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EVALUATING THE TARGETED JOBS TAX CREDIT

SR 29

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February 1981

The project was partially funded under Contract No. B9M94788 from the Office of the Assistant Secretary for Policy Evaluation and Research and by funds granted to the Institute for Research on Poverty at the University of Wisconsin-Madison by the Department of Health, Education, and Human Services pursuant to the provisions of the Economic Opportunity Act of 1964. Points of view or opinions stated in the document do not necessarily represent the official position or policy of the Department of Labor.

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Evaluating the Targeted Jobs Tax Credit

An evaluation of TJTC must deal with two types of issues involving two models. One is an economic model of the way in which the program affects the labor market behavior of program participants and others. The second is a statistical model that must effectively measure those aspects of labor market behavior specified by the economic model as the costs and benefits of the program. This paper deals mainly with the second model, but some attention to the first is essential: The economic issues may in fact be more important than the statistical ones, though they are also less capable of resolution.

In the first part of this paper, we briefly address the economic issues and conclude that two types of statistical models are required. One deals with a model of individuals within one or more sites where the program operates. The second involves sites as units of observation, with variation across sites in the amount of program penetration. In the second and third parts of the paper we present an outline of these two statistical models.

1. ECONOMIC ISSUES IN EVALUATING TJTC

Two fundamental questions about TJTC are whether the program will improve the employment and earnings of the targeted groups, and if so, whether such improvement is at the expense of other groups in the labor market. In principle, the gain of one group may be sufficiently large to compensate members of the other group for their losses. In practice, this compensation is not carried out in any direct way, although we can use the data on the amount of gains and losses to evaluate the program.

The economic problem confronting TJTC, as with any tax subsidy or government expenditure, is that the money (or resources) spent on the program could be spent elsewhere. The employment and other benefits produced by the tax expenditure are in general offset by the loss in employment and benefits in those sectors that have given up the money--whether these are taxpayers or other government programs. Why should TJTC produce <u>net</u> gains? We suggest three potential sources, each based on some type of market failure: Keynesian unemployment, existing distortions of the low-wage labor market, and investment in human capital.

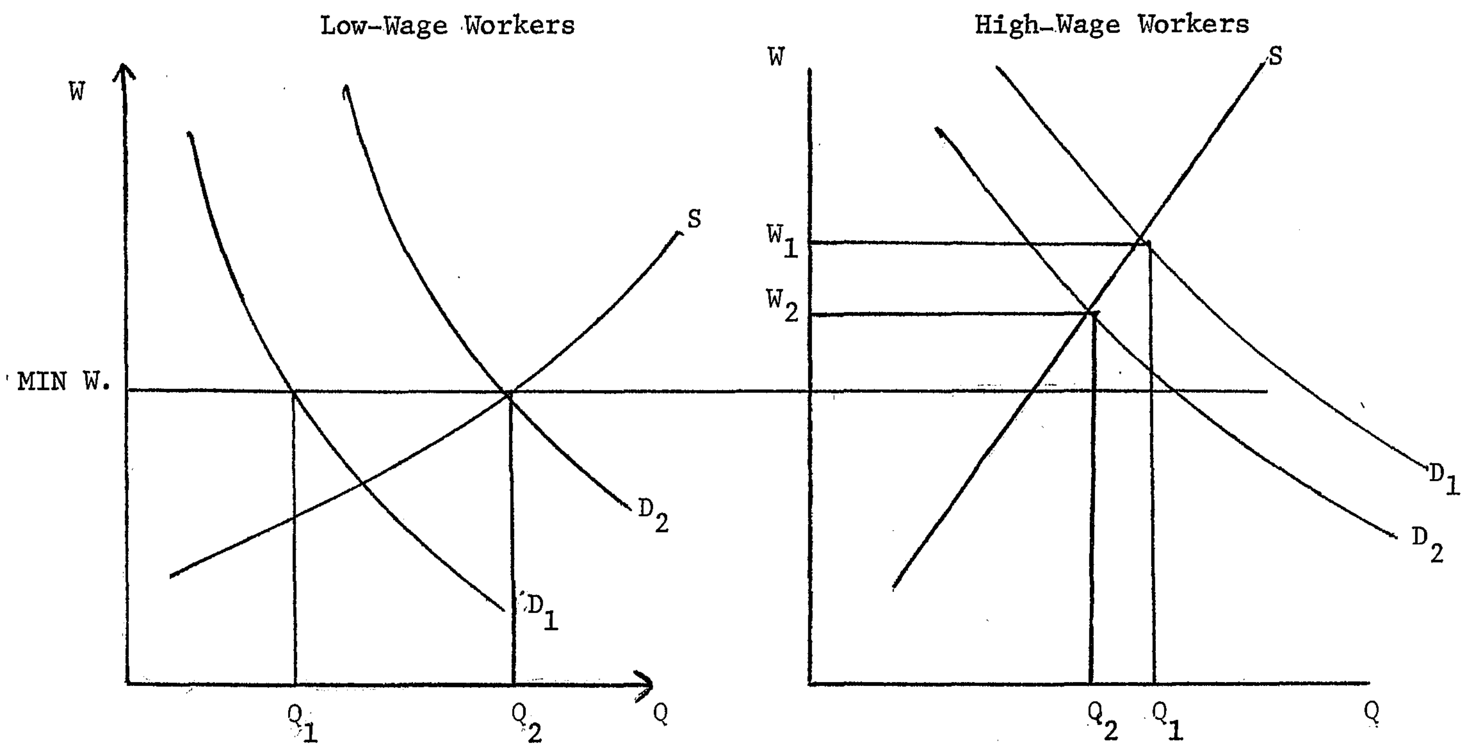
1. One, which we do not emphasize, is Keynesian-type unemployment--a cyclical disequilibrium brought about by a decline in effective demand. In this case, any sort of government expenditure can, in principle, finance itself with the unemployed resources. TJTC may, in fact, be an effective program to deal with Keynesian-type unemployment, but the more challenging question is to evaluate TJTC under "normal" labor market conditions.

2. Secondly, TJTC may correct existing price distortions that prevent the optimal allocation of resources. Minimum wage laws and welfare support systems, which prevent or discourage employment by low-wage workers, are commonly cited examples.

The Most Favorable Case for Gains

Assume, as shown in Figure 1, two sectors in the labor market, one for low-wage workers and one for high-wage workers. The low-wage

Figure 1.


Low-Wage Workers
W
S
MIN W.
D2
D1
Q1
Q2
Q
High-Wage Workers
W
S
W1
W2
D1
D2
Q2
Q1
Q

sector has unemployment due to a minimum wage that exceeds the market clearing wage. The high-wage sector is immune from the effects of the minimum because the market clearing wage is sufficiently high. In the initial period, before TJTC operates, D_1, W_1, and Q_1 represent the initial demand curves, wages and quantities of employment respectively.

The cross-elasticity of demand between the two sectors is not zero, and the TJTC program increases the demand for low-wage workers at the expense of high-wage workers. The wage subsidy for the low-wage workers lowers their factor price relative to the unsubsidized high-wage workers. Furthermore, the subsidy encourages the substitution of low-wage workers for capital, and it is likely that high-wage workers are complementary to capital. If low-wage workers are employed directly--say, in public jobs, this would be another source for an increase in demand for low-wage workers; and the money foregone reduces the demand for high-wage workers.

A net gain from these shifted demand functions, shown in each sector by D_2, stems from an employment gain among low-wage workers that is greater than the employment loss by high-wage workers. The plausibility of this favorable outcome lies in the well-known inelasticity of the supply curve of male prime-age workers, who may here represent high-wage workers. Conversely, low-wage workers, whose attachment to the labor force is more marginal, have a more elastic supply curve of labor.

Three additional points may be made. First, the gain in employment for low-wage workers results from the employment of previously unemployed workers, whereas the decline in employment among high-wage workers would likely take the form of reduced hours worked over the year rather than "full-time" unemployment. This implies less "psychic disutility" from a unit of reduction in employment among high-wage workers, compared to a unit among low-wage workers. This reinforces what we assume is already a favorable change in the distribution of benefits, because the aid goes to poor persons.

Second, if the supply curve of labor for high-wage workers were negatively sloped, there would be gains in employment in both sectors. A third point is that the gain in overall employment discussed above follows from a greater supply elasticity of labor of low-wage workers and does not require a minimum wage barrier. However, here we lose the presumptive case for a market distortion and, therefore, for a suboptimal allocation of resources. Without the minimum wage barrier or some similar distortion, such as a tax on labor or a subsidy to leisure like transfer payments, the extra market work from the wage subsidy may be more than offset by the loss in the value of nonmarket uses of time.

The Least Favorable Case for Gains

Johnson (1980) has described a realistic counter-case in which the subsidy does not cover all workers in the low-wage sector.[1] Given the presumed homogeneity of low-wage workers, we may assume that the cross-elasticity of demand among the covered and uncovered low-wage workers is close to infinity. Thus, any newly employed subsidized low-wage worker would simply replace an unsubsidized low-wage worker--either previously employed, or unemployed and about to become employed. No net gain is obtained, although the subsidy raises the wages of employed covered workers above their previous levels. Even the distributional outcome is neutral, because the losers are just as poor as the gainers, unlike the previous situation.

To avoid this unfavorable outcome, efforts could be made: (a) to increase coverage and (b) to vary the subsidy inversely with the productivity of eligible workers. In situation (b), the assumption of only two types of workers is replaced by a continuous distribution of productivities.

There are many other complications with these simple scenarios. One that deserves to be mentioned is that eligibility for TJTC depends on the household income of the worker, but the correspondence between low wages and low household income is far from perfect. This means that some ineligible workers are likely to be so-called secondary workers who are low-wage workers in high-income households. These workers may have elastic supply curves and may suffer large losses in employment. This weakens the presumptive case for a larger wage elasticity among eligible workers.

3. The two sources of net gains from TJTC discussed above do not require any increase in the stock of human capital. The gains from TJTC are obtained solely from a reallocation of the existing stock.

Perhaps TJTC has an investment component that increases the human capital of the participating workers. That is, TJTC increases their productivity (wage rate) over and above any increase in employment. One type of investment is the provision of labor market information and counseling by the placement agency. A more important investment, probably, is on-the-job training of the worker. If the current amount of investment in on-the-job training is suboptimal, a net gain could arise even if total employment is not increased (or total unemployment not decreased). Suboptimal investment may be due to the inability of low-wage workers to finance general training by accepting wages low enough to make it profitable for employers to hire them and provide the training on the job. Or, the low wages available to these workers could so contribute to high turnover that firm-specific on-the-job training is discouraged.

The traditional design for evaluating programs that invest in the human capital of the participants is to compare their subsequent employment and earnings experiences to a control group's experience during a sufficiently long post-training period. Two simplifying assumptions are often made in the analysis. If the investment (or training) program is small relative to the size of the labor market, spillover or externality effects are assumed to be negligible. In other words, the performance of the control group is not affected by the performance of the participant group. Second, full or at least "normal" employment conditions are assumed to prevail. Thus, any gain in earnings of the participant group relative to the control group is expected to be mainly attributable to gains in wage rates or earnings capacities. A long-run permanent reduction in unemployment experience may also result from the increased human capital of the worker, and greater earnings from this source are not ruled out.

The three sources of gains in employment and earnings from TJTC described above suggest two strategies for an evaluation design. Where TJTC is a relatively small investment program in a given labor market, a conventional analysis of individual participants and a control group within that labor market should be adequate. The second part of this paper discusses the statistical issues that arise when an evaluation of a program is to be based on a comparison of participants and nonparticipants within given labor markets. The crucial task, which is related to the problem of understanding the process by which workers are selected for the program, is finding a suitable control group.

A different design is needed in the situation described by Johnson in which there is no investment and the subsidy to promote hiring covers only a fraction of homogeneous workers. Here, the subsidy is not intended to have any effect, but the program may have spillover effects on nonparticipant workers. A study within a labor market that reveals gains by participant (covered) workers compared with nonparticipant (uncovered) workers who are similar in productivity will not be interpretable, for we have no way of knowing if the positive effect of the program consists merely of a shift in job acquisition from the uncovered to the covered group. The program can only be judged successful if the overall employment in the labor market is increased. To study markets as units of analysis requires a design in which markets vary in the amount of TJTC penetration. Again, statistical problems in isolating a TJTC effect from among many sources of market variation in employment (or other labor market) performance are considerable. In the third part of this paper we outline an approach that is, we feel, capable of measuring TJTC's impact on the low-skill labor market.

It is important in applying either within- or among-market designs to focus on those parts of the program that are replicable and not merely reflective of one-time, unique efforts. Here again, knowledge about the way the program is administered is needed. Finally, any evaluation depends on the adequacy of the data. This issue is being addressed by Cohen and Bressan (1979) and appears resolvable.

2. WITHIN-LABOR-MARKET COMPARISONS: STATISTICAL ISSUES

Recommended Approach

The first design deals with individuals within a labor market. The second strategy involves using labor markets as units of analysis; some being considered experimental observations and others control observations. The objective of the first design is to find persons who are like the participants in all relevant respects except that they did not participate in the program. "Relevant aspects" refer to differences in characteristics that affect the outcome of interest--employment and earnings.

Without random assignments the method that in principle achieves this objective is a statistical model with two key features. One is that the relevant differences between participants and nonparticipants are measured and "held constant" in a statistical sense. The second is that an assumption is made that the relevant unmeasured characteristics are, on average, the same for participants and nonparticipants. The most practical method of carrying out this statistical model is to find situations in which the process of selection of persons for participation is fully known and quantifiable. The selection criteria become, in effect, the set of control variables that account for relevant differences between the two groups.

The idea justifying this model is simple. If, in the selection process of the program, the systematic determination of participation is measured, then the statistical model measuring post-program experience can capture these systematic differences and leave only unsystematic (i.e., random) differences unmeasured. A more formal exposition of this model is shown below in the context of discussing alternative approaches and is described in greater detail in Cain (1975).

The feasibility of this model depends on how well the selection process is measured, and this in turn depends on the control the program administrators have over selection. Consider the following two illustrations, A and B.

Illustration A. Assume one agency in a city selects applicants to the TJTC program by measuring a vector of characteristics and determining appropriate "cutoff" points for each characteristic. Persons with characteristic values below these points become participants. Those with values above these points become the control group of nonparticipants. In this situation we assume that the self-selection process by which applicants arrived at the administrative agency is similar to the self-selection that would take place in other sites and in the future experience of the program. The participants and non-participants are then followed over time to provide data on employment and earnings in the post-program period. The statistical model uses these data and the original selection variables to permit estimating the net effect of the program. A side (but important practical) issue is whether the controls represent a "no-treatment" situation or whether they in fact receive a different treatment from the agency administering the program. This is discussed below.

Illustration B. Another type of selection process is one in which many agencies select the program participants, each with its own criteria for screening in and screening out the applicants. In this situation there are likely to be both logistical problems in identifying the nonparticipants and problems in measuring the selection criteria. The statistical model of evaluation described above, which depends on measuring the selection process, would probably be inapplicable.

We recommend, therefore, that one component of the evaluation design be a choice of sites in which all or nearly all of the selection process is handled by one agency--or at least that the evaluation be a single well-defined process, perhaps with several agencies collaborating in administering it. A common arrangement may involve the Employment Service, which will have the responsibility for certain monitoring functions in the program, and the local CETA offices.

It is not claimed that the "single-agency" situation will be common in TJTC or even that it is necessarily a good way to administer the program. What is claimed is that this type of situation would permit a valid evaluation design. The participant-nonparticipant difference that is measured in the statistical model described above should not provide an unbiased estimate of the program effect.

Selection Bias: A Formal Discussion

The preference expressed above for a design in which the selection process is modeled is based on our skepticism that alternative designs will work. An understanding of the issues involved may be helped by a more formal treatment of the problem. We pose the problem as one of measuring the "true effect," a, of participation in the program, T, on an outcome, y, such as post-program earnings.

An ideal model would include a measure of the "true ability," A, of each worker to achieve y in the absence of the program. Allowing for a purely random term, e, we have:

$$y = A + aT + e. \tag{1}$$

(We shall use linear models throughout, with one exception, noted below.) Here, the independence of e and T allows a to be an unbiased measure of T's effect in an ordinary least squares regression of (1). A and T may be correlated.

However, A is unobservable, so equation (1) is of no direct help. A model with observable variables, $\underline{x}$, may be written:

$$y = \underline{bx} + cT + e'. \tag{2}$$

(We will denote vectors by the underlying bar.) With equation (1) in mind, we need to know the relation between A and T, conditional on the vector of control variables, $\underline{x}$. This is represented by an unobserved auxiliary relation:

$$A = gT + \underline{hx} + e''. \tag{3}$$

Substituting (3) in (1),

$$y = gT + \underline{hx} + aT + e' \tag{4}$$

or

$$y = \underline{hx} + (g + a)T + e'.$$

Equation (4) is (2) rewritten, and shows that $c = g + a$ is unbiased only if $g = 0$. But g, from equation (3), is equal to 0 only in the special case that, conditional on $\underline{x}$, the assignment to T is random with respect to A.

Modeling the selection process is essentially a representation of conditions that guarantee that T and A are unrelated, conditional on the observable variables, $\underline{x}$, that are included in the estimation model. Our suggestion for such a model is actually only one among alternatives, but we believe it is the most dependable. It is Case 1 among three cases examined below. The second and third cases are possible models, and we describe these to indicate their uses and to enhance our understanding of the issues. Case 2 assumes that one or more variables affect program participation but have no direct effects on the outcome. This assumption does not seem tenable for the within-labor-market evaluation, although we apply it to the across-labor-market design. The third case, which uses the nonlinearities of the selection model to identify program effects, does not seem to be robust enough to provide reliable results (see Barnow, Cain and Goldberger, 1980). Nevertheless, this third case is in vogue in the current econometrics literature and the technique is not costly to use. At minimum, we may see how the results from this procedure compare with alternative results.

Case 1: Conditional on a set of control variables, x, selection into the program is uncorrelated with the error on the equation predicting the outcome. Assume that selection is determined by $\underline{x}$, along with a random component, v.

$$T = d(\underline{x}) + v \tag{5}$$

where the function, $d(\underline{x})$, relating T to $\underline{x}$ is nonlinear: T = 1 (representing a participant) if $x \leq x^0$, and T = 0 (representing a control person) if $\underline{x} \geq x^0$, where $\underline{x}^0$ is some "score" on a composite set of selection criteria. The $\underline{x}$ variables may be determinants of y in their own right, as illustrated by equation (2) on page 12. Given that v is random, we are guaranteed that g = 0 in the auxiliary relation (3). Thus, c is unbiased. A simple example is when x is a continuous measure of a person's distance from the poverty line, and x^0 is the poverty line.[2]

Equation (5) might be applied in one of two ways. First, let $\underline{x}$ determine the probability that a person may be a participant, where lower values of $\underline{x}$ increase the probability. Given $\underline{x}$, a roll of a die determines who is a participant and who is in the control group. A larger probability of selection is assigned to a person the lower his x value is. This is simply a stratified random sample design, and we surmise that any program that requires random selection will be difficult to "sell." A second variety is one in which x is intended to be an exact determinant, so only errors of measurement are the source of v. This design is saleable, but, of course, difficult to administer.

Case 2: One or more variables are known to determine selection in the program and to have no effect on the outcome. Let z, not y, be the variable that determines selection. We specify a linear function for expositional simplicity:

$$T = \underline{d_1 x} + \underline{d_2 z} + v' \tag{6}$$

and, repeating:

$$y = \underline{bx} + cT + e'. \tag{2}$$

A restrictive assumption about this pair of equations is that their error terms are uncorrelated. This says that there is no characteristic of the worker affecting both program participation and the worker's earnings except for those measured by x and z. If this were not true, then e', which is correlated with v', must be correlated with T, because v' is obviously correlated with T.

A less restrictive assumption is that e' and v' are uncorrelated with x and z, but permit e' and V' to be correlated with each other. Here, the assumption that z is related to T but not to y may be exploited to obtain an unbiased estimate of T's effect on y.

Substituting (6) into (2):

$$y = \underline{bx} + c[d_1\underline{x} + d_2 z + v'] + e'$$
$$= \underline{Bx} + d_3 z + v''.$$

Here, $\underline{B}$ includes $\underline{b}$ and $\underline{cd_1}$; $d_3 = cd_2$; $v' = cv' + e'$; B and d_3 are unbiased because v" is uncorrelated with $\underline{x}$ and z. It is easy to obtain c from d_3/d_1 where d_3 is obtained from a regression of (6). [c] is unbiased because, by equation (6), the true ability of the worker does not determine selection.

We do not recommend this model as a first priority, because we are skeptical that a variable, Z, is available. Consider the possibility that the selection of program <u>sites</u> is random, or equivalently, based on purely "political" factors. If this were true, then Z could represent a site variable and be considered unrelated to y. We doubt that site selection is random, however, and when we discuss the model that uses sites as units of observation, we will attempt to control for those aspects of the site that affect y.

<u>Case 3. All observable variables, x, affect both the outcome and the selection process, which is not fully known</u>. Recent work on selection bias (Heckman, 1979) dispenses with the assumption that an identification variable, Z, that affects T and not y, is available. This work instead invokes certain assumptions about the distribution of error terms and about the unobserved "selection" and "true productivity" variables.

The worker's true wage earnings capacity, or "productivity" for short, is defined as the unobserved variable, A. We may define the worker's wage, as in equation (1) above, to be equal to A (appropriately scaled), plus a program effect aT, plus a random error term e, which is assumed normal, independent of A and T and having a zero mean.

A second fundamental relation involves an unobserved selection variable, S, defined as continuous and scaled so that T = 1 if S > 0 and T = 0 if $S \leq 0$. In reality, there are multiple selection criteria, and this model assumes that these may be formed into a composite "score" represented by S. These unobserved variables are assumed to be linearly related to the observed $\underline{X}$ vector. We have

$$A = g_1'\underline{x} + v_1, \text{ 2nd} \tag{7}$$

$$S = g_2'\underline{x} + v_2. \tag{8}$$

No causality is implied by (7) and (8). The disturbance terms, v_1 and v_2, have a mean of zero and are assumed to be bivariate normal and independent of $\underline{x}$. In summary:

$$E(v_1) = E(v_2) = 0,$$
$$E(v_1,v_1) = \sigma_{11},$$
$$E(v_2,v_2) = \sigma_{22}, \text{ 2nd}$$
$$E(v_1,v_2) = \sigma_{12}.$$

The nonzero correlation between v_1 and v_2 is indicated by $\sigma_{12} \neq 0$, and this is the source of different productivities (a different expected A value) for program and nonprogram workers, given $\underline{x}$, and this difference in productivities is the source of the bias in c in equation (2). In comparison with equation (1), $c \neq a$, and the bias in c may be attributed to the omitted variable, A, in equation (2).

The comparison between equations (1) and (2) may be facilitated by substituting equation (7) into (1):

$$w = \underline{g}_1'\underline{x} + aT + u, \tag{9}$$

where

$$u = v_1 + e.$$

The disturbance u is normally distributed with mean zero because v and e are normal, have zero means, and are independent. However, $e(u|T;\underline{x}) = 0$, because $E(v_1|T;\underline{x}) \neq 0$. Estimation of equation (9) depends on controlling for this conditional expectation.

Technical details are available elsewhere (Barnow, Cain and Goldberger, 1980), but the basic strategy may be described as follows. By the definition of u and because e has a zero mean and is independent of T, $\underline{x}$ and v_1, we have:

$$E(u|T,\underline{x}) = E(v_1 + e|T,\underline{x}) = E(v_1|T,\underline{x}).$$

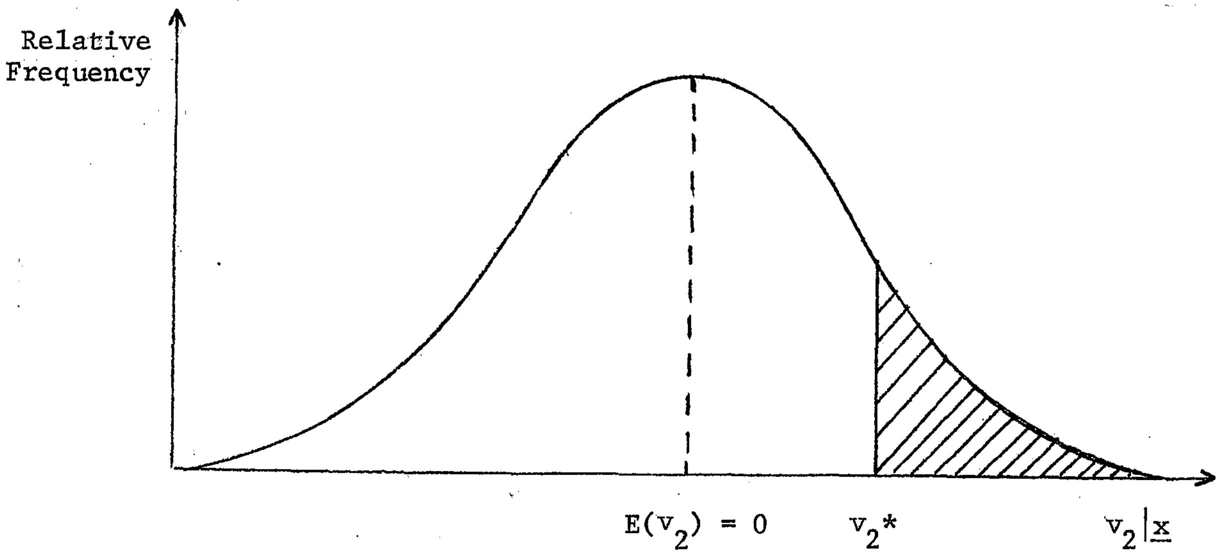
We then replace $E(v_1|T,\underline{x})$ by:

$$E\left(\frac{\sigma_{12}}{\sigma_{22}} v_2 \middle| T,\underline{x}\right) = \frac{\sigma_{12}}{\sigma_{22}} E(v_2|T,\underline{x}),$$

drawing on the bivariate normality of v_1 and v_2 and $\sigma_{12} \neq 0$. Turning our attention to $E(v_2|T,\underline{x})$, we note that the values of v_2 are truncated for program participants. In particular, $T = 1$ when $S > 0$ or when $v_2 > g_2'\underline{x}$. This is shown in Figure 2.

By a well-known formula the expected value of v_2 for program participants is, in standardized form,

Figure 2. Hypothetical Distribution of V_2--The Propensity of Workers to be Program Participants, Holding Constant $\underline{X}$.



$$f(\underline{g}_2'\underline{x})/F(\underline{g}_2'\underline{x}),$$

where the numerator is the probability density of the standard normal variable at the value $\underline{g}_2'\underline{x}$, and the denominator is the cumulative probability of the standard normal variable at $\underline{g}_2'\underline{x}$. The formula for $E(V_2)$ for those who are not program participants is

$$-(F_{-2}'\underline{x})/(1 - F(\underline{g}_2'\underline{x})).$$

Both numerator and denominator for program and nonprogram persons may be estimated by a probit function in which program status is the dependent variable and the $\underline{x}$ vectors are independent variables. The values for these estimates for each worker replace the part of u in equation (9) that is correlated with T. The replacement values are a function, $h(\underline{x},T:\underline{g}_2')$. In a phrase, the new term, h( ), controls for the remaining systematic productivity difference in program and non-program workers, given $\underline{x}$. The new regression equation is

$$w = \underline{g}_1'\underline{x} + aT + Kh(\cdot) + e. \tag{10}$$

It is crucial that the probit estimate of (5) both provide an estimate of $E(u|T,\underline{x})$, and be nonlinear in $\underline{x}$, because equations (9) and (10) include the linear form of $\underline{x}$. Only the residual variation in nonlinear values of $\underline{x}$ (that is, the variation in the nonlinear form of $\underline{x}$ after controlling for the linear form of $\underline{x}$) provides a basis for estimating the effect of the productivity differences between program and nonprogram participants, given $\underline{x}$.

This last point introduces another perspective on the procedure for correcting the selection bias which we have been describing. The probit function predicts program status on the basis of a nonlinear function of $\underline{x}$. Because the nonlinear functional form of $\underline{x}$ does not appear in equation (9), this function of $\underline{x}$ serves as the identifying Z variable from the set of equations (6) and (2). The use of predicted program participation, $\hat{T}$, instead of actual program participation, T, is another way of estimating a.

$$w = \underline{g}_1'\underline{x} + a\hat{T} + e^*, \tag{11}$$

where $\hat{T}$ is $F(\underline{g}_1;\underline{x})$, obtained from the probit function.

3. THE ACROSS-LABOR-MARKET EVALUATION DESIGN: STATISTICAL ISSUES

Introduction

An important limitation of studies that measure the impact of TJTC by comparing individuals and firms within the same labor market (even when selection bias is successfully controlled) has been mentioned above. TJTC benefits to recipients might be entirely offset by job losses experienced by eligible individuals who do not use the employment service to look for work. Moreover, reductions in employment by noneligible workers may also offset the gains. For example, assume that TJTC induces Safeway to expand employment and keep their stores open longer hours. Since total sales of food in the community are not likely to rise because of the longer hours, Safeway's increased sales are coming at the expense of the sales and, most likely, the employment of some other stores such as PDQ or Seven/Eleven (Perloff, 1979).

On the other hand, we have also described ways in which TJTC can increase total employment in the market as a whole in Section I of this paper. Assume, for instance, that the requirement that a firm hire a welfare recipient or low-income youth in order to get a tax credit induces the firm to hire and train workers of much lower productivity than they would otherwise have done. Assume further that a worker who does not get the job at firm A because of the preference given the TJTC-subsidized worker has substantially greater skills and is likely to be a part of a labor market where wage rates adjust up and down to equilibrate demand and supply. Under these assumptions the workers displaced by the subsidy received by firm A are hired by other unsubsidized firms at wages slightly below those they would have received in the absence of the subsidy.

Specification of the Data and the Statistical Model

To study the market-wide impact of TJTC, we suggest that cross-section and time-series data for many markets be used to estimate an equation predicting percentage chance in employment as a function of TJTC "penetration" and a number "control" variables. Markets are defined by counties and SMSAs. The equation, or model, could apply to specific industries for each market, or for total employment in the market. Alternatively, the outcome variable could be total earnings (for the industry or for the market), rather than employment.

The data on employment and total earnings by industry and county is available from the ES202 reports filed by all firms paying unemployment taxes. Data on earnings in uncovered industries can be obtained from the Office of Business Economics, Department of Commerce. At least five or ten years of data prior to the initiation of the TJTC would be used besides the period of TJTC operation.

The model suitable for estimating the TJTC effect on total employment or earnings in a market is:

$$E_{it}/E_{it-1} = \alpha(TJTC_{it}) + \underline{\Delta X}_{it}\underline{B} + D_t\gamma + D_i\theta + u_{it} \tag{12}$$

The model suitable for estimating TJTC effects on particular industries in a market is

$$E_{ijt}/E_{ijt-1} = \alpha_j(TJTC_{it}) + \underline{\Delta X}_{it}\underline{B}_j + D_t\gamma_j + D_i\theta_j + \Delta X_{ijt} + u_{ijt}, \tag{13}$$

where:

E_{it}/E_{it-1} = the proportionate rate of growth of total employment or earnings in the "i"th labor market,

E_{ijt}/E_{ijt-1} = the proportionate rate of growth of employment or earnings of the "j"th industry in the "i"th labor market,

D_i = dummies for location,

D_t = dummies for time period,

$\underline{\Delta X}_{it}$ = a vector of exogenous changes of local characteristics,

ΔX_{ijt} = a variable measuring exogenous changes in local demand or supply conditions that are specific to a particular industry, and

$TJTC_{it}$ = variable measuring the increase in penetration of the TJTC in the "i"th labor market between t and t-1.

The ΔX_{it} vector might include the following: changes in business tax rates, changes in state or federal spending in the locality, changes in the degree of unionization, and a predicted employment growth calculated by averaging national industry-specific growth rates for year t, using proportions of local employment in the industry as weights.

One would expect that TJTC's impacts would be focused on the low-wage industries that in the past have been the primary employers of youthful workers and welfare recipients. Comparisons of TJTC coefficients across industries will provide a test of this hypothesis. Early work would test the hypothesis that the earnings of workers in high-wage industries that sell in national markets (like mining and steel, auto, machinery, oil, chemical manufacturing) are independent of TJTC penetration. If this hypothesis is accepted, later work predicting employment in industries that sell in the local labor market or that pay low wages (textiles, retail service, construction, etc.) would enter as an ΔX_{ijt} variable changes in the total earnings of workers in these exogenous industries as an additional regressor. The coefficient (γ) on the dummy for time period measures an average effect of the national business cycle on employment. The coefficient on the location dummy measures the tendency of each location to grow faster or slower than the national average during the ten years or more which are the time period of the analysis.

Two kinds of data on TJTC penetration of specific localities will be available:

1. The number of workers vouchered by agencies (Job Service, CETA, etc.) in the area.

2. The number of vouchered workers that have jobs that have been certified by administering agencies.

Certification of the employee-employer match is necessary for the employer to receive a tax benefit. Consequently, the number of certifications will be highly correlated with both the tax benefits that employers in the locality receive and the localities' average number of subsidized employees.[3] The suggested operationalization of the TJTC penetration variable is

$$TJTC_{it} = \frac{T_{it} - T_{it-1}}{E_{it-1}} - \frac{\sum_i (T_{it} - T_{it-1})}{\sum_i E_{it-1}}, \qquad (14)$$

where:

T_{it} = the number of TJTC certifications outstanding in location "i" in period t.

Or, if desired, T could be defined as the number of subsidized employees by applying a retention rate cumulatively to each months certifications. In that case,

$$T = \sum_{m=0}^{m=-24} c_m R^{-m},$$

where m is the month, c_m the number of certifications m months ago and R is the average monthly retention rate.

The key to the success of the proposed study is the variability and exogeneity of the TJTC variable. The usage of WIN and TJTC tax credits by individuals and firms varies significantly across the nation. This variation has a variety of causes:

1. The proportion of an area's population eligible for a tax credit varies substantially. TJTC eligibility rates are much higher

in the South because family incomes are lower there. Also, to be eligible for WIN, the person must be on AFDC, so WIN eligibility rates will depend on the generosity and administrative characteristics of the local AFDC program.

2. Some states are doing almost nothing to inform workers and firms about the availability of TJTC and WIN tax credits (ETA-OPER, 1979). Other states and localities are aggressively promoting the credits. Some states have made the determination of eligibility for and award of a TJTC voucher a routine part of the Job Services intake process; others have not.

The bureaucratic response to the program seems to depend upon a) internal bureaucratic politics (i.e., pre-existing conflicts between CETA and the Job Service); b) personalities (whether the head of the appropriate agency believes in the program); c) the ideological orientation of local WIN and Job Service personnel; d) the organizational effectiveness of the local Job Service, and e) the character of the pre-TJTC relationship between the Job Service and local employers.

Estimates of equations (12) and (13) are obtained by regression techniques. The coefficient is unbiased if variation in the TJTC variable is caused by factors not otherwise included in the model that, on their own account, are unrelated to employment change during the period of TJTC operation. The proportion of the population eligible for TJTC and the above list of bureaucratic factors appear to be of this nature.[4] Unfortunately, this claim does not apply to other sources of TJTC. For example, in tight labor markets businessmen are more willing to lower their hiring standards and to hire workers previously considered unsuitable. The NAB-JOB's program, which was quite successful during the tight labor markets of the late 60's, faded away during the recession that followed. Thus, employers should be more willing to hire the types of workers subsidized by TJTC and WIN when labor markets are tight and getting tighter. One would expect, therefore, that in tight labor markets the ratio of certifications (jobs obtained) to vouchers outstanding would go up. The other impact of a tight labor market is on the demand for vouchers. One would expect that when jobs are easy to get, fewer people would be applying at the Employment Service for job search assistance and fewer people would meet the income tests for eligibility.

This view of the determinants of TJTC usage can be stated more formally as a system of equations defined for the time period in which TJTC is operating:

$$\ln(TJTC_{it}) = a\ln(TJTC\text{-}V_{it}) + b\ln(E_{it}/E_{it-1}) + \underline{Z}_{it}\underline{c} + \varepsilon_{it}; \quad (15)$$

$$\ln(TJTC\text{-}V_{it}) = d\ln(E_{it}/E_{it-1}) + \underline{Z}_{it}\underline{g} + \varepsilon'_{it}, \quad (16)$$

where:

$TJTC_{it}$ = $(T_1 - T_0)$ the change in the number of certifications outstanding (vouchered workers with certified jobs who are still employed by the certified employer),

$TJTC\text{-}V_{it}$ = the change in the number of vouchers outstanding, 2nd

$\underline{Z}_{it}$ = a vector of variables measuring the proportion of the population eligible for a TJTC and the efforts of local manpower agencies to promote it.

We anticipate that a will be close to but less than 1, b will be positive, and d will be negative. We expect a positive net effect of tighter labor markets on TJTC usage ($b + \alpha d > 0$). If the true model is represented by the system of equations (12), (15) and (16), then estimating (12) alone using OLS will yield upward-biased estimates of α. We will argue that unbiased estimates of α can be obtained by using Z as an instrument of variable for TJTC.

The simultaneity of TJTC usage has important implications for data collection. The success of instrumental variable estimation of (12) or (13) depends on the ability of the $\underline{Z}$ variables to predict the rate of TJTC penetration and on our confidence that, conditional on X_{it}, D_i and D_t, they have no direct effect on the outcome variables. Measures of the pool of eligibles can be obtained from the 1980 Census and published program data. A major effort must be made to measure the intensity and effectiveness of employment service efforts to promote TJTC and WIN. Either the research organization responsible for the study should be asked to undertake this job or DOL should assign someone systematically to survey all employment service offices and WIN agencies about their efforts to promote WIN and TJTC. Good measures of the X's are also important. In this regard, we want to stress the necessity of measuring the scale and nature of other Department of Labor Programs like YIEPP, EOPP, etc. In fact, the best way to formulate this study is to see it as a simultaneous evaluation of all of DOL's job creation efforts--both in the private and the public sector.

Statistical Precision of the Estimator of TJTC Labor Market Impacts

In this section we will address the question of whether the available aggregate data permit the detection of a substantively significant TJTC impact. A judgment on this question requires a comparison of the standard error of the estimator of TJTC's labor market impact with the expected size of the estimator of TJTC impact.

Our procedure is first to specify the statistical model that will be used to measure the impact of TJTC, to calculate the standard error of the estimator of TJTC impact, and then to compare this standard error to the expected impact of the TJTC. An additional objective is to examine how sensitive the measure of TJTC's impact is to the number of locations studied and time periods used.

The primary limiting factor on the statistical precision of our estimators of TJTC's labor-market effects is the fact that TJTC is not the only exogenous event that will affect the growth path of these local economies. Examples of other such events are hurricanes, bumper harvests, killing frosts, large construction projects, and recessions specific to particular industries. The research design attempts to control for the effect of such events in two ways: by having a large sample size, so that these random events average out, and by constructing from aggregate data an index of the likely effect of such events (as many as can be quantified) on employment and earnings and then entering the predicted value of this index into our models. The model specified in equation (12) would be estimated using combined time-series cross-section data. Since dummies for time period appear in this equation, changes in the growth rate of aggregate national employment cannot be used to identify TJTC or WIN's impact. It is the variation across labor markets in the use of TJTC and the consequent differences among the growth rates of these labor markets that allows us to measure the impact of these programs. Let us assume that the TJTC variable has been defined as

$$TJTC_{it} = \frac{T_{it} - T_{it-1}}{E_{it-1}} - \overline{\Delta T}, \tag{15}$$

where:

T_{it} = the number of TJTC-subsidized employees in location "i" in period t (assuming a 10% monthly separation rate), and

$\Delta\overline{T}$ = the ratio of the national increase in TJTC-subsidized employees to national employment in the previous year

$$= \sum_i (T_{it} - T_{it-1}) / \sum_i E_{it-1}.$$

If simultaneity is judged to be a problem, instrumental variable predictions for $TJTC_i$ would be substituted. In the unlikely event that (1) all subsidized employees had been employed in the absence of TJTC, and (2) there were no displacement, the coefficient on TJTC would be 1. If 50% would have had jobs even in the absence of TJTC and there were no displacement, the coefficient would be .5. If 50% would have had jobs anyway, and 50% of the jobs taken by those who would not otherwise have been employed were taken from someone else who remained unemployed as a result, the coefficient would be .25.

The variance of the estimator α is:

$$Var(\alpha) = \frac{Var(u)}{N \cdot Var(TJTC) \cdot (1 - R^2_{TJTC \cdot X})}, \tag{16}$$

where:

N = the number of observations (the number of locations (m) times the number of time periods (k)),

Var(TJTC) = the variance of the variable measuring the scale of the program, and

$R_{TJTC \cdot X}$ = the proportion of the variance of the TJTC variable explained by the other independent variables in the impact equation.

The TJTC variable has been defined with the objective of making it orthogonal to the other X's in the model. It is defined as the deviation of the growth of TJTC certifications in that year from the national average. The mean of this variable is zero in every year, so there is no correlation between the TJTC variable and the dummies for time period that are also entered in the model. While at least 10 years of time-series data on each labor market would be used, the TJTC variable has variance only in the years in which the program is operating. Since the dummies for location do not change over time, their correlation with the TJTC variable will be extremely low. Consequently, we feel that for purposes of making power calculations it is legitimate to assume that $R_{TJTC \cdot X}$ is zero.

Since the mean of TJTC in every year is zero, its variance may be written as a simple sum of yearly variances divided by K, the total number of years in the sample (including the pre-TJTC period):

$$\mathrm{Var(TJTC)} = \frac{\sum_{t=1}^{k} \mathrm{Var(TJTC}_t)}{k} \quad . \qquad (17)$$

A study that used data up through June, 1981 would have two years of data on periods in which TJTC was in operation:[5]

$$\mathrm{Var(TJTC)} = \frac{\mathrm{Var(TJTC_{1980}) + Var(TJTC_{1981})}}{k} = \frac{2(\mathrm{AVT})}{k},$$

where AVT is the average yearly variance of TJTC. Extending the study through 1983 would yield three years of data on program operation and the variance of TJTC would be

$$\mathrm{Var(TJTC)} = \frac{\mathrm{Var(TJTC_{1980}) + Var(TJTC_{1981}) + Var(TJTC_{1982})}}{k} = \frac{3(\mathrm{AVT})}{k}.$$

Defining p as the number of years of program operation included in the study we may write:

$$\mathrm{Var(TJTC)} = p\ \mathrm{AVT}/k. \tag{18}$$

Substituting (18) into (16) we get:

$$\mathrm{Var}(\alpha) = \mathrm{Var(U)}/[m \cdot k(1 - R^2_{\mathrm{TJTC}\cdot X}) \cdot \frac{p(\mathrm{AVT})}{k} = \mathrm{Var(U)}/m \cdot p \cdot \mathrm{AVT}. \tag{19}$$

Taking square roots, we have a formula for the standard deviation of our estimator of program impact:

$$\sigma\alpha = \frac{\sigma_u}{\sigma_{\mathrm{AVT}}\sqrt{p \cdot m}}. \tag{20}$$

This formula implies that the precision with which we measure the impact of the program can be improved by increasing the number of locations and the number of years of program operation included in the study, by lowering the error variance of the model, and by increasing the variance of the treatment variable.

We will now turn to the determinants of the variance of the treatment variable (AVT). The study proposed to measure the impact of TJTC by comparing labor markets with large eligibility pools and rapid program implementation to labor markets with few eligibles and low rates of program implementation. For this approach to succeed the program must achieve a reasonable scale and there must be substantial variation across states and localities in the proportion of the population that receive TJTC vouchers. The standard deviation of the variable that represents the impact of the TJTC is

$$\sigma AVT = CV(\Delta T) \cdot \Delta\bar{T}; \qquad (21)$$

where:

$CV(\Delta T)$ = the coefficient of variation of the growth rate of TJTC certifications, and

$\Delta\bar{T}$ = the mean rate of growth of TJTC certifications.

Preliminary data on the number of TJTC certifications (vouchered workers whose employers have requested a certification) suggest that there is substantial variation across the ten Employment Service regions in the utilization of TJTC. The Southern region (Alabama, Florida, Georgia, Kentucky, Mississippi, North Carolina, South Carolina, and Tennessee) is responsible for 40% of all certifications nationwide and has a utilization rate that is 2.8 times the national average. The region that includes Arizona, California, Hawaii, Nevada and New Mexico, in contrast, has a utilization rate that is one third of the national average.

Relative to the mean utilization rate the standard deviation of the regional utilization rates is quite large; the coefficient of variation is .786. While this estimate of the coefficient of variation is based on preliminary data, we feel it is a reasonable lower bound on the variability across SMSA's and rural labor markets of the rate of increase in the utilization of TJTC as the program matures.[6]

Let us now return to the formula for the standard deviation of the estimator of program impact. Using .786 as the value of the coefficient of variation and substituting (21) into (20), we have:

$$\sigma_\alpha = \frac{\sigma_u}{.786 \cdot \Delta\bar{T} \cdot p \cdot m} . \tag{22}$$

The other determinant of the variance of the treatment variable (AVT) is $\Delta\bar{T}$, the yearly rate of increase in the number of outstanding certifications. Predicting the eventual scale of this program is not easy. The program does not seem to have settled down to a steady rate of expansion. Every month the number of certifications has increased: 5,300 in July, 9,200 in August, 14,200 in September, 21,000 in October, 31,000 in December and 33,000 in February, 1980. If the rate of new certifications stops growing and is maintained at the February rate and the separation rate of subsidized employees is 10% per month, the average number of employed subsidized workers will be 199,000 in 1980 and 303,000 in 1981. This would seem to be a lower-bound estimate on the eventual scale of the program. Every month 7 to 9 million people start new jobs. Thirty percent of these new hires are between 16 and 19 years old and 25% are between 20-24 years old (Cohen and Schwartz, 1979). At least 10% of the new hires under the age of 25 are eligible for TJTC, so at minimum 385,000 eligibles are hired each month. Thus, TJTC is serving less than 10% of its target group. The anticipated liberalization of the definition of cooperative education student will expand the pool of eligible 16-to-19-year-olds still further. The number of youth being hired each month that will be eligible may well be close to a million. With a market of this size, continuing growth in the number of new certifications has to be expected. If the certification rate were to top out at 100,000 a month, the number of subsidized workers would be 916,000 in the steady state. We expect the impact of TJTC to be concentrated on low-wage industries. Low-wage retail, service and manufacturing industries employ 40,000,000 workers. If the number of TJTC subsidized workers were to increase 300,000 per year in these industries, $\Delta\bar{T}$ would be equal to .0075.

In Table 1 we present estimates of the standard deviation of σ_α for various assumptions about the terminal date of the study, the number of locations studied and the standard error of estimate. For our preferred assumptions of 900 locations, $\Delta\bar{T} = .0075$, $\sigma_u = .03$, and a terminal date of 1981.II, the standard deviation of $\alpha = .12$. If the true value of α is .4, the power of a one-tailed test of the no-effect hypothesis using a 10% significance level is 98%. The power of a test of the hypothesis of 100% effectiveness is 99.9%. If the true value of α is .3, the test of the no-effect hypothesis has a power of 95%. For a true α of .25, the power of the test is 80%, whereas for a true α of .2, the power of the test is 65%. The estimates of $\sigma_u = .03$ are conservatively high. A successful model of local-area employment change

Table 1

Standard Deviation on the Coefficient Measuring TJTC's Impact on Low-Wage Industrial Employment (σ_α)

Yearly Increase in No. of Subsidized Employees in Low-Wage Industries ($\Delta\bar{T}$)	Terminal Date					
	1981.II			1980.II	1982.II	1983.II
	σ_u=.03 m=900	σ_u=.03 m=400	σ_u=.02 m=900	σ_u=.03 m=900	σ_u=.03 m=900	σ_u=.03 m=900
100,000 (.0025)	.36	.54	.24	.51	.29	.25
200,000 (.005)	.18	.27	.12	.25	.147	.127
300,000 (.0075)	.090	.18	.08	.17	.098	.085
400,000 (.01)	.090	.13	.06	.127	.073	.063
600,000 (.015)	.067	.09	.04	.085	.049	.042

Note: A terminal date of 1981.II means that two years of data during which TJTC has been operating are available.

σ_u = the standard error of the estimate.

m = the number of labor markets included in the study.

that takes account of exogenous shifts in demand originating in mining, agriculture, government and high-wage manufacturing should be able to lower σ_u to the neighborhood of .02. When $\sigma_u = .02$, the power of the test is 97% for a true α of .25 and 89% for a true α of .2.

4. SUMMARY AND RECOMMENDATIONS

The appropriate objective of the TJTC and WIN programs is to induce an expansion in the employment of the target groups without reducing the employment of other workers. Hence, the task of an evaluation of these programs is to provide estimates of their impact on both a) target group employment and b) total employment. No single study is able to provide definitive answers to both questions. The conclusions about program impacts that can be drawn from two alternative research designs are outlined in Table 2. Let us review what a study of job service registrants who receive TJTC certifications can tell us. (We will assume that the study controls for selection bias.) If the tax credit has no appreciable effect on this group we may also conclude that it has not affected either the full target group or total employment. The opposite finding--that target group members registering at the job service did get more or better jobs--does not, however, imply that total employment necessarily increased or even that total employment of all target group members increased. With this limitation in mind we turn to the administration of this type of evaluation.

Table 2

What Can Be Learned from Alternative Studies

Possible Findings From Studies That Successfully Control Selection Bias	Conclusions That Can Be Drawn about the Subsidy's Impact			
	Employment of Target Group JS Registrant	Employment of all Target Group Members	Total Employment	Employment in Firms Receiving Subsidy
Job Service registrant data (within-labor-market comparisons)				
No change by target group rej hyp $\hat{C} \geq C^*$	0	0	0	0
Increase by target group rej hyp $\hat{C} \leq 0$	+	?	?	?
<u>Aggregate data on SMSAs or county employment levels</u>*				
No change in total employment rej hyp $\hat{b} \geq b^*$	?	?	0	?
Increase in total employment rej hyp $\hat{b} \leq 0$	+	+	+	?

*Requires the assumption that migration responses to differential eligibility and use of TJTC are small.

The Within-Site Evaluation Using Job Service Registrant Data

A within-site evaluation could be undertaken as a component of the Cohen-Bressan data acquisition plan, but the evaluation we have recommended does not require data as extensive as in that plan.[7] Cohen and Bressan propose compiling TJTC data that include applicant characteristics and their subsequent wage records, using social security numbers of the applicants. The Cohen-Bressan plan also calls for collecting similar data for a large sample of nonapplicants from the Employment Security Automated Reporting System (ERARS) and/or the Applicant Data System (ADS).[5] In the Cohen-Bressan plan these records are to be used in an evaluation that compares the employment and earnings experience of TJTC participants with the ESARS control group.

The evaluation we have recommended needs only to use the applicant component of the Cohen-Bressan data. The applicant data may or may not be part of the ESARS. If they are part of ESARS, we need to know the steps by which a client of the Employment Service becomes an applicant to TJTC. Are the eligibles referred to TJTC or do they volunteer themselves? Given a well-defined group of applicants, our suggested strategy is to select sites where the program administrators have control over the selection of participants by explicit rules of eligibility. The applicants who are ineligible comprise the control group, and the eligibility criteria are necessary control variables.

In the Cohen-Bressan plan the ESARS sample is intended to constitute a control group without reference to the status of the members as applicants to TJTC. Assume the persons in the ESARS control group are not applicants.

The problem here is that unless we know the reasons they are not applicants we must question their comparability with the TJTC participants. Is the ESARS sample a higher quality of worker (i.e., more productive) because some members of the sample chose to pass up the TJTC program--perhaps believing they could obtain better jobs by other routes? Or, is the sample lower in quality or productivity, as evidenced by their unwillingness to participate in TJTC (or perhaps their not knowing of the availability of the TJTC program)? When the TJTC participants and ESARS control sample are located in different markets there is less reason to suspect their selectivity differences, but there is a new problem of a labor market difference that may be responsible for their respective outcomes in employment and earnings.

These question and reservations about the research design and control groups proposed by Cohen and Bressan do not imply that their data will not be useful. The evaluation design they propose is the type that most researchers have used in the past. It is one in which the burden is on the recorded characteristics of the workers and the markets they are in to control for all relevant determinants of labor market performance except for participation in TJTC. ("Relevant" means a determinant that is correlated with TJTC participation status). These studies are useful, but they do not appear as satisfactory as the method of "modelling the selection procedure" that we recommend for within-site evaluations. This latter method has the additional virtue that it does not require collecting data on a large scale. On the other hand, a disadvantage of the method is that it must be confined to sites that permit extensive knowledge and control of the selection procedures. Several evaluation designs are needed to hedge against the risks.

A third alternative that we discussed was the "selection bias adjustment" econometric method (see Heckman, 1979). This technique is currently receiving wide application in econometrics, and we advocate its use as a supplementary technique. It may easily be adapted to deal with Cohen-Bressan data and design. We have previously expressed our doubts about its robustness, however (we developed this point further in Barnow, Cain, and Goldberger, 1980). Our first priority of a within-site design remains that of "modelling the selection process."

Across-Site Evaluation Using Aggregate Data

The proposed study using aggregate data complements the job-service registrant study for it answers some of the questions that the registrant study cannot. It focuses on what happens to total employment. A finding that TJTC and WIN have caused total employment to go up necessarily implies that target-group employment went up as well (most likely by more than the amount that total employment increased). The opposite finding--rejection of the hypothesis that TJTC's impact on total employment was equal to or greater than some minimal level--does not, however, imply that the tax credit did not benefit its target population.

NOTES

[1]Some of these characteristics must be continuous variables, or at least have more than two values, in order that both these variables and program status variables can be included in the final statistical model determining post-program outcomes. In other words, there cannot be perfect collinearity between program (or treatment) status and any other control variable.

[2]Note that _x_ itself must be entered in equation (2) and not the dummy variable, 1 if $\leq \underline{x}^0$; 0 if $\geq x^0$. Were a dummy variable for _x_ used, there would be perfect collinearity between T and _x_ (measured as a dummy), so equation (2) would not be estimable.

[3]The correlation is not perfect because the length of tenure of subsidized employees varies from locality to locality.

[4]Note that since the location dummy captures the impact of all location-specific factors that are constant over time, and the TJTC variable is nonzero only during the latter part of the sample period, the crucial assumption here is that eligibility rates and bureaucratic factors do not have direct effects on employment growth during the TJTC period that are different from their direct effects in the pre-TJTC period.

[5]TJTC did not get started till June 1979.

[6]The use of regional data to measure the variability of utilization rates almost certainly produces downward biased estimates of the coefficient of variation. Within-region variation of utilization rates is suppressed. Over time there may be a tendency for utilization rates of the different counties and cities to converge. Since the TJTC variable is defined as

the rate of change (not the level) of cumulated certifications outstanding, the coefficient of variation can remain high even if during the third and fourth year of the program the laggard regions and localities start to catch up to those areas that led the way.

[7]These sampled workers should be "ES registrants who are disadvantaged, age 18-24, or disadvantaged Vietnam era veterans" (Cohen and Bressan, p. 22).

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