

1. Introduction

To an increasing degree, the material well being of the poor depend on the earnings and employment prospects of poor adults. Recent welfare reforms emphasizing self-sufficiency and benefits time limits coupled with the expansion of the Earned Income Tax Credit have increased the returns to employment for poor households, or conversely, the penalty for non-employment (Ellwood 2000). In addition, a high and growing proportion of poor men are former prison inmates (Raphael 2004) that are ineligible for many public benefits. In light of these trends, public training programs targeted towards the poor and other disadvantaged groups are by default, becoming an increasingly important component of U.S. anti-poverty policy.

Gauging the effectiveness of public training efforts is clearly important to local, state, and federal policy makers. Indeed, recent federal reforms to federal training efforts embodied in the Workforce Investment Act (WIA) place heavy emphasis on evaluation as an accountability tool. However, accurately estimating program effects is a difficult task, especially in the absence of experimental data. The key concern with past non-experimental research arises from the non-random selection of training participants into training programs. Systematic differences in application rates across observable and unobservable dimensions as well as non-random rationing by service providers are both factors that complicate the construction of non-experimental comparison groups. This selection bias coupled with the large variation in non-experimental program effect estimates have led many to downplay non-experimental methods as a viable set of evaluation tools (Barnow 1987, Friedlander et. al. 1997).

Despite this pessimism, recent research by Heckman et. al. (1997) demonstrates that under certain conditions non-experimental methods may be sufficient to address selection bias. Moreover, the authors show that selection bias is often small relative to the bias caused by other

factors, such as differences in the distribution of treatment and comparison samples across local labor markets and the use of different survey instruments to gauge outcomes. A key conclusion of this research is that constructing comparison samples from individuals that are administered the same surveys and who reside in similar local labor markets as program participants eliminate much of the bias to previous non-experimental research. Moreover, a rich set of observable characteristics coupled with standard probabilistic matching methods can substantially improve non-experimental methods.

In this paper, we examine the effectiveness of the Massachusetts workforce development system on the labor market outcomes of disadvantaged adults using JTPA non-experimental data from the late 1990s. We construct a comparison sample for program participants using individuals that completed an objective assessment of their eligibility, were deemed eligible and offered services, yet did not participate in a training program. Using these programmatic “no-shows” provides several benefits. First, in line with the prescriptions of Heckman et. al. (1997), the comparison group is well matched to the treatment group with respect to local labor markets. Moreover, members of both groups were administered similar surveys at the eligibility assessment and we have similar unemployment insurance records for members of both groups. In addition, mean earnings in the comparison sample prior to the date of program intervention exhibit the same pre-program earnings dip exhibited by members of the treatment sample, suggesting that our comparison group members experience similar employment and earning dynamics prior to the assessment of program eligibility.

We present a series of difference-in-difference estimates of the impact of the Massachusetts workforce development efforts on the earnings of low-income adults, and make several efforts to adjust for selectivity bias. We estimate standard econometric models that

regression-adjust for observable characteristics, fixed effect models that adjust for time-invariant person fixed effects, as well as simple probabilistic matching techniques that use the rich set of observable characteristics to more finely align the treatment and comparison samples. We find fairly large effects of the Massachusetts implementation of JTPA. On average, program participants experienced 20 percent increases in annual earnings roughly one year after receiving training and 25 percent increases after two years. We uncover considerable heterogeneity in these effects, suggesting that the most difficult to serve and the most job-ready benefit the least.

2. Alternative Non-Experimental Strategies for Constructing Comparison Groups

The principal methodological challenge faced by any non-experimental program evaluation is to define a group of non-participants against which the outcomes of program participants can be compared. The adequacy of such comparison groups is usually judged against the ideal of a control group of individuals that have been randomized out of a program. Heckman et. al. (1997) note that ideal social experiments are characterized by four qualities. First, the unobservable attributes of participants and controls are similar. Second, so are the observable attributes. Third, the data are collected in the same way for treatment and control group members. Finally, participant and control groups are exposed to similar economic environments. These four characteristics provide a set of baseline criteria for discussing the relative merits of alternative non-experimental strategies.

The training evaluation literature distinguishes between external comparison groups and internal comparison groups. External comparison groups are those drawn from non-participating eligible or otherwise comparable individuals having nothing to do with the program under study. Internal comparison groups consist of all non-participants in a program's applicant pool or some

subset thereof (Bell et. al. 1995). Using external comparison groups may avoid selection bias due to differences in motivation, or the selective rationing by service providers. Moreover, comparison samples are usually drawn in a manner that minimizes observable differences between the treatment and comparisons samples, and perhaps unobserved differences as well (see, for example, Ashenfelter 1978, Ashenfelter and Card 1985, Barnow 1987, and Heckman, Smith, and Taber 1994).

However, external comparison groups often require using data from national household surveys, such as the Current Population Survey (CPS) or Survey of Income and Program Participation (SIPP). It is quite difficult to match comparison samples to program participants by local economic conditions with such survey data. In addition, using external data sources results in outcome measures that are not collected in a uniform manner. Both factors have been shown to introduce bias to non-experimental evaluations above and beyond the bias resulting from selective difference between participant and comparison group members (Heckman et. al. 1999).

The benefits of using an internal comparison group include the fact that non-participating applicants and participants go through the same intake process, and thus, data for both groups come from a single source. In addition, by definition the two groups are exposed to similar economic conditions. A further benefit particular to training evaluations concerns the fact that non-participating applicants often experience comparable pre-program earnings and employment dynamics. The well-documented pre-program dip in earnings poses particular problems for the evaluation of training programs. To the extent that the decline in earnings is transitory, pre-post changes in earnings observed for participants may likely reflect recovery from a transitory shock. If a bounce-back would occur regardless, the effect of the training intervention will be over-estimated unless the comparison group exhibits similar earnings paths (Bell et. al. 1995,

Heckman and Smith 1999). Early examples of research using internal comparison groups include Borus (1964), Cain (1968), Stromsdorfer (1968), and Cooley et. al. (1979).

The principal disadvantage of internal comparison groups arises from the many possible stages between application and training receipt where applicants either selectively withdraw or are selectively screened out of the final pool. Those who withdraw may be less motivated relative to applicants who follow through. Alternatively, to the extent that applying for training services is akin to a form of job search, withdrawals may be disproportionately comprised of those who recover quickly from a transitory earnings shock. Both possibilities are likely to introduce unobservable differences between the comparison and treatment groups.

Applicants may either be systematically or randomly screened out of the pool of those offered services. An example of the former would be if service providers cream skim in an attempt to manipulate external measures of program efficacy, while examples of the latter include random errors in the eligibility assessment process or random problems inhibiting the communication of eligibility. Random screening out does not pose a problem to an internal comparison sample, and in fact, would justify the use of such a design. Cream-skimming and other forms of systematic screening, however, create selectivity-bias and compromise the validity of the internal comparison group.

Bell et. al. (1995) assess the adequacy of internal comparison groups by comparing several alternative non-experimental program effect estimates to estimates based on a randomized control group using the AFDC Homemaker-Home Health Aide Demonstrations.¹ The authors evaluate three non-experimental comparison groups: (1) all non-participating

¹ This program included seven state-run efforts that provided AFDC recipients with four to six weeks of training and up to a year of subsidized employment as homemakers and home health aids. The basic demonstration and program evaluation was based on an experimental design, yet various internal control groups were collaterally generated along the way. The experimental evaluation is summarized in Bell and Orr (1994).

applicants, (2) applicants that were screened out by service providers, and (3) applicants that were offered services yet did not participate in the program (referred to as no-shows). With the exception of applicants that were screened out², the estimation methodology consisted of basic post-program differences in means, both unadjusted and adjusted for a limited set of observable covariates. In general, the non-experimental program effect estimates are biased upwards relative to the experimental estimates. However, this bias is smallest for the non-experimental estimate based on no-shows. Moreover, regression adjusting for a small number of observable characteristics eliminates a fair portion of the difference.

Assessments of the adequacy of both external and internal comparison samples are presented in Heckman et. al. (1997). Using data from the national experiments mandated by the Job Training Partnership Act (JTPA), the authors present a detailed decomposition of the bias to several alternative non-experimental strategies by comparing the mean post-program earnings of the randomly selected control group to various non-experimental comparison groups. The comparison samples studied include the sample of eligible non-participants included in the JTPA research design, a comparison of eligibles taken from the SIPP, and a comparison group generated from program no-shows. The study demonstrates that for adults, the overall earning bias for no-shows is the smallest, relative to that for eligible non-participants and SIPP eligibles.³ Moreover, the authors demonstrate that with a sufficiently rich set of observable characteristics much of the remaining selection bias can be eliminated through matching on the probability of participating. A chief conclusion of this research is that uniformity of survey instruments and

² For models where those that are screened out are used as a comparison sample, the authors generate regression-discontinuity estimates of the program effect using the relationship between post-program earnings and the service provider's index assessments of the likelihood that the individual would benefit for non-recipients to generate the counterfactual post-program earnings for participants.

³ The study does find that the proportion of the bias accounted for by selectivity bias is high for the no-shows. However, the bias is small relative to estimated program effects.

choosing comparison samples subject to similar local economic conditions vastly increases the quality of non-experimental estimates.

Based on the findings of this research, we pursue a non-experimental evaluation strategy based on programmatic no-shows. We now turn to our identification strategy.

3. Methodological Approach

Here we present our empirical strategy for estimating the effects of JTPA training services in the state of Massachusetts during the late 1990s.⁴ Our identification strategy is three-pronged. First, we identify individuals that completed an objective assessment of their eligibility for workforce development services, were deemed eligible and offered services, yet did not participate in a training program, as the principal comparison group. Second, we estimate a series of random and fixed effects earnings models that adjust for all observable baseline differences and the impact of all time-invariant characteristics on earnings. These models are used to estimate the before-after change in the earnings differential between our treatment and comparison samples (our difference-in-difference estimator). Finally, we use probabilistic matching to more finely align program participants to members of the chosen comparison sample and to explore heterogeneity in the effect of the program.

A. Treatment and Control Groups and the Dimensions of the Panel Data Set

The group of program participants includes over seven thousand individuals who participate in a Massachusetts workforce development program in the late 1990s and the year

⁴ JTPA Title IIA funds training programs for economically disadvantaged adults. This group includes individuals or members of a family that receive welfare benefit or have a total family income below the official poverty line or 70% of the lower living standard income level, as well as individuals receiving food stamps, or qualified as homeless. In addition, 65% of JTPA participants had to be “hard-to-serve” as defined by any of the following: High school drop out, offender or ex-offender, disabled, homeless, basic skills deficient (below 9th grade equivalent in reading or math tests), or a local category defined by the local Regional Investment Board.

2000. We refer to members of this group as program exits. The comparison group includes roughly five thousand individuals who were offered training yet did not participate in an activity. We refer to members of this group as objective-assessment-only (OAO) individuals. Given the fact that training start dates for our treatment group can occur any time over a period of several years, we must align the treatment and comparison groups around a hypothetical intervention date. Here we discuss how we approach this problem.

We arrange the data into a person-quarter panel data set, where the time dimension is defined relative to the quarter in which the objective assessment takes place. For program exits, the date of the objective assessment corresponds to the begin date of training. For the OAO group there is no begin date for training yet a date for an objective assessment. Hence, the objective assessment provides a natural time period along which to identify the hypothetical point of intervention that would have occurred for OAO individuals had they followed through. We observe quarterly earnings and employment information for eight quarters prior to the objective assessment quarter, the objective assessment quarter, and the eleven following quarters (twenty quarters of data in all). Since nearly all of the individuals in the exits sample complete their training activities within four quarters (inclusive of the quarter of objective assessment), we have at least two years of post-intervention earnings, as well as two years of pre-intervention earnings.

The individual dimension of the panel data is indexed by $i = (1, \dots, I)$ while the time dimension is indexed by $j = (-8, \dots, 11)$, with quarter zero being the quarter of objective assessment and/or beginning of training activities. Since the time dimension is defined relative to the assessment date, j may occur in different calendar quarters for any two individuals.⁵

⁵ For this reason, we use the index j rather than t to describe the temporal dimension of the panel. The index t is used below to label the time fixed effects.

We construct dummy variables for each quarter relative to the objective assessment quarter. Specifically, the dummy variables Q_{ij}^{-7} through Q_{ij}^{11} are defined by the set of equations

$$(1) \quad Q_{ij}^k = \begin{cases} 1, & \text{where } j = k \\ 0, & \text{otherwise} \end{cases} \text{ for } k = (-7, \dots, 11),$$

In addition, we also define the dummy variable PE_{it} as equal to one if the observation corresponds to a program exit and zero if the observation is for an OAO individual.

B. Regression Specifications and Estimating the Program Effect

Our earnings effect estimates are based on the pre-post intervention change in the average earnings differential between the program exits and OAO samples. These difference-in-difference estimators come from a series of regression models that estimate the quarter-by-quarter earnings differentials after adjusting for observable covariates as well as for time and person fixed effects. Before discussing the specification details of the regression models, we will first lay out the underlying base regression and our summary measure of the program's impact.

Define W_{ij} as quarterly earnings for person i in quarter j . Average differences in quarterly earnings for each quarter of the panel between program exits and OAO observations can be summarized with the simple regression model

$$(2) \quad W_{ij} = \alpha_{-8} + \sum_{k=-7}^{11} \alpha_k Q_{ij}^k + \beta_{-8} PE_{ij} + \sum_{k=-7}^{11} \beta_k PE_{ij} Q_{ij}^k + \varepsilon_i + \nu_{ij},$$

where Q_{ij}^k and PE_{ij} are the dummy variables for quarter and program exits, α_{-8} through α_{11} and β_{-8} through β_{11} are parameters to be estimated, ε_i is a person-specific error component, and ν_{ij} is a mean zero, independently and identically distributed variance component. For the OAO sample, the expected value of quarterly earnings in any given quarter is given by the equations

$$(3) \quad E(W_{ij} | Q_{ij}^k = 1, PE_{ij} = 0) = \left\{ \begin{array}{l} \alpha_{-8}, \text{ for } k = -8 \\ \alpha_{-8} + \alpha_k, \text{ for } k = (-7, \dots, 11) \end{array} \right\},$$

while the comparable set of conditional expectations for program exits is given by

$$(4) \quad E(W_{ij} | Q_{ij}^k = 1, PE_{ij} = 1) = \left\{ \begin{array}{l} \alpha_{-8} + \beta_{-8}, \text{ for } k = -8 \\ \alpha_{-8} + \beta_{-8} + \alpha_k + \beta_k, \text{ for } k = (-7, \dots, 11) \end{array} \right\}.$$

Finally, quarterly earning differentials (exits minus OAO) are given by the equations

$$(5) \quad E(W_{ij} | Q_{ij}^k = 1, PE_{ij} = 1) - E(W_{ij} | Q_{ij}^k = 1, PE_{ij} = 0) = \left\{ \begin{array}{l} \beta_{-8}, \text{ for } k = -8 \\ \beta_{-8} + \beta_k, \text{ for } k = (-7, \dots, 11) \end{array} \right\}.$$

Our principal test for an impact of the training program on earnings is a test of whether the difference in earnings between the exits and the OAO groups increases after the training intervention. For a number of reasons, the estimated changes in relative earnings will be sensitive to the choice of base period as well as the post-assessment time period. For one, any pre-program earnings dip may impact our estimates. In addition, to the extent that there are outlier quarters, the estimates will be sensitive to chosen time period.

To avoid the effects of a dip and recovery in earnings, we choose a base period that occurs sufficiently prior to the date of program intervention. To ensure, that our difference-in-difference estimate of the program effect is not driven by outlier quarters, we base the program effect estimates on the change in relative annual earnings observed for the two samples. We also use multiple post-intervention time periods to assess how the program effect estimates change over time.

To explicitly illustrate the difference-in-difference estimator, here we lay out our estimate of the training effect occurring at least two years post-intervention. This estimate is calculated

by subtracting the annual earnings difference for the first four quarters of the panel ($j=-8, \dots, -5$) from the difference in annual earnings for the last four quarters of the panel ($j=8, \dots, 11$). Specifically, define the variables $W_{i,j-j+4}$ as the annual earning of person i during the four quarters of j through $j+4$. Using the parameters of the regression model specified above, the difference in the expected value of earnings two years pre-intervention is given by the equation

$$(6) \quad E(W_{i,-8--5} | PE_{ij} = 1) - E(W_{i,-8--5} | PE_{ij} = 0) = 4\beta_{-8} + \beta_{-7} + \beta_{-6} + \beta_{-5},$$

while the difference in the expected value two years post-intervention is

$$(7) \quad E(W_{i,8-11} | PE_{ij} = 1) - E(W_{i,8-11} | PE_{ij} = 0) = 4\beta_{-8} + \beta_8 + \beta_9 + \beta_{10} + \beta_{11}.$$

Our summary difference-in-difference estimate of the program effect is computed by subtracting equation (6) from equation (7) to get

$$(8) \quad DD = \beta_8 + \beta_9 + \beta_{10} + \beta_{11} - \beta_{-7} - \beta_{-6} - \beta_{-5},$$

which is a simple linear function of a subset of the parameters that are estimated with equation (2). We calculate the difference-in-difference estimator in equation (8) for various sample and model specifications as well as various post-intervention time periods.

The regression specification in equation (2) yields estimates of the differences in quarterly earnings that do not account for observable differences in potentially relevant background characteristics. Indeed, we do have considerable information on demographic and human capital characteristics. Moreover, since we have repeated observations on individuals, we can exploit the panel aspects of the data set to further adjust the program effect estimate given by equation (8) for interpersonal differences in earnings.

To do so, we estimate two additional specifications of the underlying regression model. The first specification change adds observable covariates and time fixed effects. Specifically, we estimate the expanded model

$$(9) \quad W_{ij} = \alpha_{-8} + \sum_{k=-7}^{11} \alpha_k Q_{ij}^k + \beta_{-8} PE_{ij} + \sum_{k=-7}^{11} \beta_k PE_{ij} Q_{ij}^k + \gamma' X_{ij} + \lambda_t + \varepsilon_i + v_{ij},$$

where X_{ij} is a vector of observable control variables, γ is a vector of corresponding parameters, λ_t are time-specific fixed effects, and all other components are as defined above. Adding time fixed effects purges the data of common temporal factors such as inflation or the state unemployment rate.⁶ Below, we estimate equation (9) using a random-effects regression model.

The second specification change replaces the intercept for the first quarter of the panel with person-specific fixed effects. The fixed effects model is given by the equation

$$(10) \quad W_{ij} = \alpha_i + \sum_{k=-7}^{11} \alpha_k Q_{ij}^k + \sum_{k=-7}^{11} \beta_k PE_{ij} Q_{ij}^k + \gamma' Z_{ij} + \lambda_t + v_{ij},$$

where α_i are person-specific intercepts, Z_{ij} is a vector of time-varying control variables (all time invariant factors are removed from the specification), and all else is as defined above. With the inclusion of fixed effects, a separate base effect for program exits is unidentifiable, due to the fact that the program exits dummy variable can be written as a linear combination of the person effects. Hence, this term is dropped from the specification.⁷ In addition, the random person-specific error components (ε_i in equations (2) and (8)) are dropped.

The fixed effects model identifies the quarterly earnings coefficients relying on within-person variation in quarterly earnings and the average difference in this variation between the treatment and comparison groups. Thus, including the fixed effects in equation (10) provides a more stringent test for a difference-in-difference program effect than that calculated from the regression specifications.

⁶ Note, since the time dimension of the panel is defined relative to the quarter of intervention (which varies across individuals) the time index t is different from the panel index j .

⁷ Calculating a base difference in earnings in the first quarter requires calculating the means of the fixed effects conditional on being a member of program exits sample or being a member of the OAO sample.

Below, we present estimates of quarterly earnings effects using the regression specifications outlined in equations (2), (9) and (10). These specifications provide alternative estimates of the parameters needed to calculate the difference-in-difference in equation (8). Thus, with these three alternative specifications, we are able to explore the robustness of our program effect estimates to different background controls.

C. Probabilistic matching of the exits and OAO samples: refining the comparisons

The specifications of the regressions in equations (9) and (10) are designed to adjust the program effect estimates for observed differences in earnings potential, as well as unobserved yet time-invariant differences. Such adjustments are crucial to the analysis since the program non-shows are likely to differ along observable and unobservable dimensions from program participants. Nonetheless, the regression models outlined thus far are unlikely to completely address the issue of selection bias. Moreover, these models do not permit one to investigate whether there is heterogeneity in the effect of the program on potential program participants.

Here, we outline a probabilistic matching strategy that provides a finer match between the non-experimental treatment and comparisons groups. The procedure relies on first modeling the factors that influence the likelihood that an individual who completes an objective assessment and is offered training actually participates in a training activity. Based on this model, it then estimates the predicted likelihood of participating for all exits and OAO observation. This predicted likelihood is then used to segment the samples into sub-samples with separate program effects estimated for each sub-sample.⁸

⁸ The benefits of this procedure are several. Segmenting the analysis sample such that the between-group differences in observable characteristics are similar is likely to reduce the difference in unobservable factors. In addition, this procedure permits exploration of heterogeneous program effects. Finally, estimating separate models for sub-samples enriches the specifications of the regression models in equations (2), (9), and (10), as the coefficients on background variables are permitted to vary across groups.

To outline the matching procedure, note that we are explicitly modeling the likelihood that the variable PE_{ij} equals one as a function of observable demographic and socioeconomic characteristics. Suppose that the probability of becoming a program exit is given by the equation

$$(11) \quad P(PE_i = 1 | X_i) = F(\beta' X_i)$$

where β is a column vector of regression coefficients to be estimated and X_i is a column vector containing the values for observable characteristics for individual i .⁹ We assume that PE_i is a linear function of the explanatory variables and thus estimate equation (11) with a linear probability model.¹⁰

The parameters from estimating equation (11) can be used to predict the likelihood that each individual in the OAO and the program exits group continues beyond the objective assessment. For individual i this predicted value is given by

$$(12) \quad \hat{P}_i = F(\hat{\beta}' X_i)$$

where the symbol, $\hat{\cdot}$, above a variable indicates an estimated value.

We use the predicted probabilities, \hat{P}_i , to segment the pooled sample of OAO and program exit observations into sub-samples with similar likelihood of continuing beyond the initial assessment. Specifically, we segment the sample into quintiles by the predicted probability of continuing past objective assessment and estimate separate program effects for each sub-group.

⁹ Note, we have dropped the subscript j since either the observation is a program exit or not.

¹⁰ We have estimated this equation using probit, logit and linear probability regression models. The results in terms of the predicted probability of participating and the quality of the final match were nearly identical.

4. Description of the Data and Comparison of the Treatment and Comparison Samples

The data for this project was provided by the Commonwealth Corporation of the state of Massachusetts. We were provided with over seven thousand records for individuals that participated in a Massachusetts workforce development activity between the first quarter of 1995 and the second quarter of 2000. We were also provided with over five thousand records for individuals who completed an objective assessment with a service provider yet did not follow through with a training activity. Program exits records cover all individuals participating in a JTPA workforce development program in the state for this time period. The OAO records cover all such individuals with the exception of those OAO individuals who completed the initial assessment with a provider located within the city of Boston.

In addition to the administrative records on program participants, which include data on activity start dates, type of activity, and all information collected during the objective assessment process, we were also provided with unemployment insurance quarterly earnings records for each individual. Earnings data were provided for the period covered by the first quarter of 1995 through the first quarter of 2002. For person quarters with no earnings, this variable is set to zero. These data provide our principal dependent variable.¹¹

We impose two time restrictions on the final sample. First, we drop all observations that do not have eight pre-intervention quarters of earnings data. This restriction amounts to limiting the sample to observations with objective assessment dates occurring no earlier than the first quarter of 1997. This restriction eliminates roughly four percent of the pooled sample.

¹¹ Kornfield and Bloom (1999) analyze the difference between program effects estimated with UI quarterly earnings data and effects estimated with survey data. In general, average earnings from UI records tend to be somewhat lower than average earnings from respondent surveys. However, proportional effect sizes are similar. Thus, UI records present a reliable metric of post-program outcomes.

Second, we restrict the sample to those observations with at least twelve quarters of post-objective assessment earnings records (inclusive of the quarter of the assessment). The purpose of this restriction is to ensure that we have a sufficient observation period post-assessment to measure an impact of the program. This additional sample restriction ensures that we observe roughly two years of post-activity earnings records for 90 percent of the sample, and roughly one year of post activity earnings records for nearly the entire sample.¹² This restriction limits the analysis sample to observations with objective assessment dates occurring no later than the second quarter of 1999, eliminating roughly 19 percent of the records from the pooled file. The final sample includes 4,025 OAO observations and 5,586 program-exits observations.

Table 1 presents average values for several demographic, basic skills and education, and socioeconomic characteristics for the two groups before imposing the time restrictions. The first column of figures presents averages for exits, the second column presents means for the OAO group, while the final column presents the difference in means between the two groups. Even before matching the two samples are quite similar. Both are overwhelmingly female, disproportionately minority, have low levels of educational attainment as well as reading and math skills, are quite likely to receive public assistance, and are very likely to be single parents.

Nonetheless, there are some notable differences. Relative to the OAO sample, program exits are slightly more educated. In addition, exits have higher test scores for reading and math skills, are less likely to be TANF recipients, and are more likely to have recent work experience.

Appendix Tables A1 and A2 present similar tabulations for the five probabilistically matched sub-samples while Table 2 summarizes the differences within sub-samples. The predicted probabilities are based on a linear regression of the program exits dummy on all of the

¹² Nearly 90 percent of exit complete their training activity within four quarters, while 99 percent do so within seven quarters.

control variables listed in Table 1 and a complete set of interaction terms between these variables and a female dummy variables. While the matches are considerably better for the middle quintiles, and perhaps the worst for the bottom quintile, matching considerably narrows the observed differences between the two groups. For example, the inter-group differences in formal education, math and reading test scores, the proportion that are TANF recipients, as well as the proportion with poor work histories are considerably smaller within the sub-samples compared with the unmatched differences presented in Table 1.

An alternative manner to illustrate the impact of probabilistic matching on the similarity (or lack thereof) between the treatment and comparison groups is to compare the average values of the outcome variable for the period prior to the program intervention. Ideally, one would like to observe pre-intervention values of the outcome variables that are (1) similar in magnitude for the treatment and comparison groups, and (2) move in similar paths over time.¹³

Figures 1 through 8 present such comparisons. Figure 1 presents average quarterly earnings for the eight pre-intervention quarters and 12 post intervention quarters for all exits and all OAO individuals. Figures 2 and 3 present similar presentations for men and women, respectively. Figures 4 through 8 present the same comparisons for the five probabilistically matched quintiles. Beginning with Figure 1, there are notable differences in average quarterly earnings during the pre-intervention quarters (quarters -8 through -1). Quarterly earnings for the exits sample are roughly four hundred dollars greater on average than quarterly earnings for the OAO sample prior to the date of intervention. Similar pre-intervention differences are observed for men and women in Figures 2 and 3. Note, in all three figures there are pre-program earnings

¹³ See Card and Krueger (1995), and Meyer (1995) for thorough discussions of the quality of comparisons groups in non-experimental research designs.

dips for both the exits and OAO groups, with subsequent bounces in earning post-objective assessment.

For the matched sub-samples, the pre-intervention earnings for exits and OAO participants are quite similar. For the bottom three quintiles (Figures 4 through 6), average quarterly earnings are visually indistinguishable from one another. The match performs the poorest for the top quintile, where pre-intervention differences in earning are on the order of the average differences observed for the pooled samples depicted in Figure 1. Thus, matching on observable characteristics, in addition to narrowing observable differences, considerably narrows pre-intervention differences in quarterly earnings.

5. Main Empirical Results

We begin with a series of unadjusted estimates of the effect of program participation on the relative annual earnings of the exits group. Tables 3 and 4 present average annual earnings for the program exits and OAO groups for the five years covered by the panel data set. In both tables, the first two columns present average earnings for the two pre-program years while the following three programs present average earnings for the three post-objective assessment years. The final three columns present the within-group changes in earnings relative to the first year of the panel for years three, four, and five respectively. Included in these tabulations are the change in relative earnings (exits minus OAO), or the unadjusted difference-in-difference program effects. Table 3 presents tabulations for the entire sample and by gender, while Table 4 present tabulations by the predicted probability quintiles.

As was evident in Figures 1 through 8, average annual earnings dip in the year just prior to assessment for both exits and OAO members for all participants and for all of the sub-samples

analyzed. In addition, post-assessment earnings bounce back for both our treatment and comparison samples for all groups. During the first post-program year (year three), the relative earnings of training participants either decrease or increase by an insignificant amount relative to the earning differential two-years before the objective assessment. However, in years four and five, the relative earnings differential widens considerably. For the entire sample, the unadjusted difference-in-difference estimate indicates that program participation increased average earnings by \$1,833 in year four and \$2,282 in year five, amounting to a 20 and 25 percent increase in annual earning (respectively) above what earnings would have been in the absence of receiving the training services.¹⁴ For men, the dollar value of the program impact in year five is larger as is the proportional effect (\$2,829 and 29 percent respectively). For women, the estimated program effects are basically in line with those for the sample overall. All of the difference-in-differences estimates in years four and five in Table 3 are statistically significant at the one percent level of confidence.

Table 4 reveals considerable heterogeneity across the five probability quintiles. The smallest absolute and proportional effects occur for the least and most likely to participate in a training program. For the first quintile, program participation increases annual earnings by approximately \$1,300 or 20 percent. The comparable figures for the top quintile are \$1,600 and 14 percent. For quintiles two and three (the sub-groups with perhaps the best matches) program participation increases annual earnings in year five by over 35 percent, while for quintile four the comparable figure is 25 percent. Again, all of the difference-in-difference estimates in years four and five are statistically significant at the one percent level.

¹⁴ The 25 percent figure comes from subtracting the \$2,282 difference-in-difference estimate from the average annual earnings of \$11,321 for the exits sample for year five to arrive at a counterfactual average annual earnings. The difference-in-difference estimate amounts to 25 percent of this counterfactual.

Figures 9 through 16 present graphical summaries of the relevant regression output from estimating the models discussed above. Each figure graphs the estimated average difference in quarterly earnings between the exits and OAO groups (equation (5) above) for each quarter of the panel using the three alternative regression specifications. The “no controls” earnings differentials are estimated using equations (2).¹⁵ The “time effects and personal characteristics” earnings differentials are estimated using the specification in equation (9) where the control variables include all of the variables listed in Table 1, controls for age and age squared, and a complete set of dummy variables for calendar quarters. Finally, the “time effects, fixed effects, and age” earnings differentials are estimated using equation (10) which includes a complete set of person fixed effects, calendar quarter fixed effects, and controls for age and age-squared. Recall, equations (2) and (9) are estimated with a person random effects model while equation (10) is, of course, estimated with a person fixed effects model.¹⁶

The estimation results using the entire sample (Figure 9) show a clear pre-post intervention increase in the earnings difference between the exits and OAO groups. The unadjusted results indicate that the average quarterly earnings differential increases from roughly \$400 pre-intervention to over \$800 in the latter post-intervention quarters, a relative increase of over \$400 per quarter.¹⁷ Adjusting for observable characteristics and time effects reduces the estimates of the quarterly earnings differentials for all quarters, but does not impact the relative change in quarterly earnings. Specifically, once time effects and personal characteristics are accounted for, the pre-intervention earnings differential between the two samples drops from

¹⁵ The parameters from this specification can be used to generate the figures presented in Tables 3 and 4.

¹⁶ Again, the earnings differential between exits and OAO group members for quarter –8 are estimated by calculating the average fixed effect for each group and taking the difference.

¹⁷ These calculations refer to the average quarterly earnings differentials in quarters –8 through –5 and quarters 8 through 11. Similar time period apply to the discussion of the results by gender and by predicted probability quintile.

roughly \$400 per quarter to roughly \$100 per quarter. Similarly, post-intervention estimates of the earnings differential decline from over \$800 to slightly over \$600.

Finally, adjusting for fixed person effects has basically no impact on the estimated relative change in earnings. The graph of quarterly earnings differentials using the fixed effects model corresponds quite closely with the base estimates using no control variables.¹⁸ Note, since the fixed effects model discards all inter-personal variation in estimating the changes in quarterly earnings differentials, the concordance with the unadjusted earnings differentials indicates that the observed program effect is quite robust. Similar patterns are observed when the data are analyzed separately by gender.

Figures 12 through 16 present similar summaries of the regression results for the five matched sub-samples. In all of the figures, the basic patterns are the same: earnings differentials increase post-intervention relative to the pre-intervention benchmark and the relative increases are comparable across model specifications. The heterogeneity of the program effect estimates observed in Table 4 is evident in the figures and survives adjusting for observable covariates and person-specific fixed effects. Roughly speaking, the pre-post intervention increase in the difference in average quarterly earnings between exits and OAO samples is \$300 for the bottom quintile, \$600 for the second quintile, \$700 for the middle quintile, \$600 for the fourth quintile, and \$450 for the top quintile. Thus in dollar terms, the program has the smallest impact on those individuals who are either the most likely or least likely to continue past the objective

¹⁸ Note, the concordance between the estimated earnings differential from the fixed effects and no controls models for the first quarter is to be expected, even if there were no measurable program effect. Since the difference in earnings for quarter -8 using the fixed effects model is calculated by subtracting the average of the fixed effects for the OAO sample from the average of the fixed effects for the exits sample, the differential for the first quarter should be quite close to the unobserved differential. However, subsequent earnings differential estimates (which are based on data that is now purged of inter-personal variation in earnings) need not resemble those estimated from the unadjusted cross sections.

assessment, while the program effects are largest for those in the middle of this predicted probability distribution.

Table 5 summarizes the effect estimates in a manner similar to the unadjusted results presented in last three columns of Tables 3 and 4. For the entire sample, the sub-samples stratified by gender, and the sub-samples stratified by the predicted probability quintiles, the table presents the difference-in-difference estimates resulting from the three model specifications for years three, four, and five. In all comparisons, the difference-in-difference estimates are calculated relative to year one (two years prior to objective assessment) in order to avoid problems associated with the pre-program dip in earnings. Perhaps the most notable pattern in Table 5 concerns the uniformity of the program effect estimates across specifications. Adjusting for observables and adjusting for fixed effects have negligible impacts on the effect estimates in all comparisons. Thus, the basic unadjusted results presented in Tables 3 and 4 survive.

To summarize, program participation had sizable and statistically significant impacts on the quarterly and average earnings of program participants. These effects are somewhat larger for men than women, yet are sizable and significant for both. In addition, we observe heterogeneity in the program effects by the predicted likelihood of participating beyond the objective assessment. We observe the smallest training effects for those participants with the lowest and highest likelihood of participating beyond the objective assessment and the largest effects for those with predicted probabilities in the center of the distribution. Nonetheless, we find significant and substantial effects for all participants.

6. Probing Robustness to Changes in Sample Specification

Here we assess whether certain idiosyncratic aspects of our main analysis sample are driving the strong earnings results that we have presented thus far. In particular, we explore the effects of imposing the following four additional sample restrictions:

- *Dropping observations for individuals over 55 years of age:* While the average member of both the exits and OAO samples is fairly young (around 30), there are several observations in both samples that one might consider to be beyond the age of retirement. To the extent that there are systematic differences across the treatment and comparison groups in the proportion of observations in this age range, program effect estimates may be biased.
- *Dropping program exits individuals who have multiple treatment periods:* A small number (under 200) of program exits in our analysis sample participated in a workforce development activity prior to the objective assessment and training activities that we analyze. If those participating in multiple service periods are less job-ready and more difficult to employ, excluding them from the analysis is likely to increase the estimated earnings effect. On the other hand, given the fact that earnings are low while in training and the likelihood that the unobserved training activity occurred during the eight pre-intervention quarters that we use as our benchmark, inclusion of these observations may impart an upward bias to the program effects.
- *Dropping program exits from the Boston Area:* A key component of our identification strategy involves using individuals who complete an objective assessment but who do not follow through with a workforce development activity as a comparison group. Unfortunately, in the Boston area records on those who did not follow through were not kept. Given the findings of past research concerning the importance of matching on local labor market conditions, assessing the effect of dropping the Boston program exits from the analysis is clearly merited.
- *Dropping OAO observations for individuals that received an alternative service:* Roughly twenty percent of the OAO sample received an alternative training or job-readiness service outside of the state workforce development program.¹⁹ Thus the earning paths for the OAO sample are in part influenced by training activities and thus do not chart the hypothetical course of earnings that would have occurred in the absence of workforce development services. To the extent that these alternative treatments have an

¹⁹ Individuals in the OAO group could have participated in some other program such as basic education and skills training programs funded under local contract with the Massachusetts Department of Transitional Assistance (DTA) for recipients of Temporary Assistance to Needy families (TANF) or Aid to Families with Dependent Children (AFDC). 1,697 of the 5,395 Objective Assessment only group are known to have participated in one of these DTA funded programs.

effect on earnings, the empirical earnings trajectories of the OAO sample should be higher than they otherwise would have been in the absence of a service.

Table 6 presents adjusted and unadjusted difference-in-difference summary estimates (for year five only) of the program effects when imposing each of these four alternative sample restrictions. For comparison, the first column of figures reproduces the summary estimates from the final column of Table 5. The second column presents the results when we drop the observations for individuals over 55. This is followed by estimation results dropping multiple service observations (column 3), results dropping program exits observations from the Boston area (column 4), and results dropping OAO observations for individuals that received an alternative training service (column 5). Paralleling the results from Table 5, we present the estimation results using the entire sample, the sample stratified by gender, and the sample stratified by the predicted probability quintiles using the three model specifications. In our discussion, we focus on the results for the entire sample, since the results by gender and by probability quintile are comparable.

While the alternative sample specifications do alter the point estimates of the earnings effects to a modest degree, in all instances the earnings effects are still large and statistically significant. Dropping observations over 55 and dropping Boston program exits lead to marginal declines in the estimated earnings effects while dropping individuals with multiple treatment periods causes marginal increases.²⁰ Dropping OAO observations that receive an alternative service causes a fairly substantial increase in the estimated effects of participating in a workforce

²⁰ Dropping the Boston sample from the treatment group has small effects on overall program impact estimates partly because there are no differences in the means of control variables for the treatment sample with or without the Boston residents.

development activity. This specification change increases the estimated earnings effects by approximately \$1,100 for the entire sample, by \$500 for men, and by roughly \$1,200 for women.

To summarize, except for the sizable change associated with dropping OAO observations receiving alternative treatments, these additional sample specifications have negligible effects on the estimate program impacts. In all instances, the earnings effects are large and statistically significant. Hence, the program effect estimates are quite robust to changes in sample specification.

7. Conclusion

This paper estimates the earnings effects of participating in a JTPA-funded Massachusetts workforce development program for disadvantaged adults. The results indicate uniformly positive, statistically significant, and sizable earning effects. Our estimates are robust to alternative model specifications and to matching comparison and treatment group members on the predicted likelihood of participating.

Our estimates are somewhat larger than prior estimates from other JTPA program evaluations. The National JTPA experiments found earnings effects of approximately 10 percent for women and 5 percent for men. The findings from non-experimental studies vary considerably, yet are centered around comparable point estimates. In contrast, we find that the annual earnings of program participants roughly two and half years after participating increase by roughly 25 percent. A recent evaluation yielding similar results to those found here is Hollenbeck (2003). The study of Washington state during the late 1990s finds comparable results for net annual earnings impacts (for quarters 8-11 after exit) of \$2,172 for JTPA II-A and

of \$1,864 for JTPA III for Washington State. Comparable to our findings, the study also finds insignificant short-term earning effects (within one year of participating).

What then explains the large estimates for Massachusetts during the late 1990s? One glaring possibility is the extremely strong labor market in the Northeast of the United States during this time period. By the year 2000, the state unemployment rate for Massachusetts fell to 2.6 percent, marking the end of an unprecedented strong local labor market. In such a tight labor market, wages were likely to be higher than they otherwise would, and employees more likely to be choosy about which offers they accepted. Unfortunately, with quarterly earnings data we are unable to estimate program effects on the rate of pay.

A strong labor market will also mechanically contribute to the earning effect via a strong program employment (or hours) effect. That is to say, even in the absence of a program impact on the rate of pay, the increase in the proportion of workers employed (as well as the proportion of time employed during any given quarter) will increase quarterly and ultimately hourly earnings.

Indeed, in results not reported in this paper, we find considerable evidence of a program effect on employment (measured as positive quarterly earnings in the UI administrative records).²¹ For example, a difference-in-difference estimate of the effect of participating on quarterly employment rates (years five relative to year one) yields an average employment effect of 5 percentage points. Moreover, the employment effect estimates are larger for our subsamples exhibiting the largest proportional increases in earnings. For example, for probability quintiles one through five, the employment rate effect estimates are 8, 12, 10, 6 and 3 percentage points, respectively. The corresponding proportional effects on earnings are 20, 35, 35, 25, and 14 percent. Thus, the strong labor market during this time period and in this state are likely to

²¹ These results are available upon request.

explain in part, the particular effectiveness of the workforce development efforts that we examine.

An alternative possibility is that the administration of workforce development programs in Massachusetts may be particularly effective. Massachusetts incorporated elements of competition and performance standards into their workforce development system under JTPA well before these were required under WIA. Optimistically speaking, perhaps these results are suggestive of the potential effect of recent federal reforms.

References:

- Ashenfelter, Orley (1978), "Estimating the Effect of Training Programs on Earnings." *Review of Economics and Statistics*, 60(1) : 47-57.
- Ashenfelter, Orley and David Card (1985), "Using the Longitudinal Structure of Earnings to Estimate the Effect of Training Programs," *Review of Economics and Statistics*, 67(4):648-660.
- Barnow, Burt S. (1987), "The Impact of CETA Programs on Earnings," *Journal of Human Resources*, 22(2): 157-93.
- Bell, Stephen H. and Larry L. Orr (1994), "Is Subsidized Employment Cost Effective for Welfare Recipients? Experimental Evidence from Seven State Demonstrations," *Journal of Human Resources*, 29(1): 42-61.
- Bell, Stephen H.; Orr, Larry L; Blomquist, John D. and Glen C. Cain (1995), *Program Applicants as a Comparison Group in Evaluating Training Programs*, Kalamazoo, MI: W. E. Upjohn Institute for Employment Research.
- Borus, Michael E. (1964), "A Benefit Cost Analysis of the Economic Effectiveness of Retraining the Unemployed," *Yale Economic Essays*, 4: 371-430.
- Cain, Glen G. (1968), "Benefit Cost Estimates for Job Corps," Discussion Paper #68: Institute for Research on Poverty, University of Wisconsin, Madison.
- Card, David E., and Alan B. Krueger (1995), *Myth and Measurement: The New Economics of the Minimum Wage*, Princeton, NJ: Princeton University Press.
- Cooley, Thomas; Mcguire, Timothy W. and Edward C. Prescott (1979), "Earnings and Employment Dynamics of Manpower Trainees: An Exploratory Econometric Analysis," in Farrell E. Bloch (ed.) *Evaluating Manpower Training Programs*, JAI Press: Greenwich, CT.
- Ellwood, David T. (200?), "The Impact of the Earned Income Tax Credit and Social Policy Reforms on Work, Marriage, and Living Arrangements," *National tax Journal*, 53(4):1063 – 1105.
- Friedlander, Daniel; Greenberg, David H. and Philip K. Robins (1997), "Evaluating Government Training Programs for the Economically Disadvantaged," *Journal of Economics Literature*, 35(4): 1809-1855.
- Heckman, James J., Hidehiko Ichimura, and Petra Todd (1997) "Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Program." *Review of Economic Studies*, 64(4): 605-654.

Heckman, James J., Robert J. Lalonde, and Jeffrey A. Smith (1999), "The Economics and Econometrics of Active Labor Market Programs" in Orley Ashenfelter and David Card Eds. *Handbook of Labor Economics*, NY: Elsevier, Chap. 31: 1865-2097.

Heckman, James J., and Jeffrey A. Smith, (1999) "The Pre-Program Earnings Dip and the Determinants of Participation in a Social Program: Implications for simple Program Evaluation Strategies," *The Economic Journal*, 109(457): 313-348.

Heckman, James J.; Smith, Jeffrey A. and Christopher Taber (1994), "Accounting for Dropouts in Evaluations of Social Experiments," National Bureau of Economic Research Working Paper #166.

Hollenbeck, Kevin M. (2003), "Net Impact of the Workforce Development System in Washington State," Working Paper # 03-92. W.E. Upjohn Institute for Employment Research: Kalamazoo, MI.

Kornfeld, Robert and Howard Bloom (1999), "Measuring Program Impacts on Earnings and Employment: Do Unemployment Insurance Wage Records Agree with Survey Reports of Individuals?" *Journal of Labor Economics*, 17(1): 168-197.

Meyer, Bruce D. (1995), "Natural and Quasi-Experiments in Economics," *Journal of Business and Economic Statistics*, 13(2): 151-161.

Raphael, Steven (2004), "The Socioeconomic Status of Black Males: The Increasing Importance of Incarceration," unpublished manuscript.

Stromsdorfer, Ernst E. (1968), "Determinants of Economic Success in Retraining the Unemployed: The West Virginia Experiment," *Journal of Human Resources*, 3: 139-158.

Figure 1: Average Quarterly Earnings for OAO and Exits Groups, Two Years Pre and Three Years Post Program Intervention, All Observations

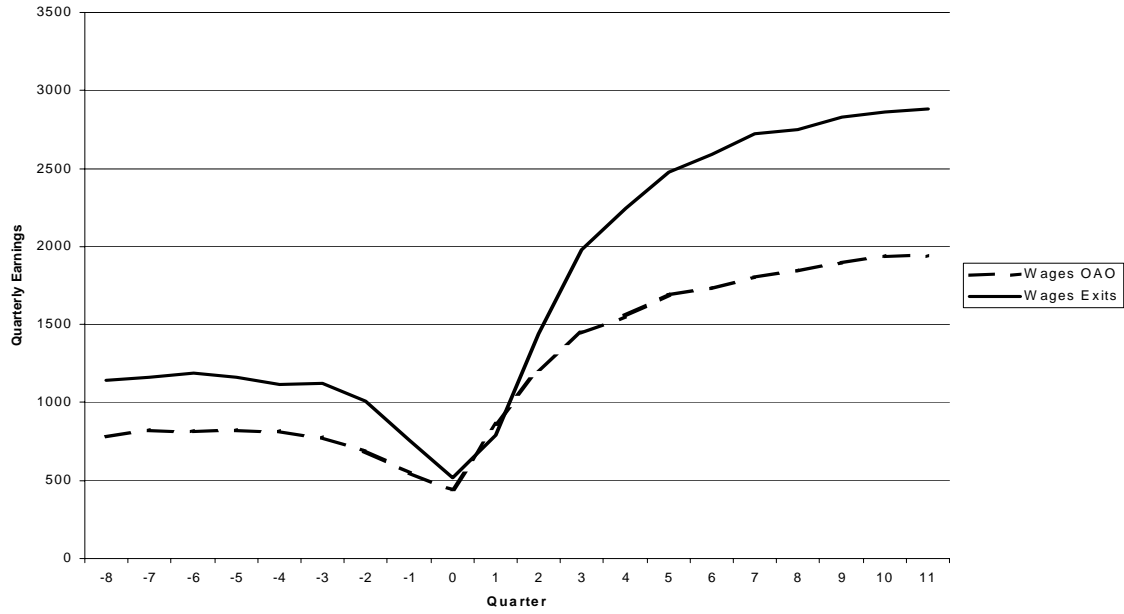


Figure 2: Average Quarterly Earnings for OAO and Exits Groups, Two Years Pre and Three Years Post Program Intervention, Men

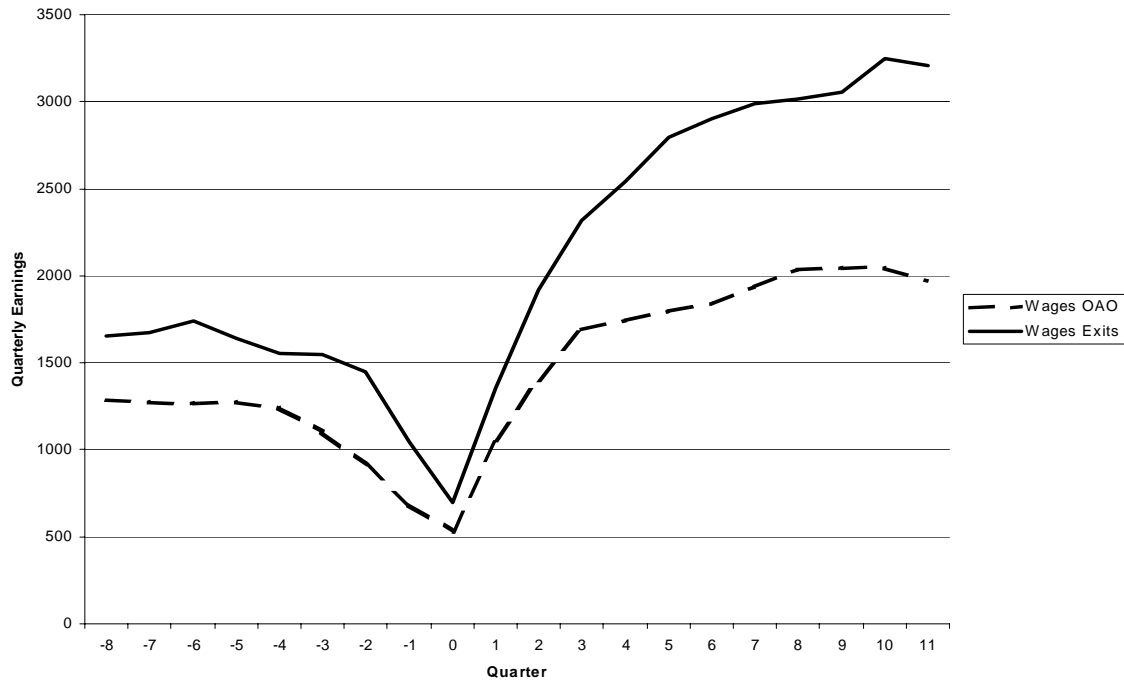


Figure 3: Average Quarterly Earnings for OAO and Exits Groups, Two Years Pre and Three Years Post Program Intervention, Women

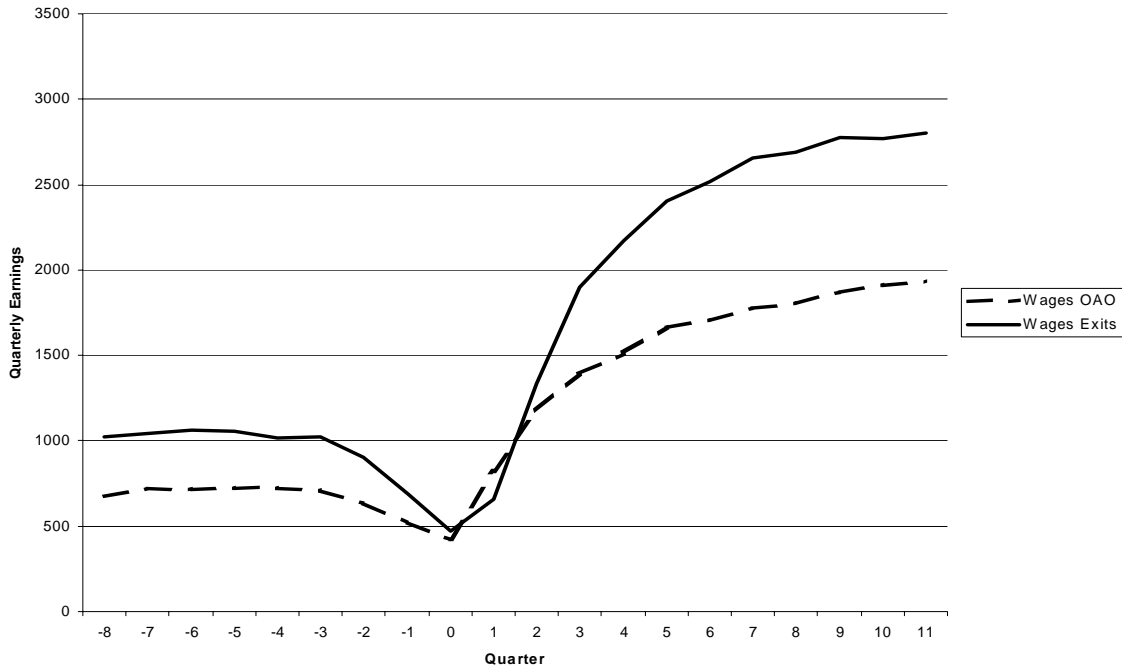


Figure 4: Average Quarterly Earnings for OAO and Exits Groups, Two Years Pre and Three Years Post Program Intervention, First Quintile

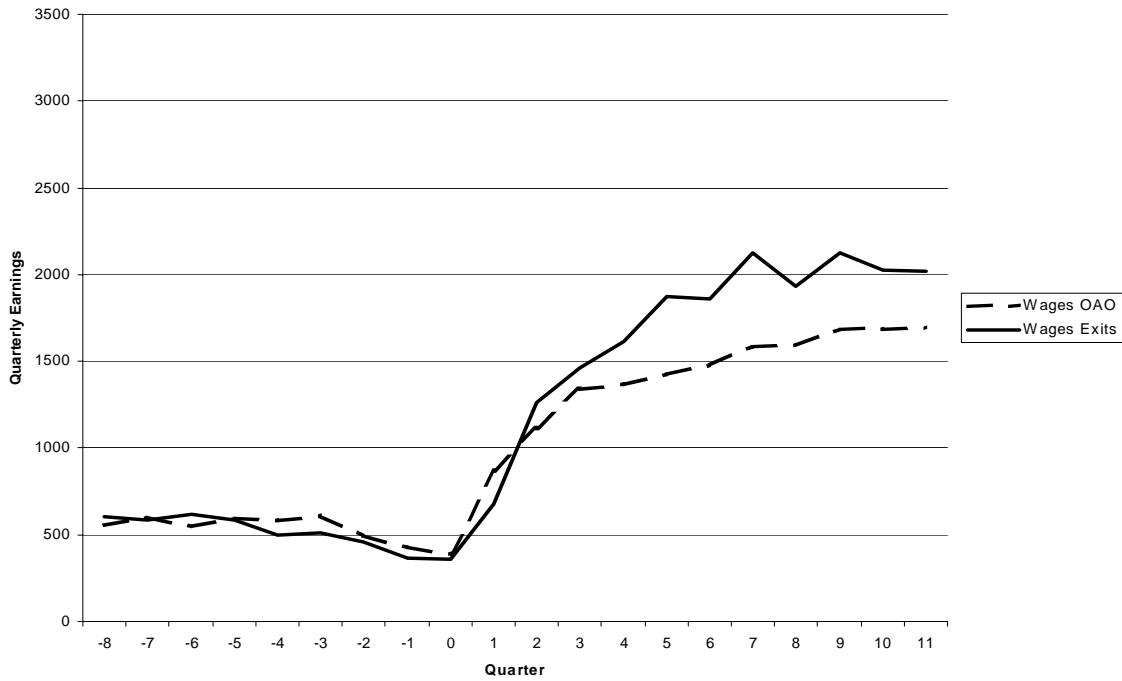


Figure 5: Average Quarterly Earnings for OAO and Exits Groups, Two Years Pre and Three Years Post Program Intervention, Second Quintile

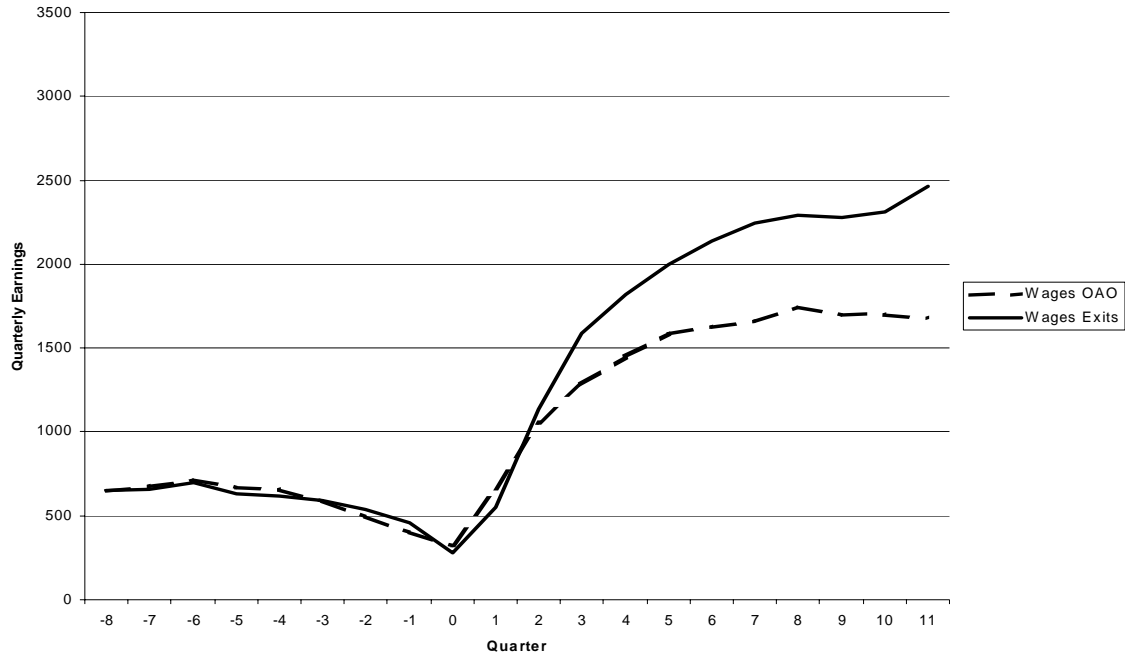


Figure 6: Average Quarterly Earnings for OAO and Exits Groups, Two Years Pre and Three Years Post Program Intervention, Third Quintile

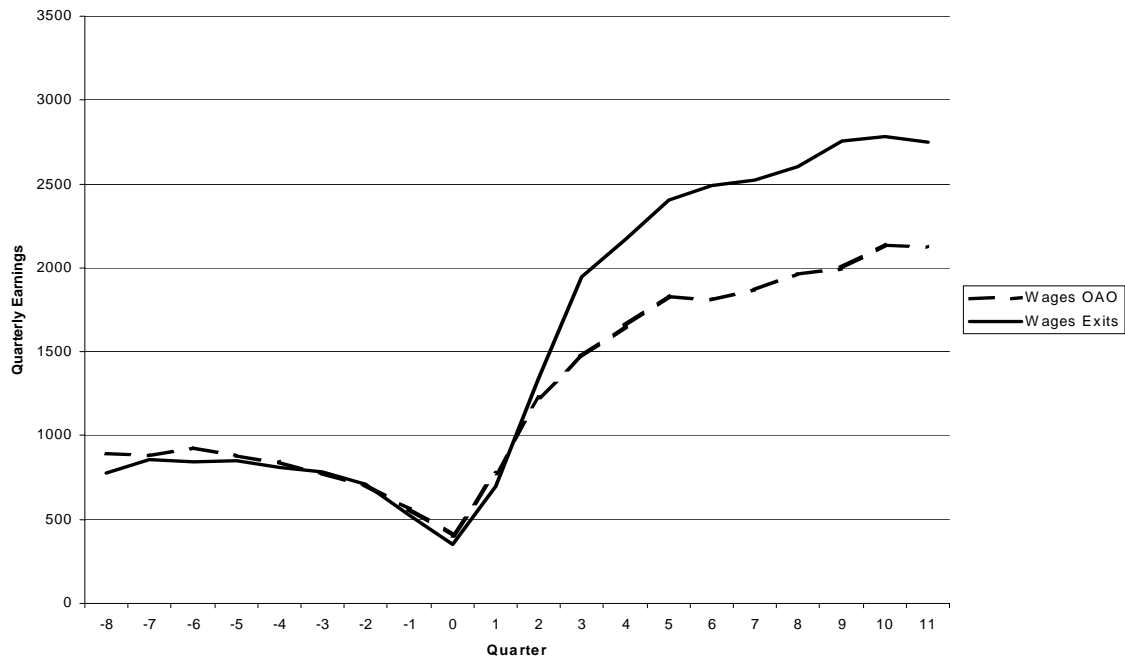


Figure 7: Average Quarterly Earnings for OAO and Exits Groups, Two Years Pre and Three Years Post Program Intervention, Fourth Quintile

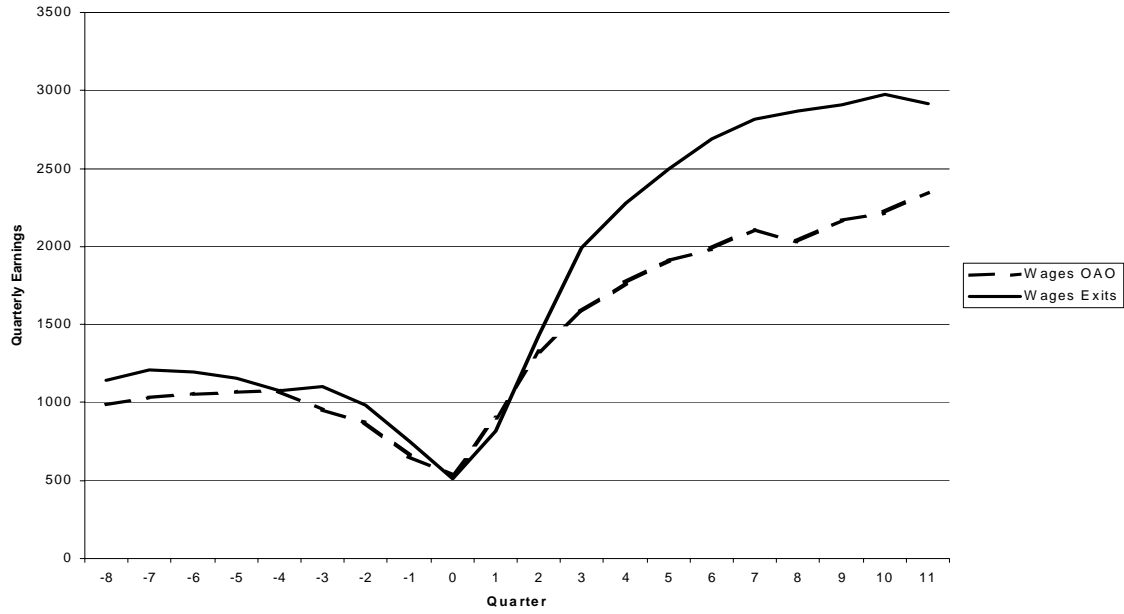


Figure 8: Average Quarterly Earnings for OAO and Exits Groups, Two Years Pre and Three Years Post Program Intervention, Fifth Quintile

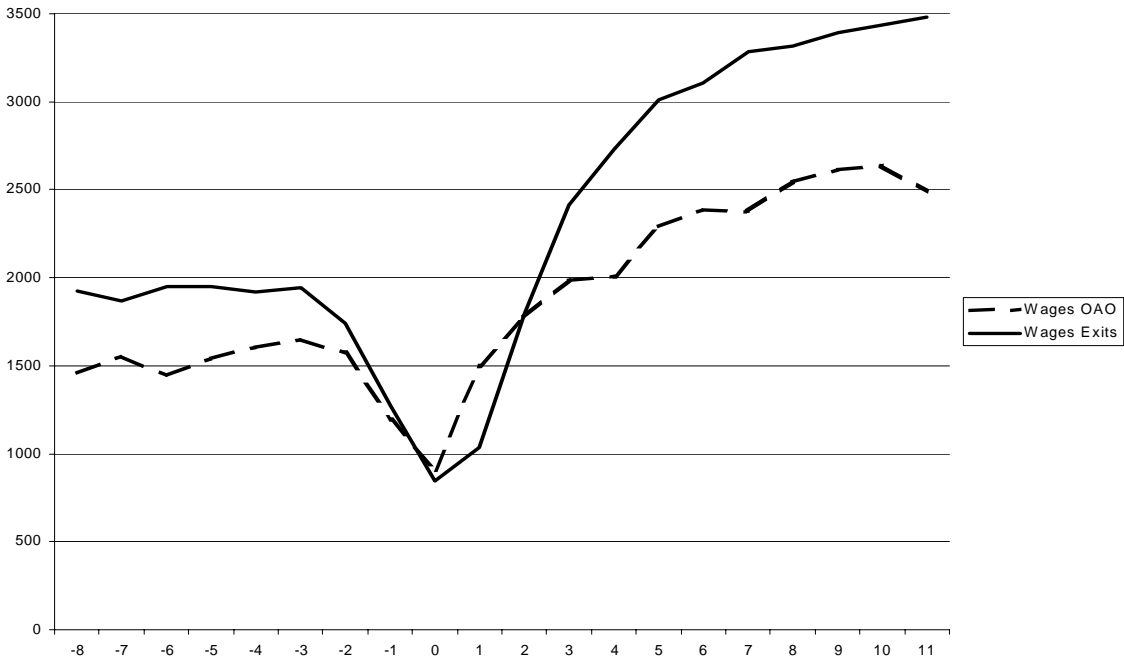


Figure 9: Difference in Average Quarterly Earnings Between Program Exits and OAO Groups, No Controls, the Random Effects Model, and Fixed Effects Model, All Observations

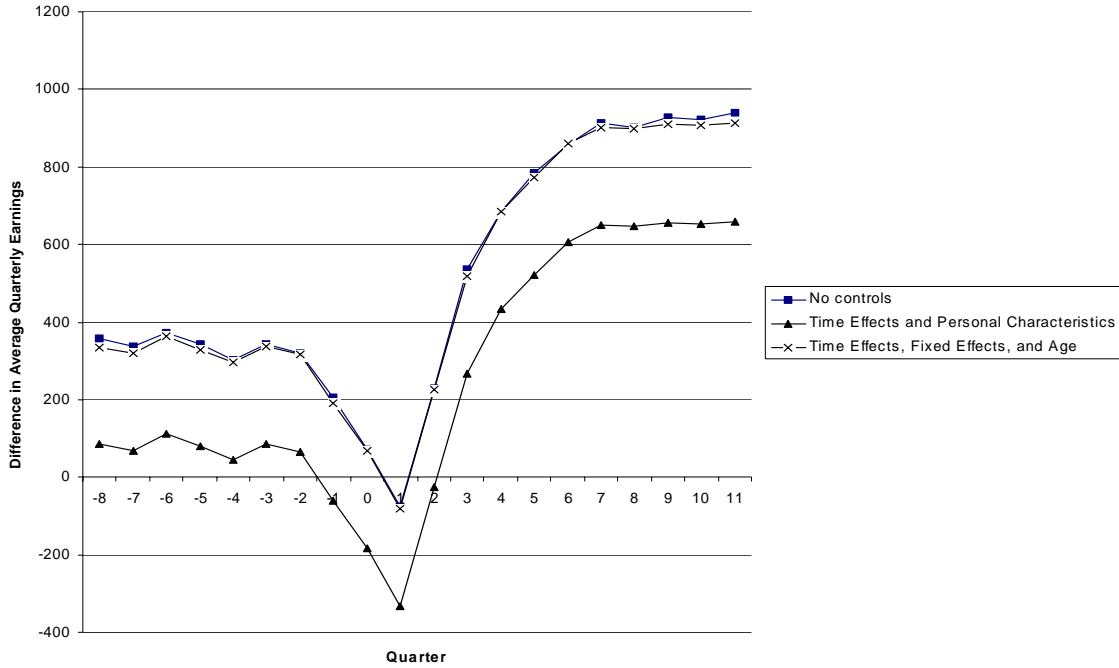


Figure 10: Difference in Average Quarterly Earnings Between Program Exits and OAO Groups, No Controls, the Random Effects Model, and Fixed Effects Model, Men

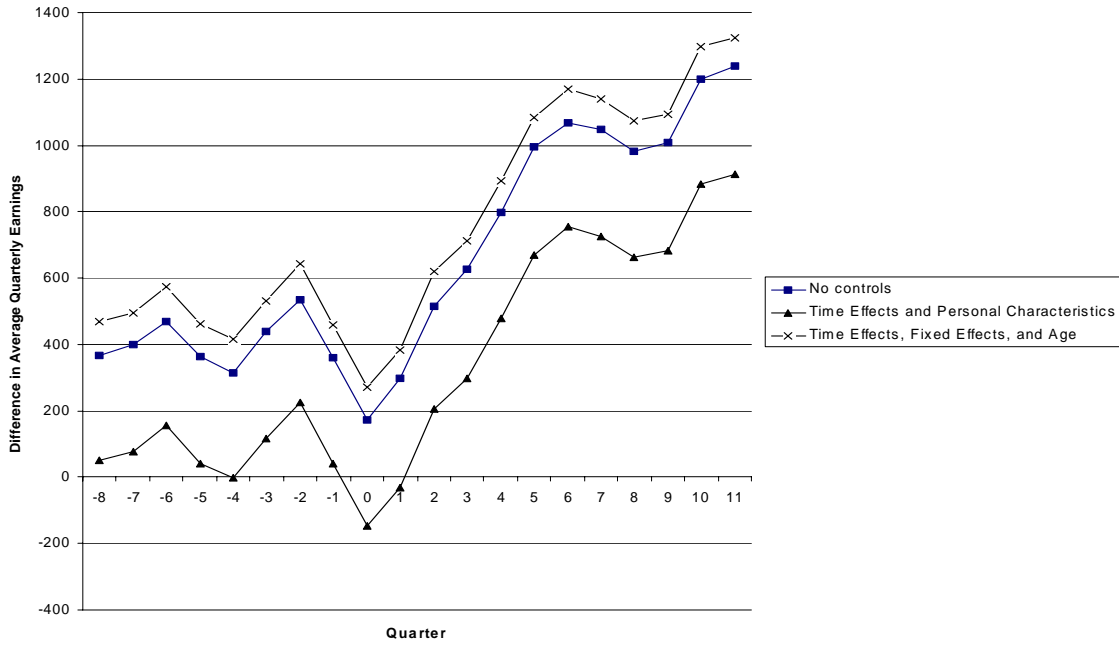


Figure 11: Difference in Average Quarterly Earnings Between Program Exits and OAO Groups, No Controls, the Random Effects Model, and Fixed Effects Model, Women

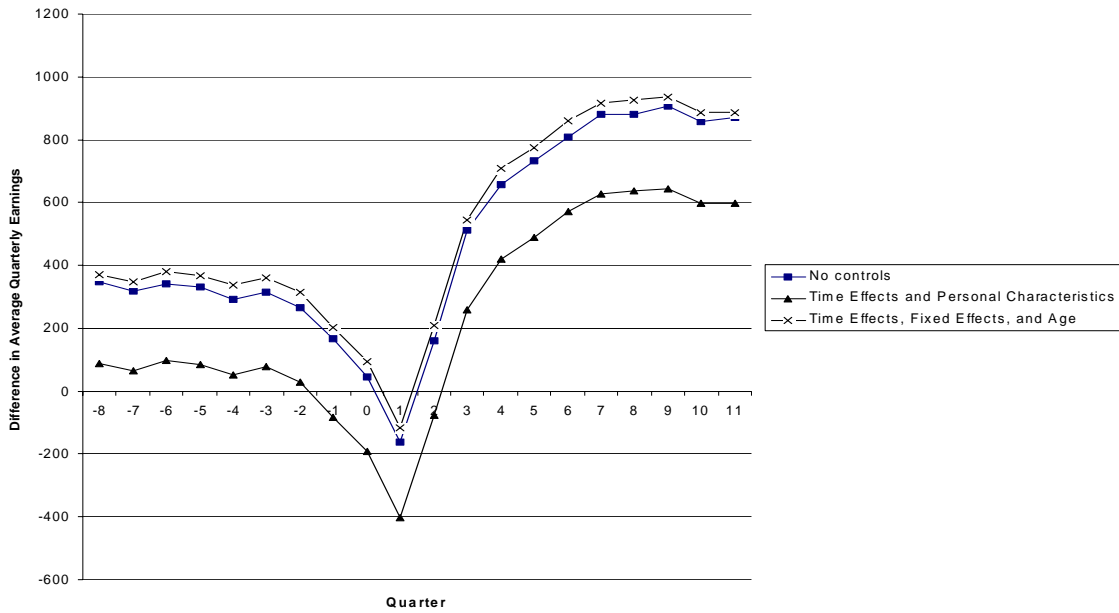


Figure 12: Difference in Average Quarterly Earnings Between Program Exits and OAO Groups, No Controls, the Random Effects Model, and Fixed Effects Model, First Quintile

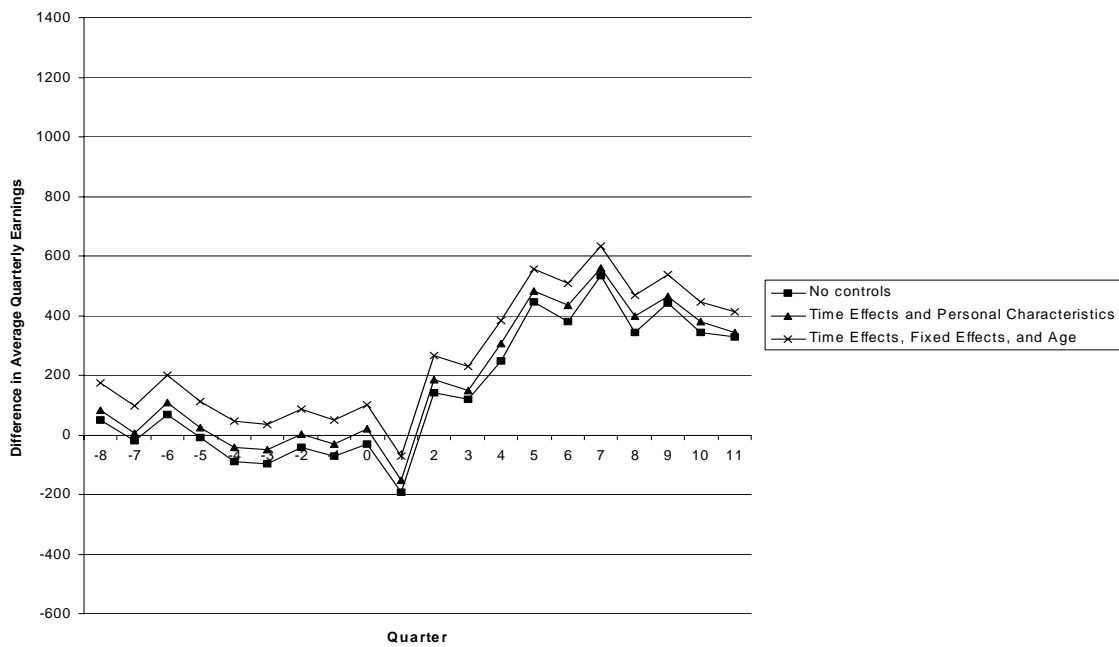


Figure 13: Difference in Average Quarterly Earnings Between Program Exits and OAO Groups, No Controls, the Random Effects Model, and Fixed Effects Model, Second Quintile

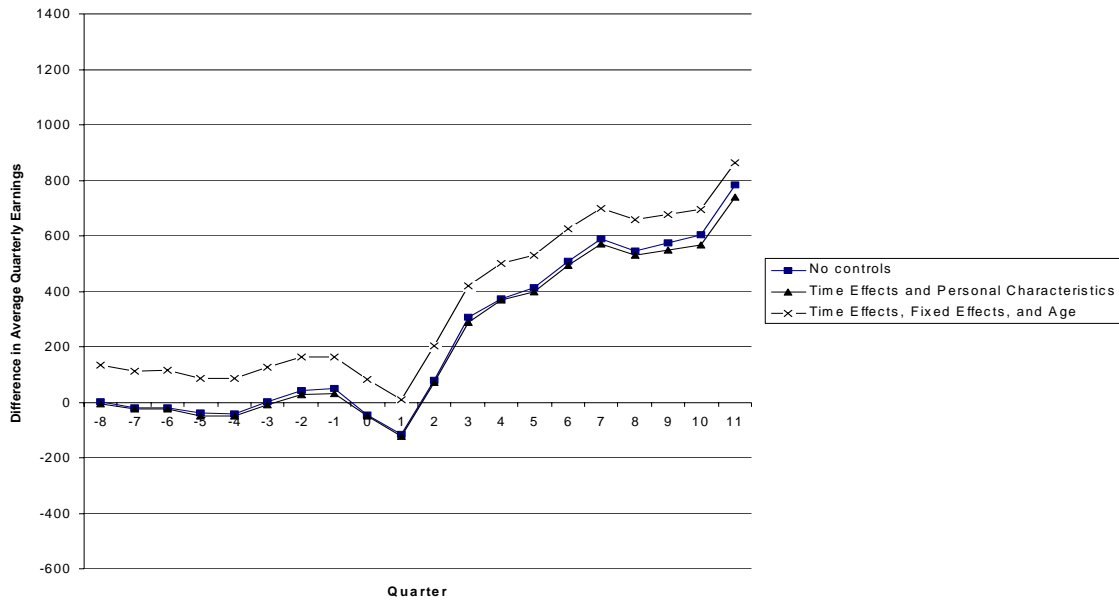


Figure 14: Difference in Average Quarterly Earnings Between Program Exits and OAO Groups, No Controls, the Random Effects Model, and Fixed Effects Model, Third Quintile

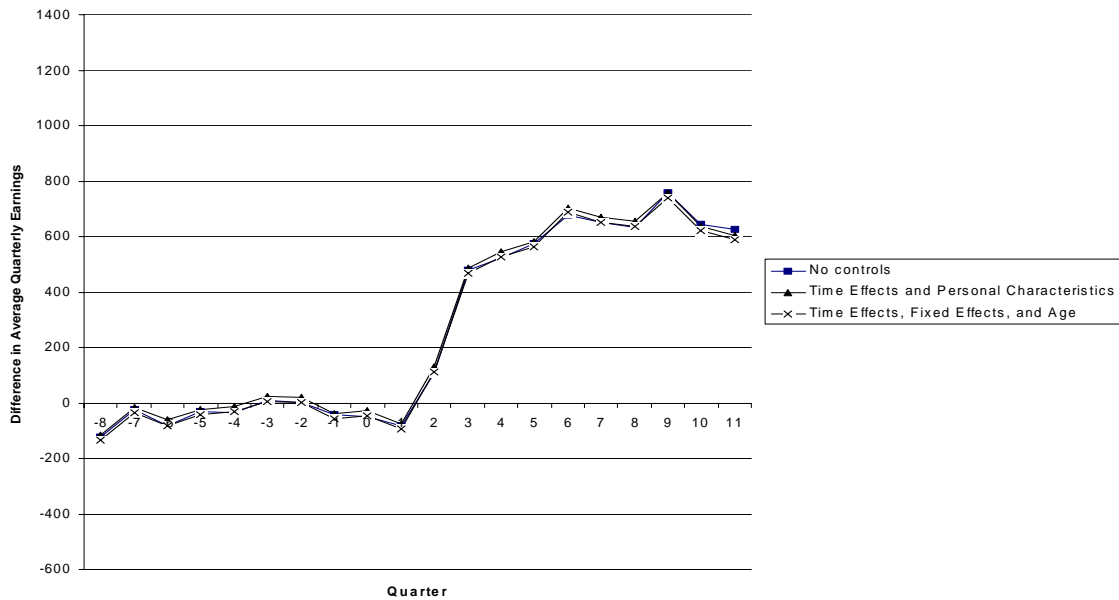


Figure 15: Difference in Average Quarterly Earnings Between Program Exits and OAO Groups, No Controls, the Random Effects Model, and Fixed Effects Model, Fourth Quintile

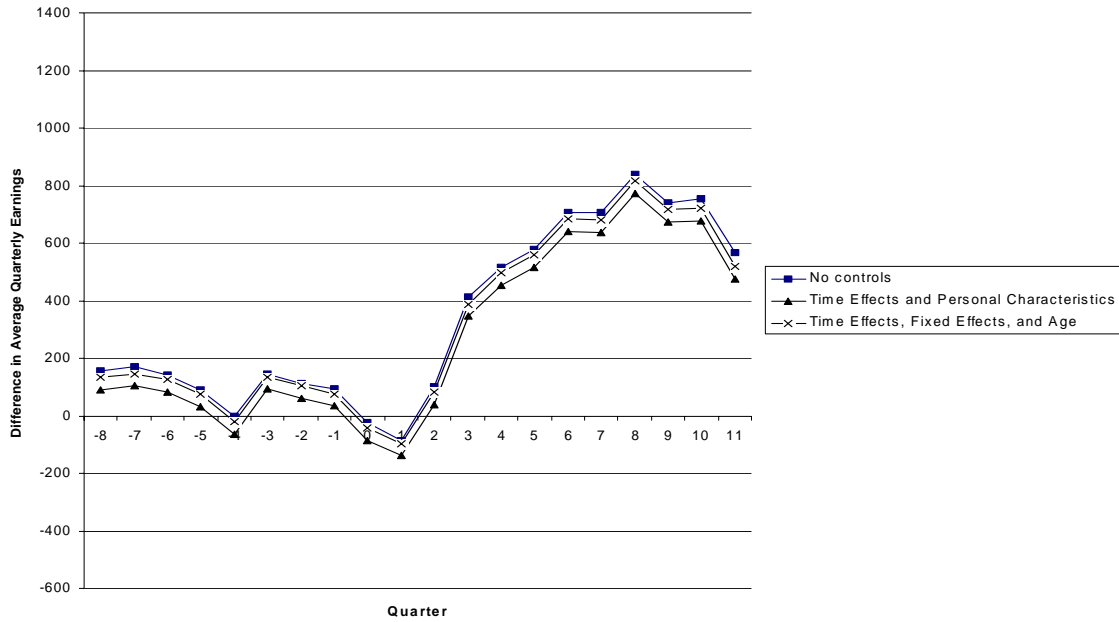


Figure 16: Difference in Average Quarterly Earnings Between Program Exits and OAO Groups, No Controls, the Random Effects Model, and Fixed Effects Model, Fifth Quintile

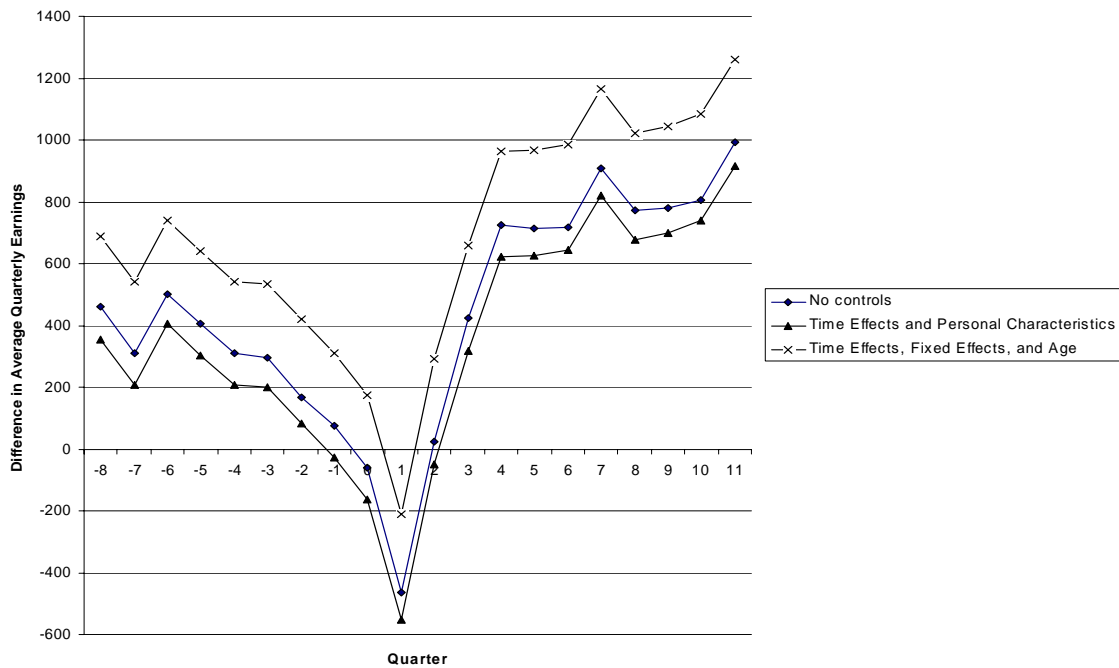


Table 1
Average Characteristics for Program Exits and OAO Samples

	Program Exits	OAO	Difference, Program Exits- OAO
Basic Demographics			
Female	0.81	0.81	0.00
Age	32.71	32.18	0.53 ^a
White	0.58	0.53	0.05 ^a
Black	0.19	0.16	0.03 ^a
Hispanic	0.19	0.28	-0.09 ^a
Asian	0.04	0.02	0.02 ^a
American Indian	0.00	0.01	0.00
Disabled	0.11	0.14	-0.03 ^a
Limited English	0.11	0.11	0.00
Veteran	0.03	0.03	0.00
Education/Skills			
In School	0.01	0.01	0.00
Highest Grade Completed	11.80	11.50	0.30 ^a
HS Dropout	0.22	0.32	-0.10 ^a
HS Student	0.00	0.00	0.00
HS Graduate	0.59	0.52	0.07 ^a
Post Secondary	0.19	0.16	0.03 ^a
Reading Level	9.02	8.07	0.95 ^a
Reading Missing	0.05	0.10	-0.06 ^a
Math Level	8.22	6.65	1.57 ^a
Math Missing	0.05	0.13	-0.08 ^a
Basic-Skills Deficient	0.64	0.56	0.08 ^a
Public Assistance Recipients			
TANF recipient	0.41	0.56	-0.15 ^a
Emergency Aid Recipient	0.02	0.03	-0.01 ^a
Refugee Assistance	0.00	0.00	0.00
SSI Recipient	0.05	0.08	-0.03 ^a
Food Stamps Recipient	0.10	0.09	0.00
TANF, Long-Term Recipient	0.17	0.25	-0.08 ^a
TANF, Near Time Limit	0.03	0.09	-0.05 ^a
TANF, Exhausted	0.00	0.00	0.00
Family Structure			
Single Parent	0.59	0.65	-0.06 ^a
Two-Parent	0.10	0.08	0.03 ^a
Zero Dependents	0.31	0.28	0.04 ^a
One Dependent	0.24	0.25	-0.01
Two-Three Dependents	0.38	0.40	-0.02 ^a
Four Plus Dependants	0.07	0.07	-0.01
Potential Employment Barriers			
Ex-Offenders	0.07	0.07	0.00
Homeless	0.02	0.02	0.00

Substance Abuse Problem	0.02	0.02	0.00
Economically Disadvantaged	0.95	0.98	-0.03 ^a
Poor Work History	0.40	0.48	-0.08 ^a
Title 2 Hard-to-Serve	0.90	0.93	-0.03 ^a
Employed at OA	0.14	0.12	0.02 ^a
UI Claimant	0.12	0.06	0.06 ^a
UI Benefits Exhausted	0.03	0.02	0.01 ^c
Non UI Claimant	0.86	0.92	-0.06 ^a
Laid Off	0.08	0.04	0.04 ^a
Number of Observations	7,038	5,395	-

- a. T-test of the difference in means indicates that the difference is statistically significant at the one percent level of confidence.
- b. T-test of the difference in means indicates that the difference is statistically significant at the five percent level of confidence.
- c. T-test of the difference in means indicates that the difference is statistically significant at the ten percent level of confidence.

Table 2**Difference in Average Characteristics between Exits and OAO Samples for All Observations and by the Predicted Probability of Participating Beyond the Objective Assessment**

	All Observations	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
Basic Demographics						
Female	0.00	-0.02	0.02	0.00	-0.02	0.01
Age	0.53 ^a	-0.57	-0.34	0.47	0.30	0.07
White	0.05 ^a	-0.02	-0.04 ^c	-0.03	0.01	0.03
Black	0.03 ^a	0.04 ^b	0.01	0.00	0.00	-0.03
Hispanic	-0.09 ^a	-0.02	0.02	0.03	0.00	-0.05 ^a
Asian	0.02 ^a	0.01	0.01 ^c	0.00	-0.01	0.05 ^a
American Indian	0.00	0.00	0.00	0.00	0.00	0.00
Disabled	-0.03 ^a	0.00	-0.01	0.01	0.00	-0.02
Limited English	0.00	0.00	0.03 ^a	0.02 ^c	-0.01	0.01
Veteran	0.00	0.00	0.00	0.00	-0.01	0.00
Education/Skills						
In School	0.00	-0.01 ^b	0.00	0.00	0.01	0.01
Highest Grade Completed	0.30 ^a	-0.17	-0.06	0.00	0.06	-0.01
HS Dropout	-0.10 ^a	0.07 ^a	0.00	0.00	-0.01	-0.03 ^b
HS Student	0.00	-0.01 ^c	0.00	0.00	0.00	0.00
HS Graduate	0.07 ^a	-0.03	0.01	0.01	-0.02	0.02
Post Secondary	0.03 ^a	-0.03 ^b	-0.01	-0.01	0.03	0.01
Reading Level	0.95 ^a	1.49 ^a	-0.03	-0.06	-0.15	-0.02
Reading Missing	-0.06 ^a	0.00	-0.02 ^a	-0.01	0.01	0.02 ^b
Math Level	1.57 ^a	2.00 ^a	-0.10	-0.12	-0.04	0.01
Math Missing	-0.08 ^a	-0.06 ^a	-0.02 ^a	0.00	0.01	0.02 ^b
Basic-Skills Deficient	0.08 ^a	0.32 ^a	-0.01	0.00	-0.04 ^b	-0.05 ^c
Public Assistance						
Recipients						
TANF recipient	-0.15 ^a	0.02	0.03	-0.03	-0.05 ^b	-0.03 ^a
Emergency Aid Recipient	-0.01 ^a	0.01	-0.01	0.00	-0.01	0.00
Refugee Assistance	0.00	0.00	0.00	0.00	0.00	0.00
SSI Recipient	-0.03 ^a	0.01	-0.01	-0.01	0.00	0.00
Food Stamps Recipient	0.00	0.02 ^c	-0.01	0.02 ^c	-0.01	-0.05 ^a

TANF, Long-Term Recipient	-0.08 ^a	0.01	0.00	-0.01	-0.01	-0.01
TANF, Near Time Limit	-0.05 ^a	0.01	-0.01	0.00	0.00	0.00
TANF, Exhausted	0.00	0.00	0.00	0.00	0.00	0.00
Family Structure						
Single Parent	-0.06 ^a	0.01	0.04 ^b	-0.03 ^c	-0.04 ^c	-0.01
Two-Parent	0.03 ^a	-0.01	0.00	0.01	0.03 ^b	0.01
Zero Dependents	0.04 ^a	0.00	-0.03 ^c	0.03	0.01	0.00
One Dependent	-0.01	0.02	0.04 ^b	-0.05 ^a	-0.02	0.02
Two-Three Dependents	-0.02 ^a	-0.03	-0.01	0.03	0.01	-0.01
Four Plus Dependents	-0.01	0.01	0.01	-0.01	0.01	-0.02
Potential Employment Barriers						
Ex-Offenders	0.00	0.03 ^b	0.00	-0.02 ^c	-0.02	-0.01
Homeless	0.00	0.01	-0.01	0.01	-0.01	-0.01
Substance Abuse Problem	0.00	0.01	0.00	0.00	-0.01	0.00
Economically Disadvantaged	-0.03 ^a	0.02 ^a	-0.01	-0.01	-0.01	-0.04 ^a
Poor Work History	-0.08 ^a	-0.07 ^a	0.01	0.01	-0.02	0.02
Title 2 Hard-to-Serve	-0.03 ^a	0.02 ^c	0.00	0.01	-0.01	-0.01
Employed at OA	0.02 ^a	-0.07 ^a	0.01	0.02 ^a	0.02	-0.02
UI Claimant	0.06 ^a	-0.01	0.00	0.00	0.01	0.03
UI Benefits Exhausted	0.01 ^c	-0.01	0.00	0.00	0.00	0.01
Non UI Claimant	-0.06 ^a	0.02 ^c	0.01	0.00	-0.02	-0.04 ^c
Laid Off	0.04 ^a	-0.01	0.00	0.00	0.01	0.03

a. T-test of the difference in means indicates that the difference is statistically significant at the one percent level of confidence.

b. T-test of the difference in means indicates that the difference is statistically significant at the five percent level of confidence.

c. T-test of the difference in means indicates that the difference is statistically significant at the ten percent level of confidence.

Table 3
Average Annual Earnings For Exits and OAO Samples by Year and Unadjusted Estimates of the Training Earnings Effects, All Observations and by Gender

	Pre-Intervention Years		Post-Intervention Years			Differences in Earnings Relative to Year One		
	Year One	Year Two	Year Three	Year Four	Year Five	Three - One	Four - One	Five - One
Entire Sample								
Exits	4,654 (88)	4,000 (89)	4,728 (89)	10,027 (89)	11,321 (88)	74 (69)	5,372 (69)	6,666 (69)
OAO	3,243 (104)	2,833 (104)	3,967 (104)	6,782 (104)	7,627 (104)	723 (81)	3,539 (81)	4,384 (82)
Diff-in-Diff	-	-	-	-	-	-650 (107)	1,833 (107)	2,282 (107)
Men								
Exits	6,708 (252)	5,590 (252)	6,294 (252)	11,229 (252)	12,527 (253)	-413 (194)	4,520 (194)	5,818 (194)
OAO	5,109 (306)	3,942 (306)	4,683 (307)	7,322 (306)	8,099 (306)	-425 (236)	2,212 (236)	2,989 (236)
Diff-in-Diff	-	-	-	-	-	12 (306)	2,307 (306)	2,829 (306)
Women								
Exits	4,181 (91)	3,634 (91)	4,367 (91)	9,749 (91)	11,043 (92)	185 (72)	5,568 (73)	6,861 (73)
OAO	2,843 (107)	2,595 (107)	3,813 (107)	6,667 (107)	7,526 (107)	970 (85)	3,823 (85)	4,682 (85)
Diff-in-Diff	-	-	-	-	-	-784 (111)	1,744 (111)	2,178 (111)

Standard errors are in parentheses. Average annual earnings, pre-post intervention changes in annual earnings, and the difference-in-difference program effects are estimated from the coefficients of equation (2). The difference-in-difference estimator subtracts the pre-post change in earnings for the OAO sample from the comparable pre-post change in earnings for the Exits sample.

Table 4
Average Annual Earnings For Exits and OAO Samples by Year and Unadjusted Estimates of the Training Earnings Effects, by Predicted Probability Quintile

	Pre-Intervention Years		Post-Intervention Years			Differences in Earnings Relative to Year One		
	Year One	Year Two	Year Three	Year Four	Year Five	Three - One	Four - One	Five - One
First Quintile								
Exits	2,387 (247)	1,823 (247)	3,762 (247)	7,469 (247)	8,104 (247)	1,374 (204)	5,082 (204)	5,717 (204)
OAO	2,302 (144)	2,126 (144)	3,722 (144)	5,863 (144)	6,652 (144)	1,419 (118)	3,560 (118)	4,349 (118)
Diff-in-Diff	-	-	-	-	-	-45 (236)	1,521 (236)	1,367 (236)
Second Quintile								
Exits	2,634 (167)	2,201 (167)	3,556 (167)	8,201 (167)	9,339 (167)	922 (140)	5,567 (140)	6,704 (140)
OAO	2,710 (172)	2,151 (172)	3,333 (172)	6,319 (172)	6,829 (172)	622 (145)	3,608 (145)	4,119 (145)
Diff-in-Diff	-	-	-	-	-	299 (201)	1,958 (201)	2,585 (202)
Third Quintile								
Exits	3,328 (176)	2,825 (176)	4,332 (176)	9,594 (176)	10,892 (175)	1,004 (146)	6,265 (146)	7,562 (146)
OAO	3,589 (221)	2,894 (221)	3,876 (221)	7,167 (221)	8,234 (221)	286 (184)	3,578 (184)	4,645 (184)
Diff-in-Diff	-	-	-	-	-	717 (236)	2,687 (236)	2,917 (236)
Fourth Quintile								
Exits	4,705 (184)	3,907 (184)	4,752 (184)	10,281 (184)	11,673 (184)	47 (142)	5,575 (143)	6,967 (143)
OAO	4,150 (281)	3,561 (281)	4,350 (281)	7,773 (281)	8,773 (281)	200 (217)	3,623 (217)	4,622 (218)
Diff-in-Diff	-	-	-	-	-	-153 (260)	1,952 (261)	2,345 (261)
Fifth Quintile								
Exits	7,688 (212)	6,870 (212)	6,091 (212)	12,127 (212)	13,635 (212)	-1,597 (162)	4,438 (162)	5,945 (162)
OAO	6,012 (439)	6,023 (439)	6,166 (439)	9,064 (439)	10,286 (439)	153 (336)	3,051 (336)	4,273 (336)
Diff-in-Diff	-	-	-	-	-	-1,751 (374)	1,387 (374)	1,672 (373)

Standard errors are in parentheses. Average annual earnings, pre-post intervention changes in annual earnings, and the difference-in-difference program effects are estimated from the coefficients of equation (2). The difference-in-difference estimator subtracts the pre-post change in earnings for the OAO sample from the comparable pre-post change in earnings for the Exits sample.

Table 5
Difference-in-Difference Estimates of the Training Program Effects on Average Annual Earnings in the First, Second, and Third Years Following the Training Intervention

	Relative change, year three – year one	Relative change, year four – year one	Relative change, year five – year one
Entire Sample			
No Controls	-650 (107)	1,833 (107)	2,282 (107)
Random Effects ^a	-617 (107)	1,864 (107)	2,270 (107)
Time and Fixed Effects ^b	-610 (107)	1,876 (107)	2,285 (107)
Men			
No Controls	12 (306)	2,307 (306)	2,829 (306)
Random Effects ^b	1 (305)	2,307 (306)	2,815 (305)
Time and Fixed Effects ^c	-11 (306)	2,290 (306)	2,791 (306)
Women			
No Controls	-784 (111)	1,744 (111)	2,178 (111)
Random Effects ^a	-748 (112)	1,775 (112)	2,143 (111)
Time and Fixed Effects ^b	-735 (109)	1,797 (109)	2,172 (112)
First Quintile			
No Controls	-45 (236)	1,521 (236)	1,367 (236)
Random Effects ^a	-17 (236)	1,563 (236)	1,363 (236)
Time and Fixed Effects ^b	-59 (236)	1,501 (229)	1,271 (236)
Second Quintile			
No Controls	299 (201)	1,958 (201)	2,585 (202)
Random Effects ^a	291 (202)	1,936 (202)	2,488 (202)
Time and Fixed Effects ^b	270 (201)	1,904 (202)	2,447 (202)
Third Quintile			
No Controls	717 (236)	2,687 (236)	2,917 (236)
Random Effects ^a	725 (236)	2,712 (236)	2,868 (236)
Time and Fixed Effects ^b	731 (236)	2,722 (229)	2,882 (236)
Fourth Quintile			
No Controls	-153 (260)	1,952 (261)	2,345 (261)
Random Effects ^a	-150 (260)	1,936 (260)	2,287 (260)
Time and Fixed Effects ^b	-149 (261)	1,938 (261)	2,289 (261)
Fifth Quintile			
No Controls	-1,751 (374)	1,387 (374)	1,672 (373)
Random Effects ^a	-1,718 (372)	1,440 (373)	1,763 (372)
Time and Fixed Effects ^b	-,1697 (373)	1,473 (373)	1,805 (372)

Standard errors are in parentheses. The difference-in-difference program effect estimates are estimated from the coefficient estimates from estimating equations (2), (9) and (10).

a. The random effects model corresponds to equation (9).

b. The time and fixed effect model corresponds to equation (10).

Table 6
Alternative Difference-in-Difference Estimates of the Program Earnings Effects Altering the
Composition of the Sample Analyzed

	Base estimates from Table 5	Observations 55 and Under	Dropping multiple service observations	Dropping Boston exits	Dropping alternative services OAO
Entire Sample					
No Controls	2,282 (107)	2,218 (108)	2,451 (107)	1,949 (108)	3,438 (122)
Random Effects	2,270 (107)	2,216 (108)	2,425 (107)	1,944 (108)	3,355 (122)
Time and Fixed Effects	2,285 (107)	2,247 (108)	2,430 (107)	1,966 (108)	3,301 (122)
Men					
No Controls	2,829 (306)	2,542 (312)	3,179 (304)	2,658 (309)	3,122 (311)
Random Effects	2,815 (305)	2,573 (311)	3,165 (304)	2,646 (309)	3,094 (311)
Time and Fixed Effects	2,791 (306)	2,575 (311)	3,141 (304)	2,629 (309)	3,066 (303)
Women					
No Controls	2,178 (111)	2,166 (112)	2,303 (111)	1,809 (113)	3,463 (131)
Random Effects	2,143 (111)	2,141 (112)	2,249 (112)	1,778 (113)	3,362 (131)
Time and Fixed Effects	2,172 (112)	2,181 (113)	2,267 (112)	1,812 (113)	3,318 (131)
First Quintile					
No Controls	1,367 (236)	1,289 (238)	1,634 (238)	765 (246)	2,949 (257)
Random Effects	1,363 (236)	1,302 (237)	1,617 (238)	824 (245)	2,875 (256)
Time and Fixed Effects	1,271 (236)	1,288 (237)	1,531 (238)	774 (245)	2,797 (257)
Second Quintile					
No Controls	2,585 (202)	2,528 (203)	2,619 (202)	2,285 (202)	3,605 (227)
Random Effects	2,488 (202)	2,434 (204)	2,523 (203)	2,190 (203)	3,457 (228)
Time and Fixed Effects	2,447 (202)	2,424 (204)	2,479 (203)	2,146 (202)	3,333 (228)
Third Quintile					
No Controls	2,917 (236)	2,929 (236)	2,993 (236)	2,736 (237)	4,485 (271)
Random Effects	2,868 (236)	2,861 (237)	2,927 (237)	2,670 (237)	4,347 (272)
Time and Fixed Effects	2,882 (236)	2,867 (237)	2,478 (203)	2,682 (231)	4,292 (271)
Fourth Quintile					
No Controls	2,345 (261)	2,367 (262)	2,493 (260)	2,027 (261)	3,150 (284)
Random Effects	2,287 (260)	2,320 (262)	2,425 (260)	1,984 (261)	3,028 (284)
Time and Fixed Effects	2,289 (261)	2,335 (262)	2,423 (260)	1,995 (262)	3,001 (284)
Fifth Quintile					
No Controls	1,672 (373)	1,722 (374)	1,975 (367)	1,192 (380)	1,961 (379)
Random Effects	1,763 (372)	1,809 (373)	2,039 (367)	1,307 (379)	2,053 (378)
Time and Fixed Effects	1,805 (372)	1,846 (372)	2,059 (366)	1,367 (379)	2,100 (378)

Standard errors for the estimates are in parentheses. The differences in average annual earnings and the difference-in-difference in average annual earnings are estimated from the coefficient estimates from estimating equations (2), (9), and (10). The three model specifications correspond to those depicted in Figures 9 through 16. All estimates are statistically significant at the one percent level of confidence unless noted otherwise.

Appendix Table A1**Average Characteristics for the Program Exits and OAO Samples for the First Three Quintiles of the Predicted Probability of Continuing Past the Objective Assessment**

	First Quintile			Second Quintile			Third Quintile		
	Program Exits	OAO	Difference, Program Exits-OAO	Program Exits	OAO	Difference, Program Exits-OAO	Program Exits	OAO	Difference, Program Exits-OAO
Basic Demographics									
Female	0.81	0.83	-0.02	0.79	0.78	0.02	0.83	0.83	0.00
Age	31.99	32.55	-0.57	30.84	31.18	-0.34	31.52	31.05	0.47
White	0.42	0.44	-0.02	0.48	0.52	-0.04 ^c	0.59	0.62	-0.03
Black	0.15	0.11	0.04 ^b	0.18	0.17	0.01	0.18	0.18	0.00
Hispanic	0.41	0.43	-0.02	0.31	0.29	0.02	0.20	0.18	0.03
Asian	0.01	0.01	0.01	0.02	0.01	0.01 ^c	0.02	0.02	0.00
American Indian	0.01	0.01	0.00	0.01	0.01	0.00	0.01	0.00	0.00
Disabled	0.17	0.18	0.00	0.12	0.13	-0.01	0.10	0.09	0.01
Limited English	0.16	0.16	0.00	0.10	0.07	0.03 ^a	0.07	0.05	0.02 ^c
Veteran	0.03	0.02	0.00	0.03	0.03	0.00	0.04	0.04	0.00
Education/Skills									
In School	0.00	0.01	-0.01 ^b	0.00	0.00	0.00	0.00	0.01	0.00
Highest Grade Completed	10.96	11.12	-0.17	11.24	11.29	-0.06	11.72	11.72	0.00
HS Dropout	0.51	0.43	0.07 ^a	0.39	0.39	0.00	0.22	0.23	0.00
HS Student	0.00	0.01	-0.01 ^c	0.00	0.00	0.00	0.00	0.00	0.00
HS Graduate	0.38	0.42	-0.03	0.50	0.49	0.01	0.62	0.61	0.01
Post Secondary	0.11	0.15	-0.03 ^b	0.11	0.12	-0.01	0.16	0.16	-0.01
Reading Level	5.71	4.22	1.49 ^a	8.45	8.48	-0.03	9.95	10.01	-0.06
Reading Missing	0.24	0.23	0.00	0.02	0.05	-0.02 ^a	0.02	0.02	-0.01
Math Level	4.90	2.90	2.00 ^a	6.97	7.07	-0.10	8.11	8.23	-0.12
Math Missing	0.26	0.32	-0.06 ^a	0.02	0.05	-0.02 ^a	0.02	0.02	0.00
Basic-Skills Deficient	0.66	0.34	0.32 ^a	0.73	0.74	-0.01	0.65	0.65	0.00
Public Assistance Recipients									
TANF recipient	0.65	0.63	0.02	0.67	0.64	0.03	0.59	0.62	-0.03

Emergency Aid Recipient	0.04	0.03	0.01	0.03	0.04	-0.01	0.02	0.02	0.00
Refugee Assistance	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
SSI Recipient	0.13	0.12	0.01	0.08	0.09	-0.01	0.04	0.05	-0.01
Food Stamps Recipient	0.09	0.07	0.02 ^c	0.09	0.10	-0.01	0.12	0.09	0.02 ^c
TANF, Long-Term Recipient	0.34	0.33	0.01	0.29	0.28	0.00	0.21	0.22	-0.01
TANF, Near Time Limit	0.18	0.17	0.01	0.09	0.10	-0.01	0.02	0.01	0.00
TANF, Exhausted	0.00	0.01	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Family Structure									
Single Parent	0.67	0.67	0.01	0.70	0.66	0.04 ^b	0.68	0.71	-0.03 ^c
Two-Parent	0.06	0.08	-0.01	0.07	0.07	0.00	0.07	0.06	0.01
Zero Dependents	0.26	0.26	0.00	0.23	0.26	-0.03 ^c	0.25	0.23	0.03
One Dependent	0.26	0.24	0.02	0.29	0.25	0.04 ^b	0.27	0.32	-0.05 ^a
Two-Three Dependents	0.39	0.42	-0.03	0.39	0.40	-0.01	0.42	0.39	0.03
Four Plus Dependents	0.09	0.08	0.01	0.09	0.08	0.01	0.06	0.06	-0.01
Potential Employment Barriers									
Ex-Offenders	0.09	0.07	0.03 ^b	0.06	0.06	0.00	0.06	0.08	-0.02 ^c
Homeless	0.03	0.02	0.01	0.01	0.02	-0.01	0.02	0.02	0.01
Substance Abuse Problem	0.03	0.02	0.01	0.01	0.01	0.00	0.01	0.01	0.00
Economically Disadvantaged	0.99	0.98	0.02 ^a	0.99	0.99	-0.01	0.98	0.99	-0.01
Poor Work History	0.50	0.57	-0.07 ^a	0.51	0.50	0.01	0.47	0.46	0.01
Title 2 Hard-to-Serve	0.98	0.96	0.02 ^c	0.95	0.95	0.00	0.93	0.92	0.01
Employed at OA	0.08	0.14	-0.07 ^a	0.07	0.06	0.01	0.11	0.09	0.02 ^a
UI Claimant	0.02	0.02	-0.01	0.03	0.04	0.00	0.05	0.05	0.00
UI Benefits Exhausted	0.01	0.02	-0.01	0.01	0.02	0.00	0.02	0.02	0.00
Non UI Claimant	0.97	0.96	0.02 ^c	0.95	0.95	0.01	0.92	0.93	0.00
Laid Off	0.01	0.02	-0.01	0.02	0.02	0.00	0.03	0.04	0.00
Number of Observations	559	1,926	-	1,245	1,242	-	1,503	985	-

- a. T-test of the difference in means indicates that the difference is statistically significant at the one percent level of confidence.
- b. T-test of the difference in means indicates that the difference is statistically significant at the five percent level of confidence.
- c. T-test of the difference in means indicates that the difference is statistically significant at the ten percent level of confidence.

Appendix Table A2**Average Characteristics for the Program Exits and OAO Samples for the Fourth and Fifth Quintiles of the Predicted Probability of Continuing Past the Objective Assessment**

	Fourth Quintile			Fifth Quintile		
	Program Exits	OAO	Difference, Program Exits-OAO	Program Exits	OAO	Difference, Program Exits-OAO
Basic Demographics						
Female	0.81	0.82	-0.02	0.81	0.79	0.01
Age	33.17	32.87	0.30	34.55	34.49	0.07
White	0.63	0.62	0.01	0.62	0.59	0.03
Black	0.21	0.21	0.00	0.20	0.23	-0.03
Hispanic	0.13	0.14	0.00	0.09	0.14	-0.05 ^a
Asian	0.02	0.03	-0.01	0.09	0.04	0.05 ^a
American Indian	0.00	0.01	0.00	0.00	0.00	0.00
Disabled	0.11	0.11	0.00	0.09	0.11	-0.02
Limited English	0.07	0.08	-0.01	0.15	0.15	0.01
Veteran	0.04	0.04	-0.01	0.03	0.03	0.00
Education/Skills						
In School	0.01	0.00	0.01	0.03	0.02	0.01
Highest Grade Completed	12.11	12.05	0.06	12.18	12.19	-0.01
HS Dropout	0.13	0.14	-0.01	0.09	0.13	-0.03 ^b
HS Student	0.00	0.00	0.00	0.00	0.00	0.00
HS Graduate	0.64	0.66	-0.02	0.65	0.63	0.02
Post Secondary	0.22	0.19	0.03	0.26	0.25	0.01
Reading Level	10.64	10.79	-0.15	10.80	10.82	-0.02
Reading Missing	0.02	0.02	0.01	0.05	0.02	0.02 ^b
Math Level	8.86	8.90	-0.04	9.27	9.26	0.01
Math Missing	0.02	0.01	0.01	0.05	0.02	0.02 ^b
Basic-Skills Deficient	0.60	0.64	-0.04 ^b	0.60	0.65	-0.05 ^c
Public Assistance Recipients						
TANF recipient	0.40	0.45	-0.05 ^b	0.06	0.09	-0.03 ^a
Emergency Aid Recipient	0.02	0.02	-0.01	0.01	0.01	0.00
Refugee Assistance	0.00	0.00	0.00	0.00	0.00	0.00
SSI Recipient	0.04	0.04	0.00	0.00	0.01	0.00
Food Stamps Recipient	0.12	0.12	-0.01	0.07	0.11	-0.05 ^a
TANF, Long-Term Recipient	0.15	0.16	-0.01	0.03	0.04	-0.01
TANF, Near Time Limit	0.00	0.00	0.00	0.00	0.00	0.00
TANF, Exhausted	0.00	0.00	0.00	0.00	0.00	0.00
Family Structure						
Single Parent	0.57	0.60	-0.04 ^c	0.44	0.45	-0.01
Two-Parent	0.10	0.07	0.03 ^b	0.16	0.15	0.01

Zero Dependents	0.34	0.33	0.01	0.41	0.40	0.00
One Dependent	0.21	0.23	-0.02	0.22	0.19	0.02
Two-Three Dependents	0.39	0.38	0.01	0.32	0.33	-0.01
Four Plus Dependents	0.07	0.06	0.01	0.06	0.07	-0.02
Potential Employment Barriers						
Ex-Offenders	0.06	0.08	-0.02	0.10	0.11	-0.01
Homeless	0.02	0.03	-0.01	0.02	0.03	-0.01
Substance Abuse Problem	0.02	0.03	-0.01	0.03	0.04	0.00
Economically Disadvantaged	0.97	0.98	-0.01	0.89	0.93	-0.04 ^a
Poor Work History	0.38	0.40	-0.02	0.28	0.25	0.02
Title 2 Hard-to-Serve	0.88	0.89	-0.01	0.85	0.86	-0.01
Employed at OA	0.15	0.13	0.02	0.20	0.22	-0.02
UI Claimant	0.10	0.08	0.01	0.26	0.23	0.03
UI Benefits Exhausted	0.03	0.03	0.00	0.04	0.03	0.01
Non UI Claimant	0.87	0.89	-0.02	0.70	0.74	-0.04 ^c
Laid Off	0.07	0.06	0.01	0.19	0.16	0.03
Number of Observations	1,716	769	-	2,015	473	-

-
- T-test of the difference in means indicates that the difference is statistically significant at the one percent level of confidence.
 - T-test of the difference in means indicates that the difference is statistically significant at the five percent level of confidence.
 - T-test of the difference in means indicates that the difference is statistically significant at the ten percent level of confidence.