ESTIMATING THE EFFECTS OF A TIME LIMITED EARNINGS SUBSIDY FOR WELFARE LEAVERS

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Working Paper **10647**

NBER WORKING PAPER SERIES

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Working Paper 10647
http://www.nber.org/papers/w10647

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
July 2004

We are grateful to SDRC for research support and for making the SSP data available, and to three anonymous referees, Costas Meghir, Douglas Tattrie, Ken Chay, Guido Imbens, David Lee, Jack Porter, James Powell, Charles Michalopoulos, and seminar participants at UC Berkeley, Princeton University, and University College London for helpful discussions. All conclusions in this paper are solely the responsibility of the authors, and do not represent the opinions or conclusions of SDRC or the sponsors of the Self Sufficiency Project. The views expressed herein are those of the author(s) and not necessarily those of the National Bureau of Economic Research.

David Card and Dean R. Hyslop
NBER Working Paper No. 10647
July 2004
JEL No. I38

ABSTRACT

In the Self Sufficiency Program (SSP) welfare demonstration, members of a randomly assigned treatment group could receive a subsidy for full time work. The subsidy was available for three years, but only to people who began working full time within 12 months of random assignment. A simple optimizing model suggests that the eligibility rules created an "establishment" incentive to find a job and leave welfare within a year of random assignment, and an "entitlement" incentive to choose work over welfare once eligibility was established. Building on this insight, we develop an econometric model of welfare participation that allows us to separate the two effects and estimate the impact of the earnings subsidy on welfare entry and exit rates among those who achieved eligibility. The combination of the two incentives explains the time profile of the experimental impacts, which peaked 15 months after random assignment and faded relatively quickly. Our findings suggest that about half of the peak impact of SSP was attributable to the establishment incentive. Despite the extra work effort generated by SSP the program had no lasting impact on wages, and little or no long run effect on welfare participation.

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Over the past decade the United States, Britain, and other countries have reformed their income support systems to enhance the financial incentives for work (see, e.g., Blundell and Hoynes, 2004). Traditional means-tested welfare programs impose high tax rates on program participants, reducing or even eliminating the payoff to work. Many analysts have argued that the result is a dynamic welfare trap. Once in the system, welfare recipients have little incentive to work, and the subsequent erosion of their skills and work habits makes it less likely they can leave in the future.[1]

In the early 1990s the Canadian government funded an innovative demonstration project – the Self Sufficiency Project or SSP – designed to test whether a time-limited earnings subsidy could help long term welfare recipients make a permanent break from program dependency. Unlike other subsidy programs (e.g., the U.S. Earned Income Tax Credit or the U.K. Working Families Tax Credit) SSP was only available for full time work.[2] Moreover, participants had to take up the subsidy within a year of being informed of their potential eligibility – otherwise they lost all future eligibility. Those who met the deadline were entitled to receive up to three years of payments, and could move back and forth between welfare and work, receiving the subsidy whenever they were working full time. At the end of three years they returned to the regular welfare environment.

The SSP evaluation used a randomized design. One half of a group of long-term welfare recipients was offered the supplement, while the other half remained in the regular welfare system. Data were collected for six years to measure the short-term impacts of the subsidy and any lasting effects once payments ended. Comparisons between the treatment and control groups show that SSP had significant short-term impacts on welfare participation and work, raising the full time employment rate and lowering

[1]See Plant (1984) for an explicit model of a dynamic welfare trap. The idea that welfare participation creates a dependency trap is a very old one. For example, Hexter (1917) analyzed the duration of relief spells at a private charity and found that people on relief longer had a lower likelihood of leaving. High implicit tax rates also create incentives to participate in the underground sector: see Fortin, Fechette, and Lemieux (1994).

[2]Phelps (1994) has argued for a general wage subsidy program for all low wage workers.

welfare participation by 14 percentage points within the first 18 months of the experiment.[3] The effects of SSP faded over time, however. By the third year after random assignment, the difference in welfare participation between the treatment and control groups had fallen to 7.5 percent, and by 69 months (a year and a half after all subsidy payments ended) the welfare participation rates of the two groups were equal.

The key contribution of this paper is to identify, both theoretically and empirically, the combination of incentives created by the time-limited eligibility rules of SSP, and to investigate the impact of SSP on welfare participation in light of these issues.[4] We first develop a simple optimizing model that suggests that the rules of SSP generated both an "establishment" incentive to find a full time job and leave welfare within a year of random assignment, and an "entitlement" incentive to choose work over welfare once eligibility was achieved. Simple experimental comparisons, while valid, confound these two effects. Given the insights of this model, we then develop a dynamic econometric framework that combines the experimental randomization associated with SSP with parametric modeling to identify the impact of the selection and treatment effects of SSP on welfare transitions.[5]

We begin by developing a suitable model of welfare entry and exit behavior among the SSP

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[3]A complete final report on the experiment is available in Michalopoulos et al (2002). This is a larger impact than was observed in other recent welfare reforms experiments in the U.S. For example, Hamilton et al (2001) review the five year impacts of eleven alternative welfare-to-work experiments on employment and welfare outcomes, and report typical impacts on employment and welfare participation of less than half that reported for SSP.

[4]To the best of our knowledge, the implications for the dynamic nature of the incentives created by the time-limited eligibility rule has received little, if any, attention. For example, there is no discussion of this issue in the background paper by Greenberg, Meyer, Michalopoulos and Robins (1992) which uses a static labor supply model to evaluate alternative subsidy parameters. Similarly, the SSP Implementation Report by Mijanovich and Long (1995, pp. 28-29) mentions 6 key questions that were addressed in the design of SSP, but the issue of time-limited eligibility is not discussed. Nor is the unusual nature of the incentives created by the time-limited eligibility discussed in the final SSP Recipient report (Michalopoulos et al, 2002) or any of the earlier reports.

[5]As noted by Ham and Lalonde (1996), even with a randomly assigned intervention the estimation of dynamic impacts requires a full specification of the process generating individual welfare histories.

control group. We show that the control group's outcomes are reasonably well described by a dynamic logistic model with second order state dependence and unobserved heterogeneity. We then augment this baseline model with treatment effects representing the establishment and entitlement incentives of SSP, and a model of the eligibility process that accounts for the potential correlation between the probability of entering or leaving welfare and the probability of attaining SSP eligibility.

Our empirical results suggest that the unusual time profile of the experimental impacts observed in the SSP demonstration arose from a combination of the short-term eligibility incentive and the longer-term entitlement incentives of the program. Indeed, our estimates attribute over one-half of the peak impact of SSP to the incentives created by the time-limit on eligibility. This decomposition helps to reconcile the large but short-lived peak impacts of the SSP demonstration with the impacts observed in other experimental welfare reforms. Our estimates also suggest that members of the program group with a higher probability of leaving welfare were more likely to establish entitlement for SSP. As a result, differences between the observed transition rates of the SSP-eligible subgroup and the control group overstate the causal effect of the supplement, even in the later years of the experiment. Finally, our analysis of wage outcomes shows that SSP no lasting effect on wages, despite the extra work effort engendered by the program's incentives. Thus, while the program generated a short-term reduction in welfare dependency, it had little or no permanent effect on long term self-sufficiency.

I. The SSP Demonstration - Description and Overview of Impacts

a. Income Assistance Programs and the SSP Recipient Experiment

Under the regular welfare system available to low income families in Canada, known as Income Assistance (IA), payments are reduced dollar-for-dollar for any earnings beyond a modest set-aside amount.[6] The implicit 100 percent tax rate on earnings, coupled with the availability of other benefits (e.g., free dental services) reduce the incentives for welfare recipients to work more than a few hours per week. As in other countries, rising caseloads in the 1980s led to concerns that the Canadian welfare system was promoting long-term dependency. Against this backdrop the Self Sufficiency Project was conceived as a test of a generous time-limited earnings subsidy for long-term welfare recipients. The overall project consisted of three separate demonstrations: the SSP "Recipient" study, conducted on long-term welfare recipients; the SSP "Plus" study, also conducted on long-term recipients but including both financial incentives and program services; and the SSP "Applicant" study, conducted on new welfare applicants. We focus here on the Recipient study and, henceforth, simply refer to it as SSP.[7]

Table 1 summarizes the main features of the study, including the eligibility criteria for the experimental sample and details of the subsidy formula. The demonstration was conducted in two provinces – British Columbia and New Brunswick – with random assignment between late 1992 and early 1995. Sample members were drawn from the pool of single-parent IA recipients who were over 18 years of age and had received welfare in at least 11 of the previous 12 months.[8] These requirements meant that nearly everyone in the sample had been on welfare continuously for at least a year.

The SSP subsidy formula is equivalent to a negative income tax with a 50 percent tax rate, a

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[6]The IA program is operated at the provincial level, but all the provincial programs share several important features, including a dollar-for-dollar benefit reduction rate. See Human Resources and Development Canada (1993) for a detailed inventory and description of income support programs in Canada in the early 1990s.

[7]See Lin et al (1998) for a comprehensive description of the program and results from the first 18 months of the experiment, Michalopoulos et al (2000) for a summary of results in the first 36 months, and Michalopoulos et al (2002) for the final report on the experiment. These reports also provide summary information on the SSP "Plus" and "Applicant" studies.

[8]No further limitations were placed on the sample. Thus, the experimental sample is in principle representative of the population of IA recipients who had been receiving welfare for a year or more in the two provinces. Roughly 90 percent of people who were contacted to participate in the experiment signed an informed consent decree and completed the baseline survey, and were then randomly assigned (Lin et. al, 1998, p.8).

"guarantee level" somewhat above average welfare benefits (but independent of family size) and a full-time hours requirement.[9] The formula was designed to provide much stronger work incentives than regular IA. For example, in 1994 a single mother with one child in New Brunswick was eligible for a maximum IA grant of $712 per month. If she were to leave welfare and take a full time job at the minimum wage her gross income would be $867 per month -- a gain of $155 per month, or about $1 per hour of work. Under SSP, however, the same person would receive an $817 subsidy, raising the relative payoff for work versus welfare to $972 per month, or $6.50 per hour. Since SSP payments were taxable, and also affected subsidized daycare costs, the payoff net of taxes and transfers is about 30 percent smaller, but still large (see Lin et al, 1998, Table G.1).

A distinguishing feature of SSP is the limited time window to establish the entitlement for payments. Since people in the treatment group who failed to initiate a subsidy payment within 12 months of random assignment lost all future eligibility, they faced a strong incentive to find a full time job within a year of entering the demonstration. For a single mother in New Brunswick, for example, SSP eligibility created an entitlement of up to $29,412 ($817 per month × 36 months) in additional income. Since some of the behavioral response to the program was arguably attributable to this "establishment" incentive, the restricted eligibility window acts to confound the interpretation of simple experimental comparisons of the outcomes of the treatment and control groups. It also makes it difficult to compare the experimental impacts of SSP to the effects of other welfare reform programs. The key goal of our econometric model is to disentangle the effects of the establishment incentive from the longer term entitlement incentive among those who achieved eligibility. Before turning to a more explicit consideration of the incentive effects of SSP, however, we summarize the key experimental findings from the demonstration.

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[9]In a conventional negative income tax with constant tax rate t and guaranteed (or minimum) income G, an individual with earnings y receives a subsidy of $G - ty$. This is equivalent to an earnings supplement equal to t times the difference between actual earnings and the "break-even" level $B = G/t$.

b. The SSP Sample Characteristics and Basic Impacts on Welfare

The data associated with the SSP experiment were assembled from three separate sources. IA data were obtained from provincial welfare records. SSP participation and receipt data were collected from SSP administrative records. Demographic data and labor market outcomes were obtained from surveys conducted at 18-month intervals, starting with a baseline survey just prior to random assignment. Table 2 gives an overview of the characteristics of the SSP sample. Columns 1 and 2 of the table show the mean characteristics of the control and program groups of the experiment, while the third and fourth columns distinguish between individuals in the program group who were either successful or unsuccessful in establishing eligibility. A small number of people selected for the sample left IA before they could be contacted for the baseline interview and randomly assigned. To simplify our empirical analysis, we ignore these people and focus on the 5,617 observations who were on IA in the two months before random assignment.[10] Income assistance records are available for 69 months following random assignment – 18 months after the last SSP recipient stopped receiving subsidy payments.

Random assignment of the treatment and control groups ensures that the "pre-assignment" characteristics of the two groups are statistically indistinguishable. The sample is mainly comprised of single mothers, with a mean age of 32 and an average of 1.5 children. Sample members show many of the characteristics associated with poor labor market outcomes, including a low rate of high school graduation (45 percent versus roughly 70 percent in the adult population of Canada), and a high probability of being raised by a single parent. Nevertheless, average work experience is relatively high (7.3 years), and about 20 percent of the sample were working at random assignment. Overall, 33.8 percent of the program group managed to establish eligibility for SSP payments. The two right-hand

[10]This restriction eliminates any "initial conditions" problems associated with the second order dynamic models we consider in the subsequent analysis. A total of 40 program group members and 27 treatment group members are excluded by this requirement. The difference in probabilities between the groups has a p-value of 10 percent. Since people did not know there program status (treatment or control) until after random assignment, we believe that the difference is coincidental.

columns of the table report the characteristics of the eligible and ineligible subgroups. As one might expect, the eligible subgroup was younger, better-educated, and more likely to be working just prior to random assignment.

The lower panel of Table 2 reports welfare participation rates at 6 month intervals after random assignment.[11] The control group (column 1) shows a steady decline in welfare participation, ending up with a 45% participation rate at the end of the sample period. Relative to this counterfactual trend the program group shows a faster initial drop. The decline is especially rapid for the SSP-eligible subgroup (column 3), but much slower for those who failed to establish eligibility (column 4), presumably reflecting both the incentive effects of SSP and the selective nature of the eligibility process. The impact of selectivity is particularly clear in the last month of the sample. At this point the average IA participation rates of program and control groups are equal, suggesting that SSP had no permanent impact.[12] But the welfare participation rate of the eligible subgroup is far below the average of the control group, while the rate of the ineligible subgroup is far above. Evidently, eligibility was more likely for those with a lower long run probability of remaining on welfare.

More insights into the impacts of the program and the behavior of the eligible subgroup are provided in Figures 1a-1c and 2a-2b. Figure 1a shows average IA participation rates in each month after random assignment, along with the estimated program impact (the difference in means between the program and control groups). SSP's impact peaked at −14 percentage points in month 15, declined steadily to −7 percentage points in month 36, and continued to decline further as people who were

[11]Some individuals may have left their original province and entered welfare in another province. These individuals would be coded as having 0 welfare benefits. SSP payments were available to people who left their original province.

[12]It is possible that SSP had offsetting long run impacts on the eligible and ineligible subgroups of the program group. Based on the nature of the program, however, we believe this is extremely unlikely.

eligible for subsidy payments came to the end of their 3 year eligibility period.[13] By month 53 all SSP payments had ended: at that point the gap in welfare participation between the program and control groups was 2.5% (standard error 1.3%). The gap continued to close for the remainder of the sample period, converging to 0 by month 69.

Figures 1b and 1c plot the welfare exit and entry rates of the control and program groups in each month after random assignment. Because of the selective risk sets for these conditional probabilities, differences in the entry and exit rates between the program and control group do not necessarily represent the causal effects of the program on transition rates. That said, the exit rate of the program group was 1-2 percentage points higher in the first 15 months of the experiment, then about half a point higher over the period from 15-48 months after random assignment (while program group members could receive SSP payments), and finally about equal to the rate for the controls in the period after the end of SSP-eligibility.[14] Conversely, welfare entry rates of the program group were 2-4 percentage points below those of the controls in the first 15 months after random assignment, about half a point lower in the period from 18 to 48 months after random assignment, and about equal to those of the control group in months 50 and later. The program group also had relatively high welfare entry rates 15-18 months after random assignment, perhaps reflecting the decision of some program group members to take a unattractive job near the end of the eligibility window, establish an entitlement for SSP, and then quit and return to welfare.

Figures 2a and 2b focus on the behavior of the eligible program subgroup at the start and end of their eligibility period. Figure 2a shows monthly IA participation rates, full time employment rates, and

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[13]There is some slippage in the measurement of the date of SSP eligibility, discussed below, though most of those who became eligible did so between 2 and 15 months after random assignment

[14]The initial peak in the difference in IA exit rates at months 3-4 corresponds with the exit of program groups members who were working full time at random assignment. The later peak (months 14-16) corresponds with the exit of program group members near the end of their eligibility window.

the fraction of people receiving SSP payments around the date of the first SSP check (month "0" on the graph). Following the jump associated with the first subsidy check, the SSP recipiency rate gradually declines to about 60 percent. As expected given the eligibility rules, the rate of full time employment rises prior to the date of the first SSP check, reaching a maximum of about 80 percent in the month before the check. (We attribute the fact that the rate never reaches 100 percent to recall errors in the labor market data – see below). Assuming that people in the program group became eligible once they started working full time, there is about a 1 month delay between eligibility and the dating of the supplement check.[15] SSP rules required supplement takers to leave IA, creating a mechanical link between the initiation of SSP eligibility and subsequent IA participation.[16] Eligibility for welfare is based on retrospective income flows, however, leading to the 1-2 month delay between the start of SSP and the end of IA shown in the figure.

Figure 2b shows welfare behavior and supplement recipiency rates near the close of the eligibility period. Again, we have aligned the data relative to the month of the first SSP check. Just before the end of eligibility about 50 percent of the eligible group were still receiving subsidy checks. The rate drops sharply at 37 months, reflecting the 3 year maximum eligibility rule.[17] The end of subsidy eligibility was associated with a spike in IA entry rates and a roughly 4 percentage point rise in IA participation, suggesting that some people who were SSP-eligible returned to IA as soon as their eligibility ended.

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[15]SSP recipients were required to mail their pay stubs to an administrative office to verify their employment. Delays in mailing and processing would be expected to generate at least a month delay between the actual commencement of full time work and the issuance of the first SSP check.

[16]This was implemented by having SSP staff notify the appropriate Income Assistance office that an individual was about to begin receiving subsidy payments.

[17]There is a small number of cases that received checks 37 or even 38 months after the first check date. We attribute this to errors in the dating of the checks and other measurement problems.

c. Impacts on Labor Market Outcomes

The SSP study included surveys at approximately 18, 36, and 54 months after random assignment that collected labor market outcomes of the treatment and control groups. Unfortunately, these data have some critical limitations relative to the administratively-based Income Assistance data. Most importantly, they are only available for 53 months after random assignment. Since some program group members were still receiving subsidy payments as late as month 52, this time window is too short to assess the long run effects of the program. Indeed, looking at Figure 1a, there is still an impact on IA participation in month 53 that does not fully dissipate until month 69. Second, because of non-responses and refusals, labor market information is only available for 85 percent of the experimental sample (4,757 people).[18] Third, there appear to be relatively large recall errors and seam biases in the earnings and wage data. Nevertheless, the labor market outcomes provide a valuable complement to the administratively based welfare participation data.

Figures 3a and 3b show average monthly employment rates and average monthly earnings of the program and control groups, along with the experimental impacts on these two outcomes. After random assignment the employment rate of the control group shows a steady upward trend. Relative to this trend the program group shows a faster rise in the first year of the experiment, reaching 40 percent by month 13 and stabilizing thereafter. The estimated program impact peaks at about 14 percentage points in month 13, declines to about 6 percentage points by month 36, and fall to 0 by month 53. The earnings data show a similar time profiles, although there are notable “jumps” for both the program and control

[18] The distribution of response patterns to the 18, 36, and 54 month surveys is fairly similar for the program and control groups (chi-squared statistic = 11.4 with 7 degrees of freedom, p-value=0.12). However, a slightly larger fraction of the program group have complete labor market data for 53 months – 85.4% versus 84.0% for the controls. Moreover, the difference in mean IA participation between the treatment and control groups in month 53 is a little different in the overall sample (2.5%) than in the subset with complete labor market histories (3.3%).

groups around months 18 and 36 attributable to the seams between surveys.[19]

A key issue for interpreting the observed impact of SSP is the quality of the jobs taken by members of the program group who would not have been working in the absence of the program. Figure 4 shows that most of these jobs paid close to the minimum wage. The upper line in the graph is the difference in the fractions of the program and control groups with a reported wage in each month. This is approximately equal to the difference in monthly employment rates. The dotted line in the figure represents the difference in the fraction of people who report an hourly wage within 25 cents of the province-specific minimum wage for the appropriate calendar month. (Note that the denominator of this fraction includes everyone in the program or control group, not just those who report a wage). Because of measurement errors in wages this is arguably an underestimate of the fraction of extra workers in the program group that earned close to the minimum.[20] The middle line (with open square markers) shows the excess fraction of the program group earning no more than $1 per hour above the minimum wage. Again, this is probably an underestimate of the true fraction. Even with potential attenuation biases, however, 60-80 percent of the extra wage earners in the program group were paid within $1 per hour of the minimum wage.

Under two key assumptions – that SSP had no effect on wages for people who would have been

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[19]Each of the three post-random assignment surveys asked people about their labor market outcomes in the 18 months months since the previous survey. Many people report constant earnings over the recall period, leading to a pattern of measured pay increases that are concentrated at the seams, rather than occurring more smoothly over the recall period.

[20]The wage data appear to be quite noisy, but the density of reported wages is highest right around the minimum wage. Assuming this is also true of the density of true wages, and that misclassification errors are symmetric, the observed fraction of workers earning near the minimum wage understates the true fraction. Formally, let $p(j)$ represent the true fraction of workers earning wages in interval j, let j' denote the interval that includes the minimum, assume that $p(j') > p(j)$ for all other intervals j, and assume that an individual with a wage in interval j has probability $1-q$ of being correctly classified in that interval, probability $q/2$ of being classified in interval $j+1$, and probability $q/2$ of being classified in interval $j-1$. Then the observed fraction of people in interval j' is $(1-q)p(j') + (p(j'-1)+p(j'+1))q/2 < p(j')$. Similar reasoning applies if misclassification errors extend to 2 or more intervals on either side of the truth.

working in the absence of the program, and only positive effects on labor supply – the ratio of the differences in earnings and hours of the program and control groups provides a consistent estimate of the average rate of pay earned during the extra hours of work attributable to SSP. To see this, let h^0_{it} represent the hours of work of individual i in month t in the absence of SSP, let h^1_{it} represent hours of work of i in month t if she is assigned to the program, let $\Delta h_{it} = h^1_{it} - h^0_{it}$ denote the treatment effect on hours for individual i, and let w^0_{it} and w^1_{it} represent average hourly earnings in the absence or presence of SSP. Because of random assignment, the difference in average monthly hours of the program and control groups in month t is a consistent estimate of $E[\Delta h_{it}]$. Likewise, the difference in average monthly earnings is a consistent estimate of

$$E[\ w^1_{it} h^1_{it} - w^0_{it} h^0_{it}\ ] = E[\ w^1_{it} \Delta h_{it} + h^0_{it} \Delta w_{it}\ ],$$

where $\Delta w_{it} = w^1_{it} - w^0_{it}$. Thus, the ratio of the difference in mean earnings of the program and control groups to the corresponding difference in mean hours is a consistent estimate of

$$m_t \equiv E[\ w^1_{it} \Delta h_{it} + h^0_{it} \Delta w_{it}\ ] / E[\Delta h_{it}] .$$

If wages for people who would have worked in the absence of SSP were unaffected by the presence of the program, then $E[\ h^0_{it} \Delta w_{it}] = 0$, and

$$m_t = E[\ w^1_{it} \Delta h_{it}\ ] / E[\Delta h_{it}] .$$

Assuming that $\Delta h_{it} \geq 0$ for all i, m_t is a weighted average of the wages earned by the people in the program group in month t, with weights proportional to the increase in hours caused by the SSP program.[21]

Figure 5 plots estimates of this ratio, along with a 95 percent confidence interval (estimated by the delta method). To account for differences in the minimum wage over time and across the two

[21]The assumption that $\Delta h_{it} \geq 0$ is identical to the monotonicity assumption required for the interpretation of local average treatment effects in an instrumental variables context (Angrist and Imbens, 1994). The full time hours limit for SSP was imposed to prevent the kinds of hours reductions often attributed to negative income tax programs. In light of this rule, and the fact that sample members are all single parents, we believe that the assumption of positive hours effects is reasonable.

provinces, we have divided the measured wage of each individual in each month by the prevailing minimum wage. The wage measure is therefore expressed in "minimum wage units", with a value of 1 implying that the average marginal wage is equal to the minimum wage. Inspection of the graph suggests that the average marginal wage is very close to the minimum wage, with no obvious trend, although the confidence intervals are rather wide after about month 30 (reflecting the small denominator of the ratio). This reinforces the conclusion from Figure 4 that the extra hours of the SSP program group were paid at wages very close to the minimum wage.[22]

The absence of a trend in the average marginal wage relative to the minimum wage is important because it suggests that the SSP program group experienced little or no relative gain in potential wages over the course of the experiment. This is confirmed by the analysis in Table 3 of labor market outcomes in the last available month (month 53). Recognizing the higher average level of wages in one of the two provinces (British Columbia) we present data for the overall sample and separately by province.[23] By month 53 there is no significant gap between the program and control groups in the fraction of people working or reporting a wage. Indeed, in one province the program group has a slightly lower employment rate than the control group, while in the other the pattern is reversed, although in neither case is the difference significant. Mean wages are also very similar in the program and control groups. This may seem a little surprising given the extra work effort by the program group over the previous 52

[22]If SSP causes people who would have worked anyway to select jobs with different wage rates, the interpretation of the average marginal wage is more complicated. Arguably, SSP provides an incentive to take a more stable job, or one with higher hours. If these jobs pay lower wages, the estimated average marginal wage will be negatively related to the fraction of people in the program group who are choosing different jobs (but would have worked anyway). The fact that the estimated marginal wage is roughly constant over the entire 53 month period suggests that any impact on wages of those who would have worked regardless of SSP is small.

[23]Wages for the labor market as a whole are 20-30% higher in Vancouver than in the areas included in the New Brunswick sample. The minimum wage varies by province, and is typically 25 percent higher in British Columbia than New Brunswick – e.g., $5.00 per hour in New Brunswick in 1993 versus $6.00 per hour in British Columbia.

months. Indeed, as shown in the table, we estimate that program group members worked a total of 0.28 years more than control group members between random assignment and month 53. Recall however that the sample had about 7 years of work experience at random assignment. Evidence on the returns to experience for less skilled female workers suggest the marginal impact of 0.2–0.3 years of work experience for such a group is small -- on the order of 1-2 percent (Gladden and Taber, 2000) – and probably undetectable.

The bottom panel of Table 3 presents results from a series of regression models that evaluate the impacts of being in the SSP program group on wages and cumulative work experience in the 53rd month of the experiment. These models are fit to the subsamples of control and program group members with wage data in month 53, and include time dummies, province dummies (in the models that pool the two provinces) and a set of covariates representing pre-random assignment characteristics. A possible concern with the models is selectivity bias, since the sample is conditioned on reporting a wage in month 53. However, given equality in the fractions of the program and control groups with a wage, and the similar characteristics of the employed subgroups, a conventional control function for selectivity bias would have the same mean value in the two groups, and is therefore orthogonal the the program group dummy.[24] The estimates in row (a) of the lower panel show that the difference in mean log wages between the program and control groups is small and statistically insignificant. By comparison, the estimates in row (b) show that among those working in month 53, members of the program group have significantly greater cumulative work experience than members of the control group.

In rows (c) and (d) we present models in which cumulative work experience is included as an

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[24]As noted in Ahn and Powell (1993), conventional models imply that the selection bias in the observed mean for a censored outcome is a monotonic function of the degree of censoring, conditional on the exogenous covariates. We evaluated the similarity of the characteristics of program and control group members who reported a wage in month 53 by running a regression model among wage reporters to predict program group status. The model included 24 pre-random assignment characteristics and interactions. The model has insignificant explanatory power (probability-value of F-test=0.94) suggesting there are no differences in the observed characteristics of the two groups.

additional explanatory variable for wages in month 53. As shown in row (c), a model that ignores the potential endogeneity of cumulative experience yields a relatively large and precisely estimated effect of work experience, on the order of 5% per year. When program group status is used as an instrument for cumulative work experience, however, the estimated effect becomes slightly negative but insignificant. Since the IV estimate is numerically equal to the ratio of the coefficients in rows (a) and (b), this is just a restatement of the fact that although the program group had greater cumulative work experience, they had marginally lower wages.

We have also examined the entire distributions of wages in month 53 for the program and control groups, and found no significant differences between them. The 10^{th}, 25^{th}, 50^{th}, 75^{th}, and 90^{th} percentiles of the two distributions are quite similar, and statistically indistinguishable. Likewise, a non-parametric rank test for equality of the distributions is insignificant. Overall, the work experience attributable to SSP appears to have had no detectable effect on wage opportunities. As we noted earlier, this is not very surprising given the mean level of experience among the single mothers in the experimental sample and existing evidence on the modest returns to added experience for lower-skilled women.

II. Interpreting the Impact of SSP on Welfare

As noted earlier, the time limit to establish eligibility for subsidy payments suggests that SSP had different incentive effects before and after the establishment of eligibility. Furthermore, for those who established eligibility, the date of entitlement potentially varies non-randomly. Consequently, although standard experimental comparisons between the treatment and control groups remain valid, the interpretation of such impacts is confounded by the different treatment effects associated with these two sets of incentives. In this section we focus on modeling the impact of SSP on welfare participation with the objective of disentangling these effects.

Our approach has three components. First, to help clarify the incentive effects of SSP and guide the formulation of our empirical model, we outline a simple dynamic model of work and welfare participation in the presence of SSP. Second, to provide a baseline model for welfare participation in the absence of SSP, we estimate a set of dynamic logistic models for the SSP control group that include unobserved heterogeneity and state dependence. Third, we combine the insights from the theoretical model with the baseline empirical model for welfare participation to develop an econometric model for estimating and interpreting the impacts of SSP. To account for possible selectivity of the SSP eligible subgroup, we model welfare participation and the determination of eligibility for SSP payments jointly. Crucial to this formulation is the maintained assumption, based on the randomization of SSP participants into the treatment and control groups, that the distribution of unobserved heterogeneity is the same across these groups. This assumption allows us to identify the treatment effects associated with establishing eligibility and the on-going entitlement by comparing the treatment group participation outcomes, conditional on the date at which eligibility is established, with the control group outcomes controlling for the common unobserved heterogeneity distribution across the two groups.

a. A Simple Benchmark Model for the Behavioral Impacts of SSP

We begin by presenting a simple dynamic model of work and welfare participation in the presence of SSP.[25] The model is a standard discrete time search model (e.g., Mortensen, 1977, 1986) in which a single parent has two options: full time employment or welfare participation. Welfare pays a monthly benefit \$b and yields a flow payoff of b. Full time employment at a monthly wage of \$w yields a flow payoff of $w-c$, where c reflects the disutility of work relative to welfare (including child care costs, work expenses, the value of foregone leisure, and potential stigma effects). Individuals maximize expected future income using a monthly discount rate of r. To keep the model as simple as possible, we assume that each month an individual receives a single job offer with probability λ, and that the arrival

[25] A more complete description of the model is presented in Appendix 1.

rate of offers is the same for workers and nonworkers. Wage offers are drawn from a stationary distribution with density f(w) and cumulative distribution F(w). Finally, we assume a constant rate of job destruction δ, which applies to new as well as existing jobs.

A key simplifying assumption is that wage opportunities do not depend on previous work effort. Based on the results in Table 3 we believe this assumption is reasonable. In fact, the evidence in Figures 4 and 5 suggests that, for most people who were offered SSP, the key issue was whether to accept a minimum wage job or not.

In this model optimal behavior in the absence of a wage subsidy program is characterized by a stationary value function U(w) that gives the discounted expected value associated with a job paying wage w, and a value V^0 of non-work (i.e., welfare participation). People who are employed at a wage w accept any offer paying more than w. People who are on welfare follow a reservation wage strategy and accept any job paying more than R, the (fixed) reservation wage satisfying $U(R)=V^0$. Under the assumptions of the model it is readily shown that the optimal reservation wage is R=b+c.[26]

This model predicts that welfare transitions in the absence of SSP are determined by a combination of the arrival rate of job offers, the rate of job destruction, the level of welfare benefits, the distribution of wages, and the pecuniary and non pecuniary costs of work. Specifically, the exit rate from welfare is $\lambda(1-\delta)\times(1-F(b+c))$, while the entry rate is δ. Individual heterogeneity in welfare exits arises from variation in λ, δ, c, and in the location of the wage offer distribution relative to the welfare benefit level. Individual differences in welfare entry rates arise from heterogeneity in δ.

If an SSP subsidy is made available at time 0 an individual currently on welfare has to evaluate three separate value functions: $V_i(t)$, the value of not working in month t, conditional on not yet having established eligibility; $U_e(w,d)$, the value of a job paying a wage w conditional on SSP-eligibility with d

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[26]If on-the-job and off-the-job search are equally productive, there is no reason to turn down a job yielding flow value (w-c) greater than the flow value of welfare (b). Hence the reservation wage is the income equivalent of welfare, b+c.

months of elapsed eligibility; and $V_e(d)$, the value of not working conditional on eligibility and d months of elapsed eligibility. The rules of SSP provide a link between these functions and the value functions in the absence of the program. In particular, $V_i(t)=V^0$ for $t \geq 13$, since those who fail to find full time work within 12 months of being offered the subsidy lose all future eligibility. In addition, $U_e(w,d) = U(w)$ for all $d>36$, since subsidy payments are only available for three years, while $V_e(d) = V^0$ for all $d \geq 36$. A revealed preference argument establishes that $U_e(w,d) > U(w)$ for all w and any $d \leq 36$, since the subsidy paid to a worker earning a wage w is strictly positive. From this it follows that $V_i(t)$ is decreasing in t (since the passage of time leaves less time to establish eligibility) and that $U_e(w,d)$ and $V_e(d)$ are both decreasing in months of elapsed eligibility (since the entitlement period is finite).

As is true in the absence of the subsidy, people who are working and eligible for the supplement accept any job offer that pays more than their current wage, while those who are on welfare with d months of elapsed eligibility follow a reservation wage strategy with a reservation wage $R_e(d)$, with $V_e(d) = U_e(R_e(d), d)$. Since people can quit jobs that are no longer acceptable once their SSP eligibility ends it is straightforward to show that the optimal reservation wage for an SSP-eligible nonworker equates the net income from a reservation-wage job to the flow value of welfare, b+c. Since b and c are fixed, R_e is independent of d and is defined by the equality $R_e+s(R_e) = b+c$, where s(w) is the subsidy for working at a wage rate w.[27]

Individuals who are still on welfare in month t and not yet SSP-eligible have a reservation wage R(t) satisfying the condition $V_i(t) = U_e(R(t), 1)$. From this equality, and the fact that $V_i(t)$ is decreasing in t, it follows that the reservation wage R(t) is decreasing in t: individuals with fewer months of potential eligibility left will accept lower wage jobs. Moreover, the reservation wage in the first month of potential eligibility, R(1), is strictly less than the reservation wage once eligible, since a full-time job for

[27]Since $s(w) \geq 0$ the reservation wage for SSP-eligibles is below the reservation wage R in the absence of the program. Indeed since R=b+c, we have that $R_e+s(R_e) = R$. Note that R_e could be well below the minimum wage. However, SSP rules required participants to earn at least the minimum wage.

someone who is not yet eligible provides the same flow benefits as for someone who is eligible, and in addition guarantees future eligibility. Thus, $R_e > R(1) \geq R(2) .... \geq R(12)$.

The effects of SSP on the welfare/work decision can be summarized by the difference between the reservation wage profiles of a representative welfare recipient in the presence or absence of SSP. Figure 6a shows the sequence of reservation wages for a person who is offered SSP but fails to establish eligibility, along with the (constant) reservation wage $R=b+c$ in the absence of the program. During the 12-month window that individuals have to establish eligibility the reservation wage is below R and declining. At the close of the window those who failed to find a job revert to the reservation wage in the absence of the program. Figure 6b shows the sequence of reservation wages for a person who is offered SSP and establishes eligibility in month $t_e \leq 12$. Prior to t_e the reservation wage is declining. At t_e the reservation wage jumps up and remains constant until the end of the entitlement period at a value satisfying the condition $R_e+s(R_e)= b+c$. After SSP entitlement ends in month t_e+36 the reservation wage reverts to b+c.

The path of the optimal reservation wage illustrates the two different incentive regimes experienced by those exposed to SSP. During the pre-eligibility period (up to the establishment of eligibility in month t_e, or 12 months after random assignment for those who don't establish eligibility), members of the treatment group have a low and declining reservation wage, leading to a faster rate of transition from welfare to work than would be expected in the absence of SSP. Those who establish eligibility then adopt a somewhat higher reservation wage, but still lower than the one in the absence of the program, implying that they are more likely to leave welfare and re-enter work than otherwise similar members of the control group. The jump in the reservation wage at t_e implies that some people who accepted low-paying jobs to gain eligibility would be expected to quit and return to welfare almost immediately. Once SSP eligibility ends (36 months after establishing eligibility, or starting in month 12 for those who don't establish eligibility), the reservation wage returns to its level in the absence of the program and the behavioral effects of SSP disappear. Again, as a result of the jump in the reservation wage at the close of eligibility, people holding jobs paying less than the reservation wage in the absence of SSP would be expected to quit and re-enter welfare (consistent with the patterns in Figure 2b).

While this stylized model provides a guide to the potential effects of SSP, it is clearly oversimplified. For example, the model assumes that the cost of work (c) is constant. More realistically, work costs vary over time (e.g., if a child becomes sick), leading people to revise their reservation wages and quit some jobs that were previously acceptable. An earnings subsidy widens the range of cost fluctuations that can be tolerated at any wage, leading to a reduction in the flow from work back to welfare. Another limitation is the assumption that people either work full time or receive welfare. In fact some people leave welfare *without* entering full time work. In our empirical model, we therefore have to distinguish between leaving welfare and becoming SSP-eligible. The model also ignores the possibility that the cost of work is affected by previous work experience. A habit persistence model, for example, might imply that individuals who work more when SSP is available eventually lower their reservation wages. While endogenous taste formation is sometimes mentioned as a welfare trap mechanism, the evidence from the SSP experiment is not particularly favorable to this story, since by month 53, just a few months after the end of subsidies, the employment rate of the program group had converged to the rate of the control group. The model also ignores wealth effects and intertemporal substitution effects which could lead to a negative impact on the probability of work in the period immediately after the end of SSP.

b. Models of Welfare Participation in the Absence of SSP

We begin our empirical analysis by estimating a series of models of welfare participation for the SSP control group. Our objective here is to formulate a statistical model for welfare participation in the absence of SSP that will provide a suitable baseline counterfactual for comparing welfare outcomes under the SSP program. We adopt a panel data approach rather than a hazard modeling approach because of the high incidence of multiple spells in our data (over half the sample have 2 or more spells on welfare), and the need to specify a tractable (low dimensional) model of the effects of unobserved heterogeneity on welfare transition rates and SSP eligibility.

Let y_{it} represent an indicator that equals 1 if person i receives IA in month t (where t runs from 1, the first month after random assignment, to T=69), and let $x_{i1}, \ldots, x_{iT}$ represent a sequence of observed covariates for individual i. We consider models in the following class:

(1) $P(y_{i1}, \ldots\, y_{iT} \mid x_{i1}, \ldots\, x_{iT}) = \int \{ \prod_t L(\alpha_i + x_{it}\beta + \gamma_1 y_{it-1} + \gamma_2 y_{it-2} + \gamma_3 y_{it-1}\, y_{it-2}) \} f(\alpha_i)\, d\alpha_i$,

where L( ) represents the logistic distribution function and $f(\alpha_i)$ represents the density of unobserved heterogeneity among the experimental population.[28] We consider two alternative specifications for f(.). In the first case we assume that $f(\alpha_i) = \phi(\cdot\, ; \sigma_\alpha)$, the normal density with mean 0 and standard deviation σ_α. The integral on the right hand side of equation (1) can then be approximated by the method of Gaussian quadrature.[29] As an alternative, we assume that f(.) is a discrete distribution with a small number of mass points, and estimate the location of the mass points and their relative probabilities.

Equation (1) describes a logistic regression model with second order state dependence and a random effect. Although our benchmark theoretical model suggests a first order state dependence specification, we show below that a second order specification leads to a considerable improvement in

[28]Chay and Hyslop (2001) have found that logistic models with state dependence and unobserved heterogeneity fit welfare behavior about as well as more computationally demanding multivariate Probit models that allow for serial correlation in the transitory component of welfare participation. Also, as discussed above, because everyone in our sample received IA in the two months prior to random assignment (periods 0 and −1), $y_{i0} = y_{i,-1} = 1$, we do not have to model the distribution of initial conditions.

[29]As noted by Butler and Moffitt (1982), the likelihood for models in the class of equation (1) when f( ) is the normal density have the form $\int g(x) \exp(-x^2)\, dx$, which can be approximated by the sum: $\sum_I w_i\, g(x_i)$, where g is evaluated at a fixed set of N points (x_i), and the sum is formed with a fixed set of weights (w_i). We use N=10 points: see Abramowitz and Stegum (1965, p. 924).

model fit. There are a surprising number of single-month spells on or off welfare, and the key assumption of a first order model – that exit or entry rates are independent of the length of the current spell – is clearly violated. A second order specification allows the transition rate in the first month of a spell to differ from the transition rate in subsequent months and is more consistent with the data.[30]

The first three columns of Table 4 present estimation results and diagnostic statistics for versions of equation (1) with normal heterogeneity. The only covariates are a fourth order polynomial in time since random assignment.[31] The model in column (1) assumes first order state dependence, while the model in column (2) allows second order dependence. The second order terms are highly significant, and their pattern implies that, after controlling for the permanent component of welfare participation, welfare transition rates are higher for those who have only been in their current state for one month than for those who have been in the state for 2 or more months. The model in column (3) further generalizes the specification by allowing the state dependence parameters to vary linearly with the random effect (i.e., $\gamma_k = \gamma_{k0} + \gamma_{k1}\,\alpha_i$, for k=1,2,3). This specification relaxes the "linear in log odds" assumption of the logistic functional form and permits the degree of state dependence to vary by whether individuals have a higher or lower long-run propensity to participate in welfare. The interaction terms are statistically significant and their addition leads to a noticeable improvement in the likelihood of the model. The sign pattern of the interactions implies that the state dependence effects are larger for those who are less likely to be on welfare in the long run.

How well do these models explain observed welfare outcomes? As a description of average IA

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[30] An alternative would be to ignore 1 month spells by "smoothing" over such spells.

[31] We have fit a variety of models that include fixed baseline covariates such as province, education, and gender, as well as time varying indicators for calendar time. In these models several of the covariates are statistically significant and absorb some of the variance attributed to the random effect. However, the ability of models with a controls for observable heterogeneity to fit the distribution of observed welfare histories is insignificantly different from models that treat all heterogeneity as unobserved.

participation rates, the answer is very good. The time path of welfare participation predicted by any of the models is fairly close to the actual path. This is not surprising, however, given that the models include a fourth order polynomial trend, and that the control group's welfare profile is fairly smooth. A more difficult challenge is to predict the distribution of *welfare histories* among the control group.[32] To evaluate the models on this dimension we compare the predicted and actual fractions of the control group in a set of mutually exclusive cells defined by the total months on IA since random assignment, the number of welfare transitions, and whether the number of transitions is odd (in which case the individual ends up off IA) or even (in which case she ends up on IA). The cells used in our comparisons, along with the actual and predicted numbers of observations from the SSP control group in each cell, are shown in Table 5. We selected the cells to yield reasonable cell sizes: thus, we grouped welfare histories with 0-2, 3-8, 9-14, ... total months on IA, with separate cells for 68 or 69 months on IA. Overall, we collapsed the 2^{69} possible welfare histories into 50 cells.

For each of the models in Table 4 we constructed a chi-squared statistic based on the deviation between the predicted and actual number of observations in each cell.[33] Allowance for second order state dependence leads to a considerable improvement in the ability of the model to predict the distribution of welfare histories (compare fit statistics in column (1) and column (2)). By comparison the addition of the interaction terms in column (3) leads to only a modest additional improvement in fit.

The upper panel of Table 5 compares the actual (in bold) and predicted (in italics) distributions of the SSP control group across the 50 cells, using the model from column (3) of Table 4. A prominent

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[32]The idea of comparing predicted and actual frequencies from multinomial probability models is discussed in Moore (1977), and is used in Card and Sullivan (1988) and Chay and Hyslop (2001). We construct predicted cell fractions by simulating each model with 10 replications per sample member.

[33]We constructed the standard Pearson statistic: $\sum_j (O_j - E_j)^2 / E_j$, where O_j is the number of observed cases in cell j=1..J and E_j is the expected number. Since the expected number is based on a model fit to the same data, the statistic does not necessarily have a chi-squared distribution with J-1 = 49 degrees of freedom. We interpret the goodness of fit statistics as informal summary measures of fit.

feature of the data is the large number of people (604 = 21.7 percent of the group) who were on IA continuously. The model under-predicts the size of this group (predicted number =546.8). The control group also includes a relatively large number of people who left welfare for one month and then returned (these are the 189 = 6.8 percent of the sample with 68 months on IA and 2 transitions). The model actually over-predicts the size of this group (predicted number = 211.7). Looking down the two right hand columns, a second order model with normal heterogeneity provides reasonably accurate predictions for the distribution of total months on IA, with the exception of the last three groups (63-67 months on IA, 68 months on IA, and 69 months on IA). The model over-predicts the fractions with 63-67 or 68 months on IA and under-predicts the fraction with 69 months on welfare.

A computationally feasible alternative to normally distributed heterogeneity is the assumption that the random effects have a point mass distribution. Column (4) of Table 4 shows estimation results for one such model, with 4 mass points. We also computed models with 5 and 6 mass points, but found relatively little improvement in either the log likelihood or the goodness of fit statistics relative to the 4 mass point model. Interestingly, the estimates of the state dependence coefficients are relatively similar in columns (3) and (4), although the mass point model has a somewhat higher log likelihood.

In comparing different mass-point models, we observed that once we allow for 3 or more points of support, one of the mass points tends to infinity (i.e., a value of 16 or more). Taken literally this means there is a subgroup of "pure stayers" in the data who never leave welfare. This feature leads to an improvement in the ability of the model to fit the distribution of welfare histories, as shown by the chi-squared statistics in Table 6, and by the comparison of the actual and predicted distributions of welfare histories from this model in the lower panel of Table 5.

Another set of diagnostic statistics for the different models is presented in the bottom rows of Table 4. These are the estimated means, variances, and 1^{st} - 5^{th} order autocorrelations of the generalized residuals from the different specifications. The generalized residual for person i in month t, evaluated at a given value of the random effect, is:

$$r_{it}(\alpha) = (y_{it} - p_{it}) / [\, p_{it}(1-p_{it})\,]^{1/2} ,$$

where $p_{it} = p_{it}(\alpha;\ \beta, \gamma,\ x_{it}, y_{it-1}, y_{it-2})$ is the predicted probability of welfare participation, conditional on x_{it}, y_{it-1}, y_{it-2}, the parameters β and γ, and the value of random effect. Note that if the model is correctly specified, at the true value of the random effect for person i, $E[r_{it}(\alpha_i)]=0$, $E[r_{it}(\alpha_i)^2\,]=1$, and $E[r_{it}(\alpha_i)r_{it-j}(\alpha_i)]=0$. Thus a potential specification check is to compute the sample analogue of one of these statistics (averaging over i and t) and compare the result to the expected value under the null hypothesis of a correctly specified model. The true value of the random effect for any given person is unknown. However, given the likelihood function, the marginal distribution of the random effects, and the observed sequence of data for individual i, we can compute the posterior distribution for the random effect for that individual.[34] We therefore evaluate the expectations using this posterior, and average the values of the resulting statistic across the entire sample. For the mass point heterogeneity model the posterior has only 4 points of support and the calculation is straightforward. For the normal heterogeneity models, we use a simulation approach, drawing 20 values of the random effect for each person, and computing the posterior distribution for a given person over this set.

The residual statistics for the first order model in column (1) show clear evidence of misspecification: the average variance is 1.11, rather than 1, and the 1st order autocorrelation is -0.07. The statistics for the other models are considerably better, although all three show a small negative value for the second order autocorrelation. These results suggest that models with unobserved heterogeneity and second order state dependence provide a reasonably good description of the welfare participation data, with relatively little serial correlation in the prediction errors from the models.

[34]Let $\ell(y|\alpha)$ denote the likelihood for the sequence of welfare outcomes, conditional on a value of the random effect, the covariates, and the other parameters, and let $f(\alpha)$ denote the marginal distribution of the random effects. The posterior distribution of the random effects for a person with outcomes y is $f(\alpha|y) = \ell(y|\alpha)\, f(\alpha) / \int \ell(y|\alpha')\, f(\alpha')\, d\alpha'$.

c. Models of Welfare Participation for the SSP Program and Control Groups

We now turn to the specification of a model for the SSP program group. Building on the insights of the simple theoretical model and the rules of the SSP program, we include two separate treatment effects in the dynamic welfare participation model. The first is the essentially mechanical effect on welfare participation associated with the rules for establishing eligibility for SSP that required recipients to leave IA. The second effect is caused by the fact that members of the eligible subgroup have a greater incentive to choose work over welfare in any given month of their entitlement period. The key to distinguishing these effects is the date that SSP eligibility is established, t^e_i.[35]

For any member of the program group we identify up to four distinct phases: (1) the pre-entitlement period, ending at t^e_i-1 for those who establish eligibility in month t^e_i, and at the close of the eligibility window for those who do not; (2) the transitional period for those who establish eligibility, lasting for three months after the establishment of eligibility,[36] when SSP program rules required newly eligible members of the program group to leave IA; (3) the entitlement period, lasting from the end of the transitional period to t^e_i+36; and (4) the post-entitlement period, when supplement payments were no longer available, beginning at t^e_i+37 for those who became eligible for SSP, and at the close of the eligibility window for those who did not achieve eligibility.

Let E_{it} represent an indicator for the event that individual i is eligible for SSP as of the start of month t. Note that the sequence $\{E_{it}\}$ makes at most a single transition from 0 to 1, and that this occurs in the eligibility month t^e_i (i.e., $t^e_i = \min_t \{E_{it}=1\}$). We assume that IA participation and the SSP eligibility indicators are correlated through their joint dependence on the unobserved heterogeneity

[35] We discuss the actual measurement of this date in Appendix 2. As discussed in the appendix, we allow the eligibility window to last for 14 months rather than the 12 months specified in the SSP rules, due to processing delays, dating errors and/or leniency in the application of the rules.

[36] Appendix 2 discusses the rationale for choosing 3 months for the transition period duration.

component α_i :

(2) $P(y_{i1}, ... y_{iT,}\ E_{i1},...E_{iT} \mid x_{i1}, ... x_{iT})$

$$= \int \{ \prod_t P(y_{it}, E_{it} \mid y_{it-1}, y_{it-2}, ...\ E_{it-1}, E_{it-2}, ..., x_{it}, \alpha_i) \} f(\alpha_i)\, d\alpha_i .$$

Using the fact that treatment status is randomly assigned, we also assume that the distribution of unobserved heterogeneity is the same for the program and the control groups.

Conditional on α_i and the covariates x_{it} we assume that E_{it} is determined independently of current or lagged IA status, while y_{it} depends on current eligibility, how long an individual has been eligible, and on 2 lags of previous IA status.[37] Specifically, we assume:

(3) $P(y_{it}, E_{it} \mid y_{it-1}, y_{it-2}, ...\ E_{it-1}, E_{it-2}, ..., x_{it}, \alpha_i)$

$$= P(E_{it} \mid E_{it-1}, E_{it-2}, ..., x_{it}, \alpha_i) \times P(y_{it} \mid y_{it-1}, y_{it-2}, E_{it}, E_{it-1}, ..., x_{it}, \alpha_i) .$$

We adopt the specifications in columns (3) and (4) of Table 4 as the baseline models for the control group, and assume that IA participation of the program group follows this model with the addition of a set of treatment effects that depend on which of the four phases the individual is currently occupying. Specifically, we assume that

(4) $P(y_{it} \mid y_{it-1}, y_{it-2}, E_{it},\ t^e_{i},, x_{it}, \alpha_i)$

$$= L(\alpha_i + x_{it}\beta + (\gamma_{10}+\gamma_{11}\alpha_i)y_{it-1} + (\gamma_{20}+\gamma_{21}\alpha_i)y_{it-2} + (\gamma_{30}+\gamma_{31}\alpha_i)y_{it-1}\, y_{it-2} + \tau(t, E_{it}, t^e_i,\ y_{it-1})) ,$$

where L(.) represents the logistic distribution function, and $\tau(t, E_{it}, t^e_i,\ y_{it-1})$ is the behavioral impact of SSP. We assume that the SSP establishment and entitlement treatment effects are confined to the

[37]The assumption that eligibility is independent of IA histories would be satisfied if people always stayed on IA until finding a full time job, and if all full time jobs satisfied the SSP eligibility conditions (as assumed in our theoretical model). In fact, some people leave IA without moving to full time work – for example, those who move in with a partner. A more complete model might deal with partnering as a "competing risk" that absorbs some welfare leavers. On average, the hazard of becoming eligible for SSP is lower for people who are off IA, or have previously left IA.

transitional and the entitlement periods respectively,[38] and are constant within each of these periods, but allow separate effects depending on whether the individual was on or off IA in the previous month. We begin with specifications that assume the treatment effects are constant across individuals:

$$\tau(t, E_{it}, t^e_i, y_{it-1}) = E_{it} \times 1(t^e_i \leq t \leq t^e_i+J-1) \{ \psi_0 1(y_{it-1}=0) + \psi_1 1(y_{it-1}=1) \}$$
$$+ E_{it} \times 1(t^e_i+J \leq t \leq t^e_i+35) \{ \lambda_0 1(y_{it-1}=0) + \lambda_1 1(y_{it-1}=1) \} ,$$

where J (=3) is the duration of the transition period; the parameters ψ_0 and ψ_1 measure the "establishment" effects of SSP eligibility during the transitional period on individuals who were off or on IA in the previous month, respectively; and the parameters λ_0 and λ_1 measure the corresponding incentive effects during the entitlement period. Later, we consider specifications that allow the treatment effects to vary linearly with the random effect.

Given the nature of the eligibility process, a natural model for E_{it} is a hazard model for the event of establishing eligibility in month t, conditional on not establishing it earlier. We assume that the hazard of eligibility depends on the individual heterogeneity effect α_i and on the time since random assignment:

$$(5) \quad P(E_{it} \mid E_{it-1}, E_{it-2}, ..., x_{it}, \alpha_i)$$
$$= \Phi[d(t) - k(\alpha_i)] \quad \text{if } E_{it-1} = 0 \ \& \ t \leq T_e ,$$
$$= 1 \quad \text{if } E_{it-1} = 1 ,$$
$$= 0 \quad \text{if } E_{it-1} = 0 \ \& \ t > T_e ,$$

where Φ is the standard normal distribution function, d(t) is a function of time, T_e is the duration of the "establishment" period (14 months), and $k(\alpha_i)$ is a simple function of the random effect. For the case of normally distributed heterogeneity, we assume that $k(\alpha_i)$ is linear (i.e., $k(\alpha_i) = k_0 \alpha_i$). For the case of mass point heterogeneity, we adopt a more flexible specification and assume that $k(\alpha_i)$ takes on a different value for each mass point (with one value normalized to 0). Note that if the probability of

[38]This assumption is consistent with the convergence of the IA participation rates of the program and controls groups after SSP payments ended, and is implied by the search model.

establishing eligibility is independent of the individual-specific determinants of welfare participation, then $k(\alpha_i)$ will be constant for all values of the random effect. Based on the comparisons of welfare outcomes near the end of the sample period in Table 2, however, we expect $k(\alpha_i)$ to be positively correlated with α_i.

Although the causal effects of the entitlement incentives on welfare transition rates are directly identified in this model through the parameters λ_0 and λ_1, the effects of the establishment incentives are embedded in the eligibility model and the establishment treatment effects, and are not directly identified. Since we observe the IA outcomes of the control group, however, we can infer the net effect of these incentives. In essence the eligibility model and the establishment period treatment effects provide a joint model of the selection process leading to the entitlement period data for members of the treatment group (including SSP-eligibility status, the date of entitlement t^e and the IA participation state in month t^e+3), and of the deviation between the welfare outcomes of the treatment and control groups in the early months of the demonstration.

d. Estimation Results for Combined Models

Table 6 presents estimates of alternative specifications of equations (1)-(5). All the models adopt the baseline specification used in Table 4, columns (3) and (4), including second order state dependence, interactions between the state dependence effects and the random effects, and a fourth order trend in the IA participation model. The specifications in columns (1)-(4) assume normally distributed random effects (corresponding to column (3) of Table 4) while the specification in column (5) uses a 4 mass point specification (corresponding to column (4) of Table 4). As a reference point, the model in column (1) ignores any correlation between SSP eligibility and the random effect, while the other specifications include an eligibility model based on equation (5), with a trend function $d(t) = d_0 + d_1(t-1)$

+ $d_2/(t-2)$.[39] The specifications in columns (1) and (2) assume that the treatment effects are constant across individuals, while the specifications in columns (3)-(5) allow the treatment effects to vary linearly with the random effects. Finally, the models in columns (4) and (5) allow for individual heterogeneity in the trend in IA participation by including interactions of the random effect with a quadratic in months since random assignment.

The models in Table 6 yield estimates of the state dependence parameters that are similar to the estimates obtained for the control group alone. The estimated treatment effects are roughly similar across specifications, with large negative estimates of the transition-period establishment-effects and smaller but significantly negative entitlement-period effects. A comparison of the treatment effects in columns (1) and (2), however, shows that the implied entitlement-period effects are about 30 percent larger when eligibility is treated is exogenous (column (1)) than when it is modeled as endogenous (column (2)). This is the pattern that would be expected if people with a lower probability of IA participation are more likely to become SSP-eligible. That is, in the model in column (2) some of the differential in entitlement-period transition rates between the eligible and ineligible program subgroups is attributed to the selectivity of eligibility status, whereas in the model in column (1) all of the difference is assigned to a causal effect of SSP. Consistent with this interpretation, the estimates of the parameter k_0 from the eligibility model are positive and highly significant in columns (2)-(4). The implication is that the distribution of the random effects among those who became eligible is much different than the distribution among the ineligibles. For example, simulations from the model in column (2) of Table 5 show that the median of the α_i's for the eligible program group is -0.98, while the median for the ineligible group is 0.36. (By assumption the mean and median of the α_i's is 0 for the overall population).

The bottom rows of Table 6 show goodness of fit statistics summarizing each model's ability to

[39]The 1/(t-2) term is included to capture the fact that the hazard of eligibility falls from 6 percent in month 3 to around 2.5 percent in months 4-10.

predict the distributions of the control and program groups across the 50 cells used in Table 5. Perhaps unsurprisingly, the specification in column (1), which treats eligibility status as exogenous, provides a slightly better fit than the specification in column (2), which treats it as endogenously determined, although the eligibility model is flexible enough to provide an accurate prediction of the fraction who achieved eligibility, and a reasonable fit to the distribution of months to eligibility.[40]

The model in column (3) generalizes the specification in column (2) by allowing the SSP treatment effects to vary with the random effects. The interaction term is especially large for the transitional period effect on IA exits, and implies that SSP eligibility raised the log-odds of leaving welfare more for people with higher values of the individual effect α_i (i.e., those who were less likely to leave in the absence of the program). Indeed, the predicted probabilities of leaving IA from this more general model are roughly the same for people with different values of the α_i's. Since most people who became eligible for SSP were off IA for at least a month in the transitional period, the generalized model gives a better description than one that assumes a homogeneous effect on the log odds.

The specification in column (4) introduces an additional degree of flexibility by including interactions of α_i with a quadratic in months since random assignment. We developed this model out of concern that imposing a homogeneous trend might inadvertently bias our estimated treatment effects, since the eligible program group has a non-random distribution of α_i's. As with the other interaction terms, the trend interactions are statistically significant, although their introduction has little effect on the size of the estimated treatment effects. The specification with trend interactions provides a slightly better goodness of fit for the program group than a comparable model without these terms, but implies very

[40]The models in columns 2-5 of Table 6 all give very accurate predictions for the fraction of the program group who achieved eligibility. Plots of the actual and predicted eligibility hazards from the models also show a good fit apart from the last 2 months. The models under-predict the hazard in month 14 and over-predict the hazard in month 15. The root mean squared prediction error from the model in column 4, for example, is 0.37% (relative to an average hazard rate of 2.89%). Excluding the last two months the root mean squared error is 0.08%.

similar treatment effects.

Finally in column (5) we adopt the same specification as in column (4), but replace the assumption of a normal distribution for the random effects with the assumption of a 4 mass point distribution. For each mass point we estimate a value for α_i, a value for the constant in the eligibility model, and the fraction of the population associated with the point. As was true for models fit to the control group only, the normal heterogeneity and 4 mass point models provide relatively similar parameter estimates, although the goodness of fit statistics are somewhat better for the mass point model. Again one of the estimated mass points is essentially infinite, implying that some fraction of the population never leave welfare. Interestingly, SSP had a significant effect on the relative size of the "never-leaver" group, reducing it from 21.7% of the control group to 17.1% of the program group. This creates something of a problem for the mass point model, which over-predicts the fraction of never leavers in the program group and under-predicts the fraction in the control group.

By including separate intercepts in the eligibility model for each mass point, the selection model in column (5) is considerably more general than the "one factor" model in columns (2)-(4). However, the estimated mass points in the welfare participation and eligibility models are very highly correlated (correlation = 0.94 across the 4 mass points) suggesting that the restriction embedded in our normal heterogeneity models may be relatively innocuous.

Table 7 compares the predictions from the models in columns (4) and (5) for the distribution of welfare histories of the program group. Overall the fits are similar, though the goodness of fit statistic is a little better for the mass point model. The two models also give very similar predictions for mean levels of IA participation in each month of the SSP experiment. In view of similarities between the estimates and predictions from the two models, we have reasonable confidence that our estimates are insensitive to the parameterization of heterogeneity.

Figure 7 shows predicted and actual IA participation rates for the program and control groups in the 69 months after random assignment, based on the normal heterogeneity model in column (4) of Table 6. (Predictions from the mass point heterogeneity model are nearly identical). Overall, the predictions are fairly accurate, although the model slightly over predicts welfare participation of the program group in the period around the close of the eligibility window (months 13-15), and also over predicts IA participation rates of both groups in months 43-50. The model explains over 99 percent of the variance in average monthly IA participation of both the program and control groups, with root mean squared prediction errors of 0.6 and 0.9 percent, respectively. (The corresponding figures for the mass point model in column (5) are 0.7 and 0.9 percent).

Further insight into the accuracy of the model is provided in Figure 8, which shows predicted and actual welfare participation rates for the eligible and ineligible program groups. The predictions for the ineligible group are relatively accurate (root mean squared error of 1.5 percent), while those for the eligible group are a little less so (root mean squared error 2.6 percent), particularly in months 13-18. The model has particular difficulty reproducing the "dip" in welfare participation just after the close of the eligibility window. A closer look at the data around this point suggests that a relatively high fraction of those who achieved SSP eligibility near the end of the eligibility window returned to IA within a few months. Such behavior is consistent with our theoretical model, which predicts that some people will take a relatively unattractive job to gain eligibility, and then quit immediately. Although our empirical model allows a bigger effect on welfare participation in the first 3 months after initial eligibility than in the subsequent entitlement period, it is evidently too restrictive to fully capture the phenomenon.

Another problem for the model is the trend in welfare participation of the eligible program group 18-36 months after random assignment. During this period the participation rate of the eligible group is very stable, whereas the model predicts a decline, particularly after month 24. The predicted trend essentially tracks the trends in the control group and the ineligible program group: both show steady declines in IA participation during months 18-36. Even allowing for heterogeneity in the trends for different values of the random effect, the best fitting model cannot explain the absence of a parallel trend for the eligible program group. The same problem is evident in the predictions from the model with mass point mixing.

Finally, it is interesting to examine the fit of the model in months 54-69, when the treatment effects are all assumed to be zero. In this interval, the average predicted welfare participation rate for the program group is a little below the actual rate, though the predicted and actual levels are nearly identical at month 69. To probe this further, we fit a model that allowed a fraction θ of the entitlement period treatment effects to persist after the expiration of SSP. For a specification parallel to the one in column (4) of Table 6 the estimate of θ is 0.43 (with a standard error 0.05), suggesting that an important fraction of the treatment effect persisted. Simulations of this model show that it does a better job of predicting IA participation of the eligible program group in months 55-64, but a worse job in months 65-69, under-predicting the rise in IA participation of the eligible program subgroup at the end of the follow up period. Based on this poor fit, and the evidence in Figure 1a of convergence in welfare participation, we believe that models that set the post expiration effects to zero provide a more robust description of the data.

e. Decomposing SSP's Effects

By simulating the models in Table 6 with the various treatment effects turned on or off it is possible to gain some additional insights into the behavioral responses of the program group, and in particular into the "hump shaped" pattern of SSP impacts on IA participation rates shown in Figure 1a. Figure 9 uses the model in column 4 of Table 6 to decompose the predicted monthly welfare participation rates of the eligible program group into selection effects, establishment effects during the transitional-period, and entitlement effects during the entitlement-period, while Figure 10 shows the predicted and actual SSP impacts on IA participation, with a decomposition of the predicted impacts into establishment and entitlement effects.

Beginning with Figure 9, we first describe the selection effect associated with the SSP-eligible program group. As a point of reference, the solid line represents the predicted path of IA participation for the control group. The dotted line shows the predicted welfare participation rate of the eligible program group in the absence of SSP. The divergence of this path from the solid line reflects the selective nature of the eligible program group. For example, in month 36 the model predicts a 46 percent IA participation rate for the eligible program group in the absence of any treatment effects, versus a 66 percent rate for the control group. Next, we plot the path of the eligible program group, taking account only of the establishment treatment effects associated with finding work and leaving IA during the eligibility window (plotted as a dotted line with squares). Although the establishment treatment effects peak just after the close of the eligibility window, they persist far longer because of the high degree of state dependence in welfare participation. Finally, the fourth line in Figure 9 (with solid squares) represents the predicted IA participation rate, taking account of selection and the transitional and entitlement period effects of SSP on IA behavior.

Comparisons of the various paths in figure 9 show that in the first year and a half of the experiment (months 6-18) most of the overall treatment effect for the eligible program group derived from the establishment effects. Over the period from the 18^{th} to 36^{th} month this effect gradually dissipated and the entitlement effects dominated. Starting in month 36 and continuing through month 52 members of the eligible program group gradually exhausted their three years of supplement eligibility, and the treatment effect faded out. Finally, after month 52 all treatment effects ended, and the eligible program group gradually returned to their path in the absence of any treatment. Although not shown in Figure 9, we have also decomposed the entitlement-period effects on IA participation into a component due to faster IA exits, and a component due to slower IA entry. Roughly three quarters of the overall entitlement-period effect is attributable to faster welfare exit rates, while one-quarter is attributable to reduced welfare entry rates.

We have conducted simulations of the other models in Table 6 and decomposed the predicted treatment effects from these models using the same approach as in Figure 9. The results are fairly similar across specifications. In particular, the models in columns 4 and 5 lead to very similar predictions for the various combinations of treatment effects. All the models suggest that the time profile of the SSP impact on IA participation on the eligible program group was driven by the combination of the one-time establishment incentive associated with the eligibility rules, and the longer run entitlement effect on welfare entry and exit rates that ended once individuals' SSP eligibility expired. The establishment-incentive impact reached a peak of about −20 percentage points at 15 months after random assignment, accounting for 55 percent of the overall impact on the eligible program group at that point. By three years after random assignment, this effect had faded and accounted for 15 percent or less of the total impact on welfare participation. The impact of the entitlement-period effects peaks at about −20 percentage points by two years after random assignment, and is fairly stable over the next year before dissipating as people come to the end of their three-year supplement entitlement window.

Figure 10 presents a decomposition of SSP's predicted impacts on the overall behavior of the program group relative to the control group, along with a comparison of the predicted and actual differences in IA participation of the two groups (using the model in column 4 of Table 6). The distinctive "V-shaped" profile of the predicted impacts is attributed to the combination of the two SSP incentive effects. Overall, the predicted and actual impacts are fairly close, although as noted earlier our model has some difficulty tracking the negative trend in impacts between months 24 and 36. The pattern of predicted and actual treatment effects in months 54-69 is also worth emphasizing. In the first part of this interval our model tends to under predict SSP's impact on IA participation of the program group relative to the controls, while by month 64 the predictions are very close. On average in the post-eligibility period, then, the predicted treatment effects are slightly too small. This explains why a specification that allows a post-eligibility treatment effect shows some evidence of persistence.

f. Evaluating Alternative Programs

Although our model of SSP is not structural, it can be used to help evaluate the impacts of alternative subsidy programs. For example, Figure 11 compares the time profile of simulated treatment effects for the actual SSP program and two variants. The first alternative has 48 months of subsidy eligibility. In our simulation, we assume that the extended entitlement period has no effect on the eligibility process: thus, we simply "turn on" the entitlement period treatment effects for an 12 extra months. Arguably, this is a lower bound on the impact of the alternative program, since a longer entitlement period would, presumably, involve a larger option value and encourage more people to become eligible for SSP. Even ignoring any effect on eligibility, however, our results suggest that an extended entitlement period would have led to a treatment effect on the order of 7 percent in months 48-54, roughly double the observed impact.

The second alternative has a slightly relaxed eligibility rule: in particular, the establishment period is extended by 3 months. We simulate this alternative by modifying the time limit parameter T_e in the eligibility model (equation (5)). This has no effect on the timing of eligibility for people who would have been become eligible under the original program, and simply continues the eligibility process for 3 more months, raising the predicted fraction of the treatment group who achieve eligibility from 33.8% to 39.2%. This simulation presumably overstates the impact of the extension, since some people who actually achieved eligibility near the close of the establishment window might have waited longer if the deadline was extended.[41] That said, the simulation suggests that allowing more time for people to establish eligibility would have led to at most a 20 percent larger program impact in months 18-45, and would have also shifted the peak program impact to the right somewhat. While these results show that

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[41]In reality there is not much of a "spike" in eligibility near the end of the establishment period. The hazard rate is 2.7 percent in the last 2 months, compared with a rate of 2.8 percent in the preceding 3 months.

our model can provide some insights into alternative subsidy programs, they also underscore the fact that a complete structural model is needed to fully evaluate even minor variants of the actual SSP program.

III. Conclusions

The SSP experiment produced one of the largest impacts on welfare participation ever recorded in the experimental evaluation literature. At peak, SSP generated a 14 percentage point reduction in welfare participation. The impact of the program faded relatively quickly, however. Within 18 months of the peak impact, the gap in welfare participation between the treatment and control groups of the experiment had closed by 50%, and by the end of the follow-up period the welfare participation rates of the two groups were equal.

In this paper we offer an explanation for this pattern of impacts. Unlike other experimental incentive programs, the SSP treatment group was not automatically eligible for the financial treatment. Instead, eligibility was limited to those who initiated subsidy payments within a year of random assignment. Program group members faced a powerful incentive to find a job within the time limit in order to establish their eligibility for up to three more years of subsidy payments. Since the program rules required subsidy recipients to leave welfare, this establishment incentive generated a transitory reduction in welfare recipiency in the program group. Members of the program group who achieved eligibility faced a continuing incentive to choose work over welfare throughout their entitlement period. We conclude that the combination of these two incentives provides a parsimonious explanation for both the large size and distinctive time profile of the SSP impact.

A second and related finding is that the additional work effort by the program group had no lasting impact on wages. Most of the extra hours were at jobs paying close to the minimum wage, and there was no upward trend in wages associated with the extra hours. By 53 months after random assignment, when subsidy payments had ended, the employment rates of the program and control groups were equal and the distributions of wages of the two groups were also essentially identical. Since the marginal gain in work experience was relatively small (less than one-third of a year, on average), and members of the experimental population had significant work experience before the experiment, the lack of wage growth is consistent with other evidence on the effects of work experience on wages of less skilled workers.

Overall, the findings from SSP suggest that welfare recipients respond to dynamic incentives in a manner remarkably consistent with the predictions from a simple optimizing model. On the other hand, the lack of effects on wages or long run welfare participation offers little support for the idea that temporary wage subsidies can have a permanent effect on program dependency.

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Appendix 1: A Simple Model of Work and Welfare Participation

a. Model in the Absence of SSP

We consider a discrete time search model with time measured in months. Individuals are risk neutral and discount the future at the monthly interest rate r. Net income if on welfare is b (which is paid at the end of the month). Net income if working at the wage w is $w-c$, which is accrued at the end of the month. Each month, an individual receives a single job offer with probability λ, drawn from a distribution with density f(w) and cumulative density F(w), with $\ell \leq w \leq m$. The job destruction rate is δ. Optimal behavior is characterized by a value function U(w), representing the value of holding a job that pays w, and by a value V^0 of unemployment. To derive U(w), note that for an individual who is currently holding a job with wage w, the expected return next month is:

$$\lambda(1-F(w))\{ (1-\delta) E[U(\omega) \mid \omega>w] + \delta V^0 \} + (1 - \lambda(1-F(w)) \{ (1-\delta)U(w) + \delta V^0 \} .$$

The first term in this expression represents the outcome if an offer is obtained (which occurs with probability λ) and it pays more than the current wage (which occurs with probability $1-F(w)$). In this case, with probability $(1-\delta)$ the job survives to the end of the month, and with probability δ it ends right away. The second term represents the outcome if no acceptable offer is obtained, in which case with probability $1-\delta$ the existing job survives and with probability δ it ends. With some re-arrangement, this expression becomes

$$\delta V^0 + (1-\delta)U(w) + \lambda (1-\delta) \int_w^m (U(\omega) - U(w)) f(\omega) d\omega .$$

Thus,

$$U(w) = (w-c)/(1+r) + 1/(1+r) \{ \delta V^0 + (1-\delta)U(w) + \lambda (1-\delta) \int_w^m (U(\omega) - U(w)) f(\omega) d\omega \} ,$$

or

$$\text{(A1)} \quad U(w) = (w-c)/(r+\delta) + \delta/(r+\delta)V^0 + \lambda(1-\delta)/(r+\delta) \int_w^m (U(\omega) - U(w)) f(\omega) d\omega .$$

To derive the value of unemployment, note that if an individual is currently unemployed, and will accept a job paying at least R, then (using the same arguments as above) expected value next month is:

$$\lambda(1-F(R))\{ (1-\delta) E[U(\omega) \mid \omega>R] + \delta V^0 \} + (1 - \lambda(1-F(R)) V^0 .$$

This can be re-written as

$$V^0 + \lambda(1-\delta) \int_R^m (U(\omega) - V^0) f(\omega) d\omega .$$

Thus,

$$V^0 = b/(1+r) + 1/(1+r) \{ V^0 + \lambda(1-\delta) \int_R^m (U(\omega) - V^0) f(\omega) d\omega \} ,$$

or

(A2) $V^0 = b/r + \lambda(1-\delta)/r \int_R^m \{ U(\omega) - V^0 \} f(\omega)\, d\omega$.

The reservation wage R has the property that $U(R)=V^0$. Comparing A1 and A2 shows that R=b+c.

b. Model with SSP

In the presence of SSP there are three value functions: $V_i(t)$, the value of welfare participation if not yet SSP-eligible, t months after assignment; $U_e(w,d)$, the value of a job paying a wage w if SSP-eligible with d months of elapsed eligibility; and $V_e(d)$, the value of not working if SSP-eligible with d months of elapsed eligibility. From revealed preference arguments we have the following inequalities:

$V_i(t) \geq V_i(t+1) \geq V^0$, with $V_i(13) = V^0$,

$U_e(w, d) \geq U_e(w, d+1) \geq U(w)$, with $U_e(w, 37) = U(w)$,

$V_e(d) \geq V_e(d+1) \geq V^0$, with $V_e(36) = V^0$.

The value of non-employment while still SSP eligible is

(A3) $V_e(d) = b/(1+r) + 1/(1+r)\, V_e(d+1) + \lambda(1-\delta)/(1+r) \int_{Re(d)}^m \{ U_e(\omega,d+1) - V_e(d+1) \} f(\omega)\, d\omega$,

where $R_e(d)$ is the reservation wage for an SSP-eligible person with d months of elapsed eligibility. The value of non-employment for those who are not yet eligible for SSP is

(A4) $V_i(t) = b/(1+r) + 1/(1+r)\, V_i(t+1) + \lambda(1-\delta)/(1+r) \int_{Ri(t)}^m \{ U_e(\omega,1) - V_i(t+1) \} f(\omega)\, d\omega$,

where $R_i(t)$ is the reservation wage in month t for people who are offered SSP but not yet eligible.

To derive $U_e(w,d)$, we proceed backward from period 36. We first show that in the final month of payment eligibility, the reservation wage is below R, the reservation wage in the absence of SSP. To see this, note that for a job paying a wage $w \geq R$, the individual will not quit once SSP ends. Thus, for $w \geq R$,

(A5) $$\begin{aligned} U_e(w, 36) = \;& (w-c+s(w))/(1+r) \\ &+ 1/(1+r) \{ \delta V^0 + (1-\delta)U(w) + \lambda (1-\delta) \int_w^m (U(\omega) - U(w))\, f(\omega)\, d\omega \} \\ =\;& U(w) + s(w)/(1+r) . \end{aligned}$$

Evaluating this expression at w=R, and using the fact that $U(R)=V^0=V_e(36)$, (A5) shows that $U_e(R, 36) = V_e(36) + s/(1+r)$, which implies that the minimum acceptable wage in month 36 is strictly less than R. Now consider the value of accepting a wage w<R in month 36. Knowing that she will quit the job in month 37, the value is

$$\begin{aligned} U_e(w, 36) &= (w-c+s(w))/(1+r) + 1/(1+r) \{ \delta V^0 + \lambda (1-\delta) \int_R^m (U(\omega) - U(w))\, f(\omega)\, d\omega \} \\ &= (w-c+s(w))/(1+r) + V^0 - b/(1+r) \end{aligned}$$

$= (w-c-b+s(w))/(1+r) + V_e(36)$.

The reservation wage at month 36, $R_e(36)$, has the property that $U_e(R_e(36), 36) = V_e(36)$. Using this fact, the previous expression implies that $R_e(36)+s(R_e(36)) = b+c = R$.

Finally, we show that in earlier months, the reservation wage of SSP-eligible people is $R_e(d)=R_e(36)=R_e$. Consider month 35. For any $w \geq R_e$, the value of a job paying wage w in month 35 is

$$\text{(A6)}\ U_e(w, 35) = (w-c+s(w))/(1+r)$$

$$+\ 1/(1+r)\ \{\ \delta\ V_e(36) + (1\text{-}\delta)U_e(w, 36)\ +\ \lambda\ (1\text{-}\delta)\ \int_w^m (U_e(\omega, 36) - U_e(w, 36)\ )\ f(\omega)\ d\omega\ \}$$

Also

$$\text{(A7)}\quad V_e(35) = b/(1+r) + 1/(1+r)\ V_e(36) +\ \lambda(1-\delta)/(1+r)\ \int_{Re(35)}^m \{\ U_e(\omega,36) - V_e(36)\ \}\ f(\omega)\ d\omega\ .$$

It is straightforward to show that when $R_e(35)=R_e$, equations (A6) and (A7) imply $U_e(R_e, 35)=V_e(35)$. The same argument can be applied to months 34, 33, ... Thus R_e is the optimal reservation wage during all months of SSP eligibility.

Appendix 2: Dating SSP Eligibility

The actual date that different individuals achieved SSP eligibility is not recorded. Based on the patterns in Figure 2a , we estimate the date of SSP eligibility as the earliest of three possible dates: (1) the first month of full-time employment; (2) the first month of SSP receipt, minus 1 month for the delay in processing; (3) 14 months after random assignment. The assumption that the duration of the eligibility window was 14 months, rather than 12 as stated in the SSP rules, reflects the presence of delays in processing and administrative laxity. There are only a handful of eligible program group members for whom the minimum of the first month of full-time employment and the first month of SSP receipt (minus 1) is greater than 14.

Using these dates, about 18 percent of the eligible program subgroup achieved eligibility in the first month after random assignment, 9 percent became eligible in each of the second and third months, and roughly 6 percent became eligible in each of the next 10 months. Just under 3 percent became eligible in the last possible month (month 14). Recognizing the delay between the start of SSP eligibility and leaving IA (Figure 2a) we then add 2 months to these dates for our analysis of welfare dynamics. The resulting distribution of adjusted eligibility dates ranges from 3 to 16 months after random assignment. A final decision, also made with reference to the patterns in Figure 2a, was to set the duration of the transition period to 3 months.

Table 1: Key Features of the SSP Recipient Demonstration

A. Program Eligibility

· Eligibility limited to single parents who have received Income Assistance (IA) for at least 12 months.

· Sample members drawn from IA registers in British Columbia and New Brunswick, with random assignment between November 1992 and February 1995.

· 2,858 single parents assigned to the program group, 2,826 assigned to the control group.

B. Program Features

· Subsidy payments available to program group members who work at least 30 hours per week (over a four-week or monthly accounting period), and earn at least the minimum wage

· Subsidy recipients become ineligible for IA.

· Subsidy equals one-half of the difference between actual earnings and an earnings benchmark, set at $2,500 per month in New Brunswick and $3,083 per month in British Columbia in 1993, and adjusted for inflation in subsequent years.

· Subsidy payments are unaffected by unearned income or the earnings of a spouse/partner, and are treated as regular income for income tax purposes.

· Subsidy payments are available for 36 months from time of first payment. Payments are only available to program group members who successfully initiate their first supplement payment within one year of random assignment.

· Once eligible, program group members can return to IA at any time. Subsidy is re-established when an eligible person begins working full time again.

· Employers are not informed of SSP status. Program group members apply for subsidy payments by mailing copies of payroll forms.

Table 2: Characteristics of SSP Experimental Sample

	Controls	Programs	Program Group, by SSP Eligibility Status: Eligible	Ineligible
In British Columbia (%)	52.6	53.2	50.9	54.4
Male (%)	4.7	5.2	4.6	5.5
Mean Age	31.9	31.9	31.1	32.4
Age 25 or Less (%)	17.8	17.1	18.5	16.3
Never Married (%)	48.1	48.3	48.0	48.5
Average Number Kids < 6	0.7	0.7	0.7	0.7
Average Number Kids 6-15	0.8	0.8	0.8	0.8
Immigrant (%)	13.8	13.3	12.2	13.9
Grew Up with 2 Parents (%)	59.7	59.4	62.1	58.1
High School Graduate (%)	44.6	45.7	56.9	39.9
Means Years Work Exp	7.4	7.3	8.6	6.7
Working at Baseline (%)	19.0	18.2	31.5	11.4
Months on IA Last 3 Years	29.6	30.1	29.2	30.6
IA Continuously Last 3 Yrs. (%)	41.5	43.8	36.3	47.7
Percent on IA by Months Since Random Assignment:				
Month 6	90.8	83.1	62.8	93.5
Month 12	83.7	72.4	39.1	89.4
Month 18	77.9	65.9	27.2	85.6
Month 24	73.0	63.3	26.5	82.1
Month 36	65.4	58.8	27.6	74.8
Month 48	56.7	53.5	29.3	65.9
Month 60	50.6	48.4	28.5	58.5
Month 69	45.0	45.0	25.4	55.0
Number Observations	2,786	2,831	957	1,874

Note: Sample includes observations in SSP Recipient Experiment who were on IA in the two months prior to random assignment. Eligible program group is subset who received at least one SSP subsidy payment.

Table 3: Summary of Labor Market Outcomes 53 Months After Random Assignment

	Both Provinces	British Columbia	New Brunswick
Control Group Outcomes in Month 53:			
Percent Employed	41.56	39.19	44.08
	(1.02)	(1.41)	(1.48)
Percent with Reported Wage	38.26	35.63	41.08
	(1.01)	(1.38)	(1.46)
Mean Log Hourly Wage	2.17	2.36	1.99
	(0.01)	(0.02)	(0.02)
Cumulative Employment Since	1.41	1.33	1.49
Random Assignment (in Years)	(0.03)	(0.04)	(0.05)
Program Group Outcomes in Month 53:			
Percent Employed	41.69	37.73	46.05
	(1.00)	(1.36)	(1.47)
Percent with Reported Wage	39.45	35.04	44.31
	(0.99)	(1.34)	(1.47)
Mean Log Hourly Wage	2.15	2.34	1.99
	(0.01)	(0.02)	(0.02)
Cumulative Employment Since	1.68	1.55	1.82
Random Assignment (in Years)	(0.03)	(0.04)	(0.05)
Difference: Program Group - Control Group			
Percent Employed	0.13	-1.46	1.97
	(1.43)	(1.96)	(2.08)
Percent with Reported Wage	1.19	-0.58	3.23
	(1.41)	(1.92)	(2.07)
Mean Log Hourly Wage	-0.02	-0.02	-0.01
	(0.02)	(0.03)	(0.02)
Cumulative Employment Since	0.28	0.22	0.33
Random Assignment (in Years)	(0.04)	(0.06)	(0.07)
Regression Models for Outcomes in Month 53:			
Reduced Form Equations:			
Program Group Effect in Model for Log Wage	-0.01	-0.02	0.00
	(0.02)	(0.03)	(0.02)
Program Group Effect in Model for Cumulative Work	0.37	0.28	0.46
(fit to subsample with reported wage)	(0.05)	(0.08)	(0.07)
Effect of Cumulative Work on Wage in Month 53:			
Estimated By OLS	0.049	0.046	0.051
	(0.007)	(0.012)	(0.009)
Estimated by IV, using Program Group Status	-0.032	-0.088	-0.004
as Instrument	(0.045)	(0.099)	(0.046)

Notes: Standard errors in parentheses. Sample includes 2,339 in control group and 2,418 in program group with complete employment data for 53 months after random assignment. Regression models in bottom panel are fit to subgroups of 895 control group members and 954 program group members with reported wage in month 53. Other covariates in regression models include year dummies, education, experience, high school completion dummy, immigrant status, age, indicators for working or looking for work at random assignment, and indicators for physical or emotional problems that limit work (measured at random assignment). See text.

Table 4: Estimated Dynamic Models for IA Participation of Control Group

	Models with Normally Distributed Random Effect:			Model with Mass Point Distribution of Random Effect
	(1)	(2)	(3)	(4)
Coefficient of:				
y(t-1)	5.22	5.19	4.76	4.65
	(0.03)	(0.07)	(0.06)	(0.10)
y(t-2)	--	2.19	2.03	1.84
		(0.05)	(0.05)	(0.07)
y(t-1) x y(t-2)	--	-1.39	-0.89	-0.87
		(0.08)	(0.08)	(0.09)
y(t-1) x a(i)	--	--	-0.70	-0.93
			(0.07)	(0.02)
y(t-2) x a(i)	--	--	-0.28	-0.58
			(0.04)	(0.03)
y(t-1) x y(t-2) x a(i)	--	--	0.81	0.61
			(0.08)	(0.04)
Standard Deviation of Random Effect	1.64	1.32	1.57	4 mass pts
	(0.03)	(0.03)	(0.06)	
Log Likelihood	-28,276.0	-27,225.6	-27,202.6	-27,067.4
Goodness of Fit	752.6	260.3	253.0	175.8
Generalized Residuals:				
Mean	-0.02	0.00	0.00	0.00
Variance	1.11	0.95	0.97	0.98
1st Order Correlation	-0.07	0.01	0.00	0.00
2nd Order Correlation	0.03	-0.02	-0.02	-0.03
3rd Order Correlation	0.03	0.00	0.00	0.00
4th Order Correlation	0.04	0.01	0.01	0.00
5th Order Correlation	0.03	0.01	0.01	0.00

Notes: Standard errors in parentheses. See text for model specifications. All models include fourth order trend. Models in columns 1-3 are estimated by maximum likelihood using Gaussian quadrature with 10 points. Model in column 4 has four mass points. Goodness of fit and diagnostic tests for generalized residuals explained in text.

Table 5: Summary of IA Participation Patterns of Control Group, with Comparisons to Model Predictions

Number of Transitions:

Actual and Predicted Cell Fractions from Model in Table 4, Column 3 (normal heterogeneity):

Months on IA:	0		1		2		3+ Even Sum		4+ Odd Sum		TOTAL	
0-2	**0**	*0*	**38**	*58.2*	**0**	*0.3*	**3**	*5.9*	**0**	0	**41**	*64.4*
3-8	**0**	*0*	**125**	*100.3*	**2**	*1.6*	**40**	*45.0*	**0**	*0.2*	**167**	*147.1*
9-14	**0**	*0*	**87**	*71.7*	**5**	*1.7*	**52**	*71.1*	**3**	*1.1*	**147**	*145.6*
15-20	**0**	*0*	**72**	*49.5*	**2**	*2.0*	**64**	*83.1*	**6**	*3.0*	**144**	*137.6*
21-26	**0**	*0*	**66**	*43.0*	**5**	*2.7*	**72**	*93.6*	**7**	*5.7*	**150**	*145.0*
27-32	**0**	*0*	**70**	*32.9*	**3**	*4.5*	**83**	*92.2*	**13**	*9.7*	**169**	*139.3*
33-38	**0**	*0*	**59**	*32.2*	**7**	*6.4*	**90**	*99.1*	**18**	*15.5*	**174**	*153.2*
39-44	**0**	*0*	**58**	*29.5*	**8**	*8.4*	**87**	*97.9*	**29**	*20.2*	**182**	*156.0*
45-50	**0**	*0*	**55**	*28.1*	**18**	*11.5*	**83**	*96.2*	**29**	*32.7*	**185**	*168.5*
51-56	**0**	*0*	**41**	*35.0*	**10**	*23.5*	**82**	*97.3*	**33**	*45.8*	**166**	*201.6*
57-62	**0**	*0*	**40**	*35.4*	**30**	*42.5*	**77**	*77.9*	**53**	*75.9*	**200**	*231.7*
63-67	**0**	*0*	**37**	*45.1*	**67**	*95.6*	**40**	*49.0*	**113**	*135.2*	**257**	*324.9*
68	**0**	*0*	**11**	*12.6*	**189**	*211.7*	**0**	*0.0*	**0**	*0*	**200**	*224.3*
69	**604**	*546.8*	**0**	*0*	**0**	*0*	**0**	*0.0*	**0**	*0*	**604**	*546.8*
Total	**604**	*546.8*	**759**	*573.5*	**346**	*412.4*	**773**	*908.3*	**304**	*345.0*	**2786**	*2786.0*

Actual and Predicted Cell Fractions from Model in Table 4, Column 4 (mass point heterogeneity):

	0		1		2		3+ Even Sum		4+ Odd Sum		TOTAL	
	Actual	*Predict.*	**Actual**	*Predict.*	**Actual**	*Predict.*	**Actual**	*Predict.*	**Actual**	*Predict.*	**Actual**	*Predict.*
0-2	**0**	*0*	**38**	*46.6*	**0**	*0.1*	**3**	*2.5*	**0**	0	**41**	*49.2*
3-8	**0**	*0*	**125**	*97.6*	**2**	*1.8*	**40**	*23.7*	**0**	*0.8*	**167**	*123.9*
9-14	**0**	*0*	**87**	*94.6*	**5**	*2.5*	**52**	*41.5*	**3**	*1.2*	**147**	*139.8*
15-20	**0**	*0*	**72**	*85.9*	**2**	*3.2*	**64**	*58.6*	**6**	*3.1*	**144**	*150.8*
21-26	**0**	*0*	**66**	*75.4*	**5**	*4.2*	**72**	*73.1*	**7**	*8.0*	**150**	*160.7*
27-32	**0**	*0*	**70**	*66.7*	**3**	*5.6*	**83**	*86.7*	**13**	*12.2*	**169**	*171.2*
33-38	**0**	*0*	**59**	*51.4*	**7**	*7.1*	**90**	*97.7*	**18**	*19.9*	**174**	*176.1*
39-44	**0**	*0*	**58**	*51.9*	**8**	*8.7*	**87**	*111.6*	**29**	*25.9*	**182**	*198.1*
45-50	**0**	*0*	**55**	*42.1*	**18**	*9.2*	**83**	*106.0*	**29**	*34.4*	**185**	*191.7*
51-56	**0**	*0*	**41**	*35.4*	**10**	*11.8*	**82**	*95.7*	**33**	*47.3*	**166**	*190.2*
57-62	**0**	*0*	**40**	*29.3*	**30**	*18.5*	**77**	*76.8*	**53**	*94.7*	**200**	*219.3*
63-67	**0**	*0*	**37**	*19.1*	**67**	*28.3*	**40**	*40.1*	**113**	*116.3*	**257**	*203.8*
68	**0**	*0*	**11**	*6.6*	**189**	*184.1*	**0**	*0.0*	**0**	*0*	**200**	*190.7*
69	**604**	*620.4*	**0**	*0*	**0**	*0*	**0**	*0.0*	**0**	*0*	**604**	*620.4*
Total	**604**	*620.4*	**759**	*702.6*	**346**	*285.1*	**773**	*814.0*	**304**	*363.9*	**2786**	*2786.0*

Note: Bold entries represent number of observations with the number of months on IA given in the row heading and the number of transitions off or on IA given in the column heading. Italics entries represent the predicted number of observations with the same IA participation history.

Table 6: Estimated Dynamic Models for IA Participation for Control and Program Groups

	Models with Normally Distributed Random Effect:				Model with Mass Point Distribution of Random Effect
	(1)	(2)	(3)	(4)	(5)
State Dependence Parameters:					
y(t-1)	4.78	4.72	4.68	4.69	4.60
	(0.04)	(0.04)	(0.04)	(0.04)	(0.06)
y(t-2)	1.90	1.86	1.84	1.84	1.69
	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
y(t-1) x y(t-2)	-0.87	-0.80	-0.79	-0.82	-0.77
	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)
y(t-1) x α	-0.74	-0.90	-0.77	-1.11	-0.93
	(0.04)	(0.04)	(0.04)	(0.07)	(0.03)
y(t-2) x α	-0.40	-0.35	-0.33	-0.40	-0.55
	(0.03)	(0.03)	(0.03)	(0.04)	(0.02)
y(t-1) x y(t-2) x α	0.76	0.79	0.74	1.09	0.61
	(0.05)	(0.05)	(0.04)	(0.08)	(0.03)
Treatment Parameters:					
Transitional Period					
ψ_1 (exit)	-3.02	-2.76	-3.26	-3.10	-2.82
	(0.06)	(0.06)	(0.07)	(0.07)	(0.06)
ψ_0 (entry)	-1.92	-1.63	-1.84	-1.75	-1.68
	(0.13)	(0.13)	(0.14)	(0.14)	(0.14)
ψ_1 x α	--	--	-0.53	-0.61	-0.10
			(0.06)	(0.07)	(0.02)
ψ_0 x α	--	--	-0.27	-0.22	-0.26
			(0.09)	(0.15)	(0.05)
Eligibility Period					
λ_1 (exit)	-1.35	-1.10	-1.08	-1.11	-0.86
	(0.04)	(0.05)	(0.04)	(0.04)	(0.05)
λ_0 (entry)	-0.86	-0.51	-0.69	-0.72	-0.45
	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)
λ_1 x α	--	--	-0.03	-0.07	0.35
			(0.04)	(0.06)	(0.05)
λ_0 x α	--	--	-0.26	-0.35	-0.06
			(0.04)	(0.07)	(0.05)

Note: table continues.

Table 6, continued.

	Models with Normally Distributed Random Effect:				Model with Mass Point Distribution of Random Effect
	(1)	(2)	(3)	(4)	(5)
Selection Parameters:					
constant	--	-2.23	-2.23	-2.22	-2.01
		(0.06)	(0.06)	(0.06)	(0.07)
linear trend	--	0.19	0.18	0.18	0.18
		(0.04)	(0.06)	(0.06)	(0.06)
1/t	--	0.55	0.55	0.55	0.57
		(0.08)	(0.08)	(0.08)	(0.08)
k = loading on α	--	0.21	0.21	0.29	mass-point
		(0.01)	(0.02)	(0.02)	specific
Interaction of Random Effect and Trend					
Linear trend x α	--	--	--	0.30	0.01
				(0.04)	(0.01)
Quadratic trend x α	--	--	--	-0.33	-0.01
				(0.06)	0.01
Standard Deviation of Random Effect	1.75	1.76	1.86	1.18	4 mass points
	(0.04)	(0.04)	(0.04)	(0.06)	
Log Likelihood	-57,018	-61,116	-61,032	-60,960	-60,779
Goodness of Fit					
Controls	277.1	285.8	283.3	262.5	158.1
Programs	194.1	232.6	233.8	233.7	209.8

Notes: Standard errors in parentheses. See text for model specifications. All models include fourth order polynomial trend. Models in columns 1-4 are estimated by maximum likelihood using Gaussian quadrature with 10 points. Model in column 5 has four mass points, with unrestricted mass points in selection model. See text for further description of model.

Table 7: Summary of IA Participation Patterns of Program Group, with Comparisons to Model Predictions

	Number of Transitions:											
	0		1		2		3+ Even Sum		4+ Odd Sum		TOTAL	
	Actual and Predicted Cell Fractions from Model in Table 6, Column 4 (normal heterogeneity):											
Months on IA:	**Actual**	*Predict.*	**Actual**	*Predict.*	**Actual**	*Predict.*	**Actual**	*Predict.*	**Actual**	*Predict.*	**Actual**	*Predict.*
0-2	**0**	*0*	**85**	*70.6*	**0**	*0.3*	**9**	*7.1*	**0**	0	**94**	*78.0*
3-8	**0**	*0*	**198**	*159.7*	**3**	*2.8*	**58**	*87.2*	**1**	*1.3*	**260**	*251.0*
9-14	**0**	*0*	**120**	*108.3*	**4**	*4.4*	**104**	*134.8*	**2**	*5.5*	**230**	*253.0*
15-20	**0**	*0*	**48**	*44.3*	**3**	*4.3*	**103**	*119.4*	**14**	*10.3*	**168**	*178.3*
21-26	**0**	*0*	**33**	*27.1*	**4**	*3.0*	**83**	*97.9*	**21**	*13.4*	**141**	*141.4*
27-32	**0**	*0*	**39**	*20.9*	**5**	*5.3*	**78**	*95.0*	**25**	*21.5*	**147**	*142.7*
33-38	**0**	*0*	**38**	*19.6*	**7**	*5.8*	**86**	*88.1*	**26**	*25.6*	**157**	*139.1*
39-44	**0**	*0*	**49**	*15.8*	**5**	*6.9*	**77**	*77.3*	**25**	*30.6*	**156**	*130.6*
45-50	**0**	*0*	**39**	*20.3*	**18**	*8.9*	**65**	*82.0*	**37**	*44.7*	**159**	*155.9*
51-56	**0**	*0*	**41**	*21.6*	**17**	*15.3*	**58**	*80.5*	**62**	*59.2*	**178**	*176.6*
57-62	**0**	*0*	**31**	*23.9*	**37**	*35.0*	**56**	*63.4*	**90**	*90.5*	**214**	*212.8*
63-67	**0**	*0*	**26**	*28.6*	**72**	*99.9*	**24**	*42.0*	**141**	*155.9*	**263**	*326.4*
68	**0**	*0*	**9**	*8.6*	**172**	*188.0*	**0**	*0.0*	**0**	*0*	**181**	*196.6*
69	**483**	*448.6*	**0**	*0*	**0**	*0*	**0**	*0.0*	**0**	*0*	**483**	*448.6*
Total	**483**	*448.6*	**756**	*569.3*	**347**	*379.9*	**801**	*974.7*	**444**	*458.5*	**2831**	*2831.0*
	Actual and Predicted Cell Fractions from Model in Table 6, Column 5 (mass point heterogeneity):											
	Actual	*Predict.*	**Actual**	*Predict.*	**Actual**	*Predict.*	**Actual**	*Predict.*	**Actual**	*Predict.*	**Actual**	*Predict.*
0-2	**0**	*0*	**85**	*79.4*	**0**	*0.1*	**9**	*9.4*	**0**	0	**94**	*88.9*
3-8	**0**	*0*	**198**	*177.6*	**3**	*2.5*	**58**	*65.5*	**1**	*0.8*	**260**	*246.4*
9-14	**0**	*0*	**120**	*142.0*	**4**	*4.7*	**104**	*97.6*	**2**	*4.3*	**230**	*248.6*
15-20	**0**	*0*	**48**	*71.5*	**3**	*6.4*	**103**	*96.5*	**14**	*8.5*	**168**	*182.9*
21-26	**0**	*0*	**33**	*47.3*	**4**	*6.5*	**83**	*90.7*	**21**	*15.2*	**141**	*159.7*
27-32	**0**	*0*	**39**	*36.0*	**5**	*5.7*	**78**	*90.8*	**25**	*23.8*	**147**	*156.3*
33-38	**0**	*0*	**38**	*31.8*	**7**	*5.6*	**86**	*87.8*	**26**	*30.7*	**157**	*155.9*
39-44	**0**	*0*	**49**	*27.8*	**5**	*6.6*	**77**	*89.8*	**25**	*38.3*	**156**	*162.5*
45-50	**0**	*0*	**39**	*23.1*	**18**	*9.0*	**65**	*78.7*	**37**	*44.7*	**159**	*155.5*
51-56	**0**	*0*	**41**	*22.6*	**17**	*10.7*	**58**	*70.1*	**62**	*52.8*	**178**	*156.2*
57-62	**0**	*0*	**31**	*17.5*	**37**	*14.8*	**56**	*62.6*	**90**	*86.0*	**214**	*180.9*
63-67	**0**	*0*	**26**	*12.8*	**72**	*35.5*	**24**	*30.5*	**141**	*138.0*	**263**	*216.8*
68	**0**	*0*	**9**	*6.9*	**172**	*166.4*	**0**	*0.0*	**0**	*0*	**181**	*173.3*
69	**483**	*547.1*	**0**	*0*	**0**	*0*	**0**	*0.0*	**0**	*0*	**483**	*547.1*
Total	**483**	*547.1*	**756**	*696.3*	**347**	*274.5*	**801**	*870.0*	**444**	*443.1*	**2831**	*2831.0*

Note: Bold entries represent number of observations with the number of months on IA given in the row heading and the number of transitions off or on IA given in the column heading. Italics entries represent the predicted number of observations with the same IA participation history.

Figure 1a: Monthly Income Assistance Participation Rates

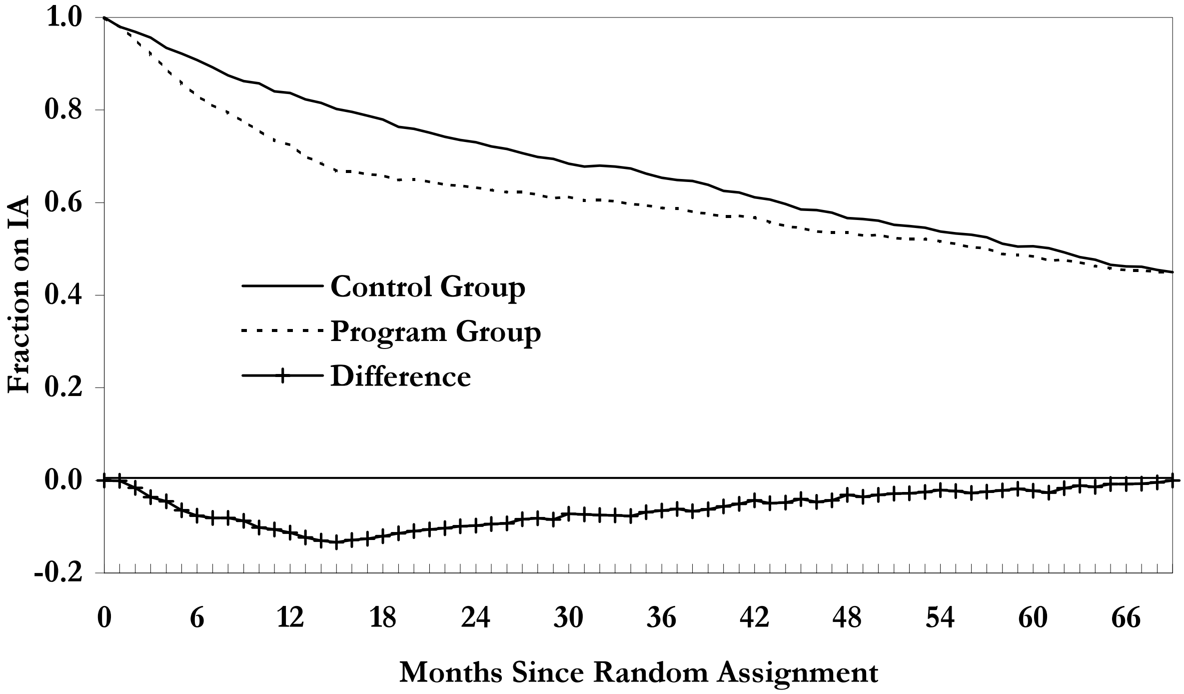

Figure 1b: Exit Rates from Income Assistance

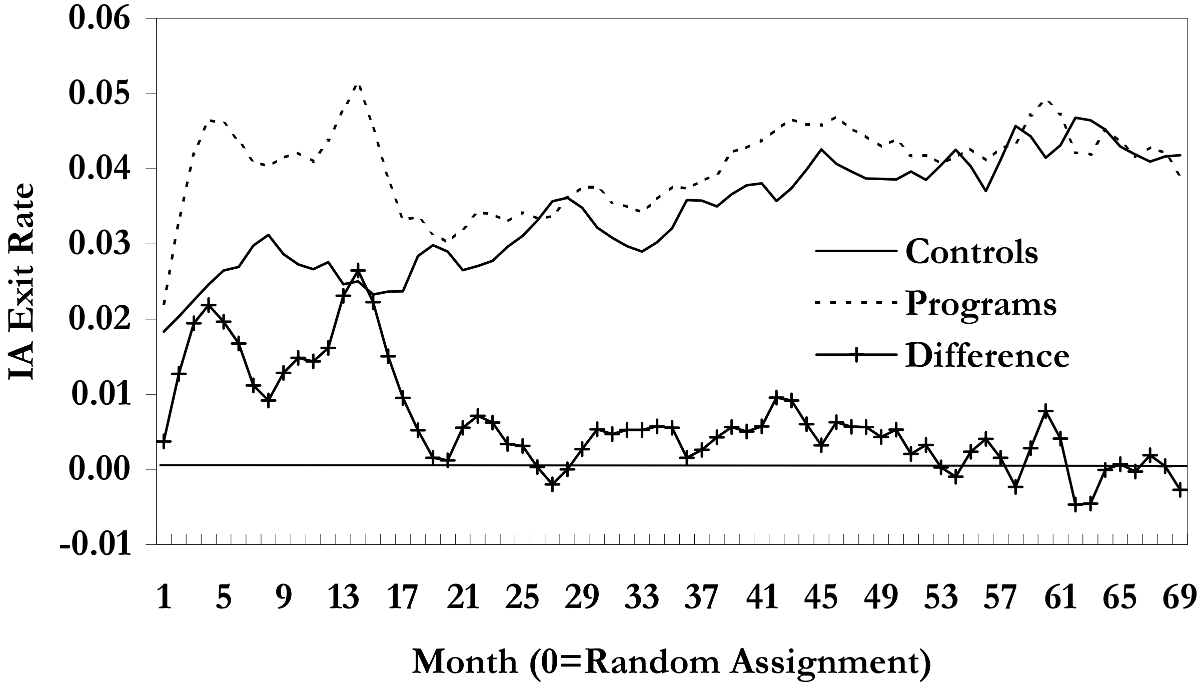

Figure 1c: Entry Rates into Income Assistance

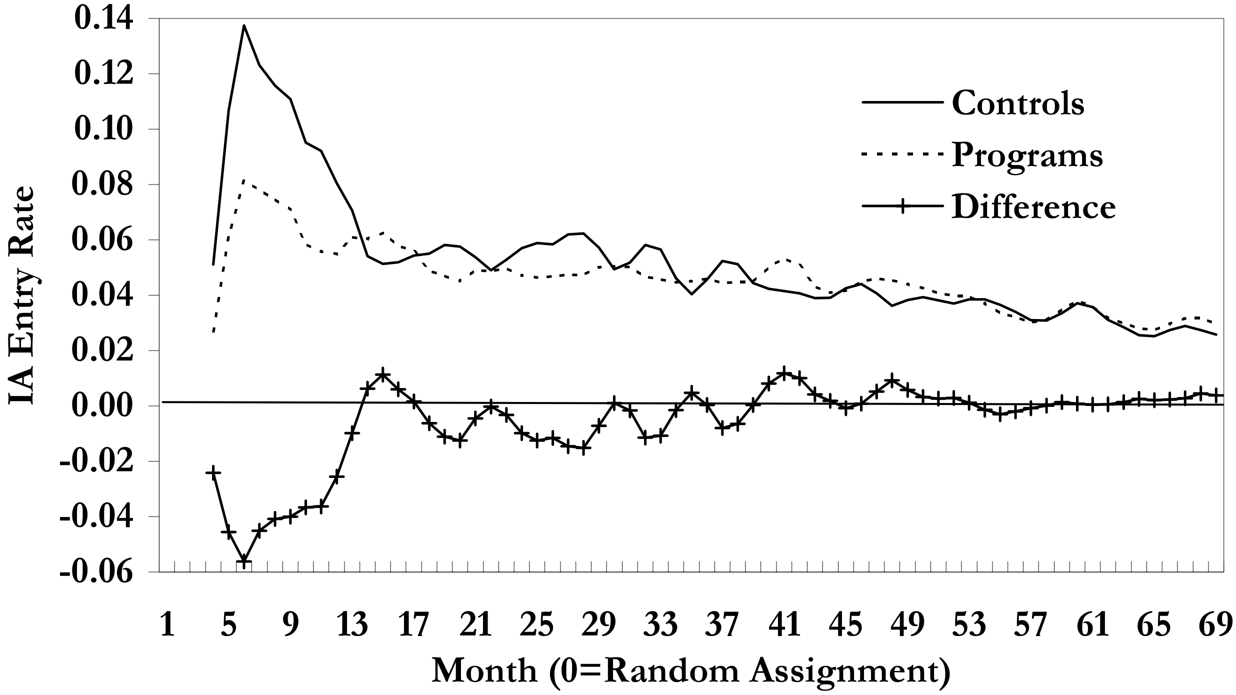

Figure 2a: Program Participation and Full Time Work Around First Month of SSP Receipt

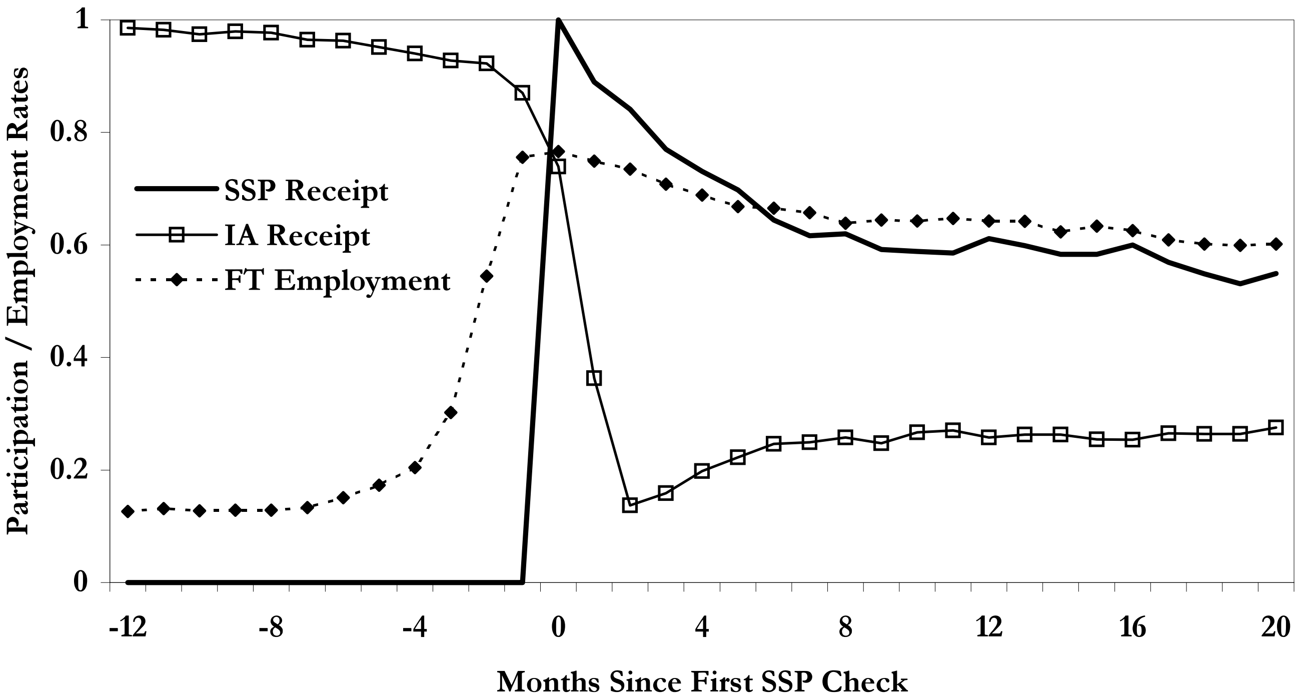

Figure 2b: Supplement Receipt and IA Participation Around End of SSP Eligibility

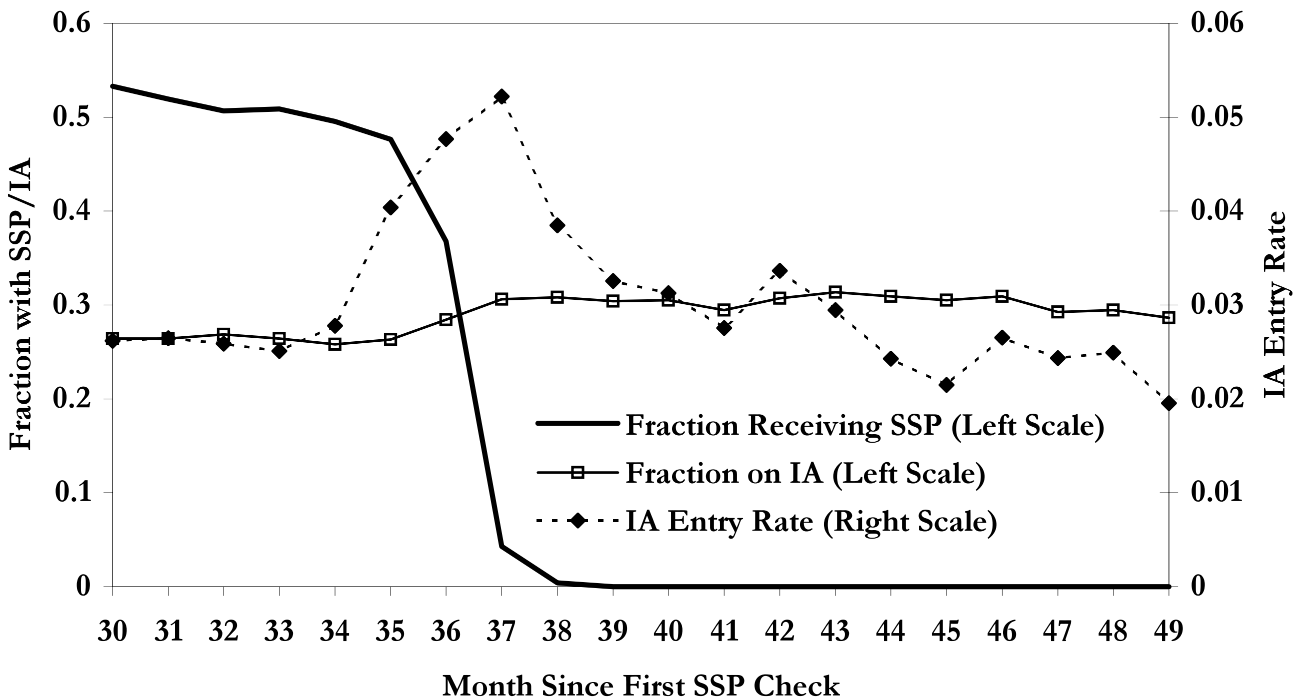

Figure 3a: Monthly Employment Rates

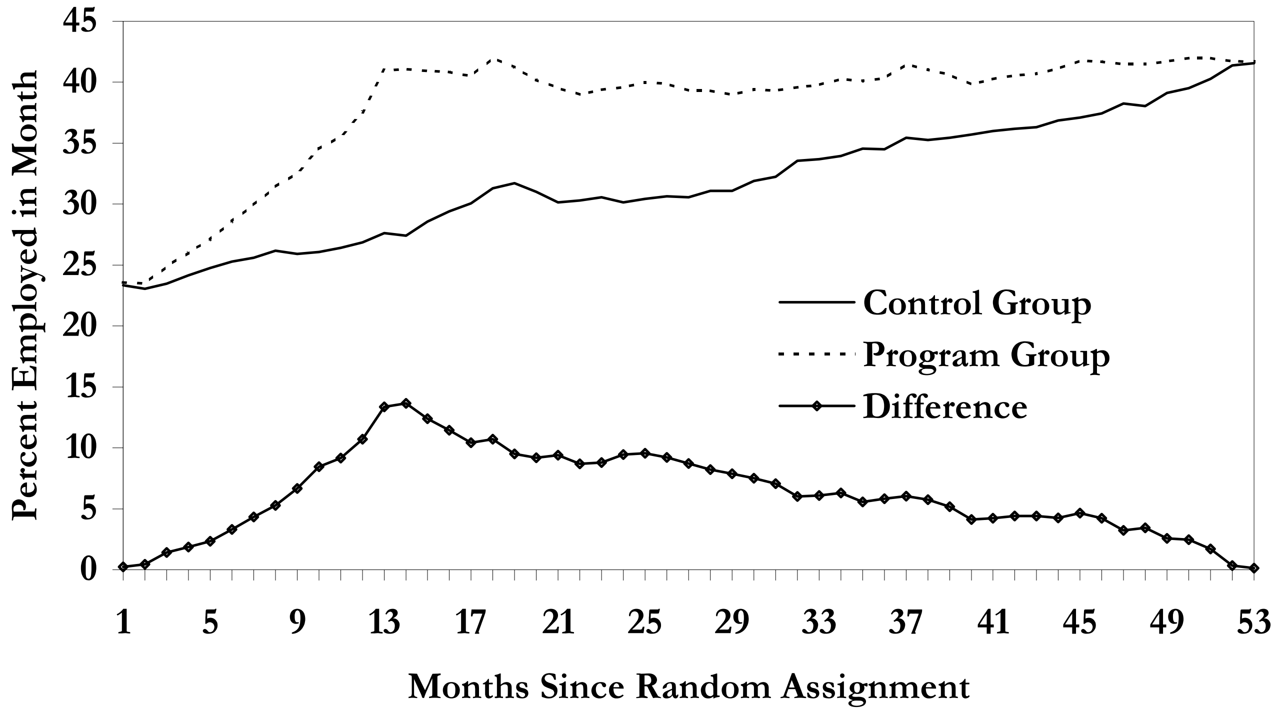

Figure 3b: Average Monthly Earnings

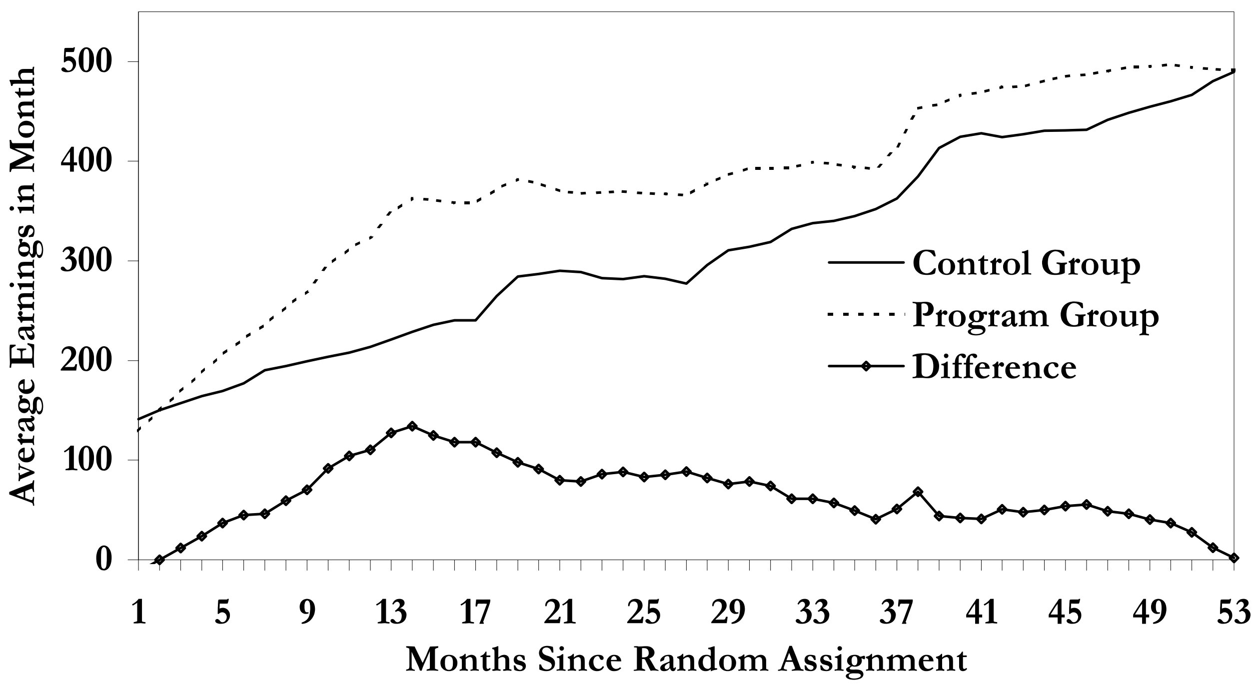

Figure 4: Distribution of Added Employment in Program Group

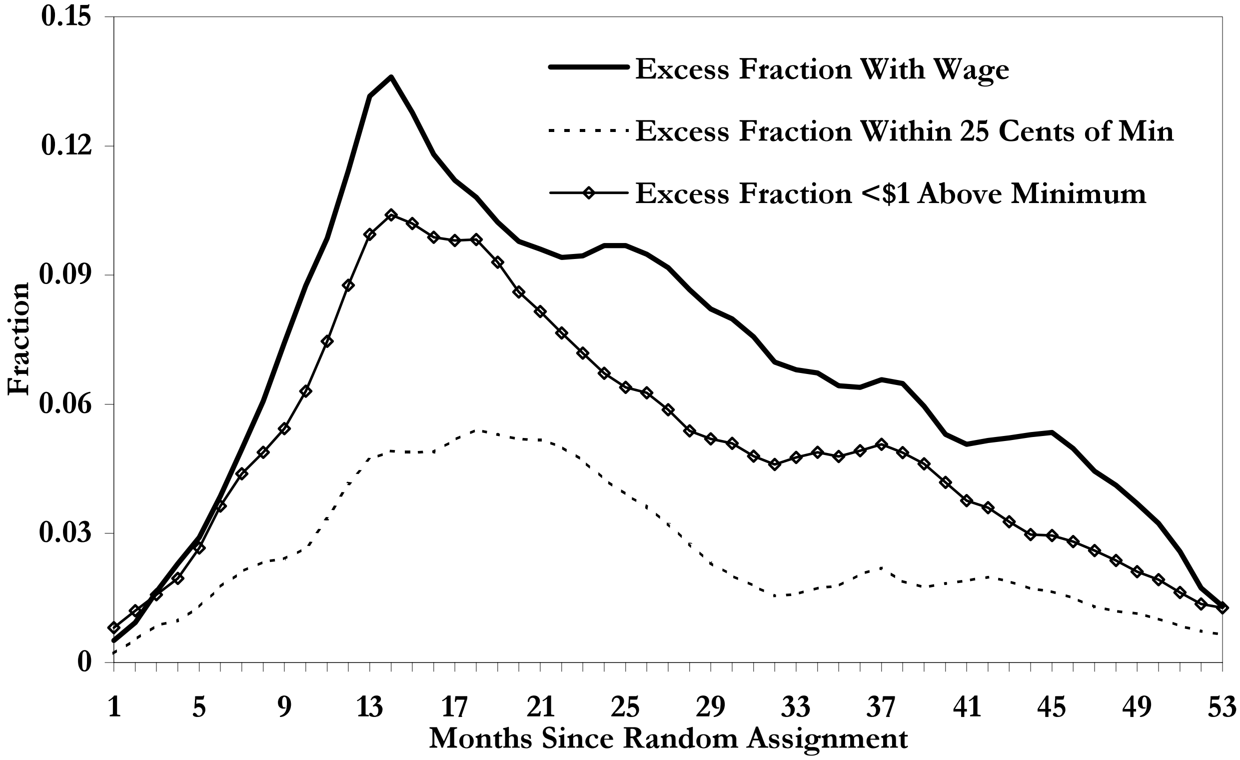

Figure 5: Average Wages Associated with Excess Earnings of Program Group

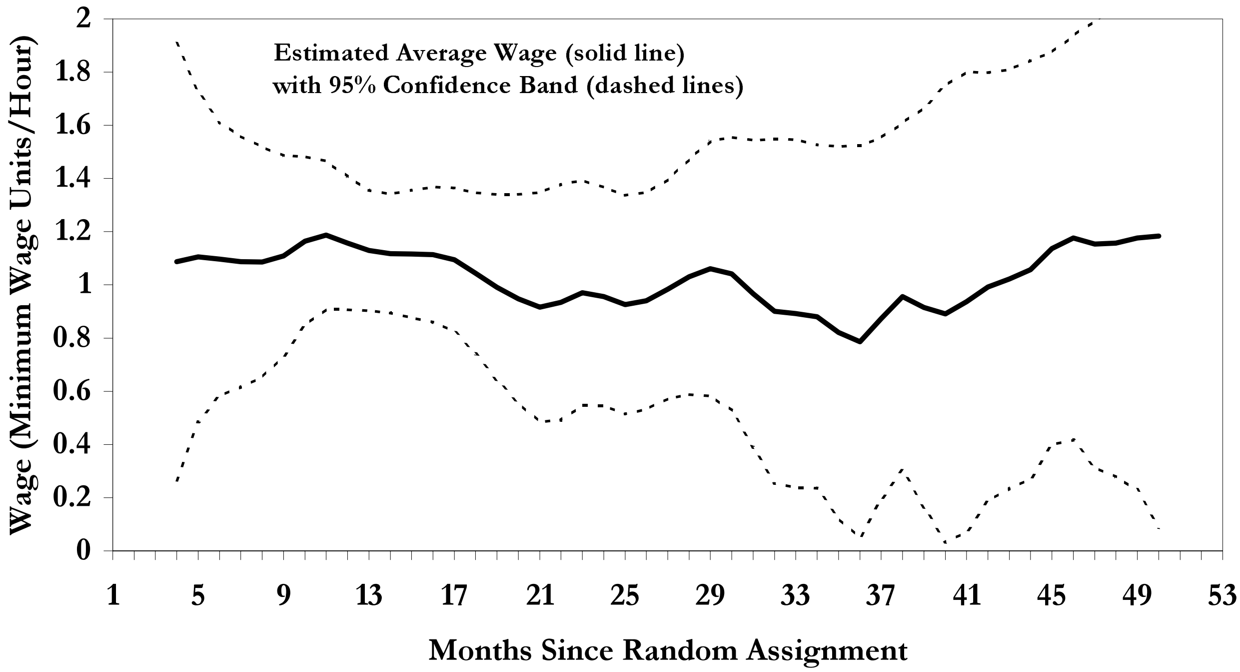

Figure 6a. Reservation Wage of Ineligible Program Group Member

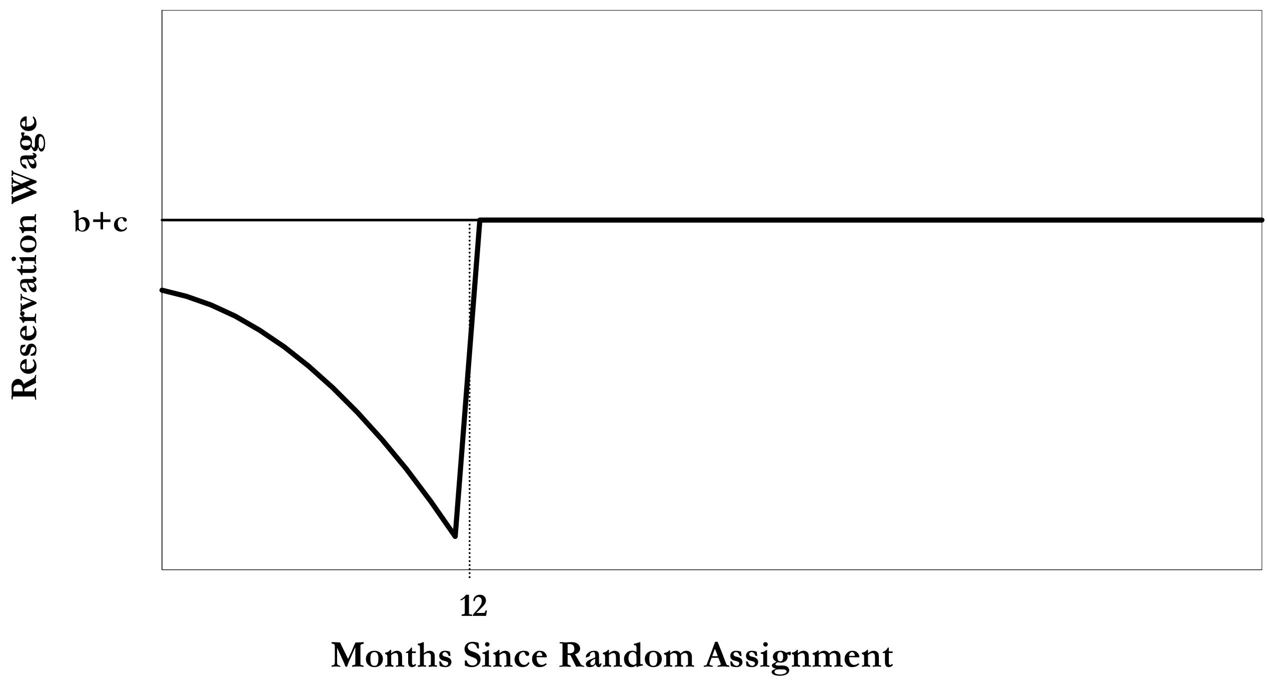

Figure 6b: Reservation Wage of Eligible Program Group Member

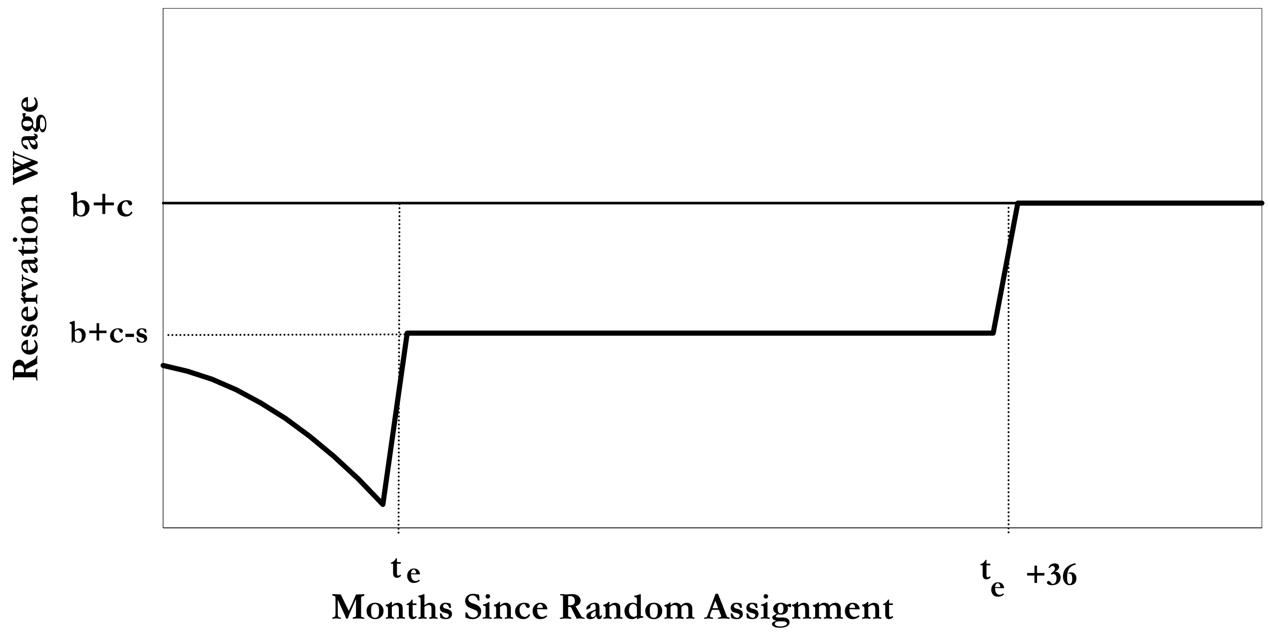

Figure 7: Actual and Predicted IA Rates for Control and Program Groups

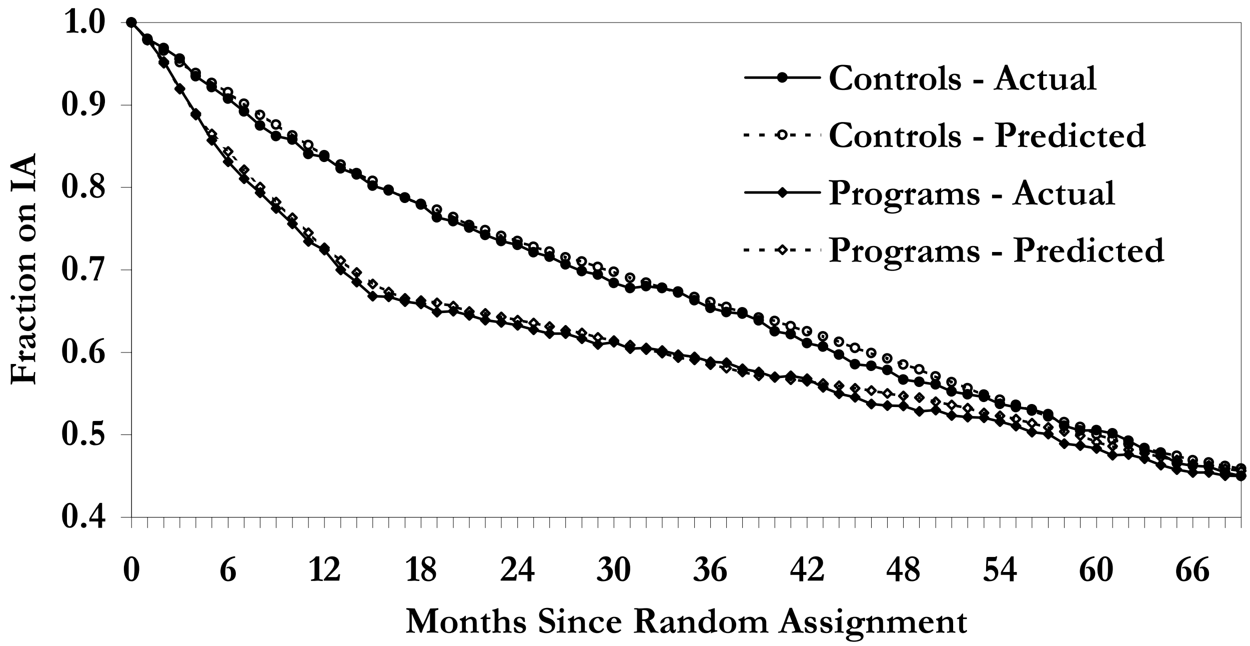

Figure 8: Actual and Predicted IA Rates for Eligible and Ineligible Program Groups

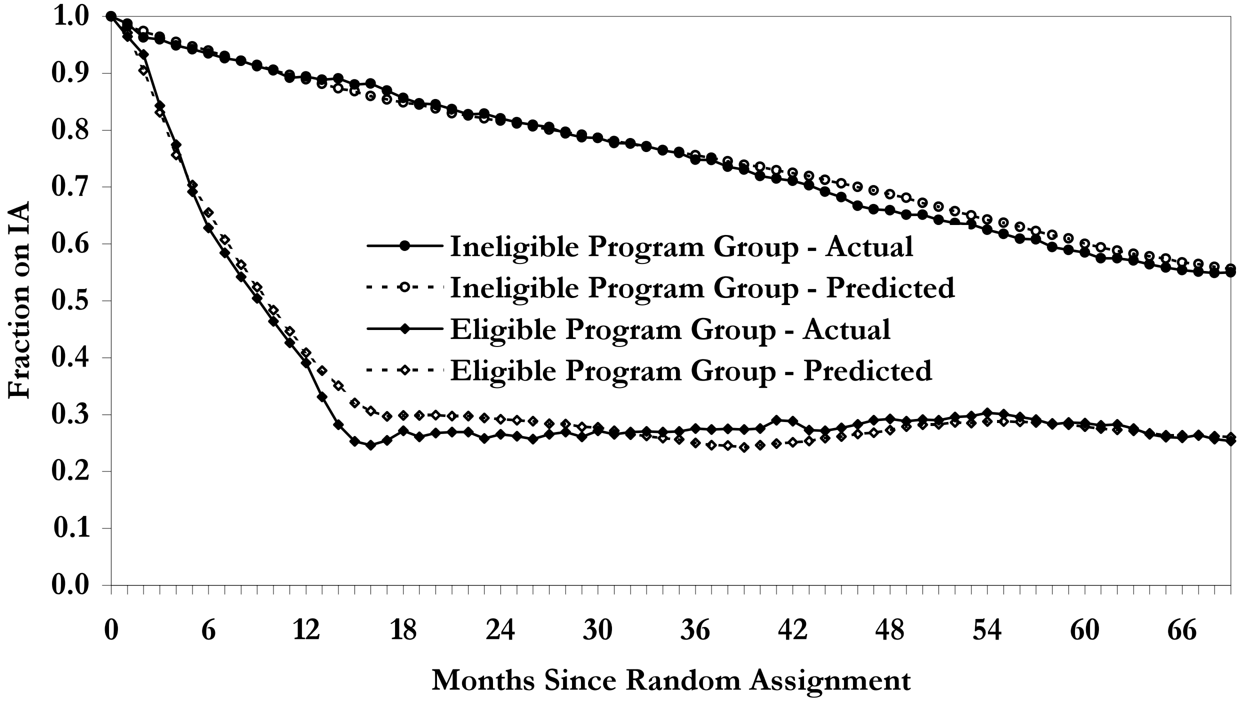

Figure 9: Decomposition of Predicted IA of Eligible Treatments

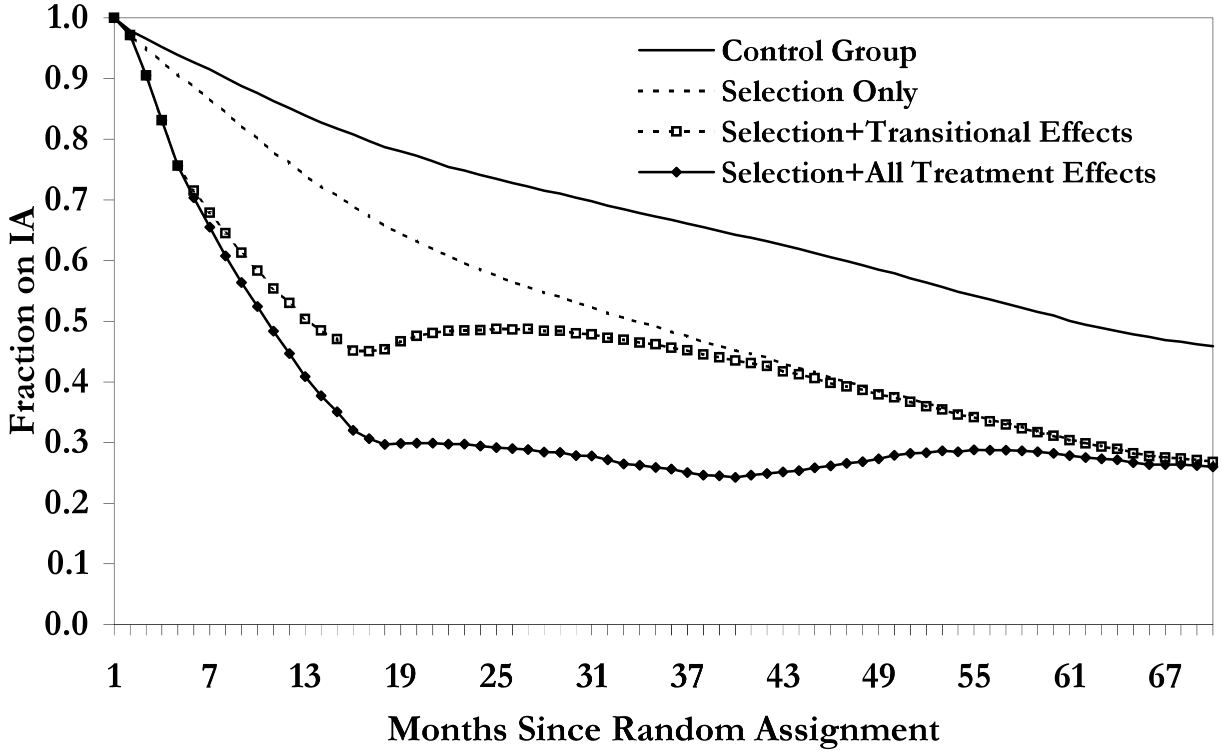

Figure 10: Actual and Predicted Treatment Effects on Probability of IA Participation

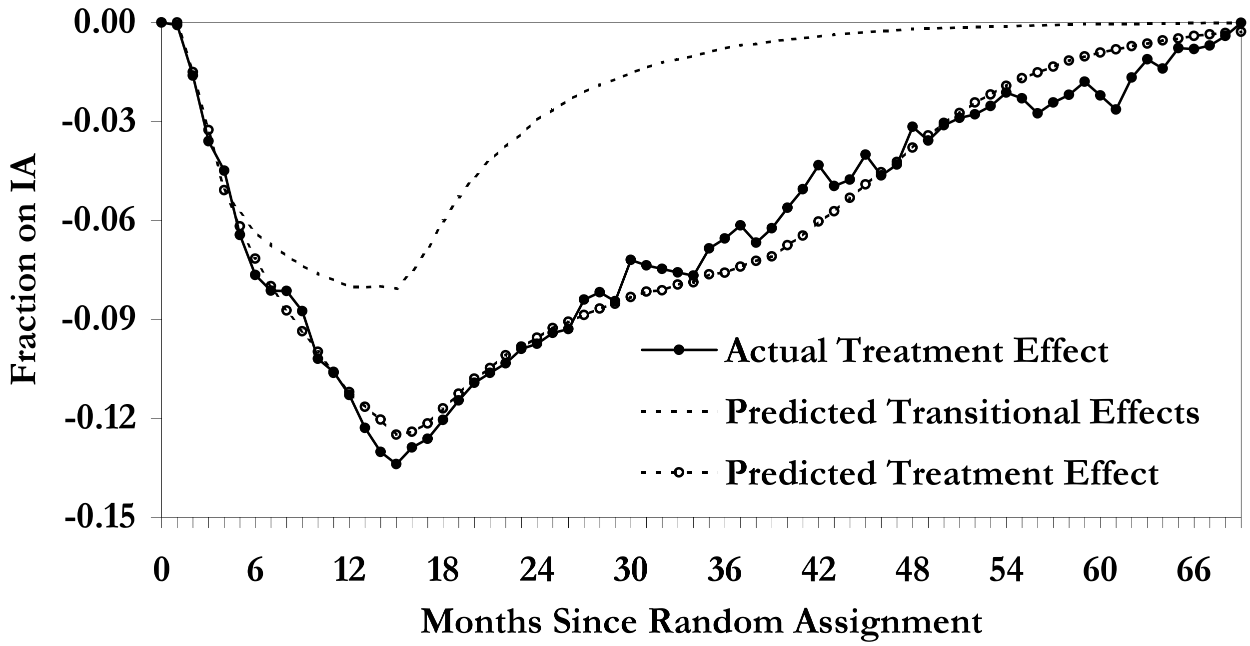

Figure 11: Simulated Treatment Effects of SSP Program and 2 Alternatives