

The Impact of Job Training on Welfare Recipients with Children

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Abstract

The Employment Services and Placement (ESP) program, an innovative job training and outplacement assistance program that was mandatory for job-ready family assistance recipients, was launched in early 2000. When the program was initiated, the entire cohort of eligible recipients could not be enrolled simultaneously. This creates the opportunity to identify the effect of the program using a natural experiment in which a cohort of recipients who were enrolled in the program on a given date are compared to a cohort who were not enrolled on that date. Ultimately, 50 percent of the latter cohort was enrolled in the ESP program, and recipients were selected for enrollment solely based on observable characteristics. After adjusting for the two previous factors, the results indicate that the ESP program increases the likelihood that a recipient will start a job by between eight and eleven percentage points. If one could fully adjust for the subsequent enrollment of members of the comparison group, the estimated treatment effect would almost certainly be larger. The findings also indicate that a majority of recipients who start a job exit and remain off welfare for the period under study.

I. Introduction

In the early 1990s, a consensus was developing that welfare programs were failing. Critics charged that the programs were expensive, not preparing recipients for gainful employment, fostering welfare dependency, and ironically, not helping the individuals for whom the programs were designed. In response to these concerns, New York City made significant changes to its welfare programs between 1995 and 2000. Since 1995, New York City's welfare caseload dropped by over 60 percent, and the number of recipients who started a job each year jumped by over 100 percent.

There are many potential explanations for New York City's success in reforming its welfare programs. These explanations can be neatly divided into two categories, those related to the underlying economic conditions in New York City, and those related to changes in institutional factors, such as new policies or programs. Comparing the welfare caseload and the unemployment rate in New York City, one observes an inconsistent relationship between the two prior to and after 1995 (see Figure 1). For example, the caseload and unemployment rate both decreased from 1998 to 2001, but the caseload also decreased from 1995 to 1997 while the unemployment rate increased. These discontinuous changes appear to rule out the conclusion that the underlying economic conditions alone, as measured by the unemployment rate, can explain the success of welfare reform in New York City.

In 1999, New York City created the Employment Services and Placement (ESP) program, an innovative job training and outplacement assistance program for general assistance¹ (GA) recipients. Private contractors were hired to provide the services, and

¹ General assistance is welfare for childless adults.

then received a bounty for each job placement. This paper investigates whether there is sufficient evidence to conclude that the ESP program contributed to the success of welfare reform in New York City.

This study takes advantage of a quirk in the ESP program's administration. When the ESP program was initiated, the entire cohort of eligible recipients could not be enrolled simultaneously due to capacity constraints. Recipients were enrolled in waves where a new group was enrolled every two weeks, and this continued until all eligible recipients were enrolled.

This creates the opportunity to identify the effect of the ESP program using a natural experiment in which welfare recipients who were enrolled on a given date, designated as the "treatment group," are compared to those who were not enrolled on that date, designated as the "control group." For example, comparing recipients who were enrolled in the first wave to those who were not, one finds that enrolled recipients are more likely to start a job.

To confirm that the ESP program consistently increases the probability that a recipient will start a job, the comparison is repeated for each of the first ten enrollment dates. The observed employment effect peaks 168 days post enrollment, at which time members of the treatment group are, on the average, 44 percent more likely to start a job than are members of the control group (27.6% versus 19.1%). The observed effect then steadily declines to eight percent 728 days post enrollment (60.0% versus 55.5%).

This initial finding raises three important issues. First, are members of the treatment and control groups comparable? Second, should the post-peak decline in the effect of the ESP program be expected? And third, does starting a job imply that a

recipient will remain off welfare? These issues are addressed in detail in this paper. One should note, however, the following facts.

First, members of the treatment group were selected based on observable characteristics. There was not, however, systematic selection on unobservable characteristics². Consequently, by including covariates in the analysis, one should be able to adjust for the observed differences. When this is done, the estimated effect of the ESP program increases marginally.

Second, as more members of the control group are enrolled, over time, in the ESP program³, the percent that start a job should increase⁴ and the observed effect of the program should decrease⁵. When one partially adjusts for the fact that many members of the control group are ultimately treated, the effect of the ESP program peaks 182 days post enrollment at 55 percent (29.0% versus 17.5%) and then steadily declines to 11 percent 728 days post enrollment (60.0 % versus 53.7%). If a pure control group existed, i.e., one in which no members were subsequently enrolled in the ESP program, then the effect of the program would almost certainly be larger, peak later, and not decline as precipitously after peaking.

Third, this paper investigates whether recipients who start a job are able to remain off welfare. The results indicate that, conditional on starting a job, the ESP program

² Recipients were selected solely based on observable characteristics stored in a computer database.

³ An individual remains a member of the control group from a given enrollment date even if he or she is subsequently enrolled in the ESP program. Ultimately about half of the control group members are enrolled in the ESP program within one year of their inclusion in the control group.

⁴ The percent that start a job should increase since the ESP program increases the likelihood that a recipient will start a job.

⁵ The observed effect of the program should decrease since the observed effect is simply the difference in the percent of members of the treatment and control groups who start a job.

helps recipients exit welfare “permanently.”⁶ Thus, the analyses contained within this paper demonstrate that the ESP program has contributed to the overall success of welfare reform in New York City; the ESP program has helped to reduce the caseload and to increase the number of recipients who start a job.

There have been many recent studies that have attempted to determine whether the welfare reforms implemented in the 1990’s, including both the reforms of the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 and the pre-1996 waiver reforms authorized under the Family Support Act of 1988, have contributed to the recent decline in welfare caseloads nationwide⁷. In a review of this literature, Moffitt (2002) reports that the evidence “indicates some TANF⁸ effects in the expected direction but the small number of studies and problems in statistical inference make the conclusions rather uncertain.” In summarizing the findings of the pre-1996 waiver reform studies, he states that, “with a few exceptions, the studies show waivers to have had positive effects on most measures of labor supply and negative effects on measures of AFDC participation, as expected.” He notes, however, that in two exceptions, the researchers entered lagged caseload data into the regression and found that the pre-1996 waiver reforms had no effect on welfare caseloads. Thus the evidence to date regarding the success of welfare reform is mixed.

⁶ “Permanently” means that the recipient exits welfare and does not return within two years of enrollment in the ESP program. Only two years of follow-up data were available since the last enrollment date was June 15, 2000 and the data for this study was collected in July 2002.

⁷ Caseloads have declined by more than fifty percent, from over 14 million in 1995 to fewer than six million in 2001.

⁸ The Aid to Families with Dependent Children program was renamed the Temporary Assistance to Needy Families (TANF) program by the Personal Responsibility and Work Opportunity Reconciliation Act of 1996

Relative to those studies, the approach of this research is unique. This study measures quite precisely the time, down to the day, that a recipient was exposed to a specific reform. Moreover, this study uses disaggregate, administrative data to measure the outcome of the reform, e.g., the date a recipient exited welfare. These innovations made it possible to demonstrate that welfare reform can increase the probability that recipients will join the labor force and contribute to the decline in welfare caseloads.

In contrast, previous studies have relied upon less precise measures to identify the effect of welfare reform. For example, Blank (2001) uses the month in which all welfare recipients within a state were exposed to a bundle of welfare reforms. Additionally, most studies use aggregate data to measure the outcome of the reforms, e.g., changes in statewide caseloads. Such an approach limits one's ability to identify precisely the effects of welfare reform. For example, a state introduces a bundle of welfare reforms, some of which are effective and others that are not. Results from an analysis of the bundle of reforms necessarily underestimate the impact of the successful reforms.

In many ways this research is methodologically more similar to studies regarding the effect of short-term training programs. These studies, however, differ from the current study in several important respects. First, many of these programs did not target welfare recipients. Of those that did, most were not mandatory or implemented on such a large scale. Second, these studies focused on the programs' effect on mean annual earnings rather than on programs' effect on the likelihood that a recipient finds a job and exits welfare. Finally, none of these programs featured service providers who were paid strictly on a performance basis (Friedlander, 1997).

In general, these studies indicate that short-term training programs at best have a modest impact and are not worthwhile social investments (Heckman et al, 1999). Of the studies that featured training programs that were mandatory, the programs had a significant and positive impact on the mean annual earnings of female recipients of family recipients and no effect on the mean annual earnings of male recipients of family assistance⁹ (Friedlander, 1997). These programs were a worthwhile social investment for women but not for men.

This paper contributes to the growing literature on welfare reforms in several ways. First, it clearly demonstrates that welfare reform can have an effect on labor force participation and on welfare caseloads. Second, it identifies the effect of a single component of welfare reform, rather than the effect of welfare reform in general. No previous study has been able to do this¹⁰. Finally, it develops an innovative approach for identifying the effect of an intervention in which individuals are treated in waves.

The success of the ESP program raises some interesting issues. Why was the ESP program effective? And are the costs of the ESP program worth the benefits? These issues are explored in detail in the discussion section of the paper.

The next section provides a brief overview of recent welfare reforms in New York City as well as a detailed description of the ESP program. The third section discusses the strategy for identifying the effect of the ESP program and presents the key results. The fourth section discusses the implications of these findings and presents a brief cost benefit analysis of the ESP program.

⁹ Male recipients of family assistance received benefits through the AFDC-U program.

¹⁰ Moffitt (2002) finds that attempts to isolate the effects of various components of welfare reform, e.g., work requirement, sanction, and time limits, have been unsuccessful.

II. Welfare Reform in New York City, 1995-2000

A. A Brief Overview

In 1994, the newly elected mayor of New York City, Rudolph W. Giuliani, made reducing New York City's welfare caseload a priority. At the time, there were over one million welfare recipients in New York City, including approximately 700,000 who were receiving Family Assistance (FA)¹¹. In 1994, New York City spent approximately three billion dollars on welfare programs, including approximately two billion dollars on the FA program.

In early 1995, Mayor Giuliani initiated the New York City Work, Accountability, and You (NYCWAY) program for Home Relief (HR)¹² recipients. As the name suggests, a central feature of NYCWAY was that able-bodied HR recipients were expected to work in exchange for their benefits, i.e., to participate in a workfare assignment at a city agency to receive their benefits. Other components of NYCWAY included: enhanced detection of welfare fraud, mandatory treatment for welfare recipients who were substance abusers, and diversion from welfare to federally funded disability insurance where appropriate. No new state laws were required to implement NYCWAY since the program was completely compatible with New York State's pre-existing Social Services Law.

The initiation of NYCWAY coincided with the start of a steep decline in the number of HR recipients in New York City. One year after NYCWAY was

¹¹ FA was New York State's Aid to Families with Dependent Families (AFDC) program. The AFDC program was subsequently renamed the Temporary Assistance to Needy Families program (TANF) as part of the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA 96).

¹² HR was New York State's general assistance program, i.e., a welfare program for childless adults. The HR program was subsequently renamed the Safety Net Assistance (SNA) program.

implemented, the number of HR recipients had declined from 297,102 in March 1995, to 218,381 in March 1996, a decrease of over 25 percent; six years after the implementation the number of recipients had declined to 87,293, a decrease of over 70 percent (see Figure 2).

In April 1996, recipients of Family Assistance (FA) were enrolled in NYCWAY as well. One year later, the number of FA recipients had declined from 824,326 in March 1996 to 724,750 in March 1997, a decrease of over 10 percent; five years later the number of recipients had declined to 424,738, a decrease of almost 50 percent.

In late 1999, to enhance NYCWAY, and to mollify critics who complained that New York City's welfare reforms were too harsh and were not focused on helping recipients gain employment, the New York City Human Resources Administration (HRA) created the Employment Services and Placement (ESP) program, an innovative job training and outplacement assistance program for all welfare participants. Welfare participants were now expected to participate concurrently in the welfare and ESP programs.

At the same time, HRA created the Skills Assessment and Placement (SAP) program, a short-term, mandatory job training and outplacement assistance program for welfare applicants. Welfare applicants were now required to participate in the SAP program for four weeks, on the average, before their regular welfare benefits would commence. While enrolled in the SAP, program applicants received "single issue," non-recurring benefits.

The number of welfare recipients starting a job jumped from less than 50,000 in 1999 to over 125,000 in one year, an increase of over 150 percent (see Figure 3).

NYCWAY, by all appearances, seems to be a success. Since its inception the number of individuals receiving welfare in New York City declined from 1,160,593 in March 1995 to 459,056 in January 2002, a decrease of over 60 percent. In addition, in the years since 1999, when the job training and the outplacement assistance components were incorporated into NYCWAY, over 100,000 welfare recipients started a job each year; in the five prior years, 1994 – 1999, less than 50,000 recipients started a job each year.

B. The ESP Program

i. Description

In late 1999, after the ESP program was implemented, welfare recipients were required to participate in a workfare assignment three days a week and in the ESP program two days a week¹³. ESP program participants received job training and outplacement assistance. Eleven private contractors were hired to provide these services; they were collectively known as the ESP vendors. Eight of the ESP vendors were not-for-profit organizations, and three were for-profit organizations. All of the ESP vendors had a history of providing social services in New York City.

The ESP vendors were paid strictly on a performance basis for the job training and the outplacement assistance they provided. The fee that the ESP vendor received increased with the number of days that the participant retained a job and the wage that the participant received. Table 1 reports the fee structure. Eight of the eleven vendors received exactly these contractual rewards; the other three received very similar ones¹⁴.

¹³ Prior to the creation of the ESP program, welfare recipients were generally required to participate only in a workfare assignment 21 hours per week.

¹⁴ Each vendor negotiated the rewards with HRA.

The ESP vendors were free to provide as much, or as little, training as they saw fit. Since the vendors were paid only after a participant was placed in a job, there was little incentive for them to provide extensive training. Rather, they focused on developing the participants' "soft skills," e.g., résumé writing and interview skills. The vendors also provided assistance to participants in arranging interviews with potential employers. ESP vendors were required to attempt to place each participant¹⁵. However, if a participant had not been placed within six weeks, then the vendor was allowed to stop providing services to him or her¹⁶.

ii. Enrollment

Once the ESP program was fully implemented, enrollment was mandatory¹⁷ for all welfare recipients who were participating in the workfare program and deemed to be "job ready"¹⁸. In the summer of 1999, prior to the initiation of the ESP program, there were approximately 30,000 welfare recipients participating in the workfare program, including approximately 15,000 FA recipients. Once the ESP program was initiated, each of these "pre-existing" workfare participants was required to participate in the ESP program if he or she were still a workfare participant and job ready.

Between June 1999 and February 2000, private contractors evaluated the job readiness of 18,384 of the pre-existing workfare participants. Of these, 5,689, or 31

¹⁵ If a participant had not been placed within six months, then the vendor was required to stop providing services to him or her. This time limit was rarely enforced.

¹⁶ The percent of participants to which a vendor could stop providing services was capped at twenty percent.

¹⁷ If a participant failed to comply with the requirements of the ESP program, e.g., attendance requirements, then he or she was engaged in a conciliation process. The conciliation process could result in the recipient being reenrolled in the ESP program, being sanctioned for up to 180 days, or being exempted from participation in the ESP program.

¹⁸ The job readiness of each applicant for welfare benefits was now evaluated as part of the application process.

percent, were FA recipients. In total, 16,578 of the pre-existing workfare participants were deemed to be job ready, including 5,052 FA recipients.

The ESP program was launched in November 1999; initially only HR recipients were enrolled. FA recipients were first enrolled in the ESP program in February 2000. At the beginning of February 2000 there were 2,270 “eligible” FA recipients¹⁹. The ESP vendors could not accommodate all of these recipients at the same time. Consequently, eligible recipients were enrolled in the ESP program in waves, with a new group starting every two weeks.

iii. Selection of Eligible FA Recipients

Beginning on February 9, 2000, and every two weeks thereafter, a group of recipients was selected from among the eligible FA recipients for enrollment in the ESP program²⁰. FA recipients enrolled in the ESP program were instructed to report to an ESP vendor at a prescribed date and time. Typically, the reporting date was twelve days after the selection date.

Prior to each enrollment date, the ESP vendors were surveyed to ascertain the number of new participants each could accommodate. From the results of this survey, New York City could determine the total number of eligible FA recipients to select for enrollment in the ESP program on a given enrollment date.

To reduce the difficulty of participating in the ESP program, eligible FA recipients were generally assigned to ESP vendors within their borough of residence²¹.

¹⁹ A FA recipient is considered “eligible,” if he or she was one of the pre-existing workfare participants, was still enrolled in the workfare program, and had been deemed job ready.

²⁰ For example, on February 9, 130 of 2,270 eligible FA recipients were selected for enrollment in the ESP program.

²¹ New York City contains five boroughs; each borough is a county.

Thus the selection of eligible FA recipients for enrollment in the ESP program was stratified by borough. The selection process was supposed to be random on a given enrollment date within each borough (see Appendix A for a complete description of the selection process). The randomness of the selection process will be tested later in this paper.

iv. Description of the Available Data

In 2001, HRA created a “data warehouse,” a large database intended for analytic, rather than operational use. The data warehouse contains the case history and demographic characteristics of each individual who has received welfare in New York City since 1996. With HRA’s permission, the case history and demographic characteristics of each eligible FA recipient was extracted from the data warehouse (see Appendix B for a complete discussion of the available data).

III. Identifying the Effect of the ESP Program

Generally, to identify the effect of a program, one compares the outcomes of individuals who were enrolled in the program to the outcomes of individuals who were not. In the case of the ESP program, each individual who remained eligible was ultimately enrolled. Consequently, if one were to conduct such a comparison, he or she would be comparing a cohort of individuals who were eligible at the time of enrollment to a cohort who became ineligible prior to enrollment. These two groups are, unfortunately, not comparable since many of the reasons that one would become ineligible are self-determined²². Thus, such a comparison would produce a biased estimate of the program’s effect.

²² For example, when an individual exits welfare, he or she becomes ineligible.

It is possible, however, to estimate the effect of the ESP program by taking advantage of the initial, incremental enrollment of eligible FA recipients. Recall that the ESP vendors could not accommodate all the eligible FA recipients simultaneously; it took the ESP vendors over five months to enroll them all²³. Consequently, the effect of the program can be estimated using a natural experiment in which eligible FA recipients who were enrolled are compared to eligible FA recipients who were not enrolled on a given enrollment date.

A. The Effect of the ESP on the Probability of Starting a Job

Comparing the percent of eligible FA recipients who were selected for enrollment on February 9, 2000 and who start a job, to the percent who were not selected on that date and who start a job, one can see that the recipients who were selected are more likely to start a job within 140 days of enrollment than are those who were not (see Figure 4). To confirm that the ESP program consistently increases the likelihood that a recipient will start a job, this comparison is repeated for each of the nine remaining enrollment dates between February and June 2000. For each of these enrollment dates, the pattern is repeated, i.e., eligible FA recipients who were selected are more likely to start a job within 140 days of enrollment than are those who were not selected (see Figure 5).

By combining all the eligible FA recipients who were selected over the ten enrollment dates into one group, the treatment group, and by combining all the eligible FA recipients who were not selected on each enrollment date into another group, the

²³ As of June 15, 2000, 1,317 eligible FA recipients had been enrolled in the ESP program, 743 remained eligible, and 2,992 who were deemed job ready were ineligible.

control group²⁴, one can estimate the treatment effect (see Figure 6). Specifically, the treatment effect is defined as:

$$E[Y_i^T (T_i = 1)] - E[Y_i^T (T_i = 0)] \quad (1)$$

where $Y_i^T(g)$ is an indicator function which equals one if individual i starts a job within T days of the enrollment date and zero otherwise, and is a function of whether recipient i is a member of the treatment group, $T_i=1$, or the control group, $T_i=0$.

The treatment effect peaks 168 days after enrollment when members of the treatment group are 44 percent more likely to start a job than are member of the control group (27.6% versus 19.1% - see Figure 7). The effect then declines steadily to eight percent 728 days post enrollment (60.0% versus 55.5%).

This initial positive finding raises some issues that need to be addressed before its validity can be confirmed. First, are the treatment and control groups comparable? Second, should the treatment effect declining over time after peaking? And third, does starting a job imply that a recipient will stay off welfare? These issues are now discussed in detail.

B. The Comparability of the Control Group and the Treatment Group

Comparing the demographic characteristics of members of the treatment and control groups, one observes that the two groups have similar gender and racial distributions. The average age of recipients, and average number of years that a recipient has been continuously on welfare, are also very similar for the two groups (see Table 2). The borough of residence distribution, on the other hand, is more disparate, e.g.. while

²⁴ Note that many workfare participants were eligible and not selected on multiple enrollment dates, and thus, may be members of the control group multiple times.

30.0 percent of control group members are from Brooklyn; only 19.3 percent of treatment group members are from Brooklyn. This difference is not surprising, however, since the selection process was stratified by borough.

Actually, all of the observed differences in demographic characteristics between the two groups may be the result of the stratification, by borough and enrollment date, of the selection process. If the selection process was random on each enrollment date within a borough, then the demographic characteristics of the eligible FA recipients should not have significantly impacted the likelihood that a recipient was selected for the ESP program on a given date and in a given borough. To test whether this is the case, the probit equation below was estimated for recipients who were selected, or eligible but not selected, in each borough except Staten Island²⁵, i.e.,

$$P[T_i = 1] = F\left(\alpha + \sum_{c=1}^C \lambda_c x_{ic} + \varepsilon_i\right) \quad (1)$$

where T_i is a treatment dummy that equals one if individual i was selected and zero otherwise; and x_{ic} is a series of C demographic characteristics for individual i on the given enrollment date.

The coefficients from estimating equation (1) show that the selection process did approximate a random one. None of the 28 estimated coefficients are significantly different than zero with a p-value of less than 0.05 (see Table 4). Three of the coefficients are significantly different than zero with p-values between 0.05 and 0.10. This finding, however, is approximately what one would expect to find by chance. Thus there is no evidence that the selection process was not random.

²⁵ The probit was not estimated for Staten Island since only 30 recipient were treated during the 10 enrollment dates (see Table 3).

Moreover, eligible FA recipients were selected solely using information that was stored in a database. The selection process was centralized and conducted by computer programmers; individual caseworkers were not involved in any manner. In other words, the selection process was conducted without human discretion. Such a selection process, even if it was not random, should not disturb the necessary assumption that there was no systematic selection on unobserved characteristics. Consequently, by including covariates in the analysis of the treatment effect, one should be able to adjust for any observed differences.

C. The Effect of ESP Program Controlling for Observed Demographic Characteristics

To control for observed demographic characteristics, one needs to include these characteristics in the analysis. Specifically, a treatment dummy and a series of demographic characteristics is regressed on an outcome dummy, i.e.,

$$y_i^T = \alpha^T + \beta^T T_i + \sum_{c=1}^C \lambda_c^T x_{ic} + \varepsilon_i^T \quad (3)$$

where y_i^T is an outcome dummy that equals one if individual i starts a job within T days of enrollment and zero otherwise, T_i is a treatment dummy that equals one if individual i was treated and zero otherwise, and x_{ic} is a series of C demographic characteristics for individual i at the time of his or her inclusion in the control or treatment group.

Since a group of recipients was enrolled in the ESP program every 14 days, regression coefficients are estimated for values of T that are multiples of 14²⁶, starting

²⁶ This approach enables one to estimate a very general, non-parametric hazard rate.

with 14 and ending with 728²⁷. The regression coefficients are calculated using OLS, and corrected standard errors are calculated by clustering the observations by individual²⁸. The coefficients on the treatment dummy are positive and significantly different than zero for values of T between 28 and 728. The coefficients peak at 0.0845 ($t = 7.69$, $p = 0$), when T equals 168, indicating that recipients who were selected for the ESP program are 8.45 percentage points more likely to start a job 168 days post enrollment than are recipients who were eligible but not selected (see second column of Table 5). The coefficient on the treatment dummy then steadily declines to 0.0471 ($t = 3.80$, $p = 0$) when T equals 728.

Comparing the coefficients on the treatment dummy with demographic characteristics included as covariates in the regression to the coefficients on the treatment dummy without the demographic characteristics included in the regression, one observes that the inclusion of the covariates increases marginally the coefficients on the treatment dummy for all values of T between 14 and 728 (see Figure 8).

The coefficient on age is the only coefficient on a demographic characteristic that is significantly different than zero when T equals 168; i.e., it equals -0.0016 ($t = -1.98$, $p = 0.048$). This indicates that a recipient who is a decade older than another recipient has a 1.6 percentage point lower likelihood of starting a job 168 days post enrollment. The coefficient on time on welfare is the only coefficient on a demographic characteristic that is significantly different than zero when T equals 728, i.e., it equals 0.0066 ($t = 2.60$, $p = 0.009$). This indicates that a recipient who had been on welfare for one more year than

²⁷ The maximum number of days post enrollment for which data is available for all individuals in the study.

²⁸ This was necessary since some individuals appeared in the dataset repeatedly. Thus each observation was not independent of the others.

another recipient has a 0.66 percentage points higher likelihood of starting a job 728 days post enrollment. This outcome may be the result of the five-year lifetime time limit that was imposed on FA recipients after Personal Responsibility and Work Opportunity Reconciliation Act of 1996²⁹.

D. The Effect of ESP Program Controlling for Enrollment Date and Borough

One might be concerned that the stratification of the selection process, by borough and enrollment date, plays an important role in the estimated treatment effect. To eliminate this concern, borough dummies, enrollment date dummies, and interaction dummies³⁰ are added to (2), i.e.,

$$y_i^T = \alpha^T + \beta^T T_i + \sum_{c=1}^C \lambda_c^T x_{ic} + \sum_{j=1}^4 \delta_j^T B_{ij} + \sum_{k=1}^{16} \gamma_k^T E_{ik} + \sum_{j=1}^4 \sum_{k=1}^{16} \eta_{jk}^T (B_{ij} * E_{ik}) + \varepsilon_i^T \quad (4)$$

where y_i^T , T_i , and x_{ic} are defined as above; B_{ij} is a borough dummy that equals one if individual i resides in borough j and zero otherwise; and E_{ik} is an enrollment-date dummy that equals one if individual i was placed in the treatment or control group on enrollment date k and zero otherwise³¹.

The regression coefficients on the treatment dummies, age, and time on welfare only change marginally after adding these additional covariates (see the third and six columns of Table 5 as well as Figure 8). Thus the observed treatment effect is stable even after controlling for the underlying economic conditions, since the borough,

²⁹ The first FA recipients to exhaust their five year lifetime limit did so in 2002, the end of the period under study.

³⁰ The interaction that is estimated is between borough and enrollment date.

³¹ As before, regression coefficients are estimated for values of T that are multiples of 14, starting with 14 and ending with 728. Regression coefficients are calculated using OLS, and corrected standard errors are calculated by clustering the observations by individual.

enrollment date, and interaction dummies should control for any macroeconomic shocks that might have occurred during the period under study.

E. Adjusting for Control Group Members Subsequent Enrollment in the ESP Program

Ultimately approximately half of all control group members were enrolled in the ESP program³². As more control group members were enrolled, the percent that started a job should have increased³³ and the observed treatment effect should have decreased³⁴. Consequently, the previously estimated treatment effect is a conservative measure of the true treatment effect, that is, the effect that one would observe if a pure control group³⁵ existed.

Although such a control group does not exist, one can construct a proxy. This is accomplished by restricting membership in the control group to recipients who were eligible, but were neither selected on an enrollment date nor selected on subsequent enrollment dates. Such a control group with a one period restriction, denoted “control group one,” would include FA recipients who were eligible but not selected on February 9, 2000 and were not selected on February 24, 2000.

As described, control group one does introduce a selection problem. Specifically, control group one contains two cohorts:

Cohort 1. All members who were eligible but not selected on the initial enrollment date, and became ineligible prior to the subsequent enrollment date, and

³² 8,096 out of 16,550 control group members were enrolled in the ESP program. It took 83 days, on the average, to be enrolled, conditional on ultimately being enrolled.

³³ The percent that started a job should have increased since the ESP program increases the likelihood that a recipient will start a job.

³⁴ The observed treatment effect should have decreased since the treatment effect is simply the difference between the percent of treatment group member that start a job and the percent of control group members that start a job.

³⁵ A control group whose members were never enrolled in the ESP program.

Cohort 2. All members who were eligible but not selected on the initial and subsequent enrollment dates.

Members of cohort two are under-represented in control group one, since all of the recipients that were enrolled on the subsequent date were selected from among those who were still eligible on that date, and who would have become members of cohort two if they had not been selected³⁶. Thus the weight placed on members of cohort two needs to be increased so that the relative size of the two cohorts is what it would have been if no one had been enrolled on the subsequent enrollment date³⁷. Specifically, the weight placed on members of cohort two is the reciprocal of the probability, conditional on being eligible, of not being selected on the subsequent enrollment date (see Figure 9). This weighting scheme corrects the selection problem.

One can extend this approach to create additional, more restrictive control groups. Control group two, i.e., a control group that contains all recipients who were eligible but were neither selected on an enrollment date nor selected on the two subsequent enrollment dates, is constructed in an analogous manner to control group one. Since control group two imposes one additional restriction on membership, it contains one additional cohort:

- Cohort 1. All members who were eligible but not selected on the initial enrollment date, and became ineligible prior to the subsequent enrollment date,
- Cohort 2. All members who were eligible but not selected on the initial and subsequent enrollment dates, and became ineligible prior to the next subsequent enrollment date, and
- Cohort 3. All members who were eligible but not selected on the three enrollment dates.

³⁶ Recall that becoming ineligible is often self-determined.

³⁷ This is the case because members of cohorts one and two may have different characteristics; thus, to keep these new restricted control groups comparable to the treatment group, one needs to keep the relative weight placed on members of cohorts one and two equivalent to the initial control group.

Again, weights are assigned to members of cohorts two and three to adjust for the fact that members of these cohorts are under-represented. Specifically, the weight placed on members of cohort two is equal to the reciprocal of the probability, conditional on being eligible, of not being selected on the subsequent enrollment date. The weight placed on members of cohort three is equal to the reciprocal of the probability, conditional on being eligible, of not being selected on the two subsequent enrollment dates (see Figure 10).

This approach is extended further to create control groups six and thirteen as well. Comparing the probability of starting a job for members of the treatment group and members of control groups zero³⁸, two, six, and thirteen³⁹, one can see that the treatment effect becomes more pronounced as the length of the restriction increases (see Figure 11). Specifically, treatment effect 6 months⁴⁰ peaks 182 days post enrollment at 55 percent (29.0% versus 17.5%) and then steadily declines to 11 percent 728 days post enrollment (60.0 % versus 53.7%).

To control for observed demographic characteristics and local economic effects, regression coefficients are estimated for equation (4) using OLS with new control groups two, six, and thirteen⁴¹. The coefficients on the treatment dummy are positive and significantly different than zero for values of T between 28 and 728 for all control groups. Again, one observes that including covariates in the regression only marginally

³⁸ Control group zero is simply the control group with no restriction, i.e., the initial control group.

³⁹ These control groups were selected for analysis because they roughly represent a control group with a one-month restriction, i.e., control group two, a control group with a three-month restriction, i.e., control group six, and a control group with a six-month restriction, i.e., control group thirteen.

⁴⁰ Let treatment effect n months denote the treatment effect when one uses a n -month restriction on membership in the control group. Thus, treatment effect 0 months uses control group 0, treatment effect 1 month uses control group 2, treatment effect 3 months uses control group 6, and treatment effect 6 months uses control group 13.

⁴¹ As before regression coefficients are estimated for values of T that are multiples of 14, starting with 14 and ending with 728. Corrected standard errors are calculated by clustering the observations by individual.

changes the coefficients on the treatment dummy (see Figure 12). Thus the observed treatment effect is stable when controlling for underlying economic conditions.

Again the coefficient on time on welfare is positive and significantly different than zero when $T = 728$, i.e., it equals 0.0080 ($t = 2.58$, $p = 0.0100$) using control group six. This indicates that a recipient who had been on welfare for one more year than another recipient has a 0.8 percentage points higher likelihood of starting a job 728 days post enrollment (see Table 6). Again, this may be the result of the five-year lifetime time limit that was imposed on FA recipients after Personal Responsibility and Work Opportunity Reconciliation Act of 1996. The coefficient on white is only other coefficient that is significantly different than zero. It equals -0.1024 ($t = -1.90$, $p = 0.057$) when T equals 168, and it equals -0.1552 ($t = -1.91$, $p = 0.056$), indicating that non-white recipients are between 10 and 15 percentage points more likely to start a job than are white recipients.

Finally, one should observe that treatment effect 3 months is superimposed over treatment effect 1 month for the first 84 days post enrollment (see Figure 13). Thus, treatment effect 3 months does not diverge from treatment effect 1 month until well after the restriction on membership in control group two has expired; recall that treatment effect 1 month is estimated using control group two, which imposes a two period, or 28 day, restriction on control group members being treated. The same pattern emerges when one compares treatment effect 1 month to treatment effect 0 months. Thus, it seems likely that for at least 84 days post enrollment, treatment effect 3 months is the upper envelope of the true treatment effect. After that, the true treatment effect almost certainly diverges from, and lies above, treatment effect 3 months.

F. The Long-Term Effect of the ESP Program

Thus far the effect of the ESP program has been estimated by observing the difference in the probability of starting a job for treated and untreated individuals. The probability that an individual starts a job, however, is not the sole outcome for which the ESP program should be evaluated. For the ESP program to be considered a true success, it must not only increase the probability that a welfare recipient starts a job, but also increase the probability that he or she remains employed and off welfare.

Unfortunately HRA does not collect job retention data for all former welfare recipients. In truth, however, one is not concerned with whether the recipient retains his or her first job, but rather whether he or she remains off welfare and becomes a long-term member of the labor force. At this time, however, data regarding an individual's labor force participation is not available. Consequently, the long-term effect of the ESP program will be estimated by determining its effect on the probability that an individual starts a job and "permanently"⁴² exits welfare.

Regression coefficients are estimated for equation (4) using a new dependent variable designed to measure whether recipients permanently exit welfare after starting a job⁴³. Specifically, y_i^T is an outcome dummy that equals one if individual i starts a job and exits welfare within T days of enrollment and does not return to welfare during the period under study and zero otherwise. The coefficients are positive and significantly

⁴² Recall that "Permanently" means that the recipient exits welfare and does not return within two years of enrollment in the ESP program. Only two years of follow-up data were available since the last enrollment date was June 15, 2000 and the data for this study was collected in July 2002.

⁴³ As before regression coefficients are estimated for values of T that are multiples of 14, starting with 14 and ending with 728. Corrected standard errors are calculated by clustering the observations by individual.

different than zero for all T between 98 and 728 using control groups zero, two, six, and thirteen.

The treatment effect 6 months peaks at 0.061 ($t = 3.36$, $p = 0.001$), when T equals 616, indicating that recipients who are enrolled are 6.1 percentage points more likely to start a job, exit welfare and remain off of welfare than are recipients who were not treated (see Figure 14). Again one can see that the treatment effect becomes more pronounced as the length of the restriction increases.

It is also interesting to note that there is a period of approximately 70 days post enrollment during which the likelihood of starting a job and exiting welfare is the same for members of both the control and treatment groups. In contrast, at 70 days post enrollment, a member of the treatment group is already eight percentage points more likely to have started a job than is a member of the control group. This observed 70 day delay is caused by the fact that few recipients who started a job within 70 days of enrollment had their case closed within those 70 days. Further research is needed to determine the cause of this delay.

Finally, note that treatment effect 6 months is superimposed over treatment effect 3 months for the first 336 days post enrollment. Thus, treatment effect 6 months does not diverge from treatment effect 3 months until well after the restriction on membership in control group 6 has expired; recall that treatment effect 3 months is estimated using control group 6, which imposes a 6 period, or 84 day, restriction on control group members being treated.. The same pattern emerges when one compares treatment effect 3 months to treatment effect 1 month and treatment effect 1 month to treatment effect 0 months. Thus, it seems likely that for at least 336 days post enrollment, treatment effect

6 months is the upper envelope of the true treatment effect. After that, the true treatment effect almost certainly diverges from, and lies above, treatment effect 6 months.

G. Robustness Check

Finally, one might be concerned that job placements are better observed for members of the treatment group than for members of the control group⁴⁴. This is unlikely for two reasons. First, HRA set the ambitious goal that over 100,000 welfare recipients would find a job in 2000. To meet this goal, HRA needed to keep accurate records of which recipients found a job. Second, approximately half of all control group members were ultimately enrolled in the ESP program and became members of the treatment group.

To rule out the possibility that the observed treatment effect is the result of measurement error, and to confirm the robustness of the findings, the effect of the ESP program on the probability that a recipient will permanently exit welfare is estimated. Specifically, regression coefficients are estimated for equation (4) where y_i^T is an outcome dummy that equals one if individual i has permanently exited welfare within T days of enrollment and has been off welfare for at least six months by the end of the study⁴⁵ and equals zero otherwise. Regression coefficients are estimated for values of T that are multiples of 14, starting with 14 and ending with 546⁴⁶.

Treatment effect six months is positive for values of T between 84 and 546 (see Figure 15). It peaks at 0.0210 when T equals 308, indicating that treated recipients are

⁴⁴ One might be concerned that job placements are better observed for members of the treatment group than for members of the control group since ESP vendors were paid after a job placement was made.

⁴⁵ This restriction is added to prevent individuals who leave welfare for only a few months at the end of the period under study from being counted as having left welfare permanently.

⁴⁶ Corrected standard errors are calculated by clustering the observations by individual.

2.1 percentage points more likely to exit welfare than are untreated recipients 308 days post treatment. Treatment effect six months then steadily declines to 0.0110 when T equals 546. Even at its peak, however, treatment effect six months is not significantly different than zero (see Table 7).

Thus, the results of this robustness check are suggestive that the findings of this study are robust. In Ifcher (2004a), a similar robustness check is performed on recipients of General Assistance who are exposed to the ESP program. In that case, the ESP program had a positive and significant impact on the likelihood that treated General Assistance recipients exited welfare. The sample for that study was approximately four times as large as the sample for this study. Perhaps with a larger sample in this study the above suggestive results would have been significant as well.

Comparing the effect of the ESP program on the likelihood that a recipient will start a job and on the likelihood that a recipient will permanently exit welfare, one observes that the former effect is between six and a half and eight and a half percentage points greater than the latter effect. This difference is probably caused by the fact that there are many paths off welfare other than employment, e.g., diversion to federally funded disability insurance. It seems that control group members are more likely than treatment group members to take such alternative paths.

The coefficient on Asian, which equals -0.1686 ($t = -2.89$, $p = 0.004$) when T equals 308, is only coefficient that is significantly different than zero. Note that this coefficient has not been significantly different than zero in any of the other results and that less than one percent of the recipients in the study are Asian (162 out of 17,867).

As before, as the length of the restriction on membership in the control group increases, the treatment effect becomes more pronounced. Again, since many members of each control group are ultimately enrolled in the ESP program, the true treatment effect lies weakly above treatment effect six months.

One final result that is worth noting is that the treatment effect is negative and significantly different than zero when T equals 14 for all control groups. For example, treatment effect 6 months equals $-.0038686$ ($t = -3.50$, $p = 0.000$) when T equals 14, indicating that treated recipients are 0.38 percentage points less likely to exit welfare 14 days post treatment. This would seem to indicate that the recipients saw the ESP program as something that increased the value of being on welfare.

IV. Discussion

The ESP program appears to have a persistent positive impact on welfare recipients. That is, it increases the probability that they will start a job and permanently exit welfare. Specifically, the ESP program increases the likelihood that a recipient will start a job by between eight and eleven percentage points, and it increases the likelihood that a recipient will start a job and exit welfare by between four and six percent. Recall that the true treatment effect, if it could be measured, would almost certainly be larger.

A. Understanding the Results

A strength of this study is that every recipient who was selected for treatment was enrolled in the ESP program, i.e., there was no ability for recipients to avoid enrollment in the ESP program, thereby biasing the measured treatment effect. This is true since each recipient was considered enrolled in the ESP program the moment they were selected for enrollment. For example, a recipient who was enrolled in the ESP program

and never attended a single training session was still considered enrolled in the ESP program. Consequently, the measured treatment effect does not suffer from a self-selection bias⁴⁷.

A limitation of this study is that only job-ready FA recipients who were enrolled in the workfare program were included in this study. This limitation is the direct result of the design and implementation of the ESP program. Recall that the ESP program was only available for job ready recipients who were concurrently enrolled in the workfare program. Additionally, the identification strategy employed in this study utilized the initial enrollment of pre-existing, eligible recipients to identify the effect of the ESP program.

There are two separate ways in which this limitation may affect the conclusions one can draw about the effect of the ESP program. First, the measured treatment effect may be biased for non-job ready welfare recipients. This is probably not a major concern since finding employment for non-job ready recipients is, in theory, not an appropriate objective. Furthermore, almost all of the FA recipients who were enrolled in the workfare program were found to be job ready.

Second, the treatment effect may be biased for welfare recipients who are not enrolled in a workfare program. That is, the observed treatment effect might be the result of an interaction between the effect of the ESP program and the effect of the workfare program. For example, ESP program participants might gain some necessary, complementary skills from participating in the workfare program, which make them more

⁴⁷ This effect has been termed the “Intent to Treat” (ITT) effect; as opposed to the more common, and potentially, biased “Treatment on the Treated” (TOT) effect. See Katz, Kling, and Liebman (2001) for a more complete discussion of the ITT and TOT effects.

employable and able to exit welfare. From a policy perspective, this is probably not a major concern since concurrent workfare participation could be incorporated into any similar job-training program, if necessary.

Perhaps, the most intriguing topic for additional research is to investigate why the ESP program had any effect at all. Recall that the ESP vendors were paid solely on a performance basis and were free to provide as much, or as little, training and services as they saw fit. Consequently, most of the vendors focused on developing the participants' "soft skills," e.g., résumé writing and interview skills. In addition, most vendors employed job developers to find job openings and arrange job interviews for their participants. Consequently, one could claim that ESP program participants were not developing substantial new skills, e.g., carpentry skills.

Furthermore, the recipients in this study had unfavorable demographic characteristics, few skills, and many barriers to employment. For example, they were, on the average, 37 years old and had been on welfare continuously for over five years; over 95 percent were people of color; less than 30 percent had completed high school; less than five percent had a driver's license; and over 20 percent had a physical condition that limited them to clerical work, indoor work, or light custodial work. Yet, the ESP program was effective in moving individuals off of welfare into work.

One possible explanation for the effectiveness of the ESP program is that it substantially increased the cost, or disutility, of participating in welfare for some recipients. Specifically, the introduction of the ESP program increased the number of hours per week, from 21 to 35, that a recipient was required to spend in structured activities. As a result, recipients' net utility from receiving welfare decreased. This

decrease would have made exiting welfare and joining the labor force preferable for some recipients. If true, one implication of this explanation is that New York City could have engaged these individuals in any structured activity for the additional 14 hours per week and the effect would have been the same. The results from the robustness check, however, seem to indicate that this explanation is probably not correct. Recall that 14 days post enrollment treated recipients are significantly less likely to exit welfare than are untreated recipients, implying that enrollment in the ESP program does not, on average, increase the disutility of welfare recipients.

Another, more appealing, explanation for the effectiveness of the ESP program is that it enabled the participants to get a “foot in the door.” That is, once a participant was able to find and start a job, he or she was able, for whatever reason, to remain employed. In such a scenario, the recipient had, or quickly developed, the requisite skills to remain employed; and they had been on welfare simply because they lacked the ability to find and start a job. There are many avenues through which the ESP program could have helped participants get their “foot in the door.” For example, participating in ESP program may provide the recipient with the necessary “soft skills” to find a job; alternatively, participating in the ESP program may provide the recipient with good connections for finding a job; or finally, participating in the ESP program may send a signal to potential employers that the recipient is very eager to have the job.

One final possible explanation for the effectiveness of the ESP program is that it was a beneficial complement to the workfare program. In other words, the ESP program in conjunction with the workfare program not only helped the recipient get his or her “foot in the door,” but also helped the recipient to develop some basic work habits that

are necessary for remaining employed, e.g., being timely, being able to work as part of team, and being able to take orders from a superior.

B. Cost Benefit Analysis

Finally, one should consider whether the benefits of the ESP program outweigh the costs. The observed cost of a placement is approximately \$3,000. However, many of the participants who the vendors placed in a job would have found a job on their own. Consequently, to calculate the true cost of a “new” placement, i.e., a placement that would not have occurred without the ESP program, one needs to adjust for the fact that many of the recipients who were placed in jobs by the ESP vendors would have found a job on their own. For example, approximately 60 percent of treated recipients start a job and 54 percent of the untreated recipients start a job. Thus for every ten placements that the ESP vendors made, nine of the placed recipients would have found a job on their own. Consequently, to get one new placements, New York City had to pay the ESP vendor for ten placements. Thus, the cost per new placement is approximately \$30,000, or $(\$3,000 \times 10)$.

The fiscal benefits per placement are harder to calculate since they are time dependent, i.e., they depend on the length of time that a recipient remains off welfare and remains employed, and they vary by recipient, e.g., they depend on the amount of general assistance and food stamps the recipient was receiving. In early 2000, a recipient of family assistance, with three dependent children, received over \$600 per month in cash benefits plus up to \$508 per month in food stamps; this totals over \$13,000 per year⁴⁸. To this one must add administrative costs and the cost of providing Medicaid benefits,

⁴⁸ Note that if the recipient exits welfare and has a low enough income, he or she could still qualify to receive food stamps.

since all recipients of FA, and their dependents, automatically qualified for Medicaid benefits. Finally, if the placed recipient pays income taxes, that amount should also be added to the calculation of benefits per placement. In total, these additional monies must add at least two thousand dollars per year, and probably substantially more, to the benefits per placement; the value of Medicaid benefits alone is probably over two thousand dollars per year on the average. Thus, a conservative estimate of the fiscal benefits per year per placement is \$15,000. Consequently, it takes only two years for a new placement to pay for itself.

Unfortunately, only two years of post enrollment data are available at this time⁴⁹. Thus, it is impossible to determine whether or not the average recipient who starts a job remains off welfare for more than two years. At the end of the study, over half of the recipients who were treated and who started a job were still off welfare. These recipients have, on the average, already remained off welfare for approximately a year.

Previous studies have generally suffered from the same limitation and have projected the benefits of short-term training programs “into the future without any firm empirical basis” (Friedlander, 1997). Without such projections, the benefits of these programs would not have exceeded the costs. For example, one could assume that one-third of the recipients who were treated, who started a job, and who were still off welfare at the end of the study return to welfare in each subsequent year. With such an assumption, the fiscal benefits of the ESP program surpass the costs. With the assumption that half of all recipients return to welfare in each subsequent year, the fiscal benefits of the program equal the costs.

⁴⁹ The longer-term effect of the ESP program is unfortunately unknown at this time since the ESP program was created less than three years ago.

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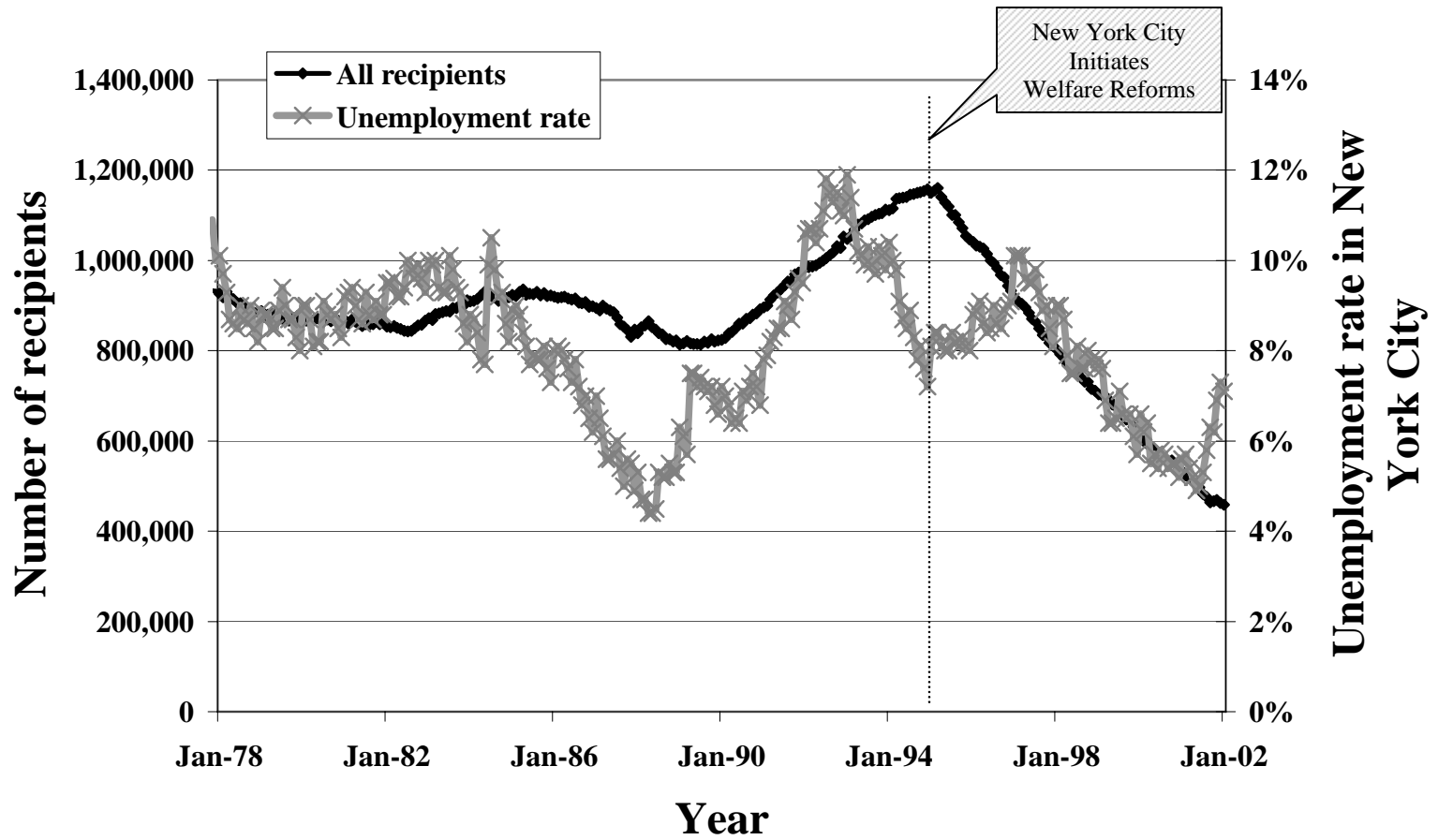
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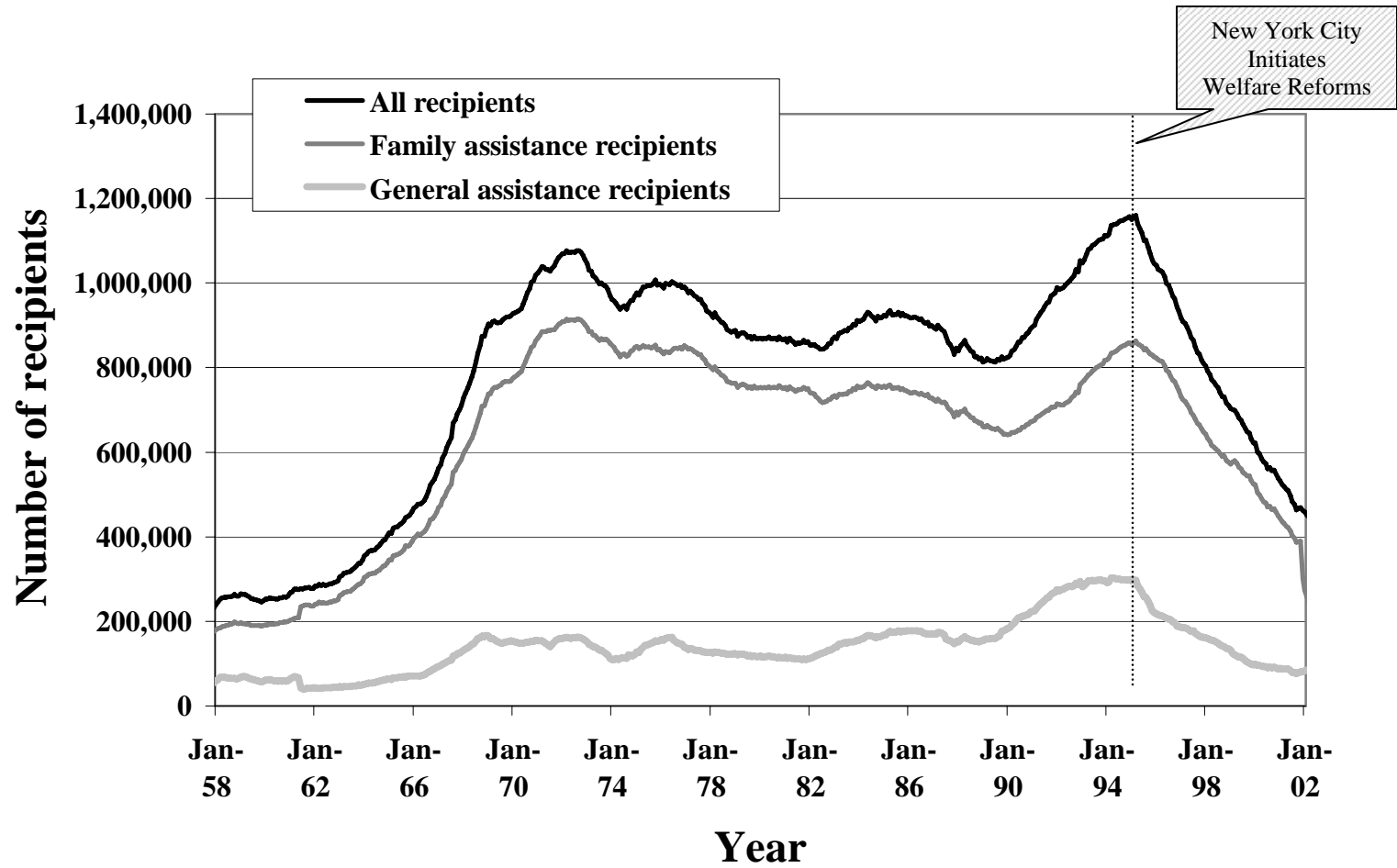
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Figure 1: The Unemployment Rate and the Welfare Caseload in New York City, January 1978 –January 2002



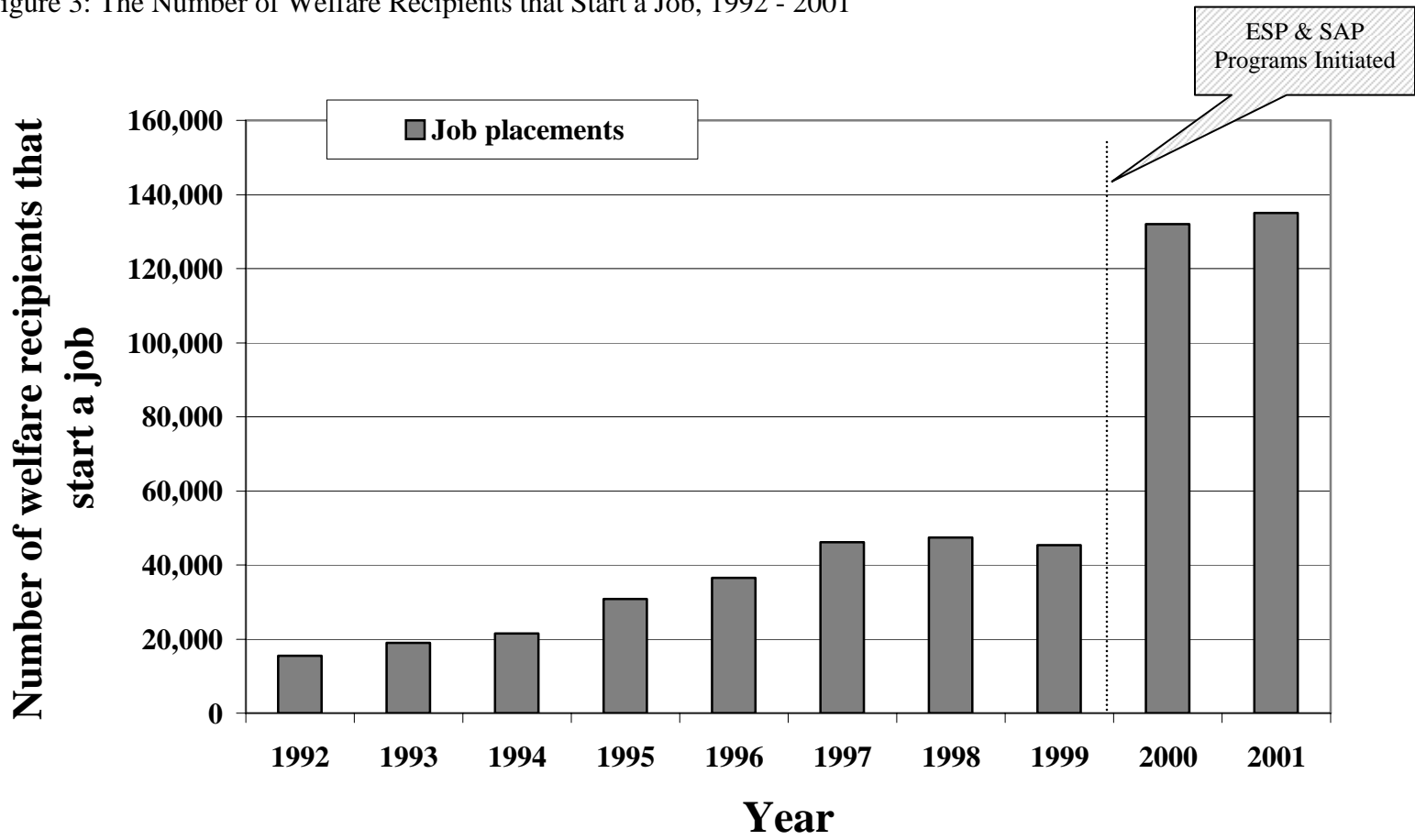
Source: Office of Policy and Program Analysis, New York City Human Resources Administration

Figure 2: The Welfare Caseload in New York City, January 1958 – January 2002



Source: Office of Policy and Program Analysis, New York City Human Resources Administration

Figure 3: The Number of Welfare Recipients that Start a Job, 1992 - 2001



Source: Office of Policy and Program Analysis, New York City Human Resources Administration

Figure 4: The Percent of Eligible FA Recipients that Start a Job, February 9, 2000

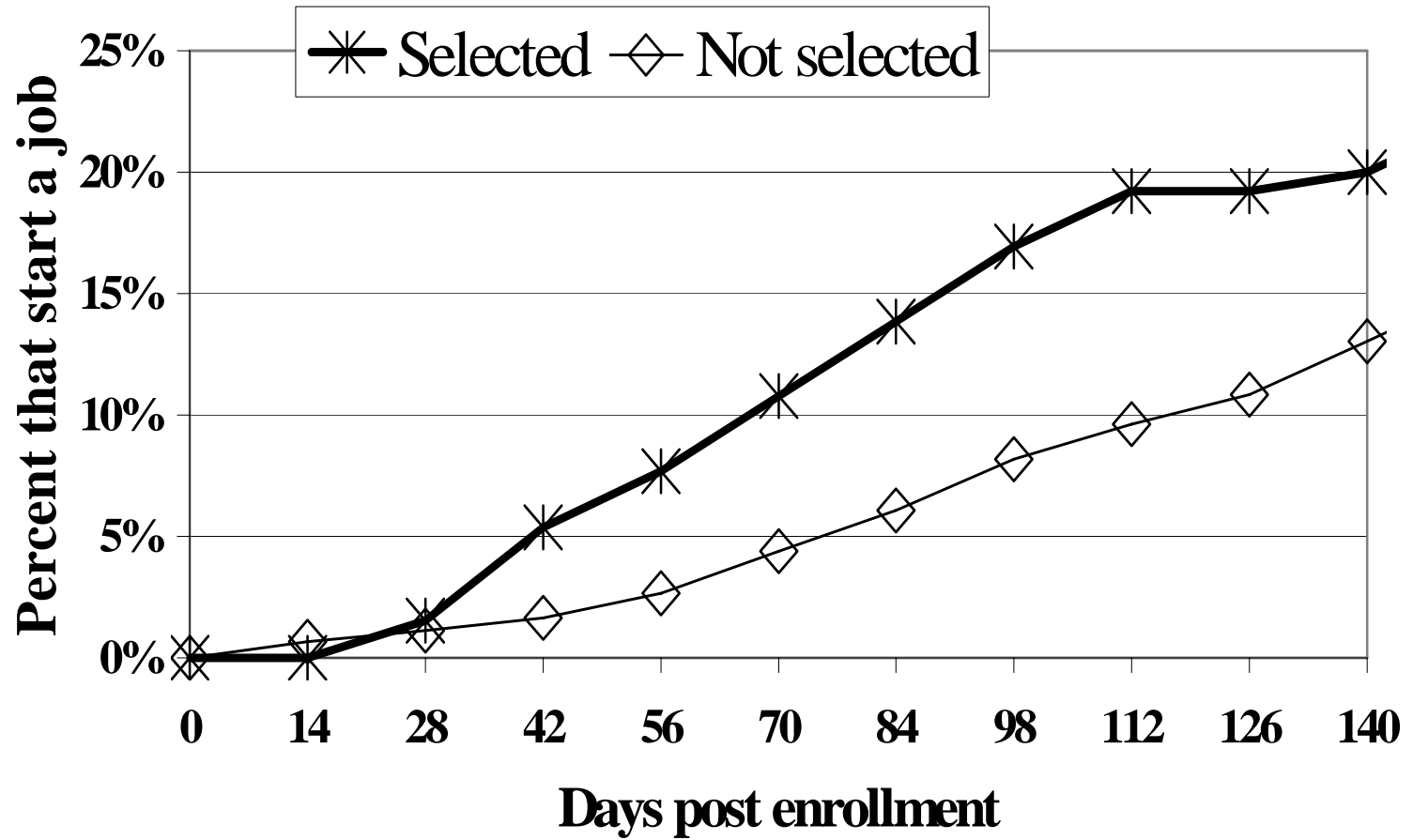


Figure 5: The Percent of Eligible FA Recipients that Start a Job, Six Dates With Largest Enrollment

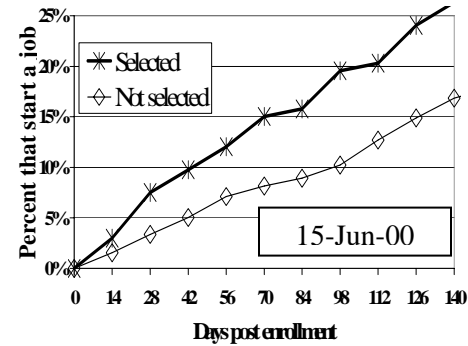
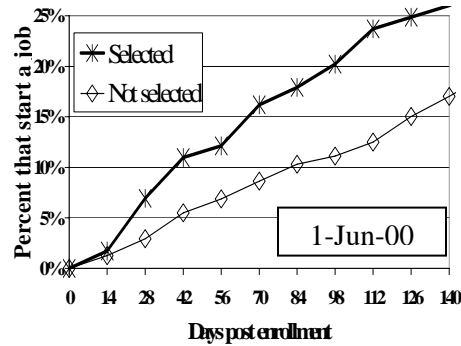
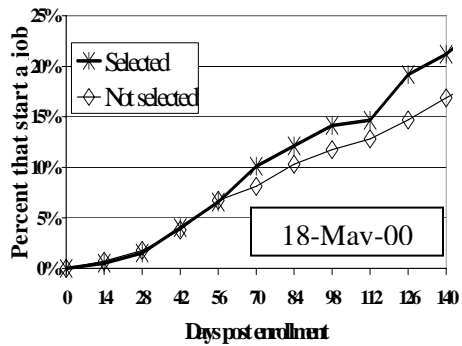
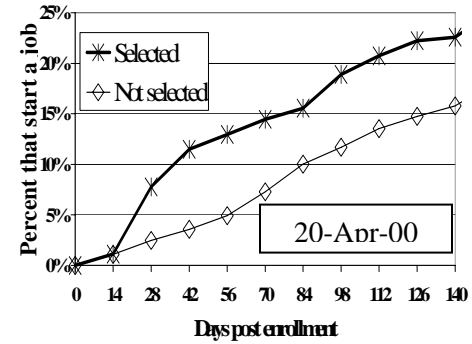
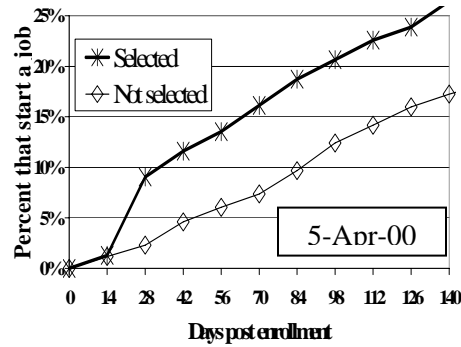
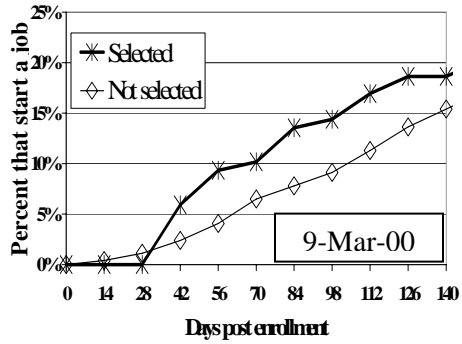


Figure 6: Formation of the Treatment Group and the Control Group

The Treatment Group:

All recipients selected for the ESP program on any of the ten selection dates between February and June 2000. Specifically it is the union of T1, T2, T3,, T10.

The Control Group:

All recipients who were eligible but not selected on any of the ten enrollment dates between February and June 2000. Specifically it is the union of C10, C20, C30,, C100. Note that many recipients were eligible but not selected on multiple dates. Consequently, some recipients are members of the control group multiple times.

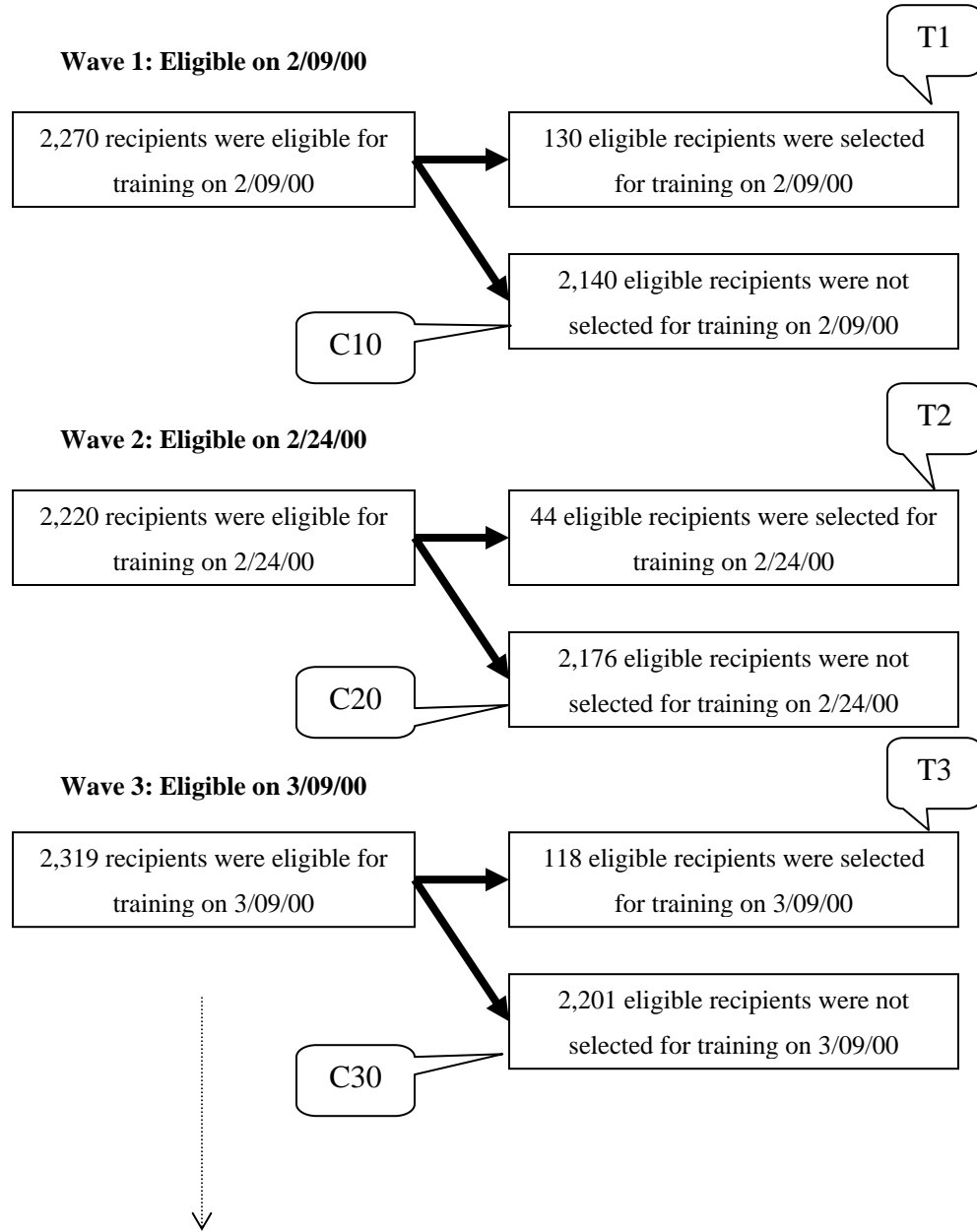


Figure 7: The Percent of Eligible FA Recipients that Start a Job Comparing Treatment Group and Control Group

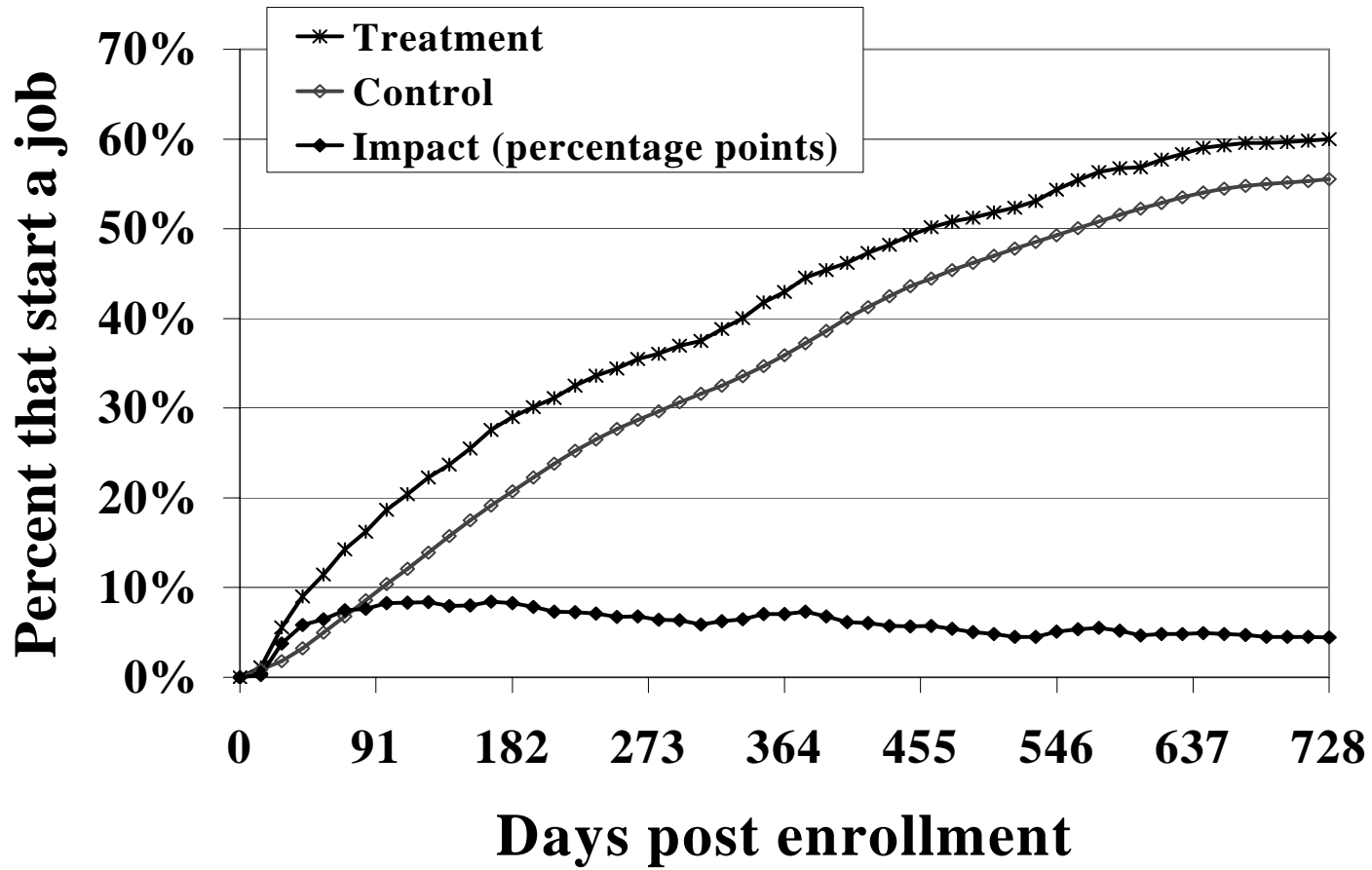


Figure 8: Coefficient on Treatment Dummy with Various Explanatory Variables Included

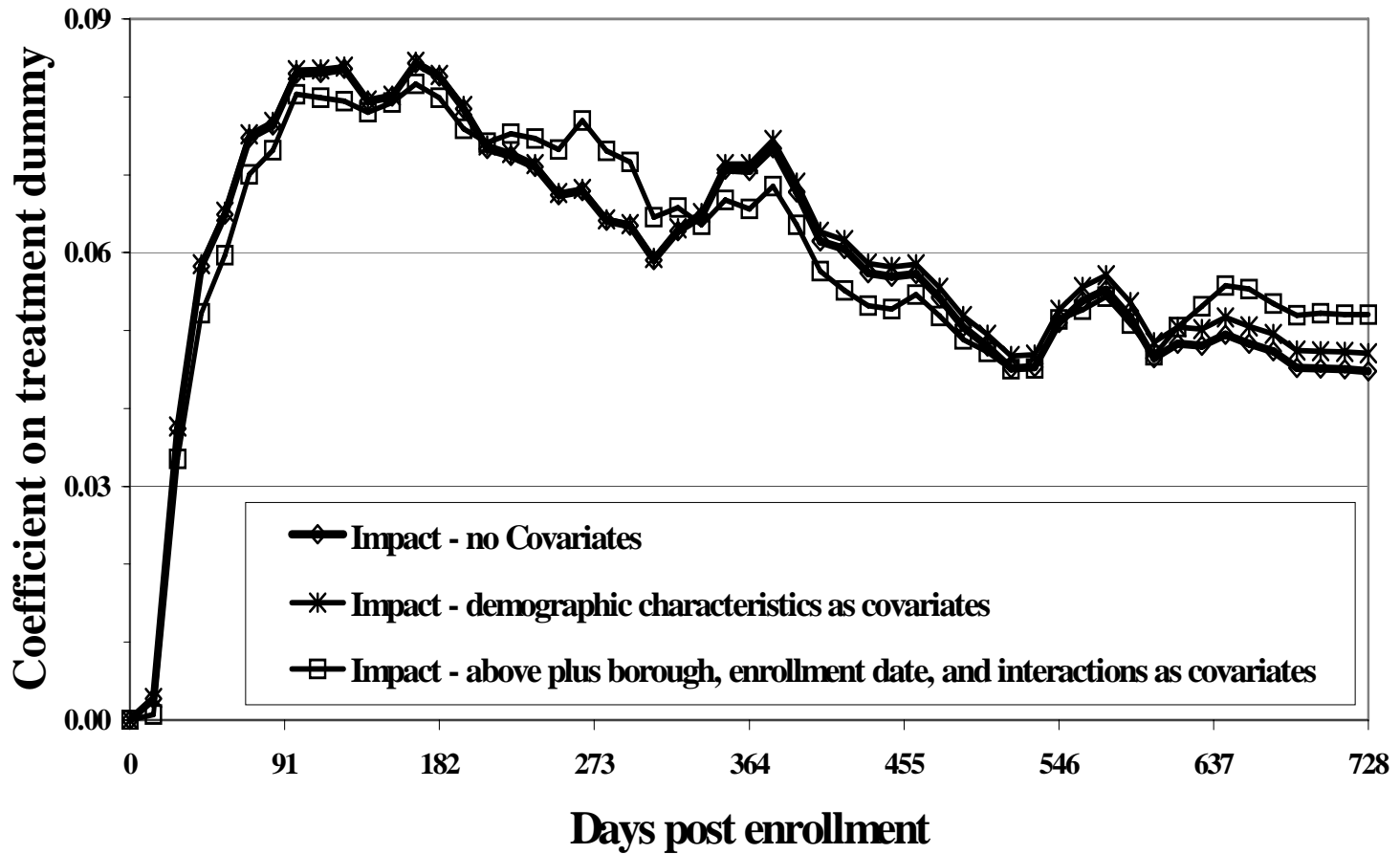


Figure 9: Formation of the Treatment Group and Control Group One

The Treatment Group:

All recipients selected for the ESP program on any of the ten selection dates between February and June 2000. Specifically it is the union of T1, T2, T3,, T10.

Control Group One:

All recipients who:

1. Were eligible but not selected on any of the ten enrollment dates between February and June 2000 and
2. Were not selected on the subsequent enrollment date.

Specifically it is a weighted union of C11a, C11b, C21a, C21b, C31a, C31b,, C101a, C101b. The weights on the a-series control groups are equal to one. The weights on the b-series control groups are equal to one divided by the percent of eligible recipients who were not selected on the subsequent enrollment date for each cohort, e.g., for C11b the weight equals one divided by 98.0 percent or 1.020.

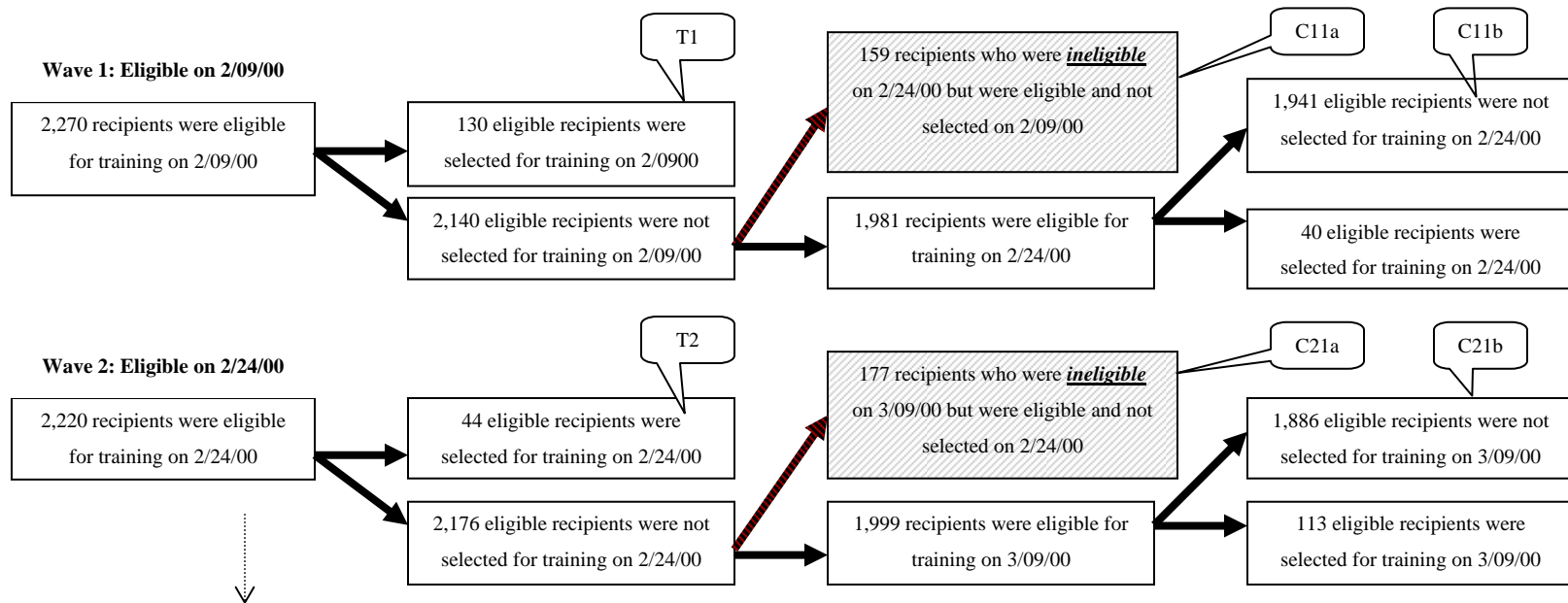


Figure 10: Formation of the Treatment Group and Control Group Two

The Treatment Group:

All recipients selected for the ESP program on any of the ten selection dates between February and June 2000. Specifically it is the union of T1, T2, T3,, T10.

The Control Group With A One-Period Selection Restriction:

All recipients who:

3. Were eligible but not selected on any of the seventeen enrollment dates between February and June 2000, and
4. Were not selected on the two subsequent enrollment dates.

Specifically it is a weighted union of C11a, C11b, C11c, C21a, C21b, C21c, C31a, C31b, C31c,, C101a, C101b, C101c. The weights on the a-series control groups are equal to one. The weights on the b-series control groups are equal to one divided by the percent of eligible recipients who were not selected on the subsequent enrollment date for each cohort, e.g., for C11b the weight equals one divided by 98.0 percent or 1.020. The weights on the c-series control group are equal to one divided by the product of the percent of eligible recipients who were not selected on the two subsequent enrollment dates for each cohort, e.g., for C11c the weight equals one divided by the product of 98.0 percent and 94.0 percent, or 1.085.

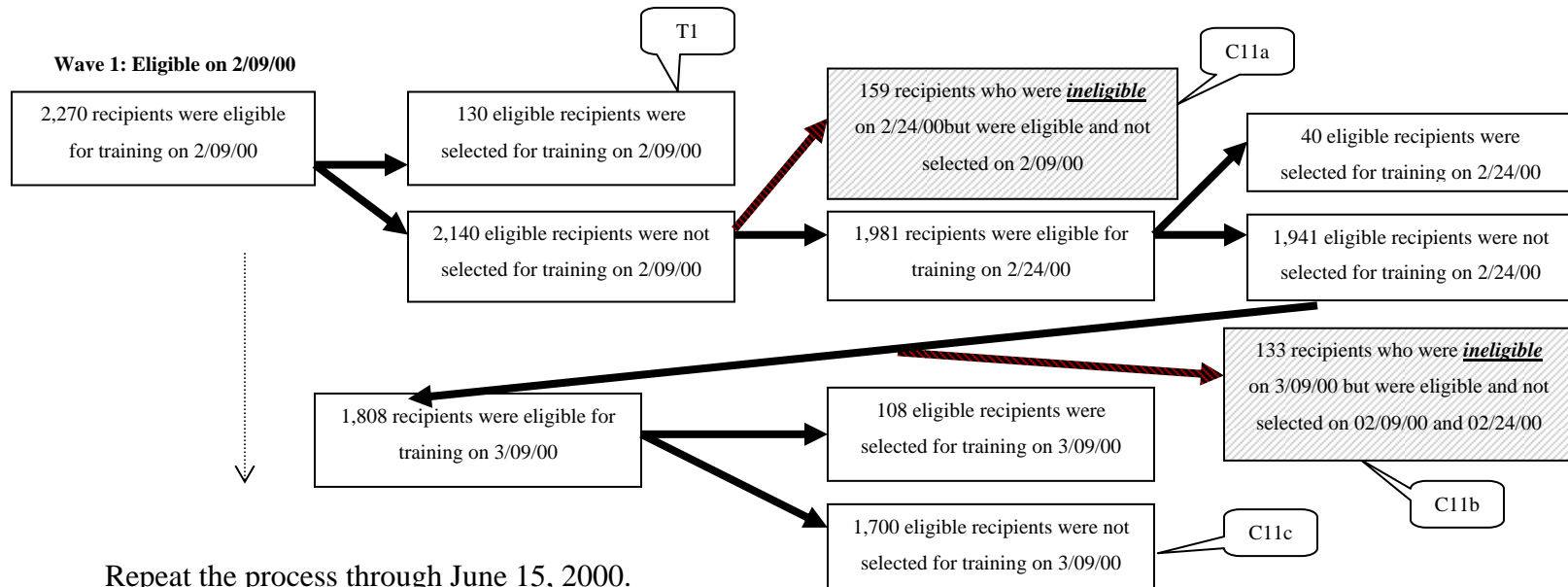


Figure 11: The Percent of Eligible FA Recipients that Start a Job, Control Groups Zero, One, Three, and Six Months

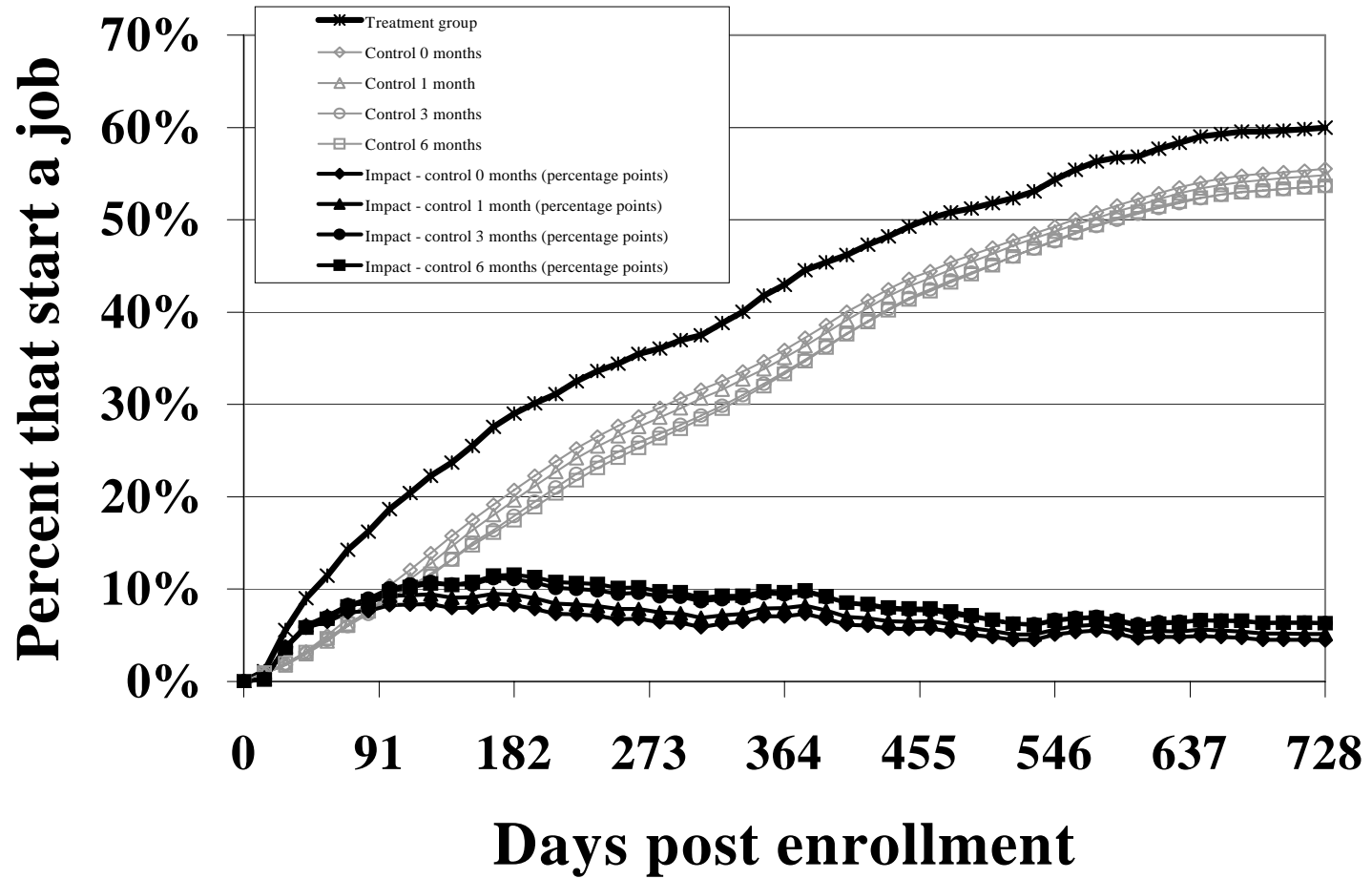


Figure 12: Coefficient on Treatment Dummy, Treatment Effects Zero, One, Three, and Six Months, With and Without Covariates

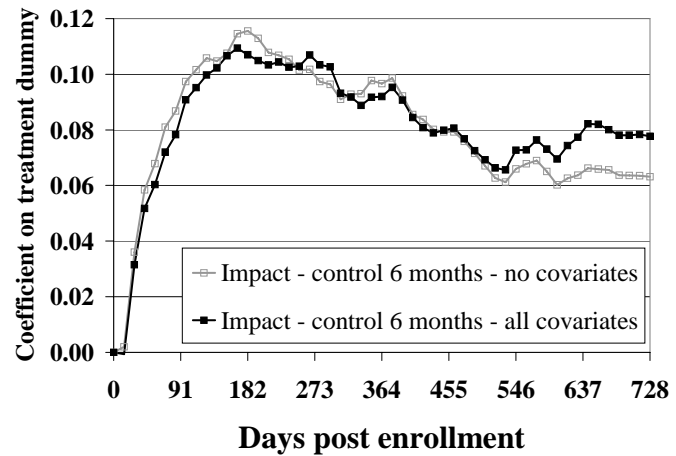
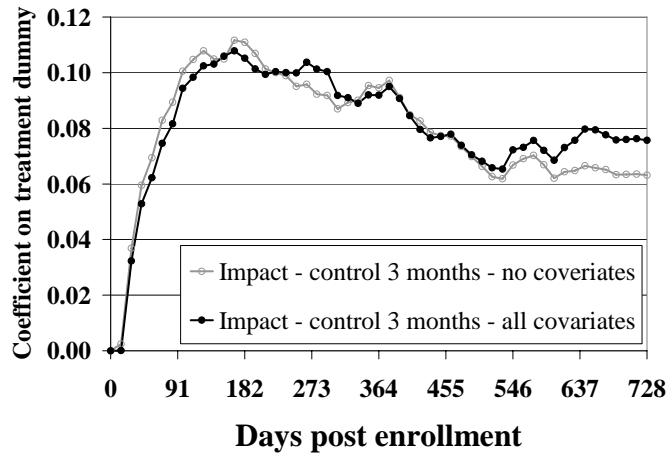
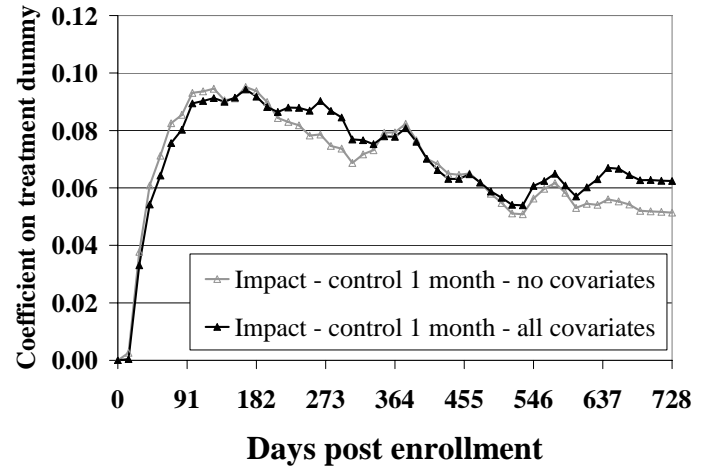
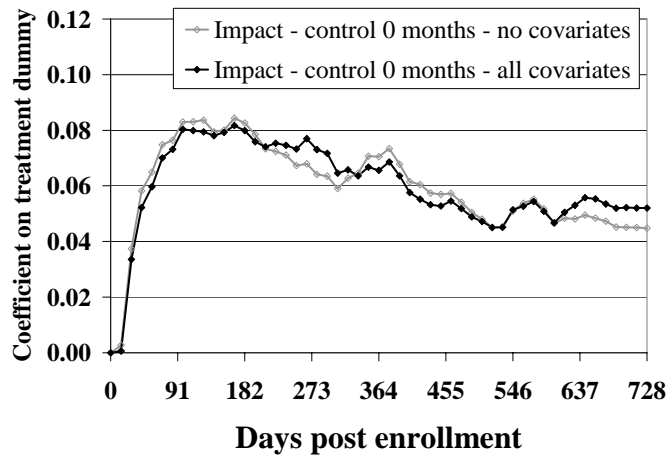


Figure 13: Coefficient on Treatment Dummy, Treatment Effects Zero, One, Three, and Six Months, With and Without Covariates

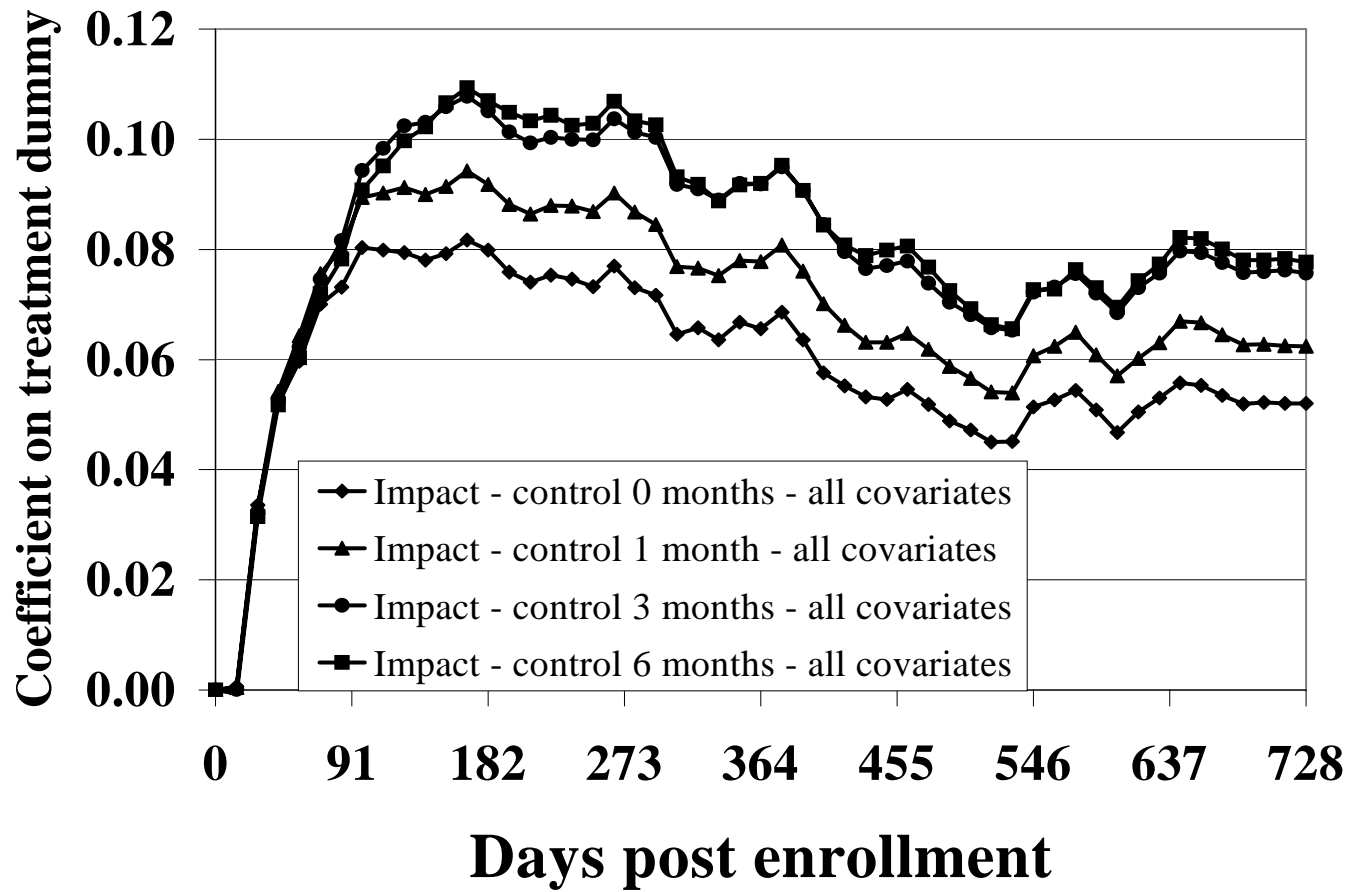


Figure 14: The Percent of Eligible FA Recipients that Start a Job and Permanently Exit Welfare

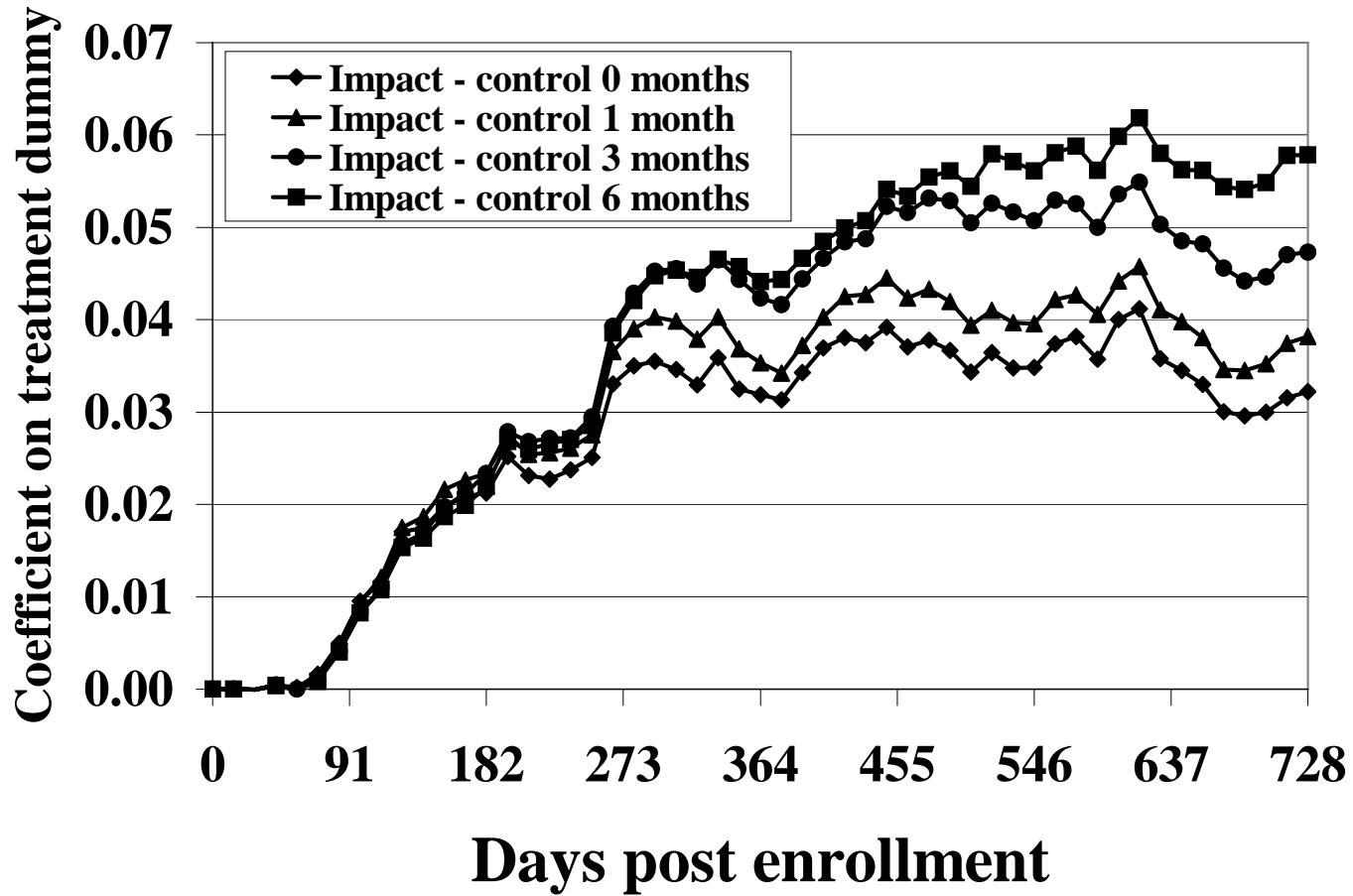


Figure 15: Coefficient on Treatment Dummy, Dependent Variable is Permanently Exit Welfare

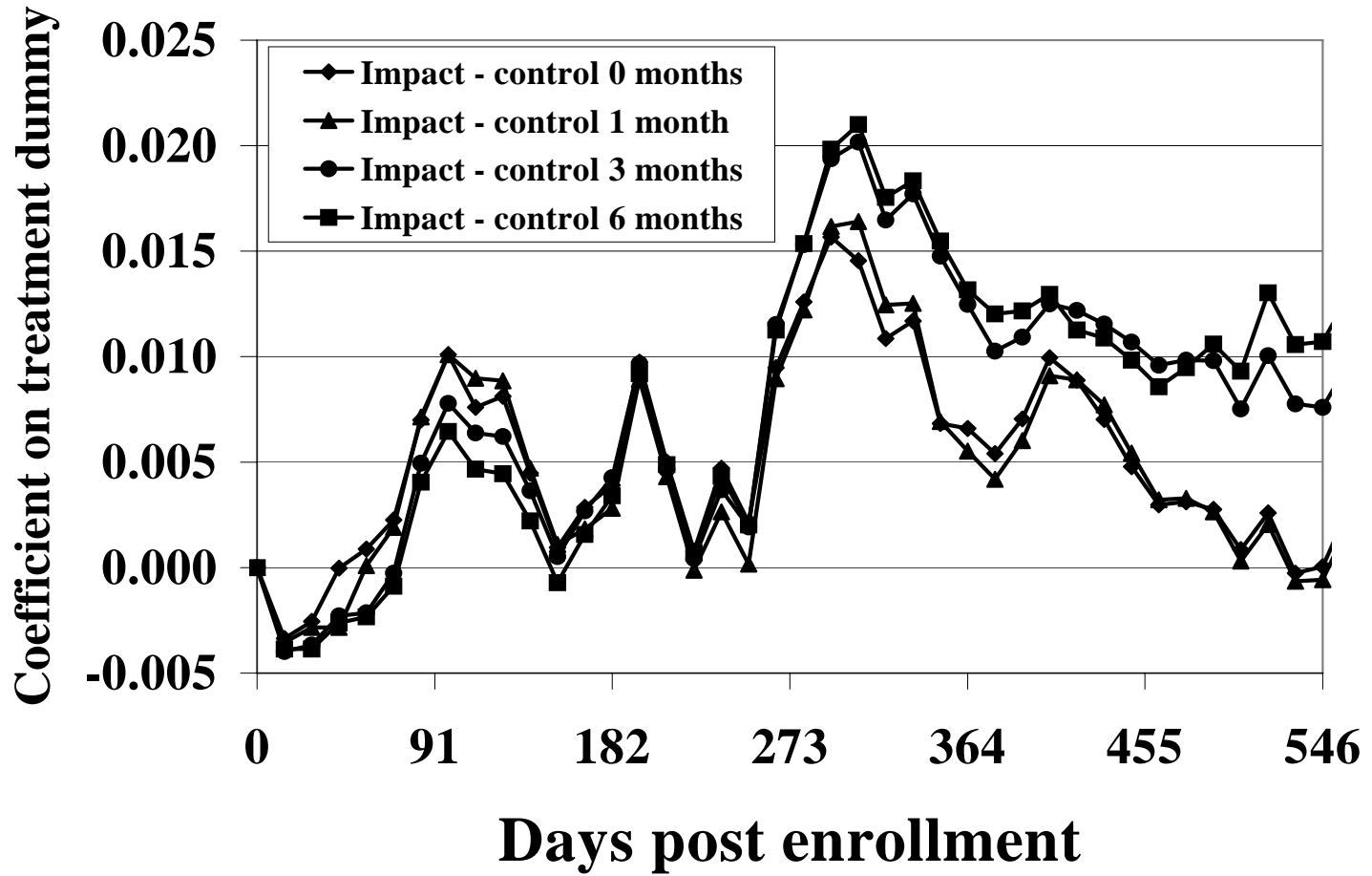


Table 1: Fee Structure for ESP Vendors

Milestone	Description	Reward
Placement Fee	Paid when a recipient was placed in a job	\$2,000
90-Day Retention Bonus	Paid when a recipient stayed in a job for 90 days	\$1,500
90-Day High Wage Bonus	Paid when a recipient stayed in a “high wage” job for 90 days	\$500
180-Day Retention Bonus	Paid when a recipient stayed in a job for 180 days	\$500
180-Day High Wage Bonus	Paid when a recipient stayed in a “high wage” job for 180 days	\$1000

Table 2: Demographic Characteristics of Members of the Treatment and Control Groups

Demographic Characteristic	Treatment	Control
Observations	1,317	16,550
Male	5.6%	6.0%
Race		
Asian	0.9%	0.9%
Black	42.0%	44.9%
Hispanic	47.5%	45.5%
White	6.4%	4.5%
Borough of residence		
Bronx	38.0%	43.3%
Brooklyn	19.3%	30.0%
Manhattan	19.7%	15.2%
Queens	20.5%	10.3%
Average age	37.5	37.4
	(8.39)	(8.43)
Average number of years continuously on welfare	5.1	5.2
	(3.8)	(3.87)

Table 3: Number of Enrollees by Borough and Enrollment Date

Enrollment Date	Bronx			Brooklyn			Manhattan			Queens			Staten Island			Borough Not Reported			All Boroughs		
	Not Enrolled	Enrolled	Total	Not Enrolled	Enrolled	Total	Not Enrolled	Enrolled	Total	Not Enrolled	Enrolled	Total	Not Enrolled	Enrolled	Total	Not Enrolled	Enrolled	Total	Not Enrolled	Enrolled	Total
9-Feb-00	970	14	984	515	13	528	418	54	472	203	43	246	32	6	38	2	0	2	2,140	130	2,270
24-Feb-00	1005	11	1016	513	0	513	387	23	410	239	8	247	30	2	32	2	0	2	2,176	44	2,220
9-Mar-00	992	35	1027	575	35	610	336	42	378	269	6	275	27	0	27	2	0	2	2,201	118	2,319
22-Mar-00	951	1	952	648	2	650	238	0	238	253	36	289	29	1	30	1	0	1	2,120	40	2,160
5-Apr-00	821	76	897	593	3	596	247	4	251	183	69	252	26	3	29	1	0	1	1,871	155	2,026
20-Apr-00	589	211	800	560	32	592	237	6	243	173	2	175	6	19	25	1	0	1	1,566	270	1,836
4-May-00	544	13	557	526	20	546	228	5	233	149	18	167	6	0	6	1	0	1	1,454	56	1,510
18-May-00	477	63	540	445	53	498	187	52	239	115	30	145	8	0	8	1	0	1	1,233	198	1,431
1-Jun-00	450	17	467	367	59	426	125	49	174	66	48	114	8	0	8	1	0	1	1,017	173	1,190
15-Jun-00	368	60	428	229	37	266	112	24	136	56	10	66	5	2	7	2	0	2	772	133	905
Total	7167	501	7668	4971	254	5225	2515	259	2774	1706	270	1976	177	33	210	14	0	14	16,550	1,317	17,867

Table 4: Coefficient on Demographic Characteristics from Probit Equation Estimation

Demographic Characteristic	Bronx	Brooklyn	Manhattan	Queens
Male	-0.2900	0.6000	0.1700	-1.0400
Race				
Asian	-0.3100	1.1800	0.0300	-1.3000
Black	0.3800	1.8500 *	0.1100	-1.4900
Hispanic	0.2800	1.6700 *	0.5500	-0.5400
White	0.8000	0.8900	0.4100	-0.5700
Average age	-0.0400	-0.5700	-0.2900	1.5900
Years on welfare	1.0900	0.0900	-1.7700 *	-0.4900

* signifies that $p < 0.10$, ** signifies that $p < 0.05$, and *** signifies that $p < 0.01$

Table 5: Coefficients with Various Covariates Included, $T = 168$ (Peak Effect) and $T = 728$

	168 days post enrollment			728 days post enrollment		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Dummy	0.0843 ***	0.0845 ***	0.0817 ***	0.0448 ***	0.0471 ***	0.0521 ***
Male		0.0227	0.0248		-0.0342	-0.0314
Race						
Asian		-0.0350	-0.0476		-0.0578	-0.0615
Black		-0.0178	-0.0315		0.0154	0.0092
Hispanic		-0.0052	0.0006		-0.0322	-0.0270
White		-0.0131	-0.0484		-0.0506	-0.0732
Average Age		-0.0016 **	-0.0015 *		-0.0010	-0.0009
Time on Welfare		0.0012	0.0012		0.0066 ***	0.0066 **
Borough Dummies	No	No	Yes	No	No	Yes
Enrollment Date Dummies	No	No	Yes	No	No	Yes
Interaction Dummies	No	No	Yes	No	No	Yes

* signifies that $p < 0.10$, ** signifies that $p < 0.05$, and *** signifies that $p < 0.01$

Table 6: Coefficients from Estimating Equation (4) with Control Group Six, $T = 168$ (Peak Effect) and $T = 728$

	168 days post enrollment			728 days post enrollment		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Dummy	0.1116 ***	0.1123 ***	0.1078 ***	0.0631 ***	0.0673 ***	0.0757 ***
Male		-0.0121	-0.0126		-0.0719	-0.0713
Race						
Asian		-0.0539	-0.0523		-0.0847	-0.0696
Black		-0.0634	-0.0653		-0.0169	-0.0176
Hispanic		-0.0258	-0.0270		-0.0669	-0.0595
White		-0.1005 *	-0.1024 *		-0.1532 *	-0.1552 *
Average Age		-0.0010	-0.0010		-0.0003	-0.0004
Time on Welfare		0.0010	0.0009		0.0082 ***	0.0080 **
Borough Dummies	No	No	Yes	No	No	Yes
Enrollment Date Dummies	No	No	Yes	No	No	Yes
Interaction Dummies	No	No	Yes	No	No	Yes

* signifies that $p < 0.10$, ** signifies that $p < 0.05$, and *** signifies that $p < 0.01$

Table 7: Coefficients from Estimating Equation (4) with Control Group Six, $T = 308$ (Peak Effect) and $T = 546$

	308 days post enrollment			546 days post enrollment		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Dummy	0.0266	0.0245	0.0210	0.0287	0.0270	0.0107
Male		0.0877	0.0886		0.0911	0.0959
Race						
Asian		-0.1704 ***	-0.1686 ***		-0.0752	-0.0733
Black		-0.0013	-0.0072		-0.0419	-0.0530
Hispanic		0.0602	0.0683		0.0117	0.0202
White		0.0532	0.0331		0.0095	-0.0257
Average age		0.0003	0.0005		0.0007	0.0009
Time on Welfare		-0.0019	-0.0020		-0.0021	-0.0021
Borough Dummies	No	No	Yes	No	No	Yes
Enrollment Date Dummies	No	No	Yes	No	No	Yes
Interaction Dummies	No	No	Yes	No	No	Yes

* signifies that $p < 0.10$, ** signifies that $p < 0.05$, and *** signifies that $p < 0.01$

Appendix A: A Detailed Description of the Selection Process

The selection process was supposed to be random on a given enrollment date within a borough. The following three-step query was used to select which of the eligible FA recipients would be enrolled in the ESP program on a given date:

1. A list of all eligible FA recipients was assembled from a table of all FA recipients. The table was indexed on the fifth, sixth, and seventh digit of each participant's eight-digit Case Identification Number,
2. The list resulting from step 1 was sorted by the participant's borough of residence, and
3. From the list of eligible FA recipients for each borough resulting from step 2, participants were chosen for enrollment in the ESP program starting with the first individual on the list and continuing down the list until all the available slots within a borough were filled; all of the eligible FA recipients who were not selected on that date were not enrolled in the ESP program on that date.

The computer programmers who designed this query believed that using it would result in a random group of eligible FA recipients being selected on a given date within a borough. They believed this to be true since sorting by the fifth, sixth, and seventh digit of the eight-digit Case Identification Number should have placed the recipients in a physical order that was random. Furthermore, sorting this list by borough should not have disturbed the underlying randomness of the list; thus the list for each borough should have also had a physical order that was random. Choosing individuals for enrollment in the ESP program from the top of each borough's list should have been equivalent to randomly selecting individuals for enrollment in the ESP program within a borough on a given enrollment date.

Appendix B: A Discussion of the Available Data

New York City and New York State maintain several databases to manage the welfare system. These include New York State's Welfare Management System (WMS), and HRA's New York City Work, Accountability, and You (NYCWAY) database.

In 2001, HRA created a data warehouse, i.e., a large database that contains information extracted from multiple source systems. Data warehouses are generally designed for analytic use, rather than operational use. They are an ideal source of data for research since the information that is contained within a data warehouse is stable. That is, while new data is added periodically, old data is never updated nor removed.

HRA's data warehouse, the Enterprise Data Warehouse (EDW), contains data extracted from multiple database systems including the WMS and the NYCWAY database. Information regarding all individuals who have received welfare in New York City since 1996 is available in the EDW. The information in the EDW is stored in a series of data tables. For example, there is the recipient data table, which contains a record for each individual who has received welfare in New York City since 1996; and there is the address data table, which contains a record for each address that each welfare recipient has had since 1996. Each record in each table contains a unique identifier so that the information contained in the record can be linked to the proper individual.

The data for this study was extracted from the EDW using a computer language that is similar to SQL (structured query language)⁵⁰. All the records for each welfare recipient, who was enrolled in the workfare program and whose job readiness was evaluated between June 1999 and February 2000, were extracted from the following data tables in the EDW:

1. The address data table;
2. The case history data table, which contains a record for each transaction for each welfare recipient, e.g., enrolled in the workfare program or failed to comply with ESP program requirements;
3. The case-type data table, which contains the case-type, e.g., general assistance or family assistance, and day that benefits commenced for each applicant who is accepted for welfare benefits;
4. The demographic data table, which contains the demographics, e.g., date of birth and gender for each recipient;
5. The employment data table, which contains the start date and salary for each job which is started by each recipient; and
6. The recipient data table, which contains a record for each recipient who received welfare since 1996.

Once the data was extracted from the EDW, a Microsoft Access database was designed to organize and analyze the extracted data. In total, over 500 megabytes of data were extracted from the EDW for use in this study.

⁵⁰ A contract was negotiated with the New York City Human Resources Administration, which regulates the use of this data.