Enterprise Zones and Individual Welfare: A Case Study of California*

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Abstract

A now popular economic development tool for states, enterprise zone programs attempt to increase business investment, employment, and wages in depressed areas by offering labor and capital subsidies to firms operating in the designated zones. While a number of studies have examined the effects of EZs on business activity, few have explored how these zones have influenced individuals. This research examines the benefits to individual workers hired under California's EZ program using information from tax returns to document changes in the economic status of workers directly affected by the designation of enterprise zones in their local areas. The analysis reveals that EZ program participation has a positive impact on both wages and adjusted gross income (AGI) of EZ participants. It is not possible, however, to determine from our data if the income boost from EZ participation is permanent or transitory in nature. The data suggest that EZ participation may benefit taxpayers with very low initial income more than those with somewhat higher initial income. We also find that participation in the EZ program increases the likelihood that an individual will file a tax return. Since this is a case study, we caution that additional analysis is needed to fully determine the extent to which these results can be generalized.

In recent years, enterprise zones (EZs) have emerged as a popular economic development tool for states. Through tax incentives, EZs are designed to promote business development and retention, as well as improve the economic circumstances of residents and workers within a specific, and very localized, geographic area. A number of studies have examined the effects of EZs on business activity, but few have explored how these zones have influenced individuals.[1] This research examines the benefits to individual workers hired under California's EZ program.

In this paper, we use information from tax returns to document changes in the economic status of citizens directly affected by the designation of enterprise zones in their local areas. The analysis employs a case study methodology. We begin by identifying a sample of employees of enterprise zone firms whose hiring qualified their employers for California's enterprise zone hiring credit. We then compare the reported earnings growth over time of this set of California EZ program participants with those of individuals from two control groups: (1) a group of non-participants who had similar economic circumstances to those of program participants in the year the participant began in the program, and (2) a group of non-participants who had similar economic circumstances to those of program participants in each of two years prior to the year the participant began in the program.

A comparison of taxpayers who filed returns in all relevant years reveals that EZ program participation has a positive impact on both wages and adjusted gross income (AGI). It is not possible, however, to determine from our data if the income boost from EZ participation is permanent or transitory in nature. The data suggest that EZ participation may benefit taxpayers with very low initial income more than those with somewhat higher initial income. We also find that participation in the EZ program increases the likelihood that an individual will file a tax return. It seems likely that this increase in filing is due to an increase in participant income. Since this is a case study, we caution that a more comprehensive analysis is needed to fully determine the extent to which these results can be generalized.

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[1] Rubin (1990), Papke (1994), Bostic (1996), and Boarnet and Bogart (1995) are examples of business-oriented EZ studies. Of these, only Rubin (1990) considers individuals.

1. EZs: Theory and Evidence

The intent of EZ programs is to increase business investment, employment, and wages in the designated location. They attempt to achieve these goals by offering labor and capital subsidies to firms operating in the designated zones. The programs are based on the general notion that firms are sensitive to tax consequences in making location and investment decisions. EZ programs have been established in more than 39 states — primarily in "distressed" localities within a state — in the hope that the subsidies would enhance the economic circumstances for the neediest of the states' populations.

Economic theory suggests that EZ program subsidies should unambiguously increase output in the zone.[2] However, theory yields ambiguous results regarding the effects of EZ programs on wages and employment. The partial equilibrium model by Papke (1993) shows that subsidies to labor increase wages, while capital subsidies reduce wages at low product demand elasticities and low labor supply elasticities. For programs that involve both labor and capital subsidies, which is the case for most EZ programs, equal-cost labor and capital subsidies reduce wages if product demand is inelastic but increase wages at higher elasticities. Papke (1993) also shows that wage increases are greater if labor subsidies are targeted to zone residents. In her general equilibrium theoretical model, Gravelle (1992) shows that EZ benefits become smaller as the number of zones increases or as a zone becomes larger. Gravelle's analysis also suggests that subsidies will not influence aggregate investment or employment. Rather, investment and employment will be geographically redistributed. Importantly from a policy perspective, such redistributions, although not Pareto improving, may represent a net increase in total welfare.

There is an extensive empirical literature on whether tax policies in general, and EZ policies in particular, influence firm location and investment decisions. The literature on the effects of general tax policy has not yielded conclusive answers.[3] Research attempting to

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[2] Even if, in contrast to theory, EZ subsidies do not increase output, this investigation of the incidence of the subsidies would still be interesting. If that were the case, the question examined below would be do the zone businesses absorb the entire subsidy, or do they share some of it with their employees?

[3] Broad reviews of the literature cite equal numbers of studies that indicate either significant effects or negligible effects (see, for example, Bartik (1991) and Wasylenko (1997)). Recent studies,

measure directly the development effects of EZs has generally focused on business creation and investment effects and employment effects. As the body of EZ research has been reviewed in several sources (Rubin and Richards, 1992; Papke, 1993; Wilder and Rubin, 1996; and Fisher and Peters, 1997) only a flavor of the research results is offered here.

In his analysis of the effects of EZs on growth in the number of businesses in a locality, Dabney (1991) finds no evidence that EZs contribute significantly to such growth. By contrast, regarding employment, both Rubin and Wilder (1989) and Papke (1994) find evidence that Indiana's EZ program had significant positive effects. However, results from additional research suggest that these benefits were not enjoyed by zone residents; non-residents appeared to be the main beneficiaries of zone incentives (Papke, 1993). In summarizing the literature, Fisher and Peters (1997, p. 129) conclude that "it is difficult to generalize other than to say that, given the similarity of enterprise zone incentives to the kinds of incentives and tax differences that have been the subject of most research, it is likely that the incentives offered will, in some zones, produce measurable gains in investment or employment."

Only a few academic studies have examined California's EZ program. Using shift share analysis, Dowall, Beyeler, and Wong (1994) find little evidence that EZ program incentives were effective in either creating jobs or spurring increased business investment. The authors argue that the relatively weak performance of EZs reflects the high levels of economic distress in designated areas, which create poor business climates. Focusing on rural EZs, Bostic (1996) similarly finds that EZ incentives have only modest investment effects. Both analyses point to improved local coordination of economic development efforts as the main benefit of the program. By contrast, a review of the California program by program staff finds that EZ program incentives have

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however, generally suggest that taxes have a non-negligible negative effect on economic development, although some are skeptical of the generality of this result (for example, McGuire, 1992; McGuire, 1997).

The literature on the effects of non-tax incentives on economic development is far less extensive than the literature on the effects of tax incentives. These studies, many of which suffer from data measurement problems, have found few robust positive development effects associated with non-tax incentives, with the majority finding little evidence of any significant impact (Fisher and Peters, 1997).

contributed to significant job growth and business investment (California Trade and Commerce Agency, 1997).

If EZs cause job growth we would like to know how the increase in demand for labor affects wages. Even if there is no job growth, however, the EZ subsidy may be shared between the businesses receiving the EZ tax credits and their employees. Therefore, EZs may benefit their target populations even in the absence of growth in business activity. Thus, an incidence analysis of the effects of EZ policies is important and interesting regardless of whether or not EZs influence business investment and hiring decisions.

The current study extends the existing body of research on California's EZ program in two ways. First, while academic research on EZs has focused on business effects (jobs and investment), the analysis here focuses on individual welfare issues — namely, the income received by program participants. Second, whereas previous studies have relied on indirect measures of zone effectiveness, this study directly tracks the incomes of clearly identified EZ program participants and compares these to similarly situated individuals who are not program participants.

While the literature on enterprise zones has not focused on individual welfare issues, the effects of targeted government employment subsidy programs on individual outcomes has been studied in other contexts. The literature describes several problems with government wage subsidies, including the stigmatizing of vouchered employees (Burtless 1985) and the potential capture of a program's regulatory structure by special interests intent on increasing windfall government handouts (Lorenz 1995). Most of the available direct evidence on the effect of wage subsidies on incomes comes from studies of the federal Targeted Jobs Tax Credit program (TJTC). The TJTC provided credits against wages for a variety of disadvantaged groups. From 1978 until 1986 the credits were available for the first two years of the employee's employment, after 1986 the credits were restricted to a single year. The program expired in 1994.

Investigations of the TJTC have produced mixed results. Tannery (1998) reports that the earnings of TJTC participants were significantly and persistently higher than those of a control group consisting of people who had applied for the TJTC but were disqualified because of paperwork or application problems. Hollenbeck and Willke (1991) found the impact of vouchering in the program to be positive for employment but zero or negative for wages. The U.S. Department of Labor Office of Inspector General (1994) finds that TJTC did not improve earnings.

2. The California EZ Program and the Research Design

California's EZ program encompasses several tax incentives, which target firms, employees, and potential investors. The most important of these are a credit offsetting the sales tax on qualified investments in a zone, a credit for hiring qualified employees in an EZ, and a subsidy for qualified loans made to zone businesses.[4] Of these, this study investigates only the enterprise zone hiring credit. Under this credit employers receive tax credits for each employee that was hired after the designation of the zone, spends at least 90 percent of their work time on activities directly related to the conduct of a trade or business located within a zone, performs at least 50 percent of the work within the boundaries of the zone, and was eligible at the time of hire for participation in the Job Training Partnership Act, Greater Avenues for Independence Act, or Targeted Jobs Tax Credit programs. In filing their tax forms with the California Franchise Tax Board (FTB), firms must document the number of eligible employees and the wages paid to these employees. For each qualified employee, firms receive tax credits equaling 50 percent of the lesser of the employee's actual wage or 1 _ times the minimum wage for the employee's first year of employment. The percentage is reduced to 40 percent in the employee's second year, 30 percent in the third year, 20 percent in the fourth year, and 10 percent in the employee's fifth year. To qualify for the credits, the employee must remain with the employer for at least 90 days.

We test the hypothesis that, after controlling for factors such as household characteristics and broad economic trends, EZ program participation improves individual economic conditions. Data filed by firms at the California Department of Trade and Commerce (DTC) and the FTB allow for the identification of program beneficiaries in 1995. Tax return data on these individuals

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[4]For a detailed description of the California EZ program, see Dowall, Beyeler, and Wong (1994) or Bostic (1996) or contact the California Department of Trade and Commerce.

from 1993 through 1997 are then used to determine the economic impact, measured as growth in wages and adjusted gross income (AGI), of EZ program participation over the short run.

One problem in determining the effects of a program on economic outcomes is that it is difficult to attribute changes in these outcomes exclusively to program incentives. This is because other events and circumstances apart from the program may have also affected these outcomes. Therefore, to isolate the effect of a program on economic outcomes, it is necessary to account for these other possible circumstances. A common method for dealing with this issue is to construct a control group that was subject to the same external events and circumstances as the program group but which did not participate in the program and compare the outcomes of this group with those of the program treatment group. We take this approach in the current study.

We used two control groups for this study. The first includes individuals with characteristics very similar to those of the EZ program participants in 1995, the year that program participant employers were identified as receiving EZ tax credits. The second control group includes a sample of program non-participants with characteristics similar to program participants in 1993 and 1994. The second control group thus looks similar to the participant sample prior to the participants' entering the program. In both cases, we compare changes in wages and AGI between 1993 and 1995, 1996, and 1997. If, after entering the EZ program, wages grow significantly faster for EZ program participants than for members of the control groups, it would suggest that the EZ program offers tangible benefits to participants relative to their non-participant peers.

We compare the earnings of the EZ sample to those of the 1995 control group going both backward and forward. The look back assesses whether program participation elevated the participants from a lower income group than their peers in 1995 had been in. The look forward evaluates whether any gains from program entry are temporary or sustained.

We use the 1993/1994 control group to try to account for issues that are known to arise in studying wage trends for populations entering these types of programs and for potential selection mechanisms that may be at work. A number of researchers studying job training programs have identified a pre-program participation earnings dip for program participants but not for members of control groups.[5] Because the EZ program targets a population similar to the population targeted by job training programs, a similar dip might occur for EZ program participants. If such a dip occurs among EZ program participants, the use of a control group selected using only data from 1994 (the year in which the dip would occur) could overstate EZ program effects. Selecting the control group based on taxpayer experiences during both 1993 and 1994 should reduce the potential problems associated with any pre-program participation earnings dip that may exist.

The second control group also controls for some potential selection mechanisms for EZ program participation. Employers could be selecting those EZ program credit-eligible individuals with the most attractive employment prospects. For example, if employers hire individuals whose wages are already growing more rapidly than the wages of individuals not hired, and these differences persist after program participation, conclusions drawn from simple comparisons of wage or income changes over 1995-96 or 1995-97 will likely overstate program effects.[6] Since the 1993/1994 control group is matched on earnings data for two pre-program years, it controls for any differences between participants and non participants in the pattern of pre-program entry wage growth, if they exist, thus enabling a more accurate determination of program effects on wage and income growth.

The analysis below presents a series of comparisons of income growth between the EZ participant sample and the two control groups in the years surrounding entry into the EZ program. The use of direct measures of economic well-being— wages and AGI— to assess EZ program benefits is an important departure from previous work. This analysis, therefore, makes an important contribution to the body of research on the economic impact of enterprise zones, and, in particular, to the very small literature that focuses on whether and how EZ incentives influence economic outcomes of individuals.

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[5]See Ashenfelter (1978) and Heckman, LaLonde, and Smith (1999). Heckman and Smith (1999) argue that this arises due to differences in the incidence and duration of unemployment spells between members of the two groups.

[6] Heckman and Smith (1999) find that such treatment group-control group differences exist and bias assessments of the effects of the Job Training Partnership Act.

3. Data

The sample of EZ program participants was gathered from two sources. The DTC provided a list of all participants that were registered by employers in 1995 in two California EZs. Additional participants were identified from forms filed by 15 employers claiming EZ tax credits for 1995 at the FTB.[7] We obtained 135 individuals from the DTC and 50 individuals from the FTB. For each of these individuals, information on filing status, number of dependents, adjusted gross income (AGI), earned income tax credits (EICs), earned wages, and residence location was collected from available California and federal tax returns filed in 1993 through 1997.[8] Thus, pre- and post-participation data were obtained for each EZ program participant.

Because the sample is a non-random sample of program participants, the control groups were restricted to match the distribution of characteristics in the EZ program participant sample. We began by requiring that each member of the control group have the same filing status and number of dependents as the sample member to whom they were "matched."[9] Recognizing that economic dynamics vary across geographies with different demographic and economic circumstances, we also required that each match reside in a ZIP code whose characteristics were similar to those of the ZIP code in which their associated EZ participant resided. These similar ZIP codes had to have the same ethnic composition, poverty profile, and income profile as the ZIP code of the program participant, as well as a comparable degree of "urbanness."[10] For each EZ participant, we selected the five ZIP codes most similar to (but not including) their own, and

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[7]Firms are not required to submit worksheets identifying employees. However, a number of firms voluntarily included such worksheets with their tax returns. These worksheets identify all of the firm's credit qualified workers. Our sample includes only those workers hired in 1995.

[8] Unfortunately, for joint filers, we are not able to distinguish the EZ participants' wages from their spouses' wages.

[9]Possible filing statuses are single, head of household, married filing jointly, married filing separate. Individuals were grouped according to whether they had zero, one, or two or more dependents. For the 1995 control group, filing status and number of dependents were required to match in 1995. For the 1993/1994 control group, filing status was required to match in both 1993 and 1994; due to data availability constraints, the number of dependents was only matched in 1994.

[10]The ethnic composition criterion grouped ZIP codes according to the percentage of the ZIP code population that is either Black or Hispanic. Five ranges were used: greater than 50 percent, 25-50 percent, 10-25 percent, 5-10 percent, and less than 5 percent. The poverty criterion was similarly constructed with 4 ranges: greater than 25 percent, 10-25 percent, 5-10 percent, and less than 5

required that all control group members matching to that participant reside in one of these five ZIP codes.

For each EZ participant, all potential control group members who met the filing status, number of dependents, and ZIP code criteria were then ranked according to their degree of similarity with the program participant, based on an index of similarity comparing the wages, AGIs, earned income credits (EICs), and ages of program participants and these potential matches. For each control group, the top three matches for each participant were selected to be members of that control group.[11] For these matches, the same information collected for program participants (wages, AGI, etc.) was obtained for 1993 through 1997 as available. The final samples thus have wage, income, and dependent and filing status information for EZ program participants and matched non-participants for 1993 through 1997.

As discussed earlier, there are two control groups. For one control group, the base year was 1995. Thus, all individual match criteria were based on 1995 characteristics. The other control group was built by matching on 1993 and 1994 characteristics. For some EZ program participants, data were available for only one of these two years. Matches for these individuals

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percent. Median incomes were required to be within 10 percent of the median income in the program participant's ZIP code.

[11] The top three matches for each control group were identified using the following procedure. For each potential 1995 match, a similarity index was computed using the formula $[(AGI_p/AGI_i)^2 + (Wages_p/Wages_i)^2 + (EIC_p/EIC_i)^2]^{-1}$, where p identifies program participants and i identifies control group members. For the 1993/1994 control group, the similarity index formula included ratios for both 1993 and 1994. In the index formulas, all ratios were required to be greater than 1. If any ratio in the sum was less than one, its reciprocal was used in computing the index. If either the numerator or the denominator of a ratio, but not both, was equal to zero, the ratio was set to be arbitrarily large. If both the numerator and denominator were equal to zero, the ratio was set equal to 1. Ten matches were chosen for each participant. These matches were then reranked for similarity using an index that incorporated supplemental age data for the program participants and their matches. This new index was computed by adding a term to account for age differential to the denominator of the formula. If the EZ participant was born before 1970 and the age differential between the participant and the potential match was less than 10 years, or if the participant was born in 1970 or later and the age differential was less than 5 years, the age factor was set equal to zero. Otherwise, the age factor was set equal to the square of the [age differential (in years) minus 10 (or for participants born 1970 or later minus 5)] divided by 100. In some cases where the EZ participant was listed as the spouse on a joint return, the supplemental source of age data only provided data on the age of the primary filer. In these cases we assumed the age of the two spouses to be equal. The top three participant matches for each EZ participant, based on this reranking, represent the control groups. Control group matches with highest similarity index were considered to be the most similar to

were required to not have data available in the same year as their sample counterpart, and were matched based on the closeness of their characteristics in the year for which data were available. The ZIP code match for the second control group was based on 1993 ZIP codes if available, and 1994 ZIP codes for those with no 1993 data.

4. Results

4.1 Filing Rates

When reviewing the data presented below, one must bear in mind that the population being studied here does not file tax returns on a consistent basis. As noted above, we identified 185 individuals that we believe were hired under the terms of the enterprise zone program in 1995. Only 171 of these individuals filed tax returns in any year between 1993 and 1997; and only 150 filed tax returns in 1995, the year in which we know they were hired. Table 1 presents the number of tax returns filed by our EZ sample in each year. The discussion below will assume that, on average, people who do not file tax returns have lower income than those who do file returns. If this assumption is correct, data missing from our panel will bias some of the results presented below, but in a known direction. In most cases, this will reinforce our qualitative conclusions, but reduce the reliability of any quantitative estimates of the program's effects that may be inferred from the results presented below.

Our first important finding is that being hired under the enterprise zone program increases the likelihood that an individual will file tax returns. The bottom panel of table 2 compares the filing rate of the sample to that of the 1995 control group. Because sample members who did not file in 1995 do not have matches in the 1995 control group, the percentages reported for the sample are the percentages of the 150 sample taxpayers who filed in 1995. It can be seen from this panel that sample members were less likely to file tax returns prior to entry into the program (1993 or 1994) than were their control group counterparts. If our assumption that nonfilers are generally worse off economically than are filers is correct, this suggests that the income of enterprise zone participants improved relative to the control group upon program entry. We also

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program participants. Similar procedures have been used in several different contexts (see Avery, Beeson, and Calem (1997) and Prohofsky (2000)).

find in the bottom panel that zone participants are somewhat more likely than the control group to file in the years after program entry. Again, this suggests that zone participation prevents some individuals from experiencing a reduction in income in the years after entry.

The top panel of table 2 presents filing rates of the EZ sample and the 1993/1994 control group. For comparability, the table only considers the 132 sample taxpayers who filed in either 1993 or 1994 (and, thus, have a match in the control group). By construction, these filing rates must be identical in 1993 and 1994. For all subsequent years program participants are more likely to file than are members of the control group.

4.2 Filing Status

As described above, the method by which the control groups were selected required that each control group match have the same filing status as its corresponding sample member in the control selection year. There are, however, differences between the EZ sample and the control groups in the distribution of taxpayers by filing status in the non-control years. Table 3 presents the distribution of filing status for the 3 groups in 1993 and 1995. The data presented in this table include only those taxpayers who filed in both years. Both control groups showed a slight decrease from 1993 to 1995 in the percentage of taxpayers filing as single, and a slight increase in the percentage filing as head of household. The 1995 control group also showed an increase in the percentage filing as married filing separately. The percentage of joint filers was the same in each year for both control groups. Like the 1995 control group, the EZ sample showed an increase in married filing separately. Unlike the control groups, the percentage of joint filers in the EZ sample dropped from 1993 to 1995, while the percentage of single filers increased.[12] It is possible that these demographic differences indicate that we have failed to generate valid control groups. If we believe that, on average, joint filers have higher income than other types of filers, we would expect this shift in filing status to drive down incomes for the EZ sample. If so, the analysis below will underestimate the effect of the EZ program on participant income.

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[12] We also examined the more restrictive set of taxpayers for whom both the taxpayer and their match filed in both years. The pattern – a decrease in joint filing for the sample, but not for the control groups –is the same.

4.3 Income Growth

Changes in average income over time are difficult to interpret when large portions of the population enter and exit from year to year. The results presented in this section, therefore, include only observations for which the data are complete.[13] In the discussion that follows, we will define a taxpayer's base year as 1993 if a taxpayer filed in 1993 and as 1994 if the taxpayer did not file in 1993. We define an observation as having complete data if both the EZ sample member and their corresponding control group member filed tax returns in a base year and in 1995, 1996, and 1997. There are 104 observations in which both the EZ member and at least one 1993/1994 control group member have complete data. There are 80 observations in which an EZ member and three corresponding 1993/1994 control group members have complete data. For the 1995 control group, there are 84 observations in which the EZ member and at least one match have complete data, and 67 observations in which the EZ member and three matches have complete data.

Table 4 presents data on the wages earned by EZ participants and the control groups during the period of this study. In three of the four comparisons shown, mean wages grew faster between the base year and 1995 (the first year of EZ participation) for the EZ sample than for the control group. In the comparison between the EZ sample and the 1995 control group, mean wages for participants increased from $17,601 in the base year to $20,190 in 1995. Consistent with the way the control group was selected, mean 1995 wages for the control group are an almost identical $20,175. Mean base year wages for the control group were $18,512, noticeably higher than participant base year wages. Thus wage growth from the base year to the initial year of program participation was greater for those who entered the EZ program than for the control group. This result is even more dramatic when comparing the EZ sample to the 1995 control group with complete data for three matches. In this comparison mean wages for the EZ participants rose from $19,711 to $23,277 compared to an increase from $22,367 to $23,364 for the control group. For the 1993/1994 control group with three matches, mean wages increased

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[13] Recall that the analysis of filing rates presented above suggests that this restriction may lead to an underestimate of the impact of the EZ program on income.

from $17,441 in the base year to $19,670 in 1995; whereas mean wages for the corresponding participants increased from $17,462 to $21,010. The comparison with the 1993/1994 control group with only one match does not fit this pattern, however. In this case participant wages increased from $16,660 to $20,048, while control group wages increased from $16,593 to $20,034.

Most of the gains from EZ participation occur at the lower end of the wage distribution. For the 25th percentile, wage gains from the base year to 1995 were greater for participants than for the control group in all four comparisons presented. For example, for the 1995 control with one match, 25th percentile wages increased from $3,796 to $7,554 for participants compared to an increase from $5,901 to $7,208 for the control group. Median wages grew faster for participants in both comparisons with the 1993/1994 control group, but more slowly in both comparisons with the 1995 control group. For the 75th percentile, wage growth (decreases) from the base year to 1995 was greater (smaller) for participants than for the control group in only one of the four comparisons.

In the year after entering the EZ program (1996) mean wage growth from the previous year was greater for participants in both comparisons with the 1993/1994 control group and in the comparison with one closest 1995 match. Mean wages grew more slowly for participants, however, when compared to the 1995 control group with three matches. In 1997, the second year after program entry, mean wages compared to the year before increased more for participants in both comparisons with the 1993/1994 control group, but less in both comparisons with the 1995 control group. Mean wage growth for the whole period (1997 – base year) was greater for the participant sample in all four control group comparisons.

Growth in 25th percentile wages from 1995 to 1996 was much greater for EZ participants than for the control groups in all four comparisons. For example, 25th percentile wages for the 1993/1994 control group grew only from $7,059 in 1995 to $7,325 in 1996; while the corresponding increase for participants was from $7,830 to $13,459. From 1996 to 1997, 25th percentile wage growth for EZ participants was faster than that for the 1995 control group, but slower than for the 1993/1994 control group. In all four comparisons 25th percentile wage growth from the base year to 1997 was greater for participants than for the control groups.

Median wages for participants also grew faster from 1995 to 1996 in all four cases, but the differences were less dramatic than for the 25th percentile. From 1996 to 1997, median wage growth for participants was slower than for control groups in all cases. In all cases, 75th percentile wage growth was less for participants than for control groups from 1995 to 1996. 75th percentile wage growth from 1996 to 1997 was similar between participants and all control groups. For the entire time period, 75th percentile wage growth was slower for participants than for the control group in all four cases.

For another view of the wage data, we calculated wage growth over time separately for each individual. We then found the average of these growth rates for each group of taxpayers. Table 5 presents a comparison of average individual wage growth between the base year and each of the three post-program entry years. In all cases, average wage growth is higher for participants than for the control group. This is consistent with the observation that participation is more likely to benefit those at the lower end of the wage distribution, because small gains in income may still be large percentage gains in income for low-income participants whereas somewhat larger increases in income may represent a somewhat smaller percentage increase for high-income control group taxpayers.

Tables 6 and 7 present data on adjusted gross income in the same manner as the wage data was reported in tables 4 and 5. Most of the comparisons described above produce the same result when using AGI instead of wages. AGI growth is higher for participants for three of the four comparisons between the base year and 1995, and for all four comparisons between the base year and 1997. In all four comparisons 25th percentile AGI grows more rapidly and 75th percentile AGI grows more slowly for participants between the base year and both 1995 and 1997. Average individual AGI growth rates are greater for participants in all cases.

4.4 Regressions

For a more rigorous test of the hypothesis that EZ participation enhances income, we performed a regression analysis using percentage growth in income as a dependent variable.[14] Each regression was run using first wages then AGI as the measure of income. For each income measure, two separate regressions were run. The first regression included the single closest match available from the control data set along with the corresponding EZ participants. The second regression included the three closest matches available in the control data sets and the corresponding EZ participants.[15] Only taxpayers with complete data (as defined above) were included in the regressions. Because the dependent variable is a growth percentage, those taxpayers reporting zero wages in the base year were not included in the regressions.

To control for the possible influence of demographic factors on wage growth, the regressions reported add dummy variables for differences in filing status and number of dependents. Because the univariate analysis (presented above) suggests that income growth rates may vary by income level, the regressions try to control for this problem by including base year income (divided by 10,000) as an independent variable. We also ran, but do not report here, regressions using only an intercept and the participation dummy variable. As expected, the inclusion of demographic factors in the regressions presented below has very little effect on the magnitude of the coefficient on the EZ participation variable, but does, in some cases, increase its statistical significance.

4.5 Results using the 1993/1994 Control Group

Tables 8 and 9 show the results of regressions analyzing EZ participants and the 1993/1994 control group. In the Table 8 regressions, the dependent variable is wage growth. In the regressions on wage growth from the base year to 1995, the coefficient on the EZ participation variable is positive and statistically significant at the .05 level in both the single

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[14] It should be noted that the choice of percentage income growth rather than absolute income growth as the dependent variable is an appropriate way to make interpersonal comparisons of well-being only if the marginal utility of income decreases as income increases.

[15] Regressions were also run using the two closest matches. The results are similar to those obtained using the three closes matches, so they are not reported here.

closest and three-closest match regressions. In the regressions on wage growth from the base year to 1996, the coefficient on the EZ participation variable is, in both cases, larger than it was when comparing wage growth from the base year to 1995. The coefficient on the participation variable is, however, statistically significant only in the regression using three matches for each participant. In the regressions on wage growth from the base year to 1997, the coefficient on the participation variable is about the same magnitude as in the base year to 1996 comparison, but is not statistically significant at the .05 level either regression.

When AGI growth is used as the dependent variable, the results, presented in Table 9, are very similar. In the regressions using AGI growth, the coefficient on EZ participation is positive in all cases. The magnitude of the coefficient is greater in the base year to 1996 comparison than in the base year to 1995 comparison, but approximately the same for the 1996 and 1997 comparisons. The coefficient is statistically significant at the .05 level for the three-match base year to 1995 and base year to 1996 regressions, but is not statistically significant in either base year to 1997 regression. In both Tables 8 and 9, the coefficient on participation is less likely to be statistically significant in regressions using only participants and their closest control match, the regressions run on the smaller amount of data.

The results in Tables 8 and 9 suggest that participation in the EZ program did boost income for program participants. Participant income increases relative to 1993/1994 control group income in the initial year of program participation (1995). The gap continues to grow in the year following entry into the program. By the second year after program entry, however, the gap in income growth levels off and loses its statistical significance. This result is consistent with two possible underlying stories. It may be that the income gains from participation are exclusively short-run in nature, and that as the subsidy is reduced (removed completely by year 5) the incomes of participants and non-participants will converge. Or it may be that participants have been bumped to a new, and higher, income trajectory, but that the bump is too small relative to other changes in income that take place over time to be detected with our small sample. Unfortunately, our data set does not enable us to distinguish between these possibilities.

4.6 Results using the 1995 Control Group

Tables 10, 11, and 12 present results of regressions on the EZ sample and the 1995 control group. In Table 10, the dependent variable is wage growth from the base year. As with the 1993/1994 control group, the coefficient on the participation variable is positive in all cases. The coefficient is not, however, statistically significant at the .05 level for either base year – 1995 or base year – 1996 run. It is statistically significant for base year – 1997 if three matches are used for each participant, but not if only the closest match is used. The magnitude of the coefficient increases as each additional year is added. For the base year – 1996 and base year – 1997 regressions, the magnitude of the coefficient on the participation variable is larger when only one match is used for each participant than when three matches are used.

In the AGI regressions presented in Table 11, by contrast, the coefficient on participation is statistically significant at the .001 level for the base year – 1995 and base year – 1996 runs with three matches, and significant at the .05 level in all other runs except for base year – 1997 with only one match. As with the wage regressions, the magnitude of the participation coefficient increases in later years and, after 1995, is smaller with three matches per participant than with only one.

Overall, the regressions using the 1995 control group support the hypothesis that EZ participation enhances income. In several of the AGI regressions, the statistical significance of the coefficient on participation drops when data from the second year after program entry (1997) is used, but the effect is not as pronounced as with the 1993/1994 control group. In the wage regressions, statistical significance increases with the 1997 data. Based on the 1995 control group, therefore, we can not conclude that the effects of EZ participation are temporary in nature.

In order to increase the number of usable observations in the analysis of the impact of EZs after the year of initial participation, we also ran regressions using only data going forward from the onset of participation, from 1995 to 1997. These results, using both wages and AGI as the dependent variable, are presented in Table 12. The coefficient on EZ participation is not statistically significant in any of these regressions. When only the closest match is used, the coefficient on the participation variable is negative, when three matches are used, the coefficient is positive. We can not tell from these results whether or not the effect of participation is only temporary.

4.7 Other Relationships

Across all tables, the relationships that were consistently significant were the negative correlation between wages and wage growth and the negative correlation between AGI and AGI growth. Holding other factors constant, those individuals with the highest wages and AGI in the initial year had the smallest percentage wage and AGI growth over every period we examined.

4.8 Robustness

When we initially collected the data presented above, there were some concerns that the results may be driven by a small number of outlier observations. To evaluate the sensitivity of the results to a few extreme outliers, all regressions were rerun excluding those observations considered to have abnormally large growth rates, which we defined as wages or AGI 50 times greater than the wages or AGI in the previous year. The results of this outlier analysis are presented in Table 13. The coefficient on the EZ participation variable from each of the regressions presented above using three control group matches per participant and including demographic variables for three different samples is presented in the first column in Table 13. The second column presents the participation coefficient for these regressions when outliers are excluded.

Excluding outliers has no effect on the primary result – the coefficient on participation remains positive in all cases. When outliers are excluded, this result becomes statistically significant at the .05 level in all six (and at the .01 level in five of the six) regressions presented using AGI growth as the dependent variable. For the 1993/1994 control group, it is statistically significant at the .05 level for both base year – 1995 regressions and the base year – 1996 wage regression, but is no longer statistically significant for the base year – 1996 AGI regression.

When outliers are excluded, the magnitude of the coefficient no longer grows dramatically in the second year of participation, and in the AGI regression with the 1993/1994 control group it actually decreases. For the 1993/1994 control group, the magnitude in the base year – 1997 regression is less than half what it was in the base year – 1996 regression. These changes suggest that post initial year benefits from participation are concentrated in a few outlying observations.

All of the preceding results were obtained using a sample restricted to include only those individuals for whom information was available for both the participant and the requisite number of control matches in either 1993 or 1994 and in each year between 1995 and 1997. This was done out of a concern that those individuals who "dropped out" of the sample in a year were not randomly drawn from the overall sample, rendering comparisons of regressions using different years difficult to interpret. To evaluate the significance of this concern, the regressions were rerun using all observations that could be used in each case (i.e. an observation would not be removed from the base year – 1995 regression due to lack of 1997 data).

The results using this unrestricted sample are shown in the final column of table 13. Consistent with the results above, the coefficient on participation remains positive in all cases. The coefficient is statistically significant at the .05 level in four (and at the .01 level in two) of the six cases with the 1993/1994 control group, and in five (four) of the six cases with the 1995 control group. The magnitude of the coefficient grows more rapidly in the second year of participation for the unrestricted sample than for the restricted sample. For the 1995 control group the magnitude continues growing in the third year of participation.

In sum, the results appear to be robust. EZ program participation continues to be associated with relative gains in wages and AGI for program participants regardless of whether outliers are omitted or important sample restrictions are eased.

5. Conclusion

While most analytical attention regarding the effects of enterprise zone (EZ) programs has focused on business outcomes, relatively little attention has been given to the impact of EZ programs on the wages and income of individuals. This analysis attempts to add to this small, but important, literature. Using data obtained from the California Department of Trade and Commerce (DTC) and the California Franchise Tax Board (FTB), we compare the wage and income growth of EZ program participants and two peer groups between 1993 and 1997. Two peer groups are used to account for known wage profile effects observed in analyses of other labor programs and for potential selection mechanisms.

Our primary result is that, despite being nominally given to employers, some of the value of the California enterprise zone wage credit does appear to flow through to the workers hired under the program. Relative to the control groups, program participant tax filing rates increase upon entry into the program. In comparisons of taxpayers who filed tax returns both before and after program entry, income increased more rapidly for participants than for controls. The effect of participation on income appears to be greater for those who are relatively less well off prior to program entry.

Although this research reflects an important advance in the understanding of EZ program effects, several key questions deserve further exploration. Of particular interest would be research that focuses on the types of jobs that program participants hold prior to participation and move into through entry into the program. We do not know, for example, whether the EZ program affects the expected length of job tenure for participants.

As a final caveat, it is important to emphasize that, because the sample of EZ participants was not randomly selected, we do not know the extent to which these results may generalized to other enterprise zones. They are, however, strongly suggestive and point to the need for a well-structured, comprehensive study of California's EZ program and its effects on the well-being of program participants. Unfortunately, such a study is highly unlikely given the current structure of California's program, which does not facilitate the systematic collection of data regarding program participation and outcomes for participants.[16] Changes in program operations to allow for such data collection would greatly enhance our understanding of the effects of EZ incentives on the economic outcomes of program participants.

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[16] The California State Auditor (1995) concludes "that the effectiveness of enterprise zones and program areas cannot be determined. The (Trade and Commerce) Agency has neither developed an adequate framework to review and evaluate the programs' progress nor measured their effectiveness."

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Table 1. Number and Percentage of Enterprise Zone Employees Filing Tax Returns

Year	Number	Percentage
1993	105	61.4
1994	117	68.4
1995	150	87.7
1996	150	87.7
1997	142	83.0

Table 2. Percentage of Filers Filing Tax Returns

Sample Year	Sample	1993 Control		1995 Control	
		Closest Match	3 Matches	Closest Match	3 Matches
1993/1994 Filers					
1993	79.5	79.5	79.5		
1994	88.6	88.6	88.6		
1995	92.4	88.6	83.6		
1996	90.2	79.5	78.0		
1997	84.8	79.5	74.2		
1995 Filers					
1993	64.0			74.0	78.9
1994	74.7			83.3	86.4
1995	100			100	100
1996	93.3			83.3	85.3
1997	88.0			78.0	80.2

Table 3. Distribution of Taxpayers Filing in Both 1993 and 1995, by filing status (percent)

Sample Filing Status	1993 Filing Status	1995 Filing Status
EZ Sample		
Single	32	35
Joint	48	42
Married, Separate	1	4
Head of Household	19	19
1993/1994 Control		
Single	34	33
Joint	49	49
Married, Separate	1	1
Head of Household	17	18
1995 Control		
Single	35	32
Joint	41	41
Married, Separate	1	3
Head of Household	24	25

Table 4. Wages of EZ Participants and Matches, by match proximity

	1993-94				1995			
	EZ w/ 1 1993 match	Closest Match	EZ w/ 3 1993 matches	3 Closest Matches	EZ w/ 1 1995 match	Closest Match	EZ w/ 3 1995 matches	3 Closest Matches
Number	104	104	80	240	84	84	67	201
Base Wages								
Mean	16,660	16,593	17,462	17,441	17,601	18,512	19,711	22,367
25th Percentile	3,944	4,164	4,237	4,109	3,796	5,901	4,858	7,950
Median	10,821	10,928	13,396	13,734	13,104	11,635	16,007	15,968
75th Percentile	20,679	21,727	21,769	22,534	23,445	25,147	29,227	29,061
1995 Wages								
Mean	20,048	20,034	21,010	19,670	20,190	20,175	23,277	23,364
25th Percentile	7,830	7,338	7,830	7,059	7,554	7,208	10,133	10,167
Median	15,250	14,409	15,985	15,525	15,791	15,993	18,331	18,577
75th Percentile	24,107	26,613	25,951	27,363	24,822	24,897	27,744	27,752
1996 Wages								
Mean	24,005	23,351	24,343	22,629	23,612	22,835	25,395	25,964
25th Percentile	13,033	7,747	13,459	7,325	13,393	10,535	14,711	12,270
Median	19,395	18,022	19,634	17,984	19,395	17,560	20,904	19,766
75th Percentile	28,131	33,435	28,873	31,711	27,078	28,979	28,444	33,532
1997 Wages								
Mean	26,506	25,693	27,871	25,510	25,588	25,409	26,984	27,996
25th Percentile	13,010	9,767	13,223	9,487	13,656	9,997	15,263	12,145
Median	19,937	19,003	20,932	20,250	20,447	18,955	21,376	21,010
75th Percentile	32,231	36,662	33,533	36,255	32,231	34,969	34,663	38,757

Note: All figures are in nominal dollars. Base Year is 1993 for all taxpayers who filed in 1993, 1994 for those who did not.

Table 5. Average Percentage Growth in Wages

	1993-94 Control Group				1995 Control Group			
	EZ w/ 1 1993/4 match	Closest Match	EZ w/ 3 1993/4 matches	3 Closest Matches	EZ w/ 1 1995 match	Closest Match	EZ w/ 3 1995 matches	3 Closest Matches
Base-1995	72	149	156	78	160	133	152	75
Base-1996	140	277	276	111	285	144	181	92
Base-1997	180	330	345	177	363	125	209	93

Note: Base Year is 1993 for all taxpayers who filed in 1993, 1994 for those who did not.

Table 6. Adjusted Gross Income (AGI) of EZ Participants and Matches, by match proximity

	1993-94				1995			
	EZ w/ at least 1 1993 match	Closest Match	EZ w/ 3 1993 matches	3 Closest Matches	EZ w/ at least 1 1995 match	Closest Match	EZ w/ 3 1995 matches	3 Closest Matches
Number	104	104	80	240	84	84	67	201
Base AGI								
Mean	18,190	18,184	19,238	19,233	19,556	20,207	22,032	24,389
25th Percentile	5,629	5,751	6,059	6,196	6,237	7,028	6,408	9,987
Median	12,382	13,250	14,128	14,763	13,716	14,802	17,915	17,340
75th Percentile	22,250	22,206	26,160	27,661	28,222	25,082	31,402	31,984
1995 AGI								
Mean	22,205	22,395	23,608	21,559	22,322	22,349	25,946	25,912
25th Percentile	9,460	8,771	9,783	8,500	8,681	9,070	11,744	11,922
Median	16,356	16,722	18,148	16,751	17,639	17,417	21,892	21,717
75th Percentile	27,363	30,523	27,740	30,276	27,509	27,512	30,049	30,027
1996 AGI								
Mean	25,178	25,825	25,781	24,848	25,068	24,435	27,146	28,488
25th Percentile	13,903	11,170	14,897	10,409	14,530	11,596	15,850	14,041
Median	20,247	19,698	20,700	19,441	20,247	18,837	21,339	23,361
75th Percentile	29,526	34,317	30,696	33,653	28,461	29,711	31,642	36,360
1997 AGI								
Mean	29,424	28,532	31,525	28,193	29,219	27,198	31,303	30,356
25th Percentile	14,777	12,727	16,421	11,189	15,947	11,923	17,152	13,779
Median	20,995	23,010	21,848	22,155	21,521	19,901	23,805	23,729
75th Percentile	33,808	37,716	35,406	37,716	34,392	35,417	36,291	40,248

Note: All figures are in nominal dollars. Base Year is 1993 for all taxpayers who filed in 1993, 1994 for those who did not.

Table 7. Average Percentage Growth in AGI

	1993-94 Control Group				1995 Control Group			
	EZ w/ 1 1993/4 match	Closest Match	EZ w/ 3 1993/4 matches	3 Closest Matches	EZ w/ 1 1995 match	Closest Match	EZ w/ 3 1995 matches	3 Closest Matches
Base-1995	143	77	147	76	137	53	127	39
Base-1996	255	153	250	119	246	76	154	60
Base-1997	314	194	324	185	327	93	185	77

Note: All figures are in nominal dollars. Base Year is 1993 for all taxpayers who filed in 1993, 1994 for those who did not.

Table 8. Wage Growth Regressions using Sample of EZ Participants and the 1993/1994 Control Group

Regressor	Program year: 1995		Program year: 1996		Program year: 1997	
	Closest match	Closest 3 matches	Closest match	Closest 3 matches	Closest match	Closest 3 matches
Intercept	1.511***	1.816***	3.389***	2.792***	4.661***	4.512***
	(.410)	(.282)	(.869)	(.498)	(1.252)	(.769)
EZ participant	.773*	.777*	1.369	1.649*	1.507	1.669
	(.397)	(.362)	(.842)	(.638)	(1.212)	(.986)
Filing status						
Joint	-.312	-.491	-1.472	-1.124	-2.200	-2.139
	(.570)	(.515)	(1.208)	(.908)	(1.740)	(1.403)
Separated	.754	-.242	.276	-.348	-.601	-1.475
	(1.447)	(1.008)	(3.067)	(1.779)	(4.418)	(2.747)
HH head	-.098	-.570	-.285	-.853	-1.451	-2.430
	(.627)	(.546)	(1.330)	(.963)	(1.915)	(1.488)
Children						
1	.626	1.036	.393	.687	.109	1.061
	(.630)	(.556)	(1.336)	(.981)	(1.924)	(1.515)
2 or more	.274	.349	-.236	.150	-.507	.099
	(.539)	(.463)	(1.143)	(.818)	(1.646)	(1.263)
Base wage/10,000	-.482***	-.536***	-.749***	-.659**	-.853*	-.860*
	(.124)	(.114)	(.263)	(.202)	(.379)	(.311)
Dep. Var. Mean	1.103	.972	2.086	1.519	2.551	2.191
N	200	304	200	304	200	304
R-squared	.114	.123	.095	.097	.070	.084

NOTE: The dependent variable is growth in nominal wages from the base year to the program year. The base year is the first year (1993 or 1994) for which EZ participant data are available, either 1993 or 1994. ***-p<.001, **-p<.01,*-p<.05.

Table 9. AGI Growth Regressions using Sample of EZ Participants and the 1993/1994 Control Group

Regressor	Program year: 1995		Program year: 1996		Program year: 1997	
	Closest match	Closest 3 matches	Closest match	Closest 3 matches	Closest match	Closest 3 matches
Intercept	1.469***	.765***	3.363***	2.844***	4.643***	4.505***
	(.378)	(.265)	(.806)	(.461)	(1.141)	(.701)
EZ participant	.660	.693*	1.023	1.303*	1.191	1.383
	(.360)	(.334)	(.769)	(.580)	(1.089)	(.883)
Filing status						
Joint	-.339	-.729	-1.307	-1.170	-1.948	-1.972
	(.517)	(.467)	(1.105)	(.812)	(1.564)	(1.237)
Separated	.518	-.444	.174	- .504	-.700	-1.601
	(1.338)	(.952)	(2.858)	(1.655)	(4.046)	(2.520)
HH head	.082	-.535	-.192	-.831	-1.363	-2.288
	(.577)	(.512)	(1.233)	(.890)	(1.745)	(1.356)
Children						
1	1.000	1.164*	.718	.868	.142	.979
	(.576)	(.516)	(1.230)	(.896)	(1.742)	(1.365)
2 or more	.383	.442	.126	.356	-.050	.258
	(.482)	(.416)	(1.030)	(.723)	(1.458)	(1.102)
Base AGI/10,000	-.459***	-.459***	-.749***	-.635***	-.853**	-.824**
	(.109)	(.100)	(.232)	(.173)	(.329)	(.264)
Dep. Var. Mean	1.100	.940	2.037	1.518	2.540	2.199
N	208	320	208	320	208	320
R-squared	.131	.130	.097	.101	.073	.088

NOTE: The dependent variable is growth in nominal AGI from the base year to the program year. The base year is the first year (1993 or 1994) for which EZ participant data are available, either 1993 or 1994. ***-p<.001, **-p<.01,*-p<.05.

Table 10. Wage Growth Regressions using Sample of EZ Participants and the 1995 Control Group

Regressor	Program year: 1995		Program year: 1996		Program year: 1997	
	Closest match	Closest 3 matches	Closest match	Closest 3 matches	Closest match	Closest 3 matches
Intercept	3.363**	2.427***	4.447***	2.718***	4.809**	2.984***
	(.958)	(.569)	(1.240)	(.511)	(1.573)	(.437)
EZ participant	.174	.663	1.326	.811	2.290	1.029*
	(.907)	(.650)	(1.173)	(.583)	(1.489)	(.499)
Filing status						
Joint	-1.038	-.847	-1.762	-.898	-1.522	-.758
	(1.331)	(.900)	(1.722)	(.807)	(2.185)	(.690)
Separated	-.829	-.294	-1.126	.597	-5.086	-2.905
	(4.108)	(3.171)	(5.314)	(2.844)	(6.742)	(2.432)
HH head	-.898	-1.003	-1.177	-.825	-1.816	-1.748*
	(1.397)	(.984)	(1.806)	(.883)	(2.292)	(.755)
Children						
1	-.091	.037	-.593	-.245	-.566	.090
	(1.468)	(.953)	(1.922)	(.855)	(2.438)	(.731)
2 or more	.115	.126	-.406	-.033	-1.239	-.217
	(1.225)	(.801)	(1.585)	(.718)	(2.011)	(.614)
Base wage/10,000	-.715*	-.462***	-.908*	-.485***	-1.030*	-.529***
	(.283)	(.160)	(.366)	(.143)	(.464)	(.122)
Dep. Var. Mean	1.486	.996	2.173	1.184	2.462	1.258
N	162	252	162	252	162	252
R-squared	.068	.063	.088	.093	.081	.144

NOTE: The dependent variable is growth in nominal wages from the base year to the program year. The base year is the first year (1993 or 1994) for which EZ participant data are available, either 1993 or 1994. ***-p<.001, **-p<.01,*-p<.05.

Table 11. AGI Growth Regressions using Sample of EZ Participants and the 1995 Control Group

Regressor	Program year: 1995		Program year: 1996		Program year: 1997	
	Closest match	Closest 3 matches	Closest match	Closest 3 matches	Closest match	Closest 3 matches
Intercept	1.393**	1.184***	2.648**	1.599***	3.684**	2.200***
	(.444)	(.221)	(.954)	(.253)	(1.388)	(.298)
EZ participant	.890*	.901***	1.777*	.964***	2.403	1.050**
	(.417)	(.250)	(.897)	(.286)	(1.305)	(.337)
Filing status						
Joint	-.669	-.780*	-1.368	-.870*	-1.664	-.876
	(.609)	(.344)	(1.308)	(.394)	(1.903)	(.464)
Separated	-.506	.676	-1.082	1.381	-3.879	-1.217
	(1.917)	(1.249)	(4.122)	(1.432)	(5.997)	(1.686)
HH head	.207	-.643	-.165	-.541	-.976	-1.147*
	(.646)	(.379)	(1.390)	(.435)	(2.021)	(.512)
Children						
1	.246	.550	-.156	-.397	-.297	.430
	(.678)	(.366)	(1.458)	(.420)	(2.121)	(.494)
2 or more	.469	.595*	.016	.493	-.587	.252
	(.558)	(.304)	(1.200)	(.348)	(1.746)	(.410)
Base AGI/10,000	-.406**	-.251***	-.628*	-.305***	-.788*	-.386***
	(.123)	(.060)	(.265)	(.069)	(.386)	(.081)
Dep. Var. Mean	.948	.611	1.611	.831	2.101	1.039
N	168	268	168	268	168	268
R-squared	.135	.163	.091	.173	.074	.171

NOTE: The dependent variable is growth in nominal AGI from the base year to the program year. The base year is the first year (1993 or 1994) for which EZ participant data are available, either 1993 or 1994. ***-p<.001, **-p<.01,*-p<.05.

Table 12. 1995-97 Regression Results Using the 1995 Control Group, all present in 1995 and 1997 included

Regressor	Wage growth		AGI growth	
	Closest match	Closest 3 matches	Closest match	Closest 3 matches
Intercept	2.451***	2.084***	2.398***	2.002***
	(.389)	(.226)	(.388)	(.224)
EZ participant	-.137	.259	-.171	.206
	(.368)	(.269)	(.366)	(.264)
Filing status				
Joint	-.611	-.448	-.721	-.522
	(.680)	(.439)	(.690)	(.442)
Separated	-.333	-.680	-.845	-.904
	(1.113)	(.755)	(1.106)	(.745)
HH head	-.920	-.789	-.880	-.801
	(.685)	(.475)	(.682)	(.470)
Children				
1	.895	.523	.711	.401
	(.687)	(.462)	(.683)	(.457)
2 or more	.133	.073	.163	.082
	(.601)	(.402)	(.602)	(.401)
1995 wage/10,000	-.475***	-.424***		
	(.118)	(.071)		
1995 AGI/10,000			-.398***	-.353*
			(.140)	(.066)
Dep. Var. Mean	1.204	1.009	1.287	1.101
N	262	472	262	472
R-squared	.087	.094	.094	.103

NOTE: The dependent variables are growth in nominal wages and nominal AGI from 1995 to 1997. ***-p<.001, **-p<.01,*-p<.05.

Table 13. Robustness Tests: EZ Coefficient for Regressions using Closest 3 Matches

	Matched Sample[1]	Matched Sample, Outliers Removed[2]	Unrestricted Sample[3]
1993 match sample			
Wage growth			
1993-95	.777 (.362)*	.693 (.331)*	.560 (.274)*
1993-96	1.649 (.638)*	.792 (.386)*	1.751 (.505)***
1993-97	1.669 (.986)	.387 (.577)	1.639 (.774)*
AGI growth			
1993-95	.683 (.334)*	.628 (.306)*	.479 (.253)
1993-96	1.303 (.580)*	.598 (.363)	1.180 (.453)**
1993-97	1.383 (.883)	.293 (.520)	1.313 (.710)
1995 match sample			
Wage growth			
1993-95	.663 (.650)	1.021 (.301)***	.797 (.430)
1993-96	.811 (.583)	1.108 (.348)**	1.957 (.640)**
1993-97	1.029 (.499)*	1.160 (.467)*	2.283 (.823)**
AGI growth			
1993-95	.901 (.250)***	.910 (.253)***	.713 (.279)*
1993-96	.964 (.286)***	.970 (.290)***	1.513 (.493)**
1993-97	1.050 (.337)**	1.051 (.341)**	2.163 (.732)**

Notes:
1. Matched Sample includes observations for which the data are available for both the EZ participant and all control group matches in all relevant years.
2. Outliers are defined as individuals whose wages or AGI in one year was more than 50 times greater than in the previous year.
3. Unrestricted Sample includes all individuals with data available in the relevant years without regard to whether or not the individual's matches also have available data.