Social Experiments

Handbook of Econometrics

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1 Social Experiments

1.1 Introduction

Consider ideal experiments with no compliance or attrition problems.

Two distinct cases for the application of the method of randomized trials.

First case advocates randomization to identify structural parameters.

Second and more recent case seeks to use randomization to identify treatment parameters.

1.2 Treatment Effects vs. Structural Parameters

Marschak (1953): goal of structural estimation is to solve a variety of decision problems.[1]

Decision problems

(a) evaluating the effectiveness of an existing policy,

(b) projecting the effectiveness of a policy to different environments from the one where it was experienced,

(c) forecasting the effects of a new policy, never previously experienced.

[1] Recall the opening sentence of his seminal article: "Knowledge is useful if it helps us make the best decisions". (Marschak, 1953, p.1).

Marschak (1953) realized that for certain decision problems, knowledge of individual structural parameters, or any structural parameter, is unnecessary. Second, and neglected, contribution of his paper, notion of decision-specific parameters.

Prototypical problem of determining the impact of taxes on labor supply h.

Interior solution labor supply equation of hours of work h, wages, W, other variables including assets, ε denote an unobservable.

(3-1)

$$h = h(W, X, \varepsilon).$$

Additively separable version of the Marshallian causal function

(3-2)

$$h = h(W, X) + \varepsilon.$$

ceteris paribus effects of W and X on h

(3-1a)

$$h = h(W, X, \varepsilon, \theta)$$

θ is a low dimensional parameter that generates h. Separable version:

(3-2a)

$$h = h(W, X, \theta) + \varepsilon.$$

a linear-in-parameters Cowles Commission type representation of h :

(3-3)

$$h = \alpha' X + \beta \ell n W + \varepsilon$$

Distinguish 3 cases.

(1) The case where tax t has been implemented in the past and we wish to forecast the effects of the tax in the future in a population with the same distribution of (X, ε) variables as prevailed when measurements of tax variation were made.

(2) A second case where tax t has been implemented in the past but we wish to project the effects of the same tax to a different population of (X, ε) variables.

(3) A case where the tax has never been implemented and we wish to forecast the effect of a tax either on an initial population used to estimate (1) or on a different population.

Suppose the goal of the analysis is to determine the effect of taxes on average labor supply on a relevant population with distribution $G(W, X, \varepsilon)$.

Case One

Case one, we have data from the same population for which we wish to construct a forecast.

In the randomized trial, persons face tax rate

$\Pr(T = t_j \mid X, W, \varepsilon) = \Pr(T = t_j \mid X, W)$.

From each regime we can identify

(3-4)

$$E(h \mid W, X, t_j) = \int h(W(1-t_j), X, \varepsilon) dG(\varepsilon \mid X, W, t_j).$$

For the entire population:

(3-4a)

$$E(h \mid t_j) = \int h(W(1-t_j), X, \varepsilon) dG(\varepsilon, X, W \mid t_j).$$

Knowledge of (3-4) or (3-4a) from the historical data. No knowledge of any Marshallian causal function or structural parameter is required to do policy analysis for case one.

Case Two

Two resembles case one except for one crucial difference. Projecting the same policy onto a different population, it is necessary to break (3-4) or (3-4a) into its components and determine $h(W(1-t_j), X, \varepsilon)$ separately from $G(\varepsilon, X, W, t)$.

Assumptions Required to Project

(1) Knowledge of $h(\cdot)$ is needed on the new population. May entail determination of h on a different support from that used to determine h in the target population. Structural estimation comes into its own.

Parametric structure (3-3)

Knowledge of $G(\cdot)$ for the target population is also required. Exogeneity enters, a crucial facilitating assumption.

$$\text{(A-1)}\ (X, W, T) \perp\!\!\!\perp \varepsilon$$

$$G(\varepsilon \mid X, W, T) = G(\varepsilon).^2$$

Distribution of unobservables is the same in the sample as in the forecast or target regime, $G(\varepsilon) = G'(\varepsilon)$, $G'(\varepsilon)$ is the distribution of unobservables in the

$$E(h \mid W, X, t_j) = \int h(W(1 - t_j), X, \varepsilon) dG(\varepsilon)$$

Can determine $h(\cdot)$ over the new support of X. If $G' \neq G$. Face a new problem.

[2] There are many definitions of this term. Assumption (A-1) is often supplemented by the additional assumption that the distribution of X does not depend on the parameters of the model (*e.g.* θ in (2′) or (1′)).

Case Three

Third case, knowledge of the target population. Taxes operate through the term $W(1-t)$. No wage variation in samples

If wages vary in the presample period, the support of $W(1-t) \stackrel{\text{def}}{=} W^*$ in the target regime is contained in the support of W in the historical regime, conditional distributions of W^* and W given X, ε are the same, supports of (X, ε) are the same in both regimes,

(a) Support $(W^*)_{\text{target}} \subseteq$ Support $(W)_{\text{historical}}$

(b) $G(w^* \mid X, \varepsilon)_{\text{target}} = G(w \mid X, \varepsilon)_{\text{historical}}$

(c) Support $(X, \varepsilon)_{\text{target}} =$ Support $(X, \varepsilon)_{\text{historical}}$

$$W^* = W(1-t)$$

Under (a), can find a counterpart value of $W(1-t) = W^*$ in the target population. If these conditions are not met, necessary to build up the G and the h functions over the new supports using the appropriate distributions. Enter the realm where structural estimation is required.

1.3 Two Different Cases For Social Experiments

Demonstrate the contrasting nature of the two cases for social experiments.

Present a form of experimentation that identifies first, historically older, case

seeks to use randomization to identify Marshallian causal functions

"Treatment" is a tax policy: proportional tax on wages.[3]

[3] Historically, randomization was first used in economics to vary wage and income parameters facing individuals in order to estimate wage and income effects in labor supply to examine the implications of negative income taxes on labor supply. Part of the goal of randomization was to produce variation in wages and incomes to determine estimates of income and substitution effects. See Cain and Watts (1973). Ashenfelter (1983) shows how estimates of income and substitution effects can be used to estimate the impact of negative income taxes on labor supply.

Determine how labor supply responds to taxes t in an experimentally determined population.

Labor supply equation is $h = h(t, W, X, \varepsilon)$, taxes T are assigned to persons so that

(A-5) $(T \perp\!\!\!\perp \varepsilon) \parallel (W, X)$.

Thus $\Pr(T = t \mid W, X, \varepsilon) = \Pr(T = t \mid W, X)$. Assuming full compliance compute the labor supply given t ("treatment" or taxes) as

$$E(h \mid t, W, X) = \int h(t, W, X, \varepsilon) dG(\varepsilon \mid t, W, X) = \int h(t, W, X, \varepsilon) dG(\varepsilon \mid W, X)$$

For tax rate t',

$$E(h \mid t', W, X) = \int h(t', W, X, \varepsilon) dG(\varepsilon \mid W, X).$$

Form contrast:

$$E(h \mid t, W, X) - E(h \mid t', W, X) = \int [h(t, W, X, \varepsilon) - h(t', W, X, \varepsilon)] dG(\varepsilon \mid W, X).$$

May remove the conditioning on (W, X) by integrating out (W, X) Population average treatment effect for taxes (t, t') is

$$E_{F_c}(h \mid t) - E_{F_c}(h \mid t') = \int [E(h \mid t, W, X) - E(h \mid t', W, X)] dF_c(W, X),$$

Applying the results of the experiment to a new population, forecasting the effects of tax rates not previously experienced, requires the same types of adjustments described in Lecture 2.

Decompose $E(h \mid t, W, X)$ into $h(\cdot)$ and $G(\cdot)$

Additive Separability Helps

$$h = h(W, t, X) + \varepsilon,$$

$$E(h \mid W, X, t) - E(h \mid W, X, t') = h(W, X, t) - h(W, X, t').$$

Treatment effect is the difference between two Marshallian causal functions.

Specialize the Marshallian causal functions

$$h = \alpha_0 + \alpha_1 \ell n(W(1-t)) + \alpha_2' X + \varepsilon = \alpha_0 + \alpha_1 \ell n W + \alpha_1 \ell n(1-t) + \alpha_2' X + \varepsilon.$$

$$E(h \mid W, t, X) - E(h \mid W, t', X) = \alpha_1[\ell n(1-t) - \ell n(1-t')]$$

α_1 is identifiable from the treatment effects.

Randomization governed by (A-5) does not identify α_2. Generally, ε and W are stochastically dependent, variation induced in T a randomization that implements (A-5) does not make W or X exogenous.

Social experiments only identify treatment terms and terms that interact with treatment.

Main effects for (W, X) not identified. Thus consider the additively separable case $h(W, X, t, \varepsilon) = h(W, X, t) + \varepsilon$.

Under it we can recover $h(W, X, t) - h(W, X, t')$.

Decompose $h(W, X, t)$ into a main effect $\varphi(W, X)$,

an interaction term plus main effect for treatment term $\eta(W, X, t)$

May write $h(W, X, t) = \varphi(W, X) + \eta(W, X, t)$.

$\varphi(W, X)$ differences out all contrasts.

Only differences in $\eta(W, X, t)$ can be identified.

Randomization identifies the treatment effect (not by creating exogeneity between "right hand" variables and error term and identifying Marshallian causal parameters) by **balancing the bias**.

A consequence of (A-5)

$$E(\varepsilon \mid t', W, A) = E(\varepsilon \mid t, W, A).$$

"Control functions" (or conditional bias terms) balance of the bias.

If we seek to project the findings from one experiment to a new population with the same, task is greatly simplified by assuming

$$\varepsilon \perp\!\!\!\perp (W, X).$$

No longer necessary to determine distribution of ε given W, X $(G(\varepsilon \mid W, X))$

1.4 Identifying Assumptions For Three Commonly Implemented Types of Social Experiments

Consider three commonly implemented forms of social experiments motivated by the goal of evaluating entire programs or "treatments." Three populations or subpopulations on which randomization is suggested are:

(a) the entire population;

(b) a choice based population of persons who would participate in the program being evaluated in the absence of randomization and

(c) a population of persons eligible for the program.

Two key identifying assumptions for all methods are:

(1) that assignment to treatment, or eligibility status conditional on observables does not depend on unmeasured variables that affect outcomes

(2) the outcomes studied and the populations participating in the program being evaluated are not affected by randomization.

Assume $J+1$ Choices.

$A_j(\omega) = 1$ if person in a designated population is randomized into program j.

$A_0(\omega) = 1$ if person is not assigned to any treatment.

$T_j(\omega)$ denotes actual receipt of treatment j.

Counterfactual treatment participation indicators in world without randomization, denoted $D_j(\omega)$.

Full compliance:

$$A_j(\omega) = T_j(\omega). \tag{A-1}$$

Assignment to j implies treatment j is received.

Persons assigned treatment j ($A_j(\omega) = 1$) complying with assignment j ($T_j(\omega) = 1$) have chosen treatment j in the counterfactual world ($D_j(\omega) = 0$).

Assumption about assignment by randomization:

$$Pr(A_j(\omega) = 1 \mid X(\omega), \varepsilon(\omega)) = Pr(A_j(\omega) = 1 \mid X(\omega)).$$

Conditional on $X(\omega)$ assignment to treatment does not depend on $\varepsilon(\omega)$.

Alternatively,

$$\Pr(A_j(\omega) = 1 \mid X(\omega), \{Y_i(\omega)\}_{i=0}^{J}) = \Pr(A_j(\omega) = 1 \mid X(\omega)).$$

Full compliance assumption (A-1): can replace $A_j(\omega)$ by $T_j(\omega)$.

(A-2) ensures that assignment to treatment is not made on the basis of unobservables.

The mechanism used to enforce (A-2) need not be randomization.

Randomization produces information about the program of interest operating under usual conditions provided that it does not alter behavior. This idea is formalized in a third assumption.

Let $A = (A_0(\omega), ..., A_J(\omega))$ and $Y = (Y_0(\omega), ..., Y_J(\omega))$.

Absence of randomization bias:

(A-3) Y invariant to mechanism of assignment or selection of outcome.

(Recall Handbook discussion.)

1.5 Randomization On Choice Based Subpopulations

Full compliance assumption (A-1) is very strong.

In a social setting, it is difficult to force people to participate in a program to secure compliance.

Randomization is often attempted on subpopulations of persons for whom $D_j(\omega) = 1$ $i.e$ persons who would have gone into program j in the absence of randomization.

This form of randomization is usually operationalized by administering it to people who apply, and are accepted into a program.

Process of randomization may alter the composition of the population that would apply and be accepted into the program.

Parallel set of potential treatment and outcome variables $\{D_j^R\}_{j=0}^J$ and $\{Y_j^R\}_{j=0}^J$, indexed by R

Let $T_j(\omega)$ denote actual treatment choice in the presence of randomization.

Full compliance requires

$$\begin{aligned} A_j(\omega) &= T_j(\omega), \;\; j = 0, ..., J \\ A_j(\omega) &= T_j(\omega) = 0, T_0(\omega) = 1, j = 0, ..., J. \end{aligned}$$

This method of randomization assumes that persons randomized out of treatment j forces persons into the no treatment state.

Method assumes that assignment is made on the basis of observables:

$$\begin{aligned}\Pr(A_j(\omega) &= 1 \mid X(\omega), \{Y_j^R(\omega)\}_{j=0}^J, D_j^R = d_j^R) \\ &= \Pr(A_j(\omega) = 1 \mid X(\omega), D_j^R = d_j^R)\end{aligned}$$

Observe (through application and acceptance) persons who apply to j in a regime of randomization.

Population generated by this randomization rule.

Random sample of (Y_j^R, Y_0^R) for $D_j^R = 1$.

To ensure that randomization generates the participants and outcomes associated with the usual no-randomization regime,

(A-2a) $$D_j^R(\omega) = D_j(\omega) \qquad j = 0, ..., J$$

$$Y_\ell^R(\omega) = Y_\ell(\omega) \parallel D_j(\omega) = 1, \qquad j = 0, ..., J, \ \ell = 0, ..., L.$$

Distribution of observed characteristics is the same in both the usual regime (with the "R" superscript) and in the randomized regime:

$$F(x \mid D_j^R = 1) = F(x \mid D_j = 1), \quad j = 0, .., J.$$

Can weaken these assumptions if we assume homogeneity in response to treatment "common coefficient model":

$$Y_j(\omega) - Y_0(\omega) = \Delta_{j,0},$$

Effect of the treatment is the same as everyone.

(Invariance through homogeneity)

1.6 Randomization of Eligibility

Randomization of eligibility creates samples that can be used to infer choice probabilities among competing programs.

Let $e_j = 1$ denote eligibility for participation in treatment j.

First assume that denial of eligibility for j implies that a person is embargoed from taking any other treatment.

Later we consider the case where a person is free to select treatments other than j.

Assume full compliance so that

$$\text{(A-4) } e_j(\omega) = 0 \Longrightarrow T_j(\omega) = 0 \text{ and } T_0(\omega) = 1.$$

Assumption that justifies this type of randomization

(A-5) $\Pr(e_j = 1 \mid X, \{Y_\ell\}_{\ell=0}^{J})) = \Pr(e_j = 1 \mid X), \qquad j = 1, ..., J,$

If eligibility is determined for only for one treatment, say j, and the purpose is to compare outcomes we can get by with a weaker requirement:

$$\Pr(e_j = 1 \mid X, (Y_0, Y_j)) = \Pr(e_j \mid X), \qquad j = 1..., J.$$[4]

[4] The condition would be modified to $\Pr(e_j = 1 \mid X, Y_\ell, Y_j) = \Pr(e_j = 1 \mid X)$ for studying treatment ℓ, j comparisons.

Changing the population of eligible persons can in principle change the program being studied compared to how it would operate without such eligibility restrictions.

Sufficient conditions ensure that randomization of eligibility does not disrupt the normal operations of the program.

"e" as a superscript denote random variables

Sufficient set of conditions for randomization of eligibility to be non-disruptive is that

(A-6) $D_j^e(\omega) = D_j(\omega) \qquad j = 0, ..., J$

$Y_j^e(\omega) = Y_j(\omega) \qquad j = 0, ..., J$

$F(x \mid D_j^e = 1) = F(x \mid D_j = 1)$ distribution of the X is the same for potential participants

Under (A-6), persons self-select (or are selected) into program in the usual way.

Randomization of eligibility creates a population of persons for whom $e_j(\omega) = 0$, $T_j(\omega) = 0$ and $T_0(\omega) = 1$

Population consists of two groups of persons:

those who would have taken treatment j $(D_j(\omega) = 1)$,

those who would not $(D_j(\omega) = 0)$ in the absence of randomization of eligibility.

For those made ineligible, we obtain $Y_0(\omega)$.

Can identify

$$F(y_0 \mid X = x) = F(y_0 \mid X = x, e_j = 0)$$

for all persons irrespective for their D_j value. By Law of iterated expectations, decompose this into $J + 1$ components:

(3-17) $F(y_0 \mid X = x) = \sum_{\ell=0}^{J} F(y_0 \mid D_\ell = 1, X = x) \Pr(D_\ell = 1 \mid X = x).$

For those made eligible, we obtain

(3-18) $F(y(j) \mid D_j^e = 1, X = x, e_j = 1)$

$$= F(y(j) \mid D_j = 1, X = x), \quad j = 1, ..., J$$

(3-19) $F(y_0 \mid D_j^e = 0, X = x, e_j = 1) = F(y_0 \mid D_j = 0, X = x)$.

From the eligible sample, determine the choice probabilities

(3-20) $\Pr(D_j = 1 \mid X, e_j = 1) = \Pr(D_j = 1 \mid X)$.

For the case of one treatment, $J = 1$, we obtain from the ineligible sample

$$F(y_0 \mid D_0 = 1, X = x) \Pr(D_0 = 1 \mid X = x)$$
$$+F(y_0 \mid D_1 = 1, X = x) \Pr(D_1 = 1 \mid X = x).$$

Supplement the data from the experiment with the additional information from nonexperimental data,

$(F(y_0 \mid D_0 = 1, X = x)$, identify $F(y_0 \mid D_1 = 1, X = x)$.

For the case $J > 1$, acquire only the combination of potential outcome distributions.

Even supplementing this with the information from nonexperimental data $F(y_0 \mid D_0 = 1, X)$, we obtain less information on outcome distributions than is obtained under randomization scheme two.

The information can be used to bound distributions but not to exactly identify them.

In this sense, randomization of eligibility for the case $J > 1$ is less informative than randomization at the stage of application and acceptance, case two experimentation.

Another type of randomization of eligibility denies access to program j but permits participation in all other programs. Enables analysts to estimate $\Pr(D_\ell = 1 \mid X)$ facilitates identification of the choice probabilities over what can be obtained in the nonexperimental case by varying the choice sets of participants randomized out of eligibility (see *e.g.* Falmage, 1990).

Assuming full compliance this form of randomization recovers

$$F(y_\ell \mid D_\ell^{e_j=0} = 1, X = x, e_j = 0) = F(y_\ell \mid D_\ell^{e_j=0} = 1, X = x)$$

$$\ell = 0, ..., J, \qquad \ell \neq j, j = 1, ..., J.$$

$D_\ell^{e_j=0}$ is choice made when choice j is removed from the choice set. For persons denied eligibility from j, where $D_\ell^{e_j=0}$ informative about outcomes of participants in a counterfactual world when choice j is eliminated and people are free to select among competing alternatives, including no alternative at all.

Compared with ordinary observational data, obtain information about such objects as

$$E(Y_\ell \mid D_\ell^{e_j=0} = 1, X = x) - E(Y_\ell \mid D_\ell = 1, X = x)$$

This experimental not informative about a variety of useful counterfactual distributions:

$$F(y_m \mid D_\ell = 1, X = x), m \neq \ell, m, \ell \neq j$$

1.7 The Data Generated By The Three Kinds of Social Experiment In Comparison With What Is Produced From Nonexperimental Data

Without additional information imposed, there is no information on joint distributions of outcomes.

For the case $J = 1$, we observe $F(y_0 \mid X), F(y_1, X)$ from Type I random assignment, do not identify $F(y_0, y_1 \mid X)$.

Conventional way to obtain joint distributions is to assume a "unit additivity" model:

$$Y_\ell(\omega) - Y_j(\omega) = \Delta_{\ell j}, \qquad \ell \neq j, \ell, j = 0, ..., J$$

where the $\Delta_{\ell j}$ are constants. Alternatively, it can be a constant conditional on $X = x$.

Type I and Type II randomizations do not produce samples that identify choice probabilities.

Case of multiple treatments ($J > 1$), randomization of eligibility does not identify the conditional distributions identified under Type II randomization.

Augmenting this information with non-experimental data, identifies

$$\sum_{\ell=1}^{J} F(Y_\ell \mid D_\ell = 1, X = x) \Pr(D_\ell = 1 \mid X = x).$$

Each of the three types of experimental data can be supplemented with non-experimental data law of iterated expectations

$$
\begin{aligned}
F(Y_0 \mid X) = {} & F(Y_0 \mid D_0 = 1, X = x) \Pr(D_0 = 1 \mid X = x) \\
& + F(Y_0 \mid D_1 = 1, X = x) \Pr(D_1 = 1 \mid X = x).
\end{aligned}
$$

From the nonexperimental data, we can identify the probabilities and $F(Y_0 \mid D_0 = 1, X = x)$. Thus we can identify

$$F(Y_0 \mid D_1 = 1, X = x) = \frac{F(Y_0 \mid X = x) - F(Y_0 \mid D_0 = 1, X = x)\,\Pr(D_0 = 1 \mid X = x)}{\Pr(D_1 = 1 \mid X = x)}$$

assuming that $\Pr(D_1 = 1 \mid X = x) \neq 0$.

Thus supplementing the data from a Type I experiment with nonexperimental obtain all of the information available from a Type II randomization. By similar reasoning, using nonexperimental data we can obtain more information than is obtained from a Type II randomization:

$$F(Y_1 \mid D_0 = 1, X = x),$$

is obtained, because

$$F(Y_1 \mid X = x) = F(Y_1 \mid D_1 = 1, X = x) \Pr(D_1 = 1 \mid X = x)$$

$$+F(Y_1 \mid D_0 = 1, X = x) \Pr(D_0 = 1 \mid X = x).$$

This analysis does not generalize to the case $J > 1$.

$$(3\text{-}21) \qquad F(Y_\ell \mid X = x) = \sum_{\ell=0}^{J} F(y_\ell \mid D_j = 1, X = x)$$
$$\Pr(D_j = 1 \mid X = x), \quad \ell = 0, ..., J, j = 0, ..., J \ .$$

Nonexperimental data, we observe

$$F(Y_j \mid D_j = 1, X = x) \qquad j = 0, ..., J$$

1.8 Marginal Experimentation

Frequently of interest to evaluate the effects of variation in policy variables Z within a program (Electricity Experiments)

Let Y be an outcome of interest and define a Marshallian causal function

$$Y(\omega) = g(X(\omega)).$$

$X(\omega)$ into $X_0(\omega)$ and $X_u(\omega)$, $X_0(\omega)$ of $X_u(\omega)$:

$$X_0(\omega) \perp\!\!\!\perp X_u(\omega).$$

excluded variables $Z(\omega)$

$$Z(\omega) \perp\!\!\!\perp X_u(\omega).$$

In place of randomization, we may assign $X_0(\omega)$

(3-22) $$X_0(\omega) = H(Z(\omega))$$

additively separable

$$g : g(X(\omega)) = g_0(X_0(\omega)) + g_u(X_u(\omega))$$

$$E(Y(\omega) \mid X_0(\omega) = x_0) = \int g(x_0, X_u(\omega))dF_u(X_u(\omega))$$

1.9 Non-compliance, Attrition and Selection Bias

1.10 Randomization Bias and Substitution Bias

Table 1
Information Obtained From Different Experiments and From Nonexperimental Data Under Full Compliance and Other Identification Conditions

	Type I **Random Assignment To the General Population**	**Type II** **Random Assignment Among Accepted Applicants**	**Type III** **Random Assignment of Eligibility**	**Non-Experimental Data**
One Treatment $J=1$	$F(Y_0 \mid X), F(Y_1 \mid X)$	$F(Y_0 \mid D_1, X=x)$ $F(Y_1 \mid D_1, X=x)$	$F(Y_0 \mid D_0=1)$ $F(Y_0 \mid D_1=1)$ $F(Y_1 \mid D_1=1)$ $\Pr(D_1=1 \mid X)$	$F(Y_0 \mid D_1, X=x)$ $F(Y_1 \mid D_1, X=x)$ $\Pr(D_1=1 \mid X=x)$
Mean Treatment Effect Identified	$E(Y_1-Y_0 \mid X)$	$E(Y_1-Y_0 \mid D_1=1, X)$	$E(Y_1-Y_0 \mid D=1, X)$	None in general
Multiple Treatments $J>1$	$F(Y_l \mid X)$, $l=0,\ldots,J$	$F(Y_0 \mid D_l=1, X=x)$ $F(Y_l \mid D_l=1, X=x)$ $l=0,\ldots,J$	$F(Y_l \mid D_l=1, X)$ $\sum_{l=0}^{L} F(Y_0 \mid D_l=1, X)$ $\Pr(D_l=1 \mid X)$ $l=0,\ldots,L$	$F(Y_l \mid D_l=1, X=x)$ $\Pr(D_l=1 \mid X=x)$ $l=0,\ldots,J$
Mean Treatment Effects Identified	$E(Y_l \mid X)-E(Y_j \mid X)$ $l,j=0,\ldots,J$	$E(Y_l-Y_0 \mid D_l=1, X)$ $l=0,\ldots,J$		None in general

Experiments

(a) Disrupt Environments

(Heckman, 1992; Hotz, 1992)

Randomization Bias

(b) Do not capture entry effects

(Heckman 1992; Moffit 1992)

(c) Substitution Bias

(Heckman, Hohmann and Khoo)

TABLE 3
Treatment Group Dropout and Control Group Substitution
in Experimental Evaluations of Active Labor Market Policies
[Fraction of Experimental Treatment and Control Groups Receiving Services]

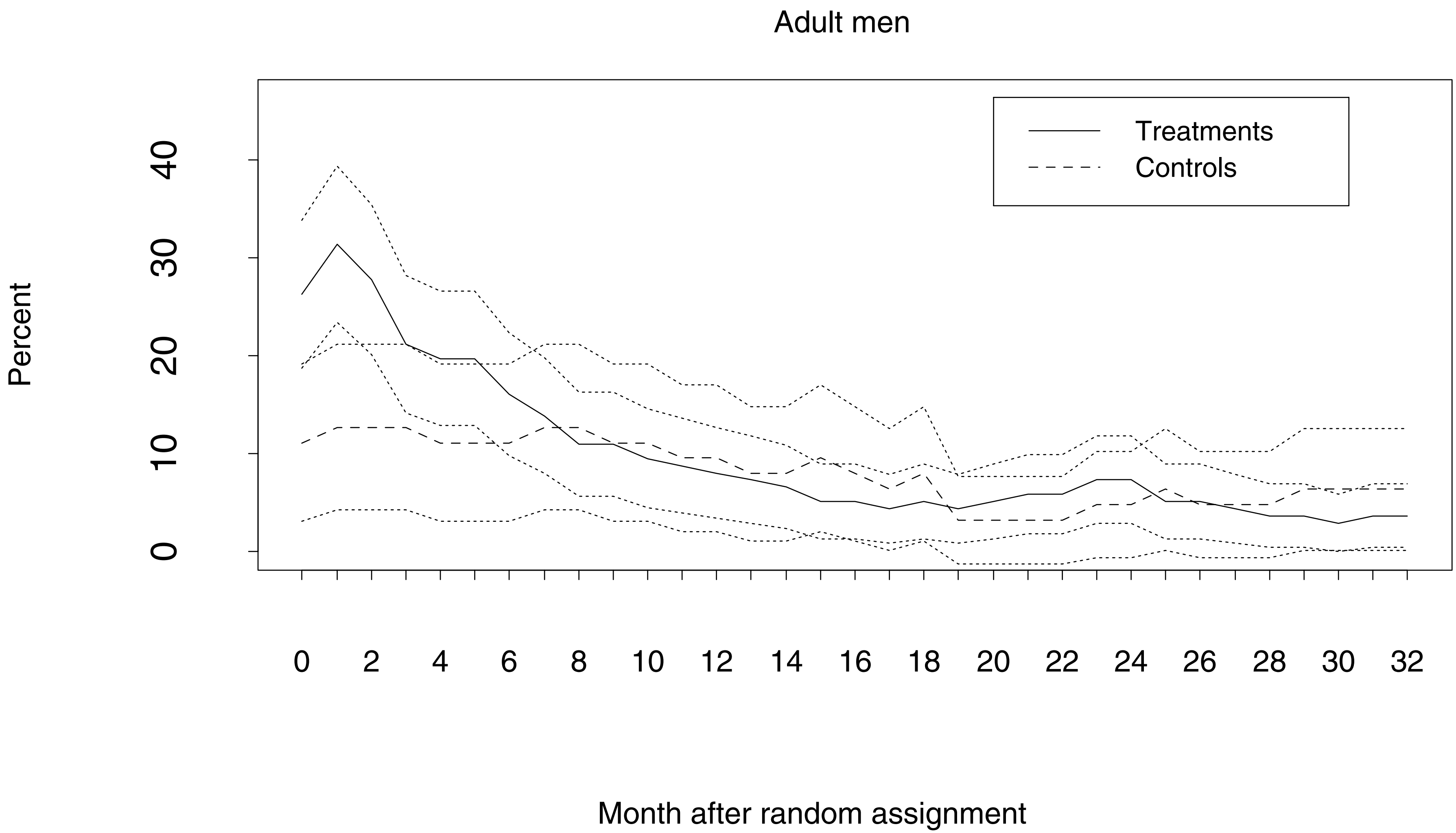
Study	Authors/Time Period	Target Group(s)	Fraction of Treatments Receiving Services	Fraction of Controls Receiving Services
1. NSW*	Hollister, et al. (1984)	Long Term AFDC Women	0.95~	0.11
	(9 months after RA)	Ex-addicts	NA	0.03
		17 - 20 year old H.S. dropouts	NA	0.04
2. SWIM	Friedlander and Hamilton (1993)	AFDC Women: Applicants and Recipients		
	(Time period not reported)	a. Job Search Assistance	0.54	0.01
		b. Work Experience	0.21	0.01
		c. Classroom Training/OJT	0.39	0.21
		d. Any activity	0.69	0.30
		AFDC-U Unemployed Fathers		
		a. Job Search Assistance	0.60	0.01
		b. Work Experience	0.21	0.01
		c. Classroom Training/OJT	0.34	0.22
		d. Any activity	0.70	0.23
3. JOBSTART	Cave, et al. (1993)	Youth High School Dropouts		
	(12 months after RA)	Classroom Training/OJT	0.90	0.26
4. Project Independence	Kemple, et al. (1995)	AFDC Women: Applicants and Recipients		
	(24 months after RA)			

		a. Job Search Assistance	0.43	0.19
		b. Classroom Training/OJT	0.42	0.31
		c. Any activity	0.64	0.40
5. New Chance	Quint, et al. (1994) (18 months after RA)	Teenage Single Mothers		
		Any education services	0.82	0.48
		Any training services	0.26	0.15
		Any education or training	0.87	0.55
6. NJS	Heckman and Smith (1998c) (18 months after RA)	Self-reported from Survey Data		
		Adult Males	0.38	0.24
		Adult females	0.51	0.33
		Male youth	0.50	0.32
		Female youth	0.58	0.41
		Combined Administrative and Survey Data		
		Adult males	0.74	0.25
		Adult females	0.78	0.34
		Male youth	0.81	0.34
		Female youth	0.81	0.42

Notes: RA = random assignment. H.S. = high school. Service receipt includes any employment and training services. The services received by the controls in the NSW study are CETA and WIN jobs. For the Long Term AFDC Women, this measure also includes regular public sector employment during the period.

Sources: Masters and Maynard (1981), p. 148, Table A.15; Maynard (1980), p. 169, Table A14. Friedlander and Hamilton (1993), p. 22, Table 3.1; Cave, et al. (1993), p. 95, Table 4-1; Kemple, et al. (1995), p. 58, Table 3.5; Quint, et al. (1994), p. 110, Table 4.9; Heckman and Smith (1998c) and calculations by the authors.

Figure 9
Percentage Receiving Classroom Training



The percentages are the proportion of persons among the sample who report the receipt of classroom training in each month following random assignment. The sample includes only those persons who responded for the entire 32 months of the survey. Month 0 is the month of random assignment. Standard error bars indicate +/- 2 standard errors about the mean.

Figure 10
Percentage Receiving Classroom Training

The percentages are the proportion of persons among the sample who report the receipt of classroom training in each month following random assignment. The sample includes only those persons who responded for the entire 32 months of the survey. Month 0 is the month of random assignment. Standard error bars indicate +/- 2 standard errors about the mean.

TABLE I

FRACTION OF EXPERIMENTAL TREATMENT AND CONTROL GROUPS RECEIVING SERVICES IN EXPERIMENTAL EVALUATIONS OF EMPLOYMENT AND TRAINING PROGRAMS

Study	Authors/time period	Target group(s)	Fraction of treatments receiving services	Fraction of controls receiving services
1. NSW	Hollister et al. {1984} (9 months after RA)	Long-term AFDC women	0.95	0.11
		Ex-addicts	NA	0.03
		17–20 year old high school dropouts	NA	0.04
2. SWIM	Friedlander and Hamilton {1993} (Time period not reported)	AFDC women: applicants and recipients		
		a. Job search assistance	0.54	0.01
		b. Work experience	0.21	0.01
		c. Classroom training/OJT	0.39	0.21
		d. Any activity	0.69	0.30
		AFDC-U Unemployed fathers		
		a. Job search assistance	0.60	0.01
		b. Work experience	0.21	0.01
		c. Classroom training/OJT	0.34	0.22
		d. Any activity	0.70	0.23
3. JOBSTART	Cave et al. {1993} (12 months after RA)	Youth high school dropouts		
		Classroom training/OJT	0.90	0.26
4. Project independence	Kemple et al. {1995} (24 months after RA)	AFDC women: applicants and recipients		
		a. Job search assistance	0.43	0.19
		b. Classroom training/OJT	0.42	0.31
		c. Any activity	0.64	0.40
5. New chance	Quint et al. {1994} (18 months after RA)	Teenage single mothers		
		Any education services	0.82	0.48
		Any training services	0.26	0.15
		Any education or training	0.87	0.55

Service receipt includes any employment and training services. RA denotes random assignment to treatment or control groups. In the NSW study, services received by controls are CETA and WIN jobs, for in the Long-term AFDC women group services received also include regular public sector employment. *Sources:* Masters and Maynard {1981}, p. 148, Table A.15; Maynard {1980}, p. 169, Table A14; Friedlander and Hamilton {1993}, p. 22, Table 3.1; Cave et al. {1993}, p. 95, Table 4.1; Kemple et al. {1995}, p. 58, Table 3.5; Quint et al. {1994}, p. 110, Table 4.9.

TABLE II

CHARACTERISTICS OF CLASSROOM TRAINING IN THE 19 MONTHS FOLLOWING RANDOM ASSIGNMENT

	Adult men			Adult women			Male youth			Female youth		
	Treatment	Control	Prob $(t > \|T\|)$	Treatment	Control	Prob $(t > \|T\|)$	Treatment	Control	Prob $(t > \|T\|)$	Treatment	Control	Prob $(t > \|T\|)$
Sample size	744	325		1,697	816		377	174		734	352	
Number receiving CT	363	89		952	272		210	60		430	141	
Percent receiving CT	48.8%	27.4%	0.00	56.1%	33.3%	0.00	55.7%	34.5%	0.00	58.6%	40.1%	0.00
Characteristics of persons with one or more training spells												
Average total months of training	6.7	7.6	0.19	7.2	8.0	0.04	7.0	6.4	0.47	6.7	7.0	0.56
Average total hours of training	680.7	699.0	0.83	705.9	779.3	0.17	745.7	661.4	0.45	765.3	585.2	0.00
Average hours per month of training	110.3	93.5	0.04	100.5	91.4	0.01	110.2	109.5	0.97	108.8	87.3	0.00
Fraction of training months employed	50.2%	46.9%	0.77	34.7%	38.1%	0.51	47.8%	59.2%	0.17	32.8%	48.0%	0.02
Percent paying for training	16.8%	41.6%	0.00	11.6%	39.0%	0.00	16.7%	48.3%	0.00	13.0%	36.2%	0.00
Average monthly payment	$209	$358	0.44	$25	$101	0.00	$39	$103	0.02	$43	$226	0.21

The sample is rectangular and includes all persons from the sixteen experimental sites with valid data on training receipt. T-tests are of the null hypothesis that the means of the treatment and control samples are equal within demographic groups. Classroom training payments are the amount paid by the entire household. Average payments are for each trainee and include trainees who reported zero expenditures.

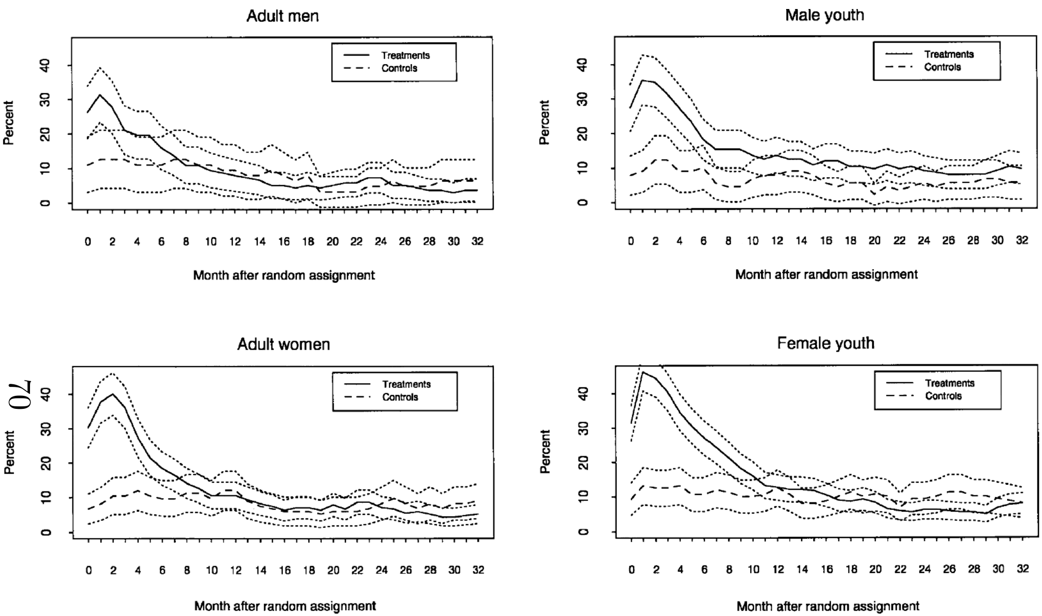


FIGURE I

Percentage Receiving Classroom Training

The percentages are the proportion of persons among the sample who report the receipt of classroom training in each month following random assignment. The sample includes only those persons who responded for the entire 32 months of the survey. Month 0 is the month of random assignment. Standard error bars indicate +/−2 standard errors about the mean.

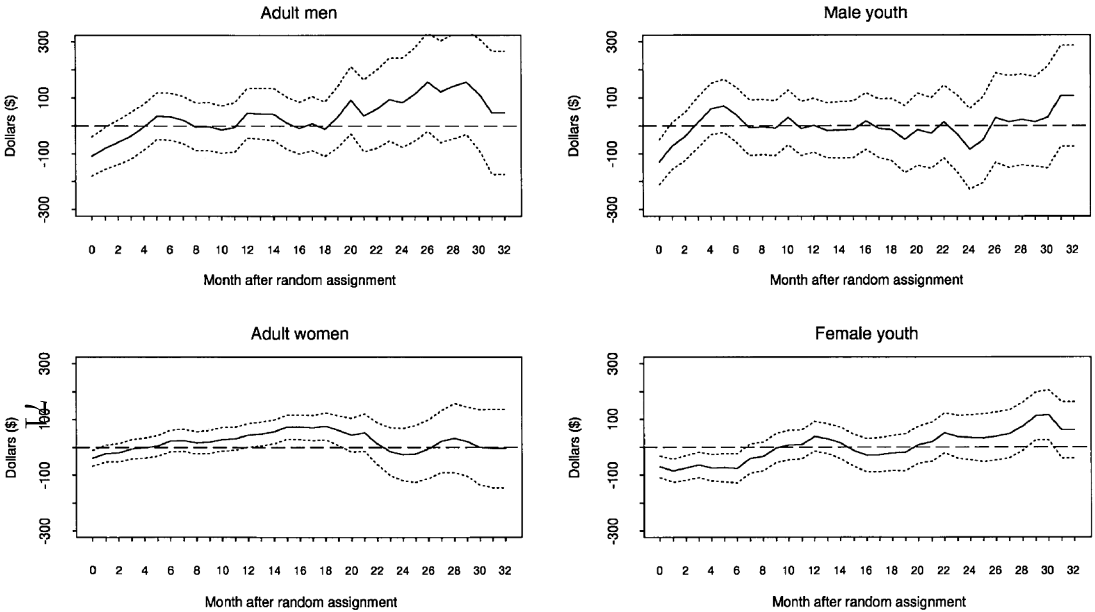


FIGURE II

Experimental Estimates of the Monthly Effect of the JTPA Program

The dependent variable in an OLS regression is self-reported monthly earnings. The sample consists of all person-months in the 32 months after random assignment (RA) with valid values for all variables. Regressors include indicators for treatment status, calendar month, month after RA, treatment status*month after RA, race, marital status, education, training center of random assignment, age, and English language preference. The top 1 percent of earnings values are dropped in each month in both the treatment and control groups. Standard error bars indicate +/− 2 Eicher-White robust standard errors about the mean.

TABLE III

MEAN DISCOUNTED EARNINGS AND ESTIMATES OF THE DISCOUNTED RETURNS TO THE JTPA PROGRAM R_0

	Adult males			Adult females			Youth males			Youth females		
	33 months	5 years	10 years	33 months	5 years	10 years	33 months	5 years	10 years	33 months	5 years	10 years
r	Discounted earnings											
0	25625	49303	101921	12737	25437	53659	20829	41765	88288	11441	22128	45877
0.03	24513	45596	87652	12163	23472	46029	19883	38525	75709	10941	20457	39439
0.10	22149	38254	63369	10945	19584	33054	17875	32115	54321	9878	17147	28483
	Estimates of the discounted returns											
0	1337	3949	9755	787	1002	1481	209	491	1117	269	1704	4892
	(102)	(478)	(1357)	(57)	(300)	(860)	(102)	(409)	(1146)	(58)	(231)	(642)
0.03	1248	3574	8214	755	947	1330	190	441	942	222	1500	4048
	(98)	(430)	(1132)	(54)	(270)	(717)	(97)	(369)	(957)	(55)	(209)	(536)
0.10	1061	2838	5609	687	833	1062	152	343	642	125	1101	2623
	(87)	(338)	(756)	(48)	(211)	(478)	(88)	(291)	(640)	(49)	(165)	(360)
	Estimates of the internal rates of returns											
	1.17	1.27	1.28	2.63	2.63	2.63	0.66	0.79	0.80	0.21	0.53	0.58

Discounted earnings are the present discounted value of mean monthly control group earnings discounted at $1/(1 + r)$, where r ranges over 0, 0.03, and 0.10. Estimates of R_0 are the present discounted value of the effect of the program based on program effects with the indicated durations. Monthly earnings beyond the 33-month sample are set at the mean level of months 22 to 33 after random assignment. Estimates are of private returns and include estimated average monthly tuition payments. The internal rate of return is the annual rate of return r such that the net present value of the earnings/cost stream, discounted at $1/(1 + r)$, is equal to zero. Rates of return are reported as fractions, *not as percentages*. Internal rate of return estimates are also private estimates and so include the estimated monthly tuition payments. Estimated standard errors appear in parentheses.

TABLE IV

ESTIMATES OF THE DISCOUNTED RETURNS TO JTPA TRAINING R_1

	Adult males			Adult females			Youth males			Youth females		
	33 months	5 years	10 years	33 months	5 years	10 years	33 months	5 years	10 years	33 months	5 years	10 years
r	Estimates of discounted returns adjusted by training incidence											
0	5268	17429	44454	3687	4622	6700	847	2203	5218	2929	12186	32756
	(530)	(2737)	(7873)	(251)	(1221)	(3474)	(526)	(2149)	(5980)	(296)	(1511)	(4318)
0.03	4862	15690	37290	3538	4371	6032	754	1962	4371	2608	10850	27292
	(504)	(2460)	(6562)	(238)	(1098)	(2898)	(500)	(1937)	(4995)	(281)	(1359)	(3601)
0.10	4012	12284	25183	3215	3851	4843	564	1487	2926	1947	8243	18061
	(450)	(1925)	(4369)	(211)	(861)	(1933)	(446)	(1526)	(3346)	(248)	(1064)	(2401)
	Estimates of discounted returns adjusted by training hours											
0	3739	12367	31542	3875	5114	7868	758	2071	4989	1281	6998	19702
	(427)	(1676)	(4654)	(274)	(1501)	(4325)	(467)	(2013)	(5641)	(200)	(959)	(2729)
0.03	3446	11129	26454	3714	4818	7019	670	1839	4172	1088	6178	16332
	(408)	(1512)	(3889)	(259)	(1348)	(3604)	(444)	(1813)	(4710)	(190)	(863)	(2276)
0.10	2831	8700	17852	3366	4209	5523	491	1385	2777	690	4579	10642
	(368)	(1195)	(2608)	(228)	(1053)	(2398)	(395)	(1426)	(3150)	(169)	(677)	(1519)
	Estimates of the internal rates of return adjusted by training incidence											
	0.79	0.96	0.97	2.45	2.45	2.45	0.45	0.62	0.65	0.43	0.70	0.73
	Estimates of the internal rates of return adjusted by training hours											
	0.73	0.90	0.91	2.44	2.44	2.44	0.41	0.59	0.62	0.26	0.56	0.61

Estimates based on incidence of classroom training receipt are constructed using estimated monthly program effects (Δ_s) adjusted upward by $1/(p_s - q_s)$, where p_s is the proportion of treatments who have received some level of classroom training by month s, and q_s is defined similarly for controls. Estimates based on hours of classroom training receipt are constructed using estimated monthly program effects (Δ_s) adjusted upward by $1/(p_s - q_s)$, where p_s is the average cumulative hours of classroom training received by treatments by month s, and q_s is defined similarly for controls. These constant hourly impact figures are then multiplied by the average cumulative hours of classroom training received by treatments who report at least one CT training spell by month s (i.e., treatments who actually receive training) to yield the monthly effect of training based on constant hourly effects. Monthly earnings beyond the 33-month sample are set at the mean level of months 22 to 33 after random assignment. Estimates are of private returns and include estimated average monthly tuition payments. The internal rate of return is the annual rate of return r such that the net present value of the earnings/cost stream, discounted at $1/(1 + r)$, is equal to zero. Estimated standard errors appear in parentheses.

TABLE V

NONEXPERIMENTAL ESTIMATES OF THE MONTHLY EFFECTS OF TRAINING Δ_d AND Δ_a ON TREATMENT GROUP MEMBERS

	Adult males		Adult females		Youth males		Youth females	
Sample size	19438		42943		11328		22052	
Mean earnings gain	777		386		632		347	
	Difference-in differences	Cross-section	Difference-in differences	Cross-section	Difference-in differences	Cross-section	Difference-in differences	Cross-section
Estimates of Δ_d for persons with 1–4 months of training								
	−57	−231	−51	−127	−17	−189	−57	−67
	(75)	(48)	(32)	(21)	(93)	(59)	(48)	(32)
Estimates of Δ_d for persons with >4 months of training, having completed 1–4 months								
	−68	−273	−75	−189	−129	−180	−66	−111
	(59)	(46)	(25)	(17)	(64)	(46)	(29)	(22)
Estimates of Δ_d for persons with >4 months of training, having completed >4 months								
	−96	−302	−117	−231	−207	−259	−44	−89
	(86)	(66)	(32)	(24)	(77)	(62)	(38)	(29)
Estimates of Δ_a for persons with 1–4 months of training								
	280	105	111	36	169	−3	77	66
	(92)	(55)	(43)	(29)	(107)	(61)	(57)	(38)
Estimates of Δ_a for persons with >4 months of training								
	230	25	215	101	70	18	169	124
	(90)	(60)	(39)	(31)	(90)	(55)	(42)	(33)

The dependent variable in the OLS regression is pooled self-reported monthly earnings. The regression sample consists of all person-months for treatments at the sixteen experimental sites in the 33 months after random assignment (RA) with valid values for all variables. Regressors include indicators for training status, calendar month, month after RA, race, marital status, education, training center of RA, age, and English language preference. The excluded training status is never receiving training. Separate sets of training status indicators are used to estimate effects for training spells of one to four months or more than five months in duration. The top 1 percent of earnings values are dropped in each month. Mean earnings is the mean level of monthly earnings for members of the control group who reported earnings over the full 33-month period. Estimated standard errors appear in parentheses.

TABLE VI

NONEXPERIMENTAL ESTIMATES OF THE DISCOUNTED RETURNS TO TRAINING, R_1 FOR TREATMENT GROUP MEMBERS

Training length	Adult males			Adult females			Youth males			Youth females		
	33 months	5 years	10 years	33 months	5 years	10 years	33 months	5 years	10 years	33 months	5 years	10 years
Private returns based on before-after estimates												
3 months	7362	13349	24086	2801	5175	9433	4421	8044	14541	1789	3428	6367
	(2529)	(4490)	(8013)	(1194)	(2122)	(3787)	(2962)	(5257)	(9380)	(1572)	(2791)	(4980)
6 months	4964	9889	18720	4601	9202	17451	533	2032	4719	3572	7193	13685
	(2233)	(4158)	(7622)	(972)	(1811)	(3321)	(2216)	(4126)	(7562)	(1046)	(1949)	(3572)
Social returns based on before-after estimates												
3 months	2730	8717	19453	−1884	490	4749	−184	3440	9936	−2842	−1203	1736
	(2529)	(4490)	(8013)	(1194)	(2122)	(3787)	(2962)	(5257)	(9380)	(1572)	(2791)	(4980)
6 months	405	5330	14161	−33	4568	12817	−3974	−2476	211	−977	2644	9136
	(2233)	(4158)	(7622)	(972)	(1811)	(3321)	(2216)	(4126)	(7562)	(1046)	(1949)	(3572)
Private returns based on cross-section estimates												
3 months	2066	4326	8377	510	1272	2639	−804	−860	−959	1464	2874	5403
	(1503)	(2668)	(4761)	(802)	(1425)	(2544)	(1693)	(3004)	(5358)	(1048)	(1861)	(3321)
6 months	−1277	−746	206	1136	3298	7174	−1037	−644	62	2203	4860	9624
	(1496)	(2781)	(5094)	(766)	(1429)	(2620)	(1361)	(2526)	(4625)	(808)	(1506)	(2761)
Social returns based on cross-section estimates												
3 months	−2566	−307	3744	−4175	−3413	−2046	−5409	−5464	−5563	−3167	−1757	772
	(1503)	(2668)	(4761)	(802)	(1425)	(2544)	(1693)	(3004)	(5358)	(1048)	(1861)	(3321)
6 months	−5837	−5306	−4353	−3498	−1336	2540	−5544	−5151	−4446	−2346	311	5075
	(1496)	(2781)	(5094)	(766)	(1429)	(2620)	(1361)	(2526)	(4625)	(808)	(1506)	(2761)

Returns are the present discounted value of the training effect, discounted at $1/(1+r)$, where $r = 0.03$, and based on estimated monthly effects of training Δ_d and Δ_a. The duration of training is set at 3 or 6 months and the effects of training persist for either 33 months, 5 years, or 10 years. Private returns include the estimated monthly tuition payments. Social returns include the estimated marginal costs incurred by the training provider, adjusted upward by 1.5 to reflect the deadweight cost of taxation.

TABLE VII

NONEXPERIMENTAL ESTIMATES OF THE MONTHLY EFFECTS OF TRAINING Δ_d AND Δ_a for Control Group Members

	Adult males		Adult females		Youth males		Youth females	
Sample size	8696		20815		5447		10449	
	Difference-in-differences	Cross-section	Difference-in-differences	Cross-section	Difference-in-differences	Cross-section	Difference-in-differences	Cross-section
Estimates of Δ_d for persons with 1–4 months of training								
	−283	−215	−107	−149	−93	−66	−138	−227
	(111)	(116)	(50)	(41)	(75)	(98)	(49)	(44)
Estimates of Δ_d for persons with >4 months of training, having completed 1–4 months								
	−146	−372	−86	−98	−133	−256	−9	−172
	(95)	(64)	(44)	(36)	(101)	(83)	(34)	(38)
Estimates of Δ_d for persons with >4 months of training, having completed >4 months								
	−198	−425	−115	−127	10	−113	−91	−254
	(120)	(76)	(58)	(45)	(107)	(93)	(43)	(42)
Estimates of Δ_a for persons with 1–4 months of training								
	85	153	95	53	−3	25	47	−42
	(141)	(117)	(72)	(55)	(99)	(70)	(65)	(56)
Estimates of Δ_a for persons with >4 months of training								
	379	152	116	104	211	88	238	74
	(139)	(87)	(72)	(56)	(115)	(105)	(62)	(58)

The dependent variable in the OLS regression is pooled self-reported monthly earnings. The regression sample consists of all person-months for controls at the sixteen experimental sites in the 33 months after random assignment with valid values for all variables. Regressors include indicators for training status, calendar month, month after RA, race, marital status, education, training center of random assignment, age, and English language preference. The excluded training status is never receiving training. Separate sets of training status indicators are used to estimate effects for training spells of one to four months or more than five months in duration. The top 1 percent of earnings values are dropped in each month. Estimated standard errors are in parentheses.

TABLE VIII

NONEXPERIMENTAL ESTIMATES OF THE DISCOUNTED RETURNS TO TRAINING, R_1, CORRECTED FOR SELECTION

Adult males			Adult females			Youth males			Youth females		
33 months	5 years	10 years	33 months	5 years	10 years	33 months	5 years	10 years	33 months	5 years	10 years
I. Regression-adjusted on X											
1912	4226	8840	1191	3037	6719	647	1537	3313	1843	4163	8789
(387)	(890)	(1977)	(195)	(456)	(1016)	(426)	(991)	(2205)	(241)	(564)	(1257)
II. Regression-adjusted on X and Z											
2255	4964	10367	2250	5085	10740	1936	3897	7809	1186	3120	6977
(447)	(1026)	(2278)	(234)	(548)	(1220)	(573)	(1342)	(2991)	(341)	(800)	(1784)
III. Regression-adjusted on X and Z through propensity score											
1672	3901	8348	1527	3635	7840	1472	3081	6291	1917	4454	9514
(516)	(1111)	(2436)	(304)	(713)	(1589)	(926)	(1856)	(3999)	(286)	(661)	(1471)
IV. Instrumental-variables estimates											
8201	14836	28071	3816	9556	21005	−2407	−5214	−10812	10520	20358	39983
(2408)	(5489)	(12171)	(1425)	(3349)	(7469)	(2071)	(4883)	(10895)	(1944)	(4655)	(10414)
V. Heckman {1979} method											
17025	28568	51595	9343	20310	42185	1074	1331	1845	4466	9346	19079
(2709)	(6233)	(13846)	(1582)	(3714)	(8279)	(2118)	(4985)	(11119)	(1637)	(3828)	(8528)

Returns are the present discounted value of the estimated monthly effects of training, discounted at $1/(1 + r)$, where $r = 0.03$. The selection-corrected monthly effect of training for treatment group members is estimated through separate regressions for months 0 through 18 after random-assignment (RA). For succeeding months, the effect of training is taken to be the mean of the training effects for months 13–18. In order to isolate the effect of training completion, the sample is restricted to either those who receive no training or those who complete their training within 12 months of RA. The top 1 percent of earnings values in each month are excluded. The dependent variable Y in each regression is a person's self-reported earnings in that month. Exogenous regressors X in the earnings equation include indicators for race, marital status, education, site of random assignment, and age. T is a treatment indicator. $T = 1$ signifies that an individual has participated in training by the month of the regression. Participation-related regressors Z include month of random assignment, household size, indicators for progressively higher levels of total family income, and indicators for receipt of adult basic education, vocational training, and job search assistance at or before random assignment. The estimated training effect for Method I is the coefficient on T in an OLS regression of Y on X. For Method II, it is the coefficient on T in an OLS regression of Y on X and Z (see Heckman, Ichimura, Smith, and Todd {1998}). For Method III, it is the coefficient on T in an OLS regression of Y on X, P, and T, where P is the predicted probability of participation from a probit of T on Z. (See Heckman and Robb {1986} for discussion of this method.) For Method IV, the 2SLS model, it is the coefficient on T in an OLS regression of Y on X and P, where P is the predicted value from an OLS regression of T on X and Z. For Method V, the Heckman two-step model, it is the coefficient on T in an OLS regression of Y on X, M, and T, where M is the estimated inverse Mills ratio from a probit of T on Z. The estimated standard error of Method V does not incorporate the additional variance component resulting from the first-stage estimation. Estimated standard errors are in parentheses.

TABLE IX
ESTIMATED BOUNDS ON THE DISCOUNTED RETURNS TO JTPA TRAINING R_1

Adult males			Adult females			Youth males			Youth females		
33 months	5 years	10 years	33 months	5 years	10 years	33 months	5 years	10 years	33 months	5 years	10 years
Lower bound with (AS-7) imposed											
−61254	−113194	−216799	−48755	−94469	−185655	−52544	−104590	−208407	−40457	−79469	−157288
(363)	(688)	(1788)	(179)	(359)	(949)	(314)	(565)	(1440)	(167)	(300)	(764)
Lower bound with (AS-7) and (AS-8) imposed											
−44294	−81262	−155004	−37048	−73848	−147254	−41059	−84791	−172024	−33611	−68192	−137171
(698)	(1271)	(3256)	(296)	(572)	(1497)	(578)	(1002)	(2519)	(281)	(470)	(1162)
Lower bound with (AS-7), (AS-8), and (AS-9) imposed											
−2214	−942	1595	1133	3002	6729	−1772	−2496	−3940	−1129	96	2540
(800)	(1441)	(3678)	(373)	(707)	(1837)	(670)	(1156)	(2901)	(341)	(581)	(1449)
Upper bound with (AS-7) imposed											
21824	40864	78842	13007	25155	49388	17160	33047	64736	11208	22186	44084
(321)	(628)	(1648)	(144)	(286)	(753)	(274)	(505)	(1299)	(141)	(261)	(674)
Upper bound with (AS-7), (AS-8), and, optionally, (AS-9) imposed											
13015	29303	61795	8255	15924	31222	9609	19532	39325	4902	11916	25909
(698)	(1271)	(3256)	(296)	(572)	(1497)	(578)	(1002)	(2519)	(281)	(470)	(1162)

Estimates of bounds on R_1 are the present discounted value of the estimated monthly bounds on the effect of training less the average monthly cost of training, discounted at $1/(1 + r)$, where $r = 0.03$. Standard errors of R_1 are derived from the discounted sum of individual monthly variance estimates on the training effect bounds. Estimated standard errors are in parentheses.

Appendix 1: Demographic Characteristics of Trainees and Nontrainees

	Adult male trainees			Adult male nontrainees			Adult female trainees			Adult female nontrainees		
	Treat-ments	Con-trols	Prob $(t > \lvert T \rvert)$	Treat-ments	Con-trols	Prob $(t > \lvert T \rvert)$	Treat-ments	Con-trols	Prob $(t > \lvert T \rvert)$	Treat ments	Con-trols	Prob $(t > \lvert T \rvert)$
Sample size	363	89		381	236		952	272		745	544	
Percent black	33.1%	37.1%	0.48	42.8%	36.4%	0.12	27.0%	26.5%	0.86	40.4%	37.7%	0.32
Percent Hispanic	11.0%	10.1%	0.80	5.0%	11.4%	0.01	15.9%	17.3%	0.58	10.1%	11.0%	0.58
Percent with 12 years schooling	68.6%	77.5%	0.08	69.8%	67.4%	0.53	60.5%	63.2%	0.41	62.6%	59.0%	0.20
Percent with >12 years schooling	26.4%	25.8%	0.91	23.1%	22.5%	0.85	16.6%	18.8%	0.42	15.4%	14.3%	0.58
Percent employed at random assignment	20.7%	29.2%	0.11	16.3%	18.2%	0.54	18.1%	20.6%	0.36	16.0%	17.8%	0.38
Percent received AFDC at RA	14.3%	20.2%	0.21	16.8%	17.4%	0.85	63.4%	59.6%	0.25	57.3%	62.7%	0.05

	Male youth trainees			Male youth nontrainees			Female youth trainees			Female youth nontrainees		
	Treat-ments	Con-trols	Prob $(t > \lvert T \rvert)$	Treat-ments	Con-trols	Prob $(t > \lvert T \rvert)$	Treat-ments	Con-trols	Prob $(t > \lvert T \rvert)$	Treat-ments	Con-trols	Prob $(t > \lvert T \rvert)$
Sample size	210	60		167	114		430	141		304	211	
Percent black	22.4%	26.7%	0.51	27.5%	27.2%	0.95	24.2%	24.8%	0.88	28.3%	25.6%	0.50
Percent Hispanic	30.0%	21.7%	0.18	21.6%	24.6%	0.56	28.1%	25.5%	0.54	17.1%	24.6%	0.04
Percent with 12 years schooling	41.4%	41.7%	0.97	30.5%	40.4%	0.09	49.3%	56.7%	0.13	47.4%	45.0%	0.60
Percent with >12 years schooling	6.2%	11.7%	0.23	0.6%	1.8%	0.40	4.7%	9.2%	0.09	4.3%	2.4%	0.22
Percent employed at random assignment	18.6%	28.3%	0.13	18.6%	17.5%	0.83	20.7%	25.5%	0.25	16.4%	21.8%	0.13
Percent received AFDC at RA	16.2%	16.7%	0.93	13.8%	8.8%	0.19	45.1%	42.6%	0.60	38.2%	39.3%	0.79

The sample is rectangular and includes all persons from the sixteen experimental sites, recommended to receive classroom training, T-tests are of the null hypothesis that means of the treatment, and control samples are equal within demographic and training groups.