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General Assistance Recipients and Welfare-To-Work Programs: Evidence from New York City

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I. Introduction

Research regarding U.S. welfare programs has primarily studied Family Assistance (FA) programs. This focus overlooks an array of state-based, anti-poverty programs. Collectively known as General Assistance (GA), these programs provide cash assistance to needy individuals who are not covered by the federal safety net. At the height of the welfare reform movement, the caseload of these often unnoticed programs was not trivial. For example, there were approximately 750,000 GA cases in 1996. This represented approximately 15 percent of the combined GA and FA caseload.[1]

Yet, GA is essentially unstudied. Not a single paper investigates the effect of welfare reform on GA recipients. Further, the only research regarding GA programs that this author found was a periodic survey of GA programs in the U.S., which was last updated in 1999.[2] In contrast, the FA literature is extensive .[3]

Absent research regarding GA, one might simply assume that results from the FA literature apply to GA as well. After all, both programs support poor individuals who presumably have barriers to employment. However, GA and FA differ in important and fundamental ways. Most notably, eligibility for GA is generally not limited to those with dependent children. Thus, the typical GA and FA recipient are likely quite different. However, even this supposition cannot be easily confirmed, since national descriptive statistics regarding GA recipients are not readily available. In New York City (NYC), a slight majority of GA recipients were men and few have dependent children; in contrast, almost all FA recipients were women and all have dependent children. Given these

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[1] Uccello, C. E., H.R. McCallum, & L.J. Gallagher. State General Assistance Programs, Washington, D.C.: The Urban Institute, 1996. See also Office of Family Assistance. AFDC Basic Caseload. Washington, D.C.: Administration for Children & Family, U. S. Department of Health & Human Services, 1996.

[2] One study examining the impact of welfare reform includes both GA and FA recipients; however, due to the small sample size the authors were unable to identify the effect of welfare reform on GA and FA recipients separately (Chernick and Reimers, 2004). Further, the authors did not attempt to demonstrate causation and acknowledged that the observed reduction in welfare use—and increase in earnings—could have been caused by welfare reform, the robust economy, or another factor. See Gallagher, L. Jerome, Cori E. Uccello, Alicia B. Pierce, & Erin B. Reidy. State General Assistance Programs. Washington, D.C.: The Urban Institute, 1998.

[3] Blank, R. "Evaluating Welfare Reform in the United States." *Journal of Economic Literature,* 40(4), 2002, pp. 1105-1166. See also Grogger, J. & L. A. Karoly. "Welfare Reform: Effects of a Decade of Change." Cambridge, Massachusetts: Harvard University Press, 2005, and Moffitt, R. A. "Means-Tested Transfer Programs in the United States." Ed. Robert A. Moffitt, Chicago, IL: The University of Chicago Press, 2003.

differences, one can develop plausible arguments for why GA recipients would respond differently to welfare reform than would FA recipients. For example, while not having to arrange childcare clearly removes a barrier to employment, being a parent may increase one's motivation to work.[4] Thus, one should probably not assume that the impact of welfare reform on GA recipients will necessarily be similar to the impact on FA recipients.

The dearth of GA research is presumably the result of both methodological and practical concerns. For example, no random-assignment experiments have been conducted with GA recipients as they have been with FA recipients. Further, GA programs are substantially smaller and more decentralized than are FA programs. Thus, many research strategies that work when studying FA do not work when studying GA. Nevertheless, GA serves a large population of economically vulnerable individuals who should be of interest to researchers, especially in light of the recent recession and resulting high unemployment rate (9.7% in January 2010).

To begin to address this void, two welfare-to-work programs, in which GA recipients participated, are studied. Using a quasi-experimental approach, the effect of each program on welfare use and employment is estimated. The results indicate that both programs increased the likelihood of exiting welfare and that the second program also increased the likelihood of starting a job; no employment data were available for participants in the first program. The next section of this paper provides an overview of (a) GA programs in the U.S., and (b) the two welfare-to-work programs studied herein. The third section describes the empirical implementation. The fourth and fifth sections, respectively, present and discuss the results.

[4] A handful of FA studies included a limited number of male FA recipients. The results indicate that mandatory job training programs had no impact on male FA recipients; in contrast, these programs did significantly increase female FA recipients' earnings (Friedlander et al, 1997). The relevance of this finding to research regarding GA is unclear given that male FA recipients were generally in two parent households that were participating in the Aid to Families with Dependent Children Unemployed Parent program.

II. Background

A. GA programs in the U.S. in 1996

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.[5] GA programs are 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, other states restricted benefits to one or two targeted groups, for example, pregnant women or disabled adults.[6] 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 550,000, or about 75 percent of all GA cases.[7]

[5] The information contained in this section was derived from a comprehensive survey of GA program in the U.S. (Uccello et al, 1996).
[6] After passage of the Personal Responsibility and Work Opportunity Reconciliation Act of 1996, some states used their GA programs to close new gaps in the federal safety net. For example, New York City used their GA program to provide benefits to families that exhausted their FA benefits.
[7] The FA caseload was not evenly distributed among states either. The five states with the largest FA caseload had about 45% of all FA cases (authors calculation based on Office of Family Assistance, 1996). See Uccello, et al, 1996.

Perhaps not surprisingly, given the dynamic welfare policy environment during this period, states also reformed their GA programs: limiting eligibility, instituting time limits, and expanding work requirements.

B. Welfare reform in New York City

NYC implemented two welfare-to-work programs, the Work Experience Program (WEP) and the Employment Services and Placement Program (ESPP). Although these programs were fairly conventional in design, they are of special interest because GA recipients were required to participate in each program.

The WEP was implemented in 1995. All able-bodied GA recipients were required to participate in unpaid WEP assignments in exchange for their benefits. 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 participants worked outdoors, typically 21 hours per week, removing litter, weeds, and graffiti from parks, vacant lots, streets, and highways.

The ESPP was implemented in 1999. All job-ready GA recipients were required to participate in the ESPP two days a week and the WEP three days a week. This increased from 21 to 35 the number of hours per week that recipients were required to spend in a structured activity. Eleven private contractors were hired to provide the ESPP services. The contractors focused on developing participants' soft skills and helped participants arrange job interviews. The contractors were paid on a performance basis, receiving an average of $3,000 to place a recipient in a job.

Since there were too many eligible recipients to be accommodated simultaneously for each program, recipients were enrolled in waves. 'Selectees' were informed of their enrollment 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 requirements. New waves were formed every other week for the ESPP and every week for the WEP until each recipient had been selected or become ineligible.

Computer programmers selected recipients for each wave. The selection process did not include intake interviews or objective assessments. For the ESPP, the intention was to generate a random sample of selectees for each wave stratified by borough. For the WEP, the selection process was based on observable characteristics. The WEP selection criteria changed frequently. For example, one wave may have included a large number of recipients from Brooklyn and the next a large number from Manhattan. The criteria were not documented; neither the program managers, nor the computer programmers, kept a record of the selection criteria.

III. Empirical implementation

A. Estimating the effect of the ESPP and WEP

The effect of the ESPP and WEP is estimated taking advantage of the wave enrollment process. Specifically, all selectees from a given wave are compared to all 'non-selectees,' recipients who were eligible but not selected, for the same wave. Absent random-assignment, non-selectees from the same wave are the best available comparison group for selectees; this approach is similar to using a waiting list as the comparison group. By conducting this comparison over multiple waves and combining all selectees into one group, the program group, and all eligible non-selectees from the same wave into another group, the control group, one can estimate the Program Effect (PE). Specifically, the PE is defined as the difference between the percent of *selectees* and *non-selectees* who started a job (or exited welfare). The PE is then calculated at various points in time, for example, one and two months after wave formation.[8]

Three important consequences of this estimation procedure are as follows: First, ***there is no control group attrition***. All non-selectees from each wave remain a non-selectee for that wave for the entire study. For example, a non-selectee from the first wave who was selected for the second wave remains a non-selectee for the first wave. Second, as the above example illustrates, ***there is control group contamination***. Specifically, the number of non-selectees who are subsequently selected increases over

[8] Mathematically, the PE is defined as, $E[Y_i^M(S_i=1)]-E[Y_i^M(S_i=0)]$, where $Y_i^M(S_i)$ is an indicator function which equals one if individual i started a job (or exited welfare) within M months of wave formation and zero otherwise; and is a function of whether individual i was a selectee, $S_i = 1$, or non-selectee, $S_i = 0$.

time. As control group contamination increases, the PE becomes more negatively biased. Ultimately, about 70 and 40 percent of ESPP and WEP non-selectees were selected, respectively. Thus, the PE is a conservative estimate of the unbiased program effect and is better suited to measure short-term rather than long-term effects. Third, ***the PE is an 'intent to treat' effect***.[9] Selectees are considered treated regardless of whether or not they participated in the assigned program. This includes selectees who failed to attend the program's orientation.[10]

B. Data

With NYC's permission, the data for this study were extracted from two administrative databases. For the ESPP, the case history and available demographic characteristics were compiled for each eligible GA recipient. For the WEP, a simple random sample of 3,595 recipients was drawn from all eligible GA recipients. A case history and the available demographic characteristics were compiled for each member of the random sample. It should be noted that traditionally NYC only collected basic demographic information, such as race, gender, and date of birth, for each welfare recipient. No additional information is available.

The following four WEP data collection issues warrant mention: First, a recipient's case status was not collected after they received a WEP assignment due to a programming error. It is conservatively assumed that such recipients remain on welfare for the remainder of the study. This negatively biases the PE since selectees were significantly more likely to have received a WEP assignment than were non-selectees. Second, due to the dated design of the administrative database each recipient's case status was only observable at the end of the month.[11] Consequently, if a recipient's status changed more than once between end-of-month 'snapshots,' then only the last case status was observed. The most important impact is that recipients who were selected for the

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[9] 'Intent to treat' effects do not suffer from self-selection bias. In contrast, estimating the effect of participating in the program, the 'treatment on the treated' effect, would suffer from self-selection bias. For example, recipients could self-select out of the program by claiming a hardship, failing to comply with program requirements, or exiting welfare. For further discussion of intent to treat and treatment on the treated effects see Katz, Kling, and Liebman (2001).

[10] A separate paper that is focused on methodology discusses the estimation procedure in greater detail.

[11] The data were collected from a legacy 'point-in-time' database that did not store case histories. NYC did archive end-of-month 'snapshots' though.

WEP between snapshots were not observed to be selected. This also negatively biases the PE since some recipients who were coded as non-selectees were actually selectees. Third, it was only possible to compile a thirteen month case history (from February 1995 to February 1996) for each recipient in the random sample.[12] Fourth, NYC did not collect employment data for welfare recipients in the mid-1990s, when the WEP was implemented, and New York State has been unwilling to provide unemployment insurance wage records for the recipients. Thus, the results regarding the WEP are less precise and complete than one would like. They still merit reporting, however, given the lack of prior research regarding GA, and are presented after the ESPP results throughout the paper.

C. Descriptive statistics

Of all ESPP selectees and non-selectees from the first 17 waves, just over half are men and about 90 percent are nonwhite. Their average age is 47 and they are likely to live in the Bronx, Brooklyn, or Manhattan (see Table 1a). Comparing selectees to non-selectees, one observes that the average age, years (continuously) on welfare, and gender and racial distributions appear similar. However, a difference of means test reveals that only the gender distribution is not significantly different for selectees and non-selectees. Finally, the distribution of borough of residence is disparate. This is presumably the result of stratifying the selection by borough.

Of all the WEP selectees and non-selectees from five consecutive waves in early 1995, just over half are men and about 85 percent are nonwhite. Their average age is 38 and they are likely to live in Brooklyn or Manhattan (see Table 1b). Comparing selectees to non-selectees, the most evident difference is that selectees are three years younger, on average, than are non-selectees. A difference of means test reveals that age is the only demographic characteristic for which there is a significant different between selectees and non-selectees.

Comparing the ESPP and WEP selectees and non-selectees, it is striking that the former are over seven years older, on average, than are the latter. This 'aging' of

[12] The data had to be printed and transcribed into an analytic database; They could not be compiled electronically. Further, the data had been archived and had to be retrieved from long-term storage in Albany, NY.

selectees and non-selectees occurred in just five years, from 1995 to 2000, and suggests that younger GA recipients, who presumably had better alternative opportunities, were more likely to exit welfare than were older GA recipients after the WEP was implemented. Additionally, the percentage of black and Hispanic recipients increased while the percentage of white recipients decreased.

D. Selection process was not approximately random

Given the results of the difference of means tests discussed above, it is unlikely that the selection process for ESPP approximated a random one. A probit is estimated and confirms this presumption.[13] A similar test of the WEP selection process finds that it did not approximate a random one either. For the ESPP, older recipients and recipients who had been on welfare longer were more likely to be selected. For the WEP, younger recipients were more likely to be selected.

One thing is certain; eligible recipients were selected for the ESPP and WEP solely using information that was stored in the administrative databases. Again, the selection process was 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.

E. Multivariate regression analysis to adjust for demographic 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 above (in section III.A) is potentially biased. To adjust for the observed differences between the demographic characteristics of selectees and non-selectee, multivariate regression

[13] 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 the borough lists and assumed that this would generate a random sample stratified by borough.

analysis is used. Specifically, selectee status is regressed on the outcome measure—starting a job or exiting welfare—controlling for observable demographic characteristics—age, race, and gender. Ordinary least squares is used and corrected standard errors are calculated by clustering the observations by individual (see Appendix A for additional details regarding the design of the regression analysis).

IV. Results

A. The effect of the ESPP on employment

The ESPP appears increase the likelihood that recipients started a job by between 7.5 and 13.3 percentage points. This estimate is established using the techniques described in the prior section and is robust to numerous specification checks. What follows is a detailed discussion of the steps that were taken to (a) develop this estimate and (b) demonstrate its robustness.

Estimating the PE using multivariate regression analysis, one finds that the PE first increases, and then after peaking, decreases in M, where M is the number of months after wave formation. Figure 1 presents the PE graphically. The observed pattern—first increasing and then decreasing as M increases—was expected for the following two reasons: first, the employment effect of the ESPP should increase over time; and second, as M increases, so does control group contamination, which negatively biases the estimated PE. The peak PE is positive and significantly different than zero, equaling 0.075 when M equals four. This indicates that selectees were 7.5 percentage points more likely, on average, to have started a job than were non-selectees four months after wave formation (unless explicitly stated otherwise all PE are estimated with waves combined and controls for demographic characteristics).

To partially correct for control group contamination, non-selectees are removed from the control group if they were selected for the ESPP within a prescribed period after wave formation. Control groups with a one-, three-, and six-month restriction on non-selectees being selected are used.[14] Estimating the PE with these restricted control groups, one finds that the peak PE increases in magnitude as the length of the restriction

[14] This process creates a selection problem and weights are employed to ameliorate it. See Appendix B for additional details regarding this issue.

increases. The peak PE equals 0.133 when *M* equals nine using the control group with the longest restriction on being selected. This indicates that selectees were 13.3 percentage points more likely to have started a job than were non-selectees nine months after wave formation (see Figure 2). Thus, the employment effect is estimated to be between 7.5 percentage points—the initial estimate—and 13.3 percentage points—the estimate with the most restricted control group.

Four additional analyses illustrate the robustness of the result and warrant reporting. First, the PE is estimated for each of the first 17 waves separately. The peak PE is positive and significantly different than zero for each wave. Thus, it appears that the ESPP consistently increased the likelihood that recipients started a job. Second, the PE decreases slightly when estimated without controls for demographic characteristics. Thus, the unadjusted PE is slightly negatively biased as expected. Recall that the probability of being selected for the ESPP was higher for recipients who were older—and who had been on welfare longer—and it was assumed that such recipients would be less likely to start a job. The coefficient on age—and years on welfare— provide further support for this assumption; each is negative and significantly different than zero. Third, the PE is unaffected when estimated with controls for borough and wave; these controls should help adjust for local and temporal economic shocks. Further, prematurely terminating the study period on September 11th, 2001—after which the unemployment rate in NYC increased by over two percentage points—does not materially affect the findings. Thus, the results do not appear to be caused by underlying economic conditions. Fourth, the ESPP also increases the likelihood that recipients started a job and *permanently* exited welfare.[15]

Finally, one might be concerned that *starting a job* is better observed for selectees than for non-selectees, since ESPP contractors were paid for each job placement (thus, contractors had a strong incentive to make sure that each placement was recorded). However, this is unlikely for the following two reasons: first, over 70 percent of non-selectees were later selected, and second, in 2000, NYC set the ambitious goal of

[15] For the purpose of this study, a recipient permanently exited welfare if he or she exited welfare and did not return within two years of wave formation; only two years of follow-up data were provided for each recipient. It would have been preferable to demonstrate that the ESPP increased the probability that recipients remained employed and off welfare. However, NYC did not collect job retention data, and New York State has been unwilling to provide the wage records for study participants.

annually placing 100,000 recipients in jobs (thus, the City had a strong incentive to record each job placement accurately). Nevertheless, in an attempt to rule out this possibility, the effect of the ESPP on welfare exits—regardless of whether or not the recipient started a job—is estimated. The ESPP significantly increased the likelihood that recipients exited welfare by between 3.8 and 7.3 percentage points.[16] In fact, if the ESPP did increase employment, then it should have increased welfare exits, as it did. In contrast, if the observed employment effect was the result of measurement error, then the ESPP would not necessarily have increased welfare exits.

B. The effect of the WEP on welfare use

The WEP appears to increase by at least 8.8 percentage points, the likelihood that recipients exited welfare. This estimate—like the ESPP estimate—is established using the techniques described in the section III and is robust to numerous specification checks. Given the similarity of the estimation procedure for the WEP and ESPP, the WEP results are presented more concisely.

Estimating the PE for the WEP, one finds the same pattern as for the ESPP. The PE first increases, and then after peaking, decreases in *M*. Figure 3 presents the PE graphically. The peak PE is positive and significantly different than zero, equaling 0.088 when *M* equals two. This indicates that selectees were 8.8 percentage points more likely, on average, to have exited welfare than were non-selectees two months after wave formation.

Four additional analyses illustrate the robustness of the result and warrant reporting. First, the PE decreases only slightly when estimated with the outcome measure redefined as *exiting* and *remaining off* welfare. Thus, the PE is not the result of recipients *churning* on and off welfare. Second, the PE increases slightly when estimated without controls for demographic characteristics. Thus, the unadjusted PE is slightly positively biased as expected. Recall that the probability of being selected for the WEP was higher for recipients who were younger, and it was assumed that such recipients

[16] Note that the estimated exit effect is smaller than the estimated employment effect. This difference may be explained by the following two factors: first, there were many paths, other than starting a job, off welfare, and second, selectees who started a job were slightly more likely to return to welfare than were non-selectees who started a job.

would be more likely to exit welfare. The coefficient on age provides further support for this assumption; it is negative and significantly different than zero. Third, the PE is unaffected when estimated with controls for borough and wave. Thus, the results do not appear to be caused by underlying economic conditions. Fourth, being selected for the WEP increased the likelihood that a recipient received a WEP assignment by 20 percentage points. Thus, treatment had the intended effect.

Finally, recall that there were a series of data collection issues for the WEP, and that conservative assumptions were made regarding the missing data (see the second paragraph of section III.B for details). For example, it was assumed that recipients who received a WEP assignment remained on welfare thereafter. Yet, selectees were 20 percentage points more likely to have received a WEP assignment than were non-selectees. Thus, if the probability of exiting welfare was the same for those who received a WEP assignment as it was for those who did not (~50 percent), then the PE may be negatively biased by as much as 10 percentage points. Further, control group contamination negatively biases the PE. The magnitude of this bias, however, cannot be estimated. There are only seven months of follow-up data for each recipient, which is insufficient to form restricted control group similar to those that were used in the ESPP analysis. In summary, although the magnitude of the bias cannot be accurately estimated, it is unambiguously negative and potentially large. Thus, the WEP is estimated to have increased the likelihood of exiting welfare by at least 8.8 percentage point, and likely by more than that.

V. Discussion

The ESPP and the WEP each had a seemingly desirable impact on GA recipients. Each significantly increased the likelihood that recipients exited welfare and the ESPP also significantly increased the likelihood that recipients started a job. The exact magnitude of each of these effects, however, is somewhat uncertain. This uncertainty is the result of control group contamination, which is a by-product of the estimation procedure, and in the case of the WEP, missing data. Yet, these findings represent the best available information regarding the impact of welfare-to-work programs on GA recipients.

Further, GA recipients may be even more disadvantaged than FA recipients. One cannot say definitively though, since national descriptive statistics are not available. What is known, however, is that GA recipients in this study were older and more likely to be nonwhite than were FA recipients in 1996.[17] Thus, these results, and GA research in general, are important since this population is understudied, sizable, and economically vulnerable.

Of the estimates presented in this paper, those for the ESPP are more precise and complete. They indicate that the ESPP increased the likelihood that recipients started a job and exited welfare by approximately thirteen and seven percentage points, respectively. Even if one were skeptical regarding the adjustment that is made to correct for control group contamination, the results still indicate that the likelihood of starting a job and exiting welfare increased by seven and four percentage points, respectively. Interestingly, these results are similar to those from the FA literature. Mandatory work programs with an emphasis on job placement, that is, programs similar to the ESPP, increased employment by an average of nine percentage points and decreased welfare use by an average of six percentage points.[18]

The estimates for the WEP are less precise and complete. They not only suffer from control group contamination, but also from missing data. To help ensure that the findings are convincing, conservative assumptions were made regarding the missing data. The results indicate that the WEP increased the likelihood that recipients exited welfare by over eight percentage points. The unbiased effect may be substantially larger.

Three additional findings warrant mention. First, the ESPP and WEP 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 interesting since few prior studies included male welfare recipients. Those that did found that mandatory job training programs significantly increased female FA recipients' earnings, but had no significant impact on male FA recipients' earnings.[19] In contrast, the ESPP and WEP each had a significant impact on men.

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[17] Grogger and Karoly, 2005.
[18] Ibid.
[19] Friedlander, D., D.H. Greenberg, & P. K. Robins. "Evaluating Government Training Programs for the Economically Disadvantaged." *Journal of Economic Literature*, 35(4), pp. 1809-1855.

Second, the two welfare-to-work programs were imposed on the same cohort of recipients. GA recipients who were eligible for the ESPP had been on welfare for an average of three years, and during that time, been exposed to the WEP. Yet, the ESPP still had the intended effect, decreasing welfare use and increasing employment. Thus, even though recipients had long-term exposure to a stringent ‘work first’ program, the ESPP still had an impact. This suggests that policy makers might be able to help recipients who were unmoved by prior reforms by introducing new welfare reform programs.

Third, the observed WEP effect is the result of being selected for the WEP and not the result of participating in a WEP assignment. Therefore, the observed effect cannot be explained by either of the following: (a) that recipients acquired new skills that were helpful in finding a job and exiting welfare, or (b) that the cost of being on welfare increased (since the number of hours per week that recipients were required to spend in structured activities jumped from zero to 21). A plausible twist on the latter explanation is that the anticipated cost of participating in a WEP assignment generated the observed effect. For this explanation to be credible non-selectees must have not anticipated being selected for the WEP. Lastly, it could have been the cost of attending the WEP orientation itself that was enough to cause some recipients to exit welfare. This last explanation implies that NYC could have engaged the recipients in any relatively insignificant activity and the effect would have been the same. Unfortunately, given the limited available data there is no way to determine which of the last two explanations is more likely. What is clear is that simply being selected for the WEP had a significant effect on GA recipients who had been previously unexposed to welfare reform policies.

An interesting implication of this last finding is that the specific design of a welfare-to-work program might not be the most salient factor in its efficacy. Rather, simply engaging recipients in activities might be more salient—especially for recipients who have been unexposed to welfare reform previously. Further, this may explain why it has been difficult to determine which welfare reform programs work best.

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Table 1: Descriptive statistics for all selectees and non-selectees

a) For the ESPP (first 17 waves combined)

Demographic characteristic	**Selectees (1)**	**Non-selectees (2)**
Observations	6,783	58,017
Male	53.93% (0.50)	54.08% (0.50)
Age	**48.19** (8.31)	**47.22** (8.73)
Years continuously on welfare	**3.71** (3.08)	**3.34** (3.10)
Race		
Asian	**0.91%** (0.10)	**1.24%** (0.11)
Black	**47.80%** (0.50)	**49.34%** (0.50)
Hispanic	**37.14%** (0.48)	**34.94%** (0.48)
White	**8.61%** (0.28)	**9.63%** (0.29)
Other	0.59% (0.08)	0.69% (0.08)
Not reported	**4.95%** (0.22)	**4.16%** (0.20)
Borough of residence		
Bronx	**30.83%** (0.46)	**39.88%** (0.49)
Brooklyn	**26.54%** (0.44)	**31.44%** (0.46)
Manhattan	**28.87%** (0.45)	**16.78%** (0.37)
Queens	**12.37%** (0.33)	**11.01%** (0.31)
Staten Island	**1.36%** (0.12)	**0.64%** (0.08)

b) For the WEP (five initial waves combined)

Demographic characteristic	**Selectees (3)**	**Non-selectees (4)**
Observations	1,047	3,868
Male	59.0% (0.49)	57.4% (0.49)
Age	**36.71** (11.2)	**39.41** (11.69)
Race		
Black	42.0% (0.49)	44.1% (0.50)
Hispanic	25.8% (0.44)	24.2% (0.43)
White	16.6% (0.37)	15.2% (0.36)
Other	1.1% (0.11)	1.0% (0.10)
Not reported	14.4% (0.35)	15.5% (0.36)
Borough of residence		
Bronx	13.9% (0.35)	17.5% (0.38)
Brooklyn	37.5% (0.48)	35.6% (0.48)
Manhattan	35.5% (0.48)	34.0% (0.47)
Queens	11.2% (0.32)	11.6% (0.32)
Staten Island	1.8% (0.13)	1.3% (0.11)

Standard errors are given in parenthesis. Bolded means are significantly different for selectees and non-selectees.

Figure 1: Estimated effect of the ESPP on starting a job

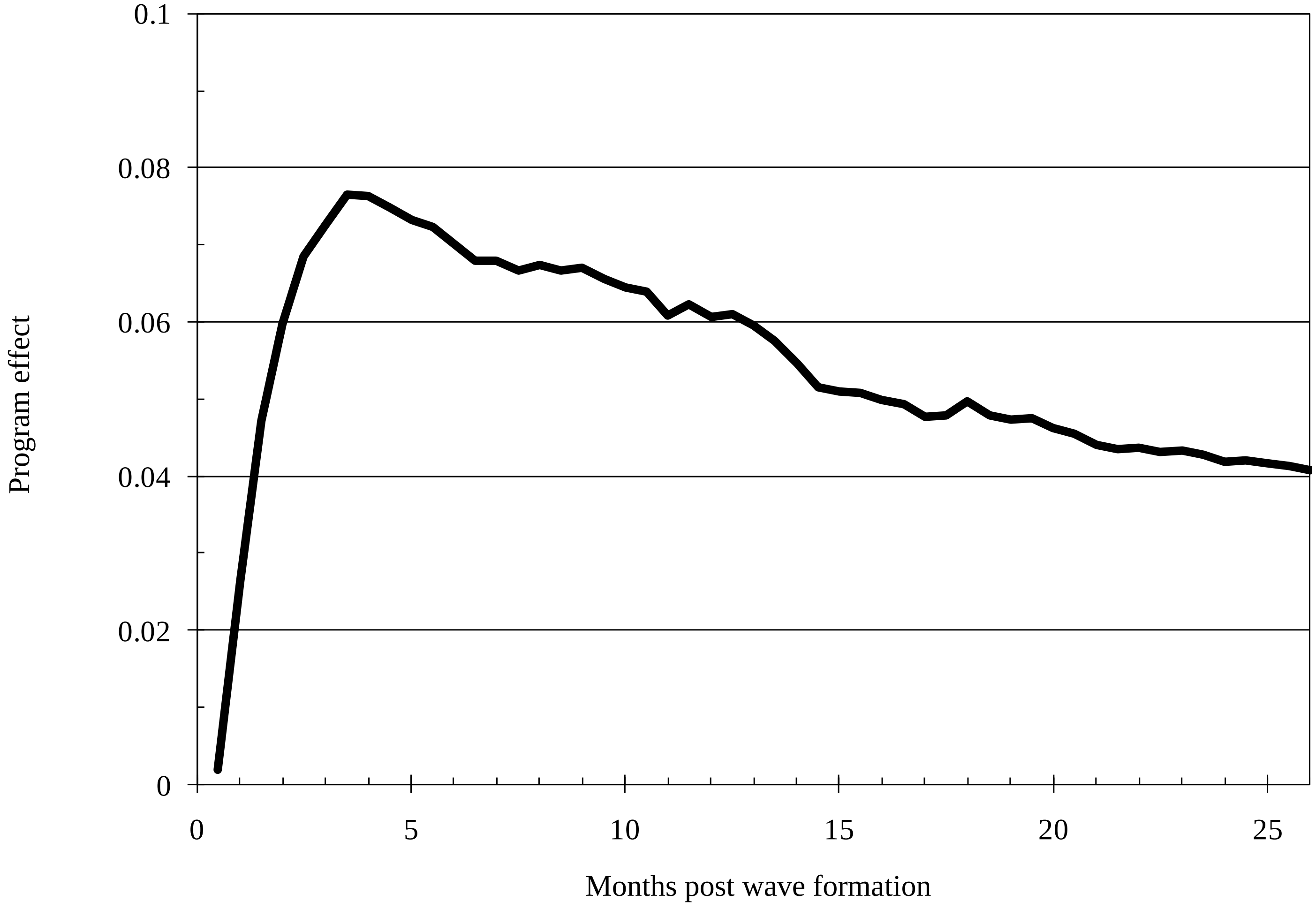


Figure 2: Estimated effect of the ESPP on starting a job using restricted control groups

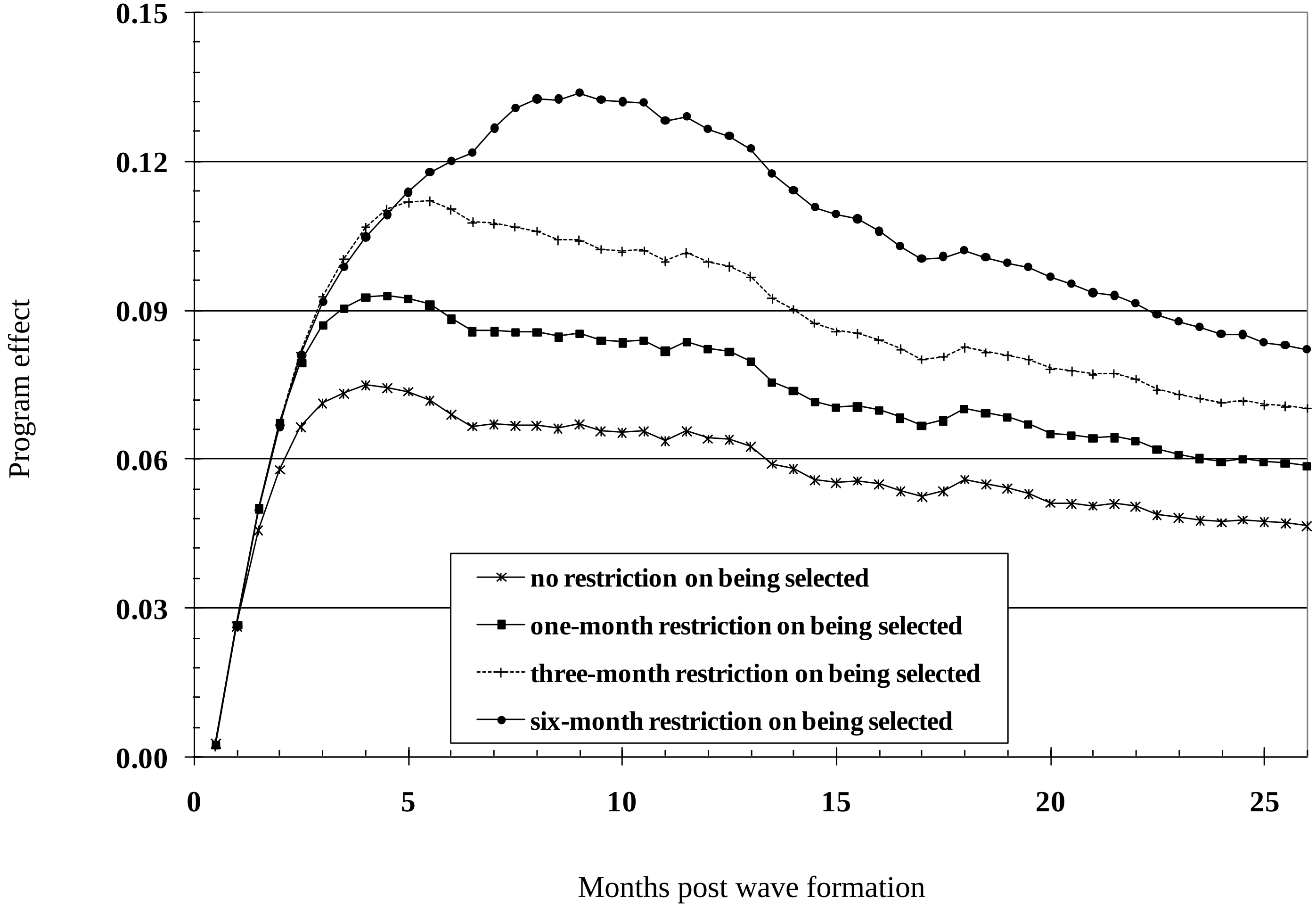
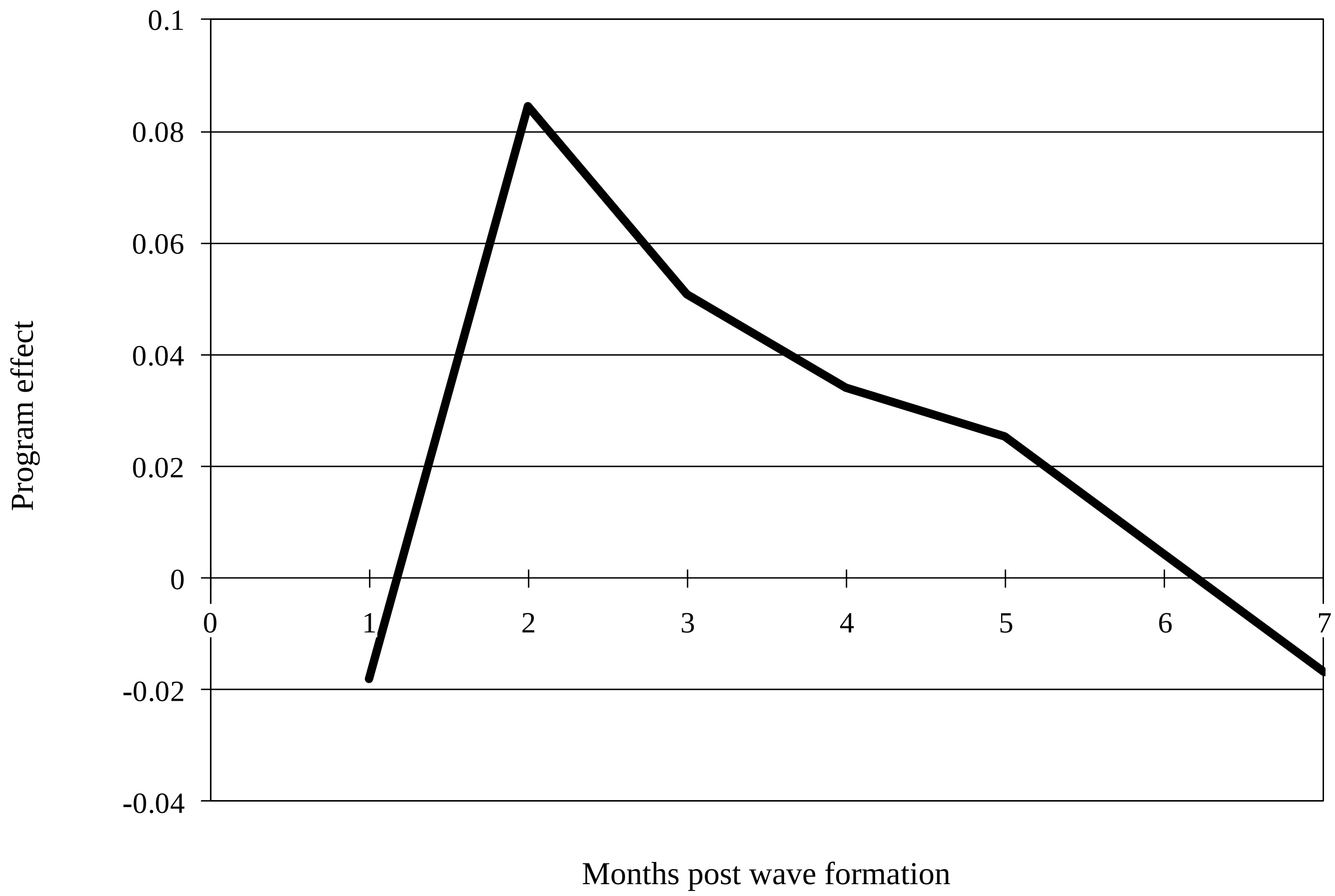


Figure 3: Estimated effect of the WEP on exiting welfare



Appendix A. Adjusting for differences between selectees and non-selectees

To adjust for the observed differences, a selectee dummy and a series of observed characteristics are regressed on an outcome dummy. That is, the following equation is estimated:

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

where y_i^M is an outcome dummy that equals one if individual *i* started a job, or exited welfare, within *M* months of wave formation and zero otherwise; S_i is a selectee dummy that equals one if individual *i* was a selectee and zero otherwise; 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* became a selectee or non-selectee in wave *k* and zero otherwise.

This equation is estimated for each wave separately, and for multiple waves combined, using OLS for values of *M* between 0.5 and 26 for the ESPP and between 1 and 7 for the WEP. This approach enables one to estimate a very general, non-parametric hazard rate. When this equation is estimated for all waves combined, corrected standard errors are calculated by clustering the observations by individual. This is necessary since some individuals are non-selectees in repeated waves. *M = 26* and 7 are the maximum number of months for which there is after wave formation data for the ESPP and WEP, respectively. Finally, for the ESPP each month *M* is assumed to have 28 days

Appendix 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 members of 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 the 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 in the restricted control group is increased. Specifically, the weight is the reciprocal of the probability that members of cohort one were not selected for the subsequent wave (see Figure A.1). The above procedure is utilized recursively to create control groups with up to a twelve-wave (six-month) restriction on being selected (assuming that each month contains two-waves, or twenty-eight days).

Figure A.1

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.