

An Overlooked Impact of Welfare Reform: The Effect on General Assistance Recipients

Evidence from Two Welfare-To-Work Programs in New York City

by

John Ifcher¹
Santa Clara University
Department of Economics

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¹ John Ifcher, Santa Clara University, Department of Economics, 500 El Camino Real, Santa Clara, CA, 95053, 408-554-5579 (phone), 408-554-2311 (fax), jifcher@scu.edu.

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Abstract

To date very little is known regarding General Assistance (GA) recipients' response to welfare reform. In New York City, GA recipients were required to participate in a workfare and a welfare-to-work program. Recipients were initially enrolled in each program in 'waves' due to program capacity constraints. I identify the effect of the programs using a quasi-experiment in which enrollees are compared to concomitantly eligible, non-enrolled recipients. I find that each program increased welfare exits and that the latter program also increased employment. The magnitude of these effects is similar to the effect that similar programs have on family assistance recipients.

I. Introduction

Over the past two decades welfare reform has transformed U.S. welfare programs. A vast literature has developed to identify the resulting changes in welfare use, employment, family structure, and well-being. One impact, however, has not been studied: the effect of welfare reform on General Assistance (GA) recipients¹. While it is true that in 1996, the year that the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) passed, the majority of welfare recipients received Family Assistance (FA), the GA caseload was not trivial. At the time, there were 750,000 GA cases, which represented 15 percent of the combined FA and GA caseload^{2,3} (author's calculations based on Uccello et al, 1996 and Office of Family Assistance, 1996). Furthermore, many of the same welfare reform policies that were imposed on FA recipients were imposed on GA recipients. For example, in New York City (NYC) welfare reform programs were often imposed on GA recipients first and then on FA recipients.

In this paper, I estimate the effect of two welfare-to-work programs in NYC, a workfare program and a job training program, on GA recipients' welfare use and employment. To do so, I take advantage of a quirk in each program's administration. Specifically, when each program was initiated, the entire cohort of eligible recipients could not be enrolled concurrently due to capacity constraints. Rather, recipients were selected for each program in 'waves.' The wave selection process enables me to identify each program's effect using a quasi-experiment in which recipients who were selected for a given wave are compared to recipients who were eligible for

¹ GA is cash assistance for needy individuals who are not covered by federally funded income maintenance programs. Section III provides an overview of GA programs in the U.S.

² The average number of recipients per case was higher for FA than for GA. Thus, GA recipients represented 7% of all welfare recipients.

³ The cost of GA was approaching \$2 billion. Moreover, many GA recipients received in-kind benefits, such as, food stamps and medical assistance. The cost of in-kind benefits is not included in this figure.

the same wave, but not selected⁴. The results indicate that that the workfare program increased the probability that GA recipients exited welfare by at least nine percentage points, and that the job training program increased the probability that GA recipients started a job and exited welfare by at least eight and four percentage points, respectively.

It is also noteworthy that the two welfare-to-work programs were imposed on the same cohort of recipients. GA recipients who were eligible for the job training program had been on welfare for an average of three years, and during that time, been exposed to welfare reform, including the workfare program. Yet, the job training program still had the intended effect, decreasing welfare use and increasing employment. This finding is important because it suggests that additional reforms can be used to help long-term recipients unmoved by prior reforms.

The next section of this paper provides a brief overview of the previous literature. The third and fourth sections describe GA programs in the U.S. and welfare reform in NYC, respectively. The fifth section explains the quasi-experimental identification strategy. The sixth section describes the empirical implementation. The seventh section presents the results. The final section discusses the implications of the results.

II. Relevant Literature

A vast number of studies have explored, and much has been learned about, the effects of welfare reform (recent reviews include Blank, 2002; Grogger and Karoly, 2005; and Moffitt, 2003). For example, it appears that in the short run, at least, welfare reform, taken as a whole, reduced welfare use and increased employment (Grogger and Karoly, 2005). The effect on family structure and well-being has been studied but remains less well understood.

⁴ Computer programmers performed the wave selection process centrally. Intake interviews and objective assessments were not performed.

GA recipients, however, have been systematically excluded from previous research. Experimental studies have excluded GA recipients since the evaluated welfare-to-work programs were wholly for FA recipients⁵ (Bloom and Michalopoulos, 2001, have synthesized the results from 29 such studies). Non-experimental studies have excluded GA recipients as well. For example, many studies have relied on inter-state variation in the implementation date of reforms to FA (e.g., Bitler, Gelbach, Hoynes, and Zavodny, 2004), or have compared FA-prone and non-FA-prone adults (e.g., Bitler, Gelbach, and Hoynes 2005), to identify the impact of welfare reform.

The only research that does not exclude GA recipients appears to be Chernick and Reimers (2004). In this paper, the authors compared the earnings and welfare use of ‘at risk’ households in NYC before and after PRWORA⁶. 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. Therefore, there should be FA and GA cases within their at risk group. Chernick and Reimers found that welfare use declined and earnings increased for the at risk group. The authors acknowledged that the observed changes could have been caused by welfare reform, the robust economy, or another factor. Moreover, while any research that does not exclude GA recipients is welcome, the authors were unable to estimate the effect on FA- and GA- prone households separately due to the small sample size. Finally, this study appears to be the only research focused on welfare reform in NYC where 23 percent and 7 percent of all GA and FA recipients lived, respectively, in 1996.

⁵ Prior to the passage of PRWORA, the federal FA program was named Aid to Families with Dependent Children (AFDC). It is now named Temporary Assistance to Needy Families (TANF).

⁶ Data for this study came from the NYC sample of the March Current Population Survey.

Unlike the dearth of research regarding the effect of welfare reform on GA recipients, there is a plethora regarding its effect on FA recipients. Many of these studies investigate the impact of welfare-to-work programs that are similar to the job training program studied in this paper: a mandatory work program with an emphasis on job placements⁷. The experimental evidence indicates that such programs reduced welfare use by an average of six percentage points and increased employment by an average of nine percentage points⁸ (Grogger and Karoly, 2005).

A ramification of not studying the effect of welfare reform on GA recipients is that little is known regarding its effect on men. Most prior research has focused on single mothers (for example, Bloom and Michalopoulos, 2001, and Grogger and Karoly, 2005). There are some exceptions though. For example, Friedlander et al (1997) reviewed a handful of studies that estimated the impact of mandatory job training programs on male and female FA recipients⁹. Interestingly, while these programs significantly increased women's earnings, they had no significant impact on men's earnings. In contrast, each welfare-to-work program studied herein has a significant impact on men.

III. GA programs in the U.S.¹⁰

GA is a catchall phrase for cash assistance programs that provide benefits to financially needy individuals and families that are not covered by the federal safety net. GA programs are

⁷ In contrast, no papers have examined the impact of a workfare program similar to the one studied herein: an unpaid work experience program that did not include additional components, such as, class room training or job search assistance.

⁸ Non-experimental studies that estimate the impact of mandatory work requirements on welfare use generate inconsistent results.

⁹ Male FA recipients received benefits through the AFDC-Unemployed Parent program.

¹⁰ The information contained in this section was gleaned from a comprehensive report regarding GA by Uccello, McCallum, and Gallagher (1996).

funded and administered by states and or localities. In 1996, 42 states had GA in at least one locality and 33 had a statewide GA program.

Eligibility criterion, benefit levels, and program requirements varied widely. For example, while 12 states, including New York, offered benefits to all low-income adults who were financially needy in 1996, other states restricted benefits to one or two targeted groups, for example, pregnant women or disabled adults¹¹. GA cash grants were generally parsimonious and averaged approximately 40 percent of the federal poverty line. Many GA recipients also received food stamps and medical assistance.

The GA caseload was not distributed evenly among the states. A minority of states had a majority of cases. For example, only five states – California, Connecticut, New Jersey, New York, and Pennsylvania – had a GA caseload over 25,000; these five states had a combined GA caseload of approximately 0.55 million, or about 75 percent of all GA cases¹² (authors calculations based on Uccello, McCallum, and Gallagher, 1996). Perhaps not surprisingly, given the dynamic welfare policy environment, these states also reformed their GA programs: limiting eligibility, instituting time limits, and expanding work requirements (Department of Public Social Services, 2006).

IV. Welfare reforms 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 GA recipients. The city was spending approximately three billion dollars annually on welfare programs, including one billion dollars on GA.

¹¹ After passage of PRWORA, some states used their GA programs to close new gaps in the federal safety net. For example, New York used their GA program to provide benefits to families that exhausted their federal FA benefits.

¹² The FA caseload was not distributed evenly among the states either. The five states with the largest caseloads had about 45% of all FA cases (authors calculation based on Office of Family Assistance, 1996).

In 1995, NYC initiated the Work, Accountability, and You (NYCWAY) program. A central tenet of NYCWAY was that able-bodied welfare recipients were expected to work in exchange for their benefits¹³. Almost all recipients fulfilled this requirement by participating in the Work Experience Program (WEP). Over the next six years, the number of welfare recipients in NYC declined by more than 50 percent and the number of GA recipients declined by 70 percent. The WEP was mandatory for all able-bodied welfare recipients. Participants received no compensation other than their welfare benefits and a nominal stipend for carfare and lunch¹⁴. Recipients generally participated in a WEP assignment 21 hours per week. Dozens of city agencies were enlisted to create tens of thousands of WEP assignments. Three departments, Parks and Recreation, 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, streets, and highways.

In 1999, NYC created the Employment Services and Placement Program (ESPP), a job training and outplacement assistance program. In the subsequent two years, over 100,000 welfare recipients started a job annually, more than twice the pre-ESPP rate. 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

¹³ Other components of NYCWAY included enhanced detection of welfare fraud, increased substance abuse treatment, and diversion to federally funded disability insurance where appropriate.

¹⁴ In 1996, the stipend was reduced so that it only covered carfare.

received a substantial payment¹⁵ for each recipient they placed in a job. The contractors focused on developing participants' 'soft skills,' for example, résumé writing and interview skills, and helped participants arrange job interviews. ESPP contractors were required to attempt to place each recipient participating in the program¹⁶.

Prior to implementing the WEP and ESPP, there were approximately 100,000 and 15,000 eligible GA recipients, respectively. Post implementation, all eligible 'pre-existing' recipients were required to participate in the WEP if they were 'able-bodied'¹⁷, and in the ESPP if they were a WEP participant and 'job ready'¹⁸.

Since not all pre-existing recipients could be accommodated simultaneously, recipients were enrolled in waves. The wave selection process started in early 1995 and late 1999 for the WEP and ESPP, respectively. 'Selectees' were informed of their status by mail, instructed to report to the proper location at a prescribed date and time, and advised that they would be sanctioned if they failed to comply with the program's requirements¹⁹. New waves were formed every week for the WEP and every other week for the ESPP until each pre-existing recipient had been selected or become ineligible²⁰.

Recipients were selected for each wave centrally by computer programmers. The selection process did not include intake interviews or objective assessments. For the WEP, the selection process was based on observable characteristics. The selection criteria changed frequently²¹ and were not documented²². For the ESPP, the intention was to generate a random

¹⁵ The average payment was approximately \$3,000.

¹⁶ After six weeks, if a participant had not been placed, the contractor could stop attempting to place the recipient.

¹⁷ Caseworkers were responsible for determining whether a recipient was able-bodied.

¹⁸ Private contractors were hired to evaluate whether recipients were job ready.

¹⁹ Typically, the program start date was two weeks after the selection date.

²⁰ To become ineligible, a recipient could have exited welfare or failed to comply with program requirements.

²¹ 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.

sample of selectees for each wave stratified by borough. In section VI, I report that neither selection process approximated a random one and discuss the attendant adjustments that are made to the estimates presented in this paper.

V. 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 WEP and ESPP, recipients who remained eligible should have been ultimately treated. Recipients who remained untreated must have become ineligible prior to being selected, and thus they do not make a valid comparison group²³.

It is possible, however, to estimate the effect of the WEP and ESPP taking advantage of the wave enrollment process. Specifically, the effect of the program is estimated using a quasi-experiment in which all selectees from a wave are compared to all recipients who were eligible to be selected for the same wave but were not²⁴. By conducting this comparison over multiple waves and combining all selectees into one group, the program group, and all eligible non-selectees into another group, the control group, one can estimate the Program Effect (PE). 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 exited welfare (in the case of the WEP) or started a job (in the case of the ESPP) 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, $P_i=0$.

²² Neither the program managers, nor the computer programmers, recorded the criteria.

²³ Recall that becoming ineligible was not exogenous.

²⁴ This method is similar to using a waitlist as a comparison group.

Note all selectees are considered treated regardless of whether or not they participated in the assigned program. This even includes recipients who failed to attend the program's orientation. Hence the PE estimates an 'intent to treat' effect and should not suffer from self-selection bias²⁵.

The PE 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 example, control group members who were subsequently selected or exited welfare remain in the control group. On the other hand, the PE does suffer from control group contamination. Over 40 percent and 70 percent of control group members were selected for the WEP and ESPP subsequent to their inclusion therein, respectively. Consequently, the PE is negatively biased and a conservative estimate of the programs' true effect.

V. Empirical implementation

For the WEP, a simple random sample of 3,595 recipients was drawn from among all eligible GA recipients. For each member of the random sample, an abridged case history and a limited set of demographic characteristics were compiled from an administrative database with NYC's permission. For the ESPP, 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. From this data the program and control group for the WEP and ESPP were formed as described in the prior section.

²⁵ In contrast, estimating the effect of participating in the assigned program, a 'treatment on the treated' effect, would suffer from self-selection bias. Recipients could self-select out of the assigned program by claiming a hardship, failing to comply with program requirements, or exiting welfare. Hardship claims were evaluated on a case by case basis. Recipients who failed to comply with program requirements were sanctioned. Intent to treat and treatment on the treated effects are further discussed in Katz, Kling, and Liebman (2001).

There were three data collection issues for the WEP that warrant mentioning here²⁶. First, it was only possible to compile 13 months of data (from February 1995 to February 1996) for a random sample of the population²⁷. Second, due to the dated design of the administrative database each recipient's case status was only observable at the end of each month²⁸. Consequently, any change in case status that occurred between the end-of-month observations was missed. The most significant impact of this limitation is that it generates a negative bias in the estimated PE since some recipients were selected for the WEP, but were not observed to be²⁹. Third, owing to a programming error, after recipients received a WEP assignment their subsequent case status was not collected. It is conservatively assumed that these recipients did not exit welfare thereafter. This also negatively biases the estimated PE since members of the program group were significantly more likely to have received a WEP assignment than were members of the control group. The negative bias introduced by the latter two is discussed in the results section of this paper.

A. Descriptive statistics

For the WEP, members of the program and control group are more likely to be male than female and are likely to be non-white, in their late thirties, and live in Brooklyn or Manhattan (see columns (1) and (2) of Table 1). Comparing the two groups, one observes that they have relatively similar borough, gender, and racial distributions. The most evident difference is that members of the program group are, on average, three years younger than are members of the control group.

²⁶ See Ifcher (2006) for additional details regarding these data collection issues.

²⁷ NYC did not have the capability to extract a dataset from the administrative database. Rather, the necessary data was printed and transcribed into an analytic database. Furthermore, the administrative database had been archived onto backup tapes that had to be retrieved from long-term storage in Albany, NY.

²⁸ The data for this study was collected from a legacy 'point-in-time' database that did not store case histories. NYC did, however, save end-of-month 'snapshots' of the data.

²⁹ Such selectees mistakenly end up in the control group rather than in the program group.

For the ESPP, members of the program and control group are also 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 columns (3) and (4) of 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.

Comparing the WEP and ESPP data, it is striking that the selectee for the ESPP was on average about 10 years older than the selectee for the WEP. This increase occurred over approximately five years. This trend suggests that young GA recipients, who presumably had better alternative opportunities, were more likely to have exited welfare between 1995 and 2000. Additionally, the percentage of black and Hispanic recipients increased while the percentage of white recipients decreased.

B. Selection process was not approximately random

It is interesting to determine whether either selection process approximated a random one. In Ifcher (2006 and 2007), I test whether this is the case and find that neither did. For the WEP, the results indicate that younger recipients were more likely to be selected than were older recipients. For the ESPP, the results indicate that older recipients, and recipients who had been on welfare longer, were more likely to be selected³⁰.

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

³⁰ 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.

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.

C. Adjusting for observed characteristics

Since recipients who were younger, and who had shorter welfare spells, should have been more likely to exit welfare and start a job, the PE as defined in equation (1) is potentially 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,

$$y_i^M = \alpha^M + \beta^M P_i + \sum_{c=1}^C \lambda_c^M x_{ic} + \sum_{j=1}^4 \delta_j^M B_{ij} + \sum_{k=1}^4 \gamma_k^M W_{ik} + \sum_{j=1}^4 \sum_{k=1}^4 \eta_{jk}^M (B_{ij} * W_{ik}) + \varepsilon_i^M \quad (2)$$

where y_i^M is an outcome dummy that equals one if individual i exited welfare (in the case of the WEP) or started a job (in the case of the ESPP) within M months of his or her inclusion in the program or control group and zero otherwise; P_i is a program group dummy that equals one if individual i was in the program group and zero otherwise; x_{ic} is a series of C demographic characteristics for individual i at the time that he or she was included in the program or control group; 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 program or

control group in wave k and zero otherwise. For the WEP and ESPP, equation (2) is estimated using OLS for values of M between 1 and 7, and 0.5 and 26, respectively³¹.

VI. Results

Estimating equation (2) with the WEP data, one finds that the PE peaks at 0.088 ($t = 6.17$, $p = 0$) when $M = 2$, indicating that selectees are 8.8 percentage points more likely to have exited welfare two months after the selection date than are members of the control group (see panel A of Figure 1). As expected, after peaking the PE decreases in M as control group contamination increases. Moreover, the peak PE is not simply the result of recipients ‘churning’ on and off welfare. Redefining $y_i^M(P_i)$ in equation (2) as an outcome dummy that equals one if individual i exited and remained off welfare for two consecutive months within M months of his or her inclusion in the program or control group and zero otherwise. The PE peaks at 0.080 ($t = 5.85$, $p = 0$) when $M = 2$ (see panel A of Figure 2). Similar results follow if one uses three consecutive months off welfare as well.

Estimating equation (2) with the ESPP data, 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 four months after the selection date than are members of the control group (see panel B of Figure 1). Again, as expected after peaking the PE decreases in M as control group contamination increases. Furthermore, the ESPP appears to increase the probability that recipients remained employed and off welfare. This is illustrated by demonstrating that the ESPP increases the

³¹ 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. $M = 7$ and 26 is the maximum number of months for which there is post selection data for each recipient for the WEP and ESPP, respectively. Finally, for the ESPP each M is assumed to have 28 days.

probability that recipients started a job and ‘permanently’ exited welfare³². Specifically, equation (2) 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 his or her inclusion in the program or control group and zero otherwise. The PE peaks at 0.045 ($t = 9.88$, $p = 0$) when $M = 17$ (see panel B of Figure 2).

A. Estimation with and without covariates

The WEP PEs increase, on average, by 0.014, or a little less than one standard error, when all covariates are excluded from the regression (see panel A of Figure 3). The positive bias in the unadjusted PEs was expected since the probability of being selected was higher for younger recipients, and such recipients were presumably more likely to have exited welfare. 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 (for example, see columns (2) through (4) of Table 2). In fact, as long as age is included as a covariate the exclusion of any other covariate has little impact on the PEs. Moreover, the estimated effect is presumably not the result of the underlying economic conditions, since including the borough and wave dummy and the interaction term should control for any macroeconomic shocks that may have occurred during the study period. Finally, the coefficient on male is 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.

³² Unfortunately, NYC did not collect job retention data, and New York State has been unwilling to provide the unemployment insurance wage records for study participants. As a proxy, the effect of the ESPP on the probability that recipients started a job and permanently exited welfare is used. For the purpose of this study, a recipient permanently exited welfare if they exited welfare and did not return within two years of his or her inclusion in the program or control group. Only two years of follow-up data was provided for each recipient.

The ESPP PEs decrease, on average, by less than 0.003, or less than one standard error, when all covariates are excluded from the regression (see panel B of Figure 3). The negative bias in the unadjusted PE was expected since the probability of being selected was higher for recipients who were older, and who had been on welfare longer, and such recipients were presumably less likely to have started a job. 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 (for example, see columns (6) and (7) of Table 2). Again, including the borough and wave dummy and the interaction term should control for any macroeconomic shocks that may have occurred during the study period. Thus, the estimated effect is presumably not the result of the underlying economic conditions. Moreover, prematurely terminating the study period on September 11th, 2001 does not materially affect the findings³³.

B. Selection for the WEP increases probability of participating in the WEP

It is also important to demonstrate that being selected for the WEP increased the probability that a recipient received a WEP assignment. Equation (2) is estimated with y_i^M redefined as an outcome dummy that equals one if individual i received a WEP assignment within M months of his or her inclusion in the program or control group and zero otherwise. The PE peaks at 0.199 ($t = 14.57$, $p = 0$) when $M = 1$, indicating that selectees are 19.9 percentage points more likely to have received a WEP assignment one month after the selection date than are members of the control group (see Figure 4). The coefficient on age is negative and significantly different than zero for all values of M , indicating that older recipients were less likely to have received a WEP assignment than were younger recipients. The coefficient on male is not significantly different than zero for any value of M . Thus, although age impacts the

³³ After September 11, 2001, the unemployment rate in NYC increased by over two percentage points.

likelihood of both exiting welfare and receiving a WEP assignment, gender only impacts the likelihood of exiting welfare.

Finally, recall that recipients' end-of-month case status was not collected if they received a WEP assignment. This resulted in the conservative assumption that all such recipients remained on welfare thereafter. To obtain an estimate 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 percent of the recipients who received WEP assignments exited welfare seven months post selection. Thus, the PEs reported above are potentially negatively biased by as much as 10 percentage points, since selectees were approximately 20 percentage points more likely to have received a WEP assignment than were members of the control group.

C. Adjusting for control group contamination in the ESPP data

To adjust for extensive control group contamination, additional control groups are created by removing recipients from the original control group if they were selected within a given number of months of their inclusion in the control group³⁴. 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, are used³⁵.

Estimating equation (2) with the restricted control groups, 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 (see panel A of Figure 5). As expected, the peak PE increases as the length of the restriction increases.

Additionally, the PEs using control group six are superimposed over the PEs using the control

³⁴ This process creates a selection problem and weights are employed to ameliorate it. See Ifcher (2007) for a comprehensive discussion of the problem and resolution.

³⁵ This approach cannot be extended further, since few control group members were not selected within six months of their inclusion therein if they remained eligible.

group three for the first three months post selection. 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 probably diverges from, and lies above, these PEs.

D. WEP robustness checks

To confirm the robustness of the findings, the effect of the WEP is estimated using two additional comparison groups. First, recall that some selectees were placed in the control group because their selection occurred between the end-of-month snapshots and was not observed. This limitation should negatively bias the estimated PEs. To confirm that this is the case and to estimate the magnitude of this bias, an additional control group, denoted ‘robust control group I,’ is formed that only includes recipients whose case status did not change between the month in which they were placed in the control group and the prior month. Estimating equation (2) with robust control group I, the PE peaks at 0.124 ($t = 8.67$, $p = 0$) when $M = 2$, indicating that selectees are 12.4 percentage points more likely to have exited welfare than are members of this control group (see panel A of Figure 6). This peak PE is over three percentage points higher than the peak PE using the original control group. The difference in the peak PEs should place an upper bound on the negative bias introduced by the absence of intra-month case status observations.

Second, one might be concerned that including covariates in the analysis does not adequately adjust for the selection process, which was based on observable characteristics. To address this concern, an additional control group, denoted ‘robust control group II,’ is formed

which only includes recipients who were ultimately selected. Specifically, all recipients who were not selected by the end of the study period were removed from the original control group. Since each member of robust control group II was ultimately selected, this control group should be a valid comparison group for the program group. The drawback of this control group is that a recipient had to be on welfare in a given month to be eligible in that month. That is, a recipient who exited and remained off welfare prior to being selected was excluded from this control group. As a result, the estimated PE will be positively biased. Estimating equation (2) with robust control group II, the PE peaks at 0.177 ($t = 12.86$, $p = 0$) when $M = 2$, indicating that selectees are 17.7 percentage points more likely to have exited welfare than are members of this control group (see panel B of Figure 6). This peak PE is more than double the peak PE using the original control group and should be considered an upper bound on the effect of the WEP.

E. ESPP robustness check

Finally, one might be concerned that ‘starting a job’ is better observed for members of the program group than for members of the control group³⁶. This is unlikely for two reasons. First, over 70 percent 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 (2) 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 his or her inclusion in the program or control group and zero otherwise. The PE peaks at 0.038 ($t = 6.29$ p

³⁶ 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.

= 0) when $M = 13$ using the original control group and at 0.073 ($t = 6.14$, $p = 0$) using control group six (see panel B of Figure 5)³⁷. That is, the ESPP increased the probability that recipients exited welfare regardless of whether or not they started a job. Thus, the observed employment effect appears to be robust.

VII. Discussion

The results appear to indicate that welfare-to-work programs have a similar effect on GA and FA recipients. The ESPP increased employment by at least eight percentage points, and possibly by as many as 13 percentage points, and decreased welfare use by at least four percentage points, and possibly by as many as seven percentage points. Similar 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 for FA recipients. The WEP decreased welfare use by at least 9 percentage points, and possibly by twice as much. Although there is no comparable unpaid work experience program that did not include classroom training or job search assistance, the effects estimated herein are also broadly similar to those for mandatory work programs for FA recipients.

This finding is interesting since one can develop plausible arguments for the effect being larger, or smaller, for GA recipients than for FA recipients. For example, while having to arrange childcare clearly introduces an additional barrier to employment for FA recipients, being a parent may increase one's motivation to act responsibly and find a job. Furthermore, GA recipients appear to be more disadvantaged than FA recipients on average. The GA recipients in

³⁷ 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.

this study were older and less likely to be white than were FA recipients in 1996 (Grogger and Karoly, 2005). Yet the effect of welfare-to-work programs appears to be similar for FA and GA recipients.

The WEP and ESPP also had a similar effect on men and women. The ESPP had a slightly larger effect on men than on women and the WEP had slightly larger effect on women than on men. This is in contrast to prior findings, discussed above, that welfare-to-work programs only have a significant impact on female FA recipients and not on male FA recipients. Again, little was known previously about welfare reform's effect on men, since few studies included male FA recipients and none focused on GA recipients.

The two welfare-to-work programs studied herein passed a rudimentary fiscal cost benefit analysis (for additional details see Ifcher, 2006 and 2007). Similar welfare-to-work programs for FA recipients typically pass a fiscal cost benefit analysis as well. These analyses, however, typically suffer from the two following limitations. First, there is often only a year or two of follow-up data regarding recipients' welfare use and employment, thus forcing researchers to make assumptions regarding recidivism rates and job tenure to complete their cost benefit analysis (Friedlander et al, 1997). Second, a fiscal cost benefit analysis does not assess the impact of the program on recipients' well-being.

Finally, this paper also contributes to the literature on welfare reform by identifying the impact of two welfare-to-work programs that were imposed on the same cohort of recipients over a five year period. The results indicate that new welfare-to-work programs can decrease welfare use and increase employment even when recipients have had long-term exposure to stringent welfare reforms. This suggests that states might be able to help recipients who have been unmoved by prior reforms by introducing new welfare reform programs.

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Table 1: Descriptive statistics for program and control groups

a) *The WEP*

Demographic characteristic	Program group (1)	Control group (2)
Observations	1,047	3,868
Male	59.0%	57.4%
Race		
Black	42.0%	44.1%
Hispanic	25.8%	24.2%
White	16.6%	15.2%
Other	1.1%	1.0%
Not reported	14.4%	15.5%
Borough of residence		
Bronx	13.9%	17.5%
Brooklyn	37.5%	35.6%
Manhattan	35.5%	34.0%
Queens	11.2%	11.6%
Staten Island	1.8%	1.3%
Age	36.7 (11.2)	39.4 (11.69)

b) *The ESPP*

Demographic characteristic	Program group (3)	Control group (4)
Observations	6,782	58,051
Male	53.9%	54.1%
Race		
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%
Borough of residence		
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%
Age	48.2 (8.315)	47.2 (8.727)
Years continuously on welfare	3.7 (3.08)	3.3 (3.1)

Standard errors are given in parenthesis.

Table 2: Coefficients from Estimating Equations (2)

Demographic characteristic	The WEP				The ESPP		
	Two months post selection (peak effect)				Four months post selection (peak effect)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
PE	0.0984 ***	0.0794 ***	0.0797 ***	0.0823 ***	0.0743 ***	0.0763 ***	0.0749 ***
Age		-0.0048 ***	-0.0045 ***	-0.0043 ***		-0.0013 ***	-0.0013 ***
Years continuously on welfare		n/a	n/a	n/a		-0.0015 *	-0.0015 *
Male			0.0651 ***	0.0615 ***		0.0058	0.0052
Race							
Asian			n/a	n/a		-0.0045	-0.0163
Black			0.0178	0.0144		0.0088	0.0059
Hispanic			0.0016	0.0016		-0.0066	-0.0038
White			-0.0212	-0.0276		0.0000	-0.0094
Borough dummy	NO	NO	NO	YES	NO	NO	YES
Wave dummy	NO	NO	NO	YES	NO	NO	YES
Interaction term	NO	NO	NO	YES	NO	NO	YES

* signifies p < 0.10; ** signifies p < 0.05; *** signifies p < 0.01; and n/a - data was not available for the WEP

Figure 1

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

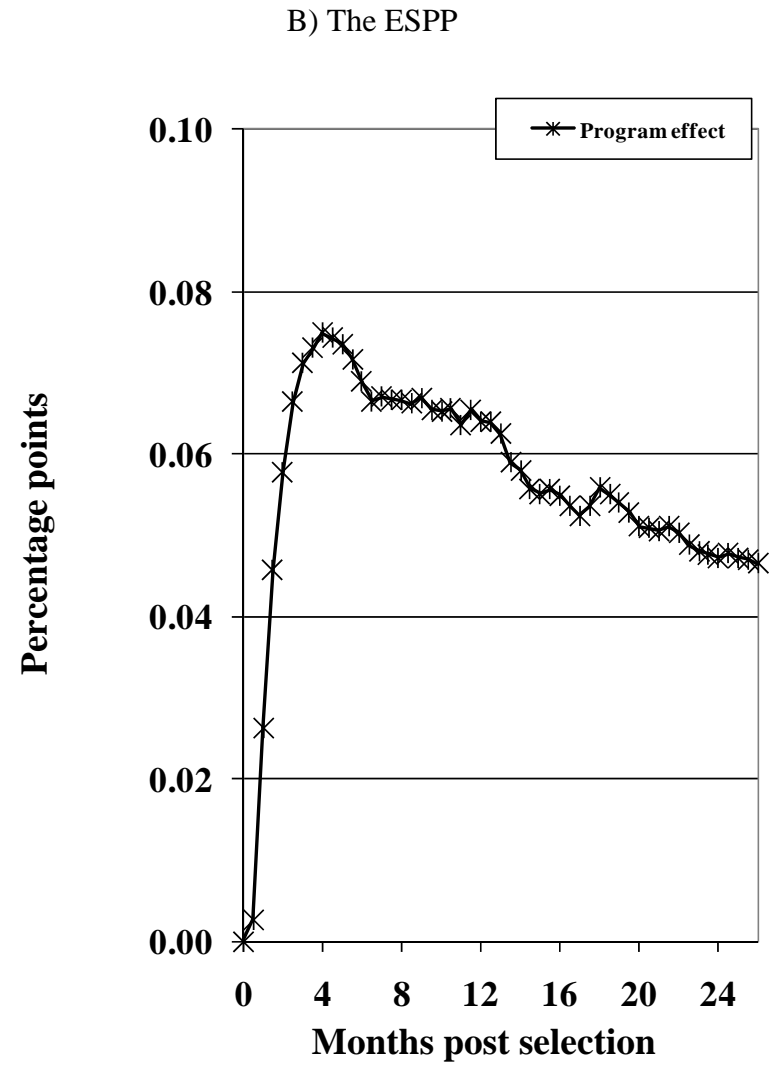
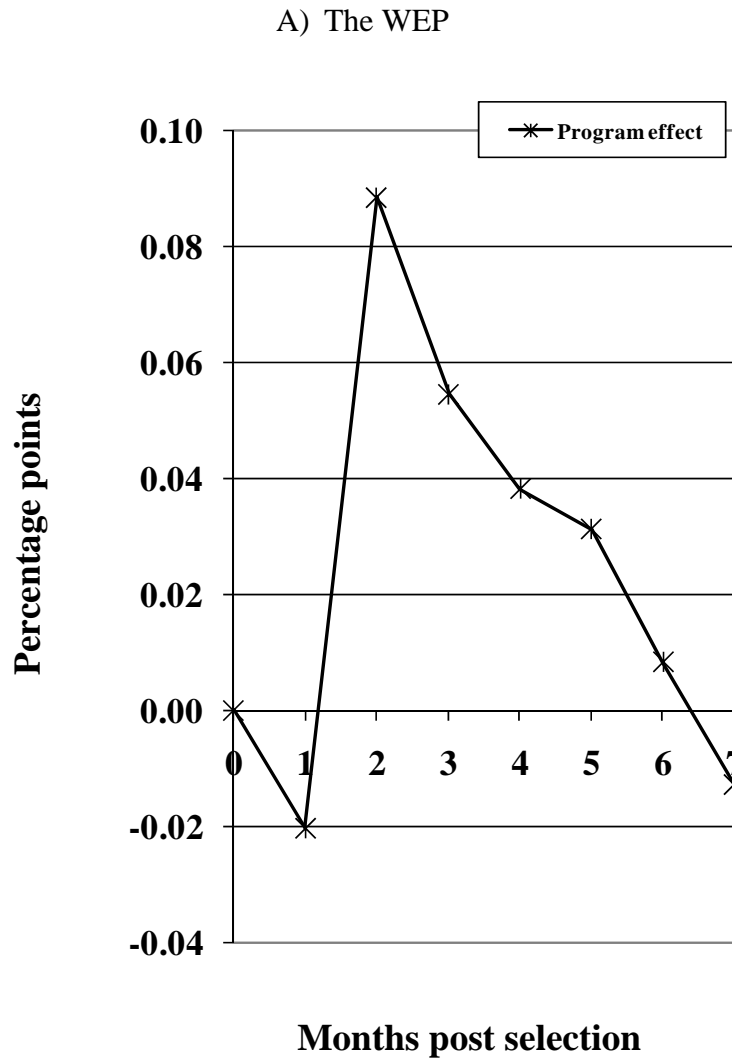


Figure 2

Estimated effect of WEP on exiting welfare for at least two months and ESPP on starting a job and permanently exiting welfare

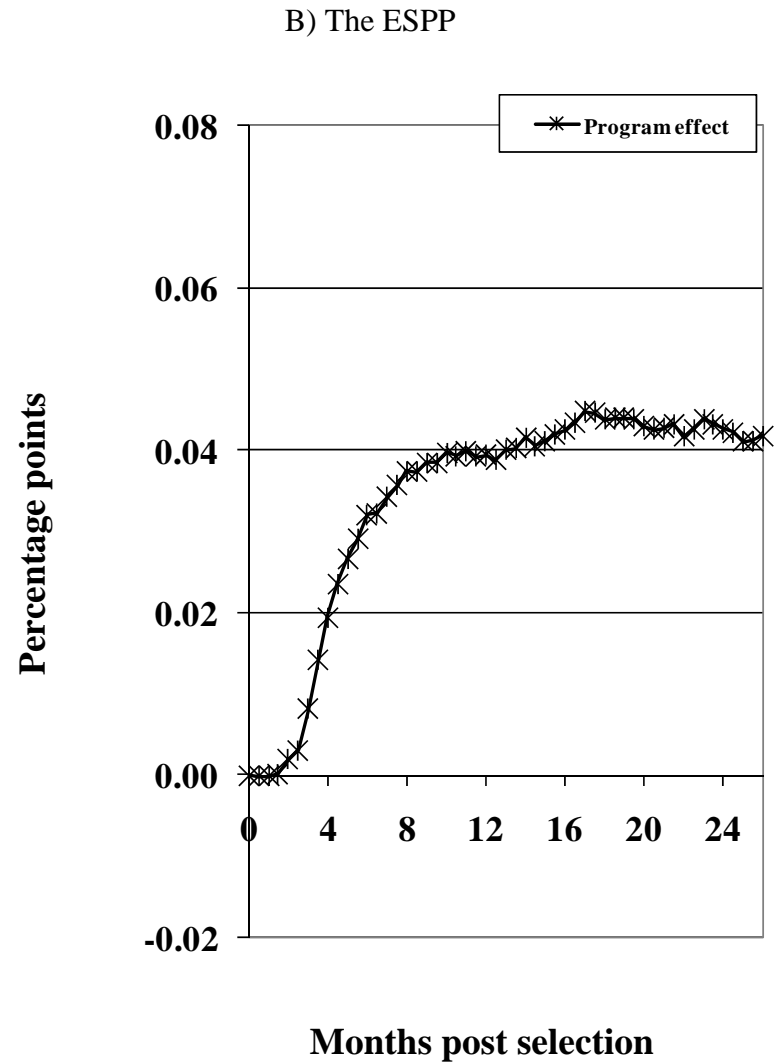
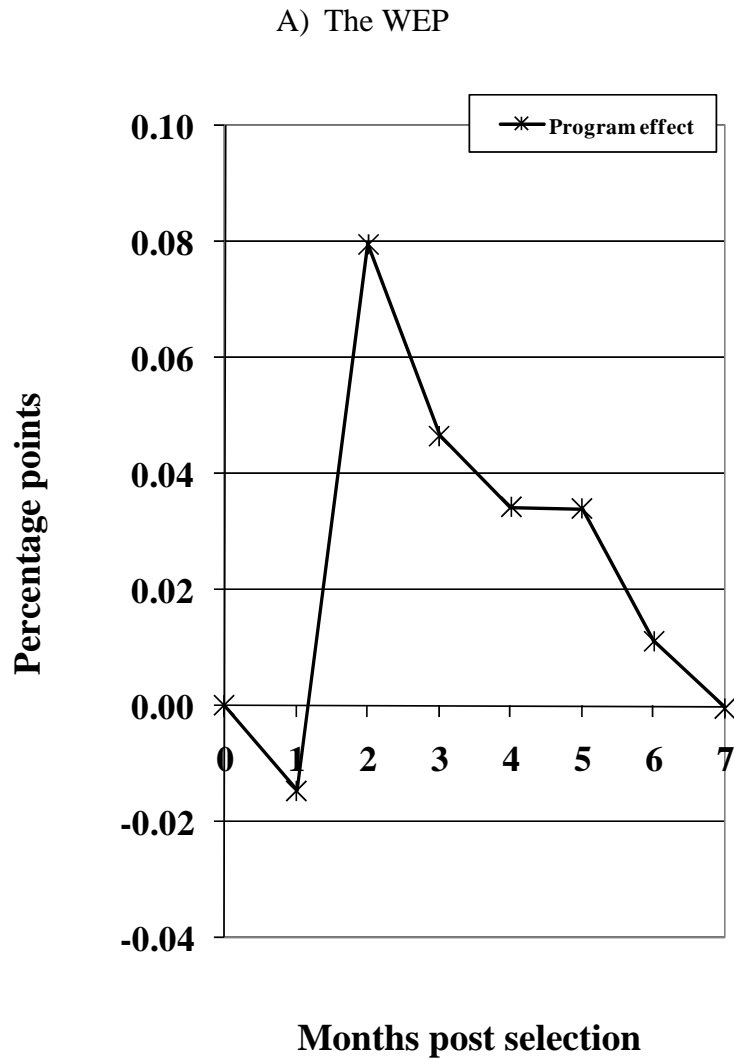
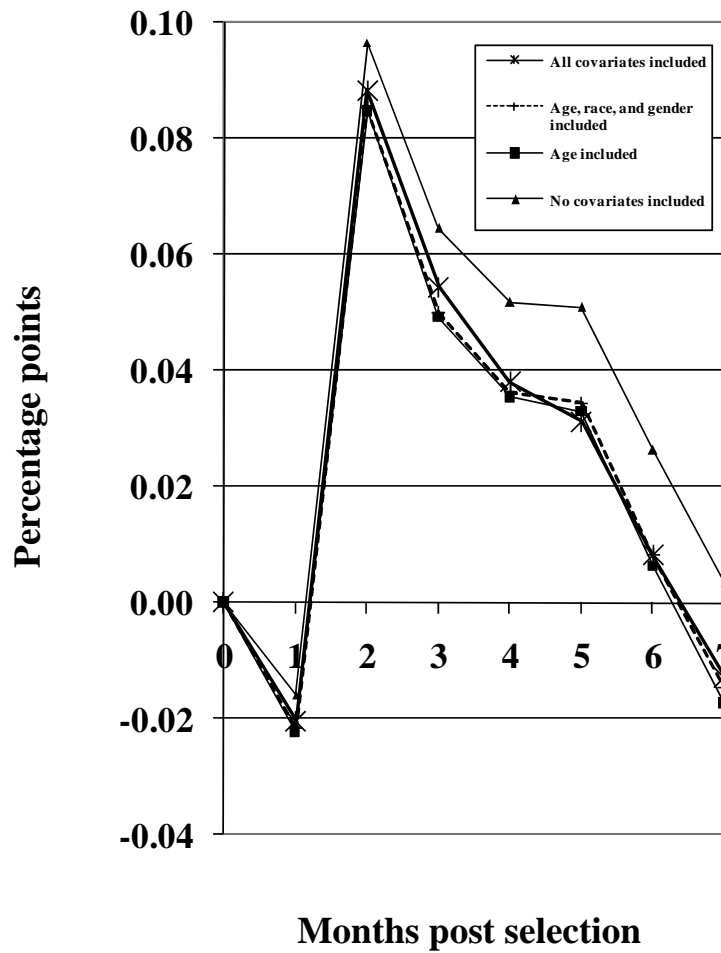


Figure 3

Estimated effect with various covariates included of WEP on exiting welfare and ESPP on starting a job

A) The WEP



B) The ESPP

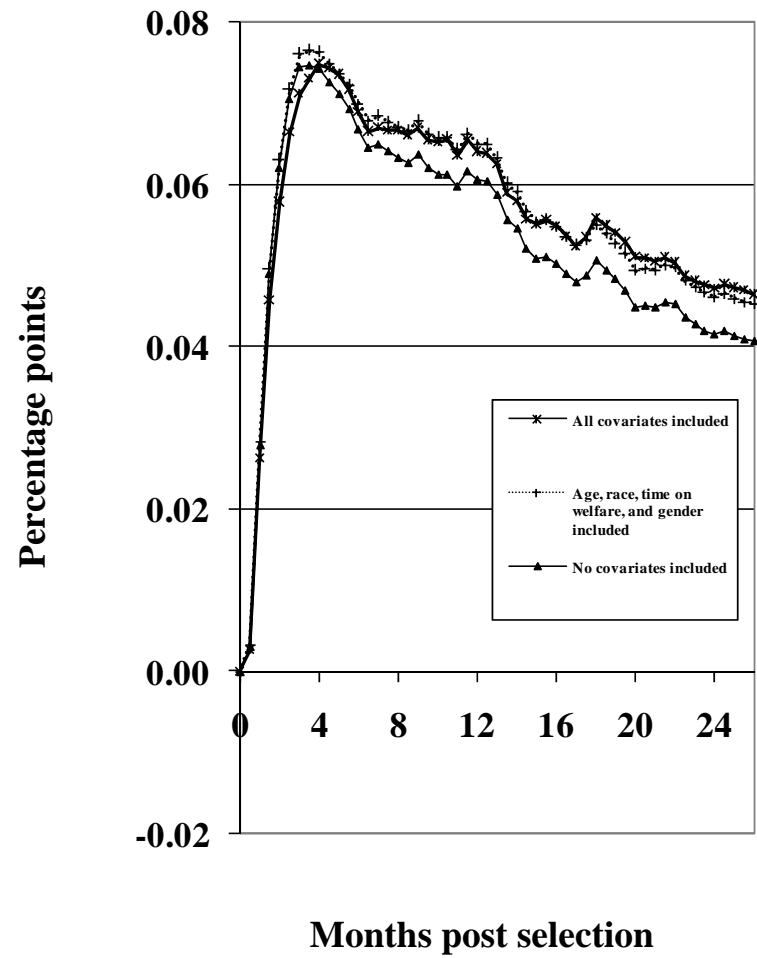


Figure 4

Estimated effect of WEP on participating in a WEP assignment

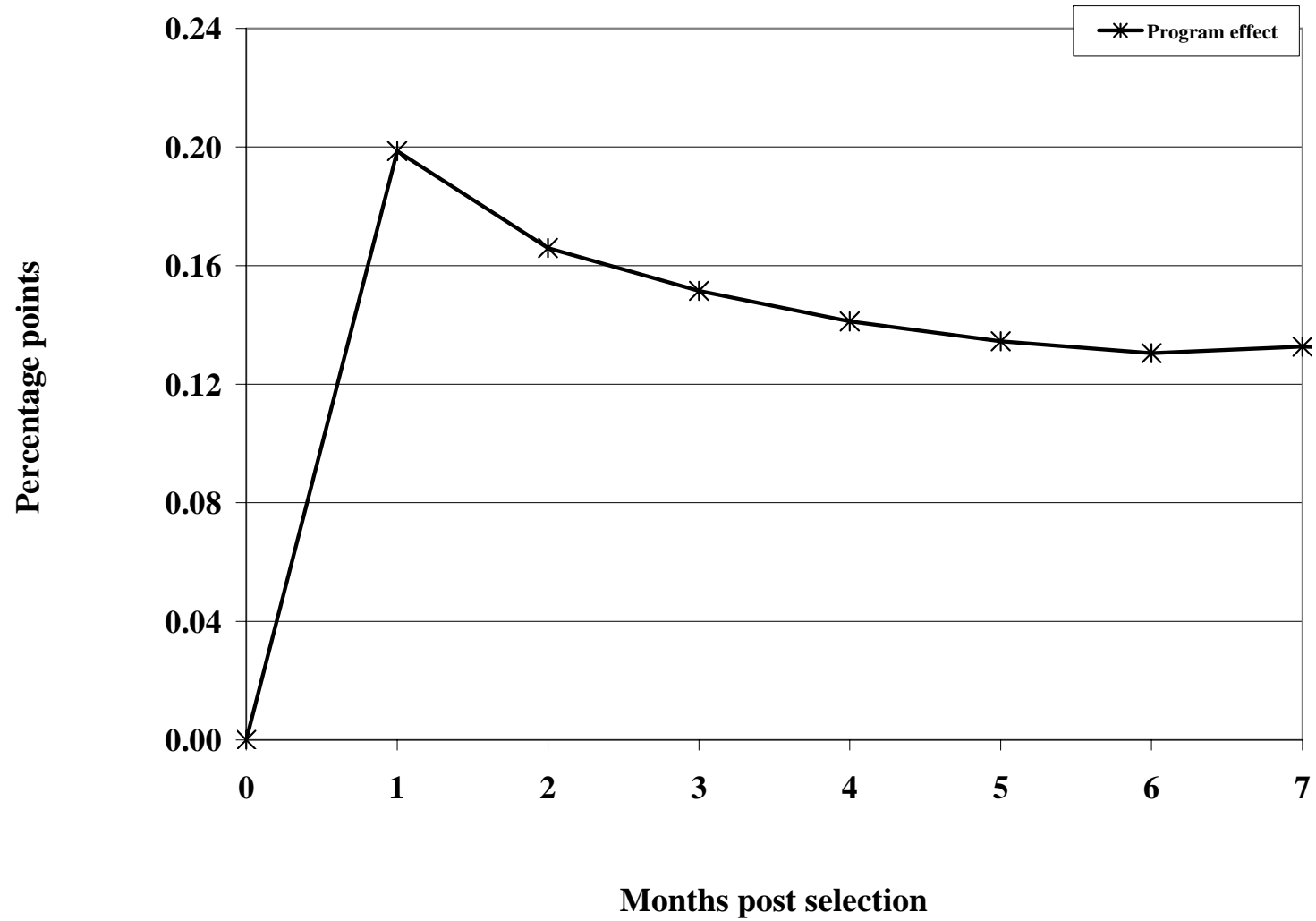
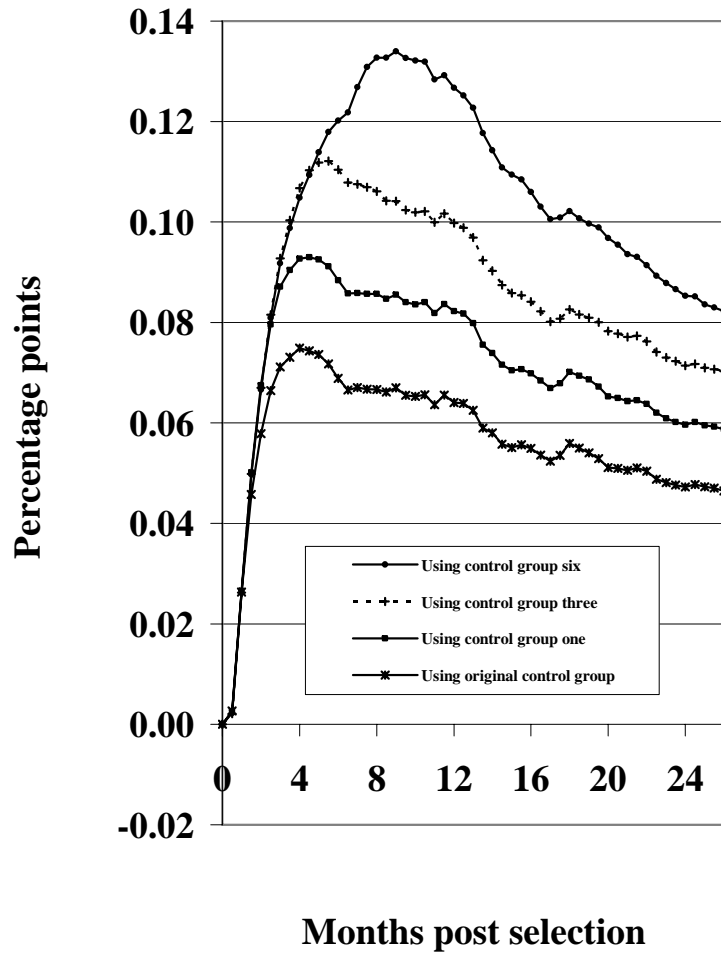


Figure 5

Estimated effect of ESPP on starting a job and on exiting welfare unconditionally using restricted control groups

A) Effect on starting a job



B) Effect on exiting welfare unconditionally

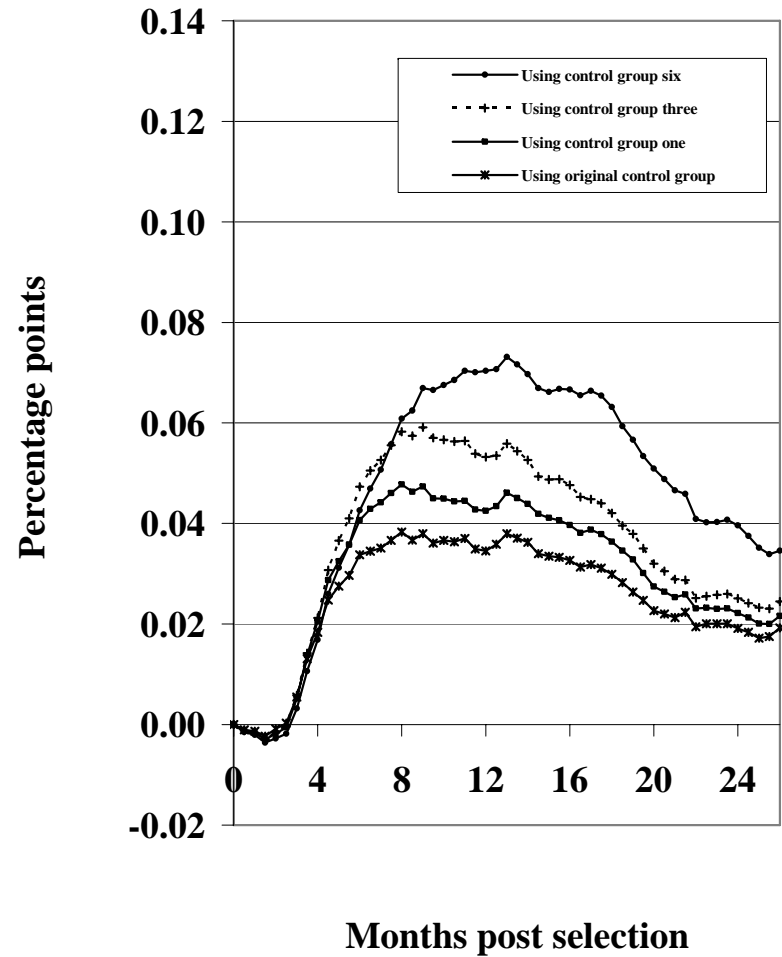
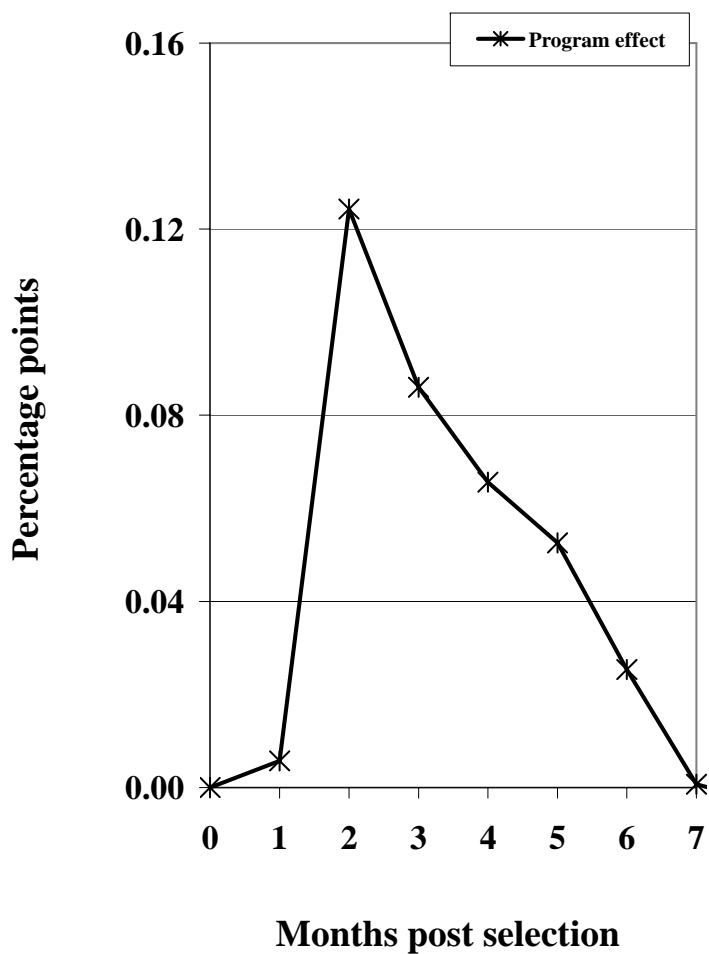


Figure 6

Estimated effect of WEP on exiting welfare using with robust control group I and II

B) Using robust control group I



B) Using robust control group II

