

The Abrupt Impact of Welfare Reform:  
Evidence from Enrolling Recipients in New York City's Mandatory Workfare Program

by

John Ifcher<sup>1</sup>  
Santa Clara University  
Department of Economics

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Abstract

Starting in 1995, welfare recipients were required to participate in a mandatory workfare program. Initially, recipients were enrolled in weekly waves. I identify the effect of enrolling recipients (who were previously unexposed to welfare reform) in the program using a natural experiment in which enrollees are compared to concomitantly eligible, non-enrolled recipients. I find that enrollees were over eight percentage points more likely to exit welfare. The majority of the former recipients remained off welfare for at least six months. Observed differences were not due to macroeconomic shocks. The program passes a cost benefit test.

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<sup>1</sup> John Ifcher, Santa Clara University, Department of Economics, 500 El Camino Real, Santa Clara, CA, 95053, 408-554-5579 (phone), 408-554-2331 (fax), [jifcher@scu.edu](mailto:jifcher@scu.edu)

## 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 1995, in response to these concerns, New York City (NYC) initiated a series of welfare reforms including the Work Experience Program (WEP), a mandatory workfare program<sup>2</sup>. NYC's welfare caseload precipitously started contracting, ultimately shrinking by over 50%, after having grown by over 40% in the preceding six years.

There are many potential explanations for NYC's success in reforming its welfare programs. These explanations can be neatly divided into two categories, those related to the underlying economic conditions, and those related to changes in institutional factors. Prior research indicates that economic conditions alone generally cannot explain post-reform caseload reductions (Blank, 2002). This appears to be true in NYC as well. Comparing the welfare caseload and the unemployment rate in NYC, one observes an inconsistent relationship between the two prior to, and after, the reforms (see Figure 1). For example, they move in opposite directions from 1992 to 1997, and in the same direction from 1998 to 2001. Furthermore, assuming that there is a lag in the effect of the unemployment rate on the caseload does not explain the observed, inconsistent relationship. Thus, institutional factors appear to have had an impact.

NYC's welfare reforms were multi-faceted. In addition to the WEP, other components included enhanced substance abuse treatment, improved fraud detection, and

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<sup>2</sup> NYC's welfare reforms were initiated prior to the passage of the Personal Responsibility and Work Opportunity Reconciliation Act of 1996.

job training. The success of the reforms could have been the result of any single component, or some combination thereof.

In this paper, I demonstrate that there is sufficient evidence to conclude that enrolling recipients in the WEP contributed to the success of NYC's welfare reforms<sup>3</sup>. To do so, I take advantage of a quirk in the program's administration. Specifically, when the program was initiated, the entire cohort of eligible recipients could not be enrolled concurrently due to capacity constraints. Rather, recipients were enrolled in "waves." Computer programmers performed the enrollment process centrally. Intake interviews and objective assessments were not performed.

The wave enrollment process creates the opportunity to identify the effect of enrolling recipients in the WEP using an innovative, natural experiment in which recipients who were enrolled in a given month are compared to those who were eligible, but not enrolled, in that month. The observed post intervention difference in the exit rate from welfare is at least eight percentage points. Moreover, the difference in the exit rate appears to be the result of enrollment in the WEP, and not participation in the WEP.

The next section of this paper provides a brief overview of the previous literature. The third section presents a short description of NYC's welfare reforms. The fourth section describes the identification strategy. The fifth section discusses the empirical implementation. The sixth section presents the results. The seventh section describes a robustness check that was performed. The final section discusses the implications of the results and presents a brief cost benefit analysis.

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<sup>3</sup> In a similar paper Ifcher (2006) demonstrates that a second component of NYC's welfare reforms, job training, increased labor force participation and exits from welfare.

## II. Previous literature

Many researchers have investigated the impact of welfare reform<sup>4</sup> and there appears to be a consensus developing that it has had the intended effect, reducing welfare caseloads and increasing the labor force participation of former recipients (for example, Blank, 2002; Moffitt, 2003). Unfortunately, that is where the consensus seems to end. Attempts to identify the effect of individual components of welfare reform, for example, time limits or workfare, have not been as persuasive.

One branch of the literature has attempted to use aggregate data, or large nationally representative datasets, to identify the effects of welfare reform. For example, Blank (2001) uses the month in which recipients within a state were exposed to a bundle of reforms to identify the effect of welfare reform. Such a strategy necessarily limits one's ability to identify the effect of an individual component of welfare reform. Attempts to solve this problem have been hampered by the large variety of welfare reform programs and the timing of the reforms (Blank, 2002).

This research is methodologically more similar to another branch of the literature that generally uses administrative data, in combination with random assignment experiments, to attempt to identify the impact of welfare reform authorized under the Family Support Act of 1988. Generally, such research has found that mandatory welfare-to-work employment programs have decreased welfare use and increased annual earnings between \$200 and \$600 (Blank, 2002).

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<sup>4</sup> Here welfare reform refers to both the waiver reforms authorized under the Family Support Act of 1988 and the Personal Responsibility and Work Opportunity Reconciliation Act of 1996.

This study differs from the previous studies in several important respects. First, this study estimates the impact of enrolling recipients in the WEP, regardless of whether, or not, they participate. In contrast, other studies generally attempt to estimate the impact of welfare-to-work programs. This distinction is important since it appears that simply enrolling recipients (who had been previously unexposed to welfare reform) in a welfare reform program generates a statistically significant increase in the welfare exit rate. Consequently, previous estimates of the impact of welfare-to-work programs may be overstated since a portion of the observed effect may have been the result of simply exposing recipients (who had been previously unexposed) to welfare reform.

Second, the previous studies generally included randomized experiments. Although the results of randomized experiments are highly convincing, these studies are still hindered by the fact that multiple components of welfare reform were often implemented at approximately the same time. Consequently, the observed effects may be the result of a combination of components. In contrast, the WEP was implemented independent of other welfare reform components. Moreover, this study identifies the effect of a genuine, non-experimental welfare reform using a natural experiment.

Third, previous studies investigated the impact of welfare reform on Family Assistance (FA) recipients. This study investigates the impact of welfare reform on General Assistance (GA) recipients<sup>5,6</sup>, and includes slightly more men than women. Previous studies found that while mandatory employment programs had a positive effect on the mean annual earnings of female recipients of FA, they had no effect on male recipients of FA (Friedlander et al, 1997). In contrast, this study found that the WEP had

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<sup>5</sup> GA is welfare for childless adults.

<sup>6</sup> Concerns regarding generalizing the findings to FA recipients are explored in the discussion section.

a larger effect on men than on women. Finally, it evaluates the effect of welfare reform in the NYC, where over five percent of all US welfare recipients live.

### III. An overview of NYC's welfare reforms

In 1994, Rudolph W. Giuliani, NYC's newly elected mayor, made reducing the city's welfare caseload a priority. At the time, there were over one million welfare recipients in NYC, including approximately 300,000 Home Relief (HR)<sup>7</sup> recipients, and the city was spending approximately three billion dollars annually on welfare programs, including over one billion dollars on HR.

In early 1995, NYC initiated the Work, Accountability, and You (NYCWAY) program for HR recipients<sup>8</sup>. A central tenet of NYCWAY was that able-bodied welfare recipients were expected to work in exchange for their benefits. Almost all recipients fulfilled this requirement by participating in a WEP assignment<sup>9</sup>. The welfare caseload precipitously started to contract (from 1,150,509 in 1995 to 533,284 in 2001), after having grown continuously for the preceding six years (from 813,420 to 1,150,509). During the same period, the HR caseload declined by 70% (from 297,102 to 87,293).

#### A. Description of the WEP

Dozens of city agencies were enlisted to create tens of thousands of WEP assignments. Three departments – Parks and Recreation (Parks), Sanitation, and Transportation – created and managed the bulk of assignments. The majority of WEP participants worked outdoors removing litter, weeds, and graffiti from parks, vacant lots,

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<sup>7</sup> HR was the name of NYC's general assistance (GA) program, welfare for childless adults. It was subsequently renamed the Safety Net Assistance (SNA) program.

<sup>8</sup> Starting in 1996, FA recipients were required to participate in NYCWAY as well.

<sup>9</sup> Other components of NYCWAY included enhanced detection of welfare fraud, increased substance abuse treatment, and diversion to federally funded disability insurance where appropriate.

streets, and highways. Those that worked indoors generally performed clerical and janitorial tasks.

The WEP was mandatory for all able-bodied HR recipients. Participants received no compensation other than their welfare benefits and a nominal stipend for carfare and lunch<sup>10</sup>. Recipients were generally required to participate in WEP 21 hours per week.

#### B. Receiving a WEP assignment

Prior to receiving a WEP assignment, enrollees were required to attend a WEP intake interview. At the interview, enrollees had the opportunity to request an exemption from the WEP based on a verifiable medical condition, or a personal hardship, that prevented them from participating. If no exemption was requested, recipients were given an assignment letter instructing them to report to a WEP assignment at a prescribed date and time.

A substantial proportion of enrollees failed to attend their intake interview<sup>11</sup>. Of those that did attend, many requested an exemption from the WEP<sup>12</sup>. In total, less than 50% of enrollees received a WEP assignment on the day of their initial intake interview.

#### C. The treatment – selection for a WEP intake interview

Any attempt to estimate the effect of participating in, or even receiving, a WEP assignment would be biased, since enrollees could, at least temporarily, self-select out of an assignment. Consequently, this study estimates the effect of being enrolled in the WEP. That is, recipients are considered treated if they were simply selected for a WEP

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<sup>10</sup> The stipend was reduced in 1996 to only cover the cost of carfare.

<sup>11</sup> Enrollees who failed to attend their interview were sanctioned. They had an opportunity to appeal the sanction before it was implemented.

<sup>12</sup> These requests were evaluated on a case-by-case basis.

intake interview, regardless of whether or not they attended the intake interview or received an assignment.

#### D. The population – able-bodied HR recipients

In early 1995, prior to implementing the WEP, there were approximately 100,000 able-bodied HR recipients. Post implementation, these “preexisting” recipients were required to participate in the WEP and form the population for this study.

#### E. The selection process – choosing enrollees for the weekly waves

Since not all pre-existing recipients could be enrolled simultaneously, recipients were selected for the WEP in waves. Beginning in early 1995, a new wave was selected each week. Enrollees were instructed by mail to report to a WEP intake interview at a prescribed date, time, and place<sup>13</sup>. The letter stated that if they failed to comply with program requirements they would be sanctioned. The enrollment process continued until each preexisting recipient was enrolled or had become ineligible<sup>14</sup>.

Computer programmers conducted the weekly selection process centrally. Enrollees were selected without an intake interview or objective assessment, in which human discretion could have played a role. The exact selection criteria changed frequently<sup>15</sup> and were not documented<sup>16</sup>. 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, one should be able to adjust for differences in observed characteristics.

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<sup>13</sup> Recipients typically received the letter two weeks prior to the reporting date.

<sup>14</sup> To be eligible, a recipient had to continue to be able-bodied and on welfare.

<sup>15</sup> For example, one wave may have included a large proportion of recipients from Queens and the next may have included a large proportion of recipients from Brooklyn.

<sup>16</sup> Neither the program managers, nor the computer programmers, recorded the criteria.

#### IV. Identification strategy

Generally, to identify the effect of a program, one compares the outcomes of treated and untreated individuals. In the case of the WEP, each member of the population who remained eligible was ultimately treated. The only members of the population who were never treated must have become ineligible prior to being selected for the WEP, and thus, do not make a valid comparison group, since becoming ineligible is endogenous<sup>17</sup>.

##### A. An alternative approach to identifying the effect of the WEP

It is possible, however, to estimate the effect of the WEP taking advantage of the wave enrollment process. Specifically, the effect of the program is estimated using a natural experiment in which all enrollees from a wave are compared to all members of the population who were eligible to be enrolled in the same wave but were not. By combining all the enrollees from March, April, May, June, and July 1995 into one group, the treatment group, and all eligible non-enrollees during each of the aforementioned months into another group, the control group<sup>18</sup>, one can estimate the Post Intervention Difference (PID) (see Figure 2). Specifically, the PID 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  $i$  exited welfare within  $M$  months of the enrollment date and zero otherwise, and is a function of whether recipient  $i$  was a member of the treatment group,  $T_i=1$ , or the control group,  $T_i=0$ .

The PID does not suffer from control group attrition, since each recipient who is placed in the control group remains in the control group for the entire study. For

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<sup>17</sup> For example, recipients who exited welfare became ineligible.

<sup>18</sup> Many recipients were eligible to be enrolled, but were not, on multiple dates. These recipients will be members of the control group multiple times.

example, control group members who subsequently were treated or exited welfare remain in the control group. On the other hand, the PID does suffer from control group contamination, since 57% of the members of the control group were treated within one year of being included in the control group. Consequently, the PID will be negatively biased and a conservative estimate of the actual effect of enrollment in the WEP.

#### B. Adjusting for control group contamination

To adjust for control group contamination, a set of additional, restricted control groups is created in which recipients are removed from the control group if they were treated within a given number of months of their inclusion in the control group. For example, a control group with a one-month restriction on being treated is created by removing all recipients from the original control group who were treated within one month of their inclusion in the original control group. So a recipient who was eligible and not enrolled in March but enrolled in April is excluded from this new restricted control group.

As described, the control group with a one-month restriction on being treated introduces a selection problem. Specifically, it contains two distinct cohorts:

Cohort 1. Members who became ineligible prior to the subsequent wave

Cohort 2. Members who were eligible but not enrolled in the subsequent wave

All recipients who are in the original unrestricted control group but are not in the new restricted control group must have been eligible to be selected in the subsequent wave, and thus, would have been in cohort two if they had not been selected in the subsequent wave.

For the new restricted control group to be comparable to the treatment group the proportion of cohort two members in the new restricted control group must be the same as in the original unrestricted control group<sup>19</sup>. Accordingly, the weight placed on each member of cohort two (in the new restricted control group) is increased such that the proportion of cohort two members in the new restricted control group is the same as in the original unrestricted control group. Specifically, the weight placed on each member of cohort two is the reciprocal of the probability that members of the unrestricted control group were not selected, conditional on being eligible to be, in the subsequent wave (see Figure 3). One can extend this approach to create additional, more restrictive control groups. Control groups with a one-, and three-month restriction on being treated, denoted respectively as control group one and control group three, are used in this paper. Thus, one can partially, but not fully, adjust for control group contamination.

## V. Empirical implementation

With NYC's permission a simple random sample of 3,595 recipients was drawn from the population. For each member of the random sample, an abridged case history, and a limited set of demographic characteristics, was compiled from an administrative database. From this data the treatment and control groups were formed as described in the previous section.

### A. Three data collection issues and their impact

First, due to limited financial and human resources, it was only possible to compile 13 months of data (from February 1995 to February 1996) for a random sample

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<sup>19</sup> This is true for the following two reasons. First, a recipient's membership in one of the two above cohorts is not exogenous. Second, the design of this natural experiment ensures that the initial control group is comparable to the treatment group.

of the population (not the entire population)<sup>20</sup>. These limitations do not interfere with demonstrating that the PID is positive and statistically significant since the effect of WEP is swift and substantial.

Second, due to the dated design of the administrative database, each recipient's case and WEP status was only observable at the end of each month<sup>21</sup>. Consequently, any change in case or WEP status that occurred between the end-of-month observations was missed. For example, if a recipient exited welfare at the beginning of a month but returned later in that same month, no change in case status would be observed. Likewise, if a recipient was treated (selected for a WEP intake interview) at the beginning of a month, it might not be observed since the corresponding WEP status typically persisted in the administrative database for less than a month. The most significant impact of this limitation is that it generates a negative bias in the estimated PID since all recipients who were treated, but not observed to be, are placed in the control group. The magnitude of this bias is explored in the discussion section.

Third, due to a programming error, recipients' end-of-month case and WEP status were not collected after they received a WEP assignment. In such cases, it is conservatively assumed that the recipient does not exit welfare thereafter. This also negatively biases the estimated PID since members of the treatment group are significantly more likely to have received a WEP assignment than are members of the control group. The magnitude of this bias is also explored in the discussion section.

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<sup>20</sup> NYC did not have the capability to create an electronic dataset from the administrative database. Rather, the necessary data was printed on paper and subsequently transcribed into an analytic database. Furthermore, the administrative database had been archived and was stored on backup tapes, which had to be retrieved from a long-term storage facility in Albany, NY.

<sup>21</sup> The data for this study was collected from a legacy database system that was no longer in use. NYC only saved end-of-month "snapshots" of the data. Each snapshot included recipients' case and WEP status as well as various demographic characteristics.

## B. Comparing the treatment and control groups

Comparing the demographic characteristics of members of the treatment and control group, one observes that the two groups have relatively similar borough, gender, and racial distributions (see columns (1) and (2) of Table 1). The most striking difference is that members of the treatment group are, on average, three years younger than are members of the control group.

To determine whether the enrollment process approximated a random one, I test whether a recipient's demographic characteristics significantly impacted the probability that he or she was selected for the WEP. Specifically, the probit equation below is estimated for recipients who were selected, or were eligible but not selected, in each of the following months: March, April, May, June, and July 1995<sup>22</sup>, i.e.,

$$P[T_i^m = 1] = F(\alpha^m + \sum_{c=1}^C \lambda_c^m x_{ic}^m + \varepsilon_i^m) \quad (2)$$

where  $T_i^m$  is a treatment dummy that equals one if individual  $i$  was treated in month  $m$  and zero if individual  $i$  was not treated, but was eligible to be, in month  $m$ ; and  $x_{ic}^m$  is a series of  $C$  demographic characteristics for individual  $i$  in month  $m$ .

The coefficients on age provide strong evidence that the enrollment process did not approximate a random one. All are negative and significantly different than zero (see Table 2). This indicates that younger recipients were more likely to be treated than were older recipients.

One thing is certain though; eligible recipients were selected solely using information that was stored in the administrative database. The selection process was

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<sup>22</sup> One would preferably perform the above test for each weekly wave since the selection criteria changed frequently. The only available data, however, was the end-of-month case and WEP status. Thus the less precise monthly approach is employed.

centralized and conducted by computer programmers. Individual caseworkers were not involved in any manner; and no intake interviews, or objective assessments, were conducted prior to selection. 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, one should be able to adjust for the observed differences.

### C. Adjusting for observed demographic differences

A treatment dummy and a series of demographic characteristics are regressed on an outcome dummy, i.e.,

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

where  $y_i^M$  is an outcome dummy that equals one if individual  $i$  exited 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 seven<sup>23</sup>, and corrected standard errors are calculated by clustering the observations by individual<sup>24</sup>.

### D. Controlling for wave and borough of residence

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<sup>23</sup> There is only seven months of post enrollment data for each recipient in the study.

<sup>24</sup> This is necessary since many individuals appear in the dataset repeatedly.

One might be concerned that wave or borough of residence may have an impact on the estimate of the PID. To address this concern, borough, wave, and interaction (between wave and borough) dummies are added to equation (3), i.e.,

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

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  $W_{ik}$  is a wave dummy that equals one if individual  $i$  was placed in the treatment or control group in wave  $k$  and zero otherwise<sup>25</sup>.

## VI. Results

The PID peaks at nine and a half percentage points two months post enrollment, at which time members of the treatment group are 68% more likely to have exited welfare than are members of the control group (24.0% versus 14.3%) (see Figure 4). The PID then steadily declines toward zero seven months post enrollment (44.7% versus 44.3%).

Moreover, the large observed PID two months post enrollment is not simply the result of recipients “churning” on and off welfare. Redefining  $Y_i^M(T_i)$  in equation (1) as an indicator function that equals one if recipient  $i$  exited and remained off welfare for two consecutive months within  $M$  months of his or her inclusion in the treatment or control group, and zero otherwise. One finds that the shape of the PID remains largely the same. The PID peaks at eight and a half percentage points two months post enrollment, at which time members of the treatment group are 70% more likely to have exited and remained

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<sup>25</sup> As before, regression coefficients are estimated using OLS with corrected standard errors for values of  $M$  between one and seven.

off welfare for two consecutive months than are members of the control group. Similar results follow when one uses three or six months consecutively off welfare in the above analysis.

#### A. Adjusting for control group contamination

Calculating the PID using control groups one and three, one finds that as the length of the restriction on control group members being treated increases the “peak difference” persists longer (see Figure 5). Additionally, the PID using control group three is superimposed over the PID using control group one for the first three months post enrollment. The former does not diverge from the latter until after the restriction on membership in the latter control group expires. The same pattern is observed when comparing the PID using control group one and the initial unrestricted control group. Thus, the PID using control group three is probably the upper envelope of the true PID for three months post enrollment. After that, the true PID presumably diverges from, and lies above, this PID.

#### B. Adjusting for observed demographic differences

To determine if the observed differences in demographic characteristics between the treatment and control group have an impact, equation (3) is estimated with, and without, the demographic characteristics included using control group three. The coefficients on the treatment dummy are positive and significantly different than zero with, and without, the demographic characteristics for values of  $M$  between two and five. The coefficients are, on average, two percentage points, or a little more than one standard error, smaller when the demographic characteristics are included in the regression for values of  $M$  between two and seven (see Figure 6). This is not surprising since the

probability of being treated was lower for older recipients, and such recipients were presumably less likely to exit welfare. This presumption is supported by the fact that the coefficients on age are negative and significantly different than zero for all values of  $M$  (see columns (3) and (7) of Table 3). Finally, the coefficients on male are positive and significantly different than zero for all values of  $M$ , indicating that male recipients are more likely to have exited welfare than are female recipients. Similar result follows when one uses the initial unrestricted control group or control group one in the above analysis.

### C. Controlling for borough and wave

To control for borough and wave, equation (4) is estimated using control group three. The resulting coefficients are very similar to those that were obtained when equation (3) was estimated (see Figure 6 and columns (4) and (8) of Table 4). Moreover, including borough, wave, and interaction dummies should control for any macroeconomic shocks that may have occurred during the study period. Thus, the observed PID is not simply the result of the underlying economic conditions. Again, the above findings hold regardless of which control group is used.

### D. Treated recipients are more likely to have received a WEP assignment

It is also interesting to determine whether being treated (selected for a WEP intake interview) increased the probability that a recipient received a WEP assignment. Equation (4) is estimated with  $y_i^M$  redefined as an indicator function that equals one if recipient  $i$  received a WEP assignment within  $M$  months of his or her inclusion in the treatment or control group, and zero otherwise.

The coefficients on the treatment dummy are positive and significantly different than zero for all values of  $M$  and using all control groups. The PID peaks at 0.20 ( $t = 12.9, p = 0$ ), indicating that treated recipients are 20 percentage points more likely to have received a WEP assignment than are members of the control group, when  $M$  equals three and using control group three (see Figure 7). The coefficients on age are negative and significantly different than zero for all values of  $M$ , indicating that older recipients are less likely to have received a WEP assignment than are younger recipients. The coefficients on male are not significantly different than zero for all values of  $M$ . Thus, although age impacts the likelihood of both exiting welfare and receiving a WEP assignment, gender only impacts the likelihood of exiting welfare.

## VII. Robustness check

To confirm the robustness of the findings, the effect of the WEP is estimated using two additional comparison groups. First, one might be concerned that the original control group is not comparable to the treatment group since changes in WEP status that occurred between the end-of-month snapshots were not observed. In particular, recipients could have been treated or become ineligible prior to being included in the initial control groups. To address this concern, an additional control group is formed which only includes recipients whose WEP status did not change in the month prior to their inclusion in the control group. Specifically, all member of the initial control group whose WEP status was different the month prior to their inclusion than in the month of their inclusion were removed. The new control group is denoted “robust control group I.” Additionally, restricted robust control group I one and three are formed as described in section IV.

Second, one might be concerned that including covariates in the analysis does not adequately control for the selection process, which was based on observable characteristics. To address this concern, an additional control group is formed which only includes recipients who were ultimately treated. Specifically, all members of the initial control group who were not treated by the end of the study period (February 1996) were removed. The new control group is denoted “robust control group II.” Additionally, restricted robust control group II one and three are formed as described in section IV.

#### A. The PID using the robust control groups

Equation (4) is estimated using the robust control groups. Using robust control group I three, the coefficients on the treatment dummy are positive and significantly different than zero for values of  $M$  between two and five. The coefficients peak at 0.114 ( $t = 6.99$ ,  $p = 0$ ) when  $M$  equals two, indicating that treated recipients are 11.4 percentage points more likely to have exited welfare than are members of this control group (see Figure 8). The peak PID is over three percentage points higher when using robust control group I three than when using the initial control group three. This is not surprising since some members of the initial control group were presumably treated between the end-of-month snapshots, but not observed to be. Robust control group I excludes most of these recipients. So the difference in the PIDs should place an upper bound on the negative bias introduced by the absence of intra-month WEP status observations.

Using robust control group II three, the coefficients on the treatment dummy are positive and significantly different than zero for all values of  $M$ . The coefficients peak at 0.246 ( $t = 7.75$ ,  $p = 0$ ) when  $M$  equals five, indicating that treated recipients are 24.6

percentage points more likely to have exited welfare than are members of this control group (see Figure 9). The peak PID is over twice as large when using robust control group II three as when using either the initial control group three or robust control group I three. This indicates that initial control group three members, and robust control group I three members, were more likely to have exited welfare than were robust control group II three members. This is not surprising since one had to be on welfare in a given month to be eligible for treatment in that month. Thus, any recipient who exited and remained off welfare for the study period was excluded from this control group. Thus, this estimate of the PID is presumably positively biased.

#### B. Comparability of the treatment and robust control groups

Comparing the demographic characteristics of members of the treatment and robust control groups, one observes that the distribution of borough, gender, and race are similar (see columns (1), (3), and (4) of Table 1). The difference in average age is in the same direction as before, recipients who were treated are younger, on average, than are recipients who were not. The magnitude of this difference is larger using robust control group I and smaller using robust control group II.

#### C. Likelihood of receiving a WEP assignment

Equation (4) is estimated using the robust control groups with  $y_i^M$  redefined as an indicator function that equals one if recipient  $i$  received a WEP assignment within  $M$  months of his or her inclusion in the treatment or control groups, and zero otherwise. The coefficients on the treatment dummy are positive and significantly different than zero for all values of  $M$  and using either robust control group. Using robust control group I three, the coefficients peak at 0.227 ( $t = 12.94$ ,  $p = 0$ ), when  $M$  equals four, indicating that

treated recipients are 22.7 percentage points more likely to have received a WEP assignment than are members of this control group. Using robust control group II three, the coefficients peak at 0.292 ( $t = 19.82$ ,  $p = 0$ ), when  $M$  equals four, indicating that treated recipients are 29.2 percentage points more likely to have received a WEP assignment than are members of this control group.

In summary, the PID appears to be robust to the choice of comparison group. Whether one uses the initial control groups or the robust control groups, the treatment had an abrupt, significant effect on the probability of exiting welfare.

## VIII. Discussion

Enrollment in the WEP appears to have had a substantial and persistent impact on welfare recipients who had previously been unexposed to welfare reform, increasing the probability that they exited welfare and received a WEP assignment.

### A. The magnitude of the PID

As was previously discussed, there were two data collection issues that introduced a negative bias to the estimate of the PID. The first, that a recipient's end-of-month case status was not collected if he or she received a WEP assignment, necessitated the conservative assumption that all recipients who received a WEP assignment remained on welfare thereafter. To obtain a rough estimate of the magnitude of the bias introduced by this assumption, assume that the probability of exiting welfare is not conditional on receiving a WEP assignment. This implies that over 50% of the recipients who received WEP assignments exited welfare seven months post enrollment<sup>26</sup>. Given such an assumption, the PID is negatively biased by approximately 10 percentage points, since

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<sup>26</sup> 57.2% of the recipients who did not receive a WEP assignment exited welfare seven months post enrollment.

treated recipients were approximately 20 percentage points more likely to have received a WEP assignment than were members of the control group. Thus, the true PID is plausibly twice as large as the one reported in the paper.

The second, that WEP status was only observable at the end of each month, meant that the treatment of some recipients was not observed. Recall that an upper bound for this bias is approximately three percentage points and was computed by comparing the estimate of the PID using the initial control group and using the robust control group I. Finally, recall that control group contamination, which is not fully controlled for, could be yet another source of negative bias. Thus, it seems plausible that the true PID is over 20 percentage points.

#### B. Why did being selected for the WEP have such a significant and rapid impact

Perhaps, the most intriguing topic for additional research is to investigate why the WEP had such a swift and significant impact on the probability of exiting welfare. One potential explanation is that participating in a WEP assignment helped participants develop the skills that were 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 supervisor. This explanation, however, is not plausible since the treatment was being scheduled for a WEP intake interview, and not participating in a WEP assignment<sup>27</sup>.

Another possible explanation for the effectiveness of the WEP is that it substantially increased the cost, or disutility, of being on welfare. Specifically, the introduction of the WEP increased the number of hours per week, from zero to 21, which recipients were required to spend in structured activities. This change may have reduced

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<sup>27</sup> Furthermore, recall that all participants who actually received a WEP assignment were presumed not to have exited welfare. Thus, the effect of participating in a WEP assignment on exiting welfare was not measured in this study.

the net utility of being on welfare enough that some recipients exited welfare. This explanation implies that NYC could have engaged the recipients in any structured activity for 21 hours per week and the effect would have been the same. This explanation is also not plausible, since the PID was generated by scheduling recipients for WEP intake interviews, and not generated by participating in WEP assignments.

A twist on this explanation is that it was the expected cost of participating in the WEP that caused the large observed PID. For this explanation to be correct, untreated member of the study population had to have not exited welfare in anticipation of their manifest treatment<sup>28</sup>. Otherwise, the PID would be small and insignificant.

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

Unfortunately, given the limited data that is available there is no way to determine which of the prior two explanations is more plausible. What is clear, though, is that simply enrolling recipients in WEP, a welfare reform program, had a significant impact on recipients who had been previously unexposed to welfare reform programs. Thus, other research that seeks to evaluate the effect of welfare reform programs may overstate the impact of the evaluated program if the recipients in the study had largely been unexposed to welfare reform.

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<sup>28</sup> Recipients were only officially notified of their impending enrollment in the WEP after they were selected for a WEP intake interview. On the other hand, recipients may have learned of the WEP from local media coverage.

### C. Generalizing the finding

A strength of this study is that all enrollees were considered treated regardless of whether, or not, they actually participated in the WEP. Thus, the PID estimates the “Intent to Treat” effect (not the “Treatment on the Treated” effect), and does not suffer from self-selection bias<sup>29</sup>. 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 program’s design and the identification strategy, and may affect the conclusions one can draw in the following two ways. First, the PID may be biased for welfare recipients who are not able-bodied. This is probably not a major concern since mandatory workfare for such recipients is, presumably, not appropriate. Second, the observed effects may be biased for FA recipients. Clearly, this is of concern since the majority of welfare recipients nationwide are FA recipients and not GA recipients. The most important, and fundamental, way in which FA and GA recipients differ is that all recipients of FA have at least one dependent child, while recipients of GA do not. Specifically, the effect of the WEP may interact with the effect of having dependent children. This interaction could theoretically bias the PID in either direction. While having to arrange childcare<sup>30</sup> may introduce an additional barrier to participating in the WEP, or to exiting welfare, being a parent may increase one’s motivation to participate in the WEP, or exit welfare and join the labor force. Finally, in previous research, Ifcher (2006) found that a different component of NYC’s welfare reform, job training, had a very similar effect on FA and GA recipients.

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<sup>29</sup> See Katz et al (2001) for a comparison of these effects.

<sup>30</sup> When FA recipients were enrolled in the WEP, NYC paid for childcare.

#### D. Cost Benefit Analysis

Finally, one should consider whether the fiscal benefits of the WEP outweighed the costs. Parks, the agency with the largest number of WEP participants, received approximately four million dollars per year to manage and supervise WEP participants; this included funds to orient, train, equip, transport, and provide on-site supervision to WEP participants. During this period, Parks received approximately 26,000 WEP assignees per year. Thus, Parks' average cost per assignee was approximately \$150.

In addition, there were administrative costs, not borne by Parks. For example, WEP intake interviews had to be scheduled and conducted. These expenses were not tracked, and thus, the costs are not known. To be conservative, it is assumed that the total cost to treat a recipient was twice Parks' cost per assignee, or \$300<sup>31</sup>.

The benefit per treated recipient is harder to calculate since it is dependent on exit rates and the length of time that recipients remained off welfare. In early 1995, a typical HR recipient received \$350 per month in GA and \$100 per month in food stamps. This equals \$5,400 per year<sup>32</sup>. Additionally, one must add administrative costs, income taxes paid by former recipients, and Medicaid costs (each HR recipient received Medicaid), to the benefit calculation. This presumably adds at least \$600 per year. Thus, an estimate of the fiscal benefit of exiting welfare is \$6,000 per recipient per year.

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<sup>31</sup> This is conservative for two reasons. First, the administrative costs were almost certainly less than were Parks' cost. Second, less than half of all treated recipients received a WEP assignment, however, this estimate assumes that each treated recipient received a WEP assignment.

<sup>32</sup> Note that the gross fiscal benefit of a FA recipient exiting welfare was approximately \$10,800 per year, making the WEP potentially more cost effective for FA recipients than for GA recipients.

A conservative estimate of the PID would seem to be 10 percentage points. Thus, the expected fiscal benefit per treated recipient is \$600 per year<sup>33</sup>. Given that the cost to treat a recipient was approximately \$300, the WEP pays for itself if treated recipients, who exited welfare, remained off welfare six months. As of the end of the study, the subset of 124 treated recipients, who exited welfare and had eleven months of follow-up data, had been off welfare, on average, seven and a half months<sup>34</sup>. Thus, the WEP appears to pay for itself<sup>35</sup>.

Finally, this fiscal cost benefit analysis ignores the welfare effects of the WEP. To fully understand and evaluate any welfare reform, one should determine whether the recipients' well being was improved or impaired. Unfortunately, when WEP was implemented, NYC did not track recipients' progress after their cases were closed. This shortcoming is not unique to this research. The welfare effects of welfare reform are not, as of yet, well understood (Blank, 2002).

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<sup>33</sup> This fiscal cost benefit analysis also ignores another benefit for the city, 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 approximately \$750 per assignee per year.

<sup>34</sup> Of these, 86% were still off welfare when the study ended.

<sup>35</sup> Other studies have also suffered from limited post enrollment data as well. These studies have generally projected the future benefits of the training program (Friedlander et al, 1997).

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Figure 1

The Unemployment Rate and Welfare Caseload in NYC, January 1978 – June 2004

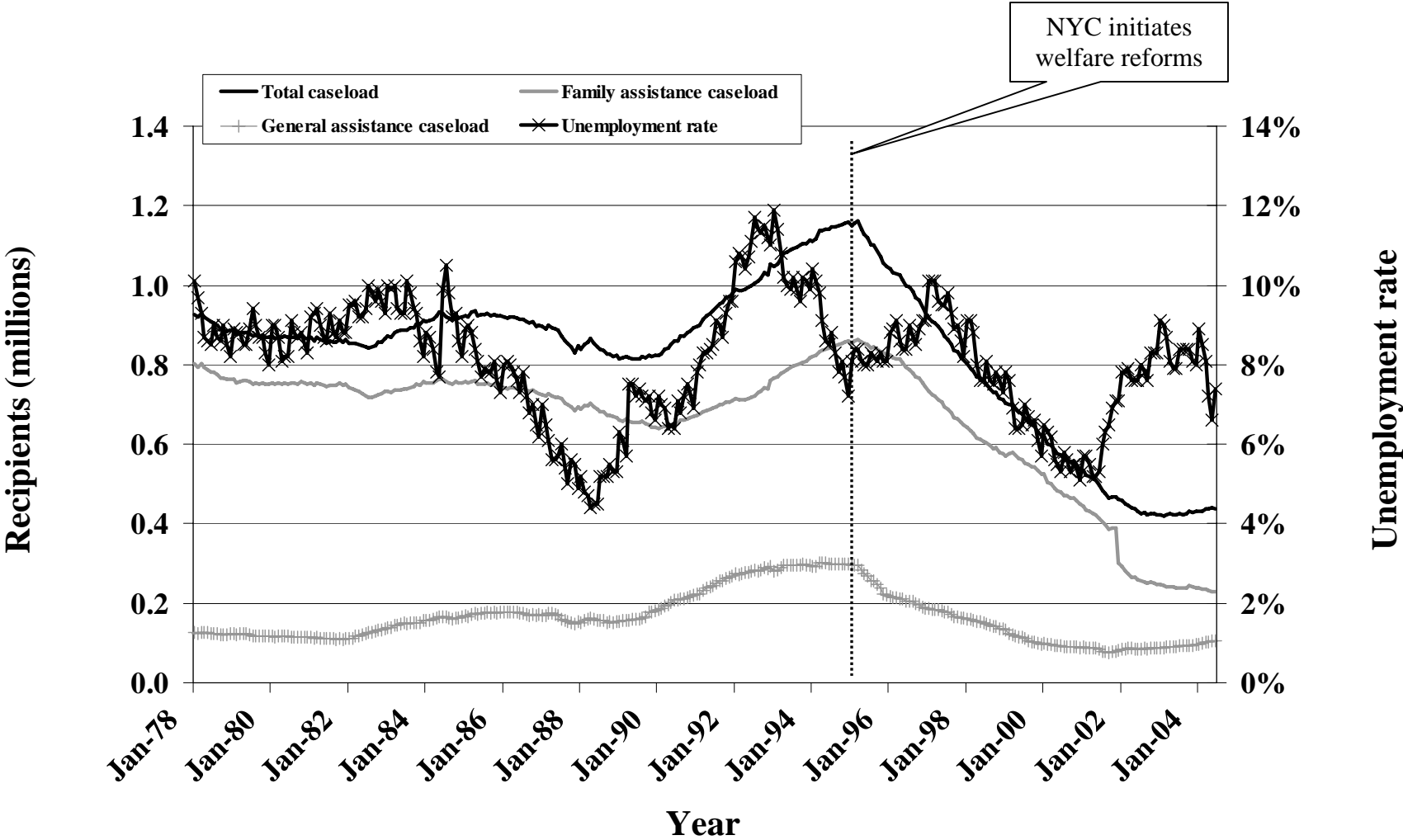


Figure 2

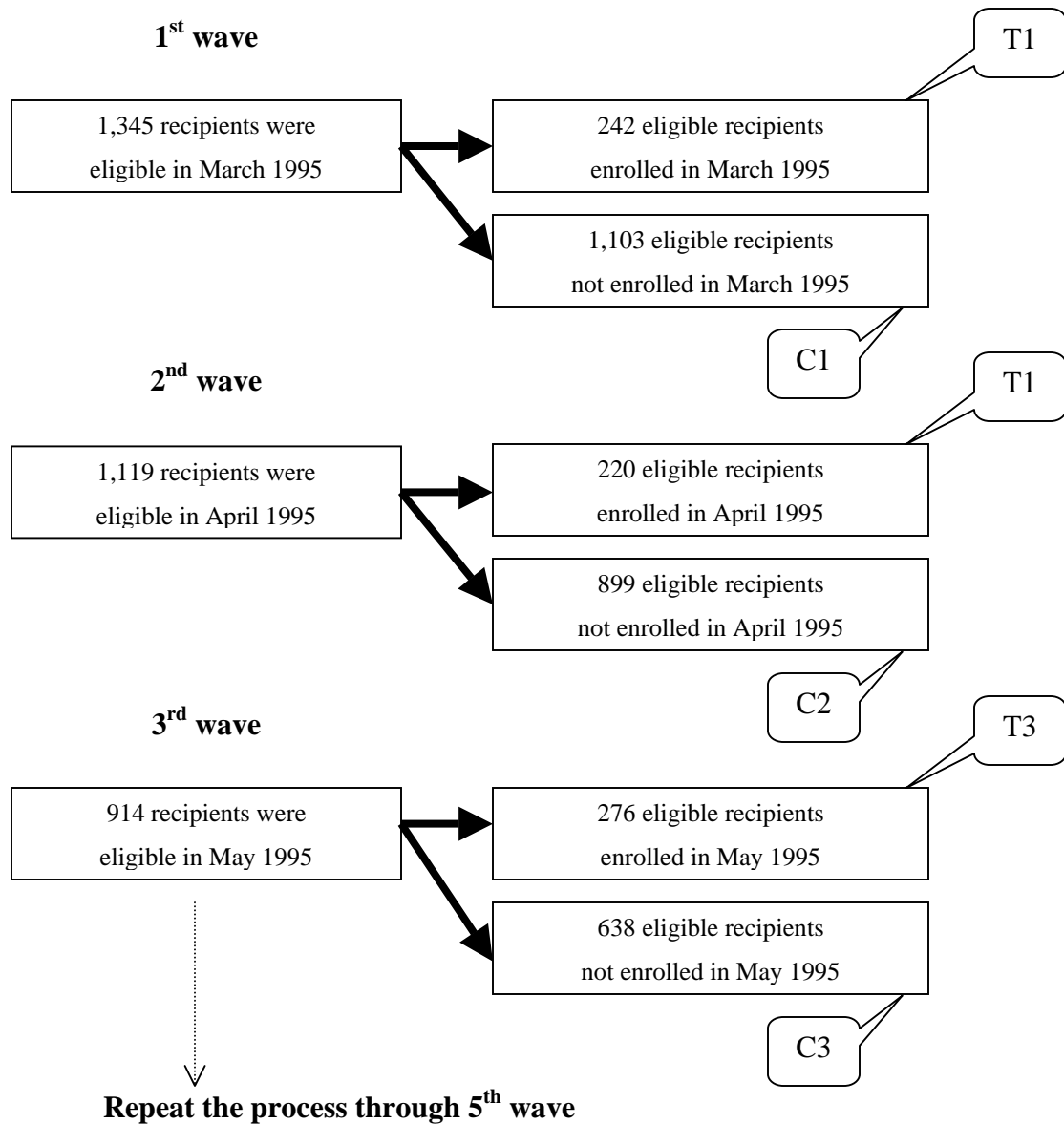
Formation of the Treatment Group and the Control Group

**The Treatment Group:**

All recipients enrolled in the WEP 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 to be enrolled, but were not, between March and July 1995. Specifically it is the union of C1, C2, C3, C4, and C5<sup>1</sup>.



<sup>1</sup> Note that many recipients were eligible but not enrolled, but were not, in multiple waves. Consequently, some recipients are members of the control group multiple times.

Figure 3

The Formation of the Treatment Group and the Control Group with a One-Month Restriction on Being Treated

**The Treatment Group:**

All recipients enrolled in the WEP between March and July 1995. Specifically it is the union of T1, T2, T3, T4, and T5.

**Control Group One:**

All recipients who were

1. eligible but not enrolled during any of the first five waves and
2. not treated in the subsequent wave.

Specifically it is a weighted union of C1a, C1b, C2a, C2b, C3a, C3b, ....., C17a, C17b. The weight placed on each member of each a-series cohort is equal to one. The weight placed on each member of each b-series cohort is equal to the reciprocal of the probability of not being enrolled in the subsequent month, conditional on be eligible to be enrolled in that month, e.g., for members of C1b the weight is the reciprocal of .787 (643/817), or 1.271.

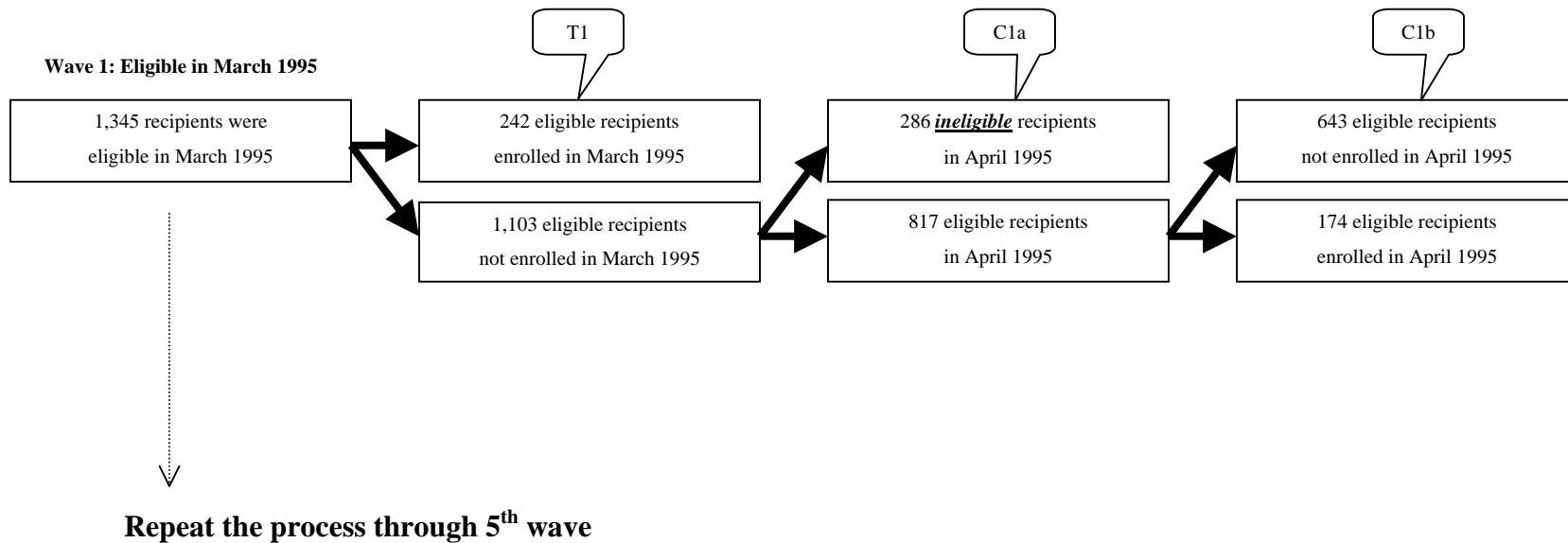


Figure 4

The PID

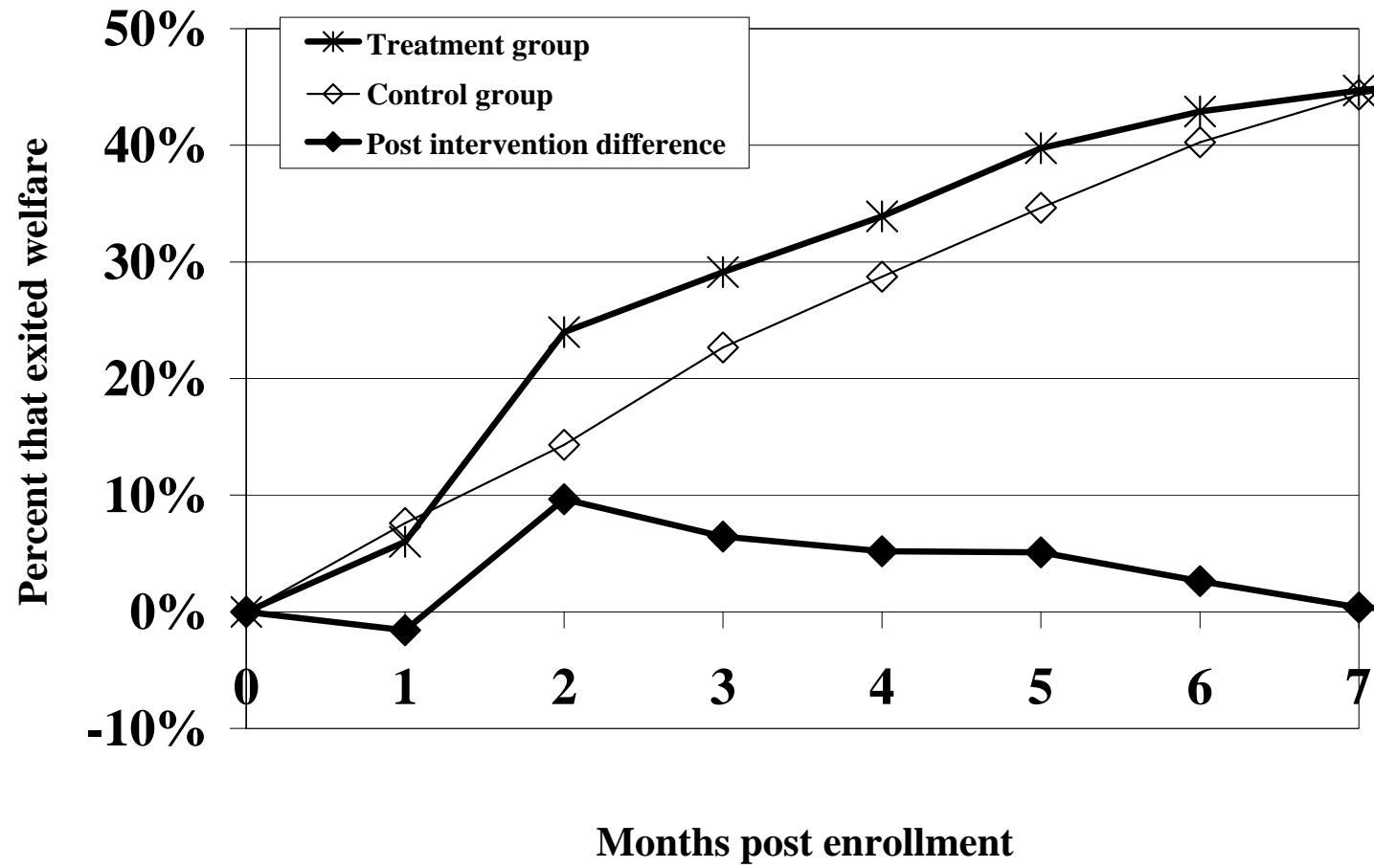


Figure 5

The PID Using the Initial Control Group, and Control Groups One and Three

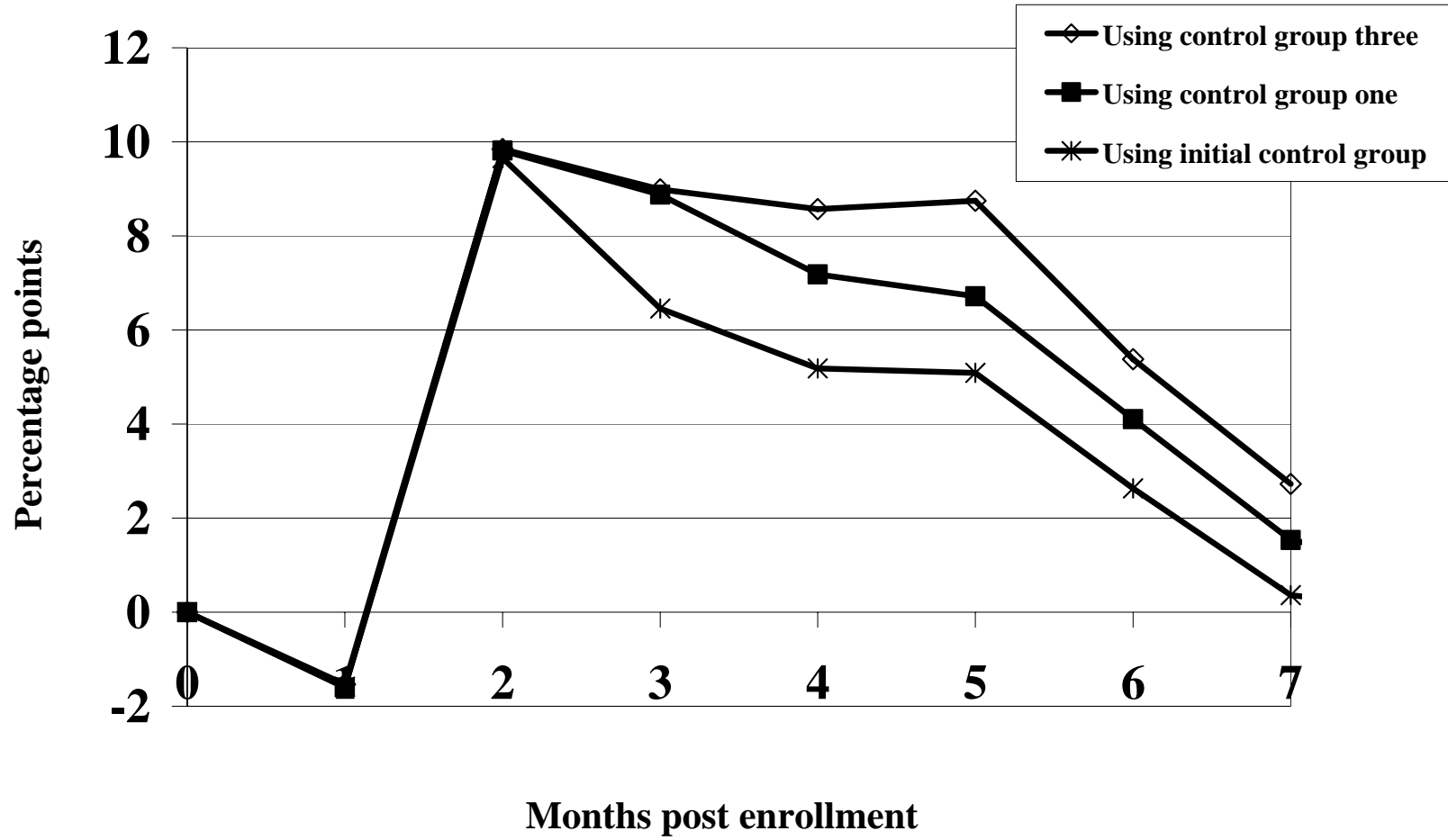


Figure 6

The PID with Various Covariates Included Using the Control Group Three

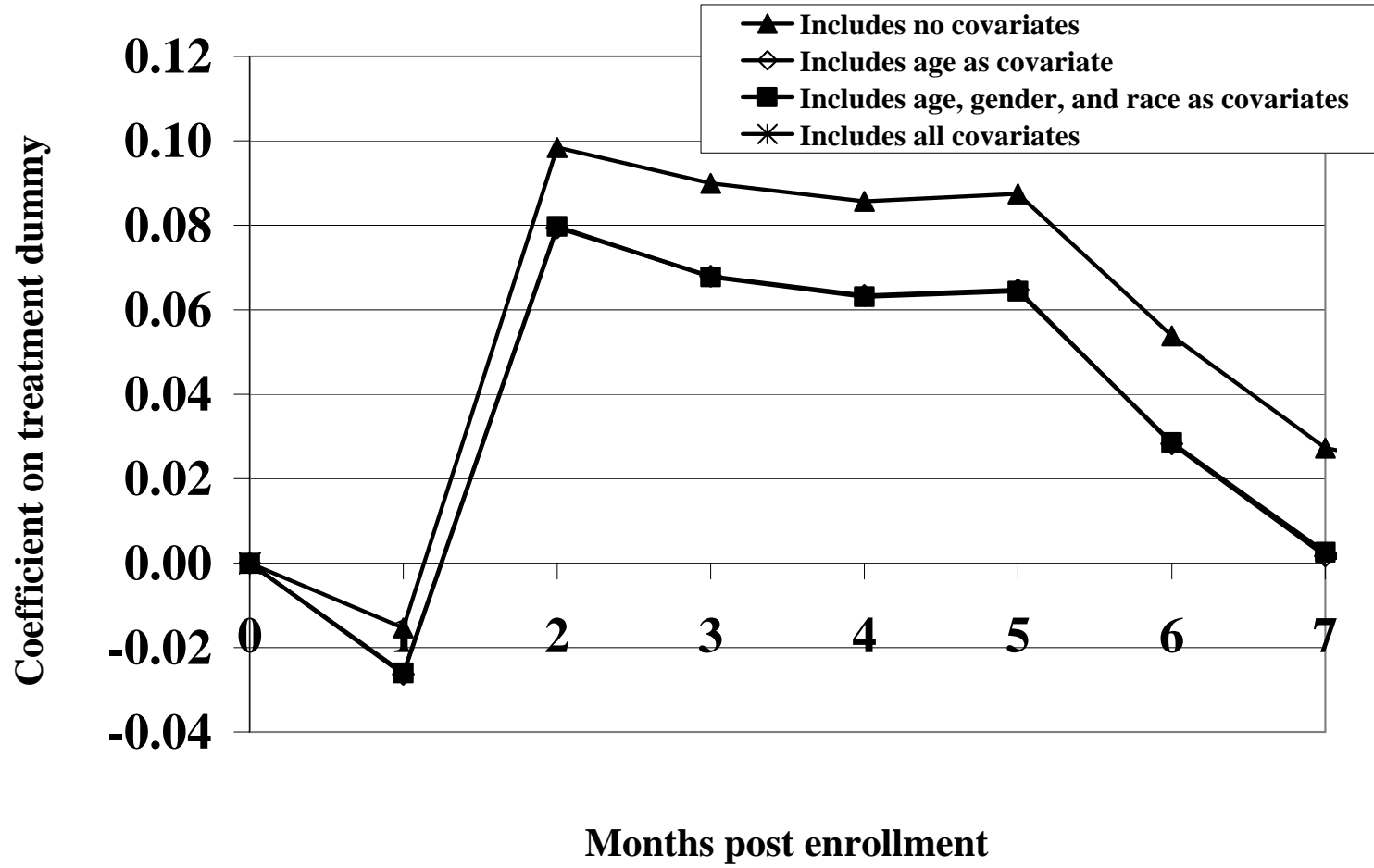


Figure 7

The PID with Receiving a WEP Assignment as the Dependent Variable

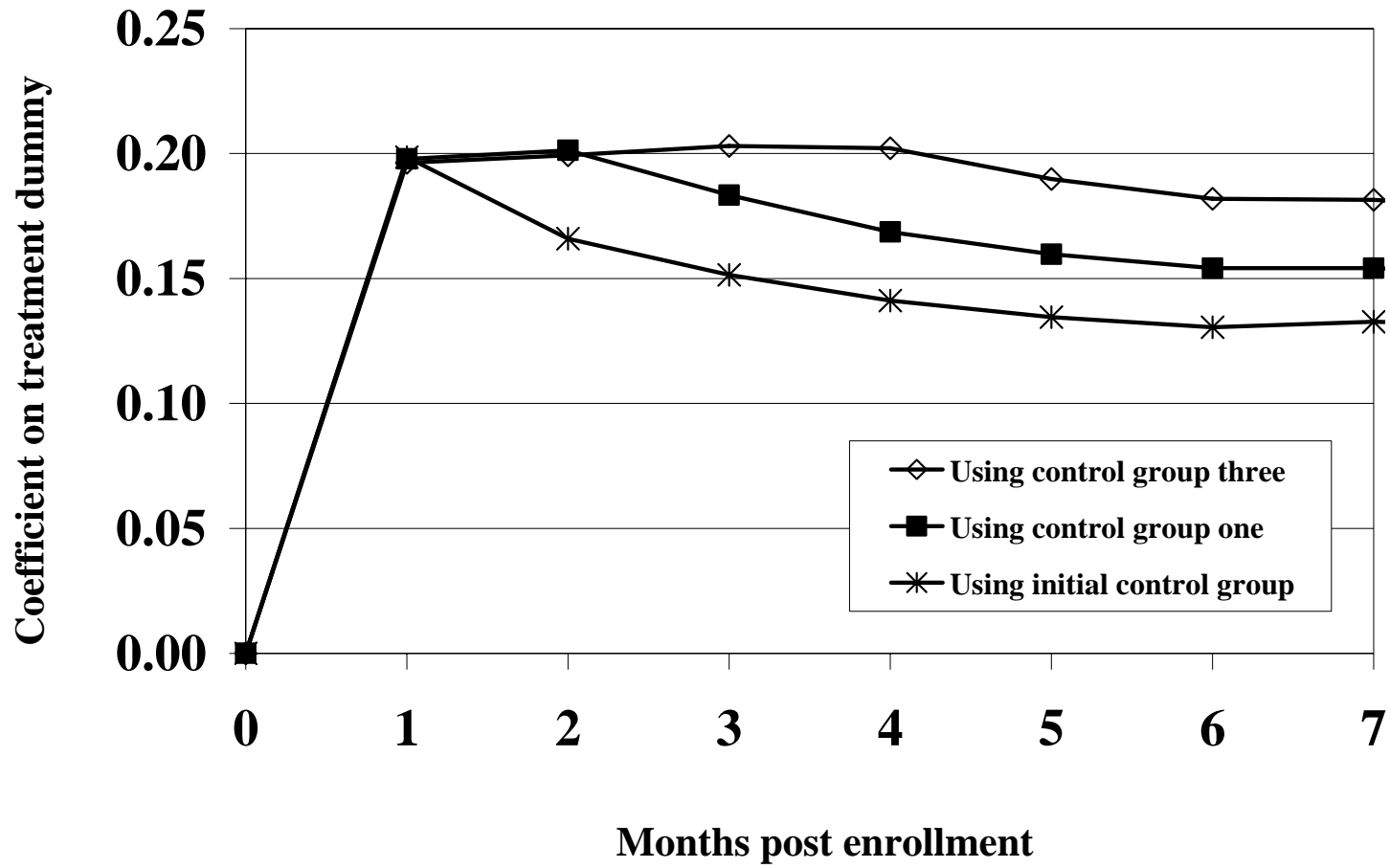


Figure 8

The PID Using the Robust Control Group I

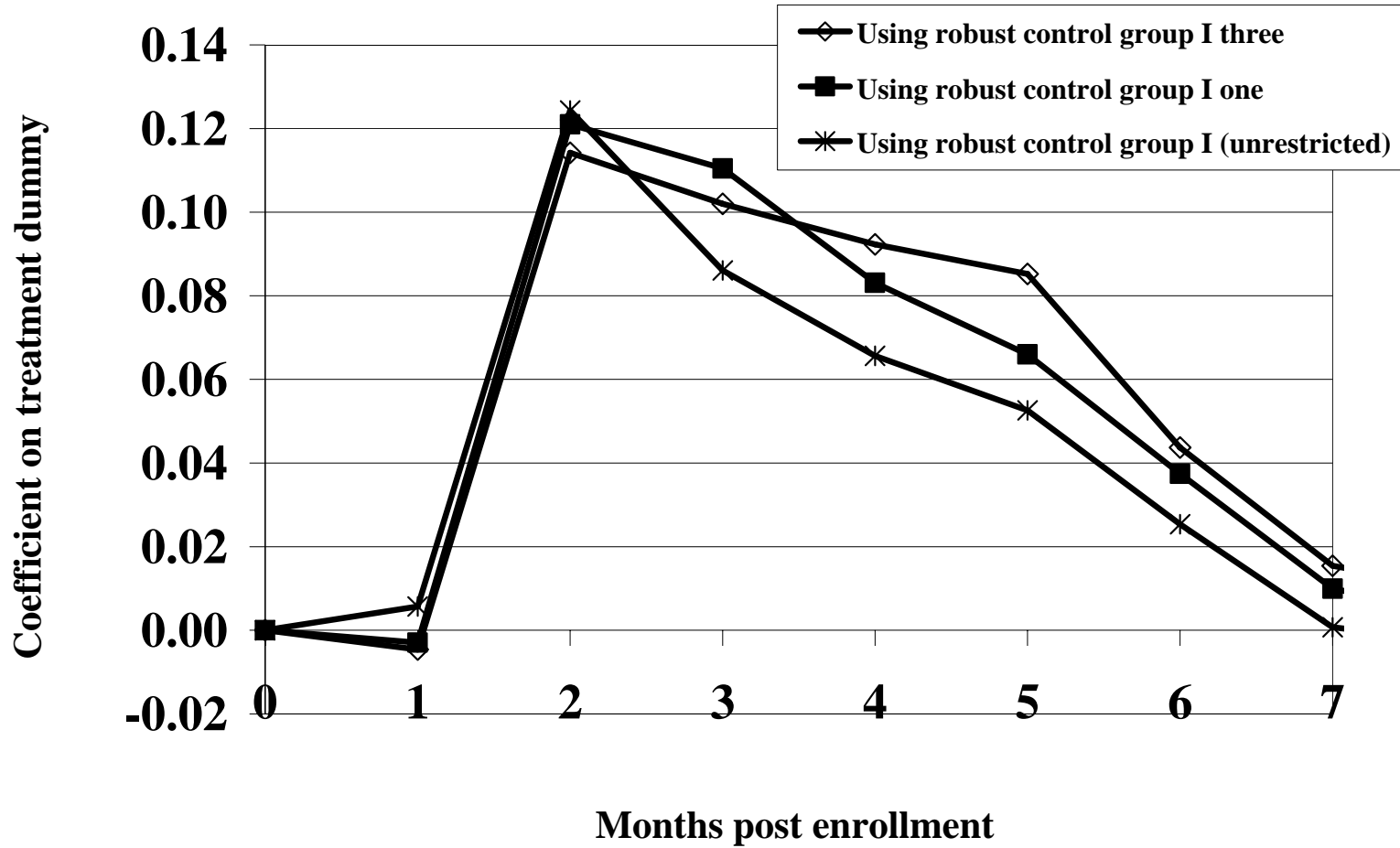


Figure 9

The PID using Robust Control Group II

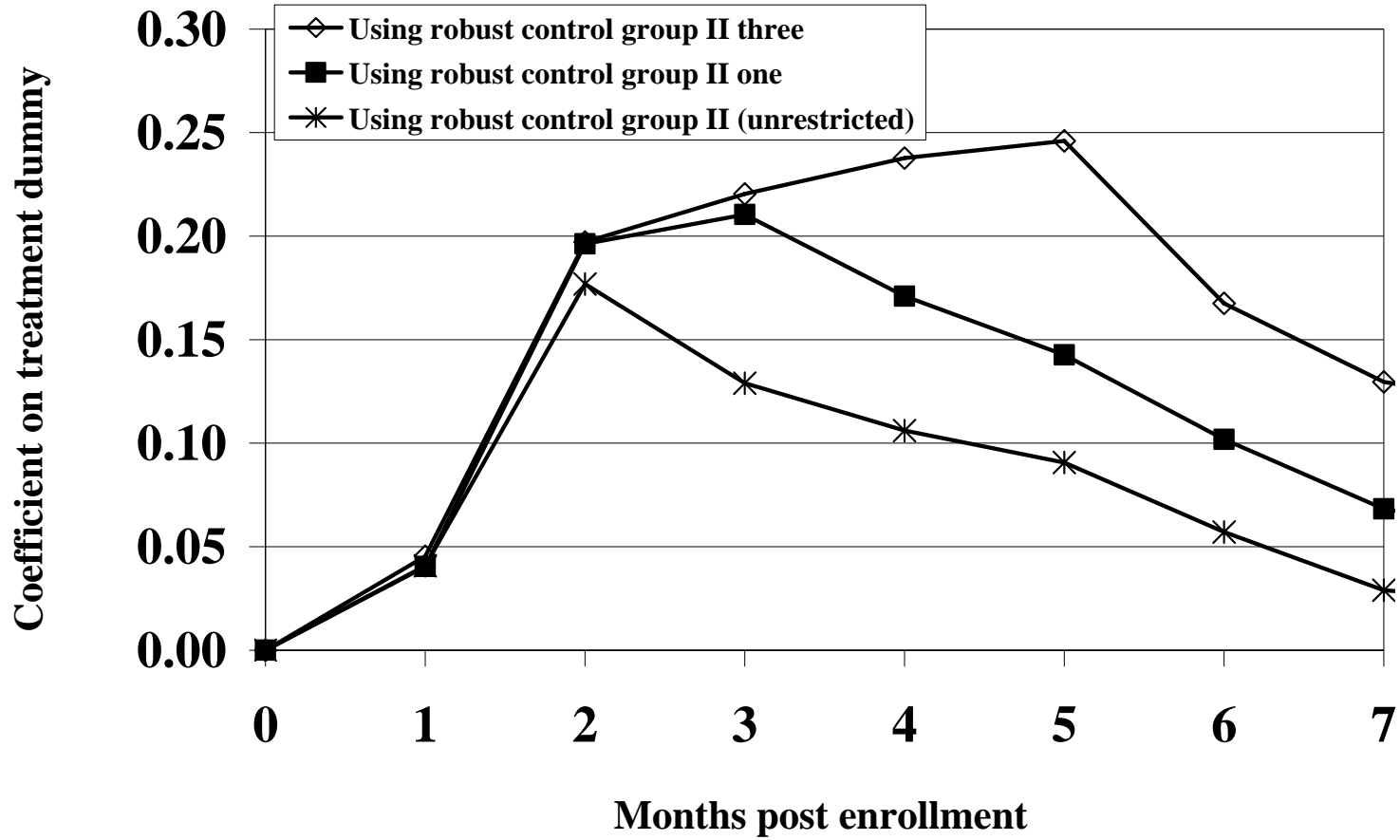


Table 1: Demographic Characteristics of Members of the Treatment and Original and Robust Control Groups

<b>Demographic characteristic</b>	<b>Treatment group (1)</b>	<b>Initial control group (2)</b>	<b>Robust control group I (3)</b>	<b>Robust control group II (4)</b>
<b>Observations</b>	<b>1,047</b>	<b>3,868</b>	<b>2,806</b>	<b>1,697</b>
<b>Male</b>	<b>59.0%</b>	<b>57.4%</b>	<b>58.0%</b>	<b>57.3%</b>
<b>Race</b>				
Black	<b>42.0%</b>	<b>44.1%</b>	<b>43.8%</b>	<b>43.8%</b>
Hispanic	<b>25.8%</b>	<b>24.2%</b>	<b>23.7%</b>	<b>24.2%</b>
White	<b>16.6%</b>	<b>15.2%</b>	<b>15.0%</b>	<b>15.1%</b>
Other	<b>1.1%</b>	<b>1.0%</b>	<b>1.0%</b>	<b>0.6%</b>
Not reported	<b>14.4%</b>	<b>15.5%</b>	<b>16.5%</b>	<b>16.3%</b>
<b>Borough of residence</b>				
Bronx	<b>13.9%</b>	<b>17.5%</b>	<b>17.1%</b>	<b>16.3%</b>
Brooklyn	<b>37.5%</b>	<b>35.6%</b>	<b>34.8%</b>	<b>32.8%</b>
Manhattan	<b>35.5%</b>	<b>34.0%</b>	<b>35.6%</b>	<b>37.1%</b>
Queens	<b>11.2%</b>	<b>11.6%</b>	<b>11.2%</b>	<b>11.8%</b>
Staten Island	<b>1.8%</b>	<b>1.3%</b>	<b>1.2%</b>	<b>2.0%</b>
<b>Average age</b>	<b>36.7</b>	<b>39.4</b>	<b>40.3</b>	<b>38.0</b>
	<b>(11.2)</b>	<b>(11.69)</b>	<b>(11.54)</b>	<b>(10.89)</b>

Table 2: Coefficients on Demographic Characteristics from Estimating Equation (2)

<b>Demographic Characteristic</b>	<b>March 1995</b>	<b>April 1995</b>	<b>May 1995</b>	<b>June 1995</b>	<b>July 1995</b>
<b>Male</b>	<b>0.0117</b>	<b>-0.0104</b>	<b>0.1887 **</b>	<b>-0.0251</b>	<b>-0.0636</b>
<b>Race</b>					
Black	<b>0.0053</b>	<b>-0.0702</b>	<b>-0.0708</b>	<b>-0.2364</b>	<b>-0.0156</b>
Hispanic	<b>0.1414</b>	<b>0.0309</b>	<b>-0.1070</b>	<b>0.0233</b>	<b>0.1716</b>
White	<b>0.2471</b>	<b>-0.0795</b>	<b>0.1255</b>	<b>-0.0370</b>	<b>-0.0229</b>
<b>Borough of residence</b>					
Bronx	<b>-0.1618</b>	<b>-0.1959</b>	<b>-0.1742</b>	<b>-0.6640</b>	<b>-0.3505</b>
Brooklyn	<b>0.0304</b>	<b>0.0397</b>	<b>-0.1549</b>	<b>-0.3360</b>	<b>-0.1480</b>
Manhattan	<b>-0.0282</b>	<b>0.0260</b>	<b>0.1220</b>	<b>-0.2808</b>	<b>-0.5649</b>
Queens	<b>-0.0136</b>	<b>-0.0659</b>	<b>-0.1432</b>	<b>-0.3288</b>	<b>-0.3647</b>
<b>Average age</b>	<b>-0.0198 ***</b>	<b>-0.0082 **</b>	<b>-0.0083 **</b>	<b>-0.0188 ***</b>	<b>-0.0099 **</b>

Table 3: Coefficients from Estimating Equations (3) and (4) using Control Group Three

Demographic characteristic	Two months post enrollment (peak effect)				Seven months post enrollment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<b>Treatment dummy</b>	0.098 ***	0.079 ***	0.080 ***	0.082 ***	0.027	0.002	0.003	0.003
<b>Age</b>		-0.005 ***	-0.005 ***	-0.004 ***		-0.006 ***	-0.006 ***	-0.006 ***
<b>Race</b>								
Black			0.018	0.014			0.041	0.038
Hispanic			0.002	0.002			0.011	0.018
White			-0.021	-0.028			-0.044	-0.048
<b>Male</b>			0.065 ***	0.062 ***			0.131 ***	0.125 ***
<b>Borough</b>								
Bronx				-0.110				0.052
Brooklyn				-0.101				0.078
Manhattan				-0.049				0.118
Queens				-0.123				0.058
<b>Enrollment period dummies</b>	NO	NO	NO	YES	NO	NO	NO	YES
<b>Interaction dummies<sup>^</sup></b>	NO	NO	NO	YES	NO	NO	NO	YES

<sup>^</sup> Between borough and enrollment date

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