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The Employment and Earnings Impacts of the Targeted Jobs Tax Credit

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**THE EMPLOYMENT AND EARNINGS IMPACTS OF
THE TARGETED JOBS TAX CREDIT**

Upjohn Institute Staff Working Paper 91-07

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Additional results for other target groups and copies of the computer programs used to generate the results presented in the paper are available from Kevin Hollenbeck at the W.E. Upjohn Institute for Employment Research, 300 S. Westnedge Avenue, Kalamazoo, MI 49007. The data reside at the Center for Education and Training for Employment, The Ohio State University, Columbus, OH 43210.

THE EMPLOYMENT AND EARNINGS IMPACTS OF THE TARGETED JOBS TAX CREDIT

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Abstract

The Targeted Jobs Tax Credit (TJTC) is intended to stimulate the employment of individuals who are members of certain groups of the labor force by providing a wage subsidy (in the form of a tax credit) to employers of recently-hired eligible workers. This intervention into the labor market has direct and indirect earnings and employment consequences for both eligible and ineligible individuals. The paper evaluates the impacts of TJTC by using a treatment and comparison group methodology. Corrections for nonrandom selection are undertaken. The primary sources of data are state quarterly wage record data from the Unemployment Insurance system and the Employment Service Automated Reporting System (ESARS).

The results indicate that the availability and usage of TJTC enhances outcomes for nonwhite male youth (both eligible and ineligible), but is stigmatizing for eligible individuals from other race/sex groups, who appear to be slightly worse off because of the program than their ineligible counterparts. Obtaining a voucher increases employment and wages, but it appears as if selection effects are responsible. Importantly, the improved outcomes are not accompanied by displacement effects. Finally, being certified results in increased wages, but higher turnover and lower total employment.

THE EMPLOYMENT AND EARNINGS IMPACTS OF THE TARGETED JOBS TAX CREDIT

Government intervention into the labor market for the purpose of assisting disadvantaged individuals usually occurs on the supply side. The training programs funded by the Job Training Partnership Act (JTPA) and those of its predecessor, the Comprehensive Employment and Training Act (CETA) Title I, are and were intended to enhance the human capital and productivity of participants.

The Targeted Jobs Tax Credit (TJTC) program is a demand-side intervention in the private labor market. Employers are given a tax credit for a portion of the wages they pay to recently-hired workers in certain eligibility categories. As originally authorized by the Revenue Act of 1978, TJTC provided employers a credit of 50 percent of wages paid in the first twelve months of employment, up to a maximum of \$6000, and 25 percent of wages (subject to the same maximum) paid in the next 12 months. Thus, the maximum credit for a worker was \$4,500. The target groups were: Economically disadvantaged youth aged 18-24, youth aged 18-24 in a cooperative education program, economically disadvantaged Vietnam veterans under 35, economically disadvantaged ex-offenders, handicapped persons receiving or having completed vocational rehabilitation, general assistance recipients, and SSI recipients. The Economic Recovery Tax Act of 1981 eliminated eligibility for cooperative education students and added two new target groups--AFDC recipients and involuntarily terminated CETA/PSE employees. The Tax Equity and Fiscal Responsibility Act of 1982 added a new target group--economically disadvantaged students 16-17--for whom employers could receive an 85 percent credit on up to \$3000 paid in Summer wages. The Deficit Reduction Act of 1984 eliminated the CETA/PSE terminnee target group and extended the program to December 31, 1985.

The program terminated on that date, but was reauthorized through 1988 under the Tax Reform Act of 1986. This Act limited the credit to 40 percent of first year wages up to \$6000, eliminated the second year allowance, and required a minimum of 90 days or 120 hours of employment to claim a credit. The Technical and Miscellaneous Revenue Act of 1988 extended the credit through 1989, but reduced the age limit for disadvantaged youth from 24 to 22 and cut the credit for Summer youth employees from 85 to 40 percent. The Omnibus Budget Reconciliation Act of 1989 extended the program through September, 1990 and required employers to certify that a "good faith effort" was made to determine individuals' eligibility prior to hiring.^{1,2}

Two basic forms are used: a voucher and a certification. A voucher is issued by the Employment Service or other vouchering agency to an eligible job seeker. The individual presents the voucher form to the employer, who, after deciding to hire the applicant, completes the employer declaration and returns the form to the Employment Service. If an employer plans to hire an individual who is thought to be eligible but does not have a voucher, the employer may request a certification in writing from the Employment Service. The certification is completed by the Employment Service after receipt of the employer declaration or certification request and is sent to the employer as back-up documentation for tax purposes.

The government does not know who might hire a disadvantaged job seeker if a subsidy is offered, so the TJTC is operated as an entitlement, i.e., any employer may receive the (refundable) credit. For a reasonably generous subsidy, the number of firms that have received the credit and the percentage of eligible individuals that have been vouchered are small. Through 1985, 6 million vouchers had been issued and 2.6 million certifications had been granted. Bishop and Kang (1988) analyze employer participation and Christensen (1984) analyzes target group member participation. The Wall Street Journal (November 7, 1989) reports that participation by firms may be as low as 1 percent.

Other examinations of the TJTC include Lorenz (1985, 1988), Burtless (1985), Arwady (1988), and Levitan and Gallo (1987). Lorenz (1985, 1988) finds, using a relatively long-run data set, that TJTC benefits are concentrated on a relatively small set of job finders with higher wages. The studies are weakened, however, by the choice of the treatment and comparison groups--vouchered individuals where certifications were issued versus vouchered, but noncertified individuals. Burtless (1985) examines data from a rigorous, classical experiment with random assignment to a control and two treatment groups who were given wage subsidy vouchers. Both treatment groups fared worse in the labor market, which was attributed to vouchering. However, this study lacks geographic generalizability (it was performed in a single urban location); it is based on small sample sizes; and it has only limited data on the experiment's participants.³ The Arwady (1988) study is a case study of widespread usage of the TJTC in a single corporation, whereas the gist of the Levitan and Gallo (1987) paper is that no conclusive evidence exists concerning TJTC's impacts, so it is premature to alter the policy based on its merit.

The present study reports on findings and analyses of one part of a large, nationwide evaluation of the TJTC program. That evaluation included a process analysis, a cost-benefit analysis, and impact analyses for four target groups using data from twelve representative states. In this paper, we focus on the effects TJTC had on the earnings and employment of the disadvantaged youth target group only.

In a classical experimental sense, three "treatments" comprise the TJTC program--eligibility, vouchering, and certification. Correspondingly, this study evaluates the employment and earnings impacts of all three. For each of the analyses, "pre- and post-treatment" outcome data for a treatment and a comparison sample of individuals are analyzed. Outcomes examined are quarters of employment and average quarterly wages (unconditional and conditioned on employment). Employment and earnings data come from states' Unemployment Insurance quarterly wage record systems. Those data were merged to data from the Employment Service Automated Record System (ESARS), which contained important individual background information.

Evaluating Treatment Effects with Nonexperimental Data

The problem of properly isolating and estimating treatment effects in nonexperimental data is the subject of continuing controversy and research.⁴ Although most of the literature on this subject has centered around training interventions rather than demand-side subsidies, the empirical considerations of the two problems are very similar.

A standard linear specification for an outcome, say earnings for example, might be as follows:

$$(1) \quad Y_{it} = \underline{b}'\underline{X}_{it} + c_t T_i + e_i + e_t + e_{it}$$

where Y_{it} is earnings (or log earnings) for individual i in period t , \underline{X}_{it} is a vector of characteristics usually including education and age or experience, \underline{b} is the corresponding vector of coefficients, T_i is a binary variable for having received the treatment, c_t is the effect of the treatment in period t , e_i is an unobserved individual effect constant over time, e_t is an unobserved time-period effect constant over individuals, and e_{it} is an unobserved random effect, possibly autocorrelated. Nonrandom selection occurs when the unobserved individual effects e_i or e_{it} are correlated with the treatment variable.

Ashenfelter (1978) used an autoregressive earnings function to analyze Manpower Development and Training Act (MDTA) classroom training effects. Cain (1975) and Goldberger (1972) pointed out that if pre-program earnings are the sole selection criteria, then an autoregressive earnings function can consistently measure program effects. Bassi (1983) noted, however, if the error term is autocorrelated, as it is likely to be in an earnings equation, this method will produce biased and inconsistent results. Other shortcomings of this approach have been discussed (e.g., Cooley, McGuire, and Prescott 1981, Director 1979, Nickell 1979, and Bloom 1984).

Kiefer (1979), in another analysis of MDTA impacts, utilized a fixed-effect model that eliminated the individual fixed effect e_i , thereby eliminating concern about correlation between e_i and T_i . However, e_{it} may be a selection factor itself; for instance, only those with temporarily low earnings may be selected. Kiefer addressed this problem by obtaining an instrumental variables estimate of T_i for use in the wage deviation equation.

Barnow, Cain, and Goldberger (1981) provided a nonlinear technique to control directly for sample selection bias based on earlier work by Heckman (1976) and Lee (1978). The approach is to model selection directly in a stochastic decision model. One receives the treatment ($T_i = 1$) if

$$(2) \quad \underline{d}'\underline{Z}_{is} + u_{is} > 0,$$

where \underline{Z}_{is} is a vector of observable characteristics in period s , the period prior to treatment, \underline{d} is a vector of coefficients, and u_{is} is a random error term. Sample selection, in which u_{is} is correlated with e_{it} can be handled (under the assumption of joint normality of u_{is} and e_{it}) by estimating the coefficients in (2) using standard probit analysis, calculating the expected value of u_{is} given the known information (via the inverse Mills' ratio), and including it in the wage equation (3):

$$(3) \quad Y_{it} = \underline{b}'\underline{X}_{it} + c_t T_i + r_{ue} k_i + v_{it}$$

Here, $k_i = E(u_{is} * Z_{is}, T_i)$ and $r_{ue} = \text{cov}(u_{is}, e_{it})$. Under the assumptions stated, this extra term purges the correlation of T_i and e_{it} . The implications of autocorrelation of the e_{it} for first-differencing have been considered by Willke (1985).

Finally, Ashenfelter and Card (1985) estimated the effects of CETA training using only means and other moments of trainee and control earnings for a nine-year period. They use a components-of-variance model employing permanent and transitory error components, as well as an individual earnings growth parameter with nonrandom selection into training.

Estimating TJTC Treatment Effects

With TJTC, eligibility and certification "treatments" are not subject to nonrandom selection. Eligibility is an entitlement to all members of the target populations. It is subject to neither self-selection nor agency selection. Certification is exempt from nonrandom selection, on the other hand, by appropriate choice of comparison groups. Therefore, in the models presented below, only the vouchering treatment effects correct for selection. The Barnow, Cain, and Goldberger approach is most appropriate for this problem, given the relatively short time series on income and the extensive background data available.

Eligibility. The notion of an eligibility impact is somewhat difficult conceptually since eligibility is not an intervention applied to or chosen by the affected population. Rather, the impact emanates from employers altering their hiring behavior and/or individuals altering their labor force behavior as a result of the TJTC program. The most direct result should be increased employment (certification) of recognized TJTC eligibles (those vouchered) due to the tax credit incentive.

The TJTC intended impact on eligibles would thus be expected to be an increasing function of vouchering and certification rates in the area, and to be zero if there is no TJTC program or activity at all. However, TJTC may have indirect effects. Being vouchered may be stigmatizing (see Burtless 1985), in that it identifies the individual as being disadvantaged and possibly on welfare. Such stigma should be diluted by increased familiarity through widespread vouchering, i.e. by higher rates of vouchering and certification.

The TJTC effects on ineligibles may be positive or negative. TJTC is intended to create new jobs, but it may be that tax-advantaged workers displace those whose wages are not subsidized. Higher vouchering and certification rates would then cause greater reductions in the employment and earnings of ineligibles. On the other hand, these wage subsidies may stimulate business to the extent the employment of all workers is increased.

Construction of the empirical model for estimating TJTC effects on eligible and ineligible populations therefore must include the effects of vouchering and certification activity in the appropriate market area, with separate effects for eligibles and ineligibles, and an allowance for an eligibility "shift" effect even at zero levels of TJTC activity, to provide for stigma or other indirect effects.⁵ To identify these effects, the estimation sample must include eligibles,

ineligibles who are 18-24 but not disadvantaged, and ineligible who are disadvantaged but outside the age range, that is over the age of 24.

The outcome equation, in general terms, is then

$$(4) \quad Y_{ijt} = a_0 + a_1A_{it} + a_2A_{it}^2 + a_3D_i + a_4D_i*A_{it} + a_5D_i*A_{it}^2 + a_6E_i + a_7PV_j + a_8E_i*PV_j + a_9PC_j + a_{10}E_i*PC_j + \underline{c}'Z_{ijt} + e_i + e_t + e_{it}$$

The outcome variable, Y_{ijt} , is indexed by individual i in region j for time period t . Age, denoted by A_i , enters the model quadratically as in the standard Mincerian age-earnings profile. Disadvantaged status, D_i , and its interaction with age create a separate disadvantaged age-earnings profile, and the eligibility treatment dummy, E_i , allows the latter profile to be shifted during ages 18-24.⁶ PV , the local vouchering penetration rate (pro-portion of eligibles vouchered), captures the average vouchering effect on ineligible, while that variable together with its interaction with the treatment dummy show the total vouchering effect on eligibles. The effect of certification is modeled similarly using the certification penetration rate PC . Other control variables, including education, an urban-rural indicator, and local employment conditions are in \underline{Z} .

Equation (4) contains unobservable fixed individual and time period effects, e_i and e_t , respectively. Correlation between these two effects would result in biased coefficient estimates for this equation. For estimation purposes, (4) was translated to an earnings change equation under the assumption that the TJTC program effect was entirely produced during the encounter or treatment year. The treatment is considered to take place at the time the individual contacts the Employment Service.

Vouchering. The principal treatment effect for vouchering is expected to be manifested in an outcome equation of the form:

$$(5) \quad Y_{ijt} = b_0 + b_1V_{ij} + b_2PV_j + b_3V_{ij}*PV_j + b_4PC_j + b_5V_{ij}*PC_j + \underline{d}'Z_{ijt} + e_i + e_t + e_{it}$$

where V_{ij} is the "treatment" dummy representing whether the individual was vouchered. The coefficient on the voucher-voucher penetration rate interaction, b_3 , measures whether there are increasing or decreasing returns to agency vouchering activity. The vector \underline{Z}_{ijt} contains other variables expected to affect earnings, much like those in the eligibility model. As in that model, outcome changes were examined so as to eliminate possible bias resulting from correlation of the treatment variables and the individual fixed effect. Thus (5) was estimated in first-differenced form.

Selection bias is still possible, however, if the voucher treatment (often the choice of Employment Service personnel) is correlated with the random error e_{is} in the pre-program period. The Barnow, Cain, and Goldberger correction for sample selection was used to correct for selection bias as a second empirical strategy. In this case,

$$(6) \quad V_{ij} = f(PC_j, W_i, VARY_i, \underline{Z}_{ij}, v_{ij})$$

where W_i = welfare status of individual i

$VARY_i$ = a measure of variation in previous earnings
 v_{ij} = a random error term, having a joint bivariate normal distribution with e_{is}
 \underline{Z}_{ij} = other individual characteristics, not necessarily the same as in equation (5).

Being on welfare may reveal a smaller "stigma" effect of vouchering, and variation in earnings may reveal higher potential ability. The function $f(\cdot)$ was estimated with a probit technique. The joint distribution of v_{ij} and e_{is} is due to unobserved factors that affect both vouchering and outcomes. The correlation between e_{is} and v_{ij} can be corrected by estimating (7):⁷

$$(7) \quad (Y_{ijt} - Y_{ijs}) = b_1 V_{ij} + b_2 PV_j + b_3 V_{ij} * PV_j + b_4 PC_j + b_5 V_{ij} * PC_j + b_6 k_{ij} + \underline{d}'(\underline{Z}_{ijt} - \underline{Z}_{ijs}) + (e_{it} - w_{is})$$

The results of estimating (5) in differenced form without the selection correction and (7) with the selection correction are compared to test for the presence of selection. In both models, b_1 , b_3 , and b_5 will be positive if TJTC favorably affects vouchered individuals. If displacement is present because of vouchering, b_2 and b_4 will be negative. If positive selection is present (i.e. creaming), then b_6 in (7) will be positive.

Certification. By limiting the comparison group for the certification study to other job finders, attention is focused on performance on the job, and the sample selection problem is mitigated. The outcome equation is:

$$(8) \quad (Y_{ijt} - Y_{ijs}) = c_0 + c_1 C_{ij} + c_2 PV_j + c_3 C_{ij} * PV_j + c_4 PC_j + c_5 C_{ij} * PC_j + \underline{d}'(\underline{Z}_{ijt} - \underline{Z}_{ijs}) + (e_{it} - e_{is})$$

Here c_1 represents the effect of certification, C_{ij} , on the outcome after controlling for the variables present in \underline{Z}_{ijt} , which will be those used in the other treatment effect studies. Once again, examining outcome changes eliminates the fixed effect problem, and again positive TJTC certification impacts imply c_1 , c_3 , and c_5 greater than zero and displacement will be suggested if c_2 and c_4 are less than zero.

Data

Data from several sources were utilized for this analysis. For the eligibility study, a random sample of automated Employment Service records (also known as the ESARS data) for individuals in twelve states was chosen.⁸ Only those individuals encountering the ES for the first time in 1982 were selected for this study, because complete data for ES services prior to 1982 were not available and thus could not be controlled. This condition creates a group of TJTC-eligibles and ineligibles whose first exposure to TJTC occurred in 1982, thus specifying 1982 as the year of treatment. For the individuals selected in this way, quarter-by-quarter earnings from 1980 to 1984 were obtained from Unemployment Insurance wage records. The absence of earnings for a quarter was assumed to indicate no employment in that quarter. Penetration rate and other location-specific variables were constructed by county. The voucher and certification

samples originated with individuals randomly selected from 1982 TJTC voucher and certification records at twenty-eight local ES offices among the twelve states.

Changes in average quarterly wages, average quarters employed per year, and average wages in employed quarters were the primary outcome variables examined. In each case, the "prior" period was the eight quarters of 1980 and 1981. The decision to use this period reflects a compromise between choosing the earlier year as probably freer of selection bias and choosing the latter year because of greater full-time labor force participation and hence greater comparability to later years. The "post" period used was the eight quarters of 1983 and 1984, which should represent a combination of short-run and long-run effects. The treatment year, 1982, was excluded because of the unemployment likely to be present, but this choice may underemphasize short-run effects. Table 1 gives means and standard deviations for year-by-year earnings, employment outcomes, and other background variables.

In the eligibility study data, ineligibles have mean earnings and employment levels consistently above those of eligibles. This difference is due in large measure to the 2.8 year mean age difference resulting from the age limit on eligibles and the inclusion of older ineligibles (aged 25-29) as comparison group members. Both groups have similar temporal patterns of outcome changes, however. Treatment and comparison group mean levels are much more similar for the voucher and certification studies, where there are no significant age differences. The patterns of earnings and employment growth here show a larger increase for treatment groups than for comparison groups in 1983, the first year following treatment. The 1984 levels become more equal, and average earnings per quarter employed stay fairly equal throughout, suggesting a temporary TJTC effect on employment.

Empirical Results and Discussion

The eligibility, vouchering, and certification impact models were estimated for four target groups in earlier work and complete results are reported in Hollenbeck, Willke, and Ershadi (1986). Here, the impact results for the disadvantaged youth target group only are presented. The models were estimated separately for four race/sex populations.

Eligibility impacts. Table 2 presents selected results of the eligibility impact analysis. It shows individual coefficient estimates for the variables that measure TJTC activity for each outcome and race/sex group. Also shown are the net effects of all TJTC variables combined, evaluated at penetration rate means, for eligibles and ineligibles. The figures shown are estimated effects on the change in the average quarterly outcome (total wages, quarters employed) between 1980-81 and 1983-84. That is, while average quarterly wages increased for both eligibles and ineligibles over this period, the net effect for eligibles shows whether average wages of eligibles in areas of average TJTC activity increased more or less than those of ineligibles in areas with no TJTC activity.

For eligible black/Hispanic males, the existence of the TJTC entitlement has a positive effect on wages, and essentially no impact on total employment. For all other race/sex groups,

the results presented in table 2 are consistent with the hypothesis of TJTC having a stigmatizing influence. All of the net effects for eligibles are negative for these groups. This suggests, assuming that the model has adequate controls, that eligible white males and white and black/Hispanic females experience lower wages and lower rates of employment because of the existence of the credit. Eligible females, in particular, have large (negative) employment impacts. Buttressing the stigma hypothesis is the fact that the net effects on ineligibles suggest that increased rates of vouchering and certification lead to increases in employment. However, the increased employment is at lower levels of wage income (except for nonwhite males.)

The model that was estimated allows us to disentangle the influence of the program factors in explaining outcomes. The first column of estimates shows the direct influence of the "treatment," i.e., being eligible.⁹ The next two columns capture the net impact of voucher penetration. Because certification is controlled separately, voucher penetration will not capture any job creation. Rather, it is intended to control for possible effects of differences in vouchering practice by the employment agency, i.e., from very selective vouchering to widespread "broadcast" vouchering. To estimate the order of magnitude of these effects, recall that table 1 shows that the mean penetration rate is about 15 percent. The impact of certification penetration can be calculated from the estimates in the 4th and 5th column (the mean penetration rate was around 1.7 percent).

For most outcomes, the net effect on eligibility approximates the direct effect of the "treatment," suggesting that the levels of vouchering and certification had little influence. The exceptions to this are for the net impact on quarters employed, which is positive for all groups but white females, for whom the net effect is approximately zero. These results reflect the intended outcome of the TJTC program, i.e., increased employment of the eligible group. Using the coefficients for white males, the net impact of a one percentage point increase in certification is an increase of .022 quarters worked for the average eligible. Put differently, a certification rate of 45 percent (all else equal) would increase (the change in) the number of quarters employed by 1 full quarter for each eligible.

TJTC effects on ineligibles (a_7 and a_9) show interesting patterns. Certification penetration has a significantly positive effect on quarters employed in all race/sex groups. TJTC job creation was apparently associated with significant job expansion in general, either as cause or effect. Thus, no net displacement is indicated here. However, the negative wage effects of certification on whites but positive wage effects for blacks suggest that job creation was probably in the low-paying end of the wage spectrum, possibly at the expense of higher-paying jobs. While the effects of voucher penetration varied somewhat, the signs of the net "displacement" effects shown in table 2 generally follow the signs of the certification effects.

Voucher impacts. The voucher impacts for the youth target group are presented in table 3. Again, the outcome measures are differences between 1983-84 quarterly averages and 1980-81 quarterly averages. The impact models were estimated two ways for the same four race/sex groups discussed above for the eligibility impacts. The differenced model described above was estimated using OLS and using the nonlinear selectivity correction suggested by Barnow, Cain,

and Goldberger (1981) (see equation 7). As a point of reference, the revenue loss to the Treasury per voucher through 1986 was about \$350.

The gross vouchering impacts for average wages during quarters employed (shown in column 1) from the OLS estimation were generally not significant except for nonwhite males, where they were negative. The (OLS) employment impacts, on the other hand, were positive and fairly large for males, and positive, although not significant, for females. These two findings suggest that vouchering has a positive influence on employment, but the jobs have similar or perhaps lower wages than the jobs obtained by the comparison group (eligible, but nonvouchered youth).

The OLS net impacts of vouchering after taking into account the penetration rate effects (the sixth column in the table) show a considerable dampening of the gross impacts. For example, for nonwhite females, the average wages during quarters employed impact fell from \$162 to \$30 and the average quarters employed impact fell from .18 to .02. Similar declines occurred for virtually all the outcome variables for all the race/sex groups. This suggests a strong declining returns to scale effect for vouchering, i.e. the more vouchering performed by program administrators, the smaller the impacts of the voucher on employment and earnings. The net effects on the comparison groups, which are referred to in the table as displacement effects, suggest some employment displacement for all groups except white males, with the magnitude of those effects particularly large for nonwhites (-.17 and -.20 quarters for females and males, respectively).

Referring back to equation (7), the coefficient on the k_{ij} variable (sometimes referred to as Heckman's lambda) measures the correlation of the outcome change with the likelihood of being selected for vouchering after controlling for known characteristics. A positive value might be interpreted as evidence of "creaming." The white male youth group shows larger voucher coefficients than in the uncorrected regressions, and thus must be the only group to have negative (lambda) coefficients.¹⁰ To the extent that the assumptions of this estimation technique are met, this result indicates that those selected for vouchering would, other things equal, have smaller increases in earnings, but the vouchering itself has a large impact. The results for black males and both female groups are just the opposite, however. The selectivity coefficients are significantly positive, suggesting that the vouchered individuals were most likely to have greater earnings and employment increases anyway, and the impact of vouchering was to dampen their improvement. Neither effect is implausible, but the size of the effect for white females is rather large. (This may be attributable to multicollinearity of the voucher variable and k_{ij} .) The reason that white male youth may be different from the other groups is that they may be the least likely to be discriminated against, so only the least employable were vouchered, while vouchers were given to the most employable of the other groups.

Certification impacts. The final set of impacts to be discussed--impacts of being certified--are presented in table 4. In addition to average quarterly wages and quarters of employment between 1983-84 and 1980-81, the change in number of quarters worked per employer was used as a measure of turnover/retention. The comparison group in this analysis was economically disadvantaged youth who were not certified but who found jobs. Because both groups were

employed, it was not necessary to estimate selectivity-corrected models. The gross impacts of being certified were insignificantly different from zero for both quarters employed and quarters worked per employer. All of the groups except nonwhite males, however, had large positive wage impacts that were estimated to be on the order of \$550 to \$700 per quarter. The average tax credit per certification was about \$830. The net impacts were again smaller than the gross impacts which suggests decreasing returns to vouchering; however, the wage effect was still positive and relatively large. The retention impacts tended to have negative signs, but were not significant.

The displacement estimates for the certification impact analysis suggest that there is some substitution of TJTC certified workers for disadvantaged, noncertified workers. When the vouchering and certification penetration rates increase, the earnings and employment outcomes for the comparison group decline. This displacement is particularly large for males. For females, the displacement estimates are still mostly negative, but are unlikely to be statistically significant.

Summary. Table 5 summarizes the major results for the eligibility, vouchering, and certification studies. The results of the eligibility study generally confirm the hypothesis of stigma, particularly for females. The existence of TJTC (the eligibility "treatment") seemed to have negative employment and wage consequences for all race/sex groups in the eligible population, except for nonwhite males. The latter group experienced positive outcomes. The comparison group for the eligibility study, nondisadvantaged youth and disadvantaged individuals aged 25-29, were not displaced by TJTC and in fact, experienced slightly better employment outcomes when vouchering and certification rates were higher. However, net impacts on wages were zero or negative. The impact on ineligibles should be weighted heavily by the fact that this group is the largest share of the general population of any of the treatment or comparison groups. Besides the positive impacts on nonwhite males, the only other benefit of TJTC indicated by the eligibility study was that the certification penetration rate was directly related to increased employment, albeit not nearly enough to overcome the large negative "gross" impacts.

Given the existence of TJTC (and its possible stigmatizing effects), the next question addressed was the effects of actually obtaining a voucher. These impacts were mixed. The vouchering impacts tended to be positive for quarters of employment and zero or negative for wages while employed. The selectivity correction technique suggested that selection was evident at the vouchering stage, with "creaming" occurring for white females and nonwhite males and females. The selection effect seemed to be just the opposite for white males, however, where it appeared the least employable were selected for treatment. The positive employment impacts do not seem to generally cause displacement of whites, but do indicate displacement of nonwhite, disadvantaged youth.

Finally, for youth that were (vouchered and) certified, large positive earnings impacts were estimated. However, the youth in the treatment group had higher turnover and fewer quarters of employment. Increases in the vouchering and certification rates tended to cause some displacement of noncertified employees by certified workers, but at the low-wage end of the scale.

Using nonexperimental data with its advantages (large sample sizes, low cost, and accurate portrayal of actual program operations) and disadvantages (potential selection bias), this study's findings echo the lackluster outcomes of TJTC found in most previous analyses. At best, TJTC is only slightly alleviating the labor market problems of disadvantaged youth. At worst, it is stigmatizing such youth and causing displacement of them by nondisadvantaged individuals. (Previous experimental evidence seems to confirm the stigmatizing nature of a wage subsidy where individuals announce their eligibility to potential employers.) Vouchering does create employment for youth, but statistical evidence suggests that (agency) selection may have been at work. Individuals who were certified had increased wage income, but potentially shorter job tenures. This supports the duration limitations that have been added to TJTC. Perhaps economic incentives such as returning to a 2-year credit, where the credit increases with tenure would be even more effective.

In short, demand-side interventions for the purpose of assisting disadvantaged individuals are unlikely to be efficacious if they are structured as the TJTC was in the early 1980's.

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Notes

¹Some observers suggest that the frequent changes to the program and the short-term authorizations have been major barriers to employer participation.

²In the early implementation of the program, employers had been allowed retroactive vouchers and certifications. That is, after the hire had been made, employers could request vouchers and certifications. This practice obviously confounded the intentions of the subsidy and was restricted by regulation. Our data do not completely identify retroactive vouchers, although we did delete cases from the analyses if date of voucher was strictly later than date of hire.

³The Burtless study analyzed data from an experiment in which welfare recipients were trained to tell potential employers of their eligibility for the TJTC. Gary Burtless has brought to our attention a study (Wisconsin Department of Health and Social Services and The Institute for Research on Poverty, 1982) that describes the results of another random assignment experiment in which target group members (welfare, ex-offenders, and handicapped) were trained to announce their eligibility for TJTC to employers. Again, the treatment groups fared worse than controls. The confirmatory results of two random assignment experiments is very strong evidence that having welfare recipients announce their eligibility for a tax credit is stigmatizing. Note, however, that the present study concerns the youth target group, which has not been the subject of experimentation.

An interesting aspect of the Wisconsin project is that an experiment was conducted with employers that showed that promotion of TJTC to employers was effective in increasing certifications.

⁴For a detailed discussion, see Heckman and Robb (1985).

⁵Eligibility, however, for the youth target group studied here, is a function of being aged 18-24 and being disadvantaged. Both of these characteristics are likely to affect earnings independently of the existence of TJTC. Earnings are generally thought to increase at a decreasing rate with age, creating the familiar concave age-earnings profile. Disadvantaged individuals, at any age, are likely to have a distinct age-earnings profile, probably lower and flatter than the nondisadvantaged one. Eligibles belong to this latter group, but only if aged 18-24. TJTC eligibility effects will then shift the age 18-24 portion of the disadvantaged age-earnings profile either upwards or downward.

⁶Ed Lorenz has indicated to us that the ESARS economically disadvantaged flag may be somewhat unreliable, although he feels that this data item did not become particularly suspect until after the termination of the CETA/PSE group in 1984.

⁷The treatment is considered to take place at the time the individual contacts the Employment Service. Hence, the TJTC treatment variables are expected to affect outcomes only in the period following treatment and are not subtracted out in this equation. Selectivity correction is as follows:

$$\begin{aligned} k_{ij} &= g(\underline{d}'\underline{X}_{ij})/G(\underline{d}'\underline{X}_{ij}) \text{ if } V_{ij} = 1 \\ &= -g(\underline{d}'\underline{X}_{ij})/(1-G(\underline{d}'\underline{X}_{ij})) \text{ if } V_{ij} = 0 \end{aligned}$$

Here $g(\cdot)$ is the normal probability density function and $G(\cdot)$ is the normal cumulative density function.

$$\begin{aligned} e_{is} &= E[e_{is} * \text{VOUCH}_{ij}, \underline{X}_{ij}] + w_{is} \\ &= b_6 k_{ij} + w_{is} \end{aligned}$$

where, by definition,

$$b_6 = \text{Cov}(v_{ij}, e_{is})/\text{Var}(v_{ij})$$

and is a coefficient to be estimated, and w_{is} is distributed normally with 0 mean.

⁸These states were California, Colorado, Florida, Georgia, Illinois, Indiana, Kansas, Missouri, Oregon, Pennsylvania, South Carolina, and Tennessee. California and Florida were not used in the eligibility study because their data did not include disadvantaged status, which was necessary to identify eligibles from ineligibles.

⁹Recall that the treatment is being aged 18-24, being economically disadvantaged, and encountering the Employment Service. The comparison group are also individuals who encountered the Employment Service and either are aged 18-24 and not economically disadvantaged or are aged 25-29 and disadvantaged. Assigning the impact to the existence of TJTC is a strong assumption. In fact, the coefficient estimates the impact of the interaction between being aged 18-24 and being disadvantaged economically.

¹⁰This is confirmed in the full regression output, which is available from the authors on request.

Table 1
YOUTH SAMPLE MEANS^a
(Standard deviations in parentheses)

| Variable | Year | Eligibility Study | | Voucher Study | | Certification Study | |
|---------------------------------------|------|--------------------|--------------------|--------------------|--------------------|---------------------|--------------------|
| | | Eligibles | Ineligibles | Vouchered | Non-Vouchered | Certified | Non-certified |
| Total Wages | 1980 | \$1,118 (2,830) | \$3,155 (5,275) | 1,215 (2,798) | 1,251 (2,723) | 1,085 (2,177) | 1,335 (2,910) |
| | 1981 | 1,654 (3,508) | 4,433 (6,434) | 1,797 (2,976) | 1,718 (2,998) | 1,765 (2,780) | 1,863 (2,974) |
| | 1982 | 1,351 (2,609) | 3,354 (4,603) | 1,846 (2,187) | 1,749 (2,679) | 2,216 (2,166) | 2,285 (2,580) |
| | 1983 | 1,916 (3,389) | 3,710 (5,169) | 2,936 (3,456) | 2,369 (3,573) | 3,294 (3,448) | 2,921 (3,802) |
| | 1984 | 2,827 (4,348) | 5,028 (6,320) | 3,251 (3,802) | 3,257 (4,367) | 3,413 (3,720) | 3,561 (4,435) |
| Average Quarters Employed | 1980 | 0.801 (1.337) | 1.427 (1.668) | 1.028 (1.251) | 1.015 (1.407) | 0.972 (1.172) | 1.059 (1.429) |
| | 1981 | 1.132 (1.519) | 1.870 (1.772) | 1.590 (1.562) | 1.515 (1.524) | 1.656 (1.578) | 1.702 (1.524) |
| | 1982 | 1.183 (1.412) | 1.854 (1.608) | 2.050 (1.361) | 1.643 (1.483) | 2.460 (1.208) | 2.329 (1.182) |
| | 1983 | 1.340 (1.568) | 1.864 (1.689) | 2.246 (1.567) | 1.741 (1.592) | 2.487 (1.531) | 2.101 (1.611) |
| | 1984 | 1.638 (1.675) | 2.100 (1.741) | 2.133 (1.534) | 2.028 (1.589) | 2.204 (1.523) | 2.150 (1.592) |
| Average Quarterly Wages When Employed | 1980 | \$1,118 (2,021) | \$1,904 (2,164) | \$1,013 (1,460) | \$1,039 (1,489) | \$ 960 (1,180) | \$1,084 (1,572) |
| | 1981 | 1,151 (1,664) | 1,983 (2,135) | 976 (1,112) | 960 (1,158) | 930 (1,001) | 938 (1,048) |
| | 1982 | 927 (1,208) | 1,560 (1,518) | 807 (720) | 912 (962) | 813 (597) | 851 (745) |
| | 1983 | 1,150 (1,372) | 1,706 (1,672) | 1,142 (965) | 1,149 (1,204) | 1,183 (908) | 1,202 (1,119) |
| | 1984 | 1,415 (1,540) | 2,095 (1,887) | 1,363 (1,161) | 1,384 (1,337) | 1,406 (1,117) | 1,461 (1,320) |
| Average Quarters Per Employer-PRE | | N/A | N/A | N/A | N/A | 1.817 (1.606) | 1.218 (1.059) |
| Average Quarters Per Employer-POST | | N/A | N/A | N/A | N/A | 2.155 (1.630) | 2.399 (1.727) |
| Age | | 21.22 (2.54) | 23.99 (3.48) | 21.34 (1.89) | 21.52 (2.03) | 21.24 (1.89) | 21.59 (1.98) |
| Education | | 11.30 (1.81) | 11.99 (1.91) | 11.64 (1.81) | 11.53 (1.95) | 11.83 (1.86) | 11.65 (1.89) |
| Voucher Penetration Rate | | .1419 (.0933) | .1673 (.1024) | .2541 (.1440) | .1278 (.2072) | .2626 (.1443) | .1287 (.1008) |
| Certification Penetration Rate | | .0166 (.0159) | .0176 (.0246) | .0264 (.0201) | .0153 (.0116) | .0276 (.0206) | .0168 (.0140) |

^aDollar figures are in 1982 dollars.

Table 2
 YOUTH ELIGIBILITY IMPACTS^{a,b}
 (Standard errors in parentheses)

| | Eligibility | Voucher Penetration | Eligibility* Voucher Penetration | Certification Penetration | Eligibility* Certification Penetration | Net effects at penetration means | | Sample Size | R ² |
|---------------------------------------|--------------------|---------------------|----------------------------------|---------------------------|--|----------------------------------|--------------|-------------|----------------|
| | | | | | | Eligibility | Displacement | | |
| <u>White Males</u> | | | | | | | | | |
| | a ₆ | a ₇ | a ₈ | a ₉ | a ₁₀ | | | | |
| Change in average quarterly wages | -\$72*** (25) | 44* (25) | -23 (36) | -1535*** (368) | 1150 (701) | -75 | -19 | 106,387 | .036 |
| Change in quarters employed | -.051** (.029) | .151*** (.029) | -.148*** (.040) | 1.50*** (.42) | .699 (.80) | -.013 | .051 | 106,387 | .029 |
| Change in quarterly wages if employed | -\$116*** (40) | -216*** (60) | 284*** (80) | -5,994*** (522) | 2843** (1113) | -158 | -66 | 47,353 | .085 |
| <u>Black/Hispanic Males</u> | | | | | | | | | |
| Change in average quarterly wages | \$38 (32) | -280*** (93) | 312** (134) | 2411*** (441) | -1315** (647) | 61 | 1 | 43,167 | .040 |
| Change in quarters employed | -.027 (.046) | -.497*** (.132) | .315* (.191) | 1.63*** (.63) | 1.12 (.92) | -.006 | .100 | 43,167 | .045 |
| Change in quarterly wages if employed | \$156** (64) | 214 (200) | -320 (281) | 2137*** (769) | -1071 (1231) | 159 | 68 | 15,009 | .070 |
| <u>White Females</u> | | | | | | | | | |
| Change in average quarterly wages | -\$82*** (20) | 11 (16) | -2 (37) | -888*** (288) | 439 (537) | -88 | -14 | 84,386 | .040 |
| Change in quarters employed | -.120*** (.033) | .093*** (.026) | -.074 (.060) | 1.92*** (.47) | -2.07*** (.88) | -.119 | .049 | 84,386 | .039 |
| Change in quarterly wages if employed | -\$71* (36) | -31 (30) | 89 (88) | -3001*** (433) | 2017** (887) | -78 | -47 | 34,762 | .071 |
| <u>Black/Hispanic Females</u> | | | | | | | | | |
| Change in average quarterly wages | -\$103*** (23) | -335*** (78) | 303*** (106) | 1371*** (359) | -1033** (505) | -102 | -24 | 41,930 | .038 |
| Change in quarters employed | -.205*** (.042) | -.573*** (.140) | .555*** (.190) | 1.73*** (.64) | -.51 (.90) | -.186 | .052 | 41,930 | .049 |
| Change in quarterly wages if employed | -\$87* (51) | -443** (176) | 156 (240) | 2509*** (675) | -908 (1058) | -100 | -20 | 12,997 | .058 |

^a Dollar figures are in 1982 dollars

*Significant at .10 level; **Significant at .05 level; ***Significant at .01 level.

^b All impacts are changes for (83, 84) vs. (80, 81).

Table 3
YOUTH VOUCHER IMPACTS^{a,b}
(Standard errors in parentheses)

| | Voucher | Voucher Penetration | Voucher* Voucher Penetration | Certification Penetration | Voucher* Certification Penetration | Net effects at penetration means | | Sample Size | R ² |
|--|-------------------|---------------------|------------------------------|---------------------------|------------------------------------|----------------------------------|--------------|-------------|----------------|
| | | | | | | Voucher | Displacement | | |
| <u>White Males</u> | a ₆ | a ₇ | a ₈ | a ₉ | a ₁₀ | | | | |
| Change in average quarterly wage - OLS | \$258*** (70) | 551*** (156) | -428 (271) | -2201 (1468) | -6220*** (2439) | 69 | 34 | 7422 | .013 |
| Change in average quarterly wage - BCG | 826*** (273) | 644*** (153) | -565** (272) | -2372 (1470) | -6058** (2441) | 625 | 43 | 7422 | .013 |
| Change in average quarters employed - OLS | .381*** (.112) | 1.24*** (.25) | .09 (.44) | -6.83*** (2.37) | -8.75*** (3.94) | .295 | .046 | 7422 | .028 |
| Change in average quarters employed - BCG | 1.47*** (.46) | 1.42*** (.25) | -.18 (.44) | -7.19*** (2.37) | -8.75*** (3.94) | 1.36 | .062 | 7422 | .028 |
| Change in average wages during quarters employed - OLS | \$16 (111) | -222 (344) | -159 (575) | -1213 (2363) | -4511 (4281) | -227 | -49 | 3132 | .034 |
| Change in average wages during quarters employed - BCG | 567** (218) | -303** (123) | -22 (217) | -1239 (1174) | -1475 (1948) | 415 | -59 | 3132 | .034 |
| <u>Black Males</u> | | | | | | | | | |
| Change in average quarterly wage - OLS | \$154** (67) | -998*** (244) | 627 (410) | 4442** (1845) | -4971* (2846) | 44 | -75 | 4619 | .024 |
| Change in average quarterly wage - BCG | -416** (170) | -671*** (235) | 419 (405) | 3623** (1827) | -4152 (2832) | -493 | -41 | 4619 | .026 |
| Change in average quarters employed - OLS | .377*** (.188) | -2.11*** (.43) | 1.07 (.71) | 6.03* (3.21) | -4.90 (4.96) | .142 | -.199 | 4619 | .048 |
| Change in average quarters employed - BCG | -.004 (1.09) | -1.49*** (.41) | .55 (.83) | 3.97 (3.11) | -2.63 (5.08) | -.211 | -.144 | 4619 | .046 |
| Change in average wages during quarters employed - OLS | -208* (127) | -1167** (532) | 681 (848) | 1456 (3455) | 969 (5444) | -269 | -135 | 1926 | .033 |
| Change in average wages during quarters employed - BCG | -642*** (153) | -650*** (212) | 667* (366) | 3473** (1650) | -2529 (2559) | -613 | -40 | 1926 | .035 |

Table 3
(Continued)

| | Voucher | Voucher Penetration | Voucher* Voucher Penetration | Certification Penetration | Voucher* Certification Penetration | Net effects at penetration means | | Sample Size | R ² |
|--|-------------------|---------------------|------------------------------|---------------------------|------------------------------------|----------------------------------|--------------|-------------|----------------|
| | | | | | | Voucher | Displacement | | |
| <u>White Females</u> | | | | | | | | | |
| Change in average quarterly wage - OLS | 143*** (61) | 395*** (136) | 166 (250) | -1601 (1287) | -4314* (2243) | 132 | 23 | 5938 | .026 |
| Change in average quarterly wage - BCG | -2037*** (224) | 565*** (131) | -34 (248) | -293 (1283) | -4095** (2225) | -2040 | 2 | 5938 | .041 |
| Change in average quarters employed - OLS | .132 (.125) | .901*** (.276) | .609 (.507) | -8.97*** (2.61) | -5.07 (4.55) | .134 | -.038 | 5938 | .040 |
| Change in average quarters employed - BCG | -3.40*** (.46) | 1.52*** (.27) | .57 (.51) | -6.87*** (2.62) | -3.77 (4.55) | -3.16 | .076 | 5938 | .040 |
| Change in average wages during quarters employed - OLS | \$85 (107) | 622 (352) | -1300 (581) | -2735 (2396) | 1550 (4516) | -122 | 33 | 2447 | .035 |
| Change in average wages during quarters employed - BCG | -1850*** (199) | -49 (116) | -90 (220) | 1642 (1139) | -420 (1974) | -1852 | 22 | 2447 | .037 |
| <u>Black/Hispanic Females</u> | | | | | | | | | |
| Change in average quarterly wage - OLS | \$95 (59) | -254 (190) | 409 (371) | 1770 (1552) | -5501** (2646) | 37 | -9 | 4453 | .029 |
| Change in average quarterly wage - BCG | -644*** (166) | -39 (185) | 230 (368) | 1413 (1540) | -4336 (2632) | -671 | 13 | 4453 | .033 |
| Change in average quarters employed - OLS | .182 (.123) | -1.69*** (.40) | 1.29* (.78) | 2.94 (3.25) | -5.06 (5.54) | .022 | -.169 | 4453 | .041 |
| Change in average quarters employed - BCG | -1.15*** (.35) | -1.20*** (.39) | .87 (.77) | 1.86 (3.23) | -2.42 (5.52) | -1.25 | -.123 | 4453 | .043 |
| Change in average wages during quarters employed - OLS | \$162 (109) | -139 (387) | -198 (720) | 4131 (2885) | -5818 (5118) | 30 | 35 | 1724 | .023 |
| Change in average wages during quarters employed - BCG | -646*** (149) | -181 (165) | 184 (329) | 3947*** (1374) | -4199* (2349) | -652 | 28 | 1724 | .026 |

^a Dollar figure are in 1982 \$. OLS means impacts were estimated using ordinary least squares from equation (5) in differenced form. BCG means the nonlinear selectivity correction suggested by Barnow, Caid, and Goldberger (1981) was used as given by equation (7).

^b All impacts are changes for (83, 84) versus (80, 81).

* Significant at .10 level; ** Significant at .05 level; *** Significant at .01 level.

Table 4
 YOUTH CERTIFICATION IMPACTS^{a,b}
 (Standard errors in parentheses)

| | Certification | Voucher Penetration | Certification * Voucher Penetration | Certification * Certification Penetration | Certification Penetration | Net effects at penetration means | | Sample Size | R ² |
|---|-----------------|---------------------|--|--|---------------------------|----------------------------------|--------------|-------------|----------------|
| | | | | | | Certification | Displacement | | |
| <u>White Males</u> | | | | | | | | | |
| Change in average quarterly wage | 314** (28) | -659*** (238) | 1190** (500) | -1294 (1688) | -12404 (3908) | 79 | -102 | 4918 | .015 |
| Change in average quarters employed | -.10 (.20) | -2.39*** (37) | 4.41*** (.78) | -2.19 (2.63) | -18.98*** (6.09) | -.15 | -.33 | 4918 | .026 |
| Change in average wages during quarters employed | 555*** (184) | 158 (337) | 550 (845) | -1680 (2427) | -15146** (6557) | 198 | -48 | 2415 | .039 |
| Change in number of quarters worked per employer | -.34 (.28) | -1.34* (.71) | 1.84 (1.26) | -3.06 (4.31) | -18.74** (9.31) | -.80 | -.21 | 1731 | .061 |
| <u>Nonwhite Males</u> | | | | | | | | | |
| Change in average quarterly wages | 129 (134) | -843*** (318) | -34 (700) | 3098 (2003) | -1540 (4283) | -59 | -78 | 2889 | .015 |
| Change in average quarters employed | .17 (.22) | -2.88*** (.52) | 1.64 (1.15) | 9.26*** (3.28) | -11.00 (7.02) | -.20 | -.29 | 2889 | .044 |
| Change in average wages during quarters employed | 253 (221) | -58 (498) | -1757 (1231) | -555 (3118) | 8430 (7307) | -16 | -18 | 1459 | .028 |
| Change in number of quarters worked per employer | -.15 (.33) | -.68 (1.04) | -.31 (1.76) | -3.10 (5.06) | -2.84 (10.11) | -.56 | -.15 | 1139 | .042 |
| <u>White Females</u> | | | | | | | | | |
| Change in average quarterly wages | 336*** (110) | -546*** (211) | 403 (425) | 1162 (1537) | -6662** (3313) | 140 | -43 | 3806 | .029 |
| Change in average quarters employed | -.12 (.21) | -3.29*** (.42) | 3.96*** (.82) | -.04 (2.95) | -16.57*** (6.36) | -.41 | -.38 | 3806 | .046 |
| Change in average wages during quarters employed | 710*** (180) | 544 (358) | -2168** (854) | -618 (2538) | 4932** (6587) | 383 | 52 | 1866 | .042 |
| Change in number of quarters worked per employer | -.03 (.36) | -1.22 (.88) | -1.47 (1.48) | 6.40 (5.89) | -9.16 (11.30) | -.85 | -.03 | 1318 | .056 |
| <u>Nonwhite Females</u> | | | | | | | | | |
| Change in average quarterly wages | 337*** (124) | -106 (258) | 245 (668) | 571 (1749) | -6962 (4231) | 192 | -6 | 2654 | .028 |
| Change in average quarters employed | .13 (.25) | -1.73*** (.51) | -.43 (1.32) | 3.15 (3.48) | .81 (8.41) | -.36 | -.19 | 2654 | .032 |
| Change in averages wages during quarters employed | 662*** (195) | 73 (413) | 365 (1046) | 495 (2701) | -10494 (6570) | 456 | -3 | 1242 | .037 |
| Change in number of quarters worked per employer | .08 (.48) | .25 (1.44) | -.79 (2.39) | -21.60*** (7.63) | 10.22 (14.44) | -.40 | -.29 | 817 | .070 |

^a Dollar figures are in 1982\$.
^b All impacts are changes for (83, 84) versus (80, 81).

* Significant at .10 level; ** Significant at .05 level; *** Significant at .01 level.

Table 5
SUMMARY OF RESULTS, BY STUDY

| Study/Description of Treatment and Comparison Group | Major Results |
|---|--|
| <u>Eligibility Study</u> | |
| Treatment: Aged 18-24, Economically disadvantaged encountered E.S. | - Eligibility has positive net impacts on nonwhite males in treatment group. Negative net impacts for all other treatment race/sex groups. |
| Comparison: Aged 18-24 and not disadvantaged or aged 25-29 and economically disadvantaged; encountered ES | - Increase in vouchering and certification result in positive outcomes for non white males in comparison group. Zero/mildly positive impacts for all other comparison race/sex groups. Certification penetration rate positively related to employment of treatment group. |
| <u>Voucher Study</u> | |
| Treatment: Eligible, vouchered, and encountered ES | - OLS estimated net outcomes are generally positive for all raace/sex treatment groups. OLS estimated relationship between vouchering and certification and outcomes are small or negative for all race/sex comparison groups. |
| Comparison: Eligible, not vouchered, encountered ES | - Selectivity-corrected estimates of outcomes are positive for white males and negative for all other race/sex groups in treatment group. Certification and vouchering penetration rates are negatively related to outcomes for treatment group suggesting, “decreasing returns.” |
| <u>Certification Study</u> | |
| Treatment: Eligible, vouchered, and certified, encountered ES | - Positive effect on wages while employed for treatment groups. Higher turnover and fewer quarters of employment for treatment groups. |
| Comparison: Eligible, employed, encountered ES | - Mildly negative outcomes of increased vouchering and certification for comparison group members. |