The Effect of a Welfare-To-Work Program on Homeless Shelter Use in New York City:

Evidence from a Quasi-Experiment

by

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JEL classification codes: I38, H72

Keywords: welfare-to-work programs, homelessness, general assistance, quasi-experiment, welfare reform

May 2008

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Abstract

While a primary intention of welfare reform was to move recipients from welfare to work, some feared that an unintended consequence would be increased homelessness. To explore whether this is the case, I estimate the impact of a welfare-to-work program on homeless shelter use. I take advantage of the fact that recipients were initially enrolled in the program in ‘waves’ due to program capacity constraints. Thus, I am able to identify the effect of the program using a quasi-experiment in which selectees are compared to eligible non-selectees. I find that the program modestly decreased, rather than increased, homeless shelter use. I also find that starting a job and exiting welfare are each associated with decreased shelter use. The dataset for this study was created by matching welfare case histories to the homeless shelter use records for New York City.

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, well-being, and family structure. The findings appear to indicate that, at least in the short run, welfare reform had the intended effect, reducing welfare use and increasing employment (recent reviews include Blank, 2002; Grogger and Karoly, 2005; and Moffitt, 2003).

Some policy makers and politicians worried that welfare reform would also have the unintended effect of increasing homelessness, if recipients were moved from welfare to work without adequate support (Berger and Tremblay, 1999). Some early anecdotal evidence appears to have supported these concerns (for example, Scherer, 1996, and Nichols & Gualt, 2003). However, no prior research has estimated the effect of welfare reform on homelessness.

In this study, I estimate the impact of a welfare-to-work program on homeless shelter use in New York City (NYC). To do so, I use a quasi-experimental identification strategy, in which recipients who were selected for the program are compared to recipients who were eligible but not selected. This is possible since not all eligible recipients could be enrolled simultaneously due to capacity constraints. The dataset for this study was compiled by matching welfare case histories from the NYC Human Resources Administration to the homeless shelter use records from the NYC Department of Homeless Services. The results appear to indicate that the program did not increase homeless shelter use, as some feared such programs would. Rather, it seems to have modestly decreased it. The results also indicate that recipients who started a job, and recipients who exited welfare, were both less likely to use a homeless shelter than were recipients who did not.

This research adds to the existing literature in two ways. First, it estimates the impact of welfare reform on a new outcome variable. No prior study has explored the effect of a welfare-to-work program on homeless shelter use. Moreover, recipients would presumably only ‘choose’ to use a homeless shelter if they had very poor housing alternatives. Thus, increased homeless shelter use would appear to foretell a reduction in one’s choice set, and thereby, reduced well-being. Conversely, decreased homeless shelter use should indicate improved well-being. Thus, this research appears to buttress previous research that indicates that welfare reform, at a minimum, did not negatively impact well-being and may have improved it (for example, Meyer and Sullivan, 2006).

Second, all recipients in this study received General Assistance (GA)[1] and not Family Assistance (FA). GA is essentially an unstudied program. There is only one paper that investigates the impact of welfare reform on GA recipients (Ifcher, 2007a). It estimates the effect of the same NYC welfare-to-work program on welfare use and employment. The results indicate that the program had the intended effect, reducing welfare use and increasing employment.

The next section of this paper provides some background information regarding welfare-to-work programs and homeless shelters in NYC. The third section describes the identification strategy, available data, and econometric methods that are used. The fourth section presents the results and the final section discusses them.

II. Background

[1] GA provides benefits to financially needy individuals who are not covered by the federal safety net.

A. Welfare-to-work programs in New York City

NYC implemented two welfare-to-work programs, the Work Experience Program (WEP) and the Employment Services and Placement Program (ESPP). This study investigates the impact of the ESPP on homeless shelter use. However, a brief description of the WEP is provided first, since ESPP participants were required to participate in the WEP concurrently.

The WEP was implemented in 1995. All able-bodied GA recipients were required to participate, working 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, thus, increasing 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 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 GA recipients to be accommodated simultaneously, recipients were enrolled in the ESPP 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 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. The intention was to generate a random sample of selectees for each wave stratified by borough.

B. Homeless shelter use in New York City

A 1981 consent decree, Callahan v. Carey (1981), established 'the right to shelter' in NYC. The City was (and still is) required to provide shelter to homeless individuals who request it. After the right to shelter was established, the average daily homeless shelter census quadrupled. In 2000, when the ESPP was implemented, there were over 100 city-funded homeless shelters with an average daily census of 23,712[2]. To use a shelter, a homeless individual had to visit one of a handful of intake centers that screened eligibility, managed shelter assignments, and tracked shelter use. As a result, NYC was able to maintain detailed homeless shelter use records.

III. Empirical implementation

A. Quasi-experimental identification strategy

I estimate the effect of the ESPP taking advantage of the wave enrollment process. Specifically, I compare all selectees from a given wave to all 'non-selectees,' recipients who were eligible but not selected, for the same wave. By combining all selectees and non-selectees from the first 17 waves I can estimate the Program Effect (PE),

$$E[Y_i^Q(S_i = 1)] - E[Y_i^Q(S_i = 0)] \qquad (1)$$

[2] Most of these shelters were managed by private organizations. Approximately 90% of all sheltered individuals in NYC were in city-funded shelters.

where $Y_i^Q(S_i)$ is the number of days individual i spent in a homeless shelter in the Q^{th} quarter post wave formation; and is a function of whether individual i was a selectee, $S_i = 1$, or non-selectee, $S_i = 0$.

Three important consequences of this identification strategy should be noted. First, selectees are considered treated regardless of whether or not they participated in the ESPP. This includes selectees who failed to attend the ESPP orientation. Hence, I am estimating an 'intent to treat' effect, which should not suffer from self-selection bias[3].

Second, the PE does not suffer from 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 a selectee in the second wave remains a non-selectee for the first wave for the remainder of the study.

Finally, as the prior example illustrates, the PE does suffer from control group contamination. Specifically, the number of non-selectees who were subsequently selected increases in Q; approximately 70 percent of all non-selectees were selected for a subsequent wave. As control group contamination increases, the PE becomes more biased against finding an effect. Thus, the PE is a conservative estimate of the true effect of the ESPP and is better suited to measuring short-term rather than long-term effects.

B. Data

With NYC's permission, the data for this study was extracted from two administrative databases. The case history and available demographic characteristics

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[3] 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).

were compiled from the NYC Human Resources Administration database. The homeless shelter use records for each recipient were compiled from the NYC Department of Homeless Services database. The records were considered a match if a recipient's (1) welfare case number, (2) social security number, or (3) last name and date of birth were the same in both databases. Each match was reviewed manually to confirm the soundness of the match. In 2000, the City only collected basic demographic information regarding each recipient, for example, race and gender. No additional source of information was available regarding recipients in the study.

C. Descriptive statistics

Of all selectees and non-selectees from the first 17 waves, just over half were men and about 90 percent were nonwhite. Their average age was 47 and they were likely to live in the Bronx, Brooklyn, or Manhattan. In the two years prior to wave formation, they had spent an average of six days in a homeless shelter. However, most recipients had not used a homeless shelter; only 3.7 percent had. This minority used homeless shelters extensively, spending an average 236 days in them in the prior two years (see column (1) of Table 1).

Comparing selectees to non-selectees, one observes that the average age, numbers of years continuously on welfare, and the gender and racial distributions appear similar (compare columns (2) and (3) of Table 1). However, a difference of means test reveals that only the gender distribution is not significantly different. In terms of homeless shelter use, a significantly lower percentage of selectees had a history of homeless shelter use than did non-selectees, 2.85 versus 3.74 percent. However, of those who had used a homeless shelter, the average number of days in one was not significantly different for the two groups. Finally, the distribution of borough of residence is disparate. This is presumably the result of stratifying the selection by borough.

D. Selection process was not approximately random

In Ifcher (2007b), I test whether the selection process approximated a random one and found that it did not[4]. One thing is certain though; eligible recipients were selected for the ESPP solely using information that was stored in the administrative databases. Again, the selection process was centralized and conducted by computer programmers. Individual caseworkers were not involved in any manner and no intake interviews or objective assessments were conducted prior to selection. In other words, the selection process was conducted without human discretion. Such a selection process, even if it did not approximate a random one, should not disturb the necessary assumption that there was no systematic selection on unobserved characteristics. Consequently, by including covariates in the analysis, one should be able to adjust for the observed differences.

E. Adjusting for observables

Since a recipient's homeless shelter use history or gender may affect his or her post ESPP homeless shelter use, the PE as defined in equation (1) is potentially biased. To adjust for the observed differences, a selectee dummy and a series of observed characteristics are regressed on recipients' quarterly homeless shelter use. Specifically, the following equation is estimated,

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

[4] 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 each borough's list and assumed that this would generate a random sample stratified by borough.

where y_i^Q is the number of days individual *i* spent in a homeless shelter in the Q^{th} quarter post wave formation; 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. Equation (2) is estimated using OLS for values of *Q* between 1 and 8[5]. Corrected standard errors are calculated by clustering the observations by individual; this is necessary since some individuals are non-selectees in repeated waves.

IV. Results

Estimating equation (2) one finds that, at first, the PE decreases in *Q*, and then after peaking, increases in *Q*, as control group contamination increases. The peak PE is -0.30 (t = -3.2, p = 0.002) when *Q* equals six, indicating that selectees spent 0.3 fewer days, on average, in a homeless shelter than did non-selectees in the sixth quarter post wave formation (see Figure 1).

To partially adjust for control group contamination, three restricted samples are created by removing non-selectees if they were selected within one, three, or six months of becoming a non-selectee[6]. Estimating equation (2) with these restricted samples, one finds that the peak PE increases in magnitude as the length of the restriction increases. The peak PE is -0.57 (t = -2.6, p = 0.009) using the sample with the longest restriction on

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[5] *Q* = 8 is the maximum number of quarters for which there is post selection data for each recipient. Each month is assumed to have 28 days, so each quarter is assumed to have 84 days.

[6] This process creates a selection problem and weights are employed to ameliorate it. See Ifcher (2007b) for a comprehensive discussion of the problem and resolution. This approach cannot be extended further, since few non-selectees were not selected within six months of becoming a non-selectee.

being selected when *Q* equals seven. This indicates that selectees spent 0.57 fewer days, on average, in a homeless shelter than did non-selectees in this restricted sample in the seventh quarter post wave formation (see Figure 2).

Estimating equation (2) without covariates, one finds that the PEs decrease by 0.04, or about one-half standard error, on average (see Figure 3). Thus the unadjusted PEs are negatively biased. This is not surprising since non-selectees were significantly more likely to have used a homeless shelter in the two years prior to wave formation, and such recipients were presumably more likely to use a homeless shelter after implementation of the ESPP as well. This presumption is supported by the fact that the coefficient on prior homeless shelter use is positive and significantly different than zero for all values of *Q* (see Table 2).

<u>A. Robustness check</u>

One might be concerned that including covariates in the analysis does not adequately control for the selection process, which was based on observable characteristics. Recall that, on average, selectees were less likely to have a history of homeless shelter use, were older, and had spent more time on welfare. To address this concern, an additional restricted sample was formed that only includes recipients who were ultimately selected for the ESPP. Specifically, all non-selectees who were not selected by the end of the study period were removed.

Estimating equation (2) with this additional restricted sample, one finds the same basic pattern.   The PE decreases in *Q*, and then after peaking, increases in *Q*, as control group contamination increases. The PE peaks at -0.19 ($t = -2.08$, $p = 0.037$) when *Q* equals six, indicating that selectees spent 0.19 fewer days, on average, in a homeless shelter than did non-selectees in this restricted sample in the sixth quarter post wave formation (see Figure 4).

Notably, the PEs are small and not significantly different than zero for values of *Q* less than four. Thus, these non-selectees appear to be comparable to the selectees in terms of homeless shelter use at the beginning of the study. Furthermore, the PE rapidly returns toward zero in *Q* after peaking. This is expected since control group contamination is complete with this restricted sample. This would appear to provide a strong check against the concern that the estimated PEs are the result of the selection process and not the effect of the ESPP.

B. The relationship between starting a job and homeless shelter use

Equation (2) was estimated with y_i^Q redefined as the number of days individual *i* spent in a homeless shelter in the Q^{th} quarter after (i) starting a job (if individual *i* started a job) or (ii) 40 weeks post wave formation (otherwise)[7]; S_i redefined as a 'started a job' dummy that equals one if individual *i* started a job during the study period and zero otherwise; and all other variables defined as before.

The results reveal the same basic pattern. The PE first decreases in *Q*, and then after peaking, increases in *Q*. The peak PE is -0.22 ($t = -1.31$, $p < 0.187$) when *Q* equals two, indicating that recipients who started a job spent 0.22 fewer days, on average, in a homeless shelter than did recipients who had not (see Figure 5). Restricting the sample to those non-selectees who were not subsequently selected for the ESPP within a given number of months, one again finds that the peak PE increases in magnitude as the length

[7] 40 weeks post wave formation is used because, on average, selectees and non-selectees started a job 40 weeks post wave formation (conditional on starting a job).

of the restriction increases. The peak PE is -0.51 (t = 2.00, p = 0.045) using the sample with the longest restriction on being selected when Q equals two.

The pattern of PEs is striking. Specifically, the PE is small when Q is negative, prior to starting a job. Then, when Q is positive, after recipients have started a job, the PE decreases in Q, peaks, and then increases in Q. Thus, it would seem to indicate that starting a job is associated with decreased homeless shelter use. It is important to note, however, that a causal link between starting a job and reduced homeless shelter use has not been established since those who started a job are not necessarily comparable to those who did not; the quasi-experimental design is not intended to ensure that this is the case.

C. The relationship between exiting welfare and homeless shelter use

Equation (2) was estimated with: y_i^Q redefined as the number of days individual i spent in a homeless shelter in the Q^{th} quarter after (i) exiting welfare (if individual i exited welfare) or (ii) 52 weeks post wave formation (otherwise)[8]; S_i redefined as an 'exit welfare' dummy that equals one if individual i exited and remained off welfare during the study period and zero otherwise; and all other variables defined as before.

The results reveal a slightly different basic pattern. After exiting welfare, the PE first decreases in Q and then is constant in Q. The peak PE is -0.69 (t = -4.39, p = 0) when Q equals four, indicating that recipients who exited welfare spent 0.69 fewer days, on average, in a homeless shelter than did recipients who had not (see Figure 6). Restricting the sample to those non-selectees who were not subsequently selected for the ESPP within a given number of months, one again finds that the peak PE increases in magnitude as the length of the restriction increases. The peak PE is -1.02 (t = 3.16, p =

[8] 52 weeks post wave formation is used because, on average, selectees and non-selectees exited welfare 52 weeks post wave formation (conditional on exiting welfare).

0.002) using the sample with the longest restriction on being selected when Q equals three.

Again, the pattern of PEs is striking. Specifically, the PEs are small when Q is negative, prior to exiting welfare. Then, when Q is positive, after recipients have exited welfare, the PE decreases in Q, peaks, and then is constant in Q. So it would seem to indicate that exiting welfare is associated with decreased homeless shelter use. It is again important to note that a causal link between exiting welfare and reduced homeless shelter use has not been demonstrated since those who exited welfare are not necessarily comparable to those who did not.

IX. Discussion

The ESPP appears to have decreased homeless shelter use, although the magnitude of the effect is clearly modest. The peak PE using the restricted sample indicates that the ESPP reduced homeless shelter use by up to 0.57 days per quarter. However, the magnitude of effect was presumably greater on ‘likely homeless shelter users.’ Only about five percent of recipients in the study used a homeless shelter in the two years prior to and post wave formation. For example, if ten percent of the recipients were likely homeless shelter users, then the ESPP would have reduced homeless shelter use by up to 5.7 days per quarter for this cohort. Regardless, the program clearly did not increase homeless shelter use; thus, the fear that such programs would did not materialize in this case.

It would have been interesting to study the impact of the ESPP on homelessness as well as on homeless shelter use. This is not possible since there is no data regarding recipients’ homeless episodes. However, NYC does conduct an annual count of unsheltered homeless individuals, the Homeless Outreach Population Estimate. The results indicate that well over half of the homeless, childless adults in NYC were in a homeless shelter on the night of the count. Assuming the probability of being in a homeless shelter is the same for a homeless welfare recipient as for a homeless individual, the estimates presented in this paper presumably measure a substantial portion of the ESPP's impact on homelessness.

Lastly, a limitation of this study is that only job-ready GA recipients are included. This raises two potential concerns. First, the PE may be biased for recipients who are not job-ready. However, 90% of the eligible population was found to be job-ready. Furthermore, a welfare-to-work program is presumably not an appropriate policy intervention for recipients who are not job-ready. Second, the PE is presumably biased for FA recipients. Clearly, this is a concern since the majority of welfare recipients nationwide receive FA and not GA. Thus, it would be useful to investigate the impact of welfare reform on the homeless shelter use of FA recipients.

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Table 1

Descriptive statistics for selectees and non-selectees

Variable	**All (1)**	**Selectees (2)**	**Non-selectees (3)**
In prior two years			
Percent used shelter	3.69% (0.19)	**2.85%** (0.17)	**3.74%** (0.19)
Days of shelter use	8.60 (58.08)	**6.64** (52.01)	**8.83** (58.75)
Days of shelter use (shelter users only)	236.04 (197.17)	233.24 (205.92)	236.29 (196.42)
In subsequent two years			
Percent used shelter	2.45% (0.15)	**1.84%** (0.13)	**2.52%** (0.16)
Days of shelter use	6.06 (51.50)	**4.13** (42.36)	**6.29** (52.46)
Days of shelter use (shelter users only)	247.71 (220.33)	**224.23** (219.98)	**249.72** (220.32)
Male	54.07% (0.50)	53.93% (0.50)	54.08% (0.50)
Age	47.32 (8.69)	**48.19** (8.31)	**47.22** (8.73)
Years on welfare (continuously)	3.37 (3.10)	**3.71** (3.08)	**3.34** (3.10)
Race			
Asian	1.21% (0.11)	**0.91%** (0.10)	**1.24%** (0.11)
Black	49.18% (0.50)	**47.80%** (0.50)	**49.34%** (0.50)
Hispanic	35.17% (0.48)	**37.14%** (0.48)	**34.94%** (0.48)
White	9.52% (0.29)	**8.61%** (0.28)	**9.63%** (0.29)
Not reported	4.24% (0.20)	**4.95%** (0.22)	**4.16%** (0.20)
Borough of residence			
Bronx	38.94% (0.49)	**30.83%** (0.46)	**39.88%** (0.49)
Brooklyn	30.93% (0.46)	**26.54%** (0.44)	**31.44%** (0.46)
Manhattan	18.05% (0.38)	**28.87%** (0.45)	**16.78%** (0.37)
Queens	11.15% (0.31)	**12.37%** (0.33)	**11.01%** (0.31)
Staten Island	0.71% (0.08)	**1.36%** (0.12)	**0.64%** (0.08)
Observations	64,800	6,783	58,017

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

Table 2

Coefficients from estimating equation (2)

Variable	Quarters post wave formation One	Two	Three	Four	Five	Six	Seven	Eight
PE	**-0.1566** (0.08)	-0.1038 (0.08)	**-0.1676** (0.08)	**-0.2072** (0.09)	**-0.2642** (0.09)	**-0.3027** (0.10)	**-0.2701** (0.09)	**-0.2028** (0.09)
Used shelter in prior two years	**17.4119** (1.93)	**15.0879** (1.83)	**13.2045** (1.74)	**11.6253** (1.67)	**10.9851** (1.65)	**9.9723** (1.58)	**9.0230** (1.56)	**8.9436** (1.57)
Age	0.0004 (0.01)	-0.0060 (0.01)	-0.0057 (0.01)	-0.0020 (0.01)	-0.0005 (0.01)	-0.0053 (0.01)	-0.0070 (0.01)	-0.0061 (0.01)
Years continuously on welfare	0.0136 (0.02)	0.0081 (0.02)	0.0074 (0.03)	-0.0015 (0.03)	-0.0110 (0.03)	-0.0168 (0.02)	-0.0089 (0.02)	-0.0044 (0.02)
Male	-0.1738 (0.15)	-0.0672 (0.15)	0.0173 (0.16)	0.0744 (0.16)	0.1424 (0.17)	0.2442 (0.16)	**0.3791** (0.15)	**0.4753** (0.14)
Race								
Asian	**-0.7232** (0.34)	-0.6075 (0.34)	-0.4686 (0.34)	-0.5087 (0.35)	-0.5043 (0.35)	-0.5962 (0.42)	-0.7582 (0.50)	-0.8335 (0.52)
Black	-0.6447 (0.35)	-0.5119 (0.36)	-0.3782 (0.36)	-0.3898 (0.38)	-0.3519 (0.37)	-0.4098 (0.43)	-0.5595 (0.50)	-0.6238 (0.52)
Hispanic	-0.4693 (0.34)	-0.3352 (0.34)	-0.2062 (0.34)	-0.2627 (0.36)	-0.3658 (0.35)	-0.5604 (0.41)	-0.6909 (0.50)	-0.6155 (0.52)
White	-0.7291 (0.44)	-0.5348 (0.46)	-0.2758 (0.46)	-0.3226 (0.47)	-0.2231 (0.48)	-0.3283 (0.52)	-0.5309 (0.58)	-0.6318 (0.58)
Borough dummy	YES	YES	YES	YES	YES	YES	YES	YES
Wave dummy	YES	YES	YES	YES	YES	YES	YES	YES
Interaction term	YES	YES	YES	YES	YES	YES	YES	YES

Bold signifies $p < 0.05$

Figure 1

Estimated effect of ESPP on homeless shelter use

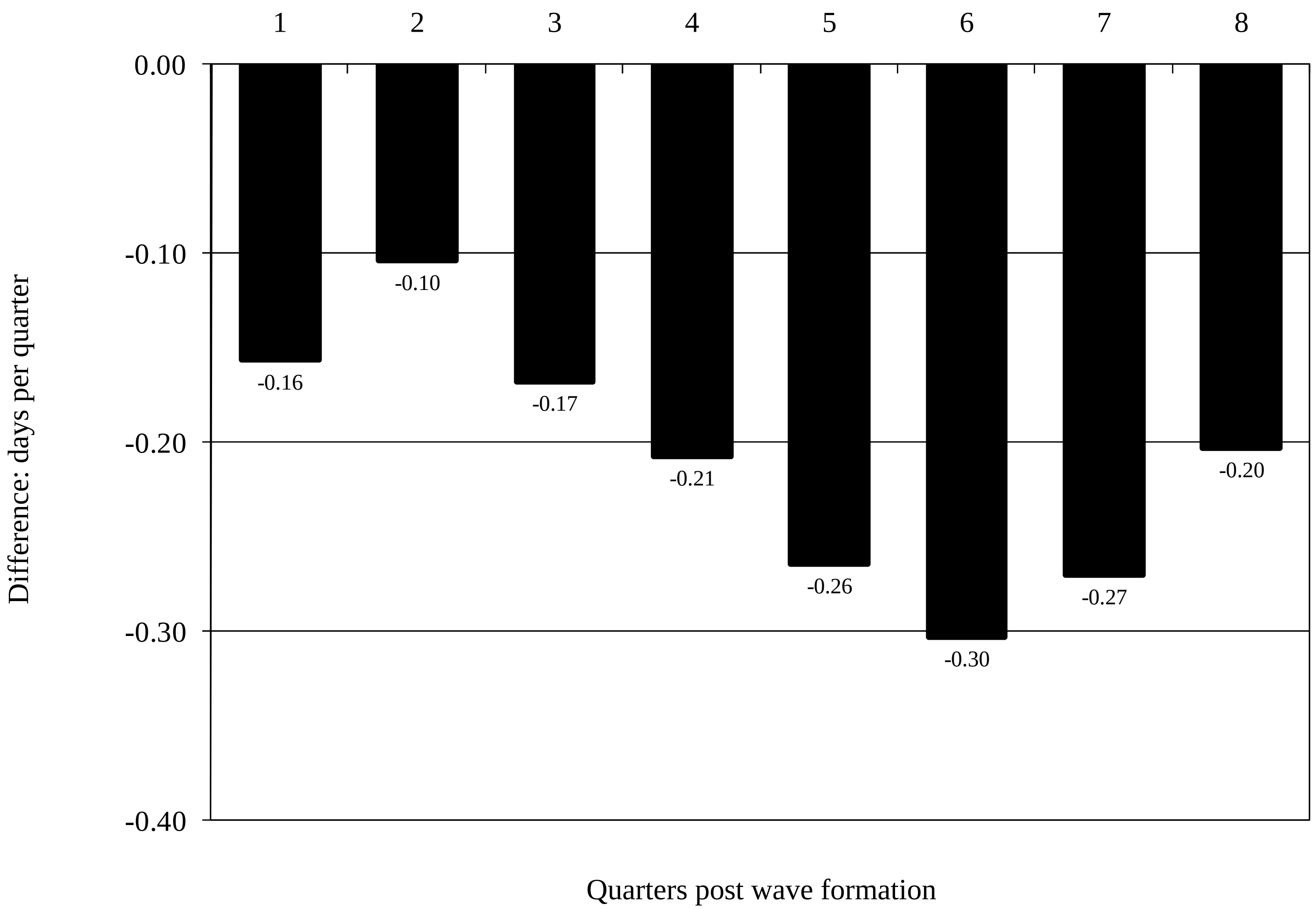


Figure 2

Estimated effect of ESPP on homeless shelter use with sample restricted to non-selectees not selected within given number of months

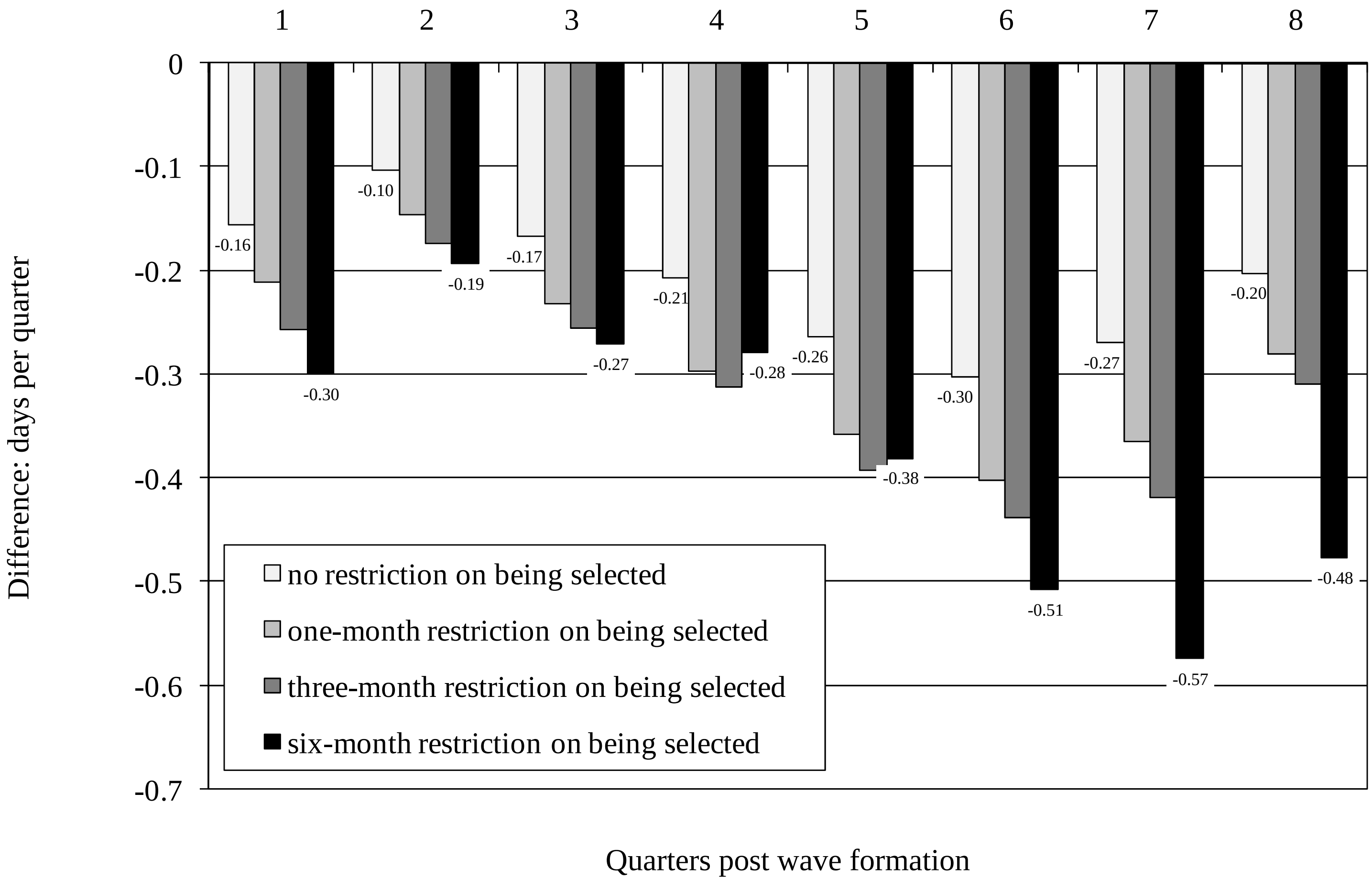


Figure 3

Estimated effect of ESPP on homeless shelter use with and without covariates

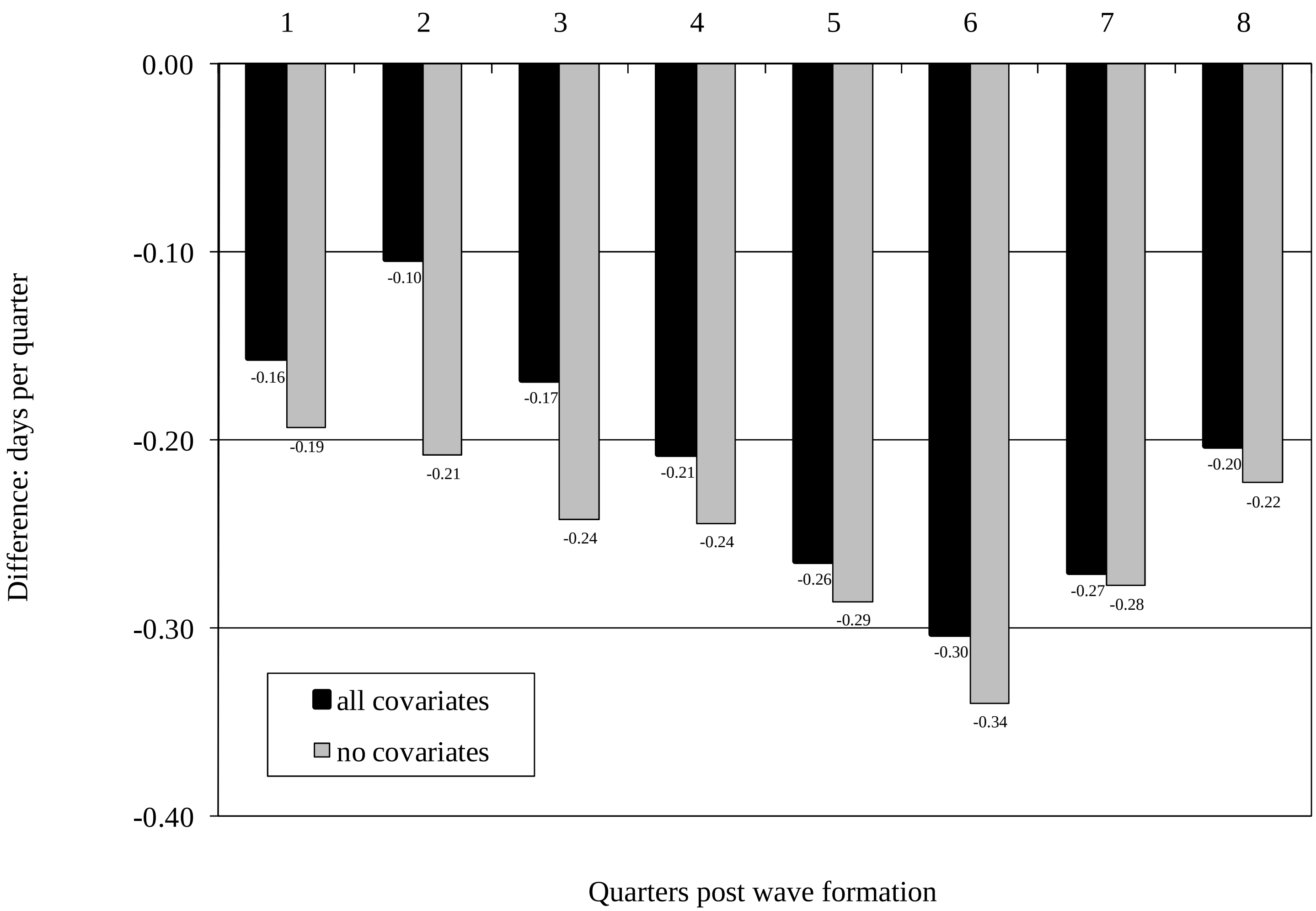


Figure 4

Estimated effect of ESPP on homeless shelter use with robust sample

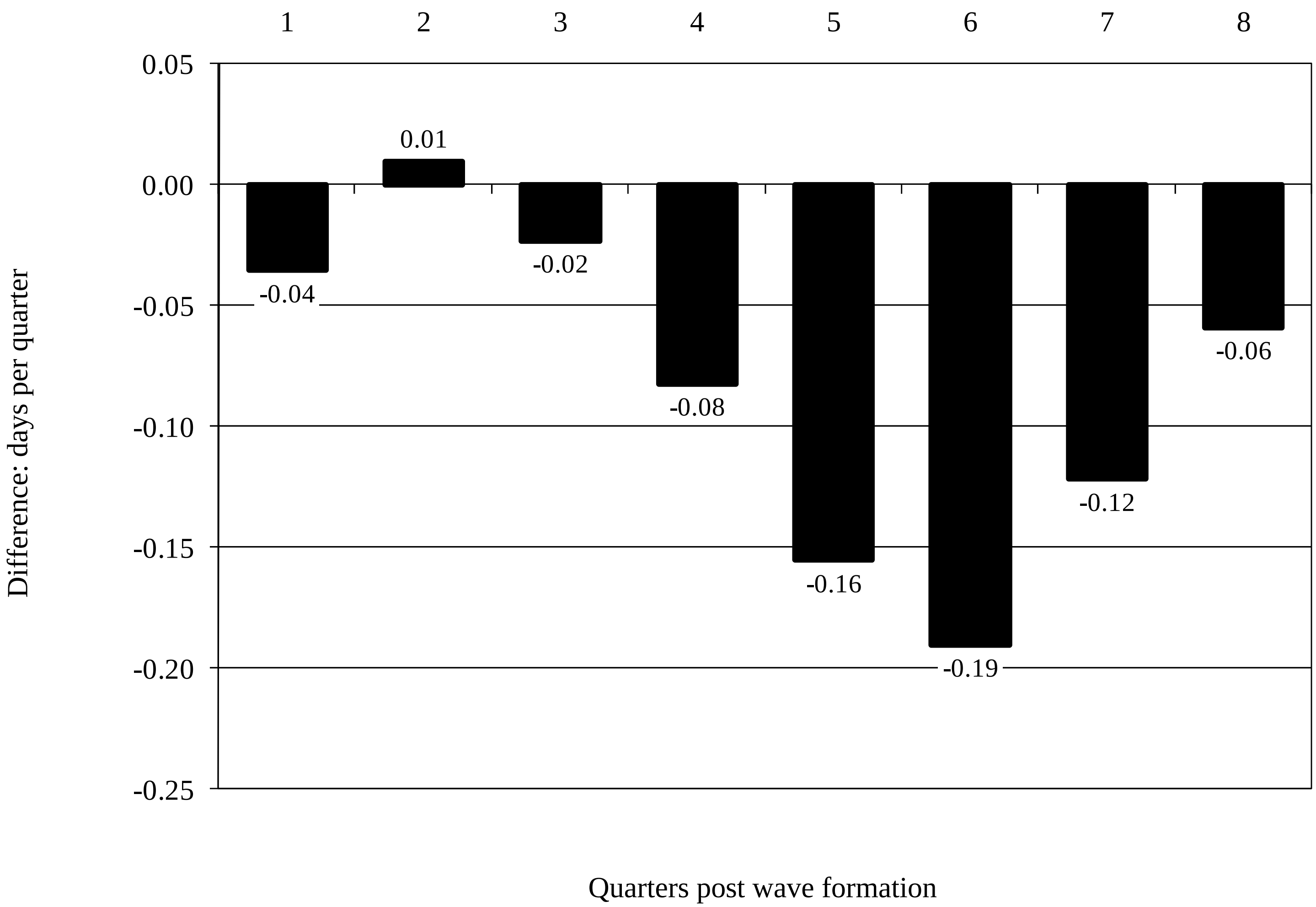


Figure 5

Estimated relationship between starting a job and homeless shelter use

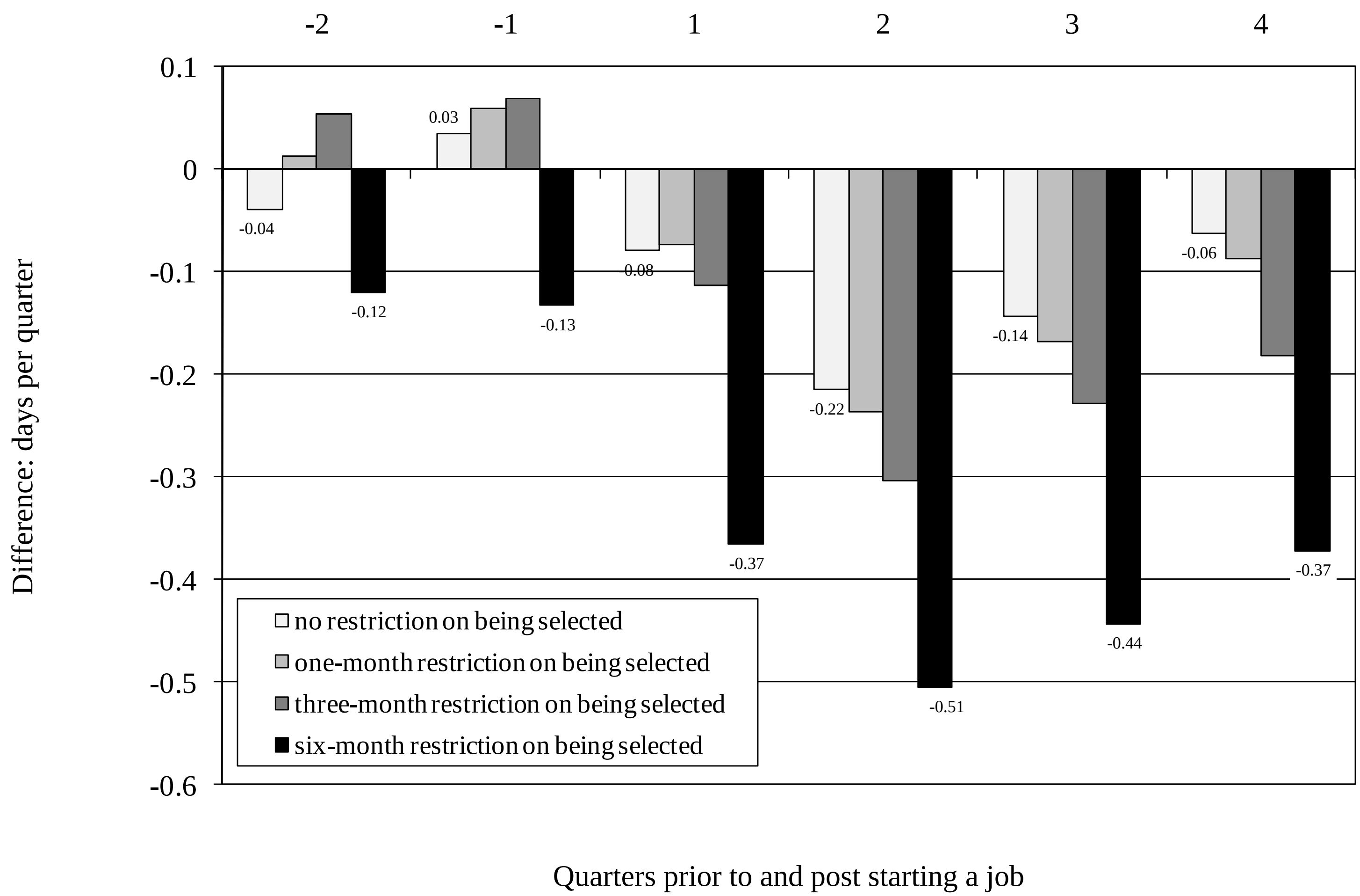
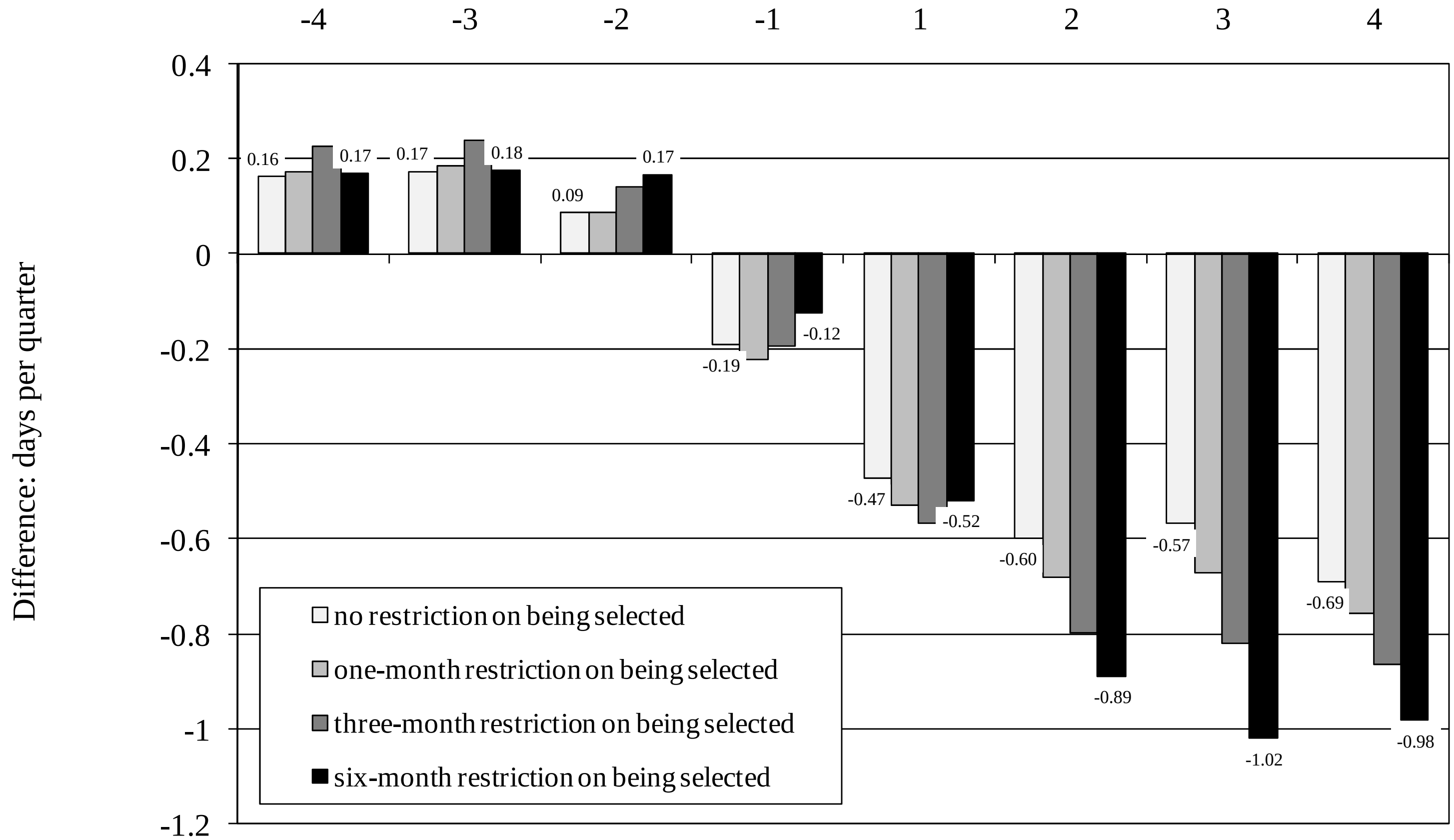


Figure 6

Estimated relationship between exiting welfare and homeless shelter use



Quarters prior to and post exiting welfare