventory regression, when the coefficients on EZYR2-EZYR5 are set to zero, the r-squared is .009, which translates into an F-test against (6) with a p-value of .33. So, this hypothesis can not be rejected at traditional levels either. It is clear, however, that the permanent shift model given by (1) explains more variation in the data (with an r-squared of .035). For unemployment claims, the first-year effect model has an r-squared of .028, which translates into a test with a p-value of .68. The permanent shift model explains slightly more variation in the data (with an r-squared of .029).

For machinery and equipment, the test that all the EZ coefficients in (6) gives a p-value of .097, marginal evidence against the null hypothesis of a permanent shift. For the first-year effects case -- when the coefficients on EZYR2-EZYR5 are restricted to be zero -- the r-squared in the first-differenced equation is .106 (giving a significance level of .65 against (6)),

compared to .059 for the permanent shift model. Apparently, for machinery and equipment, a model which imposes a first-year effect explains the data better than one which imposes a permanent shift. This is good news from a policy perspective, since the drop in machinery equipment associated with zone designation appears to occur in the first year only. Several more years of post-EZ designation data would help confirm this possibility.

I extend the random growth model (3) in a similar fashion:

$$\log y_{it} = \alpha_i + \beta_{1i}t + \beta_{2t} + \delta_1 EZYR1_{it} + \ldots + \delta_5 EZYR5_{it} + u_{it}. \qquad (7)$$

The corresponding first-differenced equation,

$$\Delta\log y_{it} = \beta_{1i} + \Delta\beta_{2t} + \delta_1 \Delta EZYR1_{it} + \ldots + \delta_5 \Delta EZYR5_{it} + \Delta u_{it}, \qquad (8)$$

is estimated by fixed effects.

The estimates from this equation, reported in the second columns of Table 8, tell a similar story. For machinery and equipment, neither the permanent shift nor the first-year effect model is rejected against the model with unrestricted EZ year effects. As with model (1), the first-year effect model fits the data better, with an r-squared of .301 compared with .287 for the permanent shift model. For inventories, neither restriction is rejected. The permanent shift model fits slightly better (an r-squared of .274 versus .268 for the first-year effect model).

The difference between the permanent shift and first-year effect models is more pronounced for unemployment claims. The first-year effect model is rejected against (8) at the 8 percent level (with an r-squared of .627); the

permanent shift model is not rejected at any level less than 30 percent (with an r-squared of .636).

To summarize, there is evidence that zone designation has a longer-run effect on the local labor market. Unemployment claims in communities surrounding zones experience a permanent decline. That is, each year after designation, claims are about 19 percent lower than they would have been in the absence of the zone program. On the other hand, the data support the conclusion that zones have a first-year only decrease in machinery and equipment, although this is barely distinguishable from a permanent decline hypothesis. The estimates suggest that zone inventories undergo an permanent increase but again the statistical tests reveal nothing conclusive.

Finally, to account for the possibility that designation is based on the lagged value of the dependent variable before designation, I estimate a third specification:

$$\log y_{it} = \alpha_i + \beta_{2t} + \rho \log y_{i,t-1} + \delta EZ_{it} + u_{it}. \tag{9}$$

To estimate (9), the equation is first-differenced. The resulting equation

$$\Delta \log y_{it} = \Delta\beta_{2t} + \rho \Delta \log y_{i,t-1} + \delta \Delta EZ_{it} + \Delta u_{it} \tag{10}$$

is estimated by instrumental variables using $\Delta \log y_{i,t-2}$ as an instrument for $\Delta \log y_{i,t-1}$ since $\Delta \log y_{i,t-1}$ and Δu_{it} are correlated.

Using this third specification, zones are estimated to have about 14 percent less machinery and equipment (with a coefficient on the EZ dummy of -0.135 and a standard error of 0.045), 6.8 percent more inventories (0.068

(0.052)), and almost 22 percent fewer unemployment claims per year (-0.219 (0.106)).

The estimated effects of the EZ program are similar in all three specifications. The estimated ρ is small and insignificant in the third specification for each of the three dependent variables; thus the more general version of the other specifications, the random growth model, is preferred.

5. Summary and Conclusion

This paper presents estimates of the effect of an EZ program on unemployment and capital investment in the zone and surrounding community. The estimates indicate that the Indiana EZ program has permanently increased the value of inventories by about 8 percent in the zones relative to what it would have been without the program. However, the value of machinery and equipment is reduced by about 13 percent. This drop could be a transitory one-time adjustment, but the imprecision of the estimates make it difficult to determine; the point estimates indicate a permanent downward shift.

While the value of the capital tax preferences exceed those claimed for employment, zone designation appears to have a large impact on the local labor market. This may reflect a demonstration effect described by zone administrators. I estimate that unemployment claims decline by about 19 percent following designation. At the mean of unemployment claims, this is about 1,500 fewer claims per year. The evidence for a permanent effect on unemployment claims is stronger than that for capital. These estimates are statistically significant in all models.

This paper does not address the indirect tax revenue effects of the EZ program, which would be necessary for a complete accounting of the program.

However, it is worth noting the implications of the capital investment estimates for the local property tax base. My estimates suggest that zone designation further reduces the local community tax base by a drop in the (taxable) value of machinery and equipment, over and above the direct loss in revenue due to the inventory tax credit.

Direct assistance to business may be the most controversial type of state and local economic development policy. Tax concessions transfer most of the discretionary authority for a public program to a non-public third party -- the firm, in the case of EZs. These tax expenditure policies should be evaluated in terms of their employment and investment effects, and their cost to the state and local community. These evaluations would serve as a useful guide for federal legislation.[22]

I regard these results as a first step toward an evaluation of EZ performance apart from survey evidence. A useful extension of this research would be a similar type of analysis for EZ programs in other states. Estimates from states with different degrees of overlap between data jurisdictions and zone boundaries would add confidence to our findings, and allow us to test the breadth and duration of EZ spillovers. Further judgement about the potential effectiveness of EZ capital incentives awaits analysis of a program which employs marginal investment incentives to target capital investment.

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[22]Proposals have included tax credits for newly constructed depreciable real property, preferential capital gains treatment, a tax credit for compensation (wages and health insurance costs), and faster recovery of the cost of child care facilities. See the Joint Committee on Taxation (1991) for a discussion of H.R. 11, H.R. 23, H.R. 1445, and H.R. 1747.

Table 1. Characteristics of Participating Firms: 1986, 1987, and 1988 Registrations

	1986	1987	1988
Number	1,054	1,068	949
Industrial Group:			
Manufacturing	189 (17.9)	206 (19.3)	202 (21.3)
Retail Trade	405 (38.4)	389 (36.4)	331 (34.9)
Wholesale/Distribution	131 (12.4)	143 (13.4)	151 (15.9)
Services	329 (31.2)	330 (30.9)	265 (27.9)
Organization Type:			
Corporation	648 (61.5)	816 (76.4)	764 (80.5)
Unincorporated	406 (38.5)	252 (23.6)	185 (19.5)
Firm Size (Employees):			
Under 20	729 (69.2)	739 (69.2)	617 (65.0)
21-50	181 (17.2)	183 (17.1)	187 (19.7)
51-100	74 (7.0)	73 (6.9)	72 (7.6)
100 +	70 (6.6)	73 (6.9)	73 (7.7)
New Jobs (Total)	2,349	2,906	2,897
New Jobs (Zone Resident)	613 (10.7)	497 (17.1)	425 (14.7)
Average Annual Wages (Total)	$21,414	$19,339	$20,434
Average Annual Wages (Zone Resident)	$12,109 (56.5)	$10, 733 (55.5)	$11,746 (57.5)
Average Percent IN Income	86	88	90
Average Percent EZ Income	61	74	74

Source: J.A. Papke and L.E. Papke (1990)
Note: Percent of totals in parenthesis.

Table 2. EZ tax savings by firm size: 1986, 1987, and 1988 registrations

Firm Size (Employees)	1986 Total		1986 Average per firm	1987 Total		1987 Average per firm	1988 Total		1988 Average per firm
Under 11	1,756,306	(14.2)	3,131	1,736,882	(14.5)	3,053	1,868,196	(14.1)	4,106
11-20	831,934	(6.8)	4,952	815,808	(6.8)	4,799	883,059	(6.7)	5,451
21-30	517,648	(4.2)	5,950	558,625	(4.7)	6,348	493,701	(3.7)	5,090
31-40	711,150	(5.8)	11,470	722,505	(6.0)	11,468	726,909	(5.5)	12,533
41-50	522,578	(4.2)	16,311	594,511	(5.0)	18,578	472,550	(3.6)	14,767
51-75	735,799	(6.0)	15,016	460,537	(3.9)	10,012	990,019	(7.5)	23,024
76-100	298,264	(2.4)	11,931	568,195	(4.8)	21,044	598,097	(4.5)	30,624
Over 100	6,951,274	(56.4)	99,304	6,504,933	(54.4)	89,109	7,190,020	(54.4)	98,493
	12,324,954		11,694	11,961,996		11,200	13,222,551		13,933

Source: J.A. Papke and L.E. Papke (1990).
Notes: Details may not add to total due to rounding. Percentage composition in parenthesis.

Table 3. EZ tax savings by major industry: 1986, 1987, and 1988 registrations

Industry	1986 Total	1986 Average per firm	1986 Average per NJ	1986 Average per NJZR	1987 Total	1987 Average per firm	1987 Average per NJ	1987 Average per NJZR
Manufacturing	7,161,396 (58.1)	37,891	8,997	27,127	6,786,372 (56.7)	34,102	4,561	23,482
Retail Trade	1,487,672 (12.1)	3,673	2,517	8,501	1,838,381 (15.4)	5,107	3,170	19,768
Wholesale/ Distribution	1,687,657 (13.7)	12,868	5,038	29,063	1,967,030 (16.4)	14,254	6,691	39,341
Professional Services	67,268 (0.5)	909	989	2,746	57,209 (0.5)	572	763	3,814
Other	1,922,961 (15.6)	7,541	3,440	21,131	1,286,652 (10.8)	6,466	1,673	25,733
All industries	12, 324,954	11,694	5,258	20,122	11,961,995	12,307	4,116	24,068

Industry	1988 Total	1988 Average per firm	1988 Average per NJ	1988 Average per NJZR
Manufacturing	8,147,748 (61.6)	40,335	4,306	29,203
Retail Trade	1,476,283 (11.2)	4,460	6,075	29,526
Wholesale/ Distribution	2,068,181 (15.6)	13,697	9,274	159,091
Professional Services	69,596 (0.5)	892	1,265	4,971
Other	1,460,742 (11.0)	7,811	3,018	21,170
All Industries	13,222,551	13,933	4,564	31,113

Source: J.A. Papke and L.E. Papke (1990).
Note: NJ: new jobs; NJZR: new job zone resident. Percentage composition in parenthesis.

Table 4. EZ costs to local property tax payers expressed per new job and per new zone resident job by zone: 1986, 1987, 1988 registrations

Zone	Average Cost per NJ	Average Cost per NJZR	Average Cost per NJ	Average Cost per NJZR	Average Cost per NJ	Average Cost per NJZR
Anderson	1,651	6,409	3,951	39,017	6,776	63,245
Elkhart	1,282	5,455	3,038	17,429	5,027	25,953
Evansville	14,923	35,926	7,068	52,209	2,490	13,098
Fort Wayne	3,674	22,241	4,162	73,316	5,708	48,465
Gary	5,627	36,578	11,123	144,599	1,847	73,654
Hammond	12,030	61,934	7,415	84,293	11,747	63,810
Madison	1,800	1,728	263	708	815	1,722
Michigan City	4,150	12,593	2,824	10,844	4,570	28,434
Richmond	3,276	7,152	6,713	27,207	3,965	15,554
South Bend	7,856	57,764	5,907	71,016	7,813	173,539
All	5,276	20,218	4,378	25,598	4,921	33,543

Source: J.A. Papke and L.E. Papke (1990).
Notes: NJ: new job; NJZR: new job zone resident.

Table 5: Summary Statistics for Machinery and Equipment, and Inventories 1981-1989 (millions of dollars)

	Machinery and Equipment			Inventories		
	All jurisdictions	Non-zones	Zones	All jurisdictions	Non-zones	Zones
Average	49.30	43.11	59.93	38.49	37.23	40.64
S.D.	53.51	57.77	43.83	34.53	41.82	15.73
Minimum	3.39	3.39	10.34	2.96	2.96	10.21
Maximum	261.29	261.29	186.52	166.72	166.72	81.42
Obs.	125	79	46	125	79	46

Table 6: Summary Statistics for Unemployment Claims 1980-1988

	All jurisdictions	Non-zones	Zones
Average	7,984.29	8,891.14	4,987.74
S.D.	7,426.40	8,139.99	2,673.44
Minimum	1,123.64	1,123.64	1,280.91
Maximum	55,600.67	55,600.67	10,487.33
Obs.	198	152	46

Table 7: Model 1 -- Jurisdiction-specific fixed effects, and linear time trend						
	Machinery and Equipment		Inventories		Unemployment Claims	
EZ	-.098 (.038)		.083 (.042)		-.250 (.109)	
EZYR1		-.099 (.038)		.082 (.043)		-.258 (.111)
EZYR2		-.020 (.058)		.108 (.066)		-.179 (.162)
EZYR3		-.026 (.075)		.066 (.085)		-.271 (.205)
EZYR4		-.076 (.091)		.060 (.103)		-.365 (.244)
EZYR5		-.126 (.111)		.082 (.125)		-.400 (.294)
R^2	.059	.127	.035	.052	.029	.041
Obs.	109	109	109	109	176	176

Note: R-squared and the number of observations are from the first-differenced equation.

Table 8: Model 2 -- Jurisdiction-specific time trend, fixed effects, and aggregate year effects						
	Machinery and Equipment		Inventories		Unemployment Claims	
EZ	-.130 (.046)		.080 (.050)		-.192 (.085)	
EZYR1		-.113 (.055)		.089 (.060)		-.238 (.093)
EZYR2		-.052 (.098)		.095 (.108)		-.358 (.155)
EZYR3		-.058 (.139)		.091 (.153)		-.538 (.213)
EZYR4		-.079 (.179)		.134 (.198)		-.542 (.269)
EZYR5		-.096 (.228)		.203 (.252)		-.495 (.338)
R^2	.287	.305	.274	.283	.636	.648
Obs.	109	109	109	109	176	176

Note: R-squared and the number of observations are from the first-differenced equation.

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