

Identifying the Effect of a Welfare-To-Work Program Using Program Capacity Constraints:

A New York City Quasi-Experiment

by

John Ifcher<sup>1,2</sup>

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<sup>1</sup> John Ifcher, Santa Clara University, Department of Economics, 500 El Camino Real, Kenna 207, Santa Clara, CA, 95053, 408-554-5579 (phone), 408-554-2331 (fax), [jifcher@scu.edu](mailto:jifcher@scu.edu)

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## Abstract

In 1999, welfare recipients were required to participate in a job training and outplacement assistance program. Initially, recipients were enrolled in 'waves' due to program capacity constraints. I identify the effect of the program using an innovative quasi-experiment in which enrollees are compared to concomitantly eligible, non-enrolled recipients. I find that enrollees were thirteen percentage points more likely to have started a job. The quasi-experimental methodology developed herein is important since randomized experiments can be costly, difficult to implement, and/or unavailable. Moreover, experiments are not impervious to criticism and this procedure addresses three of the five known shortcomings.

## I. Introduction

Over the past two decades U.S. welfare programs have been transformed. A central objective of the reforms has been to help recipients move from welfare to work. In response, a variety of welfare-to-work programs have been implemented. These programs typically include one or more of the following components: unpaid work experience, classroom instruction, on-the-job training, financial incentives, and job search assistance.

A substantial literature has developed to identify the resulting changes in welfare use, employment, well-being, and family structure (recent reviews include Blank, 2002; Grogger and Karoly, 2005; and Moffitt, 2003). The results largely support the following two conclusions. First, in the short run, at least, welfare-to-work programs had the intended effect, reducing welfare use and increasing employment. Second, the most effective welfare-to-work programs are those that include mandatory work requirements with an emphasis on job placement (Grogger and Karoly, 2005)<sup>1</sup>.

These findings were largely derived from well-designed randomized experiments<sup>2</sup>. While such experiments are certainly an excellent method for identifying the effect of a pilot welfare-to-work program, they are not impervious to criticism. The following five shortcomings have been identified and were summarized in Grogger and Karoly (2005). First, since experimental programs are often implemented by above-

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<sup>1</sup> A recent study by Hotz et al (2006), however, has challenged the latter conclusion. The authors found the effect of a welfare-to-work program that emphasized human capital development was larger, but emerged later, than the effect of a program that emphasized labor force attachment.

<sup>2</sup> Prior to the passage of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), many states received a 'waiver,' permission from the federal government, to implement pilot welfare-to-work programs for a portion of their Aid to Families with Dependent Children (AFDC) caseload. As a condition of receiving such a waiver, the state had to evaluate the pilot program using a randomized experiment.

average managers, it is unclear whether successful pilot programs can be expanded without losses in effectiveness. Second, experiments miss some general equilibrium effects. For example, whereas full-scale implementation of a welfare-to-work program might crowd-out other job seekers or even subsequent program participants, a pilot program might not. Third, the ‘message’ of the pilot program may cross-over from the program to the control group. Fourth, experiments that only include current welfare recipients do not capture entry effects. That is, a welfare-to-work program may change the attractiveness of receiving welfare and therefore not only effect the exit rate, but also the entry rate. Finally, random experiments can be costly and hard to implement.

The first four shortcomings are noteworthy because each could cause the experimental estimates to be biased. For example, the first presumably introduces a positive bias and the third a negative bias. Thus, available estimates, which are largely based on experimental evidence, are potentially biased with the direction and magnitude of the overall bias unknown. Moreover, no new randomized experiments have been conducted since the passage of PRWORA, rendering this identification strategy obsolete.

Non-experimental methods have also not been successfully employed to identify the effect of welfare-to-work programs<sup>3</sup>. This is largely because welfare-to-work programs vary in numerous respects and along many dimensions, making it difficult to parameterize the programs in a manner that is useful for identification purposes.

Moreover, post-PRWORA there was little time variance in reform implementation dates

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<sup>3</sup> This is generally true for other components of welfare reform as well. One exception is time limits. Using non-experimental methods, Grogger (2003) has shown that they decreased welfare use. Another successful quasi-experimental study is Autor and Houseman (2005). They demonstrate that temporary help jobs do not change the likelihood that the recipients are employed one to two years post placement. To identify this effect, they use the random assignment of welfare recipients to various job training contractors.

across states, again making identification difficult. Thus, developing new identification strategies could be helpful.

In this paper, I demonstrate a quasi-experimental identification strategy to estimate the effect of a welfare-to-work program. 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 participate concurrently due to capacity constraints. Rather, recipients were selected for the program in 'waves.' The wave enrollment process creates the opportunity to identify the effect of the program by comparing 'selectees,' recipients who were selected on a given date to 'non-selectees,' recipients who were eligible, but not selected, on that date. The results indicate that the program doubled the likelihood that a recipient started a job from approximately 10 percent to over 20 percent.

This quasi-experiment is similar to a randomized experiment in that it estimates the effect of a single welfare-to-work program. It, however, ameliorates some of the shortcomings discussed above. First, the program was not a small-scale, pilot program implemented by above-average managers. Rather, it was the principal welfare-to-work program for all welfare recipients in New York City (NYC). Second, this analysis should capture general equilibrium effects in the labor market given the size of the program, there were over ten thousand selectees in the first year of the program. Furthermore, over 100,000 NYC welfare recipients started a job in 2000 and 2001, the first two years of the

program. Finally, this program was not a costly, or difficult to implement, randomized experiment. So this approach addresses three out of five shortcomings<sup>4</sup>.

The next section of this paper presents a short description of welfare reform in NYC. The third section describes the quasi-experimental identification strategy. The fourth section discusses the empirical implementation. The fifth section presents the results. The sixth section describes a robustness check that was performed. The final section discusses the implications of the results and presents a brief cost benefit analysis.

## II. Welfare reform in NYC

In 1994, Rudolph Giuliani, newly elected mayor of NYC, made reforming the city's welfare programs a priority. At the time, NYC had over one million welfare recipients, including almost 300,000 General Assistance (GA) recipients<sup>5</sup>. The city was spending approximately three billion dollars annually on welfare programs, including one billion dollars on GA.

In 1995, NYC initiated the Work, Accountability, and You (NYCWAY) program. A central tenet of NYCWAY was that able-bodied welfare recipients were required to work in exchange for their benefits<sup>6</sup>. Almost all recipients fulfilled this requirement by participating in a Work Experience Program (WEP) assignment 21 hours per week<sup>7</sup>. The majority of WEP participants worked outdoors removing litter, weeds, and graffiti from parks, vacant lots, streets, and highways. Over the next six years, the number of welfare

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<sup>4</sup> It does not enable the measurement of entry effects and it also suffers from control group contamination. The latter is discussed at length in the third section of the paper.

<sup>5</sup> GA is cash assistance for financially needy individuals who are not covered by federally funded income maintenance programs.

<sup>6</sup> Other components of NYCWAY included enhanced detection of welfare fraud, increased substance abuse treatment, and diversion to federally funded disability insurance where appropriate.

<sup>7</sup> Participants received no compensation other than their welfare benefits and a nominal stipend for carfare and lunch.

recipients in NYC declined by more than 50% and the number of GA recipients declined by 70%.

In 1999, NYC created the Employment Services and Placement Program (ESPP), a job training and outplacement assistance program. With the implementation of the ESPP, welfare recipients were required to participate in a WEP assignment three days a week and in the ESPP two days a week. This increased, from 21 to 35, the number of hours per week that recipients were required to spend in structured activities.

Eleven private contractors were hired to provide ESPP services. All had a history of providing social services in NYC. The contractors were paid on a performance basis. They received a substantial payment for each recipient they placed in a job; the average payment was approximately \$3,000. The contractors focused on developing participants' 'soft skills' including résumé writing and interview techniques, and helped participants arrange job interviews. ESPP contractors were required to attempt to place each participant for at least six weeks.

Prior to implementing the ESPP, there were approximately 30,000 'pre-existing,' eligible welfare recipients. Post implementation, all of these pre-existing eligible recipients were required to participate in the ESPP if they were a WEP participant and deemed 'job ready'<sup>8</sup>. Since not all pre-existing recipients could be accommodated simultaneously, recipients were enrolled in waves. Selectees were informed by mail of their status, instructed to report to the proper location at a prescribed date and time (the program start date was typically two weeks after the selection date), and advised that they would be sanctioned if they failed to comply with the program's requirements. New

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<sup>8</sup> Private contractors were hired to evaluate whether recipients were job ready.

waves were formed every other week until each pre-existing recipient had been selected or become ineligible.

Recipients were selected for each wave centrally by computer programmers. The selection process did not include intake interviews or objective assessments. The intention was to generate a random sample of selectees for each wave stratified by borough. In the fourth section, I report that the selection process did not approximate a random one and discuss the attendant adjustments that are made to the estimates presented in this paper.

### III. Quasi-experimental identification strategy

Generally, to identify the effect of a program one compares the outcomes of treated and untreated individuals. In the case of the ESPP, recipients who remained eligible should have ultimately been treated. Thus, recipients who remained untreated must have become ineligible prior to being selected for the ESPP and do not make a valid comparison group<sup>9</sup>.

#### A. An alternative approach to identify the effect of the ESP program

It is possible, however, to estimate the effect of the ESPP taking advantage of the wave enrollment process. Specifically, the effect of the program is estimated using a quasi-experiment in which all selectees for a wave are compared to all non-selectees for the same wave. By performing this comparison for each of the first 17 waves, and combining all selectees into one group, the program group, and all non-selectees into

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<sup>9</sup> Becoming ineligible was not exogenous. Recipients could become ineligible by exiting welfare or failing to comply with various program requirements.

another group, the control group (see Figure 1), one can estimate the Program Effect (PE)

<sup>10</sup>. Specifically, the PE is defined as,

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

where  $Y_i^M (P_i)$  is an indicator function which equals one if individual  $i$  started a job within  $M$  months of his or her inclusion in the program or control group and zero otherwise, and is a function of whether recipient  $i$  was a member of the program group,  $P_i=1$ , or the control group,  $P_i=0$ .

All selectees are considered treated regardless of whether or not they participated in the ESPP. This even includes recipients who failed to attend the ESPP orientation. Hence the PE estimates an ‘intent to treat’ effect and should not suffer from self-selection bias<sup>11</sup>.

This identification strategy also does not suffer from control group attrition, since each recipient who is placed in the control group remains in the control group for the remainder of the study. For example, control group members who subsequently were selected, and/or exited welfare, remain in the control group.

On the other hand, the PE does suffer from control group contamination. Over 70% of control group members were selected for the ESPP subsequent to their inclusion therein. Consequently, the PE is negatively biased and a conservative estimate of the program’s true effect.

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<sup>10</sup> Recall that ‘selectees’ are recipients who were selected for a given wave and that ‘non-selectees’ are recipients who were eligible, but not selected, for a given wave. After 17 waves, 6,782 recipients had been selected, 953 remained eligible, and 3,791 had become ineligible.

<sup>11</sup> In contrast, estimating the effect of participating in the ESPP, a ‘treatment on the treated’ effect, would suffer from self-selection bias. Recipients could self-select out of the ESPP by claiming a hardship, failing to comply with program requirements, or exiting welfare. Intent to treat and treatment on the treated effects are further discussed in Katz, Kling, and Liebman (2001).

## 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 selected within a given number of waves of their inclusion in the control group. For example, a control group with a one-wave restriction on being selected is created by removing selectees from the original control group if they were selected in the subsequent wave. So, a non-selectee from the first wave, who was a selectee for the second wave, would be excluded from this restricted control group.

It is important to note that the original control group includes two distinct cohorts: (1) non-selectees who remained eligible and were ultimately selected, and (2) non-selectees who became ineligible prior to being selected. This distinction is important because all members of the original control group who are removed to form the restricted control groups must have remained eligible (since they were selected). That is, they must have been in the first cohort. For each restricted control group to be valid a comparison group for the program group, the ratio of members of cohort one to members of cohort two must be the same in the restricted control group as in the original control group. Accordingly, the weight placed on members of the first cohort is increased in the restricted control group. Specifically, the weight is the reciprocal of the probability that members of cohort one were not selected for the subsequent wave (see Figure 2). I utilize the above procedure recursively to create control groups with a one-, three-, and

six-month restriction on being selected, denoted respectively as control group one, control group three, and control group six<sup>12</sup>.

#### IV. Empirical implementation

The case history and a limited set of demographic characteristics were extracted with NYC's permission from an administrative database for each eligible GA recipient<sup>13</sup>. From this data, the program and control groups were formed as described in the prior section. Members of the program and control groups are slightly more likely to be male than female and are likely to be non-white, in their late forties, and live in the Bronx, Brooklyn, or Manhattan (see Table 1). Comparing the two groups, one observes that the average age is similar, as well as the gender and racial distributions. Only the distribution of borough of residence is disparate, which presumably was the result of stratifying the selection by borough.

##### A. Selection process was not random

It is important to determine whether either selection process approximated a random one as was intended. I test whether recipients' demographic characteristics significantly impacted the probability of being selected. Specifically, the following probit equation was estimated,

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

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<sup>12</sup>This approach, unfortunately, cannot be extended further, since there were few control group members who remained eligible and were not selected within six months of their inclusion therein.

<sup>13</sup>The job readiness of each eligible GA recipients was evaluated, and GA recipients were selected for the ESPP first. Thus, pre-existing, eligible GA recipients are an ideal group for piloting this identification procedure. In contrast, the job readiness of less than half of the eligible family assistance recipients was evaluated prior to implementing the ESPP.

where  $P_i^{wb}$  is a program group dummy that equals one if individual  $i$  was a selectee and zero if individual  $i$  was a non-selectee in wave  $w$  and borough  $b$ ; and  $x_{ic}^{wb}$  is a series of  $C$  demographic characteristics for individual  $i$  in wave  $w$  and borough  $b$ . The number of selectees per borough varied widely across waves. To decrease the chance of generating spurious results, equation (2) is only estimated for the three largest waves in each of the four largest boroughs.

The coefficients on years continuously on welfare provide strong evidence that the selection process was not random<sup>14</sup>. Of the twelve coefficients, eight are positive and significantly different from zero (see Table 2). A similar, but weaker, pattern was observed for the coefficients on age. Seven are positive and significantly different from zero. Thus, the probability of being selected increased with age and welfare tenure. For the other demographic characteristics there was no discernable selection pattern.

One thing is certain though; eligible recipients were selected solely using information that was stored in the administrative database. The selection process was centralized and conducted by computer programmers. Individual caseworkers were not involved in any manner. 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.

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<sup>14</sup> The computer programmers who conducted the selection process mistakenly believed that sorting the list of eligible recipients by borough would cause each resulting borough list to be randomly ordered. Thus they simply selected recipients from the top of these lists.

## B. Adjusting for observed characteristics

Since recipients who were older, and who had longer welfare spells, should have been less likely to start a job and/or exit welfare, the PE as defined in equation (1) is potentially negatively biased. To adjust for the observed differences, a program dummy and a series of observed characteristics are regressed on an outcome dummy.

Specifically, the following equation is estimated,

$$y_i^M = \alpha^M + \beta^M P_i + \sum_{c=1}^C \lambda_c^M x_{ic} + \sum_{j=1}^4 \delta_j^M B_{ij} + \sum_{k=1}^4 \gamma_k^M W_{ik} + \sum_{j=1}^4 \sum_{k=1}^4 \eta_{jk}^M (B_{ij} * W_{ik}) + \varepsilon_i^M \quad (3)$$

where  $y_i^M$  is an outcome dummy that equals one if individual  $i$  started a job within  $M$  months of being a selectee or non-selectee, and zero otherwise;  $P_i$  is a program group dummy that equals one if individual  $i$  was a selectee and zero if individual  $i$  was a non-selectee;  $x_{ic}$  is a series of  $C$  demographic characteristics for individual  $i$  at the time that he or she became a selectee or non-selectee;  $B_{ij}$  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 a selectee or non-selectee in wave  $k$  and zero otherwise. Equation (3) is estimated using OLS for values of  $M$  between 0.5 and 26<sup>15</sup>.

## V. Results

Estimating equation (3), the PE peaks at 0.075 ( $t = 19.80$ ,  $p = 0$ ) when  $M = 4$ , indicating that selectees are 7.5 percentage points more likely to have started a job than

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<sup>15</sup> This approach enables one to estimate a very general, non-parametric hazard rate. Corrected standard errors are calculated by clustering the observations by individual. This is necessary since some individuals appear in the dataset repeatedly.  $M = 26$  is the maximum number of months for which there is post selection data for each recipient. Finally, each month is assumed to have 28 days. So there are 13  $M$ 's each year.

are non-selectees four months after the selection date (see Figure 3). After peaking, the PEs steadily decline in  $M$ . This pattern should be expected since control group contamination increases in  $M$ ; the intensity of contamination is illustrated by the ‘Percent of control group members selected’ line in the figure.

To adjust for control group contamination, equation (3) is estimated with control group one, three, and six. The PE peaks at 0.134 ( $t = 15.19$ ,  $p = 0$ ) when  $M = 9$  using control groups six, indicating that selectees are 13.4 percentage points more likely to have started a job than are members of control group six nine months after the selection date (see Figure 4).

As the length of the restriction increases, the months required to reach, and the magnitude of, the peak PE increases. Moreover, the PEs using control group six are superimposed over the PEs using control group three for more than three months. The former do not diverge from the latter until after the three month restriction on being selected expires. The same pattern is observed when comparing the PEs using control group three and one, and using control group one and the original control group. Thus, the PEs using control group six are probably the upper envelope of the true employment effect for six months post selection. After that, the true employment effect presumably diverges from, and lies above, these PEs.

The estimated effect is presumably not the result of the underlying economic conditions, since including the borough dummy, wave dummy, and interaction term should control for any macroeconomic shocks that may have occurred during the study

period. Moreover, prematurely terminating the study period on September 11<sup>th</sup>, 2001 does not materially affect the findings<sup>16</sup>.

Starting a job is not the sole outcome by which the ESPP should be evaluated. For it to be considered a true success, it must have also increased the probability that recipients remained off welfare and employed. This is illustrated by demonstrating that the ESPP increases the probability that recipients started a job and ‘permanently’ exited welfare<sup>17</sup>. Specifically, equation (3) is estimated with  $y_i^M$  redefined as an outcome dummy that equals one if individual  $i$  has started a job and permanently exited welfare within  $M$  months of becoming a selectee or non-selectee and zero otherwise. The PE peaks at 0.076 ( $t = 8.22$ ,  $p = 0$ ) when  $M = 17$  using control group six, indicating that selectees are 7.6 percentage points more likely to have started a job and permanently exited welfare than are members of control group six 17 months after the selection date (see Figure 5).

Finally, when all covariates are excluded from the regression, the PEs decrease by approximately 0.003, or less than one standard error (see Figure 6). The negative bias in the unadjusted PEs was expected since the probability of being selected was higher for recipients that were older and had longer welfare spells. This presumption is supported by the fact that the coefficient on age is consistently negative and significantly different than zero for all values of  $M$  (see Table 3).

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<sup>16</sup> After September 11, 2001, the unemployment rate in NYC increased by over two percentage points.

<sup>17</sup> For the purpose of this study, recipients permanently exited welfare if they exited welfare and did not return within two years of becoming a selectee or non-selectee. Only two years of follow-up data was provided for each recipient. NYC did not collect job retention data, and New York State was unwilling to provide the unemployment insurance wage records for study participants. Thus, the effect of the ESPP on the probability that recipients started a job and permanently exited welfare is used.

## VII. Robustness check

One might be concerned that ‘starting a job’ is better observed for members of the program group than for members of the control group<sup>18</sup>. This is unlikely for two reasons. First, over 70% of the control group was ultimately selected for the ESPP. Second, in 2000, NYC set the ambitious goal of placing 100,000 recipients in a job annually. To meet this goal, NYC had a strong incentive to accurately record each job placement.

Nevertheless, to rule out this possibility, I estimate the effect of the ESPP on recipients’ welfare use. Specifically, equation (3) is estimated with  $y_i^M$  redefined as an outcome dummy that equals one if individual  $i$  has permanently exited welfare within  $M$  months of becoming a selectee or non-selectee and zero otherwise. The PE peaks at 0.073 ( $t = 6.14$ ,  $p = 0$ ) when  $M = 13$  using control group six (see Figure 7)<sup>19</sup>. That is, the ESPP increased the probability that recipients exited welfare by 7.3 percentage points regardless of whether or not they started a job. Again, the following three patterns emerge: (1) after peaking, the PEs steadily decline in  $M$ , (2) as the length of the restriction increases, the magnitude of the peak PE increases, and (3) the PEs using control groups with long restrictions on being selected are superimposed over the PEs using control groups with shorter restrictions on being selected. Thus, the PEs using control group six are probably the upper envelope of the true exit effect for six months

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<sup>18</sup> This concern arises since ESPP contractors were paid for each job placement. Thus the contractors had a strong incentive to make sure that each placement was recorded.

<sup>19</sup> Note these PEs are smaller than other PEs presented in this paper. This difference is explained by two factors. First, there were many reasons, other than starting a job, why a recipient might have exited welfare. The probability of exiting welfare, conditional on not starting a job, was about one-third for the program and control group. Since fewer control group members started a job, more exited for other reasons. Second, selectees who started a job were slightly more likely to return to welfare than were non-selectees who started a job.

post selection. After that, the true effect presumably diverges from, and lies above, these PEs.

Comparing the estimated effect of the ESPP on the probability that a recipient started a job to the estimated effect on the probability that a recipient permanently exited welfare, one observes that the former effect peaks approximately six percentage points higher than does the latter. This is explained by two factors. First, there were many paths off welfare other than starting a job, for example, moving out of NYC or failing to comply with program requirements. This potentially explains over half the variance, since that the probability of exiting welfare, conditional on not starting a job, is about one-third for selectees and non-selectees, and more selectees started a job. Second, selectees who started a job were slightly more likely to return to welfare than were non-selectees who started a job. Thus, the observed employment effect appears to be robust.

## VIII. Discussion

The ESPP appears to have had a persistent positive impact. It significantly increased the probability that selectees started a job and/or exited welfare. Moreover, most selectees were placed in direct hire jobs, and such jobs were shown to increase earnings and quarters of employment by Autor and Houseman (2005). Finally, previous experimental studies found that similar welfare-to-work programs, mandatory work programs with an emphasis on job placement, increased employment by an average of 9 percentage points and decreased welfare use by an average of 6 percentage points (Grogger and Karoly, 2005). The estimated effect of the ESPP is quite similar.

The primary contribution of this paper is the development of a new quasi-experimental approach to identify the effect of such programs. This is important since

randomized experiments can be costly, difficult to implement, and/or unavailable. Moreover, experiments are not impervious to criticism, and as was discussed previously, this methodology addresses three of the five known shortcomings. Finally, if other programs faced capacity constraints, this technique should be usable even without advanced preparation<sup>20</sup>.

The identification strategy developed in this paper is not without its own shortcomings. Most notable is the control group contamination that is inherent in using non-selectees, who later become selectees, as the comparison group. Thus, one can only estimate short-term peak effects; long-term effects are not identifiable. This limitation is not unique to this study, however. Often only a year or two of follow-up data regarding recipients' welfare use and employment is available (Friedlander et al, 1997), so only short-term effects are identifiable.

This research also contributes to the literature by studying the effect of welfare reform in NYC. Neither randomized experiments nor non-experimental studies have been conducted on welfare-to-work programs there. Yet, 23 percent of all GA recipients, and 7 percent of all family assistance recipients, lived there in 1996. Moreover, Chernick and Reimers (2004) appears to be the only non-experimental study that focuses on NYC. Their study found that welfare use declined and earnings increased for the 'at risk'

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<sup>20</sup> I will use the procedure in a series of future research projects that investigate the following: the effect of two different welfare-to-work programs on the same population of recipients, the effect of the same welfare-to-work program on family assistance and general assistance recipients, and the effect of a welfare-to-work program on recipients' homeless shelter use.

group<sup>21</sup>. The authors acknowledged, however, that the observed changes could have been caused by welfare reform, the robust economy, or another factor<sup>22</sup>.

Finally, one should consider whether the fiscal benefits of the ESPP outweighed the costs. The observed cost of a placement was approximately \$3,000. However, some selectees would have found a job on their own. Consequently, the relevant cost is that of a ‘new’ placement, one that would not have occurred without the ESPP. Given that approximately 35% of selectees started a job within two years of being selected and that the peak PE was approximately 13 percentage points, NYC had to pay for 35 placements to generate 13 new placements. Consequently, the cost of a new placement was approximately \$8,100, or  $\$3,000 \times (35/13)$ .

The fiscal benefit per placement is harder to calculate since it is time dependent, that is, it depends on how long a recipient remained employed and off welfare<sup>23</sup>. In early 2000, a typical GA recipient received \$350 per month in GA and \$120 per month in food stamps. This totals almost \$6,000 annually. Additionally, one must add the cost of Medicaid benefits (each GA recipient qualified for Medicaid), administrative costs, and any income taxes paid by former recipients, to the benefit calculation. This presumably adds at least a \$1,000 annually. Thus, an estimate of the fiscal benefit is \$7,000 annually.

Unfortunately, only two years of post enrollment data is available, and it is not possible to determine whether recipients who started a job, remained off welfare past the break even point, 1.2 years, or  $\$8,100 \div 7000$ . At the end of the study, over two-thirds of

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<sup>21</sup> At risk households were defined as those headed by a mother with a minor child as well as those headed by an uneducated, non-elderly, childless adult.

<sup>22</sup> Another unique aspect of this study is that it investigates the impact of a welfare-to-work program on GA recipients. Ifcher (2007) explores the implications of this.

<sup>23</sup> It also varies by recipient. Different recipients may have received different benefits.

the selectees who started a job were still off welfare. These recipients had, on average, already been off welfare for a year. Projecting the benefits into the future, assume that in each subsequent year one-third of the selectees, who started a job and were still off welfare, returned to welfare<sup>24</sup>. With such an assumption, the fiscal benefits of the ESP program easily surpass the costs. Even with an extremely conservative assumption that 60% of those still off welfare return to it in each subsequent year, the fiscal benefits of the program outweigh the costs. Thus, it seems highly likely that the ESPP passes a fiscal cost benefit test.

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<sup>24</sup> Previous studies have suffered from the same limitation and have projected the benefits of short-term training programs into the future as well (Friedlander, 1997).

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Table 1

## Demographic Characteristics of Members of the Program and Control Groups

<b>Demographic characteristic</b>	<b>Program group</b>	<b>Control group</b>
<b>Observations</b>	6,782	58,051
<b>Male</b>	53.9%	54.1%
<b>Race</b>		
Asian	0.9%	1.2%
Black	47.8%	49.3%
Hispanic	37.1%	34.9%
White	8.6%	9.6%
Other	0.6%	0.7%
Not reported	5.0%	4.2%
<b>Borough of residence</b>		
Bronx	30.8%	39.9%
Brooklyn	26.5%	31.4%
Manhattan	28.9%	16.8%
Queens	12.4%	11.0%
Staten Island	1.4%	0.6%
<b>Age</b>	48.2 (8.315)	47.2 (8.727)
<b>Years continuously on welfare</b>	3.7 (3.08)	3.3 (3.1)

Standard deviations are given in parenthesis.

Table 2

Coefficients from Estimating Equation (2) for Select Borough-Wave Pairs

Demographic characteristic	Bronx 3rd wave	Bronx 4th wave	Bronx 12th wave	Brooklyn 3rd wave	Brooklyn 11th wave	Brooklyn 16th wave	Manhattan 1st wave	Manhattan 2nd wave	Manhattan 3rd wave	Queens 4th wave	Queens 5th wave	Queens 6th wave
<b>Male</b>	-0.043	0.059	0.009	0.188 **	-0.132	0.398 ***	0.140 **	-0.020	0.018	0.150	-0.105	-0.016
<b>Race</b>												
Asian	-0.023	-	-	-	-0.161	-1.541 ***	-0.579	-0.661	0.527	0.140	-0.305	0.266
Black	-0.072	-0.048	0.116	-0.086	0.281	-0.417	-0.299 **	-0.040	-0.220	-0.056	0.051	0.293
Hispanic	0.087	-0.049	0.234	-0.050	0.371	-0.353	-0.249 *	-0.020	0.113	0.099	0.117	0.513
White	0.206	-0.122	-0.013	-0.318 *	0.187	-0.687 **	-0.142	-0.226	-0.429	-0.175	-0.031	0.042
<b>Average age</b>	0.008 **	0.002	0.018 ***	0.012 ***	0.003	0.008	0.007	0.009 **	0.011 *	0.007	0.021 ***	0.018 **
<b>Years on welfare continuously</b>	0.080 ***	0.067 ***	-0.020	0.044 ***	-0.057 ***	0.045 ***	0.109 ***	0.079 ***	-0.010	0.093 ***	0.012	-0.042

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

- variable was dropped because there were not enough observations

Table 3

Coefficients from Estimating Equation (3) using Control Group Six

Demographic characteristic	Four months post selection			26 months post selection		
	(1)	(2)	(3)	(4)	(5)	(6)
<b>PE</b>	0.134 ***	0.139 ***	0.134 ***	0.075 ***	0.083 ***	0.082 ***
<b>Age</b>		-0.002 ***	-0.002 ***		-0.004 ***	-0.004 ***
<b>Years continuously on welfare</b>		0.002	0.003		0.001	0.000
<b>Male</b>		-0.007	-0.004		-0.020	-0.016
<b>Race</b>						
Asian		0.109	0.093		0.080	0.061
Black		0.012	0.003		0.022	0.013
Hispanic		-0.007	-0.006		-0.052	-0.043
White		0.004	-0.020		-0.014	-0.046
<b>Borough</b>						
Bronx			-0.038			-0.076
Brooklyn			0.051			0.041
Manhattan			-0.004			0.026
Queens			0.045			0.022
<b>Wave dummy</b>	NO	NO	YES	NO	NO	YES
<b>Interaction term</b>	NO	NO	YES	NO	NO	YES

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

Figure 1

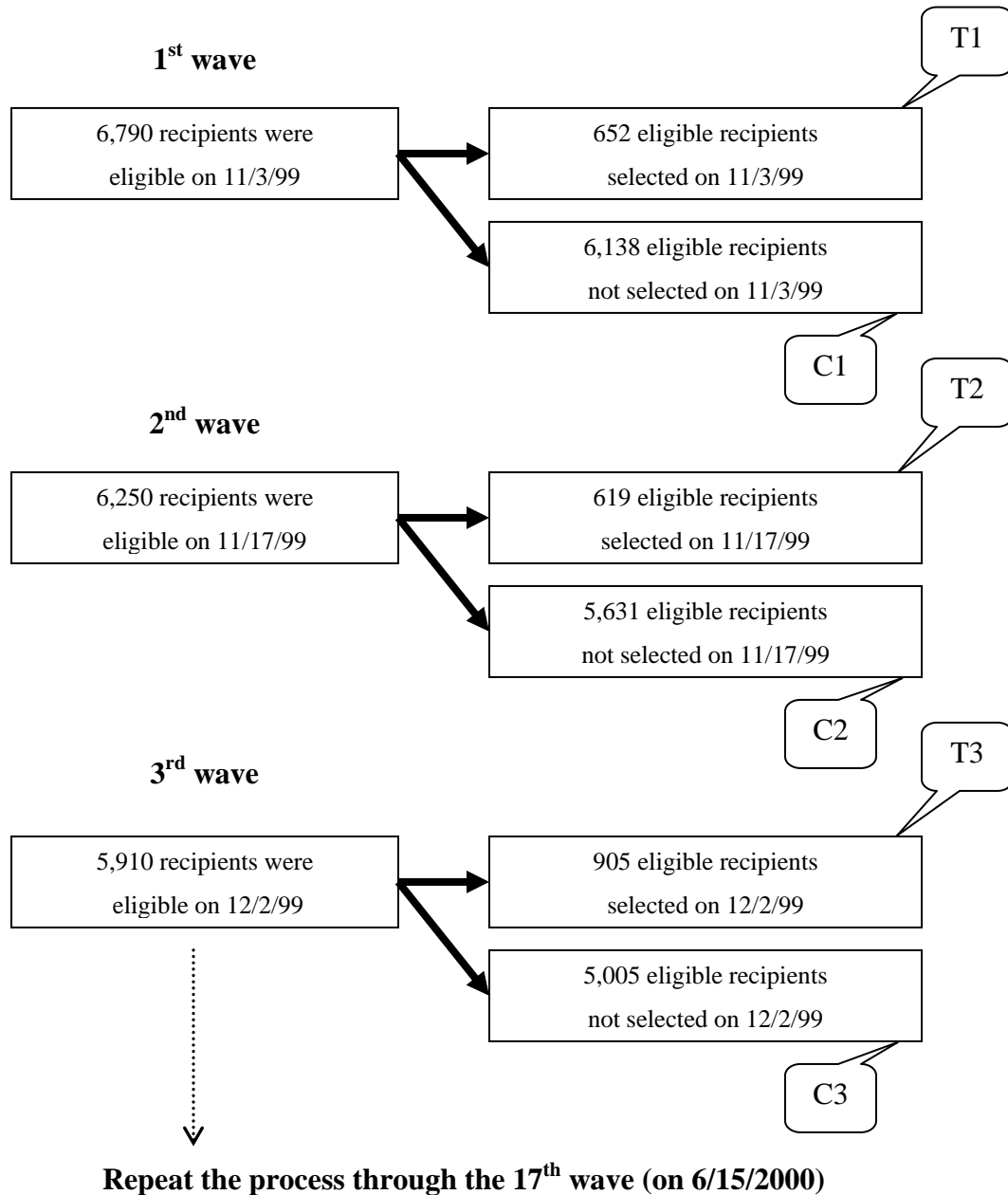
The Formation of the Program and Control Groups

**The program group:**

All selectees from the first 17 waves. Specifically it is the union of T1, T2, T3, ....., T17.

**The control group:**

All non-selectees from the first 17 waves. Specifically it is the union of C1, C2, C3, ....., C17<sup>1</sup>.



<sup>1</sup> Note that many recipients were non-selectees in multiple waves. Consequently, many recipients are members of the control group multiple times.

Figure 2

The Formation of the Program Group and the Control Group with a One-Wave Restriction on Being Selected

**The treatment group:**

All selectees from the first 17 waves. Specifically it is the union of T1, T2, T3, ....., T17.

**The control Group with a one-wave restriction on being selected**

All recipients who were:

1. non-selectees during any of the first 17 waves and
2. not selected in the wave subsequent to the one in which they were a non-selectee.

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 selected in the subsequent wave, conditional on being eligible to be selected in that wave, for example, for members of C1b the weight is the reciprocal of  $(5,248/5,860)$ , or  $(1/0.896) = 1.116$ .

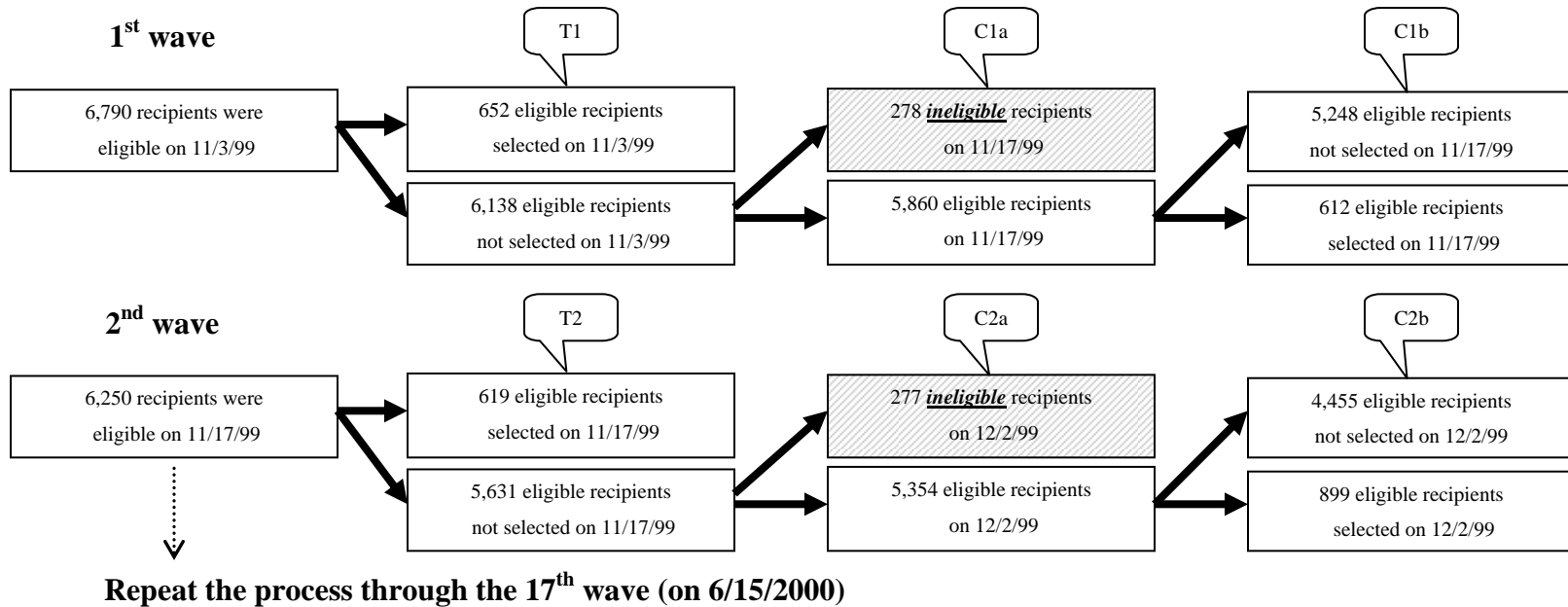


Figure 3

Estimated effect of ESPP on starting a job

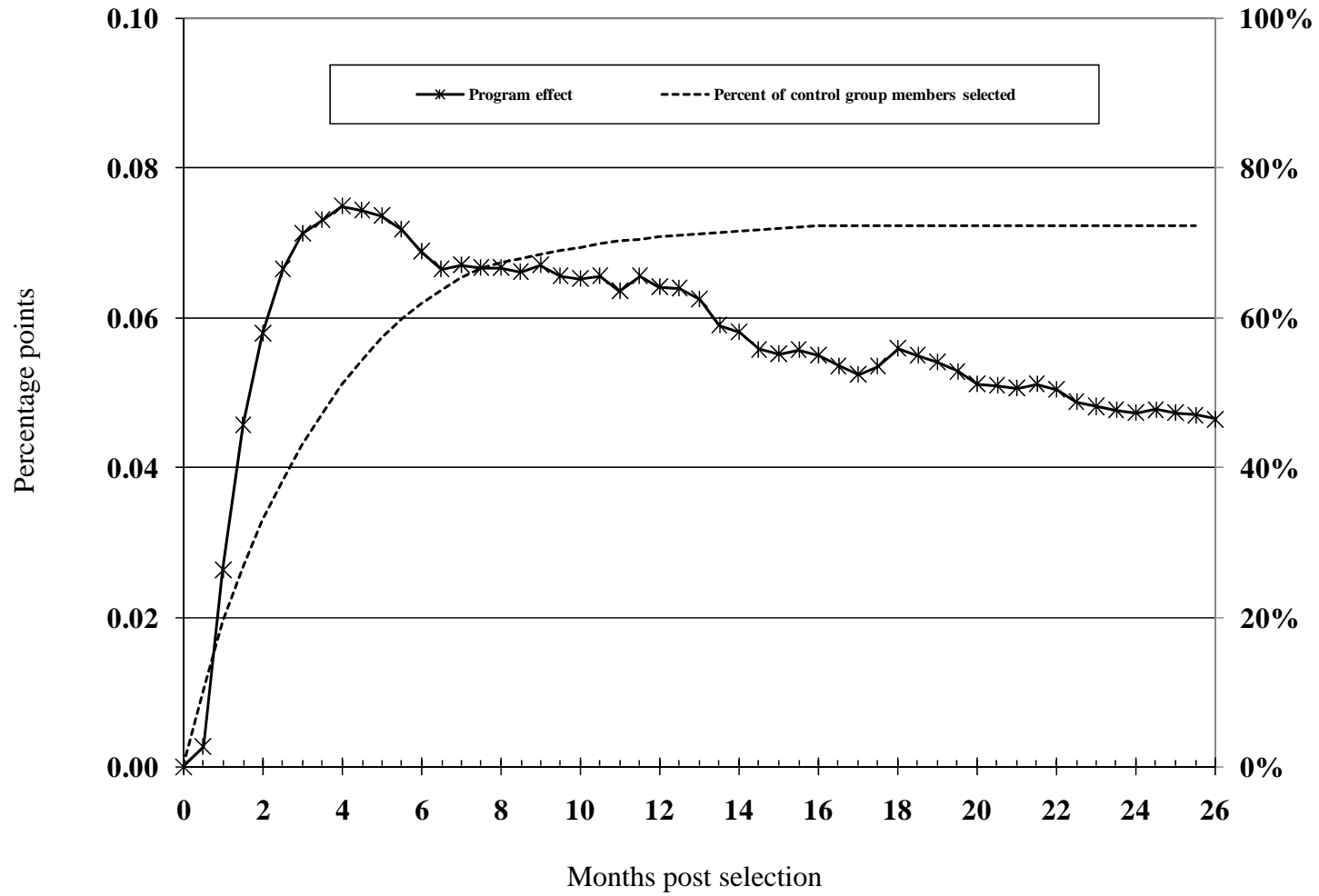


Figure 4

Estimated effect of ESPP on starting a job using various control groups

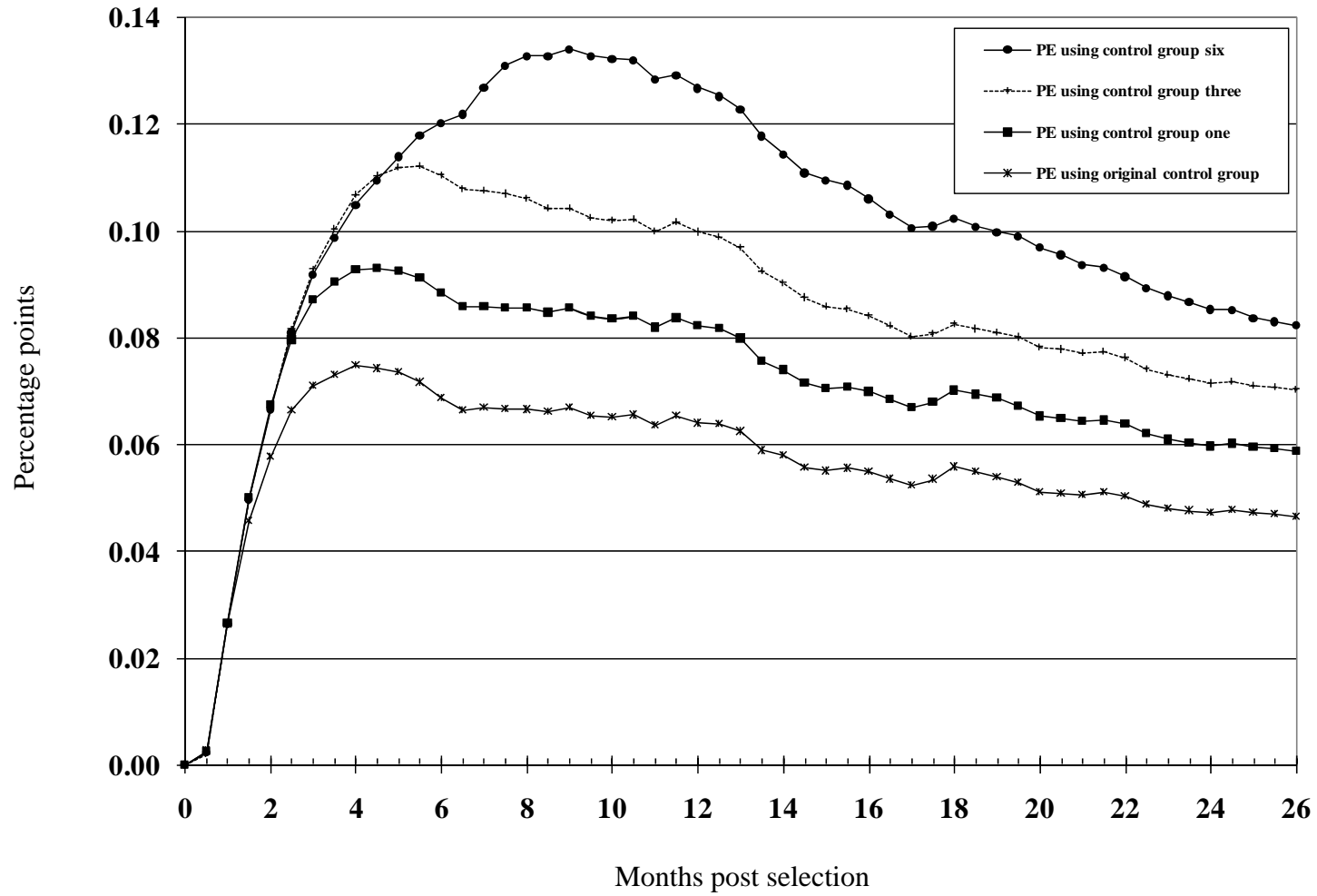


Figure 5

Estimated effect of ESPP on starting a job and permanently exiting welfare

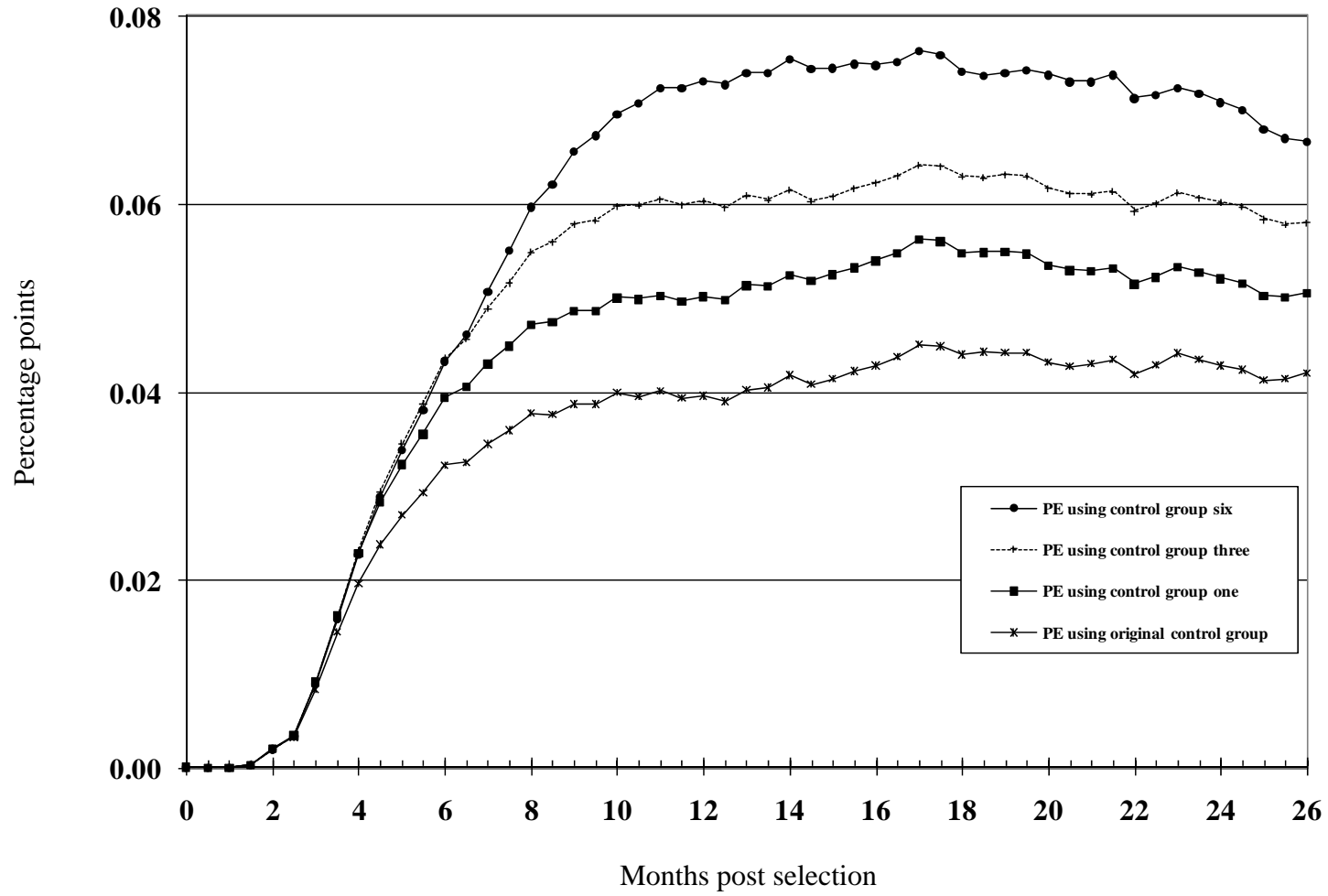


Figure 6

Estimated effect of ESPP with various covariates included using original control group

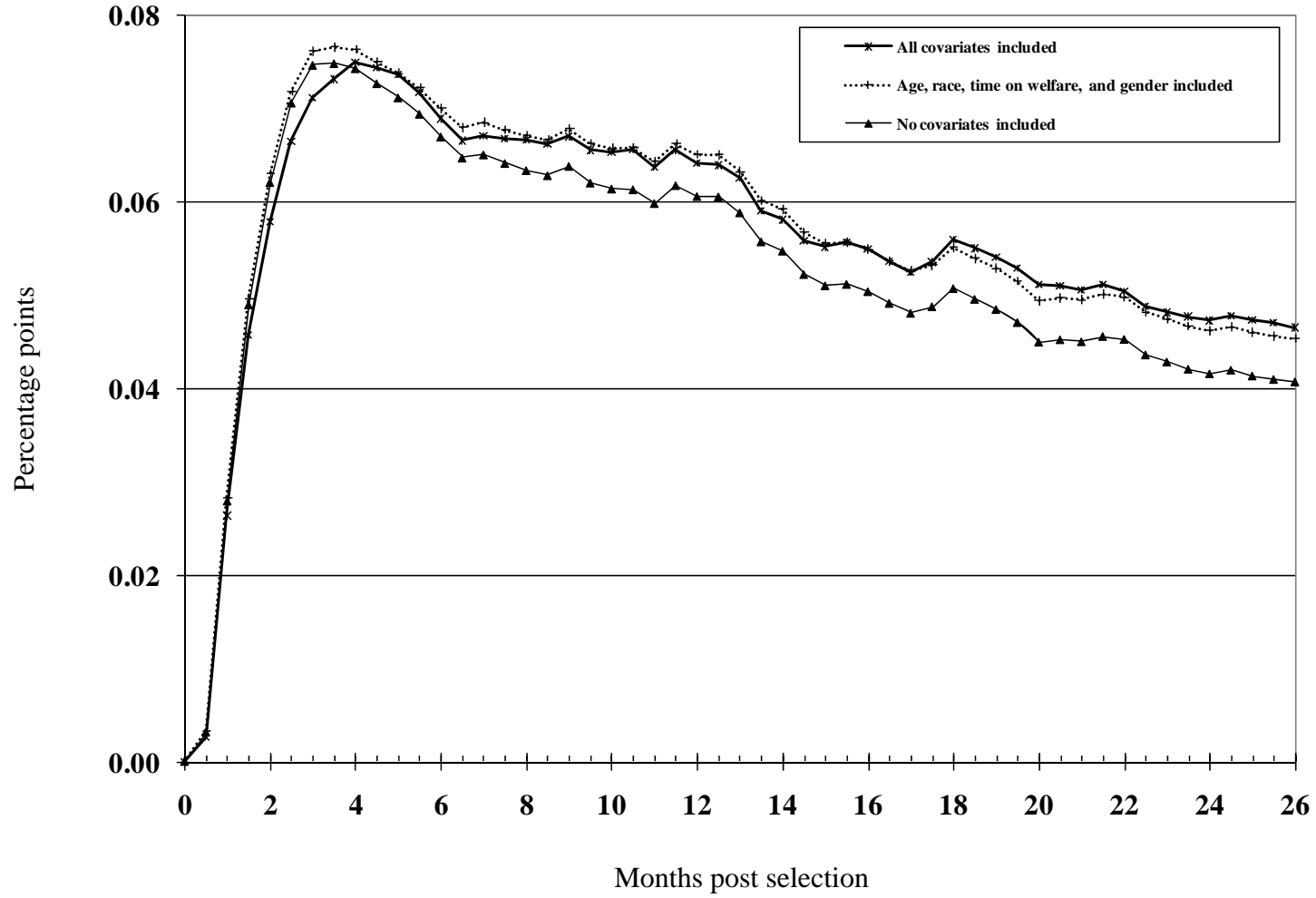


Figure 7

Estimated effect of ESPP on permanently exiting welfare

