

The Effect of Workfare Before Workfare Begins

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Abstract

The Work Experience Program (WEP), an innovative workfare program that was mandatory for all able-bodied general assistance recipients, was launched in early 1995. 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 selected for the program on a given date are compared to a cohort who were not selected on that date. Ultimately, each member of the latter cohort who remained eligible was selected for the WEP, and recipients were selected solely based on observable characteristics. After adjusting for the two previous factors, the results indicate that the WEP increases the likelihood that a recipient will exit welfare by approximately seventeen and a half percentage points. One of the most intriguing aspects of this finding is that recipients exit welfare before their WEP assignment begins; that is, simply being informed that one was selected for the WEP generates the observed effect. If one could fully adjust for the subsequent enrollment of members of the comparison group, the estimated treatment effect would almost certainly be larger. Finally, the robustness of the findings are confirmed using a second comparison group and the WEP easily passes a rudimentary cost benefit test.

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 1995, New York City created the New York City Work, Accountability, and You (NYCWAY) program, an innovative program for general assistance¹ (GA) recipients. The NYCWAY program stipulated that able-bodied GA recipients participate

¹ General assistance is welfare for childless adults.

in the Work Experience Program (WEP) in exchange for their benefits. This paper investigates whether there is sufficient evidence to conclude that this work requirement contributed to the success of welfare reform in New York City.

This study takes advantage of a quirk in the administration of the WEP. When the program was initiated, all of eligible recipients could not be enrolled simultaneously due to capacity constraints. Rather, recipients were enrolled in waves; a new group was enrolled every week until all eligible recipients were enrolled.

This creates the opportunity to identify the effect of the WEP using a natural experiment in which welfare recipients who were selected for the program – the treatment group – are compared to those who were eligible but not selected – the control group. For example, comparing recipients who were selected for the program in March 1995 to those who were eligible but not selected, one finds that those who were selected are more likely to exit welfare than are those who were not.

To confirm that the WEP consistently increases the probability that a recipient will exit welfare, the comparison is repeated for the four subsequent months. The observed effect peaks two months post enrollment, at which time recipients who were selected for the program are, on the average, over two and half times as likely to exit welfare as are recipients who were not selected (27.9% versus 10.3%). The observed effect then steadily declines to 22 percent seven months post enrollment (50.9% versus 41.5%).

One of the most intriguing aspects of this finding is that recipients exit welfare before their WEP assignment begins; that is, simply being informed that one was selected

for the WEP generates the observed effect. This interesting finding is discussed in detail in the discussion section of the paper.

This initial finding raises two important issues. First, are recipients who were selected for the WEP comparable to recipients who were not selected? Second, should the post-peak decline in the effect of the WEP be expected? These issues are addressed in detail in this paper. One should note, however, the following facts.

First, recipients were selected for the program 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 any differences between those who were selected in a given month and those who were not. When one makes such an adjustment, the estimated effect of the WEP decreases marginally.

Second, as more members of the control group are selected, over time, for the WEP³, the percent that exit welfare should increase⁴ and the observed effect should decrease⁵. When one partially adjusts for the fact that many members of the control group are ultimately treated, the effect of the WEP increases.

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-

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 month even if he or she is subsequently enrolled in the WEP.

⁴ The percent that exit welfare should increase since the WEP increases the likelihood that a recipient will exit welfare.

⁵ The observed effect of the program should decrease since the observed effect is simply the difference between the percent of recipients who were selected for the program that exit welfare and the percent of recipients who were not selected that exit welfare.

1996 waiver reforms authorized under the Family Support Act of 1988, have contributed to the recent decline in welfare caseloads nationwide⁶. In a review of this literature, Moffitt (2002) reports that the evidence “indicates some TANF⁷ 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.

Relative to those studies, the approach of this research is unique. This study measures quite precisely the time, down to the month, 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 exit welfare.

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

⁶ Caseloads have declined by more than fifty percent, from over fourteen million in 1995 to fewer than six million in 2001.

⁷ 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

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 exits welfare. Finally, none of these programs targeted recipients of general assistance.

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 assistance and no effect on the mean annual earnings of male recipients of family assistance⁸ (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 welfare caseloads. Second, it identifies the effect of a single component of welfare reform, rather

⁸ Male recipients of family assistance received benefits through the AFDC-U program.

than the effect of welfare reform in general. No previous study has been able to do this⁹. Finally, it develops an innovative approach for identifying the effect of an intervention in which individuals are treated in waves.

The success of the WEP raises some interesting issues. Why was simply informing recipients that they were selected for a WEP assignment so effective? Would the WEP be as effective on recipients of family assistance? And finally, are the costs of the WEP 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 WEP. The third section discusses the strategy for identifying the effect of the WEP and presents the key results. The fourth section presents the results of a robustness check, in which a second control group is used to bolster the validity of the findings. The fifth section discusses the implications of these findings and presents a brief cost benefit analysis of the WEP. The final section briefly describes a planned extension of this work.

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 300,000 who were

⁹ 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.

receiving Home Relief (HR)¹⁰. In 1994, New York City spent approximately three billion dollars on welfare programs, including over one billion dollars on the HR program.

In early 1995, Mayor Giuliani initiated the New York City Work, Accountability, and You (NYCWAY) program for HR 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 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 were enrolled in NYCWAY as well. One year later, the number of AFDC 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.

¹⁰ 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.

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 Work Experience Program (WEP)

i. Description

Under the NYCWAY program, able-bodied HR recipients were required to participate in the WEP in exchange for their welfare benefits. WEP participants worked part-time, generally 21 hours per week. Several dozen city agencies were enlisted to create tens of thousands WEP assignments; three agencies - the department of Parks & Recreation, Sanitation, and Transportation – created and managed the bulk of assignments.

WEP participants were engaged in a wide variety of activities from indoor assignments, such as clerical or janitorial work, to outdoor assignments, such as litter removal, graffiti abatement, landscaping, or light maintenance. The majority of WEP participants worked outdoors.

In exchange for participating in the WEP, able-bodied HR recipients were able to remain on welfare and received a small stipend to cover the cost of carfare and lunch¹¹. Participants were also potentially developing new job skills that might help them find and retain a job.

ii. Enrollment in the WEP

Prior to receiving a WEP assignment, potential participants were required to attend a WEP intake interview. At the intake interview, each recipient was given the opportunity to request an exemption from the WEP due to a personal hardship or a verifiable medical condition. Recipients who did not request an exemption were given an assignment letter which included the name of the agency for which the recipient would work and the time and location of the agency's WEP orientation.

¹¹ The lunch stipend was eliminated in the spring of 1996.

A large portion of HR recipients who were scheduled for WEP intake interviews failed to attend their appointments. Furthermore, many recipients who did attend their scheduled appointments requested an exemption from the WEP. In total, less than half of all the recipients who were scheduled for an intake appointment received a WEP assignment on the day of their appointment.

Consequently, estimating the effect of participating in a WEP assignment would be biased, since recipients could, at least temporarily, self-select out of the program; either by failing to attend the intake interview or by requesting an exemption from the program. Thus, the cohort of recipients who participate in a WEP assignment are probably not be comparable to the cohort who do not participate in a WEP assignment.

iii. Treatment Is Being Selected for a WEP Intake Interview

This paper, instead, estimates the effect of being selected for a WEP intake interview. That is, each recipient was considered treated the moment they were selected for the WEP. For example, a recipient who was selected for the WEP and did not even attend the WEP intake interview was still considered to be treated¹². The implications of this are discussed in detail in the discussion section of the paper.

Participation in the WEP was mandatory¹³ for all able-bodied HR recipients.

When the program was initiated, there were over 100,000 such recipients. Each of these “pre-existing” recipients was required to participate in the program, and thus needed to be enrolled.

¹² 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.

¹³ If a participant failed to comply with the requirements of the WEP, 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 WEP, being sanctioned for up to 180 days, or being exempted from participation in the ESP program.

HRA could not accommodate all of these recipients at the same time¹⁴.

Consequently, eligible recipients were scheduled for intake interviews in waves; a new group was selected every week. HR recipients who were selected were instructed by mail to report to a HRA office on a prescribed date. Typically, the reporting date was two to three weeks after the selection date. This process continued until each able-bodied HR recipients was enrolled in the WEP or was no longer eligible.

iv. Selection of Recipients to Be Treated

The weekly selection of able-bodied HR recipients for WEP intake interviews – the treatment – was centralized and performed by computer programmers. Eligible HR recipients were selected solely using information that was stored in a computer database; caseworkers were not involved in any way.

The selection of recipients for each wave was stratified by the borough of residence and the primary activity in which the recipient was engaged¹⁵. Each week, the specifications of the stratification were modified depending on current priorities, e.g., one week the priority might be to increase the number of Bronx recipients who participate in the WEP. Within an activity, recipients could be selected using either LIFO, last-in first-out, or FIFO, first-in first-out.

Unfortunately, HRA did not document the stratification criteria that were used each week. None of the programmers who performed the weekly selection process, nor the policy makers who determined the stratification parameters, had any notes or records regarding the choices that were made each week regarding the selection criteria. They all

¹⁴ In late 1994, HRA began to schedule each of these pre-existing recipients for a WEP intake interview.

¹⁵ HRA's database stored a code for each recipient that identified the primary activity in which the recipient was engaged.

agreed, however, that the criteria changed frequently. Fortunately, even though the exact parameters are not recoverable, the selection process was based solely on observable characteristics. Thus, one should be able to adjust for non-random selection by including observable characteristics as explanatory variables in the analysis.

v. Description of the Available Data

The data for this study was collected from a legacy database system that is no longer in use. Fortunately, HRA saved monthly “snapshots” of the data stored in this system. From these snapshots one can reconstruct a rough case history for each recipient. Specifically, each monthly snapshot includes the recipients’ age, gender, ethnicity, borough of residence, case status, and primary activity in which they were engaged at the end of the month.

Since these snapshots were stored in a COBOL database on magnetic tapes in Albany, NY, there was a considerable cost in terms of time and effort to retrieve data for this study. Furthermore, HRA could not generate an electronic version of the required data. Thus, the data for this study was printed and then entered into a new, analytic database.

Given these limitation, it was unfortunately not feasible to collect data for the population of eligible recipients. Rather, a random sample of 3,595 recipients was drawn from the population, of approximately 100,000 eligible recipients, in February 1995. The monthly snapshots for each recipient in sample was collected for each month between February 1995 and February 1996. Using this data, a rough case history was reconstructed for each recipient in the sample for a one-year period, February 1995 to February 1996, which coincides with the first year of NYCWAY. Note that the case

status of each recipient, and activity in which each recipient was engaged, is only known at the end of each month¹⁶.

Finally, due to a computer programming oversight, after a recipient received a WEP assignment, their subsequent end-of-month snapshots were not collected. That is, if a recipient was selected for the WEP, attended his or her WEP intake interview, and received a WEP assignment at the end of the interview, then there is no subsequent data regarding that individual's case status or activity in which he or she is engaged.

Throughout this paper, it is assumed that each of these recipients does not exit welfare, and remains in a WEP assignment, for the remainder of the period under study. One should note, however, that this is a very conservative assumption that will bias against finding a treatment effect for the WEP; the observed treatment effect is almost certainly negatively biased by more than five percentage points. This issue is discussed in detail in the following section of the paper.

III. Identifying the Effect of the WEP

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 WEP, 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-

¹⁶ Thus, the exact date that a change of status or activity occurred is not known. Furthermore, a recipient could have had multiple changes of status or activity occur between two monthly snapshots. If this occurred only the final status or activity would appear in the recipient's rough case history.

determined¹⁷. Thus, such a comparison would produce a biased estimate of the program's effect.

It is possible, however, to estimate the effect of the WEP by taking advantage of the initial, incremental enrollment of eligible HR recipients. Recall that the HRA could not accommodate all the eligible HR recipients simultaneously; it took over six months for the HRA to schedule all of them for an intake appointment. Consequently, the effect of the program can be estimated using a natural experiment in which recipients who were selected for treatment¹⁸ – a WEP intake interview – are compared to HR recipients who were eligible¹⁹ but not selected in the same month.

A. The Effect of the WEP on the Probability of Exiting Welfare

Comparing recipients who were selected for WEP in March 1995 to those who were eligible but not selected in March 1995, one finds that those who were selected are more likely to exit welfare than are those who were not (see Figure 4). To confirm that the program consistently increases the likelihood that a recipient will exit welfare, this comparison is repeated for each month between April 1995 and July 1995. For all of these months, the pattern is repeated, i.e., eligible HR recipients who were selected for

¹⁷ For example, when an individual exits welfare, he or she becomes ineligible.

¹⁸ Recipients whose end of month activity was “scheduled for a WEP intake interview” are considered selected for treatment in month they entered this activity.

¹⁹ To be eligible a recipient has to be on welfare at the end of the month in question and has to have been engaged in the same activity at the end of the month in question as well as at the end of the previous month. The latter criteria prevents recipients who were treated, or engaged in any way by HRA, during the prior month from being included in the comparison group. Activity codes were generally changed as the result of some engagement of the recipient by HRA. Furthermore, since the activity code “scheduled for a WEP intake interview” typically only persisted in the legacy database for approximately two weeks, recipients could have been treated in the middle of the month and not had their treatment recorded in the monthly snapshot from the preceding or succeeding month. Thus, their treatment would not be recorded in the rough case history built from the monthly snapshots. The latter criteria prevents such recipients from being placed in the comparison group.

the WEP are more likely to exit welfare than are those who were not selected (see Figure 5).

By combining all the eligible HR recipients who were selected between March and July 1995 into one group, the treatment group, and by combining all the HR recipients who were eligible but not selected in each month between March and July 1995 into another group, the control group²⁰, one can estimate the treatment effect (see Figure 6). Specifically, the treatment effect is defined as:

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

where $Y_i^M (T_i)$ is an indicator function which equals one if recipient i exits welfare within M months of his or her inclusion in the control or treatment group 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 two months post enrollment when members of the treatment group are over two and half times more likely to exit welfare than are members of the control group (27.9% versus 10.3% - see Figure 7). The effect then declines steadily to 9.4 percentage points seven months post enrollment²¹ (50.9% versus 41.5%).

One might be concerned that this observed effect is simply transitory, and that many of the recipients who exit in response to the treatment rapidly reenter welfare. This, however, is not the case. Members of the treatment group are over two and half times more likely to exit welfare, and remain off of welfare for six consecutive months,

²⁰ Note that recipients could be eligible and not selected in multiple months, and thus, may be members of the control group multiple times.

²¹ There are only seven post enrollment months for which there are case statuses for all of the recipients in the study; the last enrollment month is July 1995 and the data is available through February 1996.

than are members of the control group two months post enrollment²² (see Figure 8). When the dependent variable is exiting welfare and remaining off of welfare for two consecutive, or three consecutive months, one obtains similar results. The treatment effect peaks two months post enrollment when members of the treatment group are over two and a half times more likely, or 15 percentage points, to exit welfare than are control group members. The effect then steadily declines to approximately 12 percentage points five months post enrollment.

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, why is the treatment effect declining over time? These issues are discussed in detail below.

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 borough of residence, gender, and racial distributions (see Table 1). The one striking difference between the two groups is that members of the treatment group are, on the average, three and half years younger than are members of the control group²³.

To determine whether this is a significant difference, one can test whether a recipient's demographic characteristics significantly impact the probability that he or she was selected for treatment – scheduled for a WEP intake interview. If a recipient's

²² There are only two post enrollment months for which there are case statuses for all of the recipients in the study.

²³ Even this difference is only one-third of a standard deviation from each average.

demographic characteristics have an impact on the probability of being selected, then the selection process does not approximate a random one.

Recalling that a new wave of recipients was selected each week and the selection criteria changed frequently, one should perform the above test for each new wave of recipients. The only available data, however, is the end-of-month snapshots. Thus, an alternative, less precise monthly approach was employed. Specifically, the probit equation below was estimated for recipients who were selected or eligible but not selected in each of the following months March, April, May, and July 1995²⁴, i.e.,

$$P[T_i = 1] = F(\mathbf{a} + \sum_{c=1}^C \mathbf{I}_c x_{ic} + \mathbf{e}_i) \quad (2)$$

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 at the time of his or her inclusion in the control or treatment group.

The coefficients from estimating equation (2) provide evidence that the selection process did not approximate a random one. The coefficients on the recipient's age are negative in all four months and are significantly than zero in three of the four months (see Table 2). This indicates that older eligible recipients were less likely to be selected for the WEP than were younger eligible recipients. It also appears that white recipients were somewhat more likely to be selected for the WEP than were non-white recipients; in two of the four months the coefficient on white is positive and significantly different than zero. This pattern, however, is not nearly as strong as the one for age.

²⁴ The probit was not estimated for June 1995 since there were only 13 recipients selected for the WEP in June within the sample.

One thing is certain though; eligible HR 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 did not approximate a random one, 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 the observed differences.

C. The Effect of the WEP Controlling for Observed Demographic Differences

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

$$y_i^M = \mathbf{a}^M + \mathbf{b}^M T_i + \sum_{c=1}^C \mathbf{I}_c^M x_{ic} + \mathbf{e}_i^M \quad (3)$$

where y_i^M is an outcome dummy that equals one if individual i exits welfare within M months of his or her inclusion in the control or treatment group 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.

Regression coefficients are calculated using OLS for values of M between one and seven²⁵ using age as a covariate²⁶; corrected standard errors are calculated by clustering the observations by individual²⁷. The coefficients on the treatment dummy are positive and significantly different than zero for all values of M. The coefficients peak at 0.167 (t = 9.2, p = 0), when M equals two, indicating that recipients who were selected for the WEP are 16.7 percentage points more likely to exit welfare two months post enrollment than are recipients who were eligible but not selected (see the second column of Table 3). The coefficients on the treatment dummy then steadily decline to 0.067 (t = 3.17, p > 0.002) when M equals seven (see the second column of Table 4).

Comparing the coefficients on the treatment dummy with age included in the regression to the coefficients on the treatment dummy without age included in the regression, one observes that the inclusion of age does reduce the coefficients on the treatment dummy (see Figure 9). This is not surprising since the likelihood of being selected for the WEP was higher for younger recipients than it was for older recipients; and younger recipients were presumably more likely to exit welfare than were older recipients, since younger recipients probably have more attractive alternative opportunities. This presumption is confirmed by observing that the coefficient on age is negative and significantly different than zero for all values of M, indicating that older recipients were less likely to exit welfare than were younger recipients (see the second column of Tables 3 and 4).

²⁵ The maximum number of months post enrollment for which data is available for all recipients in the study.

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

²⁷ This is necessary since some individuals appeared in the dataset repeatedly. Thus each observation is not independent of the others.

Including additional demographic characteristics as explanatory variables in the regression only marginally change the coefficients on the treatment dummy (see Figure 9). The coefficients on male and black are positive and significantly different than zero for all values of M, indicating that male recipients are more likely to exit welfare than are female recipients and that black recipients are more likely to exit welfare than are non-black recipients (see the fourth column of Tables 3 and 4).

D. The Effect of the WEP Controlling for Borough and Month of Enrollment

One might be concerned that the stratification of the selection process, by borough and month of enrollment, plays an important role in the estimated treatment effect. To eliminate this concern, borough, month of enrollment, and interaction dummies²⁸ are added to (3), i.e.,

$$y_i^M = \mathbf{a}^M + \mathbf{b}^M T_i + \sum_{c=1}^C \mathbf{l}_c^M x_{ic} + \sum_{j=1}^4 \mathbf{d}_j^M B_{ij} + \sum_{k=1}^4 \mathbf{g}_k^M E_{ik} + \sum_{j=1}^4 \sum_{k=1}^4 \mathbf{h}_{jk}^M (B_{ij} * E_{ik}) + \mathbf{e}_i^M \quad (4)$$

where y_i^M , 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 a month of enrollment dummy that equals one if individual i was placed in the treatment or control group in month k and zero otherwise.

As before, regression coefficients are calculated for values of M between one and seven using OLS; corrected standard errors are calculated by clustering the observations by individual. The regression coefficients on the treatment dummy, and the other prior covariates, only change marginally after the addition of these new covariates (see the fifth column of Tables 3 and 4 as well as Figure 9). Thus, the observed treatment effect is

²⁸ The interaction is between borough and enrollment month.

stable even after controlling for the underlying economic conditions. The month of enrollment 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 WEP

Ultimately each member of the control group, who remained eligible, should have been selected for the WEP²⁹. As more control group members were selected, the percent that exited welfare should have increased³⁰ and the observed treatment effect should have decreased³¹. 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 group³² existed.

Although such a control group does not exist, one can construct a proxy. This is achieved by restricting membership in the control group to recipients who were eligible but not selected in the initial enrollment period and were not selected in subsequent enrollment periods. Since recipients who had a change of activity between the initial enrollment period and a subsequent enrollment period were probably treated in the middle of the month that the change occurred, such recipients are excluded from this new control group³³.

²⁹ It is not possible to determine the number of control group members who were ultimately treated, since only rough case histories could be reconstructed based on the end of month snapshots. Many members of the control group were presumably treated in between two snapshots and no record of their being treated is available.

³⁰ The percent that exited welfare should increase since the WEP increases the likelihood that a recipient will exit welfare.

³¹ The percent that exited welfare should increase since the treatment effect is simply the difference between the percent of treatment group member that exit welfare and the percent of control group members that exit welfare.

³² A control group whose members were never selected for the WEP.

³³ See footnote 17 for additional details.

For example, a control group with a one month restriction, denoted “control group one,” would include all HR recipients who were eligible but not selected in March 1995, not selected in April 1995, and engaged in the same activity in March and April 1995. Control group one does introduce a selection problem. Specifically, control group one contains two cohorts:

- Cohort 1. Members who were eligible and not selected in the initial enrollment period but had their welfare case closed prior to the subsequent enrollment period, and
- Cohort 2. Members who were eligible but not selected in both the initial and subsequent enrollment periods.

Members of cohort two are under-represented relative to members of cohort one in control group one. This is true since members of the initial control group who were selected in the subsequent enrollment period – or who had a change in activity between the initial and subsequent enrollment period – were still eligible at the time of their selection – or change of activity – and would have been in cohort two if they had not been excluded from control group one³⁴. Consequently, the weight placed on members of cohort two needs to be increased so that the ratio of eligible to ineligible recipients in control group one is equivalent to what it would have been if no recipients were selected – or had a change of activity – in the subsequent enrollment period³⁵. Specifically, the weight placed on members of cohort two is the reciprocal of the probability, conditional

³⁴ Recall that becoming ineligible is often self-determined. Thus the ratio of eligible to ineligible recipients in control group needs to be preserved.

³⁵ 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.

on being eligible, of not being selected in the subsequent enrollment period or having a change of activity (see Figure 10). This weighting scheme should correct any selection problem.

One can extend this approach to create additional, more restrictive control groups. For example, control group two would include all HR recipients who were eligible but not selected in March 1995, not selected in April and May 1995, and engaged in the same activity in March, April, and May 1995. Since control group two imposes one additional restriction on membership, it contains one additional cohort:

- Cohort 1. Members who were eligible and not selected in the initial enrollment period but had their welfare case closed prior to the subsequent enrollment period,
- Cohort 2. Members who were eligible and not selected in both the initial and subsequent enrollment periods but had their welfare case closed prior to the next subsequent enrollment period, and
- Cohort 3. All members who were eligible and not selected in the initial, subsequent, and next subsequent enrollment periods.

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 or having a change of activity in the subsequent enrollment period. The weight placed on members of cohort three is equal to the reciprocal of the probability, conditional on being eligible, of not being selected or having a change of

activity in either of the two subsequent enrollment periods. This approach is extended further to create control groups three and six as well.

Comparing the likelihood of exiting welfare between members of the treatment group and members of control group zero³⁶, control group one, control group three, and control group six, one observes that the treatment effect becomes more pronounced as the length of the restriction increases (see Figure 11). Specifically, treatment effect six³⁷, peaks five months post enrollment at 19.6 percentage points, indicating that members of the treatment group are 19.6 percentage points more likely to exit welfare than are members of control group six. Treatment effect six then steadily declines to 11.2 percentage points seven months post enrollment.

Regression coefficients are calculated using OLS for equation (4) with control groups one, three, and six for values of M between one and seven; corrected standard errors are calculated by clustering the observations by individual. The coefficients on the treatment dummy are positive and significantly different than zero for all values of M and all control groups. Again, one observes that for all values of M the treatment effect becomes more pronounced as the length of the restriction increases (see Figure 12), that the coefficient on age is negative and significantly different than zero, and that the coefficients on male is positive and significantly different than zero (see Table 5).

Comparing the coefficients on the treatment dummy with all the covariates included in the regression to the coefficients on the treatment dummy without covariates included in the regression, one observes that the inclusion of the covariates reduces the

³⁶ Control group zero is simply the control group with no restriction, i.e., the initial control group.

³⁷ Let treatment effect n denote the treatment effect from using control group n . For example, treatment effect six denotes the treatment effect from using control group six.

coefficients on the treatment dummy (see Figure 13). As with control group zero, the majority of this reduction is generated when one includes age as a covariate. The inclusion of other covariates only marginally changes the coefficients on the treatment dummy when age has already been included as a covariate in the regression.

Unfortunately this approach cannot be extended further since there are few members of the control group who remain eligible and are not selected for the WEP within six months of their inclusion in the control group³⁸. If this approach could be extended indefinitely, the resulting control group would approximate a pure control group and the observed treatment effect would approach the true treatment effect.

F. The effect of treatment –selection for a WEP intake interview – on receiving a WEP assignment

It is also interesting to determine whether being selected for a WEP intake interview increased the likelihood that a recipient received a WEP assignment – the intended effect of the treatment. Members of the treatment group are ten times more likely to receive a WEP assignment than are members of the control group one month post enrollment (18.8% versus 1.86% - see Figure 14). The effect then steadily declines to 10.0 percentage points seven months post enrollment (29.9% versus 19.9%).

Regression coefficients are estimated for equation (4) with receiving a WEP assignment as the dependent variable for values of M between one and seven; corrected standard errors are calculated by clustering the observations by individual. The coefficients on the treatment dummy are positive and significantly different than zero for all values of M. Comparing the coefficients on the treatment dummy with covariates

³⁸ If one extends this approach further, too much weight is put on too few individuals.

included in the regression to the coefficients on the treatment dummy with covariates not included in the regression, one observes that the inclusion of the covariates marginally increases the coefficients on the treatment dummy (see Figure 15).

The coefficient on Brooklyn is positive and significantly different than zero for M equals one and two, indicating that recipients from Brooklyn were more likely to receive a WEP assignment than were recipients from other boroughs one and two months post enrollment (see the first and fifth column of Table 6). It is interesting to note that the coefficient on age and male are not significantly different than zero for all values of M . Recall that the coefficient on age was negative and significantly different than zero, and the coefficient on male was positive and significantly different than zero when the dependent variable was exiting welfare.

Regression coefficients are calculated using OLS for equation (4) with receiving a WEP assignment as the dependent variable using control groups one, three, and six for values of M between one and seven; corrected standard errors are calculated by clustering the observations by individual. The coefficients on the treatment dummy are positive and significantly different than zero for all values of M and all control groups. Again, one observes that for all values of M the treatment effect becomes more pronounced as the length of the restriction increases (see Figure 16). Treatment effect six, peaks seven months post enrollment at 29.9 percentage points, indicating that members of the treatment group are 29.9 percentage points more likely to receive a WEP assignment than are members of control group six. The coefficient on Brooklyn is positive and significantly different than zero for low values of M using all control groups, and the

coefficient on male is positive and significantly different than zero for low values of M using control groups one, three, and six.

Note the fact that treated recipients are more likely to receive a WEP assignment means that the estimated effect of the WEP on the likelihood that recipients exit welfare is very conservative. Recall that a recipient's end-of-month case status was not tracked after he or she received a WEP assignment³⁹ and that it was assumed that such recipients did not exit welfare during the period under study. This assumption negatively biases the estimated effect of the WEP on the likelihood that recipients exit welfare, since the percent of recipients that received a WEP assignment was significantly higher for members of the treatment group than for members of the control group.

Unfortunately, one can only roughly estimate the magnitude of the negative bias that this missing data produces. For example, if one used the same exit rate for recipients in WEP assignments as for recipients not in WEP assignments, then approximately half of recipients in WEP assignments should have exited welfare seven months post enrollment⁴⁰. Seven months post enrollment, treated recipients were approximately 11 percentage points more likely to be in WEP assignments than were non-treated recipients. Consequently, the observed effect on recipients exiting welfare is almost certainly negatively biased by more than five percentage points.

IV. Robustness Check – limiting membership in the control group to recipients who were ultimately treated

³⁹ This was due to a data collection oversight.

⁴⁰ Of the 3616 recipients in the study that did not receive a WEP assignment, 1970 – or 54.5% – had exited welfare seven months post enrollment.

One might be concerned that including covariates in the analysis does not adequately address the fact that there was a non-random selection process, which resulted in younger recipients being more likely to be selected for the WEP than were older recipients. To rule out this possibility, and to confirm the robustness of the findings, the effect of the WEP is estimated using only those recipients who were ultimately treated. Specifically, all member of the control group who were not selected for treatment by the end of the study, February 1996, are dropped from the control group; this new control group is denoted “control group members ultimately treated.”

After making this adjustment, members of the treatment group are four and a half times more likely to exit welfare than are members of the new control group two months post enrollment (27.9% versus 6.1% - see Figure 17). The treatment effect then steadily declines to 3.9 percentage points seven months post enrollment (50.9% versus 47.0%). The peak treatment effect is greater using this new control group (21.8 percentage points versus 17.6 percentage points); the treatment effect also declines more steeply after peaking using this new control group (17.9 percentage points versus 8.2 percentage points). The fact that the treatment effect peaks higher with the new control group and then declines further indicates that members of the original control group who become ineligible prior to being selected for the WEP are more likely to exit welfare soon after their inclusion in the control group but are less likely to do so over time.

The fact that the treatment effect declines more steeply after peaking is not surprising since all the members of the new control group were ultimately treated by the end of the study. As a matter of fact, if the effect of the WEP is consistent over time, the treatment effect using the new control group should ultimately go to zero since each

member is eventually treated. There is not enough post enrollment data, unfortunately, to determine if this is the case for the entire sample. If one, however, limits the analysis to those recipients who were included in the treatment group and the new control group in March and April 1995⁴¹, then the treatment effect does indeed go to zero ten months post enrollment (see Figure 18).

Comparing the demographic characteristics of members of the treatment group and the new control group, one observes that the average age and the distribution of borough, gender, and race are quite similar between the two groups (see Table 7). The difference in average age between the two groups is still in the same direction as before, recipients who were selected are younger, on average, than are recipients who were not selected. The magnitude of the difference, however, is less than half as much.

To determine whether the recipients chosen for the WEP from the new control group approximate a random selection, coefficients are estimated for equation (2) using probit for March, April, May, and July 1995⁴². This selection process did not approximate a random one either. In this case, the coefficients on age are significantly different than zero in two of the four months, more than is likely by chance (see Table 8). The two coefficients on age that are significantly different than zero are of opposite sign, for March 1995 the coefficient is negative and for May 1995 the coefficient is positive. Thus, even though the process does not necessarily approximate a random selection process, there is no clear pattern, as there was before, that older recipients were less likely to be selected for the WEP than were younger recipients. It also appears that white

⁴¹ Now there are ten post enrollment months of data regarding the case status of each included recipient.

⁴² The probit was not estimated for June 1995 since there were only 13 recipients selected for the WEP in June within the sample.

recipients were somewhat more likely to be selected for the WEP than were non-white recipients; in two of the four months the coefficients on white are positive and marginally, significantly different than zero.

Regression coefficients are estimated using OLS for equation (4) with the new control group using age as a covariate for values of M between one and seven; corrected standard errors are calculated by clustering the observations by individual. The coefficients on the treatment dummy are positive and significantly different than zero for values of M between one and six. Comparing the coefficients on the treatment dummy with age included in the regression to the coefficients on the treatment dummy with age not included in the regression, one observes that the inclusion of age does reduce the coefficients on the treatment dummy (see Figure 19). The reduction, not surprisingly, is smaller than with the original control group, since this selection process was less biased toward choosing young recipients. Again, the coefficient on age is negative and significantly different than zero for all values of M, indicating that older recipients were less likely to exit welfare than were younger recipients (see the second column of Tables 9 and 10). The coefficients on age using this new control group are only marginally different than the coefficients on age using the original control group, demonstrating that this result is robust to the choice of control group.

Including additional demographic characteristics – as well as borough, enrollment period, and interaction dummies – as covariates in the regression only marginally change the coefficients on the treatment dummy (see Figure 19). As before, the coefficients on male is positive and significantly different than zero for all values of M, indicating that male recipients are more likely to exit welfare than are female recipients (see the fourth

column of Tables 9 and 10). The coefficient on black is no longer significantly different from zero.

Control groups one, three and six are recreated with control group members ultimately treated. Again, one observes that the treatment effect becomes more pronounced as the length of the restriction increases (see Figure 20). As a matter of fact, these new restricted control group intensify the treatment effect much more effectively than do the original restricted control groups. Specifically, the new treatment effect six, peaks seven months post enrollment at 34.1 percentage points, indicating that members of the treatment group are 34.1 percentage points more likely to exit welfare than are members of new control group six. This peak effect is nearly twice as large as the one that was observed using the original control group six (34.1 percentage points versus 19.6 percentage points). If more post enrollment data were available, one should ultimately observe new treatment effect six decline overtime.

Regression coefficients are calculated using OLS for equation (4) using new control groups one, three, and six⁴³ for values of M between one and seven; corrected standard errors are calculated by clustering the observations by individual. The coefficients on the treatment dummy are positive and significantly different than zero for all values of M and all control groups except when M equals seven with new control group one. Again, one observes that for all values of M the treatment effect becomes more pronounced as the length of the restriction increases (see Figure 21). The

⁴³ When estimating equation (4) with new control group six, the borough, enrollment period, and interaction dummies are dropped. These covariates were dropped because of the small size of control group. For example, there are only four members of the new control group six who were eligible and not selected in July 1995. Since including these dummies in previous regression has had no effect on the coefficients of interest, dropping them here should likewise have no impact on the coefficients of interest.

coefficients on age and male are no longer consistently, significantly different than zero (see Table 11).

Furthermore, note that new treatment effect six is superimposed over new treatment effect three for the first five months post enrollment. Thus, new treatment effect six does not diverge from new treatment effect three until after the restriction on membership in new control group three has expired. The same pattern emerges when one compares new treatment effect three to one, and one to zero. Thus, it seems likely that for six months post enrollment, new treatment effect six is the upper envelope of the observed treatment effect⁴⁴. After that, the observed treatment effect almost certainly diverges from, and lies above, new treatment effect six⁴⁵.

Comparing the coefficients on the treatment dummy with all the covariates included in the regression to the coefficients on the treatment dummy without covariates included in the regression, one observes that the inclusion of the covariates reduces the coefficients on the treatment dummy (see Figure 22). As with new control group zero, the majority of this reduction is generated when one includes age as a covariate. The inclusion of other covariates only marginally changes the coefficients on the treatment dummy when age has already been included as a covariate in the regression.

Finally, the effect of treatment on receiving a WEP assignment is analyzed using the new control groups. Regression coefficients are calculated using OLS for equation (4) with receiving a WEP assignment as the dependent variable and all the new control

⁴⁴ Recall that there is a negative bias of at least five percentage points caused by missing data.

⁴⁵ If this approach could be extended indefinitely, the resulting control group would approximate a pure control group and the observed treatment effect would approach the true treatment effect. Unfortunately this approach cannot be extended further since there are few members of the control group who remain eligible and are not selected for the WEP within six months of their inclusion in the control group.

groups⁴⁶ for values of M between one and seven; corrected standard errors are calculated by clustering the observations by individual. The coefficients on the treatment dummy are positive and significantly different than zero for all values of M and all control groups. Again, one observes that for all values of M the treatment effect becomes more pronounced as the length of the restriction increases (see Figure 23). Treatment effect six, peaks seven months post enrollment at 29.7 percentage points, indicating that members of the treatment group are 29.7 percentage points more likely to receive a WEP assignment than are members of new control group six.

In summary, the treatment effect is robust to the choice of control groups. Whether one is using the entire control group or the control group in which membership is limited to recipients that are ultimately treated, the treatment clearly has a large, significant effect on the likelihood that a recipient will exit welfare and on the likelihood that a recipient will receive a WEP assignment.

V. Discussion

The treatment had a persistent, positive impact on the likelihood that recipients will exit welfare and also on the likelihood that recipients will receive a WEP assignment. Specifically, the program increased the likelihood that recipients will exit welfare by between 20 and 34 percentage points, depending on the measurement strategy. Moreover, recall that this estimate is negatively biased by at least five percentage points due to the previously discussed data collection oversight. Thus, one can conclude that the treatment was highly effective and substantially increased the likelihood that recipients would exit welfare.

⁴⁶ As before, when estimating equation (4) with new control group six, the borough, enrollment period, and interaction dummies are dropped.

The treatment also increased the likelihood that a recipient would receive a WEP assignment by approximately 30 percentage points. Finally, the treatment was at least as effective on men as it is on women; recall that the coefficient on male was often positive and significantly different than zero. In contrast, previous studies have found that mandatory short-term training programs have no significant effect on male welfare recipients (Friedlander, 1997).

A. Understanding the Results

A strength of this study is that every recipient who was selected for treatment was scheduled for a WEP intake interview, i.e., there was no ability for recipients to avoid treatment, and thereby bias the observed treatment effect. This is true since each recipient was considered treated the moment they were selected for the WEP. Consequently, the measured treatment effect does not suffer from a self-selection bias⁴⁷.

A limitation of this study is that only able-bodied HR recipients were included in this study. This limitation is the direct result of the design and implementation of the WEP. Recall that the WEP was initially only available for able-bodied HR recipients and that the identification strategy employed in this study utilized the initial enrollment of pre-existing, eligible recipients to identify the effect of the WEP.

There are two separate ways in which this limitation may affect the conclusions one can draw about the effect of the WEP. First, the measured treatment effect may be biased for non-able-bodied welfare recipients. This is probably not a major concern since

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.

requiring non-able-bodied recipients to work in exchange for their benefits is probably not an appropriate policy.

Second, the treatment effect may be biased for recipients of family assistance. Clearly, this is of concern since there are many more recipients of family assistance than of general assistance nationwide. The most important, and fundamental, way in which recipients of family assistance and general assistance differ is that all recipients of family assistance have dependent children, or a dependent child, while recipients of general assistance normally do not. Specifically, the effect of the WEP may interact with the effect of having dependent children, or a dependent child. This interaction could theoretically bias the treatment effect in either direction. While having to arrange childcare⁴⁸ clearly introduces an additional barrier to employment, being a parent may increase one's motivation to exit welfare and join the labor force.

Friedlander (1997) found that mandatory short-term training programs for welfare recipients are more effective on women than on men and women make up the vast majority of family assistance recipients. Furthermore, in a study of the effect of the another mandatory, short-term training program for welfare recipients in New York City, the Employment Services and Placement program, Ifcher (2004a) found that the program had an equally positive and persistent impact on family assistance recipients as it had on general assistance recipients. Thus, it seems likely that the WEP would have had a persistent and positive impact on family assistance recipients as well.

Perhaps, the most intriguing topic for additional research is to investigate why the WEP had such a significant and rapid impact on likelihood that recipients exit welfare.

⁴⁸ When Family Assistance recipients were enrolled in the WEP, New York City paid the cost of childcare for these recipients.

One potential explanation is that participating in a WEP assignment helped the participant develop the soft skills that are necessary for gaining, and remaining, employed, e.g., being timely, being able to work as part of team, and being able to take orders from a superior. This explanation, however, is not plausible since the treatment effect was generated by being scheduled for a WEP intake interview, not participating in a WEP assignment. Moreover, recall that all participants who actually received a WEP assignment were presumed not to exit welfare due to the previous discussed data collection oversight. Thus, any effect of participating in a WEP assignment on exiting welfare was not be measured in this study.

Another potential explanation is that the observed treatment effect was the result of the substantially increased cost, or disutility, of receiving welfare when it was mandatory for all recipients to participate in a WEP assignment. Specifically, the introduction of the WEP increased the number of hours per week, from zero to 21, that a typical recipient was required to spend in structured activities, presumably, not of his or her choosing. As a result, recipients' net utility from receiving welfare decreased. This decrease would have made exiting welfare preferable for some recipients. This explanation is also not plausible, since the observed effect was generated by scheduling recipients for WEP intake interviews, and not generated by participating in WEP assignments.

A twist on this explanation is that merely anticipating the cost of participating in a WEP assignment was enough to cause a large exodus from the welfare rolls. Furthermore, for this twist to be true, able-bodied recipients who had not yet been scheduled for a WEP intake interview, and thus had not received any information from

HRA regarding the WEP⁴⁹, did not also exit in anticipation of their manifest selection for the WEP. Otherwise, one would not observe a treatment effect.

One final possible explanation for the effectiveness of the WEP is that the cost of attending the WEP intake interview was high enough to cause recipients to exit welfare. This explanation implies that New York City could have engaged the recipients in any relatively insignificant manner and the effect would have been the same⁵⁰.

Unfortunately, given the fact that only a rough case history is available there is no way to determine which of the last two explanations is more plausible.

Finally, it would be interesting to determine recipients' progress after exiting welfare. For example, some recipients could have had cash jobs while receiving welfare that they kept after exiting welfare, other recipients might have chosen to start working rather than participate in the WEP, and others might have fallen deeper into poverty. Unfortunately, at the time that the WEP was implemented, HRA did not track recipients' progress after their cases were closed.

B. Cost Benefit Analysis

Finally, one should consider whether the benefits of the WEP outweighed the costs. The Department of Parks and Recreation (Parks), the largest sponsor of WEP assignments, received approximately four million dollars per year to manage and supervise WEP participants; this includes the cost to orient, train, equip, transport, and provide on-site supervision to the WEP participants. During this period, Parks received

⁴⁹ Recipients were not notified of the program until they were scheduled for a WEP intake interview. There was not a mass mailing to all eligible recipients warning them of the impending program changes. On the other hand, local newspapers and TV news programs had stories regarding the inception of the WEP.

⁵⁰ Prior to the inception of the WEP, most recipients were not engaged in any way by HRA except to perform semi-annual re-certifications.

approximately 26,000 WEP assignees per year from HRA. Thus, the average cost per assignee was approximately \$150. In addition, HRA incurred some additional, small expenses to treat recipients. For example, HRA employees scheduled and conducted WEP intake interviews. HRA did not track these expenses and thus no precise figure is available for the cost to HRA of treating a recipient . It is safe to assume, however, that HRA's expenses per treated recipient were no larger than were Parks' expense per assignee⁵¹.

To be extremely conservative, it is assumed that the total cost to treat a recipient was twice the cost to Parks per assignee, or \$300. This is conservative for two reasons. First, for HRA to treat a recipient was almost certainly less expensive than for Parks to manage and supervise a WEP assignment, and second, less than half of all treated recipients received a WEP assignment, however, this estimate assume that each treated recipient received a WEP assignment.

The benefit per treated recipient is harder to calculate since they are dependent on exit rates and the length of time that a recipient remains off of welfare. In early 1995, a typical HR recipient received \$350 per month in general assistance and \$100 per month in food stamps. This equals \$5,400 per year. To this one must add administrative costs and the cost of providing Medicaid benefits, since each HR recipient automatically qualified for Medicaid. These additional expenses are hard to calculate but potentially add thousands of dollars per year to the cost of supporting a general assistance recipient;

⁵¹ The majority of the cost to HRA was to conduct a single, short interview with the recipient. On the other hand, Parks potentially had to manage and supervise the recipients for many months or even years.

Medicaid benefits alone could cost over a thousand dollars per recipient per year. Thus, a very conservative estimate of the benefit of an exit is \$6,000 per year⁵².

The impact of the WEP on the likelihood of exiting welfare was estimated to be between 20 and 40 percentage points. Thus, the net fiscal benefit per treated recipient is between \$1,200 and \$2,400 per year. Given that the cost to treat a recipient was approximately \$300, the WEP pays for itself if recipients who exit welfare subsequent to being treated remain off of welfare, on average, between one and a half and three consecutive months.

The WEP easily surpasses this requirement and pays for itself many times over. As of the end of the study, the subset of 234 recipients who exited welfare subsequent to being treated, and who had at least nine months of follow-up data, had remained off of welfare, on average, seven and a half consecutive months. Of these 234 recipients, 155, or 66%, were still off of welfare when the study ended. Thus, the true average is significantly higher than seven and a half months. Even with only nine months of post enrollment data, the program pays for itself between two and a half and five times.

Previous studies have generally suffered from the same problem of limited post enrollment data. The authors of these studies have generally projected the benefits of the training program “into the future without any firm empirical basis” (Friedlander, 1997). Without such projections, the benefits of these programs would not have exceeded the costs. Thus the WEP is much more cost effective than other programs since it paid for itself between two and a half and five times during the period under study.

⁵² Note that if the recipient exits welfare and has a low enough income, he or she could still qualify to receive food stamps.

Moreover, making a conservative projection regarding the reentry rate to welfare – half of all the recipients who remain off of welfare return to welfare in each subsequent six month period – results in the average recipient remaining off of welfare for approximately 14.5 months. This projection implies that the program pays for itself between five and nine times.

This cost benefit analysis also ignores another tremendous benefit for the city. That is, the work that was accomplished by the WEP participants. Parks estimated that the value of the work accomplished by WEP participants was over \$20,000,000 per year, or \$4,000 per WEP participant per year.

Finally, one additional fact to keep in mind is that the gross benefit per year of a family assistance recipient exiting welfare is much larger than is the gross benefit for a general assistance recipient. In early 2000, a family assistance recipient, with three dependent children, received over \$600 per month in cash benefits plus up to \$400 per month in food stamps; this totals \$12,000 per year, as compared to \$5,400 for general assistance recipients. Thus, if the program is as effective on family assistance recipients as it is on general assistance recipients, then the WEP would be twice as cost effect for a family assistance recipient as for a general assistance recipient.

VI. Extension

To help determine what is happening to all of the recipients who exit welfare after treatment, the impact of the WEP on the use of homeless shelters will be analyzed. The New York City Department of Homeless Service (DHS) has released data regarding the use of homeless shelters in New York City by the individuals in this study from 1994 till 1997.

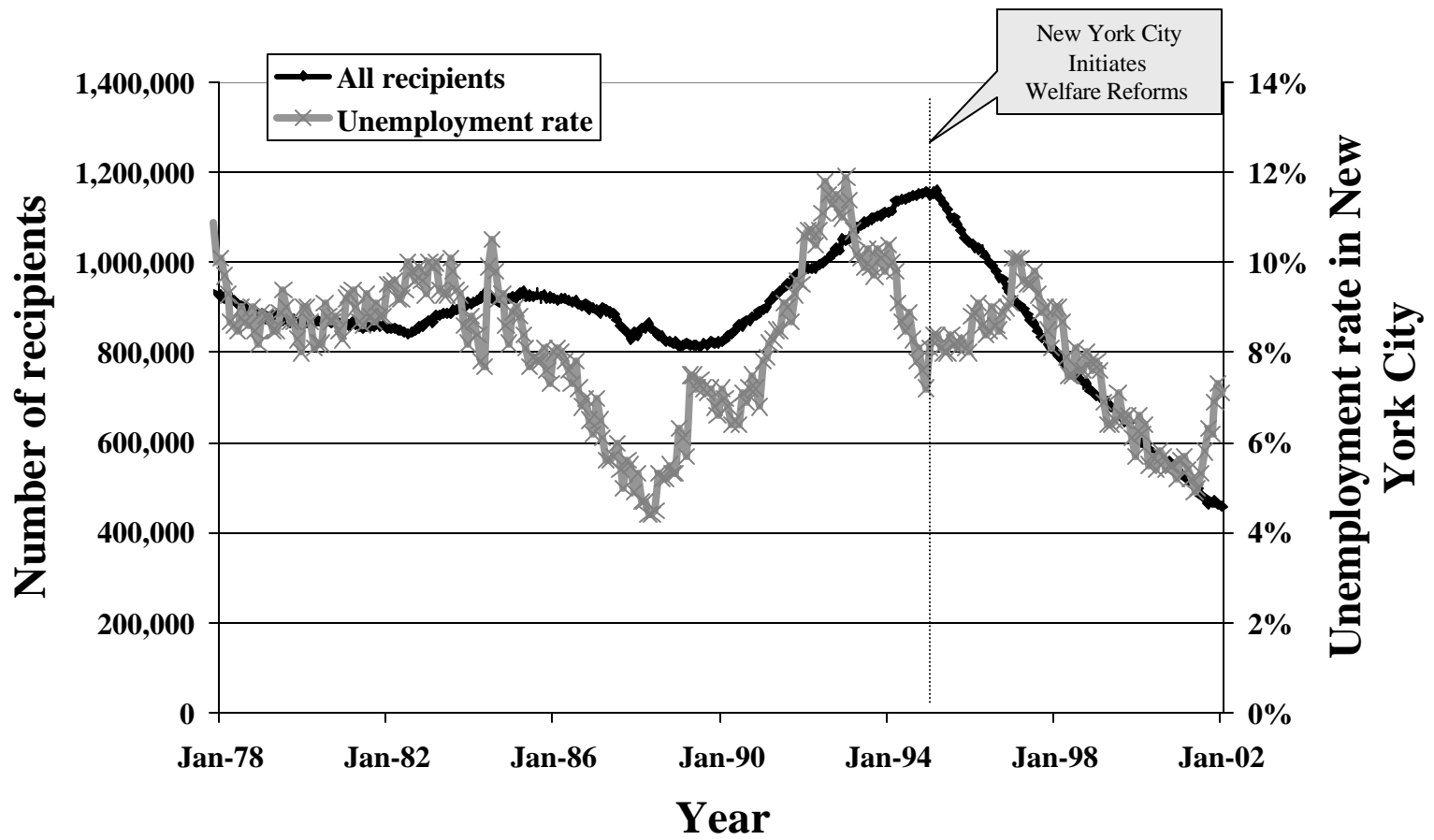
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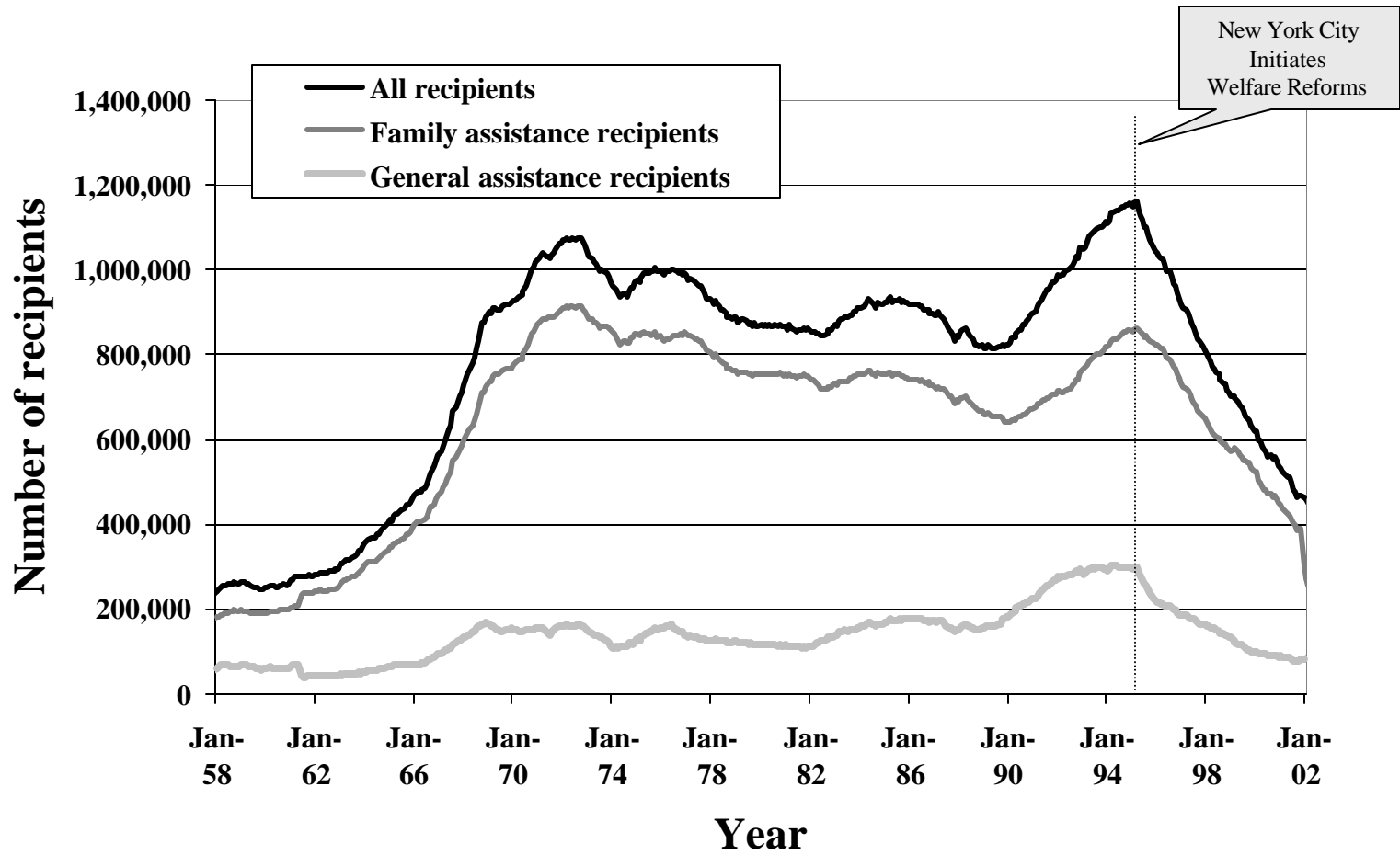
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Figure 1: The Unemployment Rate and the Welfare Caseload in New York City, January 1978 –January 2002



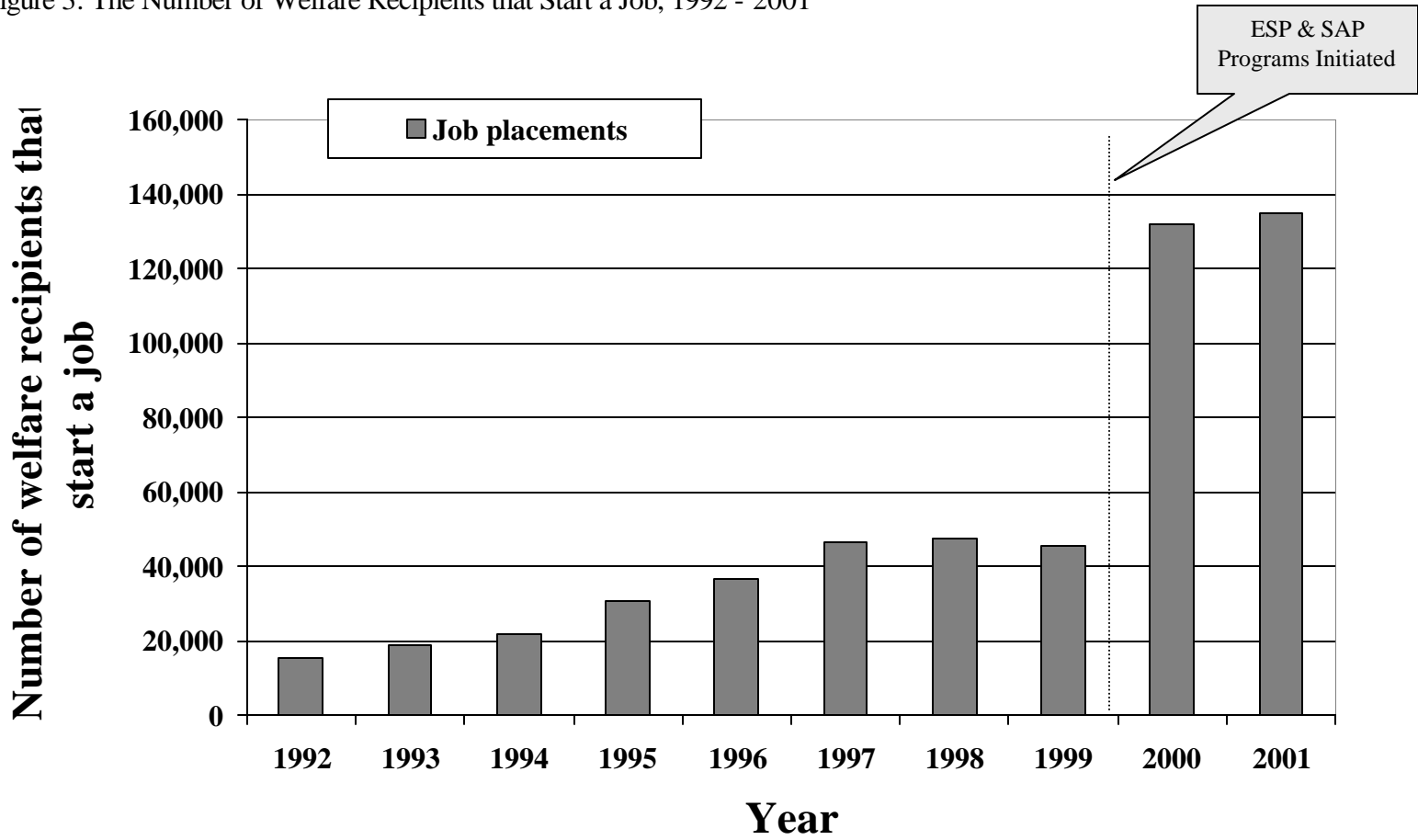
Source: Office of Policy and Program Analysis, New York City Human Resources Administration

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 HR Recipients that Exit Welfare, March 1995

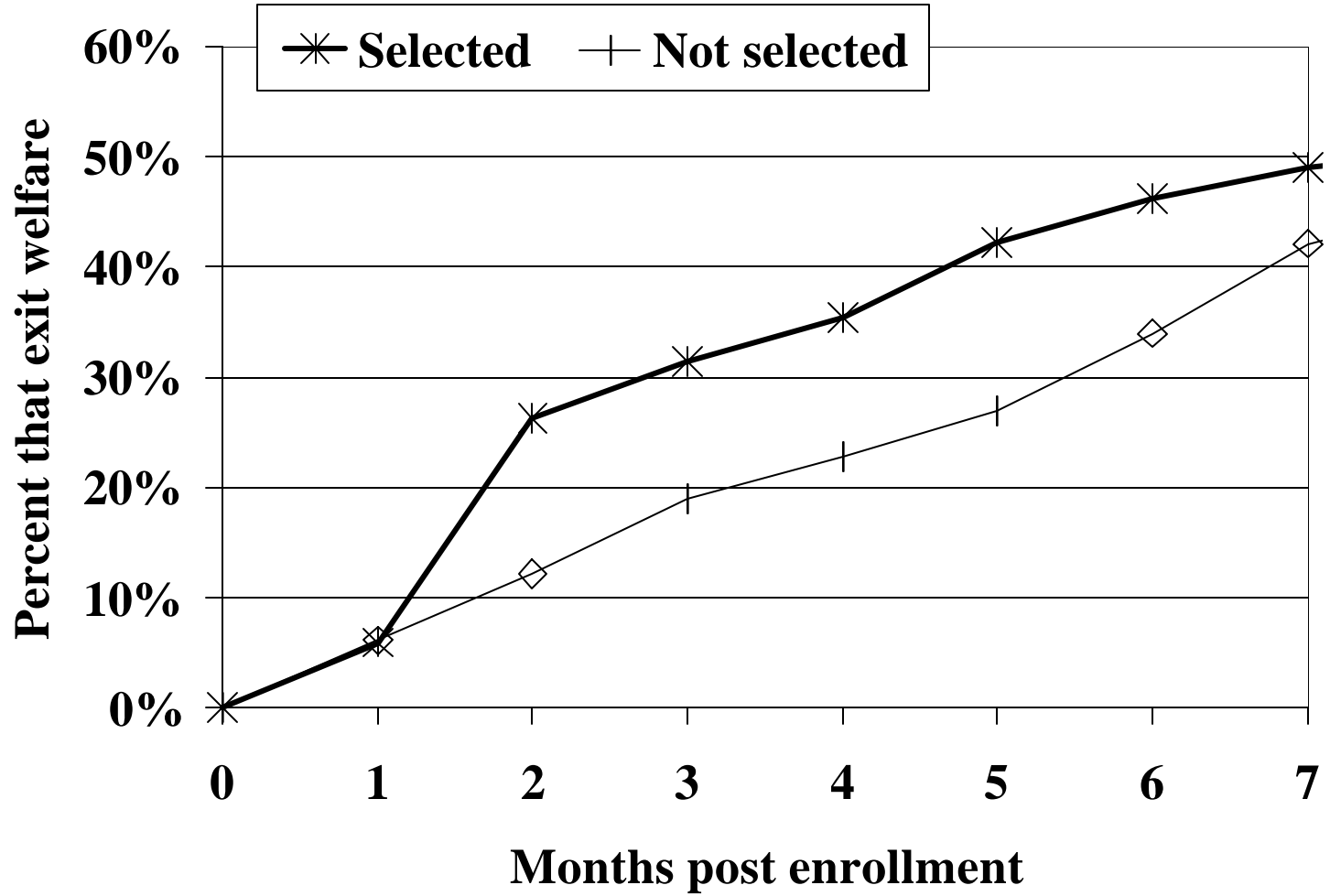


Figure 5: The Percent of Eligible HR Recipients that Exit Welfare, April through July 1995

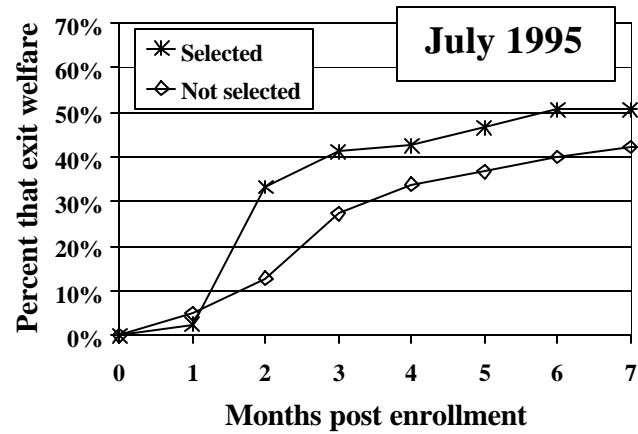
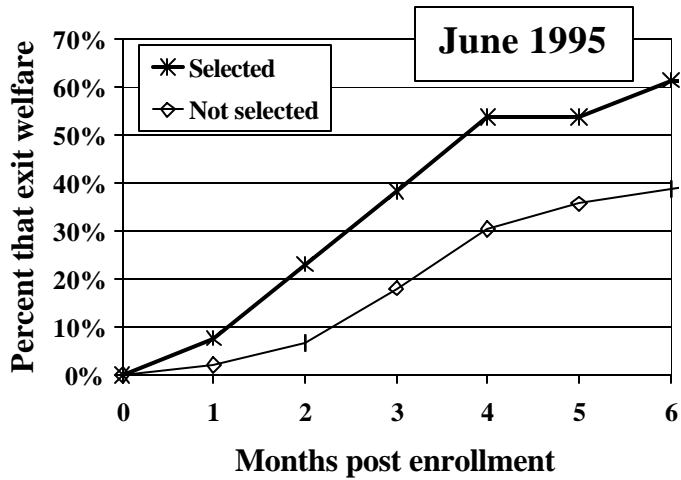
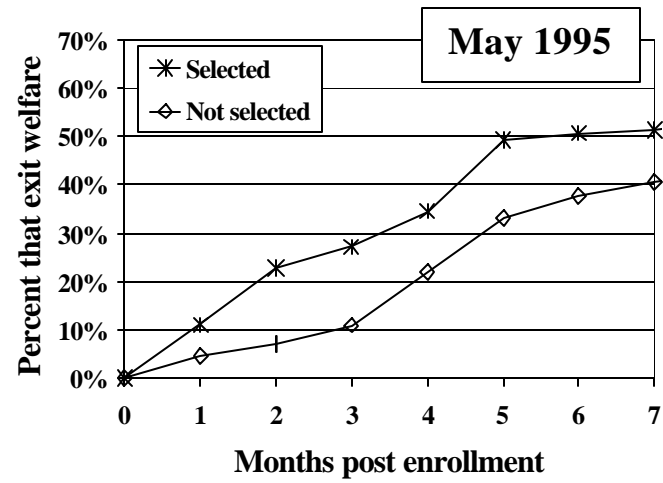
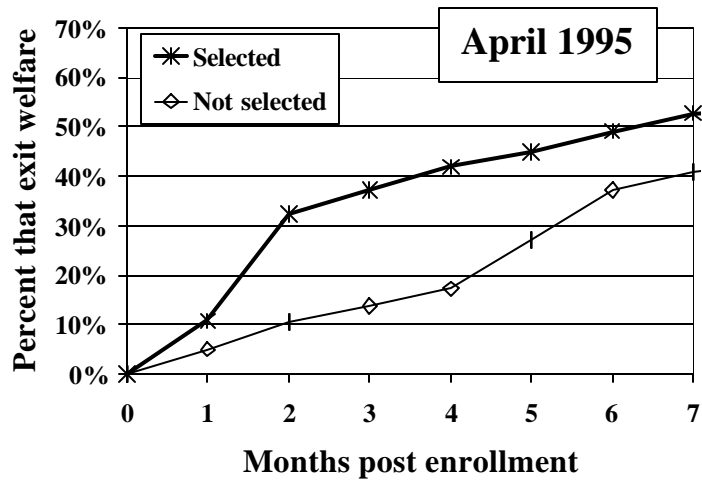


Figure 6: Formation of the Treatment Group and the Control Group

The Treatment Group:

All recipients selected for a WEP intake interview in any of the five months between March and July 1995. Specifically it is the union of T1, T2, T3, T4, and T5.

The Control Group:

All recipients who were eligible but not selected in any of the five months between March and July 1995. Specifically it is the union of C10, C20, C30, C40, and C50. Note that many recipients were eligible but not selected in multiple months. Consequently, some recipients are members of the control group multiple times.

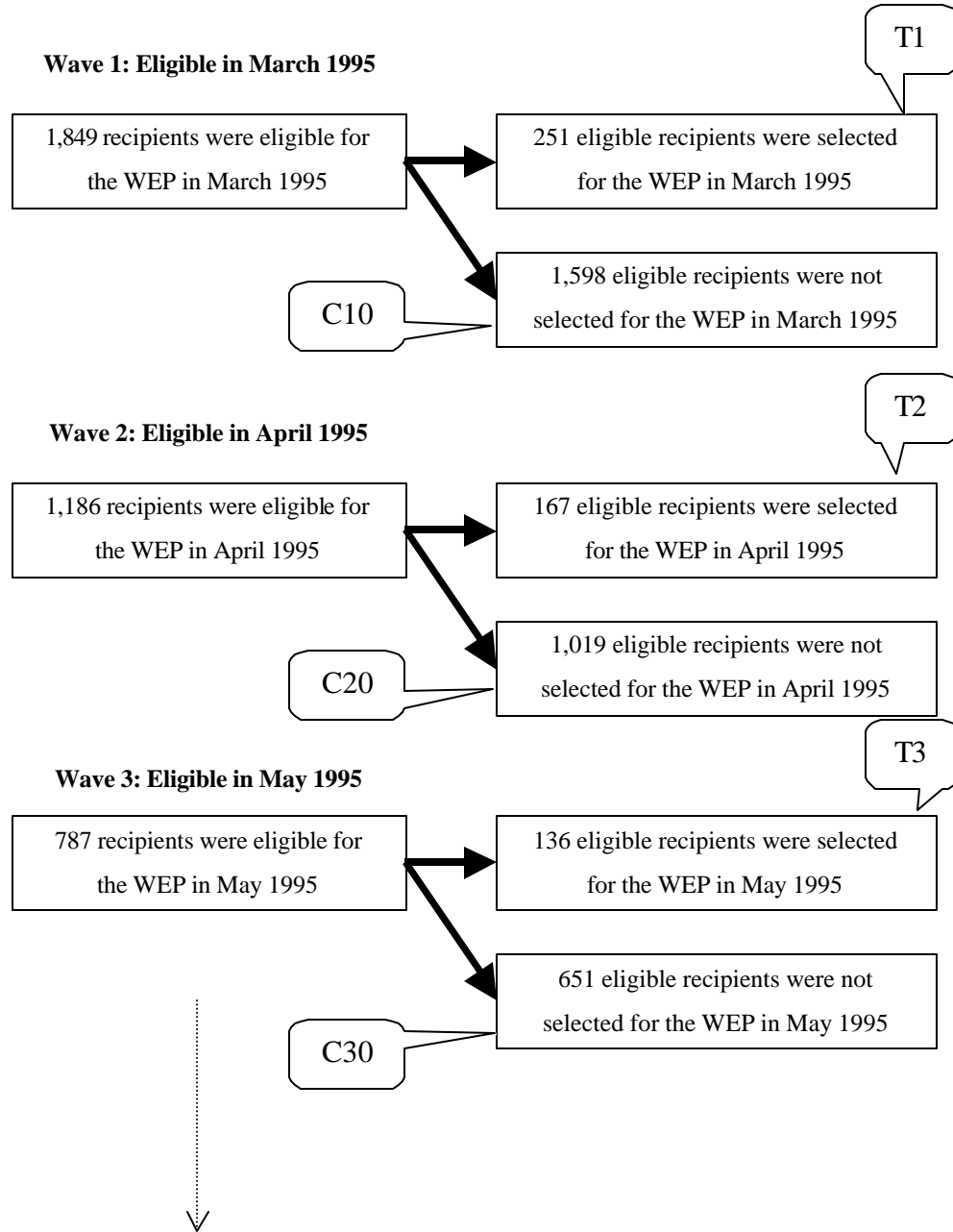


Figure 7: The Percent of Eligible HR Recipients that Exit Welfare, Treatment and Control Groups

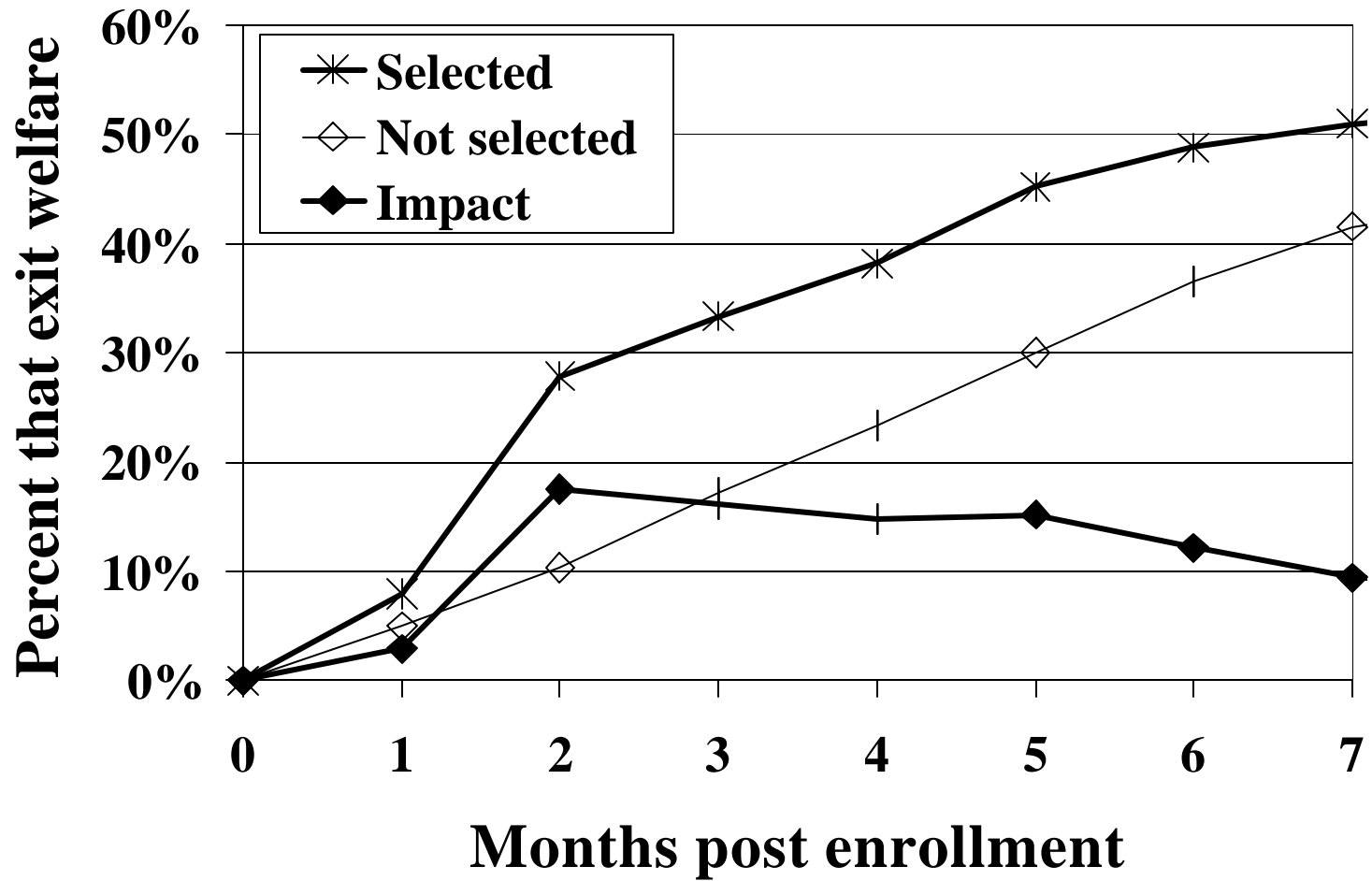
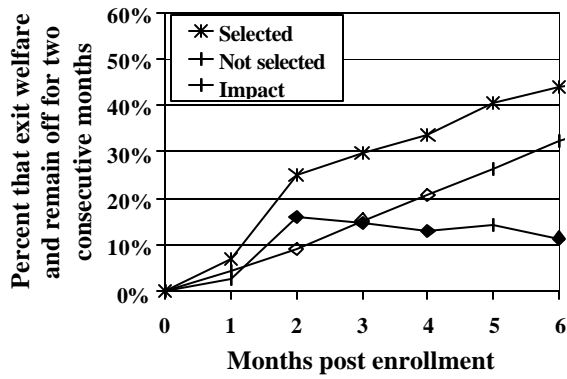
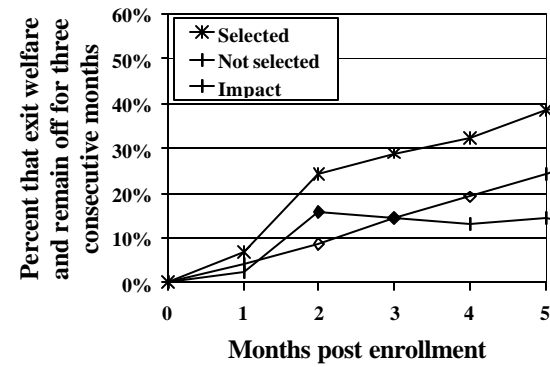


Figure 8: The Percent of Eligible HR Recipients that Exit Welfare and Remain Off of Welfare for Two, Three, & Six Consecutive Months

Remain Off Welfare for Two Consecutive Months



Remain Off Welfare for Three Consecutive Months



Remain Off Welfare for Six Consecutive Months

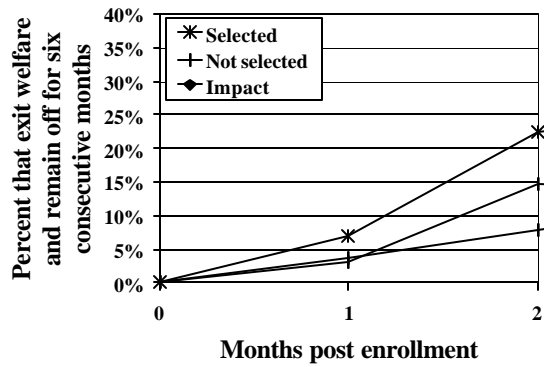


Figure 9: Coefficients on Treatment Dummy with Various Covariates Included

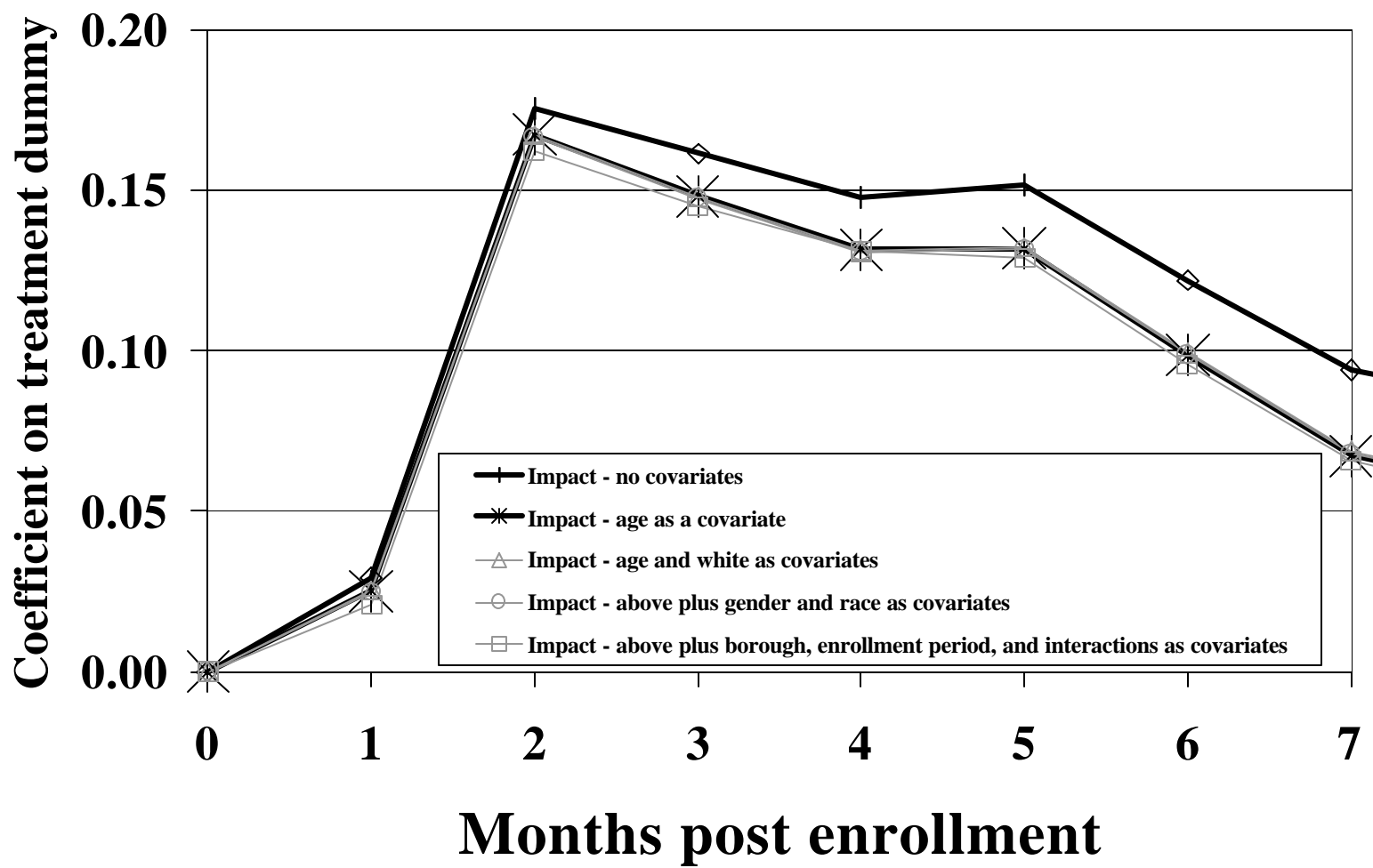


Figure 10: Formation of the Treatment Group and Control Group One (

The Treatment Group:

All recipients selected for a WEP intake interview in any of the five months between March and July 1995. Specifically it is the union of T1, T2, T3, T4, and T5.

Control Group One:

All recipients who:

1. Were eligible but not selected in any of the five months between March and July 1995, and
2. Were not selected in the subsequent month.

Specifically it is a weighted union of C11a, C11b, C21a, C21b, C31a, C31b,, C17a, C17b. 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 in the subsequent month for each cohort, e.g., for C11b the weight equals one divided by 68 percent or 1.47.

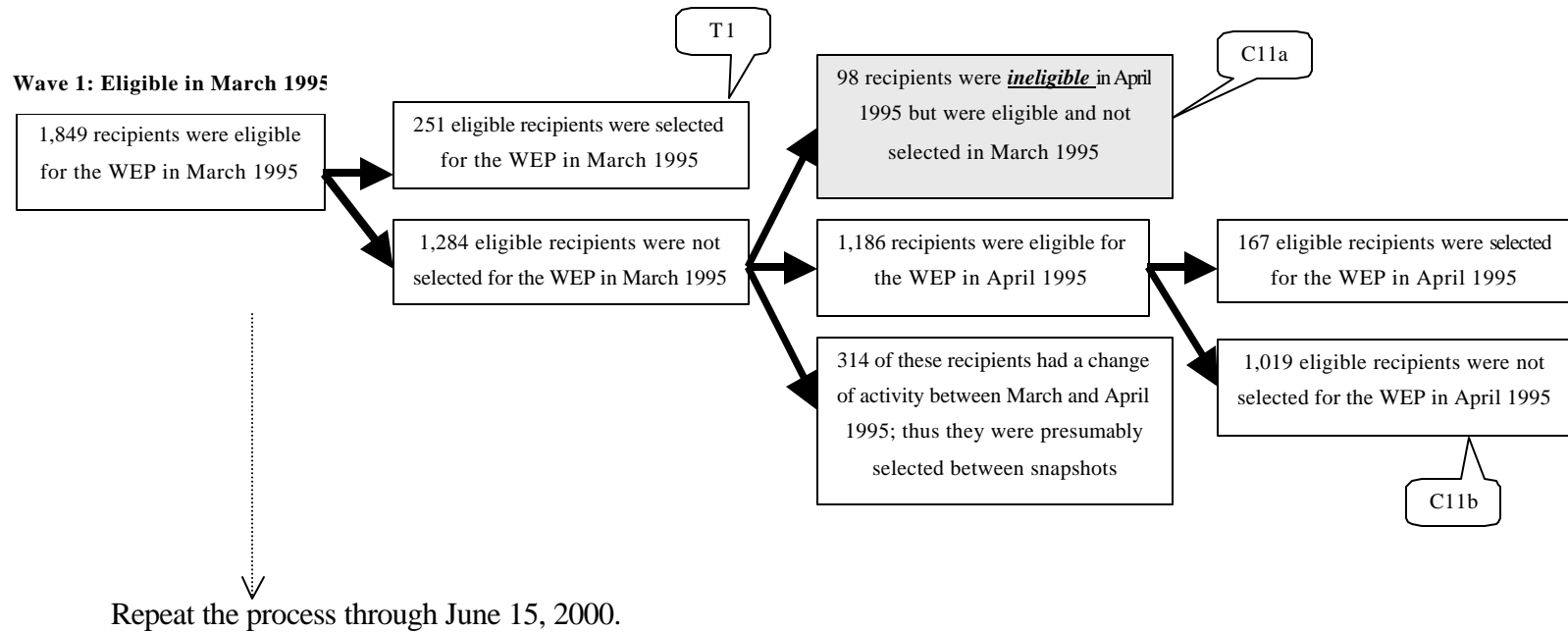


Figure 11: The Percent of Eligible HR Recipients that Exit Welfare Using Control Groups Zero, One, Three, and Six

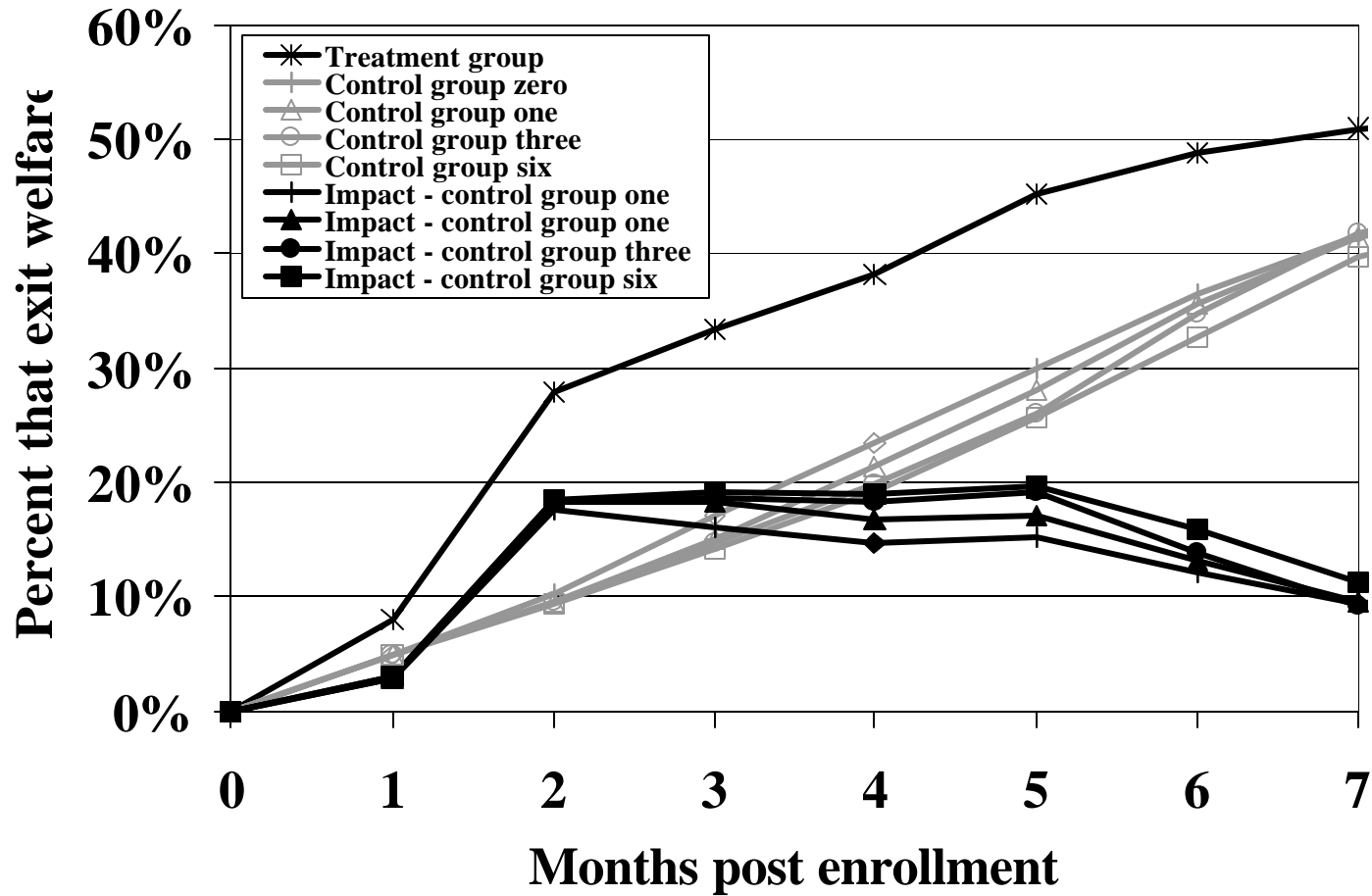


Figure 12: Coefficients on Treatment Dummy Using Control Groups Zero, One, Two, and Six, All Covariates Included

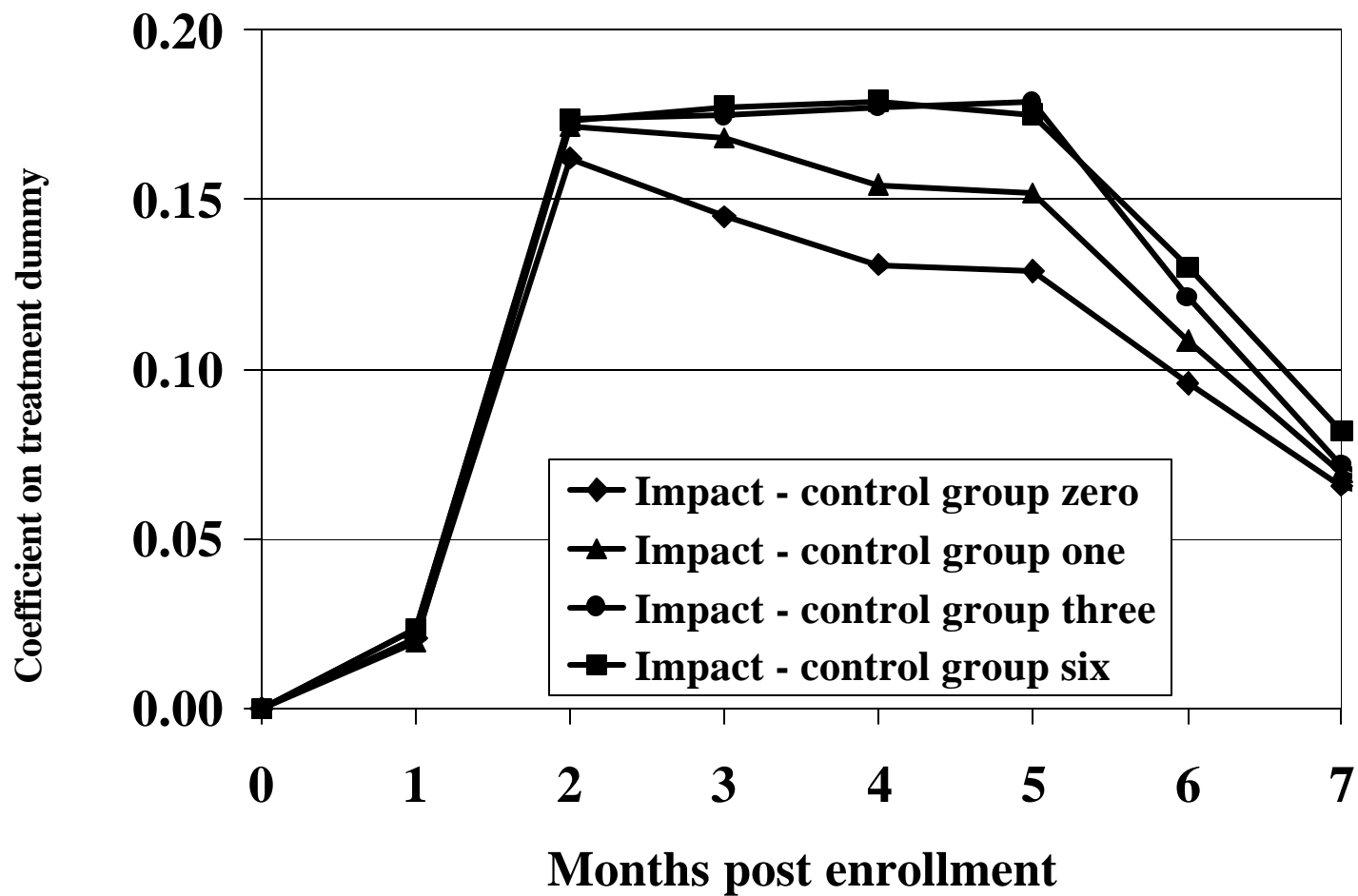


Figure 13: Coefficient on Treatment Dummy Using Control Groups Zero, One, Two, and Six, No Covariates and All Covariates

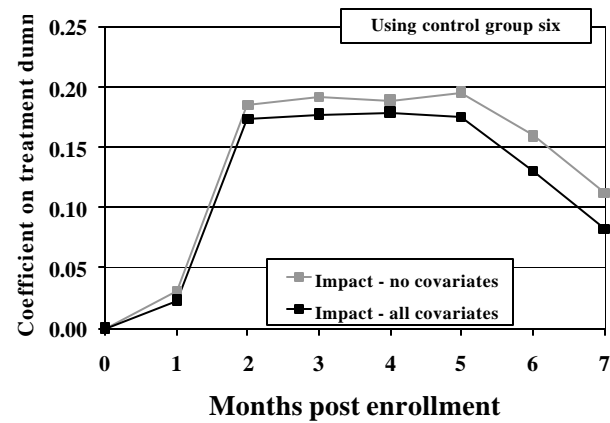
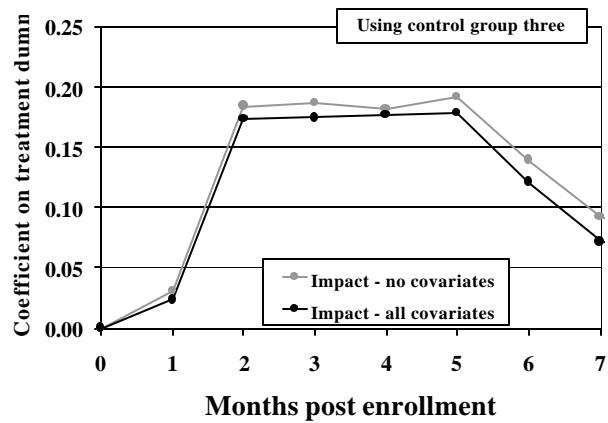
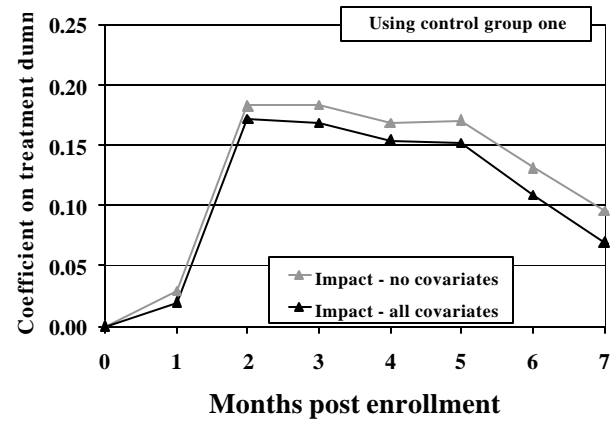
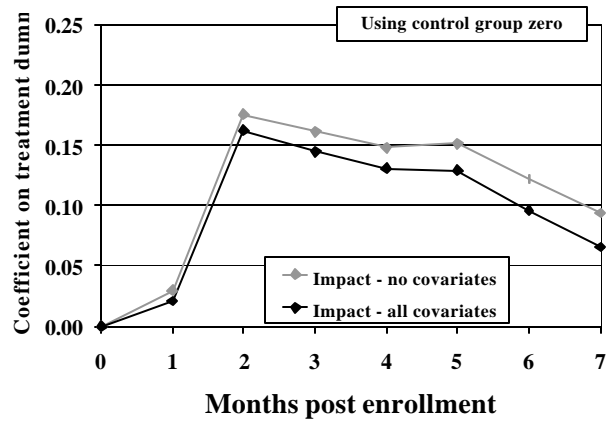


Figure 14: The Percent of Eligible HR Recipients that Receive a WEP Assignment, Treatment and Control Groups

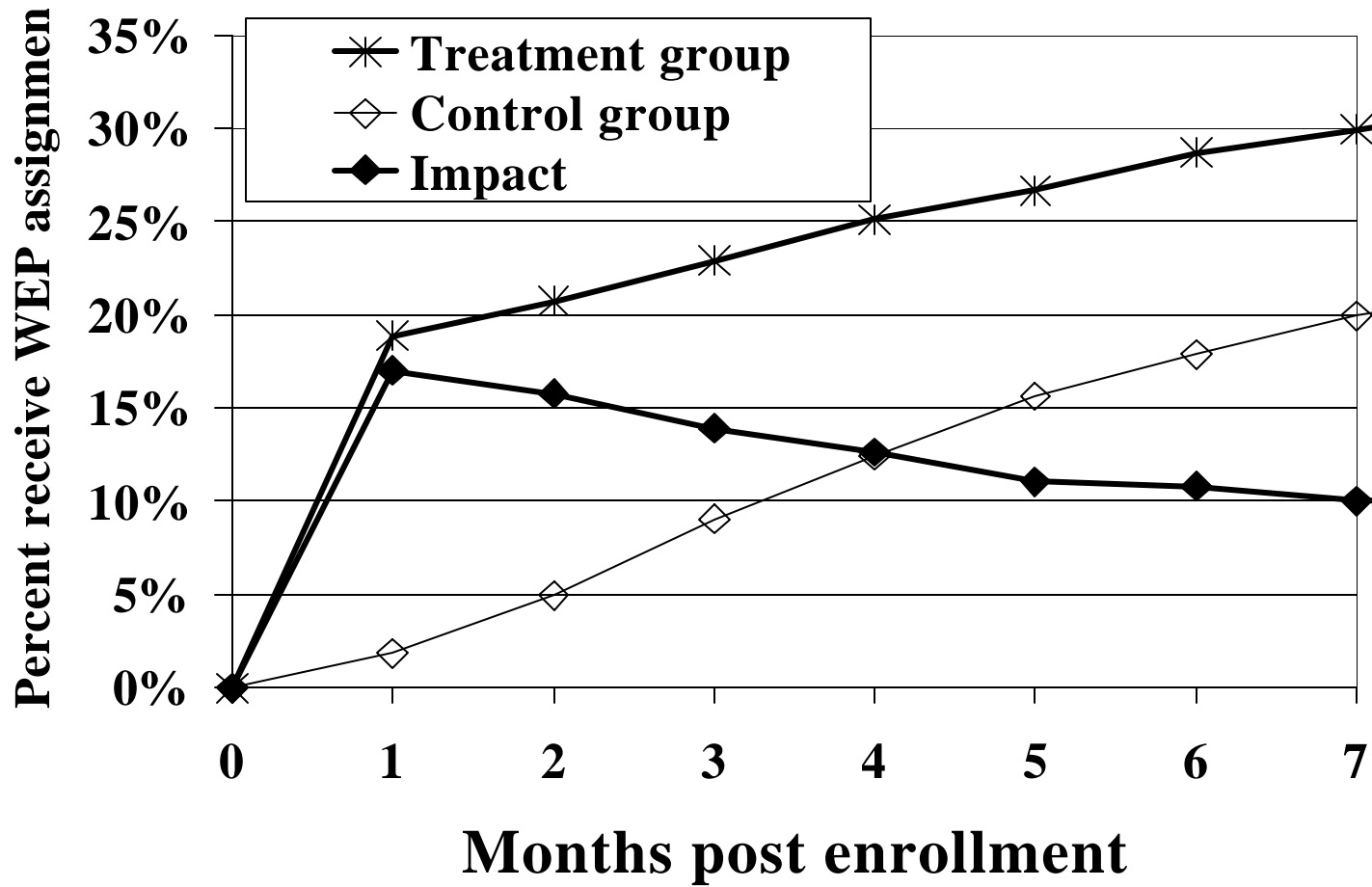


Figure 15: Coefficients on Treatment Dummy with Various Covariates Included, Receive a WEP Assignment is Dependent Variable

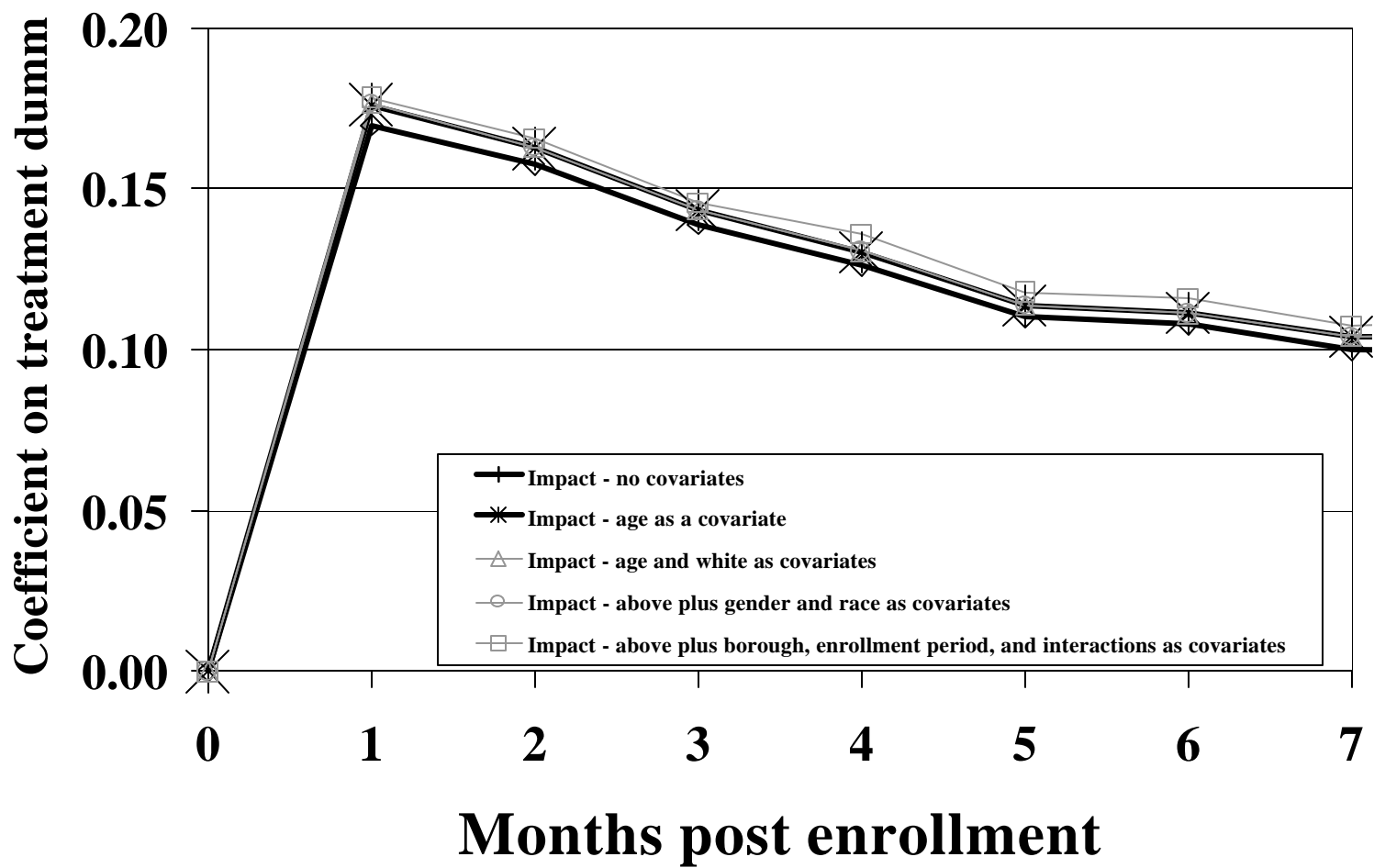


Figure 16: Coefficients on Treatment Dummy Using Control Groups Zero, One, Two, and Six, Receive a WEP Assignment is Dependent Variable, All Covariates Included

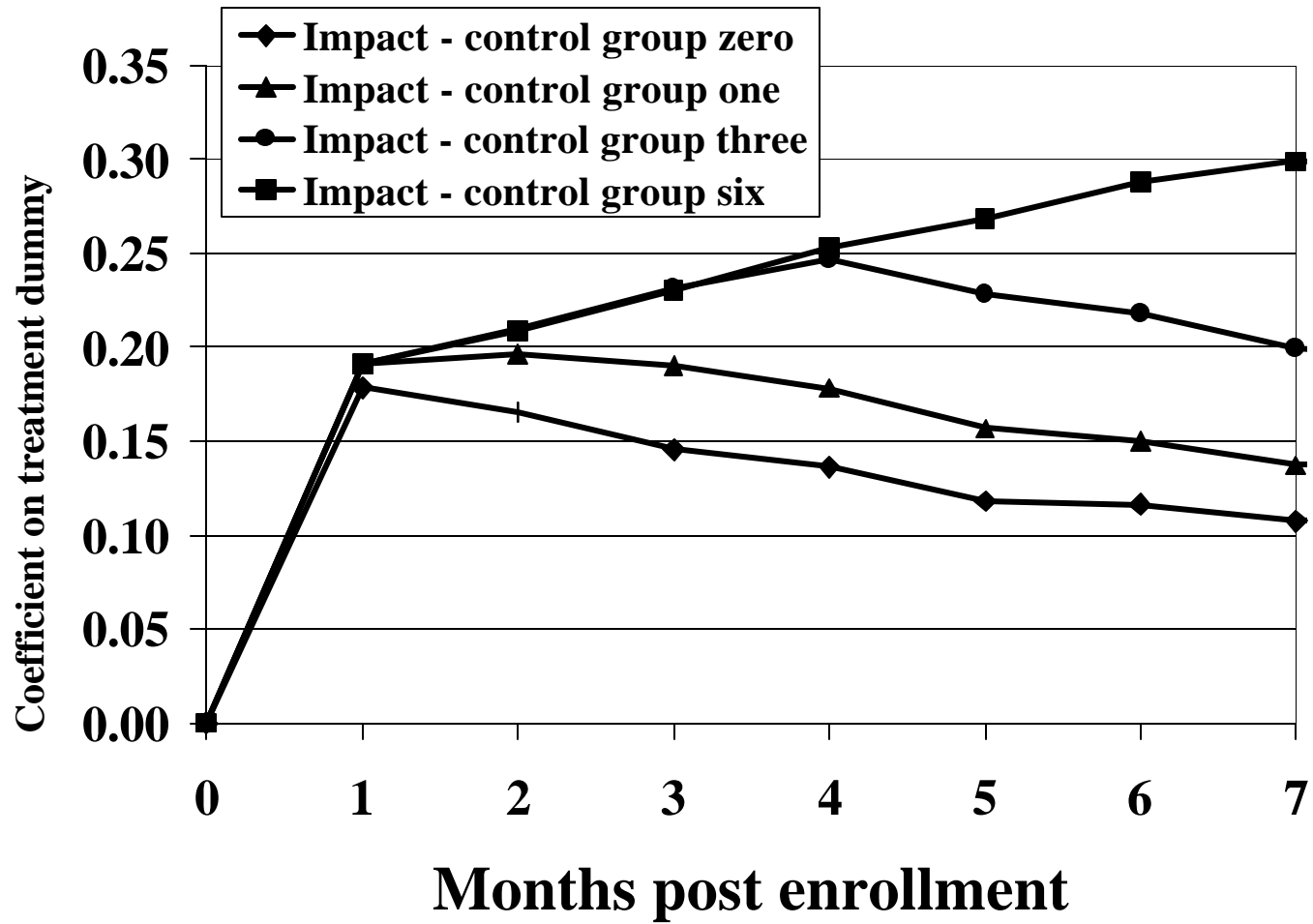


Figure 17: The Percent of Eligible HR Recipients that Exit Welfare, Treatment Group and Control group members ultimately treated

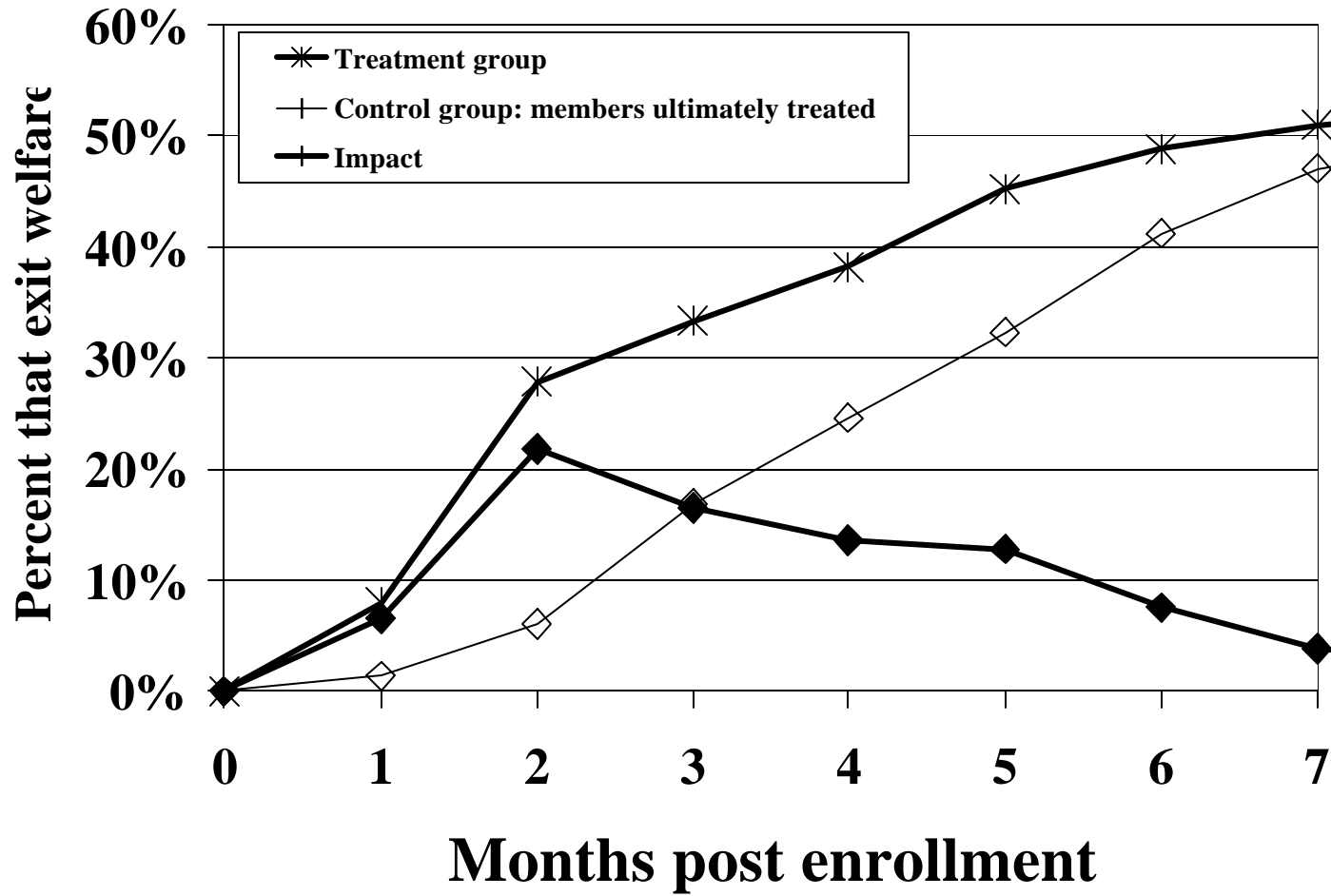


Figure 18: The Percent of Eligible HR Recipients that Exit Welfare, Recipients Selected and Recipients Eligible and not Selected: Ultimately Selected in March or April 1995

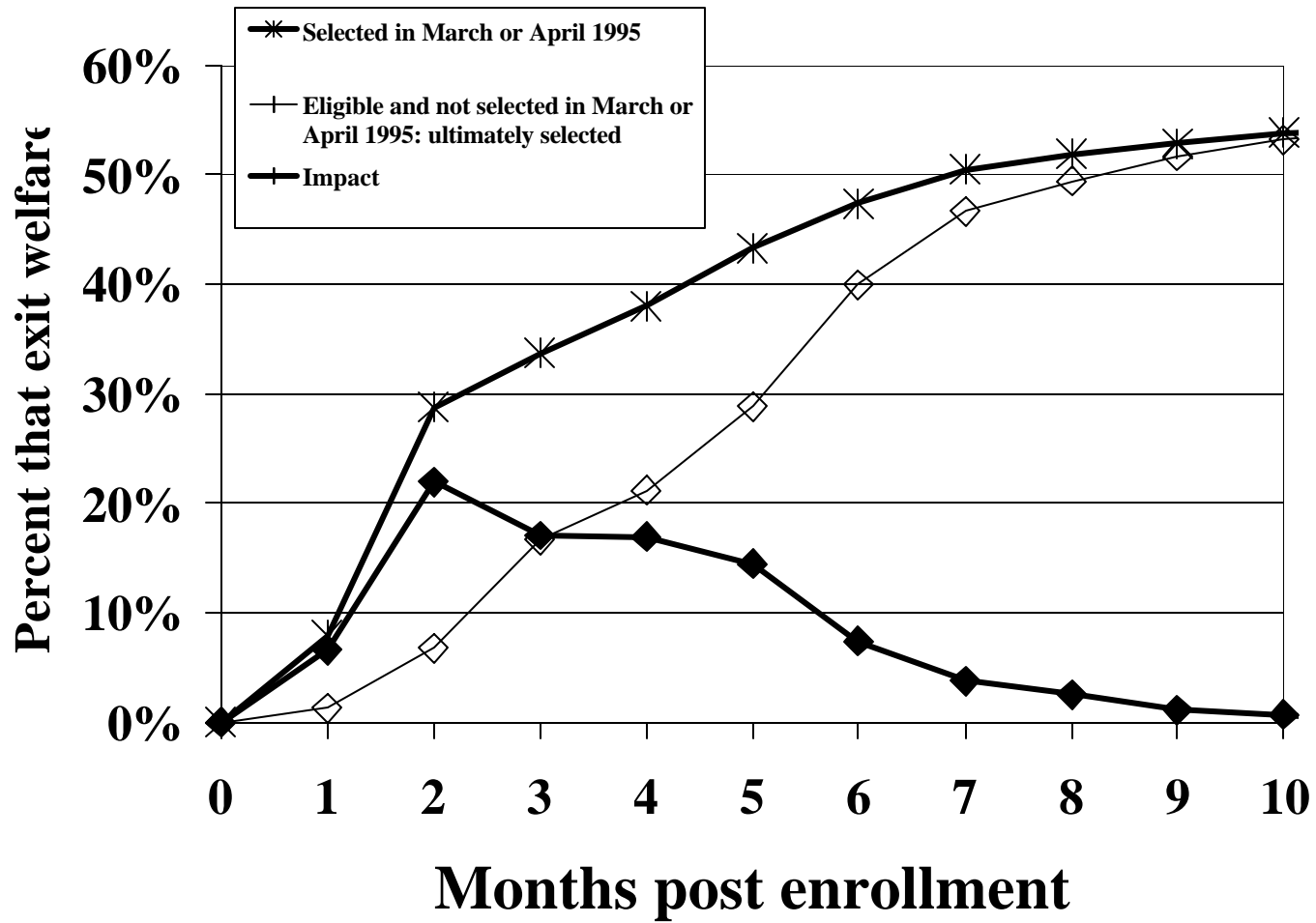


Figure 19: Coefficients on Treatment Dummy with Various Covariates Included Using Control group members ultimately treated

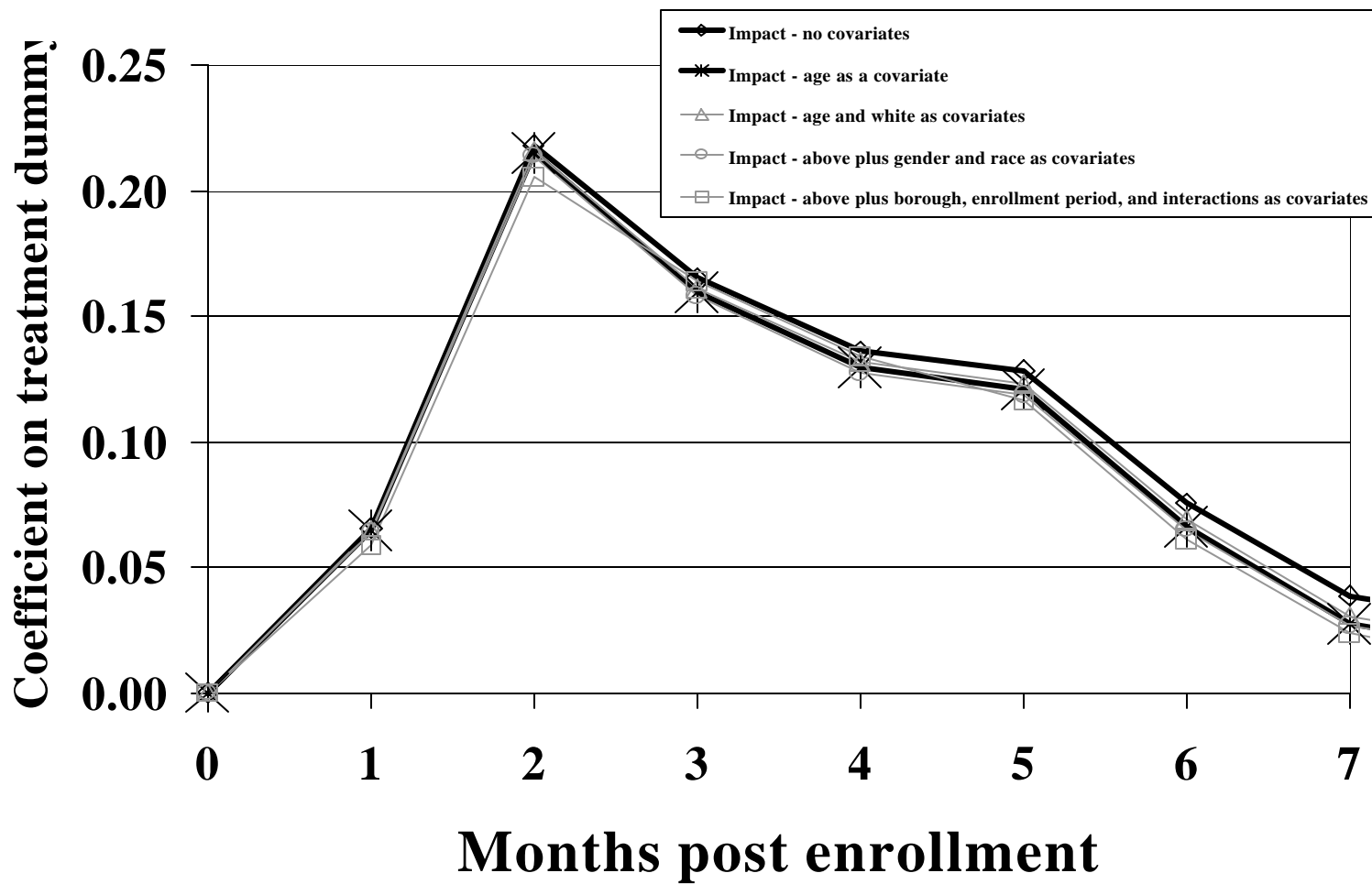


Figure 20: The Percent of Eligible HR Recipients that Exit Welfare, New Control Groups Zero, One, Three, and Six

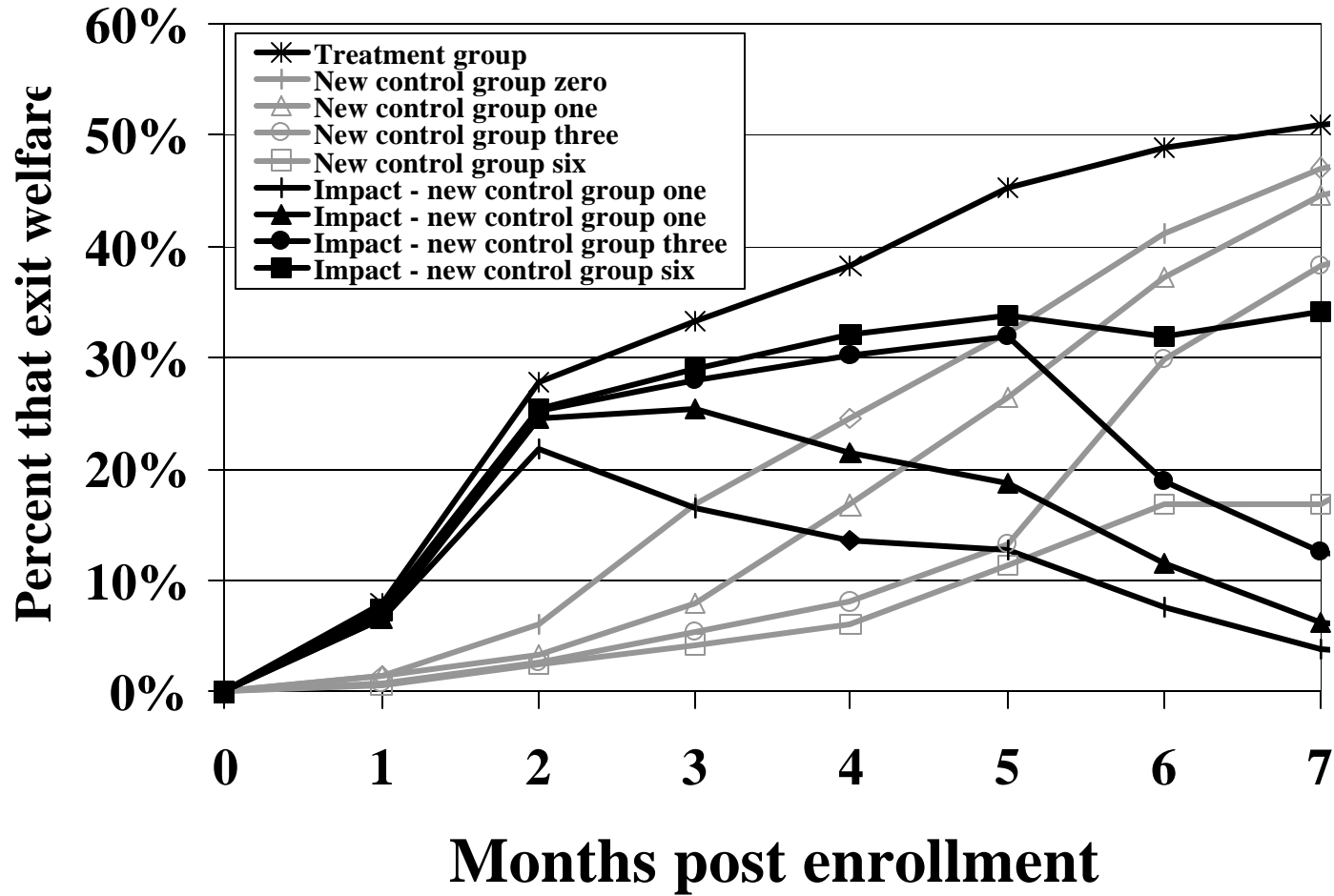
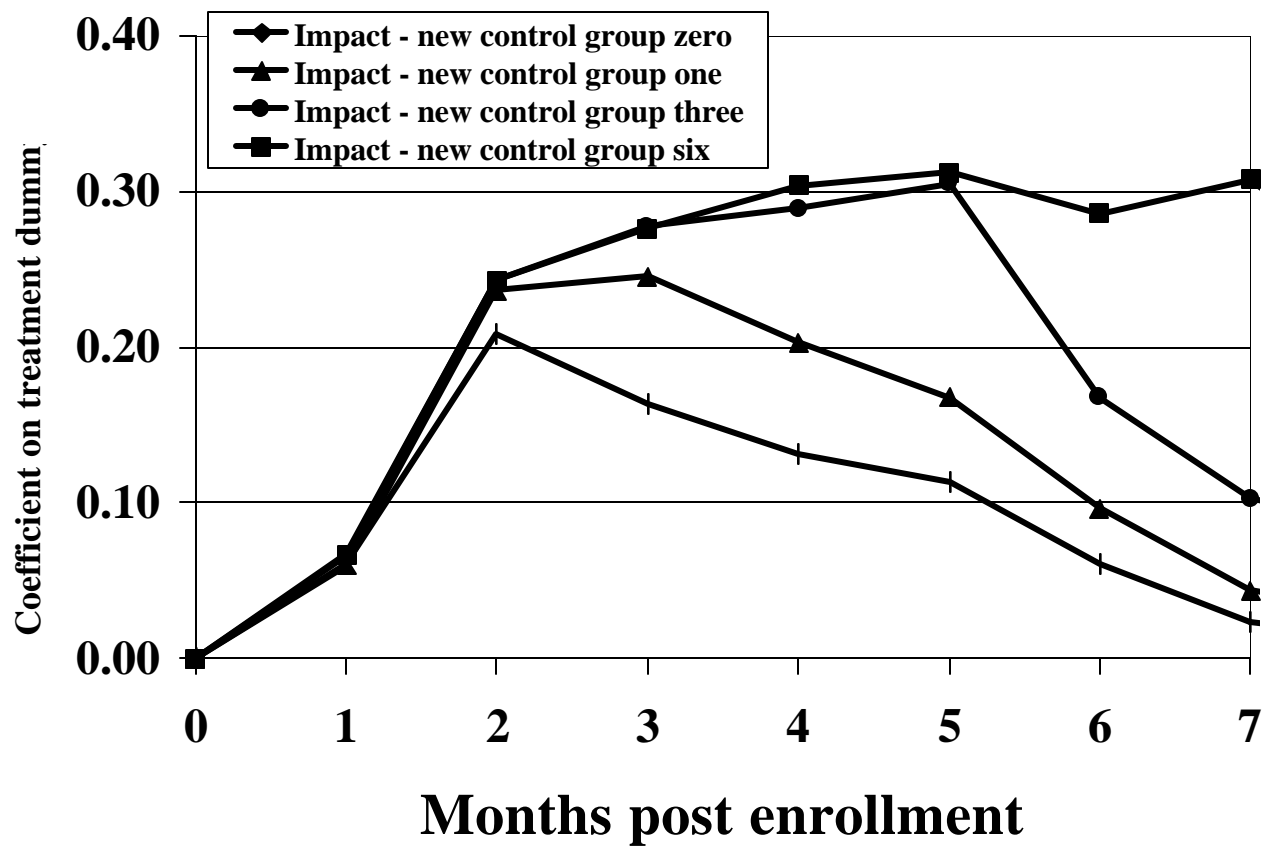


Figure 21: Coefficients on Treatment Dummy Using New Control Groups Zero, One, Two, and Six, All Covariates Included⁵³



⁵³ Except borough, enrollment period, and interaction dummies when using new control group six.

Figure 22: Coefficient on Treatment Dummy Using Control New Groups Zero, One, Two, and Six, No Covariates and All Covariates

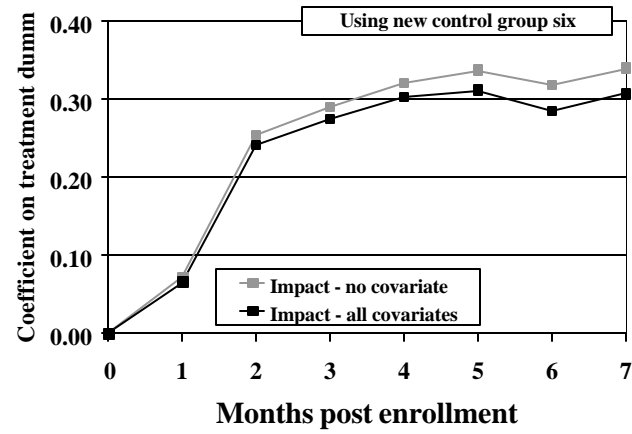
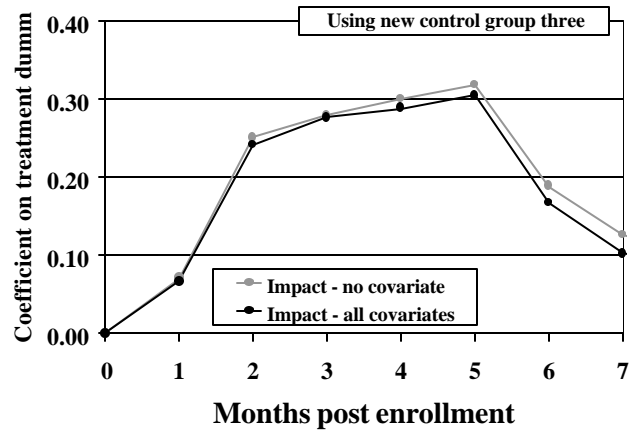
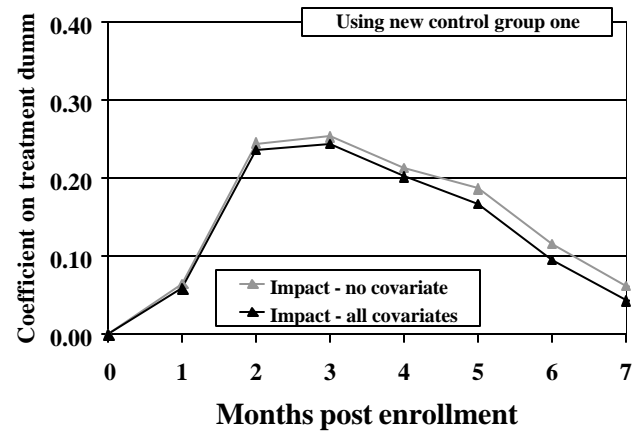
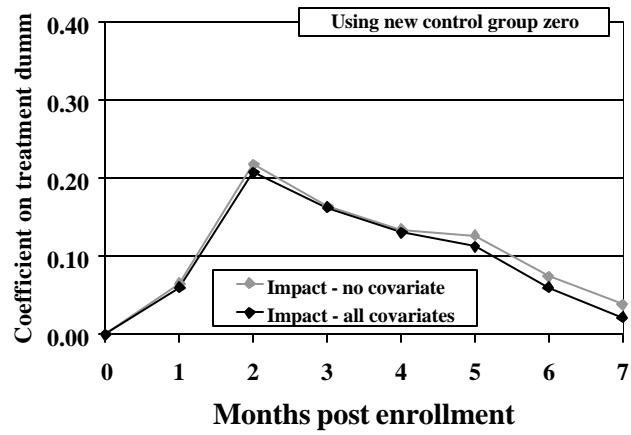


Figure 23: Coefficients on Treatment Dummy Using New Control Groups Zero, One, Two, and Six, Receive a WEP Assignment is Dependent Variable, All Covariates Included

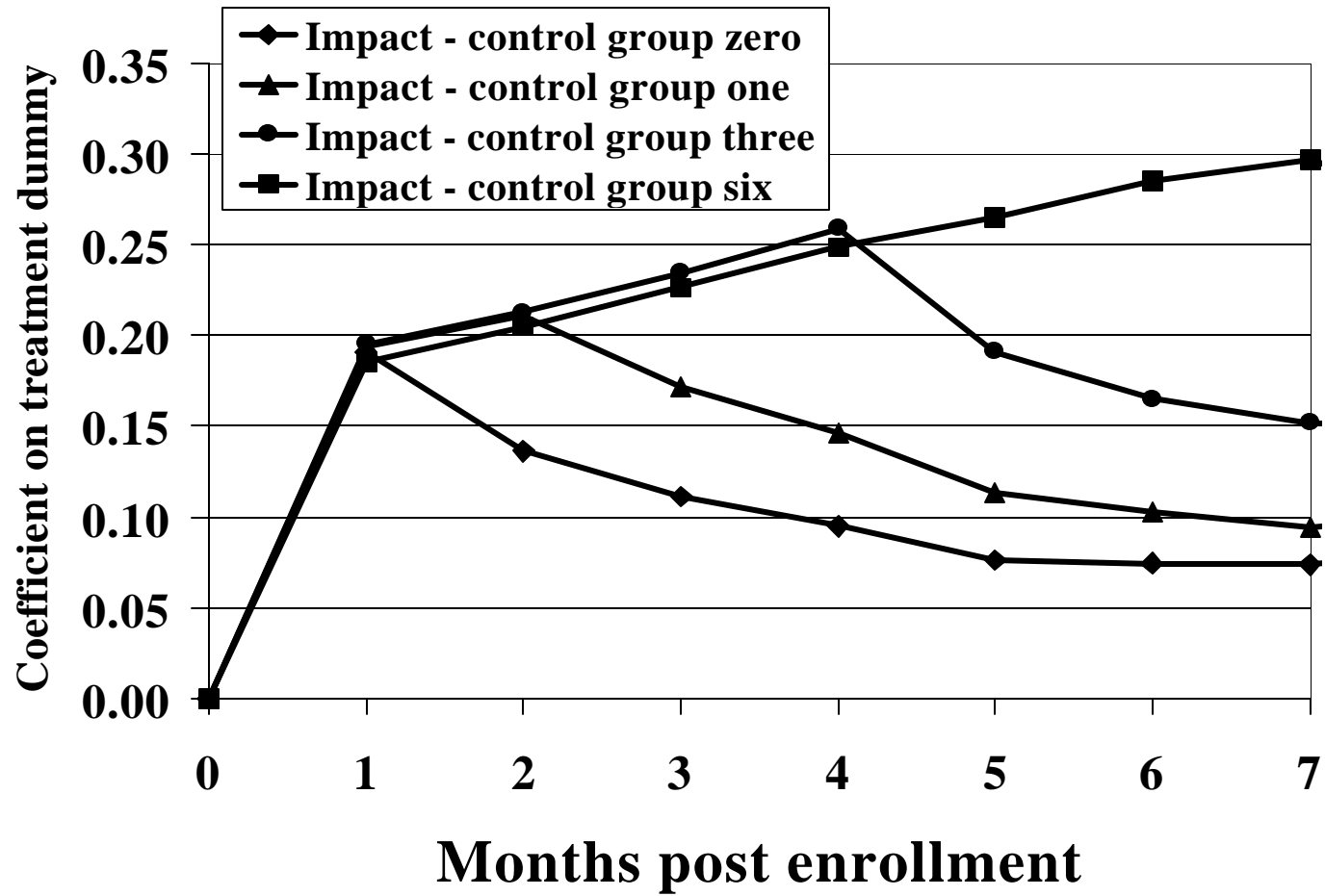


Table 1: Demographic Characteristics of Members of the Treatment and Control Groups

Demographic characteristic	Treatment group	Control group
Observations	642	4,196
Male	62.5%	60.5%
Race		
Black	44.4%	46.0%
Hispanic	24.0%	24.0%
White	16.8%	13.8%
Other	1.6%	1.3%
Not reported	13.2%	14.9%
Borough of residence		
Bronx	11.7%	15.7%
Brooklyn	35.2%	34.7%
Manhattan	40.8%	36.4%
Queens	10.1%	11.4%
Staten Island	2.2%	1.8%
Average age	35.4 (10.59)	38.9 (11.56)

Table 2: Coefficients on Demographic Characteristics from Probit Equation Estimation

Demographic characteristic	March 1995	April 1995	May 1995	July 1995
Male	0.0369	-0.0027	0.0999	-0.1473
Race				
Black	0.0462	0.0242	-0.1713	0.0556
Hispanic	0.1158	0.0885	-0.2543	0.2358
White	0.3113 **	-0.1035	0.3381 *	-0.0312
Borough of residence				
Bronx	-0.0236	-0.2833	-0.3848	-0.2812
Brooklyn	0.0861	-0.0843	-0.1044	-0.1904
Manhattan	0.0703	0.0713	0.3643	-0.9214 *
Queens	0.0724	-0.1751	-0.3711	-0.5411
Average age	-0.0205 ***	-0.0167 ***	-0.0038	-0.0173 ***

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

Table 3: Coefficients with Various Covariates Included, $M = 2$ (Peak Effect)

Demographic characteristic	(1)	(2)	(3)	(4)	(5)
Observations	4,838	4,812	4,812	4,812	4,812
Percent treated	13.27%	13.34%	13.34%	13.34%	13.34%
Treatment dummy	0.17562 ***	0.16730 ***	0.16719 ***	0.16663 ***	0.16225 ***
Age		-0.00221 ***	-0.00222 ***	-0.00204 ***	-0.00199 ***
Race					
Black				0.02777 *	0.02637 *
Hispanic				0.01843	0.01633
White			0.00260	0.01722	0.02153
Male				0.05565 ***	0.05102 ***
Borough					
Bronx					-0.25742
Brooklyn					-0.22692
Manhattan					-0.20387
Queens					-0.26511
Enrollment period dummies	NO	NO	NO	NO	YES
Interaction dummies[^]	NO	NO	NO	NO	YES

[^] Between borough and enrollment date

* signifies $p < 0.10$; ** signifies $p < 0.05$; and *** signifies $p < 0.01$

Table 4: Coefficients with Various Covariates Included, $M = 7$

Demographic characteristic	(1)	(2)	(3)	(4)	(5)
Observations	4,838	4,812	4,812	4,812	4,812
Percent treated	13.27%	13.34%	13.34%	13.34%	13.34%
Treatment dummy	0.09395 ***	0.06745 ***	0.06872 ***	0.06807 ***	0.06570 ***
Age		-0.00688 ***	-0.00679 ***	-0.00634 ***	-0.00634 ***
Race					
Black				0.09268 ***	0.09187 **
Hispanic				0.04006	0.03794
White			-0.03089	0.02016	0.02465
Male				0.09485 ***	0.09087 ***
Borough of residence					
Bronx					-0.17128
Brooklyn					-0.21158
Manhattan					-0.24218
Queens					-0.25973
Enrollment date dummies	NO	NO	NO	NO	YES
Interaction dummies[^]	NO	NO	NO	NO	YES

[^] Between borough and enrollment date

* signifies $p < 0.10$; ** signifies $p < 0.05$; and *** signifies $p < 0.01$

Table 5: Coefficients with all Covariates for Control Groups Zero, One, Three, and Six, $M = 2$ and $M = 7$

Demographic characteristic	Control group zero, $M = 2$ (1)	Control group one, $M = 2$ (2)	Control group three, $M = 2$ (3)	Control group six, $M = 2$ (4)	Control group zero, $M = 7$ (5)	Control group one, $M = 7$ (6)	Control group three, $M = 7$ (7)	Control group six, $M = 7$ (8)
Observations (unweighted)	4,812	3,735	2,687	1,971	4,812	3,735	2,687	1,971
Percent treated	13.34%	17.19%	23.89%	32.57%	13.34%	17.19%	23.89%	32.57%
Treatment dummy	0.16225 ***	0.17163 ***	0.17391 ***	0.17345 ***	0.06570 ***	0.06957 ***	0.07160 **	0.08189 **
Age	-0.00199 ***	-0.00173 ***	-0.00161 ***	-0.00185 ***	-0.00634 ***	-0.00593 ***	-0.00536 ***	-0.00498 ***
Race								
Black	0.02637 *	0.01990	0.01088	0.00570	0.09187 **	0.10079 **	0.08673 *	0.04064
Hispanic	0.01633	-0.00114	-0.00347	-0.00602	0.03794	0.03725	0.02017	-0.04566
White	0.02153	0.02181	0.01290	0.00009	0.02465	0.04778	0.04229	-0.01209
Male	0.05102 ***	0.04129 ***	0.03702 ***	0.03928 ***	0.09087 ***	0.07690 **	0.06884 *	0.08619 *
Borough of residence								
Bronx	-0.25742	-0.24644	-0.54181 **	-0.57312 **	-0.17128	-0.19054	-0.21755	-0.26726
Brooklyn	-0.22692	-0.22185	-0.53042 **	-0.55524 **	-0.21158	-0.20134	-0.30335	-0.35419
Manhattan	-0.20387	-0.20626	-0.51176 **	-0.49012 *	-0.24218	-0.24561	-0.26199	-0.23115
Queens	-0.26511	-0.26151	-0.57224 **	-0.58173 **	-0.25973	-0.28164	-0.30809	-0.35398
Enrollment date dummies	YES	YES	YES	YES	YES	YES	YES	YES
Interaction dummies[^]	YES	YES	YES	YES	YES	YES	YES	YES

[^] Between borough and enrollment date

* signifies $p < 0.10$; ** signifies $p < 0.05$; and *** signifies $p < 0.01$

Table 6: Coefficients with all Covariates for Control Groups Zero, One, Three, and Six, $M = 2$ and $M = 7$

Demographic characteristic	Control group zero, M = 1 (1)	Control group one, M = 1 (2)	Control group three, M = 1 (3)	Control group six, M = 1 (4)	Control group zero, M = 7 (5)	Control group one, M = 7 (6)	Control group three, M = 7 (7)	Control group six, M = 7 (8)
Observations (unweighted)	4,312	3,735	2,687	1,971	4,312	3,735	2,687	1,971
Percent treated	14.89%	17.19%	23.89%	32.57%	14.89%	17.19%	23.89%	32.57%
Treatment dummy	0.17842 ***	0.19063 ***	0.19096 ***	0.19099 ***	0.10731 ***	0.13713 ***	0.19931 ***	0.29874 ***
Age	0.00013	0.00021	0.00022	0.00020	-0.00024	-0.00021	-0.00072	0.00030
Race								
Black	0.00855	0.00683	0.00709	0.00786	-0.00902	-0.01812	-0.00416	0.00545
Hispanic	0.01000	0.00882	0.00953	0.01028	0.00713	-0.00325	0.00385	0.01504
White	-0.00088	-0.00340	-0.00183	-0.00045	0.00027	-0.01717	-0.01021	0.00328
Male	0.00596	0.00950 **	0.00992 **	0.00975 **	-0.00309	-0.00401	-0.00830	0.00880
Borough of residence								
Bronx	0.02334	0.01922	0.05403	0.05683	0.09270	0.07937	0.15772 **	0.13873
Brooklyn	0.06963 **	0.06652 *	0.10607 **	0.10473 **	0.00154	-0.01746	0.13659 **	0.14065 *
Manhattan	0.04172	0.03987	0.07813	0.07810	0.10841	0.09636	0.22034 ***	0.16344 **
Queens	0.06126	0.05856	0.09513 *	0.09666 *	0.05487	0.05299	0.17134 **	0.15202 *
Enrollment date dummies	YES	YES	YES	YES	YES	YES	YES	YES
Interaction dummies[^]	YES	YES	YES	YES	YES	YES	YES	YES

[^] Between borough and enrollment date

* signifies $p < 0.10$; ** signifies $p < 0.05$; and *** signifies $p < 0.01$

Table 7: Demographic Characteristics of Members of the Treatment Group and the Two Control Groups

Demographic characteristic	Treatment group	Control group: members ultimately treated	Original control group
Observations	642	1,005	4,196
Male	62.5%	60.2%	60.5%
Race			
Black	44.4%	44.8%	46.0%
Hispanic	24.0%	22.3%	24.0%
White	16.8%	15.0%	13.8%
Other	1.6%	0.7%	1.3%
Not reported	13.2%	17.2%	14.9%
Borough of residence			
Bronx	11.7%	15.2%	15.7%
Brooklyn	35.2%	35.4%	34.7%
Manhattan	40.8%	37.6%	36.4%
Queens	10.1%	10.0%	11.4%
Staten Island	2.2%	1.8%	1.8%
Average age	35.4	36.9	38.9
	(10.59)	(10.51)	(11.56)

Table 8: Coefficients on Demographic Characteristics from Probit Equation Estimation Using Control group members ultimately treated

Demographic characteristic	March 1995	April 1995	May 1995	July 1995
Male	0.0416	-0.0076	0.0823	-0.1701
Race				
Black	0.1234	0.1469	-0.0063	0.4521
Hispanic	0.2190	0.2871	0.0598	0.5693
White	0.3060 *	-0.2354	0.5539 *	0.3243
Borough of residence				
Bronx	0.2136	-0.4413	-0.7553	-0.0496
Brooklyn	0.2301	-0.2386	-0.3561	0.1427
Manhattan	0.0852	-0.0828	0.4606	-0.7739 *
Queens	0.3754	-0.2073	-0.4502	-[^]
Average age	-0.0172 ***	-0.0086	0.0184 **	-0.0141

[^] Control group members only came from four of the five boroughs of New York City. So coefficients could only be estimated for three boroughs.

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

Table 9: Coefficients with Various Covariates Included Using Control group members ultimately treated, $M = 2$ (Peak Effect)

Demographic characteristic	(1)	(2)	(3)	(4)	(5)
Observations	1,647	1,647	1,647	1,647	1,647
Percent treated	38.98%	38.98%	38.98%	38.98%	38.98%
Treatment dummy	0.21812 ***	0.21485 ***	0.21587 ***	0.21380 ***	0.20528 ***
Age		-0.00227 **	-0.00214 **	-0.00185 **	-0.00181 *
Race					
Black				0.03746	0.03511
Hispanic				0.04949	0.05507 *
White			-0.04662 **	-0.01588	-0.01519
Male				0.05475 ***	0.04860 **
Borough					
Bronx					-0.24420
Brooklyn					-0.14486
Manhattan					-0.16190
Queens					-0.27269
Enrollment period dummies	NO	NO	NO	NO	YES
Interaction dummies[^]	NO	NO	NO	NO	YES

[^] Between borough and enrollment date

* signifies $p < 0.10$; ** signifies $p < 0.05$; and *** signifies $p < 0.01$

Table 10: Coefficients with Various Covariates Included Using Control group members ultimately treated, $M = 7$

Demographic characteristic	(1)	(2)	(3)	(4)	(5)
Observations	1,647	1,647	1,647	1,647	1,647
Percent treated	38.98%	38.98%	38.98%	38.98%	38.98%
Treatment dummy	0.03870	0.02730	0.03066	0.02698	0.02343
Age		-0.00791 ***	-0.00748 ***	-0.00697 ***	-0.00676 ***
Race					
Black				0.06197	0.07817
Hispanic				0.08514	0.10135
White			-0.15223 ***	-0.10090	-0.07296
Male				0.10067 **	0.08358 *
Borough of residence					
Bronx					-0.00038
Brooklyn					-0.06128
Manhattan					-0.00213
Queens					-0.02962
Enrollment date dummies	NO	NO	NO	NO	YES
Interaction dummies[^]	NO	NO	NO	NO	YES

[^] Between borough and enrollment date

* signifies $p < 0.10$; ** signifies $p < 0.05$; and *** signifies $p < 0.01$

Table 11: Coefficients⁷ with all Covariates for New Control Groups Zero, One, Three, and Six, $M = 2$ and $M = 7$

Demographic characteristic	New control group zero, $M = 2$ (1)	New control group one, $M = 2$ (2)	New control group three, $M = 2$ (3)	New control group six, $M = 2$ (4)	New control group zero, $M = 7$ (5)	New control group one, $M = 7$ (6)	New control group three, $M = 7$ (7)	New control group six, $M = 7$ (8)
Observations (unweighted)	1,647	1,252	901	708	1,647	1,252	901	708
Percent treated	38.98%	51.28%	71.25%	90.68%	38.98%	51.28%	71.25%	90.68%
Treatment dummy	0.20528 ***	0.23206 ***	0.24013 ***	0.24274 ***	0.02343	0.04589	0.10644 **	0.30781 ***
Age	-0.00181 *	-0.00130	-0.00163 *	-0.00139	-0.00676 ***	-0.00604 **	-0.00448	0.00097
Race								
Black	0.03511	0.02052	0.01532	0.02109	0.07817	0.09283	0.10085	-0.00263
Hispanic	0.05507 *	0.01913	0.01349	0.04754	0.10135	0.11436	0.10529	0.03643
White	-0.01519	-0.02329	-0.02663	-0.01173	-0.07296	-0.06578	-0.06750	-0.09711
Male	0.04860 **	0.03352 *	0.01917	0.03197	0.08358 *	0.06189	0.07472	0.20417 **
Borough of residence								
Bronx	-0.24420	-0.22795	-0.22262	-^^	-0.00038	-0.10494	-0.10885	-^^
Brooklyn	-0.14486	-0.10413	-0.13575	-^^	-0.06128	-0.01529	-0.05460	-^^
Manhattan	-0.16190	-0.15155	-0.14231	-^^	-0.00213	-0.01802	-0.04870	-^^
Queens	-0.27269	-0.26427	-0.27105	-^^	-0.02962	-0.06829	-0.15446	-^^
Enrollment date dummies	YES	YES	YES	NO^^	YES	YES	YES	NO^^
Interaction dummies[^]	YES	YES	YES	NO^^	YES	YES	YES	NO^^

[^] Between borough and enrollment date

^^ borough, enrollment period, and interaction dummies were dropped when using new control group six due to the small size of the control group.

* signifies $p < 0.10$; ** signifies $p < 0.05$; and *** signifies $p < 0.01$

