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Estimating the Effects of a Time-Limited Earnings Subsidy for Welfare-Leavers

The Self-Sufficiency Project

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The Authors

Abstract

In the Self-Sufficiency Project (SSP) welfare demonstration, members of a randomly assigned program 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. Because of the time-limited eligibility window, SSP generated an initial entitlement incentive to find a job and leave welfare within a year of random assignment and a post-entitlement incentive to choose work over welfare once eligibility was achieved. Building on the insights from a simple search model, we develop an econometric model of welfare participation that allows us to distinguish between these two effects. The decomposition provides an explanation for the time profile of the experimental impacts, which peaked at about 15 months after random assignment and faded relatively quickly. Our estimates suggest that about half of the peak impact of SSP was attributable to the entitlement incentive. Despite the extra work effort engendered by the program's incentives, SSP had no long-run impact on wages and little or no long-run effect on welfare participation.

Introduction

Over the past decade a number of countries have reformed their income support programs. The goal of many of these reforms is to increase the financial reward for work (see for example Blundell & Hoynes, 2004; Blank, Card, & Robins, 2000). Traditional means-tested welfare systems create high tax rates, reducing or even eliminating incentives for more than a few hours of work per week. Many analysts have argued that the absence of incentives creates a "welfare trap." Once in the system, welfare recipients become increasingly detached from the labour market, and the subsequent erosion of skills and work habits makes it less likely they can leave in the future.[1]

In the early 1990s the government of Canada funded the evaluation of an earnings subsidy program — the Self-Sufficiency Project or SSP — that was designed to enhance the financial incentives for work by long-term welfare recipients. SSP typically *doubled* the payoff to work for low-wage single parents. Unlike other subsidy programs (e.g. the US Earned Income Tax Credit or the UK Working Families Tax Credit), SSP was available only to those who found full-time work. Moreover, people had to begin receiving the subsidy within a year of the initial offer — otherwise they lost all future eligibility. Once eligible, however, individuals could move back and forth between welfare and work, receiving payments whenever they were working full time. At the end of three years participants returned to the regular welfare environment.

SSP was conducted as a randomized experiment. One half of a group of long-term welfare recipients in two Canadian provinces was offered the supplement, while the other half remained in the regular welfare system. Data were collected over the next six years, permitting an analysis of the immediate impacts of the subsidy and the longer-term effects after supplement payments ended. Comparisons between the program and control groups show that SSP had significant impacts on welfare participation and work, raising the full-time employment rate and lowering welfare participation by 14 percentage points within the first 18 months of the experiment. (A complete final report on the experiment is available in Michalopoulos et al., 2002). This is a larger impact than has been observed in other recent welfare reforms experiments in the US. The effects of SSP faded relatively quickly, however. By the third year after random assignment, the difference in welfare participation between the program and control groups had fallen to 7.5 per cent, and by 69 months (a year and a half after all subsidy payments ended) the welfare participation rates of the two groups were equal.

In this paper we develop a dynamic econometric model to describe the effects of SSP on welfare participation. A key feature of our model is the incorporation of two distinct incentive effects created by the eligibility rules of SSP. A simple optimizing model suggests

[1] See Blank (1997, pp. 151–156) for a brief overview and Phelps (1994) for discussion of a general wage subsidy program. 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, Frechette, & Lemieux, 1994).

that SSP generated both a one-time "entitlement incentive" to find a full-time job and leave welfare within a year of random assignment and a "post-entitlement incentive" to choose work over welfare once eligibility was achieved. We argue that the combination of these two incentives gives a plausible explanation for the time profile of the impacts observed in the experiment. Separating the effects of the two incentives not only provides a better description of the program, but also clarifies the comparison between SSP and other incentive programs.

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. We use a logistic model with state dependence and unobserved heterogeneity to model welfare entry and exit rates. We augment this model with treatment effects associated with the eligibility process and the post-entitlement incentives of SSP. We model welfare participation and the determination of eligibility status jointly to account for the potential selectivity of the subset of the program group who became eligible for the subsidy.

Our findings confirm that people in the program group with a higher probability of staying on welfare were less likely to establish eligibility 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. We also find that over one half of the peak impact of SSP was attributable to the entitlement incentive created by the time limit on eligibility, helping to reconcile the differences in the size and time pattern of effects recorded in SSP relative to other experimental welfare reforms. Despite the extra work effort engendered by the program's incentives, SSP had little or no long-run effect on welfare participation and no lasting effect on wages.

The SSP Demonstration: Description and Overview of Impacts

INCOME ASSISTANCE PROGRAMS AND THE SSP EXPERIMENT

Under the regular welfare system available to low-income families in Canada, known as income assistance (IA), recipients who work have their welfare payments reduced dollar-for-dollar for any earnings beyond a modest set-aside amount.[1] The implicit 100 per cent tax rate on earnings, coupled with the availability of other benefits for welfare recipients (e.g. free dental services) limit the incentives for people in the system to work more than a few hours per week. Rising caseloads in the 1980s led to concerns over the costs of the Canadian welfare system. Against this backdrop, the Self-Sufficiency Project (SSP) was devised as a test of a generous earnings subsidy for long-term welfare recipients.[2] To encourage a clean break from program dependency, SSP was available only to full-time workers who left welfare, and subsidy payments were limited to a maximum of three years.

Table 1 summarizes the main features of the demonstration, including the eligibility criteria for the experimental sample and details of the subsidy formula. The SSP formula is equivalent to a negative income tax with a 50 per cent tax rate, a "guarantee level" somewhat above average welfare benefits (but independent of family size), and a full-time hours requirement.[3] The formula provides a significant enhancement to work incentives. Under the regular welfare system, a single mother in New Brunswick with one child was eligible for a maximum IA grant of $712 per month in 1994. If she[4] were to leave welfare and take a full-time job at the minimum wage, her gross income would be $867 per month — a net gain of $155 per month, or only about $1 per hour of work. Under SSP, however, she would receive an $817 subsidy, raising the relative payoff for work versus welfare to $972 per month, or $6.50 per hour. The net payoff is smaller when taxes and transfers are taken into account, but is still large (see Lin et al., 1998, Table G.1).

A key feature of SSP is time-limited eligibility. Individuals who began full-time work within a year of random assignment could receive subsidy payments over the next three years in any month they were working full time. Those who failed to initiate a subsidy payment within the allotted time lost all future eligibility. Members of the program group therefore

[1] 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.

[2] See Lin, Robins, Card, Harknett, & Lui-Gurr (1998) for a comprehensive description of the program and results from the first 18 months of the experiment; Michalopoulos, Card, Gennetian, Harknett, & Robins (2000) for a summary of results in the first 36 months; and Michalopoulos et al. (2002) for the final report on the experiment.

[3] 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 "breakeven" level $B = G/t$.

[4] The feminine pronoun is used throughout this paper because the vast majority of single parents receiving income assistance are women.

had 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 payments. Once eligibility was established, members of the program group faced continuing work incentives relative to the program group, but they could move back and forth between work and welfare without jeopardizing future availability of the subsidy.

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 available only 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.

The restricted eligibility window makes it hard to generalize the results from SSP to other settings, since some of the behavioural response to the program was arguably attributable to the entitlement incentive. The key goal of our econometric model is to disentangle the effects of the entitlement incentive from the longer-term post-entitlement incentive among those who achieved eligibility. Before turning to a more explicit consideration of the incentive effects of SSP, however, we briefly summarize the key experimental findings from the demonstration.

THE SSP RECIPIENT SAMPLE AND BASIC IMPACTS ON WELFARE

The SSP demonstration was conducted in two Canadian 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.[5] These requirements meant that nearly everyone in the sample had been on welfare continuously for at least a year. A small number of people selected for the sample left welfare in the time between their initial selection and the time that they were randomly assigned. To simplify our empirical analysis, however, we focus on the 5,617 people in SSP who were on IA in the month of random assignment and the month just before.[6] We have access to welfare records data for 69 months following random assignment — 18 months after the last SSP recipient stopped receiving subsidy payments.

Table 2 provides an overview of the characteristics of the SSP sample. The first two columns show the means for 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. Because of random assignment, the "pre-random assignment" characteristics of the program and control groups are statistically indistinguishable. The sample is mostly 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 labour market outcomes, including a low rate of high school graduation (45 per cent versus roughly 70 per cent in the adult population of Canada) and a high probability of being raised by a single parent. Nevertheless, average work experience was relatively high (7.3 years), and about 20 per cent of the sample were working at random assignment. Overall, 33.8 per cent of the program group managed to achieve eligibility for SSP payments. Comparisons of columns 3 and 4 in Table 2 show that the eligible subgroup was younger, better educated, and more likely to be working just prior to random assignment.

The lower panel of Table 2 shows welfare participation rates after random assignment.[7] Looking first at the control group (column 1) there is a gradual decline in welfare participation even without SSP. Relative to this trend, the program group shows a faster drop. The decline is especially rapid for the SSP-eligible subgroup (column 3). Of course the behaviour of this subgroup cannot be attributed solely to SSP, since some people would have left welfare and begun working even without the subsidy and these people were automatically eligible for SSP. Comparisons of IA participation in the last month of the

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[5] 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 per cent of people who were contacted to participate in the experiment signed an informed consent form and completed the baseline survey and were then randomly assigned (Lin et al., 1998, p. 8).

[6] This restriction eliminates any "initial conditions" problems. A total of 40 program group members and 27 control group members are excluded by this requirement. The difference in probabilities between the groups has a p-value of 10 per cent. Since people did not know their program status (program or control) until after random assignment, we believe that the difference is accidental.

[7] The IA data are taken from administrative records for the two provinces where the SSP sample was drawn. Some individuals may have left their original province and entered welfare in another province — such behaviour would not be captured by the available measures. As noted in the text, our sample is restricted to individuals who were on IA in the month of random assignment and the previous month. Without this restriction, the IA rates are very similar to those shown in Figure 1a, but about one half of a percentage point lower.

sample period confirm the selective nature of the eligible subgroup. By Month 69 the program and control groups had equal participation rates, suggesting that SSP had no permanent impact.[8] But the participation rate of the eligible subgroup was far below the average of the control group, while the participation rate of the ineligible subgroup was far above the control group. These comparisons highlight the positive correlation between the individual-specific determinants of welfare participation and the determinants of eligibility.

Table 2: Characteristics of SSP Experimental Sample

			Program Group, by SSP Eligibility Status	
	Control Group	Program Group	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 of kids < 6	0.7	0.7	0.7	0.7
Average number of 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 experience	7.4	7.3	8.6	6.7
Working at random assignment (%)	19.0	18.2	31.5	11.4
Months on IA in last 3 years	29.6	30.1	29.2	30.6
IA continuously for last 3 years (%)	41.5	43.8	36.3	47.7
Per cent 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 of observations	2,786	2,831	957	1,874

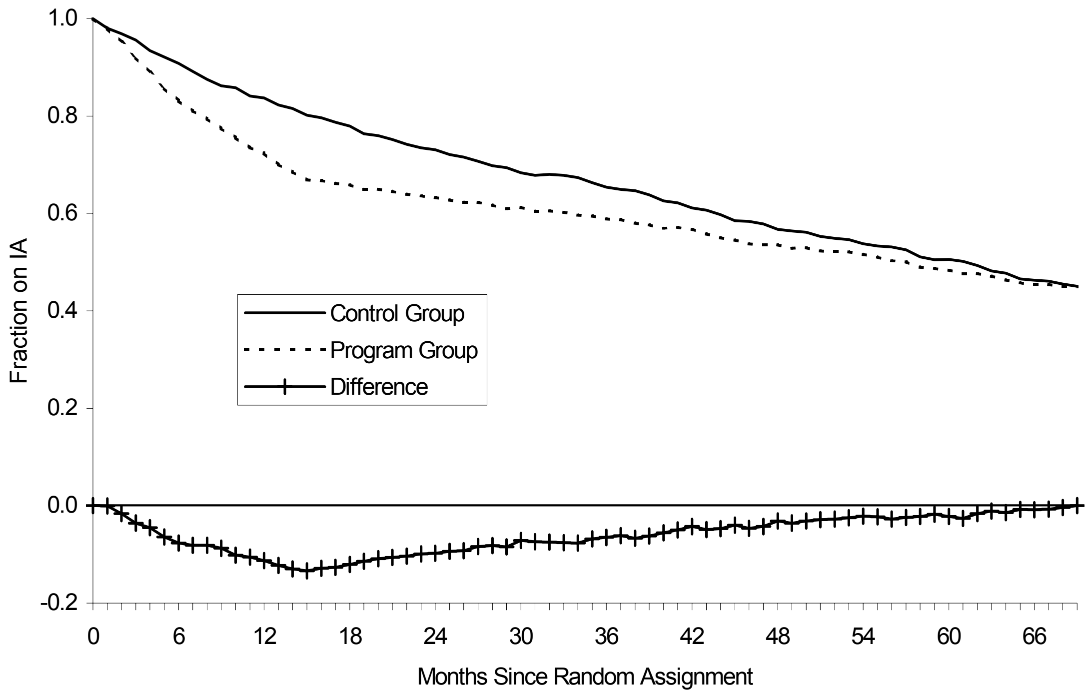
Note: Sample includes observations in the SSP Recipient experiment who were on IA in the month of random assignment and the previous month. The eligible program group is the subset that received at least one SSP subsidy payment.

More insights into the impacts of the program and the behaviour of the eligible subgroup are provided in figures 1a to 1c and 2a to 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 peaks at -14 percentage points in Month 15, declines steadily to -7 percentage points in Month 36, and then declines even faster as people who were eligible for subsidy payments came to the end

[8] 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.

of their three-year eligibility period.[9] By 52 months after random assignment all SSP payments had ended: at that point the gap in welfare participation between the program and control groups was 2.5 per cent (standard error 1.3 per cent). The gap continued to close for the remainder of the sample period, converging to 0 by Month 69.

Figure 1a: Monthly IA Participation Rates



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 nature of the risk sets for these conditional probabilities, differences in the entry and exit rates between the program and control group do not necessarily provide unbiased estimates of the causal effect of the program on transition rates. That said, the exit rates of the program group, broadly speaking, were about 1 to 2.5 percentage points above the rates for the control group in the first 15 months of the experiment, then declined rapidly to the range of about half a point higher over the period from 15 to 48 months after random assignment, and finally were about equal to the rates for the control group in the period after the end of SSP eligibility.[10] Conversely, the welfare entry rates of the program group were, broadly speaking, two to four percentage points below those of the control group in the first 15 months after random assignment, then closer to those of the control group in the period from 18 to 48 months after random assignment, and about equal to those of the control group in Month 50 and later. The program group also had relatively high welfare entry rates 15 to 18 months after random assignment,

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

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

perhaps reflecting the decision of some program group members to take a job near the end of the eligibility window, establish SSP eligibility, and then quit and return to welfare.

Figure 1b: IA Exit Rates

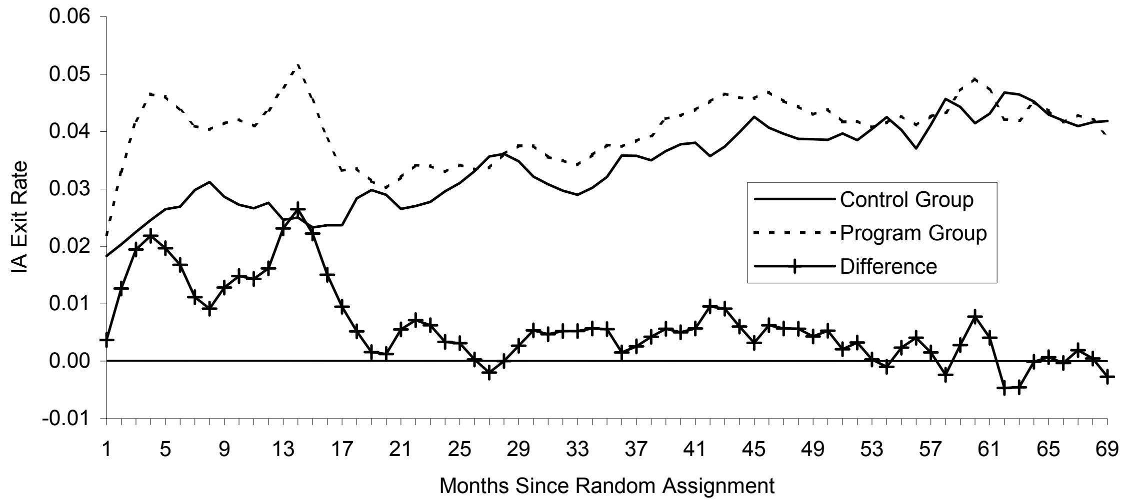
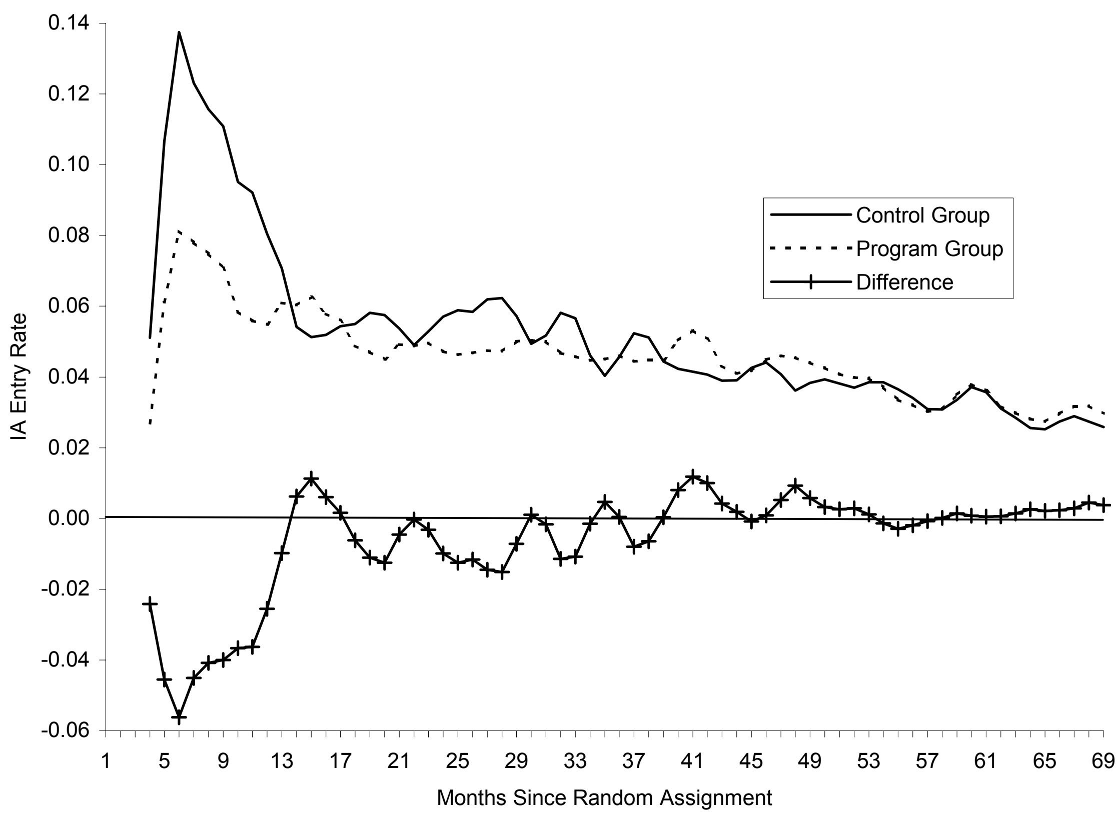
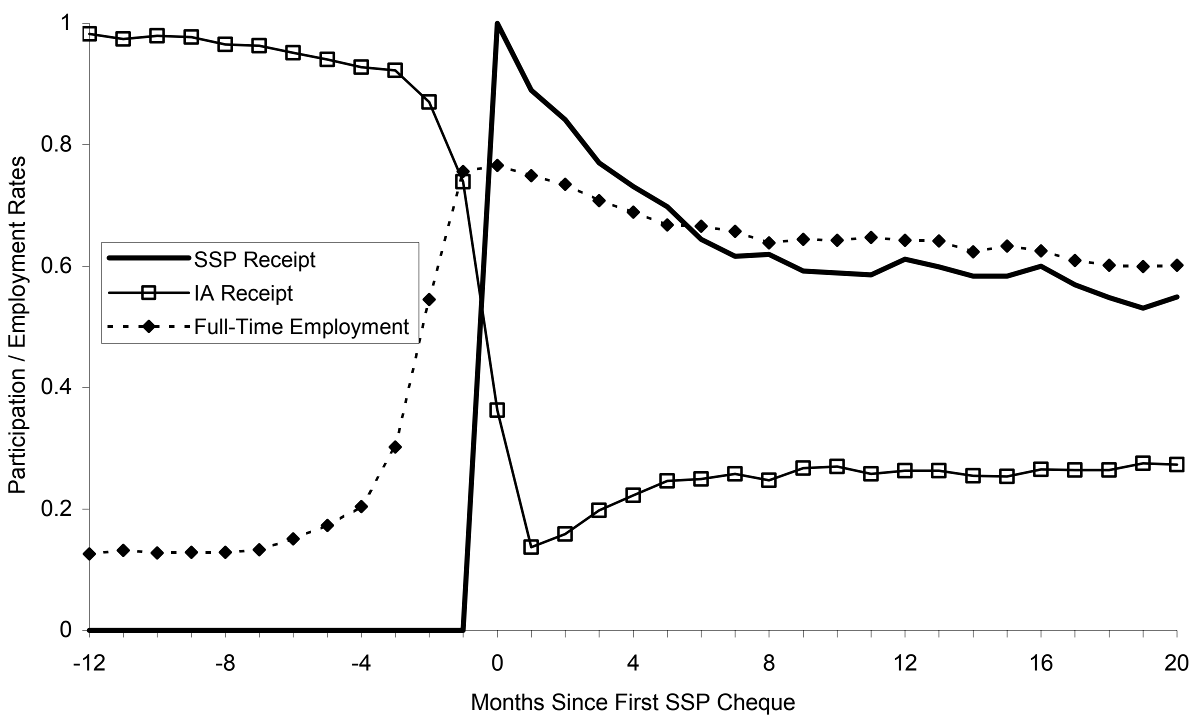


Figure 1c: IA Entry Rates



Figures 2a and 2b focus on the behaviour 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 the fraction of people receiving SSP payments around the date of their first SSP cheque (Month "0" on the graph).[11] Following the jump associated with the first cheque, the SSP recipiency rate falls off, gradually drifting down to about 60 per cent. As expected given the eligibility rules, the rate of full-time employment rises prior to the date of the first SSP cheque, reaching a maximum of about 80 per cent in the month before the cheque. Assuming that people in the program group became eligible once they started working full time, there appears to be about a one-month delay between eligibility and the dating of the supplement cheque.[12] The IA participation rates in the figure show a further one-month lag between SSP initiation and welfare leaving. SSP required supplement takers to leave IA, creating a direct mechanical link between initiation of SSP eligibility and subsequent IA participation.[13] However, IA eligibility is based on retrospective income flows, leading to a delay between the start of SSP recipiency and the end of IA.

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



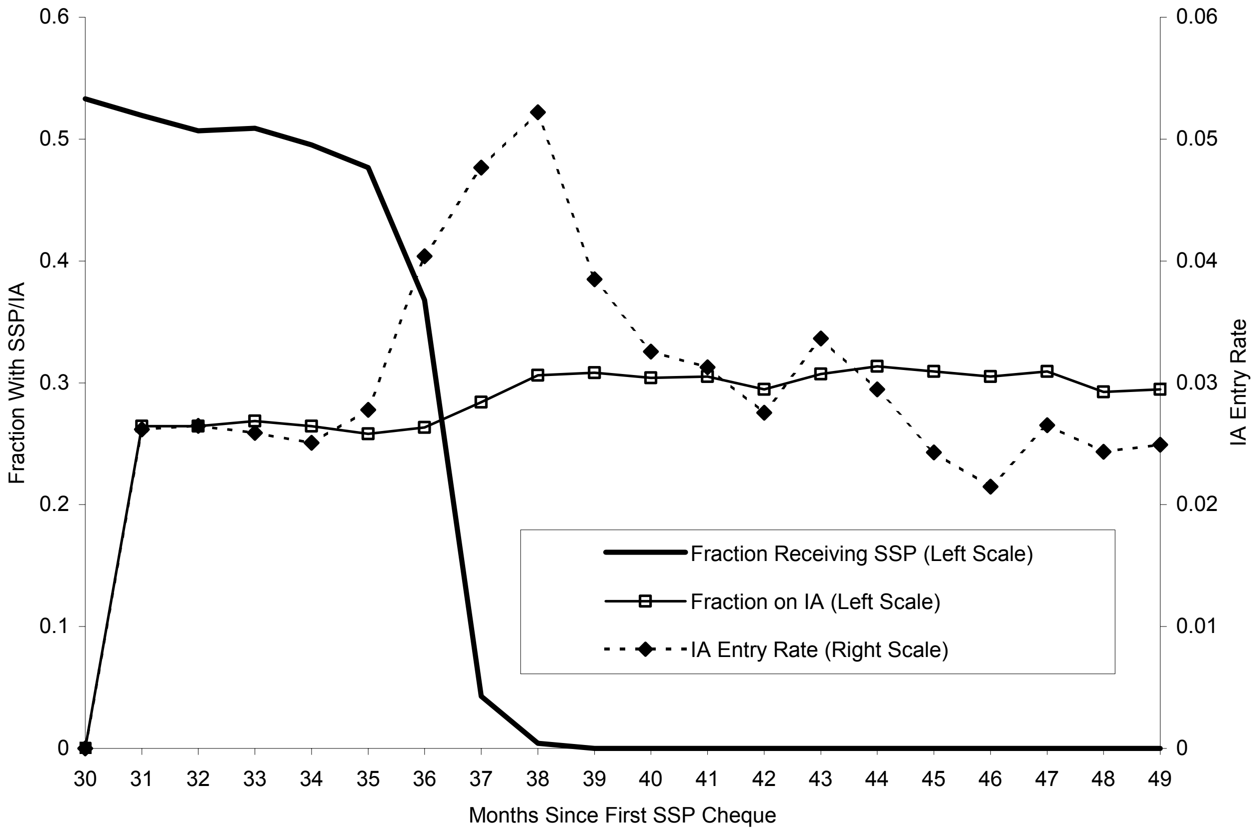
[11] As explained below, labour market data are available only for a subset of observations. Figures 2a and 2b are based on this subset.

[12] 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 cheque.

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

Figure 2b shows welfare behaviour 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 cheque. Just before the end of eligibility, about 50 per cent of the eligible group were still receiving subsidy cheques. The rate drops sharply at 37 months, reflecting the three-year maximum eligibility rule.[14] Associated with the decline in subsidy payments is a spike in IA entry rates and a roughly four percentage point rise in IA participation. These patterns suggest that some people who were SSP-eligible returned to IA as soon as their eligibility ended.

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



IMPACTS ON LABOUR MARKET OUTCOMES

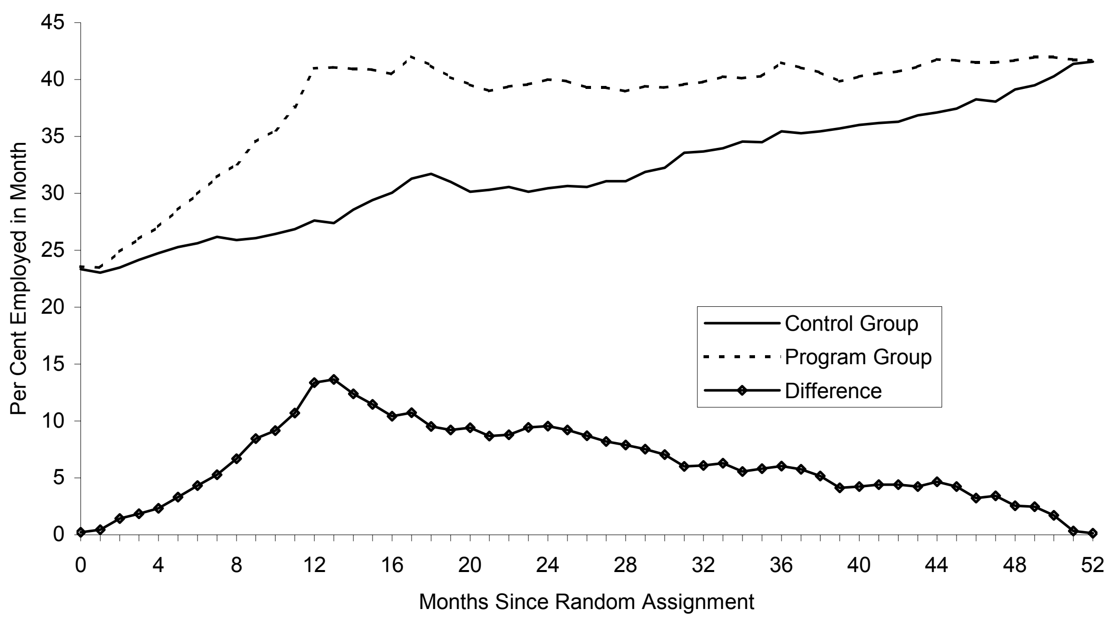
The SSP demonstration included surveys at approximately 18, 36, and 54 months after random assignment that collected labour market outcomes of the program and control groups. Relative to the income assistance data (which are derived from administrative records), these data have some limitations. Most importantly, complete data are available only for 52 months after random assignment. Since SSP payments continued for up to 52 months (for people who achieved eligibility at the last possible date), this time window is arguably too short to allow a full assessment of the long-run effects of the program. Indeed,

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

looking at Figure 1a, there is still a small impact on IA participation in Month 52 that fully dissipates only by Month 69. Second, because of survey non-response and refusals, complete labour market information is available only for 85 per cent of the experimental sample (4,757 people).[15] Third, because of recall errors over the 18 months covered by each survey, there are clear seam biases in the data. Nevertheless, the labour 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.[16] After random assignment the employment rate of the control group shows a steady upward trend. The program group shows a faster rise in the first year of the experiment, reaching 40 per cent by Month 13 and holding roughly constant thereafter. The estimated program impacts peak at about 14 percentage points in months 12 to 13, decline to about 6 percentage points by Month 36, and fall to 0 by Month 52. The earnings data have similar time profiles, although there are notable "jumps" for both the program and control groups around months 18 and 36, generated by the seams between surveys.[17]

Figure 3a: Monthly Employment Rates

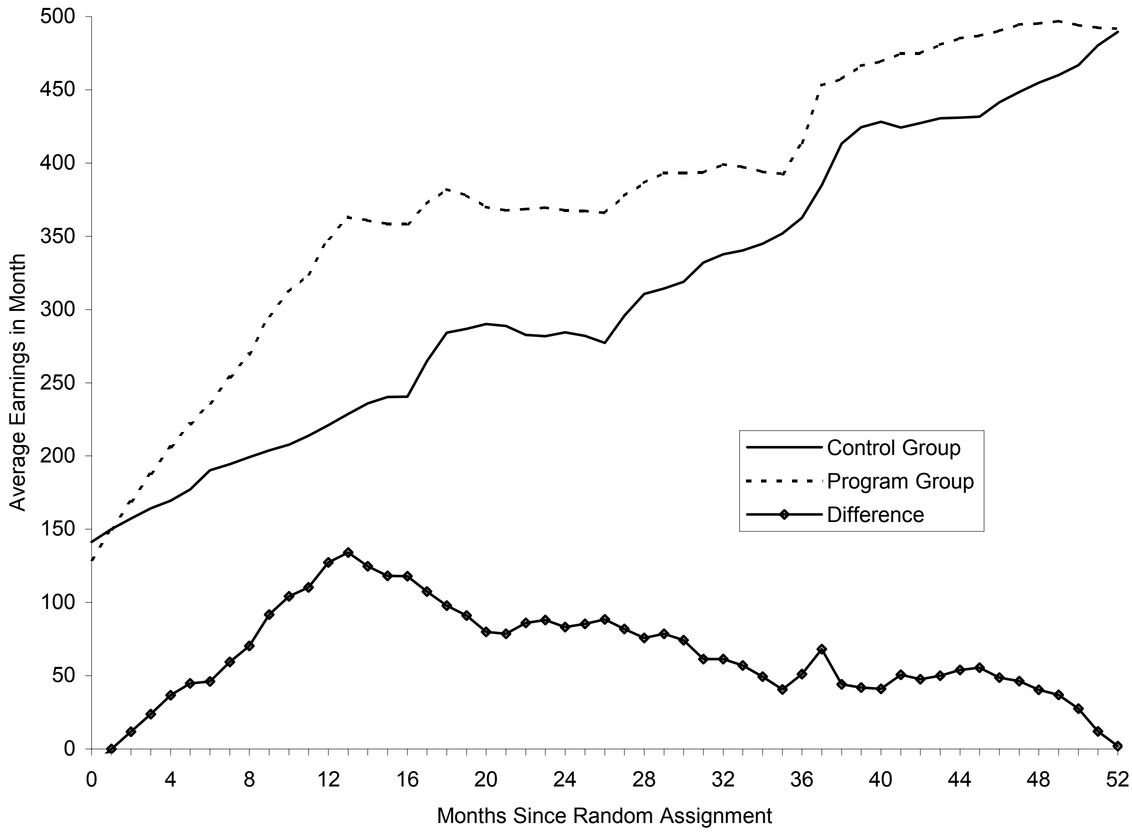


[15]The distribution of response patterns to the 18-, 36-, and 54-month surveys is fairly similar for the program and control groups (chi-squared statistic = 11.4 with 7 degrees of freedom, p-value = 0.12). However, a slightly larger fraction of the program group has complete labour market data for 52 months — 85.4 per cent versus 84.0 per cent for the control group. Moreover, the difference in mean IA participation between the program and control groups in Month 52 is a little different in the overall sample (2.5 per cent) than in the subset with complete labour market histories (3.3 per cent).

[16]Labour market information in SSP is dated relative to the month of the baseline interview date, which typically occurred in the month of random assignment. In this paper we assume that labour market information in the SSP analysis file for "Month 1" refers to the month of random assignment.

[17]Each of the three post-random assignment surveys asked people about their labour market outcomes in the months since the previous survey. Many people appear to have answered that their earnings were constant throughout 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.

Figure 3b: Average Monthly Earnings



A key issue for understanding the impacts 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 wages relatively 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 roughly equal to the program effect on the monthly employment rate, although the fraction of employed people who report the necessary information to construct an hourly wage is typically a little higher for the program group than the control group.[18] The dotted line in the figure represents the difference between the program and control groups in the fraction of people who report an hourly wage within 25 cents of the province-specific minimum wage. (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 probably an underestimate of the excess fraction of jobs that paid close to the minimum.[19] The middle line (with open square markers)

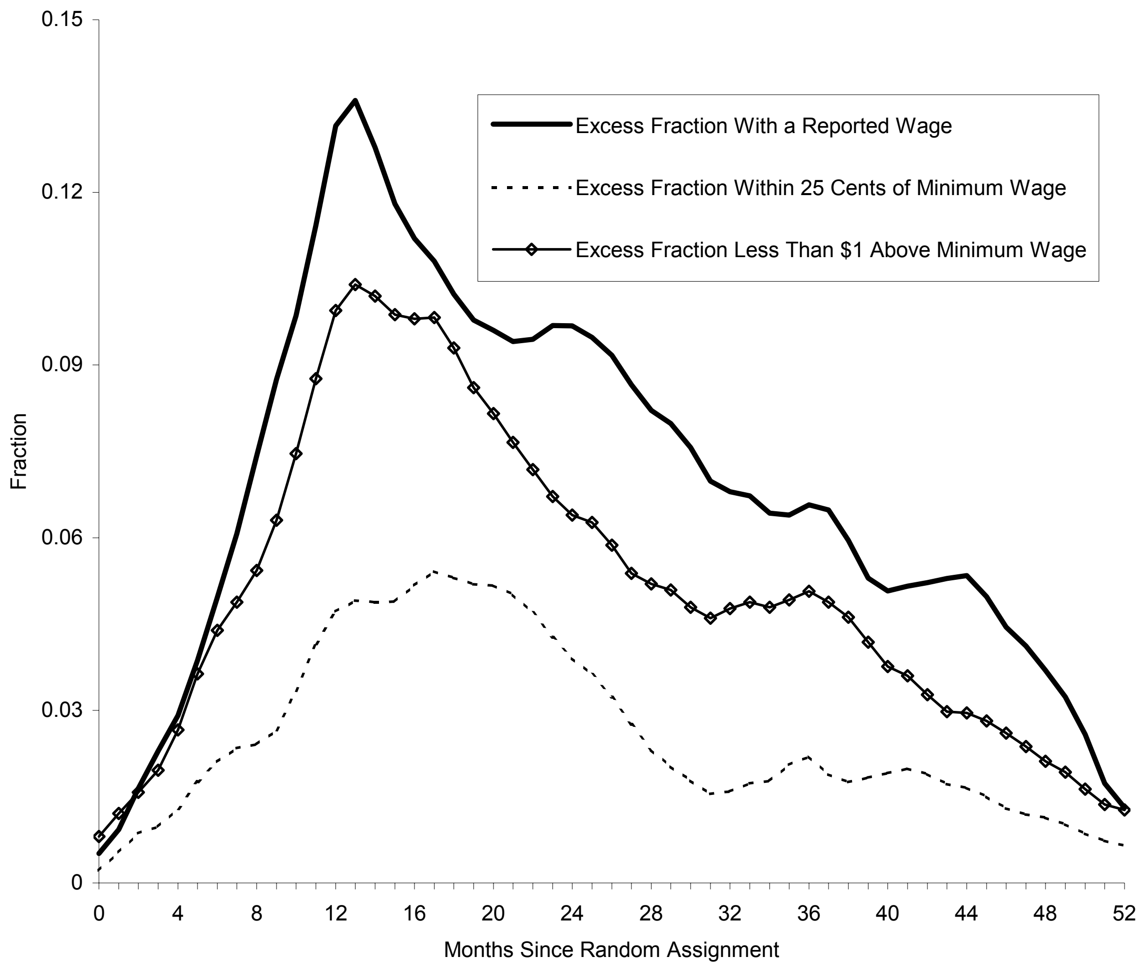
[18]This may be an effect of SSP, since informal/irregular jobs did not qualify for supplement payments.

[19]The wage data appear to be quite noisy. 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

(continued)

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 ratio. Even with potential attenuation biases, however, 60 to 80 per cent of the extra jobs in the program group paid within $1 per hour of the minimum wage.

Figure 4: Distribution of Added Employment in Program Group



Under some reasonable assumptions, the differences in earnings and hours of the program and control groups can be used to estimate the average wage rate associated with 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

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 two or more intervals on either side of the truth.

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 in month t to the corresponding difference in mean hours is a consistent estimate of

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

Suppose that wages for all the people who would have worked in the absence of SSP are unaffected by the presence of the program. Then $E[h^0_{it} \Delta w_{it}] = 0$, and

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

If $\Delta h_{it} \geq 0$ for all i (which is perhaps reasonable given the full-time hours rules of the SSP program), 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.[20] In summary, then, under the assumptions that SSP has no effect on wages for people who would have worked in the absence of the program, and only positive effects on hours, the ratio of the program impacts on earnings and hours gives an estimate of the "average marginal wage" for the incremental hours associated with the program.

Figure 5 plots estimates of this ratio, along with a 95 per cent confidence interval (estimated by the delta method). To account for differences in the minimum wage over time and across the two provinces, we have divided earnings of each individual in each month by the appropriate 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 approximately equal 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 work effort of the SSP program group was largely at minimum wage jobs.[21]

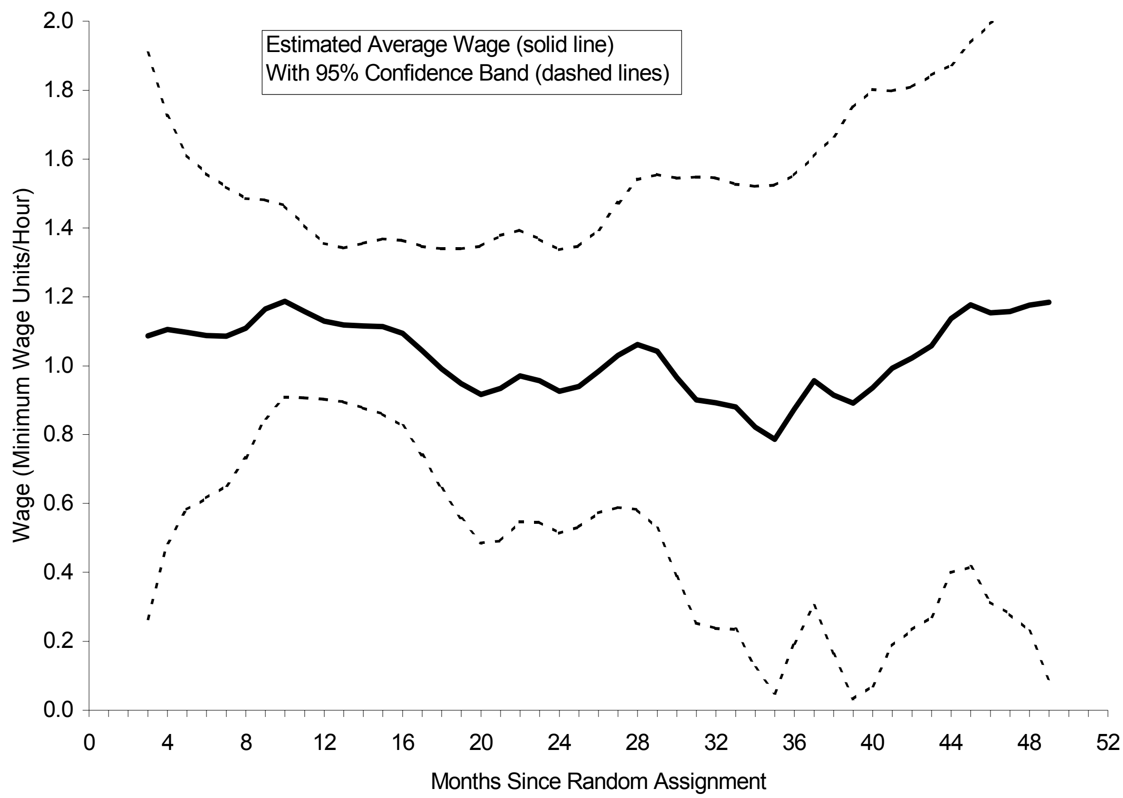
The absence of a trend in the average marginal wage relative to the minimum wage is important because it suggests that the SSP program group experienced little or no relative gain in potential wages over the course of the experiment. This is confirmed by an examination of labour market outcomes at the end of the sample period. Table 3 summarizes the data on employment and wages in the 52nd month after random assignment (which is referred to as

[20] The assumption that $\Delta h_{it} \geq 0$ is identical to the monotonicity assumption required for the interpretation of local average treatment effects (Angrist & Imbens, 1994).

[21] 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 52-month period after random assignment suggests that any impact on wages of those who would have worked regardless of SSP is small.

"Month 52"). 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.[22] By Month 52 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 four years. 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 52. Recall, however, that the sample had about seven 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 to 0.3 years of work experience for such a group is small — on the order of one to two per cent (Gladden & Taber, 2000).

Figure 5: Average Wages Associated With Excess Earnings of the Program Group



The bottom panel of Table 3 presents results from some regression models that evaluate the impacts of SSP on wages and cumulative work experience in the 52nd month after random assignment. These models are fit to the subsample of people with wage data in

[22]Wages for the labour market as a whole are 20 to 30 per cent higher in Vancouver than in the areas included in the New Brunswick sample. The minimum wage varies by province and is typically 25 per cent higher in British Columbia than New Brunswick —— for example, $5.00 per hour in New Brunswick in 1993 versus $6.00 per hour in British Columbia.

Month 52 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 wage models is selectivity bias, since the sample is conditioned on reporting a wage in Month 52. Given the very similar characteristics of the employed subgroups of the program and control groups and the fact that the fractions with a wage are equal, a conventional control function for selectivity bias would have the same mean value in the two groups.[23] The estimates in row (a) of the lower panel show that even conditioning on a broad set of control variables, the differences in mean log wages between the program and control groups are small and statistically insignificant. By comparison, the estimates in row (b) show that among those working in Month 52, 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 additional explanatory variable for wages in Month 52. 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 five per cent per year. When program group status is used as an instrument for cumulative work experience (as shown in row (d)), however, the estimated effect becomes slightly negative but insignificant. Since the instrumental variables (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 work cumulative work experience, they had marginally lower wages.

Table 3: Summary of Labour Market Outcomes 52 Months After Random Assignment

	Both Provinces	British Columbia	New Brunswick
Control group outcomes in Month 52			
Per cent employed	41.56	39.19	44.08
	(1.02)	(1.41)	(1.48)
Per cent 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 random assignment (in years)	1.41	1.33	1.49
	(0.03)	(0.04)	(0.05)
Program group outcomes in Month 52			
Per cent employed	41.69	37.73	46.05
	(1.00)	(1.36)	(1.47)
Per cent 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 random assignment (in years)	1.68	1.55	1.82
	(0.03)	(0.04)	(0.05)

(continued)

[23] 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 compared the characteristics of program and control group members who report a wage in Month 52 by running a regression model to predict program group status using 24 pre-random assignment characteristics (including some 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.

Table 3: Summary of Labour Market Outcomes 52 Months After Random Assignment (Cont'd)

	Both Provinces	British Columbia	New Brunswick
Difference: program group - control group			
Per cent employed	0.13 (1.43)	-1.46 (1.96)	1.97 (2.08)
Per cent with reported wage	1.19 (1.41)	-0.58 (1.92)	3.23 (2.07)
Mean log hourly wage	-0.02 (0.02)	-0.02 (0.03)	-0.01 (0.02)
Cumulative employment since random assignment (in years)	0.28 (0.04)	0.22 (0.06)	0.33 (0.07)
Regression models for outcomes in Month 52			
Reduced form equations			
(a) Program group effect in model for log wage	-0.01 (0.02)	-0.02 (0.03)	0.00 (0.02)
(b) Program group effect in model for cumulative work (fit to subsample with reported wage)	0.37 (0.05)	0.28 (0.08)	0.46 (0.07)
Effect of cumulative work on wage in Month 52			
(c) Estimated by OLS	0.049 (0.007)	0.046 (0.012)	0.051 (0.009)
(d) Estimated by IV, using program group status as the instrument	-0.032 (0.045)	-0.088 (0.099)	-0.004 (0.046)

Notes: Standard errors are in parentheses. The sample includes 2,339 in the control group and 2,418 in the program group with complete employment data for 52 months after random assignment. Regression models in the bottom panel are fit to subgroups of 895 control group members and 954 program group members with a 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.

OLS = Ordinary Least Squares

IV = Instrumental Variables

We have also examined the entire distributions of wages in Month 52 for the program and control groups and found no significant differences between them. The 10th, 25th, 50th, 75th, and 90th 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 lasting effect on wage opportunities.

Behavioural Impacts of SSP: A Simple Benchmark Model

To help clarify the incentive effects of the Self-Sufficiency Project (SSP) and to provide a guide for our empirical model, this section outlines a simple dynamic model of work and welfare participation.[1] 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 cost of work (including child-care costs, work expenses, and the value of foregone leisure). 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 rate of offers is the same for workers and non-workers. Wage offers are drawn from a 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 in this model is that wage opportunities are exogenous to previous work effort. Based on the evidence 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 affected by SSP, the key issue was whether to accept a minimum wage job or not.

In this model, optimal behaviour 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 (or 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.[2]

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-style 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 months of elapsed eligibility; and $V_e(d)$, the

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

[2] 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.

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 function 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 available only for three years. Similarly, $V_e(d) = V^0$ 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, s(w), is strictly positive. By the same token $V_i(t)$ is decreasing in t, since the passage of time leaves less time to establish eligibility. Finally, since SSP ends after three years, $U_e(w, d)$ and $V_e(d)$ are both decreasing in months of elapsed eligibility.

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 $V_e(d) = U_e(R_e(d), d)$. Assuming that 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 non-worker 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 $R_e + s(R_e) = b + c$.[3]

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 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 are 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. Thereafter, 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. From month t_e to month $t_e + 36$ (i.e. in the period that subsidy payments are available) the reservation wage satisfies the condition $R_e = b + c - s(R_e)$. After eligibility ends (in month $t_e + 36$) the reservation wage reverts to b + c.

[3]Since $s(w) \geq 0$, the reservation wage for those who are eligible for SSP is below the reservation wage in the absence of the program — that is, $R_e = b + c - s(R_e) \leq b + c = R$.

Figure 6a: Reservation Wage of Ineligible Program Group Members

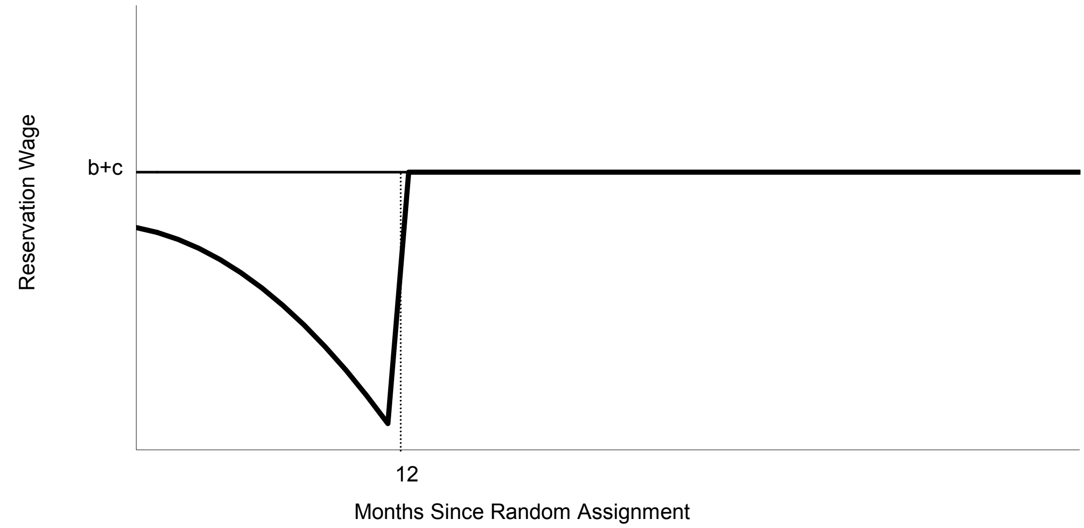
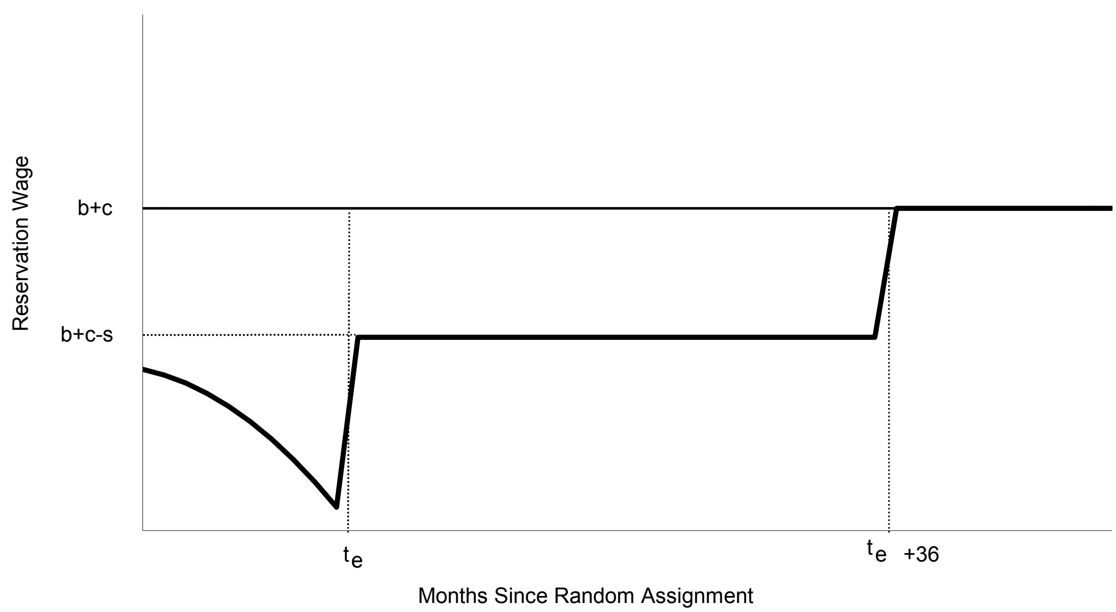


Figure 6b: Reservation Wage of Eligible Program Group Members



The path of the optimal reservation wage illustrates the three different incentive regimes experienced by the program group of the SSP experiment. During the pre-eligibility period (up to 12 months after random assignment or the establishment of eligibility), members of the program group have a low and declining reservation wage, leading to a faster rate of transition from welfare to work than would be expected for the control group. Members of the program group who achieve eligibility adopt a somewhat higher reservation wage, but still lower than they would 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. Once subsidy eligibility ends (or starting in Month 12 for those who never attain eligibility), the reservation wage returns to its level in the absence of the program, and the behavioural effects of SSP disappear. 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. Similarly, at the close of eligibility, people who were 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 obviously oversimplified. For example, the model assumes that the pecuniary and non-pecuniary costs of work are constant. More realistically, the costs of work can change over time (e.g. if a child becomes sick), leading people to revise their reservation wages and quit some jobs that were previously acceptable. A generous 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 of the model is the assumption that people either work full time or receive welfare. In fact some people leave welfare *without* entering full-time work, introducing slippage between the event of first exiting welfare and the event of first entering full-time work. In our empirical model, we therefore have to distinguish between leaving welfare and becoming SSP-eligible. Finally, the model ignores the possibility that the cost of work, c, is affected by previous work experience. A habit persistence model, for example, could have the implication that individuals who work more when SSP is available eventually lower their reservation wages. While proponents of welfare reform sometimes mention endogenous taste formation as a behavioural channel, the evidence from the SSP experiment is not particularly favourable since by Month 52, 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.

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

(1) $$P(yi1, ... y_{iT} \mid x_{i1}, ... x_{iT}) = \int\{\Pi_t L(\alpha_i + x_{it}\beta + \gamma_1 y_{it-1} + \gamma_2 y_{it-2} + \gamma_3 y_{it-1}\, y_{it-2})\}\ f(\alpha_i)\, d\alpha_i,$$

where L( ) represents the logistic distribution function and $f(\alpha_i)$ represents the density of unobserved heterogeneity among the experimental population.[1] We consider two alternative specifications for f( ). In the first case, we assume that $f(\alpha_i) = \varphi(\cdot\ ; \sigma_\alpha)$, 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.[2] As an alternative, we assume that f( ) is a discrete distribution with a small number of mass points, and we 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 the benchmark search model outlined in the previous section suggests only a first-order state dependence specification for the control group's behaviour, we show below that a second-order specification leads to a considerable improvement in model fit. There is a surprising number of one-month spells on or off welfare, and the key assumption of a first-order model — that exit or entry rates are independent of how long the current spell has been in progress — is clearly violated. A second-order specification allows a different transition rate in the first month of a spell and in all subsequent months and is more consistent with the data. Chay and Hyslop (2001) have found that logistic models with second-order state dependence provide a reasonably good fit to high frequency welfare behaviour in the Survey of Income and Program Participation. In particular, such models fit about as well as more computationally demanding multivariate probit models that allow for serial correlation in the transitory component of welfare participation.

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.[3] The model in column (1) assumes first-order

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[1]Note that, as everyone in our sample was receiving IA in the two months prior to random assignment (periods 0 and -1), $y_{i0} = y_{i-1} = 1$, and we do not have to model the distribution of initial conditions.

[2]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 $\Sigma_i\, w_i\, g(x_i)$, 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 & Stegum, 1965, p. 924).

[3]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

(continued)

state dependence, while the model in column (2) allows second-order dependence. The second-order terms are highly significant. Their pattern implies that controlling for the permanent component of welfare participation, welfare entry rates are higher for those who have been off welfare for only one month than for those who have been off two or more months, while exit rates are higher for those who have only been on welfare for one month than those who have been on longer. 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.

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

	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	
	(0.03)	(0.03)	(0.06)	4 mass points
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 are in parentheses. See text for model specifications. All models include a fourth-order trend. Models in columns 1–3 are estimated by maximum likelihood using Gaussian quadrature with 10 points. The model in column 4 has four mass points. Goodness of fit and diagnostic tests for generalized residuals are explained in the text.

the variance attributed to the random effect. However, the ability of models with a control for observable heterogeneity to fit the distribution of observed welfare histories is not much different than models that treat all heterogeneity as unobserved.

How well do these models explain observed patterns of welfare dependence? As a description of average IA participation, the answer is “very well.” The time path of welfare participation predicted by any of the models is fairly close to the actual path. This is not too 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.[4] 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 to 2, 3 to 8, 9 to 14, etc. 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.[5] The comparison of the fit statistics for the first-order model in column 1 and the second-order model in column 2 shows that adding the second-order terms leads to a big improvement in the ability of the model to predict the distribution of IA histories. 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 feature of the data is the large number of people (604 = 21.7 per cent 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 per cent of the sample with 68 months on IA and two 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 to 67 months on IA, 68 months on IA, and 69 months on IA). The model over-predicts the fraction with 63 to 67 months on IA and under-predicts the fractions with 68 or 69 months on welfare.

A computationally feasible alternative to normally distributed heterogeneity is the assumption that the random effects have a mass-point distribution. Column 4 of Table 4 shows estimation results for one such model with four 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 four-mass-point model. Interestingly,

[4]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.

[5]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.

the estimates of the state dependence coefficients are relatively similar in columns 3 and 4, though the mass-point model has a somewhat higher log likelihood.

Table 5: Summary of IA Participation Patterns of Control Group Members, With Comparisons With Model Predictions

	Number of Transitions											
	0		**1**		**2**		**3+ Even Sum**		**4+ Odd Sum**		**Total**	
Months on IA	Actual and Predicted Cell Fractions From the Model in Table 4, Column 3 (normal heterogeneity)											
	Actual	*Predict.*	**Actual**	*Predict.*	**Actual**	*Predict.*	**Actual**	*Predict.*	**Actual**	*Predict.*	**Actual**	*Predict.*
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*	**2,786**	*2,786.0*
Months on IA	Actual and Predicted Cell Fractions From the Model in Table 4, Column 4 (mass-point heterogeneity)											
	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*	**2,786**	*2,786.0*

Note: Bold entries represent the 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. Italicized entries represent the predicted number of observations with the same IA participation history.

In comparing different mass-point models, we observed that once we allow for three 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 models in Table 4 is presented in the bottom rows of the table. These are the estimated means, variances, and first-order to fifth-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 test is to compute the sample analogue of one of these statistics and compare the result to the expected value under the null hypothesis of a correctly specified model. We do not know the true value of the random effect for person i. However, given the likelihood function, the observed sequence of data for individual i, and the marginal distribution of the random effects, we can compute the posterior distribution for the random effect for that individual.[6] We can then 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 four 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 first-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 very little serial correlation in the prediction errors from the models.

[6] Let $l(y|\alpha)$ 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 outcome y is $f(\alpha|y) = l(y|\alpha)\, f(\alpha) / \int l(y|\alpha')\, f(\alpha')\, d\alpha'$.

Models of Welfare Participation for the SSP Program and Control Groups

MODELING SSP'S INCENTIVE EFFECTS

We now turn to a specification of the incentive effects of Self-Sufficiency Project (SSP) on the program group on welfare dynamics. Building on the insights of the simple theoretical model presented earlier in this paper, we include two separate treatment effects: the essentially mechanical effect on welfare participation for those who become SSP-eligible, caused by the rule that supplement recipients had to leave income assistance (IA); and the post-entitlement effect, caused by the fact that members of the eligible subgroup have an incentive to choose work over welfare in any given month up to the end of their eligibility period. The key to distinguishing these effects is the date of SSP eligibility, t^e_i. We discuss the actual measurement of this date after we present our model for the behavioural impacts of SSP conditional on the eligibility date.

For any member of the program group there are four distinct phases: (1) the *pre-eligibility* 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*, lasting for a couple of months after the establishment of eligibility, 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 $t^e_i + 36$; (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 sequence of eligibility indicators are both related to the unobserved heterogeneity component α_i :

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

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

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 two lags of previous IA status.[1] Specifically, we assume that

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

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

We further assume that IA participation of the program group follows the same model as the control group, 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

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

$$= L(\alpha_i + x_{it}\beta + \gamma_1 y_{it-1} + \gamma_2 y_{it-2} + \gamma_3 y_{it-1}\, y_{it-2} + \tau(t, E_{it}, t^e_i, y_{it-1})),$$

where L( ) represents the logistic distribution function, and $\tau(t, E_{it}, t^e_i, y_{it-1})$ is the behavioural impact of SSP. Based on the convergence of the IA participation rates of the program and control groups after SSP payments ended, we assume that effects of SSP are confined to the transitional period and the entitlement period, and we allow separate treatment effects depending on whether the individual was on or off IA in the previous period:

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

where J is the duration of the transition period. The parameters ψ_0 and ψ_1 measure the effects of initial SSP eligibility during the transitional period on individuals who were off or on IA in the previous month, respectively, while the parameters λ_0 and λ_1 measure the corresponding incentive effects during the entitlement period. We consider specifications that assume constant treatment effects, and others that allow the values of $(\psi_0, \psi_1, \lambda_0, \lambda_1)$ to vary 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 achieving eligibility in month t, conditional on not achieving it earlier. We assume that the hazard of eligibility depends on the individual heterogeneity effect α_i and on the time since random assignment:

$$(5) \qquad P(E_{it} \mid E_{it-1}, E_{it-2}, \dots, x_{it}, \alpha_i)$$

$$= \Phi[d(t) - k(\alpha_i)] \quad \text{if } E_{it-1} = 0 \quad \& \quad t \le T_e,$$

[1]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.

$= 1$ if $E_{it-1} = 1$,

$= 0$ if $E_{it-1} = 0$ & $t > T_e$,

where Φ is the standard normal distribution function, d(t) is a smooth function of time, T_e is the duration of the eligibility period, 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 achieving 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.

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, plus one month for delays; (2) the first month of SSP receipt; and (3) 14 months after random assignment.[2] Using these dates, about 18 per cent of the eligible program subgroup achieved eligibility in the first month after random assignment, 9 per cent became eligible in each of the second and third months, and roughly 6 per cent became eligible in each of the next 10 months. Just under three per cent 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 two 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 three months.

ESTIMATES FOR THE PROGRAM AND CONTROL GROUPS

Table 6 presents estimates of alternative specifications of equations (1) to (5). All the models allow for second-order state dependence, with interactions between the state dependence effects and the random effects, and include a fourth-order trend in the IA participation model. The specifications in columns 1 to 4 assume normally distributed random effects, while the specification in column 5 uses a four-mass-point specification. As a reference point, the model in column 1 ignores any correlation between SSP eligibility and the random effect. The other specifications include an eligibility model based on equation (5), with a trend function $d(t) = d_0 + d_1(t - 1) + d_2/(t - 2)$.[3] The specifications in columns 1 and 2 assume that the treatment effects are constant across individuals, while the specifications in

[2]We assume the duration of the eligibility window is 14 months, rather than 12 or 13, to reflect delays in processing and errors in dating. There is only a handful of eligible program group members for whom the minimum of the first month of full-time employment (plus 1) and the first month of SSP receipt is greater than 14.

[3]The $1/(t - 2)$ term is included to capture the fact that the hazard of eligibility falls from 18 per cent in Month 3 to around 8 per cent by months 4 to 10.

columns 3 to 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.

Table 6: Estimated Dynamic Models for IA Participation for the 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 (0.04)	4.72 (0.04)	4.68 (0.04)	4.69 (0.04)	4.60 (0.06)
y(t - 2)	1.90 (0.03)	1.86 (0.03)	1.84 (0.03)	1.84 (0.03)	1.69 (0.04)
y(t - 1) x y(t - 2)	-0.87 (0.05)	-0.80 (0.05)	-0.79 (0.05)	-0.82 (0.05)	-0.77 (0.06)
y(t - 1) x α	-0.74 (0.04)	-0.90 (0.04)	-0.77 (0.04)	-1.11 (0.07)	-0.93 (0.03)
y(t - 2) x α	-0.40 (0.03)	-0.35 (0.03)	-0.33 (0.03)	-0.40 (0.04)	-0.55 (0.02)
y(t - 1) x y(t - 2) x α	0.76 (0.05)	0.79 (0.05)	0.74 (0.04)	1.09 (0.08)	0.61 (0.03)
Treatment parameters					
Transitional period					
ψ_1 (exit)	-3.02 (0.06)	-2.76 (0.06)	-3.26 (0.07)	-3.10 (0.07)	-2.82 (0.06)
ψ_0 (entry)	-1.92 (0.13)	-1.63 (0.13)	-1.84 (0.14)	-1.75 (0.14)	-1.68 (0.14)
ψ_1 x α	—	—	-0.53 (0.06)	-0.61 (0.07)	-0.10 (0.02)
ψ_0 x α	—	—	-0.27 (0.09)	-0.22 (0.15)	-0.26 (0.05)
Eligibility period					
λ_1 (exit)	-1.35 (0.04)	-1.10 (0.05)	-1.08 (0.04)	-1.11 (0.04)	-0.86 (0.05)
λ_0 (entry)	-0.86 (0.05)	-0.51 (0.05)	-0.69 (0.05)	-0.72 (0.05)	-0.45 (0.06)
λ_1 x α	—	—	-0.03 (0.04)	-0.07 (0.06)	0.35 (0.05)
λ_0 x α	—	—	-0.26 (0.04)	-0.35 (0.07)	-0.06 (0.05)
Selection parameters					
constant	—	-2.23 (0.06)	-2.23 (0.06)	-2.22 (0.06)	-2.01 (0.07)
linear trend	—	0.19 (0.04)	0.18 (0.06)	0.18 (0.06)	0.18 (0.06)
1/t	—	0.55 (0.08)	0.55 (0.08)	0.55 (0.08)	0.57 (0.08)
k = loading on α	—	0.21 (0.01)	0.21 (0.02)	0.29 (0.02)	mass-point specific

(continued)

Table 6: Estimated Dynamic Models for IA Participation for Control and Program Groups (Cont'd)

	Models With Normally Distributed Random Effect				Model With Mass-Point Distribution of Random Effect
	(1)	(2)	(3)	(4)	(5)
Interaction of random effect and trend					
Linear trend x α	—	—	—	0.30 (0.04)	0.01 (0.01)
Quadratic trend x α	—	—	—	-0.33 (0.06)	-0.01 0.01
Standard deviation of random effect	1.75 (0.04)	1.76 (0.04)	1.86 (0.04)	1.18 (0.06)	4 mass points
Log likelihood	-57,018	-61,116	-61,032	-60,960	-60,779
Goodness of fit					
Control group	277.1	285.8	283.3	262.5	158.1
Program group	194.1	232.6	233.8	233.7	209.8

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

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 effects and smaller but significantly negative treatment effects in the entitlement period. A comparison of the treatment effects in columns 1 and 2, however, shows that the implied entitlement-period effects are about 30 per cent larger when eligibility is treated as exogenous (column 1) than when it is modelled 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. 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, 3, and 4. The implication is that the distribution of the random effects among those who became eligible is much different than the distribution among those who were ineligible. 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 three rows of Table 6 show the predicted fraction of the program group who achieved eligibility and goodness of fit statistics summarizing each model's ability to predict the distribution 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.

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). As a result, the predicted probabilities of leaving IA in the period just after the establishment of eligibility 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 the model that assumes a homogeneous effect on the log odds. Consistent with this observation, the goodness of fit statistics for the model in column 3 are somewhat better than those of the simpler specification in column 2.

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 estimates of the 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 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 four-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 four-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 per cent of the control group to 17.1 per cent 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 to 4. However, the estimated mass points in the welfare participation and eligibility models are very highly correlated (correlation = 0.94 across the four 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 quite 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.

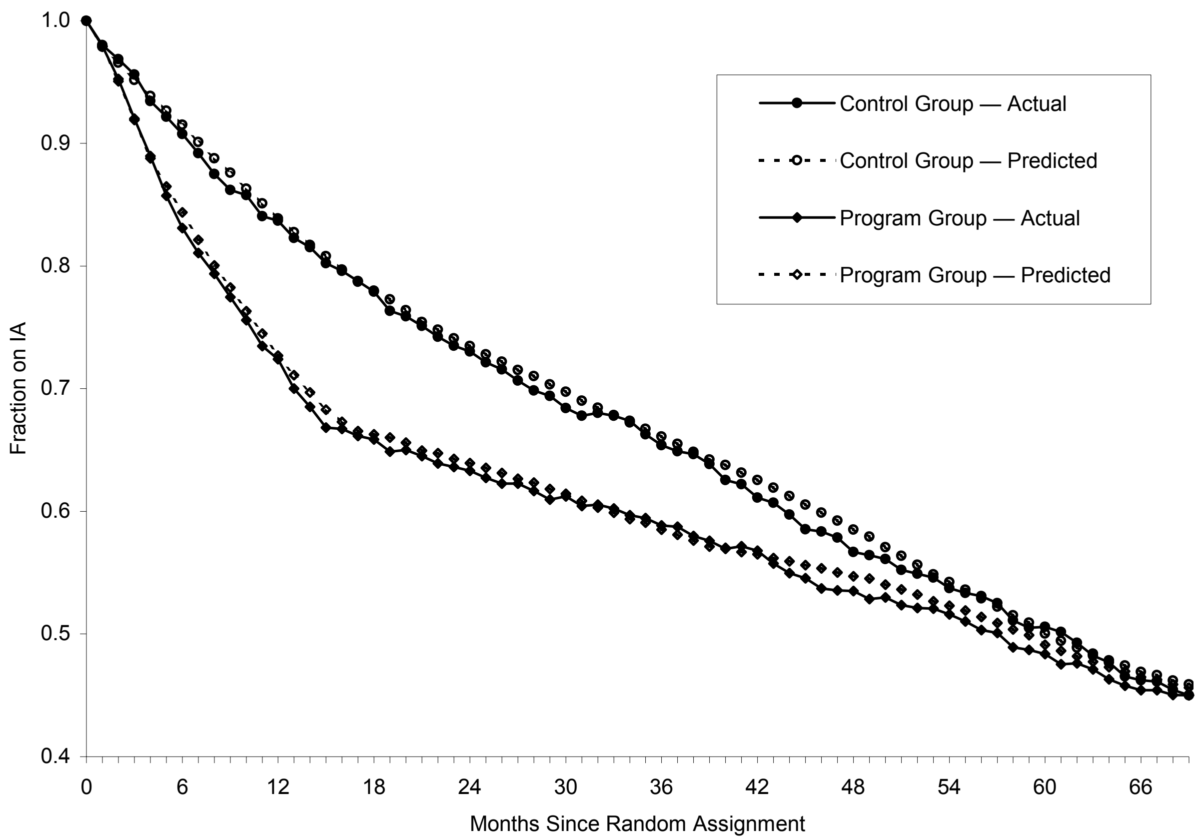
Table 7: Summary of IA Participation Patterns of the Program Group, With Comparisons With Model Predictions

	Number of Transitions											
	0		1		2		3+ Even Sum		4+ Odd Sum		Total	
	Actual and Predicted Cell Fractions From the 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*	**2,831**	*2,831.0*
	Actual and Predicted Cell Fractions From the Model in Table 6, Column 5 (mass-point heterogeneity)											
Months on IA	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*	**2,831**	*2,831.0*

Note: Bold entries represent the 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. Italicized entries represent the predicted number of observations with the same IA participation history.

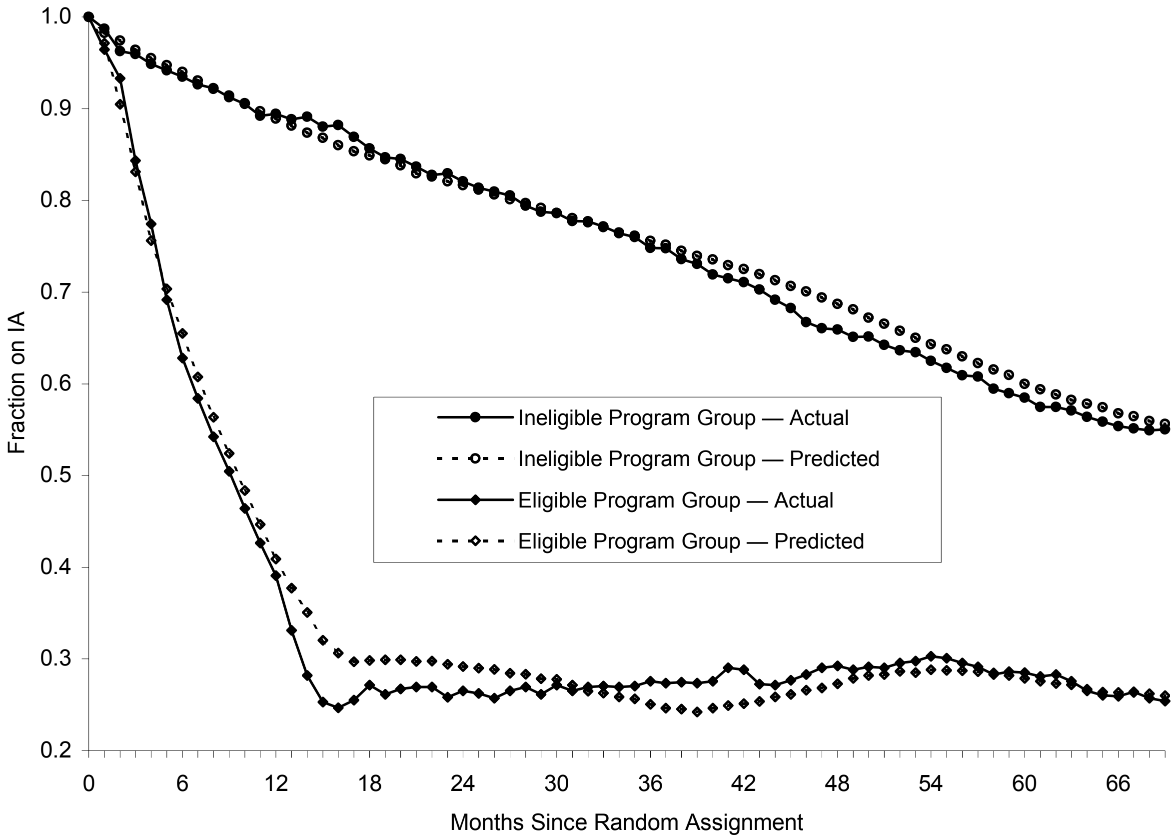
Figure 7 shows predicted and actual IA participation rates for the program and control groups in the 69 months after random assignment, based on the normal heterogeneity model in column 4 of Table 6. (Predictions from the mass-point heterogeneity model are nearly identical.) Overall, the predictions 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 to 15) and also over-predicts IA participation rates of both groups in months 43 to 50. The model explains over 99 per cent 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 per cent, respectively. (The corresponding figures for the mass-point model in column 5 are 0.7 and 0.9 per cent.)

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



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 per cent), while those for the eligible group are a little less so (root mean squared error 2.6 per cent), particularly in months 13 to 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 behaviour is consistent with our theoretical model, which predicts that some people will take a relatively unattractive job to gain eligibility and then quit immediately. It is also potentially consistent with the empirical model, which allows a bigger effect on welfare participation in the first three months after initial eligibility than in the later entitlement period. However, the parameterization is evidently a little too restrictive to fully capture the phenomenon.

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



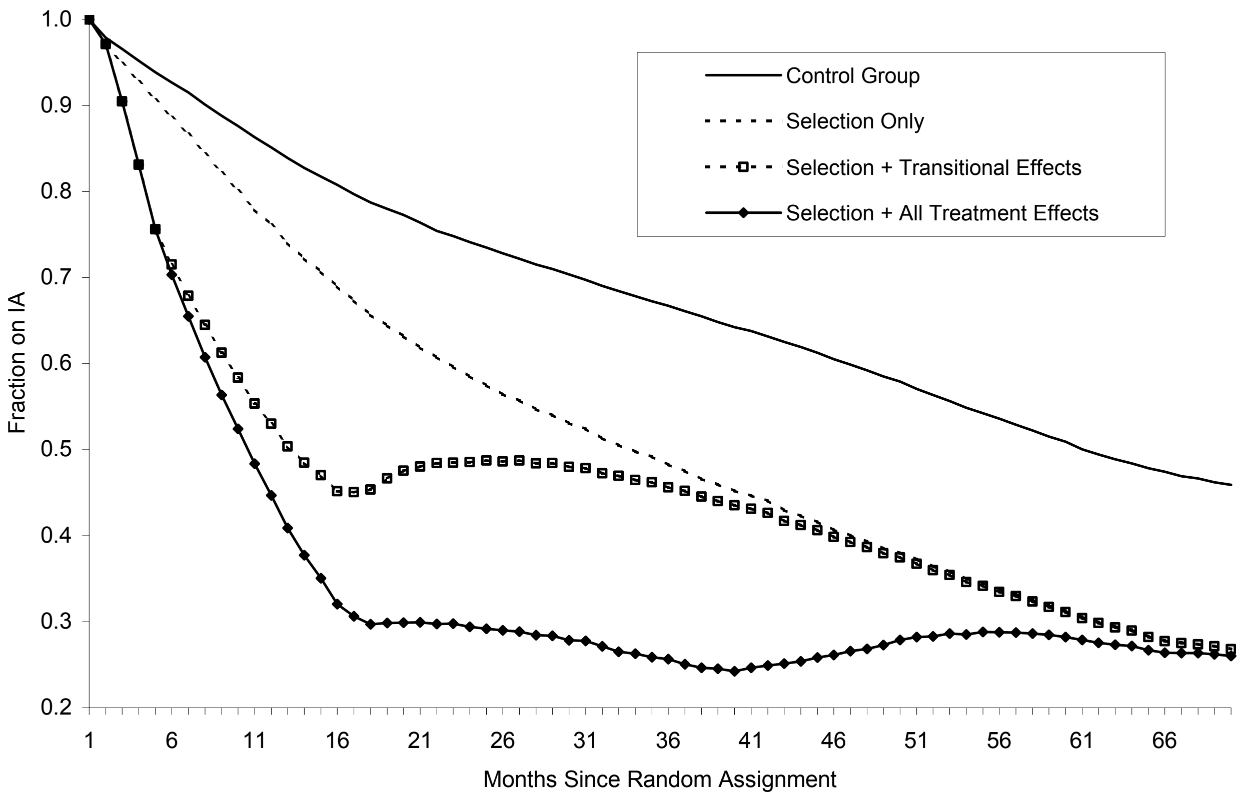
Another problem for the model is the trend in welfare participation of the eligible program group 18 to 36 months after random assignment. During this period the participation rate of the eligible group is very stable, whereas the model predicts a decline, particularly after Month 24. The predicted trend essentially tracks the trends in the control group and the ineligible program group: both show steady declines in IA participation during months 18 to 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 to 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 of 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 to 64 but a worse job in months 65 to 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.

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 behavioural 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, transitional-period effects, and entitlement-period effects, while Figure 10 shows the predicted and actual SSP impacts on IA participation, with a decomposition of the predicted impacts into transitional and entitlement-period effects.

Figure 9: Decomposition of Predicted IA of Eligible Program Group Members



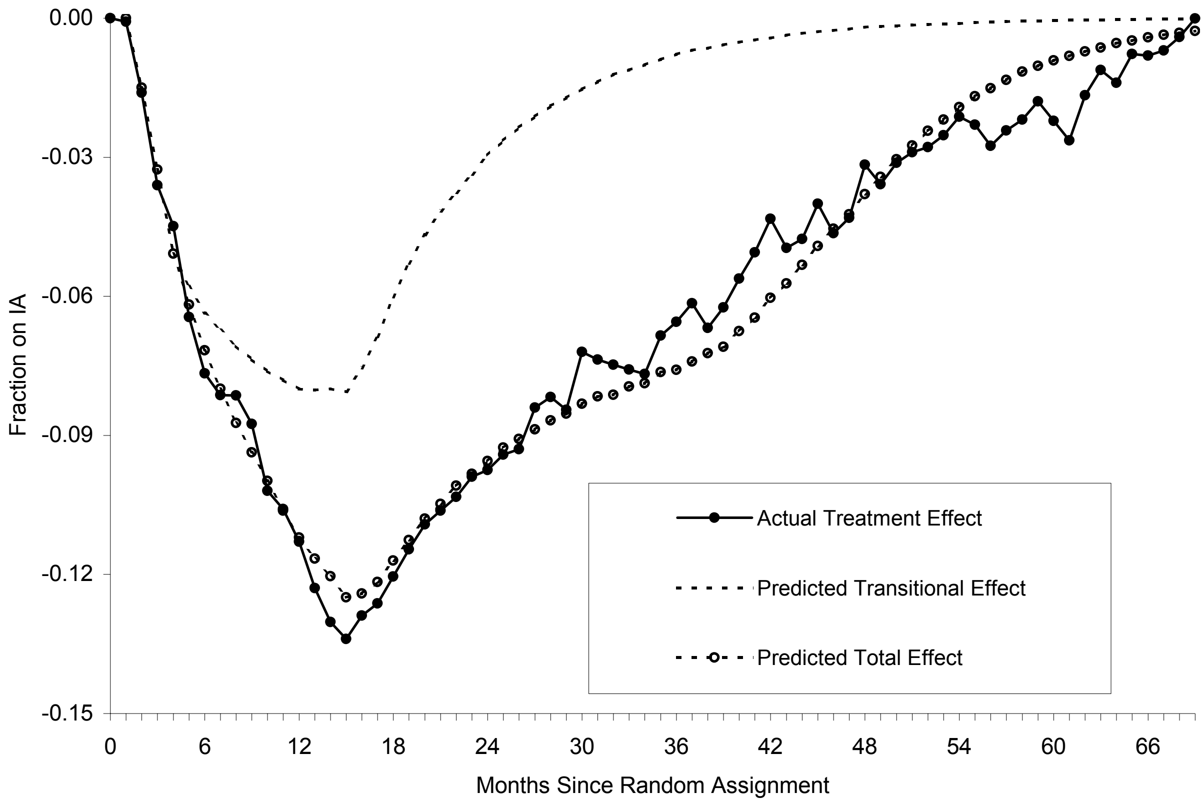
Beginning with Figure 9, the upper solid line shows the predicted welfare participation rate of the control group and the dotted line shows the predicted welfare participation rate of the eligible program group in the absence of SSP. The divergence from the solid line reflects the selective nature of the eligible program group. For example, in Month 36 the model predicts a 46 per cent IA participation rate for the eligible program group in the absence of any treatment effects versus a 66 per cent rate for the control group.

Next, we plot the path of the eligible program group, taking account only of the transitional-period treatment effects. These represent the "mechanical" effect of becoming eligible and forced to leave IA. Although the transitional treatment effect peaks 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 both the transitional and entitlement-period effects of SSP on IA behaviour. Comparisons of the various paths show that in the first year and a half of the experiment (months 6 to 18) most of the overall treatment effect for the eligible program group derived from the transitional effects. Over the period from the 18th to 36th month, this effect gradually dissipated and the entitlement-period effects dominated. Starting in Month 36 and continuing through Month 50, members of the eligible program group gradually exhausted their three years of supplement eligibility and the treatment effect faded out. Finally, after Month 50 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 eligibility-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 a one-time reaction to the eligibility rules, and a longer-run post-entitlement effect on welfare entry and exit rates that ended once individuals' SSP eligibility expired. The transitional eligibility effect reached a peak of about -20 percentage points at 15 months after random assignment, accounting for 55 per cent of the overall impact on the eligible program group at that point. By three years after random assignment, the transitional effect had faded and accounted for 15 per cent 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 eligibility window.

Figure 10 presents a decomposition of SSP's predicted impacts on the overall behaviour 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 to 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.

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



Conclusions

The SSP experiment produced one of the largest impacts on welfare participation ever recorded in the experimental evaluation literature. At its 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 program and control groups of the experiment had closed by 50 per cent, 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 program 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 guarantee their eligibility for up to three more years of subsidy payments. Since the program rules required subsidy recipients to leave welfare, the eligibility 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 short-lived nature 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 is no evidence of any upward trend in the wages associated with the extra hours. By 52 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 consistent with standard reasoning and that time-limited work incentives have little or no permanent effect on welfare dependency.

Appendix:
A Simple Model of Work and Welfare Participation

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 $l \leq w \leq m$. The job destruction rate is δ. Optimal behaviour 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 rearrangement, 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) the 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 rewritten 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) $V^0 = b/r + \lambda(1\text{-}\delta)/r \int_R^m \{U(\omega) - V^0\}\ f(\omega)\ d\omega.$

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

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) $U_e(w, 36) = (w - c + s(w))/(1 + r)$

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

$$= U(w) + s(w)/(1 + r).$$

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

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

$$= (w - c + s(w))/(1 + r) + V^0 - b/(1 + r)$$

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

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

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

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

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, etc. Thus R_e is the optimal reservation wage during all months of SSP eligibility.

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