

Reforming the Financial Incentives of the Welfare System

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Reforming the Financial Incentives of the Welfare System: Lecture 1

I. Introduction

After many years of debate the United States is now embarked on a major experiment in the design of its income support programs. The federal government's Aid to Families with Dependent Children program, which had provided cash assistance to low-income parents and children for over forty years, was abolished and replaced by a new program that has shifted the orientation of the welfare system toward the goal of "work". Fortunately for those in the system – but unfortunately for those who would like to know the real effect of welfare reform – these changes have occurred in the midst of an extraordinary economic boom. Unemployment rates are now lower than at any time in the past 30 years, and the real earnings of less-skilled workers show the first signs of sustained growth since the 1970s. Because of this coincidence of timing I suspect there will be no easy consensus on the impact of welfare reform.¹ Nevertheless, the dramatic fall in welfare caseloads shown in Figure 1 is widely interpreted as evidence that welfare reform is working. Given the salience of the American example in policy discussion around the world, "work-oriented" welfare reform is now a topic of intense interest in many other countries.

Recent changes in U.S. income support programs have involved two components: changes in eligibility rules designed to force welfare recipients to work ("sticks"); and changes in benefit formulas designed to increase the financial incentives for work ("carrots"). Much of the attention so far has focused on the sticks. In these lectures, however, I want to focus on the carrots – specifically, on the prospects for reforming the financial incentives embedded in the income support program for low-income single parents. Much of what I have to say is based on findings from a large scale social experiment in Canada – the Self Sufficiency Project or SSP – that is evaluating the effect of high powered financial incentives for work by long-term welfare recipients.

¹Two opposing views are articulated by R. Haskins and W. Primus in a recent forum published by the National Center for Children in Poverty (NCCP, 2000). Recent research on the effects of welfare reform includes Schoeni and Blank (2000) and Council of Economic Advisors (1999).

The fact that SSP is located in Canada has turned out to be an unexpected advantage, since although the Canadian income support program looks a lot like the old Aid to Families with Dependent Children program in the U.S., Canada has had only modest changes in its welfare system during the 8-year period of the demonstration. Thus, the underlying environment for those not offered the SSP incentive has remained relatively stable. Moreover, any analysis of welfare programs in the United States is complicated by subtle interactions with two other programs – Food Stamps and Medicaid – that are ostensibly independent of the cash assistance program but provide important benefits to cash assistance recipients.² The absence of food vouchers, and the presence of national health care system in Canada, help to simplify the interpretation of a demonstration project like SSP, and may make the results more interesting in countries that are closer to Canada than the U.S. in the design of their income support programs.

The primary scientific attractions of the SSP demonstration are its randomized experimental design, its large scale, and its long-term follow-up. SSP consists of two main experiments: one conducted on single parents who had been on the welfare roll for at least a year, and a second conducted on single parents who had recently applied to receive benefits. In each case, the SSP incentive package was offered to one-half of the experimental population (the treatment or program group), while the other half had access to the regular welfare system (the control group). When it comes to the evaluation of complex program interventions, there is no substitute for a randomized design. The SSP demonstration involves about 9,000 single parents who are followed for five years after entering the experiments. The size of the experimental population, the length of the follow-up period, and the richness of the data collected in a series of surveys of the SSP population allow us to reach informed judgements about the effect of enhanced financial incentives for work among the single parent welfare population.

To preview the main message of these lectures, I will argue that the SSP demonstration confirms

²As noted in a recent study by Dion and Pavetti (2000), restrictions on eligibility for cash welfare benefits have apparently led to reductions in Food Stamp and Medicaid reciprocity, even though eligibility rules for these programs were not directly affected by welfare reform legislation.

the central role of financial incentives in the welfare participation and work activity of low-income families. Enhanced incentives can induce a significant fraction of welfare recipients to leave the roll and take up full time work. Moreover, when the incentives are offered to relatively short term welfare recipients they can actually save the government money. Evidence from the SSP demonstration also lead to a rather pessimistic view about the other kinds of welfare reform – those that rely on sticks. People who left welfare and entered work in response to SSP earn relatively low wages (typically within \$1 of the minimum wage) and even after two years of work have not moved far up the wage ladder. If these people had been forced to leave welfare by time limits or other compulsory mechanisms, their standard of living would be quite low. Thus, findings from SSP suggest there is no “free lunch” in welfare reform: we cannot expect people who have chosen welfare over work to enter the work force and be significantly better off than they were on welfare. In fact, many will be much worse off.

II. Financial Incentives in the Welfare System

Most of the conceptual issues underlying the role of financial incentives in the welfare system can be illustrated in a simple graph like Figure 2. The horizontal axis on this graph represents hours of work by a single parent, while the vertical axis represents monthly cash income. To simplify things, assume that a parent has no income sources other than earnings or welfare, that she can freely choose her hours of work at a constant wage of \$w per hour, that a welfare grant of \$G is available for those with no other income, and that the grant is reduced by \$t per dollar of other income. In the graph I have assumed that the benefit reduction rate t is 1: each additional dollar of income reduces the welfare grant by a dollar. This was true in the American AFDC system in the 1980s and 1990s, and also in the Canadian Income Assistance program during the same period. Notice that once earnings exceed \$G, an individual can no longer receive welfare benefit. At an hourly wage of \$w, this breakeven point occurs at $h^1 = G/w$ hours of work per month.

A critical feature of the budget constraint in Figure 2 is the flat range of cash income for hours up to h^1 .³ Obviously, the 100 percent benefit reduction rate creates a substantial work disincentive for welfare participants. Absent mandatory work requirements, one would not expect to see welfare recipients working at all.⁴ In fact, the labor supply behavior of single mothers in the 1980s and early 1990s was broadly consistent with this prediction. Tabulations of 1988-93 U.S. Current Population Survey reveal that only about 20 percent of single mothers who were on AFDC continuously during the year had any labor earnings. A similar employment rate is measured among welfare recipients who were eligible for the SSP program in Canada.

Many analysts have argued that a simple modification of the welfare system illustrated in Figure 2 could raise the employment rates of welfare recipients and supplement the incomes of the working poor. This modification – lowering the benefit reduction rate (BRR) to something under 100 percent – is the standard design of a negative income tax (NIT). The effect on the budget constraint of a single mother is illustrated in Figure 3. Now, individuals continue to receive welfare benefits until their income exceeds the threshold $\$G/t$. At an hourly wage of w , the breakeven level of hours is $h^2 = G/tw$. A welfare system with a BRR less than 1 can also be recast as an earnings supplement: an individual who earns an amount $\$y$ receives a supplementary check for a fraction t of the difference between $\$y$ and the breakeven earnings level $\$G/t$.

Lowering the BRR has potentially important incentive effects on two distinct subgroups of low-income people. The first is the group who are already on welfare in the absence of financial incentives. For simplicity, I will refer to these as the “previous recipients”. In a welfare system with a 100% BRR, the

³Under the old AFDC system in the U.S. h^1 was about 83 hours (19 hours per week) for a single mother with one child in California in 1996 who could earn \$7.00 per hour, and about 36 hours (8 hours per week) for a comparable mother in Arkansas

⁴The U.S. and Canadian welfare systems actually include a so-called “disregard” (typically around \$100 - \$200 per month) that a welfare recipient is allowed to earn before benefits are reduced. Many welfare recipients report earning amounts close to the disregard amount.

vast majority of previous recipients are not working at all.⁵ For these nonworkers, the elimination of the “flat” segment of the budget constraint introduces an unambiguously positive work incentive. For the minority of previous recipients who were working, the situation is a little more complicated, since a reduction in the BRR raises the net wage (providing an incentive to work more) but also raises income (providing an incentive to work less).⁶ The size of the income effect, however, is proportional to earnings in the absence of incentives, and since most working recipients have low earnings, the positive incentive arising from the higher wage is presumably more important.

A second group of low-income people potentially affected by the introduction of financial incentives are those who were off welfare and earning more than the base level of welfare benefits (G), but who could potentially become welfare beneficiaries when the breakeven level of earnings rises to G/t . For simplicity I will refer to these as the “windfall beneficiaries” of financial incentives. Again, it is helpful to distinguish two sub-groups. The first are those were earning less than the new breakeven amount (G/t): with no change in behavior these people are automatically eligible for welfare when the BRR is lowered. A reduction in the BRR has an unambiguously negative effect on work incentives for this group, since it lowers their marginal wage rate (from w to $w(1-t)$) while raising their incomes. A second sub-group of windfall beneficiaries consists of people who were earning a little more than the new breakeven: some of these people may decide to lower their hours and “opt in” to the welfare system. Notice that for this subgroup the introduction of financial incentives can only lower work effort. The size of the potential opt-in group depends on the willingness of individuals to give up some income in order to gain some

⁵Or at least, are not working openly. Studies of welfare participants in systems with high BRR’s suggest that some fraction are working “under the table” – see e.g. Fortin, Frechette, and Lemieux (1994), and Edin (1997)

⁶Although one would expect very few people to work if their earnings are taxed at 100%, in reality there are always people in this situation. Working welfare recipients may be holding a part-time job with the intention of moving to full-time work in the near future. Alternatively, they may value some benefit of welfare participation (such as subsidized child care or access to public housing) that is missing from the simplified budget analysis in Figure 2.

additional leisure (i.e., on the curvature of indifference curves, usually parameterized by the compensated elasticity of labor supply).⁷ Evidence from the Seattle-Denver Negative Income Tax experiments in the 1970s (Ashenfelter, 1983) suggests that this group is relatively small.

Although many economists have argued in favor of reduced benefit reduction rates, 100% rates are surprisingly common. The main reason is cost: lowering the BRR raises the breakeven level and extends eligibility to a wider population of low income families, raising the number of welfare recipients and the cost of the program. If the distribution of earnings is bell-shaped, the number of windfall beneficiaries associated with the introduction of financial incentives can be large relative to the number of people who are on welfare with a 100% BRR. Moreover, to the extent that the windfall beneficiaries reduce their earnings in response to newly available benefits, a financial incentive program can actually end up lowering total hours and earnings of the low-income population (especially if the ratio of windfall beneficiaries to “incentivized” beneficiaries is large). Such a perverse effect was actually found in the Negative Income Tax experiments (see Keeley et al, 1978; Robins, 1985). In the early 1980s, cost pressures and a shift in political sentiment led to a reform of the U.S. AFDC program that eliminated financial incentives by replacing the previous 67% BRR with a 100% rate.⁸ Ironically, the low level of work activity among the recipient population after the change came to be viewed as evidence that the system had failed, fueling the demand for the reforms of the 1990s.⁹

Over the past two decades there have been several responses to the policy conundrum of how to increase the work incentives for low income single parents while restraining the size and cost of the

⁷Ashenfelter (1983) used a simple labor supply model to show that this group consists of people who had earnings between B and $B(1+d)$ prior to the reduction in the BRR, where $B=G/t$ is the new breakeven income level and $d = \frac{1}{2} \epsilon^c t$, where ϵ^c represents the compensated elasticity of labor supply. If there is some stigma to entering welfare, the opt-in group will be even smaller.

⁸The BRR in the AFDC program had been lowered from 100% to 67% in 1967.

⁹Between 1979 and 1982 the employment rate of single-mother welfare recipients fell by 30 percent: see Blank, Card, and Robins (2000, Table 8).

welfare system. One response has been to limit eligibility for welfare benefits by imposing mandatory job search or work requirements, and/or imposing time limits on the maximum duration of benefits. Such restrictions are a much-discussed feature of the 1996 welfare reform legislation in the United States¹⁰, and indicate an important shift in the philosophy of the welfare system away from a “rights-based” system and toward a more “discretionary” system in which case workers have considerable latitude to reward or punish individual recipients.

Another response is the creation of financial incentives outside the traditional welfare system. The U.S. Earned Income Tax Credit (EITC) works in this manner. The EITC is a variable earnings subsidy linked to family size and paid as a refundable income tax credit.¹¹ Treated as a package, the EITC plus conventional welfare lead to a budget constraint similar to the one in Figure 3 (see Blank, Card, and Robins, 2000, Figure 7). The EITC program is not perceived as “welfare”, however, so the size of the recipient population and the cost of the program do not attract the same attention as they would if the EITC and welfare systems were combined. Interestingly, national spending on the EITC is now three-to-four times larger than spending on traditional cash welfare. A series of recent studies conclude that the expansion of the EITC has caused a significant rise in the work activity of single mothers (Eissa and Liebman, 1996; Meyer and Rosenbaum 1998). My guess is that when the smoke has cleared, an important fraction of the welfare caseload reduction seen in Figure 1 may be attributed to the EITC, rather than to welfare reform

A third response is to target financial incentives at particular subgroups, rather than the entire low-income population. The Self Sufficiency Project (SSP) is an example of this approach. SSP offers a three-

¹⁰Because states are allowed a lot of flexibility in designing their welfare systems under the new law, a precise description of the eligibility limits is a major undertaking: see Gallagher et al (1998) for a summary of legislation up to 1997, and U.S. General Accounting Office (1998).

¹¹The EITC is 0 for a family with no earnings, and rises with earnings to a maximum level before “phasing out”. Unlike cash welfare, the EITC is available to dual-headed families.

year earnings supplement (equivalent to a welfare system with a 50% BRR) to individuals who have previously been on welfare for at least a year. Since most long-term welfare recipients would remain on welfare in the absence of incentives, there are relatively few windfall beneficiaries of the program, at least in the months immediately following eligibility determination. Moreover, anyone who wants to become eligible has to enter welfare and remain on the roll for a full year. As will be discussed in more detail below, the results from the SSP demonstration suggest that this entry barrier greatly limits the size of the windfall population.

In addition to limiting eligibility to former long-term welfare recipients, SSP requires individuals to work at least 30 hours per week in order to receive the earnings supplement. Relative to an unrestricted financial incentive plan, a full-time hours restriction has two effects. On one hand, a full time eligibility restriction greatly limits the scope for any negative labor supply responses among windfall beneficiaries (a key problem that confounded the Negative Income Tax experiments). Indeed, some people who would be windfall beneficiaries of an unrestricted program actually have to increase their hours to achieve eligibility. On the other hand, a full-time restriction also limits the scope of the program, since only those who can find and maintain a full time job will be affected by financial incentives. Thus, while a full-time hours restriction can help prevent a financial incentive program from actually reducing overall labor supply, it may or may not lead to a rise in the number of former recipients who increase labor supply in response to the incentive, relative to the number of windfall beneficiaries who can receive welfare with no increase in work effort. This ratio is a critical determinant of the net government cost of a financial incentive program, since windfall beneficiaries increase government cost, whereas “incentivized” former recipients typically lower government cost.

III. The Self Sufficiency Program: Design and Basic Findings

With this introduction, I now turn to a more in-depth discussion of the Self Sufficiency Program.

SSP is an initiative of Human Resources and Development Canada, the federal agency charged with oversight of the country's income support programs. During the 1970s and 1980s, Canada experienced massive increases in spending on programs for low-income families.¹² As in the U.S., the welfare program for non-elderly/non-disabled adults and their children – known as Income Assistance (IA) – came under attack for discouraging work and encouraging long-term dependence. In this context, SSP was conceived as a rigorous test of enhanced financial incentives to raise employment and lower program participation by long-term welfare recipients.¹³ Under SSP, an individual who leaves IA and finds a full time job (or combination of jobs) receives a supplement equal to one-half of the difference between her actual earnings and a breakeven level set well above the level of conventional IA benefits. The supplement is available for three years. In the short run, SSP substantially increases the financial reward for leaving IA and entering work. Over the longer run, proponents of SSP argue that the accumulation of work experience could lead to rises in wage opportunities and/or changes in tastes that induce beneficiaries to leave welfare permanently.

The SSP demonstration includes two main experiments. The first, which I shall focus on for the moment, was conducted on long-term IA recipients in two sites: one in lower mainland British Columbia (including Vancouver and surrounding areas), and the other in southern New Brunswick. Half of the individuals in this so-called Recipient Experiment were randomly assigned into the program group and were eligible for the SSP supplement, while the other half remained in the regular IA system and form the control group. Both groups are followed for 5 years after random assignment and are interviewed at

¹²For example, combined federal and provincial spending on income assistance programs for low-income families rose close to 300 percent over the 1980s (Courchene, 1994).

¹³Several similar programs were developed and tested in the United States in the early 1990s, including the Minnesota Family Investment Program and New Hope. Blank, Card, and Robins (2000) provide a survey of other recent financial incentive programs for welfare participants in the U.S.. Berlin (2000) provides an in-depth comparison of SSP to the Minnesota Family Investment Program and New Hope.

regular intervals about their labor market experiences, income sources, living arrangements, and attitudes toward work.

Table 1 summarizes the main features of the Recipient Experiment, including sample eligibility requirements and the supplement program rules. Relative to the existing IA system SSP is quite generous, because it lowers the benefit reduction rate and also raises the “base” level of benefits (the G parameter) for most participants.¹⁴ For example, a single mother with one child in New Brunswick could receive a maximum monthly IA grant of $G = \$712$ in 1994, and was subject to a 100% BRR (after a \$200 per month disregard). The SSP program parameters in New Brunswick in 1994 were equivalent to a welfare system with $G = \$1250$ and a BRR of 50%. SSP also substantially increases the financial rewards for work. Under the existing IA system, a single mother in New Brunswick with one child who left welfare and entered full time work at the minimum wage would earn a before-tax income of \$867 per month -- a gain of only about \$36 per week for 40 hours of work. Under SSP, however, the same woman would be eligible for a monthly supplement payment of \$817, raising the income advantage for work over welfare to \$225 per week. The relative reward to work under SSP is smaller when income taxes and other transfers are taken into account, but is still relatively large (see Lin et al, 1998, Table G.1).

An important feature of SSP is its time-limited eligibility. Individuals who initiate supplement payments within the first year after random assignment can receive SSP payments for up to three years in any month that they are working full time and off IA. Program group members who fail to initiate an SSP payment within the first year lose all future eligibility. The latter restriction has some implications for thinking about the work incentives of the SSP program. Most importantly, SSP only provides an incentive to move off of welfare and into full time work within a year. This means that the behavioral responses to the SSP program may be somewhat different than would be observed if the program were offered on a

¹⁴SSP has a fixed base payment that is independent of family size, while the benefits in IA are related to family size. For most (smaller) families the base level of benefits is higher under SSP, although this base level is not directly payable, since SSP can only be received by full-time workers.

continuing basis.

The Research Sample

About 5,700 single parents were recruited to take part in the SSP Recipient Experiment during an enrollment period from late 1992 to early 1995.¹⁵ Table 2 provides an overview of the characteristics of the experimental population. The data here are drawn from baseline interviews conducted just prior to random assignment, and are shown for the overall sample as well as separately by province. (Because of random assignment, the average characteristic of the program and control groups are virtually identical, and to avoid clutter I have shown the data for both).¹⁶ The experimental population is over 95% single mothers (versus 5% single fathers), and has relatively low education. About 40 percent of the sample were themselves raised by a single parent, and one quarter report a physical limitation that affects their work activity. In terms of family structure, the sample is roughly evenly divided between those with one dependent child and those with two or more, and also split fairly evenly between those that have been married in the past and those that were never married. Virtually all of the sample report some work experience, and average years of work experience is relatively high. Nevertheless, only 20% of the sample were working at the baseline interview, and those that were working had fairly low weekly earnings. Finally, most sample members had spend a considerable fraction of the three years prior to the baseline interview on welfare.

The information in Table 2 leads to two conclusions about the long-term single parent welfare

¹⁵The sample was randomly selected from administrative rosters of IA recipients in the two sites who were single parents over 18 years of age and had received IA payments for at least 12 of the past 13 months. No other restrictions (for example, on health status) were imposed on eligibility. Thus, the experimental population is fairly representative of long-term single parent welfare recipients in Canada.

¹⁶The data in this Table and throughout this section pertain to the subsample of individuals in the SSP recipient demonstration who completed both the baseline and 36 month interview. This represents 87.2 percent of the original sample, roughly equally balanced between the program and control groups.

population. First, this group is largely comprised of poorly educated women from disadvantaged family backgrounds. Normally, such individuals would be expected to have low wages and intermittent employment histories. Second, although most of the SSP population has held a job sometime in the past, and many have long work histories, their recent labor market attachment is relatively weak. Thus, the SSP population could be characterized as having relatively disadvantaged permanent characteristics (such as education and family background) and relatively poor transitory outcomes (such as low levels of work in the past year). This combination suggests that, in the absence of the SSP program, one might expect to see some modest improvement in labor market outcomes for many of the sample members over the next few years, but that members of the SSP target population would be likely to experience low wages and intermittent employment rates for most of their lives.

Basic Impacts

Data are currently available on the outcomes of the program and control groups in the Recipient Experiment for three years following random assignment. Figure 4 shows average monthly employment, earnings, and welfare participation by members of the two groups over this period.¹⁷ Under random assignment, the two groups would be expected to have identical outcomes, apart from the presence of the SSP program. Thus, the difference in outcomes between the program and control groups (also shown in the graphs) is an estimate of the causal impact of the program in any particular month. An important feature of the graphs is the tendency for steady improvement in the outcomes of the control group. Even in the absence of SSP, long-term welfare recipients gradually move off welfare and experience rising employment and earnings. Relative to these trends, members of the program group have more rapid gains in employment and earnings, and leave welfare more quickly. As might be expected given the time-limited eligibility for SSP, the experimental impacts peak in months 12-14. (Recall that program group members

¹⁷Outcomes for the month just prior to random assignment are plotted as month -1 in the figures.

have to have left welfare and entered full time work within one year of random assignment in order to gain eligibility for 36 months of SSP payments). After this point the employment rate of the program group is roughly constant, while the rate of the control group continues to rise, leading to a gradual reduction in the experimental impact. A similar pattern appears in earnings and IA participation, although for these outcomes the program group continues to experience modest gains after months 12-14.

Table 3 summarizes the employment, earnings, and program participation outcomes of the control and program groups in each of the first three years of the Recipient Experiment. All the outcomes in this table are expressed as annual averages of monthly outcomes. In the interests of simplicity I do not report sampling errors of the various quantities. However, all of the experimental impacts in column 3 are significantly different from 0 at the 1% level. Consistent with the patterns in Figure 4, the effects of SSP are largest in the second year of the experiment, but are still sizeable in the third year. For example, the full time employment rate of the experimental group is over 50 percent higher for the program group than the control group (27.9% versus 18.3%). Notice that the fraction of full time workers in the control group is an estimate of the fraction of windfall beneficiaries of SSP, since these people would presumably be eligible for the supplement without changing their behavior.¹⁸ Assuming that all the full time workers in the program group are SSP recipients (which is an over-estimate), two thirds ($=18.3 \div 27.9$) of SSP recipients are windfall beneficiaries and one third ($=9.5 \div 27.9$) are “incentivized” beneficiaries – people who would not be working full time without the supplement. Thus, despite the targeted eligibility and full-time work requirements of SSP, it still has a relatively high fraction of windfall beneficiaries in the Recipient Experiment. (Using similar calculations the proportion of windfall beneficiaries is 63% in year 1 and 55% in year 2).

The presence of windfall beneficiaries explains another interesting feature in Table 3, which is the fact that the fraction of people who receive either IA or SSP is higher in the program group than the

¹⁸Almost no one in the control group earns “too much” to be eligible for SSP.

control group. Consider individuals who would be on IA and not working in the absence of SSP, but who would move to full time work under SSP. Each such “incentivized beneficiary” removes 1 from the count of IA recipients in the program group relative to the control group, but adds 1 to the count of SSP recipients. Thus, if this was the only group in the experimental population, the fraction of people who receive IA or SSP would be the same in the program and control groups. On the other hand, windfall beneficiaries (people who would be working full time in the absence of SSP) typically are off IA in the control group but receiving SSP in the program group. The presence of windfall beneficiaries therefore leads to a higher fraction of people on IA or SSP in the program group than the control group.

North American discussions of welfare reform often seem to be dominated by a simple question: does the proposed reform raise or lower government costs? A first look at this question is provided by comparing average of IA and SSP costs in the control and program groups. As shown in the bottom rows of Table 3, cash assistance costs for the long term welfare population in the Recipient Experiment are higher under SSP than under the alternative system.

A more complete accounting of government costs versus recipient benefits is provided in Table 4. The data in the upper panel pertain to the period between the 13th and 15th month of the Recipient Experiment, when the SSP impact on employment and earnings is close to its peak, while the entries in the lower panel pertain to the 36th month (the latest currently available). In terms of government cost, an important consideration of SSP is the fact that all of the extra earnings of the program group and their SSP income are taxed. Thus, the program group “gives back” some of its additional earnings and SSP payments. After accounting for income taxes, payroll taxes, and various other transfer programs, the program group receives a net transfer that is \$55 per month higher than the control group in months 13-15, and \$56 higher than the control group in month 36.

On the other side of the coin, individual recipients’ net incomes after taxes and transfers are \$179 higher for the program group than the controls in months 13-15, and \$153 higher in month 36. By

spending an additional \$55 per month on the experimental population, the SSP program raises after-tax incomes by \$150-\$180 per month. (Note that these are averages over the entire experimental population, and not just those who actually receive SSP or any other component of income). Depending on one's point of view, this could be interpreted as a reasonable investment. It is certainly a better ratio than was achieved in the Negative Income Tax experiments of the 1970s, which ended up raising net income by less than the government's added transfer cost. The extra cost of SSP relative to conventional welfare has a notable anti-poverty effect, reducing the fraction of people whose family income is below Canada's poverty threshold by 12 percentage points in months 13-15 and 9 percentage points in month 36.

A reasonable question raised by the results in Tables 3 and 4 is whether some further narrowing of the target population for SSP might have a more favorable impact. We will take this issue up further in Lecture 2, when we discuss the results of offering SSP to people who have only been on the welfare roll for exactly 12 months. With respect to the Recipient Experiment, however, some interesting comparisons across various sub-populations are summarized in Table 5. Here, using data for months 13-15 of the experiment, I report the fraction of SSP supplement recipients in the program group (column 1), the full-time employment rates of the control and program groups (columns 2 and 3), the impact of SSP on the full time employment rate, and an estimate of the fraction of all supplement takers who are windfall beneficiaries. Evidently, the impact of SSP and the fraction of windfall beneficiaries varies by education and labor force status just prior to the experiment. Although SSP appears to have a bigger impact on more highly educated workers, and those who were working or looking for work just before random assignment, the fraction of windfall beneficiaries is also bigger among these groups. Targeting SSP at very disadvantaged welfare recipients would therefore lead to a smaller impact, but perhaps a more favorable effect on government cost, since fewer of the highly disadvantaged are likely to be windfall beneficiaries.

How big a concern is the presence of windfall beneficiaries in the SSP Recipient Population? As so often seems to be the case, where one stands on this issue depends on one's attitude toward the

desirability of income support for low-income families. One view – often ascribed to North American voters in the 1990s – is that any increase in welfare spending is unacceptable. In this view, SSP for long-term welfare recipients is a “failure”. (We will see next lecture that SSP’s cost implications are different for samples of people with shorter welfare histories). However, SSP funds are only received by people who have left the regular welfare system and are working full time. In this regard SSP is like a restricted Earned Income Tax Credit that is only available for a limited time to full-time workers. As I noted earlier, although U.S. welfare caseloads have plummeted, the EITC program has grown enormously, and now outspends the traditional cash welfare program (TANF) by three or four to one. My interpretation of these trends is that increases in low-income assistance that are targeted to workers, and are not labeled “welfare”, may be quite saleable, even in the United States.

The results in Figure 4, and in Tables 2-5, raise a lot of other interesting questions. One very important question is this: would the presence of an SSP-style program ultimately backfire, because low-wage workers will flock into the welfare system in order to gain eligibility for SSP supplement payments? This is precisely the question that the other experiment in the SSP Demonstration, the New Applicant Experiment, was designed to test. I will discuss this in Lecture 2.

A second question concerns the labor market progress of those who take part in SSP. Is it true that motivating people to take a full time job – any job, even an entry-level job – will set off a process of skill accumulation that can ultimately pull them out of the welfare system? I will also examine this question in some depth in the next Lecture. A third and closely related question is, does it last? To the extent that program participants move up the wage ladder over the course of the demonstration, SSP is likely to have a permanent effect. To the extent that they do not, its long term effects seem more doubtful.

Reforming the Financial Incentives of the Welfare System: Lecture 2

IV. Would SSP Prolong Welfare Spells?

The SSP Recipient Experiment was conducted on a sample of long-term welfare recipients who were suddenly informed of their eligibility for the supplement. While the results from this experiment confirm that enhanced financial incentives can increase work effort and lower welfare participation, they do not necessarily provide much guidance on what would happen to the welfare caseload if SSP were made a permanent feature of the Income Assistance program. The reason is that the availability of SSP makes a longer spell of welfare participation more attractive. This could have two effects. On one hand, some individuals who otherwise would not be on welfare might decide to begin an IA spell – a “new applicant” effect. On the other hand, some IA recipients who otherwise would leave welfare within a year might decide to extend their stay to gain SSP eligibility – a “delayed leaver” effect. Either of these effects would be expected to raise the costs of a permanent SSP program over and above the costs that were measured in the Recipient Experiment, since new applicants and delayed leavers expand the number of people who are on welfare for at least a year, and also presumably raise the fraction of windfall beneficiaries among the recipient population who pass the 12 month mark.

Early in the planning of the SSP Demonstration the possibility of delayed leaver and new applicant effects was recognized, and it was decided to conduct a separate experiment to measure the potential magnitude of these effects. The ultimate design of this experiment, known as the Applicant Experiment, focuses only on the delayed leaver effect. The reasons for this are mainly related to cost and feasibility. The delayed leaver effect can be measured by informing a group of new welfare applicants that they are eligible for SSP if they stay on IA for a year, and comparing the duration of their welfare spells to those of a control group who are not eligible for SSP. By comparison, an experiment to assess the new entrant effect would have to inform a large group of low-income families that are not currently on welfare that if they entered IA and stayed for a year, they would be eligible for SSP. Such a design would require a very

large sample. It would also be hard to tell whether people believed or understood the offer of SSP. As it turns out, the results of the Applicant Experiment strongly suggest that any new entrant effect is likely to be small.

Design and Sample Characteristics

The Applicant Experiment was conducted at the Vancouver SSP site. One-half of a group of 3,300 single parents who started a new Income Assistance spell between February 1994 and March 1995 was assigned into the program group of the experiment, while the other half became the control group.¹⁹ The "treatment" received by the program group took the form of a letter and brochure informing them of their potential eligibility for SSP and explaining the nature of the supplement offer in more detail. (The same brochure was used in orientation sessions for the program group of the Recipient Experiment). Program group members were also mailed a reminder 7 months after their baseline interview re-explaining the supplement offer and the eligibility criteria. Some 11-12 months after random assignment (12-13 months after the start of their welfare spell) the two groups were re-interviewed, and members of the program group who had received IA payments in 12 of the 13 months since the start of their welfare spell were informed that they were eligible for SSP. From that point onward, eligible program group members were treated exactly like program group members in the Recipient Experiment, and had one year in which to find a full time job and begin receiving supplement payments.

Information from the baseline interview conducted just prior to random assignment provides a portrait of new welfare applicants that can be compared to the longer term recipients described in Table 2. New applicants are somewhat better-educated than longer-term recipients (41% high school dropouts

¹⁹Eligibility was predicated on not having received IA for at least two months prior to the new spell. New applicants were interviewed as quickly as possible after completing their IA application, and were then randomly assigned to the program or control groups. The date of random assignment is in most cases 1 or 2 months after the start of the welfare spell.

versus 53%), are more likely to have been married in the past (76% versus 57%), are less likely to have been raised by a single mother (35% versus 48%), and are a little less likely to report a physical disability. For the most part, then, new applicants could be described as “less disadvantaged” than longer term welfare recipients. The baseline survey also collected some information on reasons for beginning a spell of welfare. About one-third entered IA because of a relationship breakdown, while the remainder applied for welfare for a variety of reasons, including job loss, financial difficulties, etc. Many of those in the former group were relatively well-educated, and were presumably using welfare as a temporary measure until their family and living arrangements stabilized.

Was There a “Treatment”?

A fundamental issue in any social experiment is the question of external validity: does the design accurately reflect the program innovation that the experiment is meant to evaluate? This is an especially difficult question in the Applicant Experiment because the treatment is the provision of information about a *potential* benefit available one year in the future. To evaluate the quality of the information provided to the program group, a series of questions was asked in the survey conducted just prior to eligibility determination (11-12 months after random assignment). Members of the program and control group were asked about their understanding of various features of the IA system in British Columbia, and members of the program group were asked about their knowledge of the SSP supplement offer.

A summary of the answers is presented in Appendix Table 1. Among the program group, about three-quarters of the sample recalled being informed of their potential eligibility for SSP. (Keep in mind that these responses are averaged over the entire program group, including those who were only on welfare for only a few months 6-8 months prior to the interview). To probe their knowledge of the program individuals were asked an open-ended question "what does the self sufficiency project offer participants?" Just over one-half responded that it offered extra money if they took a job, or mentioned a wage

supplement. A similar fraction knew that they had to remain on welfare for a year to become eligible for SSP, and about 60 percent know that they had to find a job in order to qualify for benefits.

Based on these responses, it seems that between 50 and 75% of the program group had relatively good knowledge of the SSP program, including the fact that it would provide extra income, and the key eligibility requirements of receiving IA for a year and then leaving IA for a job. It is hard to gauge whether this is a reasonable level of knowledge. As a benchmark, members of the program and control groups were asked about other features of the regular IA system. About 55 percent knew that IA were recipients were allowed to earn some income without affecting their benefits, although only one-quarter knew the exact amount of the earnings disregard. The same fraction knew that some services (so-called transitional benefits) were available to individuals who left IA. These figures suggest that a majority of IA recipients and former recipients have some knowledge of long-established IA benefit provisions, although the knowledge is far from complete. Knowledge of the SSP supplement among program group members of the experiment appears comparable or slightly better.²⁰

Estimates of Delayed Leaving

How did the offer of SSP eligibility affect the rate that new welfare recipients left the IA system? The answer is provided in Table 6, and illustrated graphically in Figure 5. The first two columns of Table 6 show the fractions of the control and program groups ending their welfare spell in each month, while the third and fourth columns of this table show the fractions who have not yet left (i.e., the so-called “survivor functions”). The survivor functions are graphed in Figure 5. Because of the design of the Applicant Experiment, people who received IA for only 1 or 2 months are under-represented in the sample. These

²⁰A second source of information on the extent of knowledge about the SSP supplement is a set of focus-group interviews conducted on program group members about a year after the start of their welfare spell (Bancroft, 1996). When queried about the SSP supplement almost all participants (26 of 30) recalled the program, and a handful mentioned that they had been tempted to remain on IA by the supplement offer, although only one volunteered that she had stayed on IA specifically to get the SSP supplement

people were not on welfare long enough to be interviewed before they had left the system, and in many cases they were unwilling to participate in the experiment, or could not be located. Although this feature affects the program and control groups equally (since random assignment occurred after the baseline interview) it means that the percentages leaving IA after 1 or 2 months are lower in the experimental sample than would be observed from welfare records.²¹ In any case, a glance at Figure 5 suggests that members of the program group were only slightly more likely to remain on welfare for a year than were members of the control group. Indeed, as shown in Table 6, about 2.2% more of the program group remained on IA continuously for 13 months than the control group. The difference is just on the margin of statistical significance.²² Perhaps even more surprisingly, the program group has about as many people leaving welfare in months 10-11 (just prior to establishing eligibility for SSP) as the control group. There is little indication that people in the program group who would have otherwise left welfare in the 10th or 11th months “hung on” to obtain SSP eligibility.

Although the program group were initially told that they had to remain on welfare for 12 months to become eligible for SSP, this criterion was relaxed slightly to allow eligibility for anyone who collected IA payments in 12 of the 13 months following the start of their welfare spell. As shown in the bottom rows of Table 6, 54.3% of the control group met this criterion, versus 56.9% of the program group, implying an impact of 2.6 percentage points.

The results in Table 6 (and a much more extensive analysis in Card, Robins, and Lin, 1998) suggest that the delayed leaver effect caused by the potential availability of SSP is fairly small, on the order of 3 percent of all new applicants for IA. Moreover, there do not appear to be any differences in the

²¹It also means that the Applicant Experiment probably overstates the delayed-leaver effect, because the experimental sample includes too few very short-term welfare applicants, who seem to show no evidence of delayed-leaver effects.

²²If the probability of remaining on welfare is adjusted for various observed characteristics, the difference widens a bit (to around 4 percentage points) and is significant at the 10 percent level (Card, Robins, and Lin, 1998, Appendix Table 3).

employment or earnings of members of the control or program groups. There are several explanations for the modest size of this effect. Perhaps most importantly, over one-half of new welfare applicants stay on income assistance for a year or more anyway. These people are presumably unaffected by delayed leaver incentives. In addition, the evidence in Table 6 and qualitative evidence from focus group studies suggest that people who would normally leave welfare within 3 or 4 few months do not find the offer of SSP sufficiently attractive to alter their behavior. Thus, only about one-third of new applicants to income assistance (those who would have stayed on welfare for 5-11 months) are substantially "at risk" to change their behavior in response to the supplement offer. Finally, as is true for other features of the welfare system, not all participants in the entry effect experiment were fully aware of the details of the SSP supplement offer. Nevertheless, an examination of the behavior of those who were well informed about SSP shows that even in this group the delayed leaver effect is small (no bigger than 5 percentage points).

The conclusion that short-term income assistance recipients are unaffected by the supplement offer suggests the new applicant effect generated by SSP may also be quite small. If people who have already born the costs and stigma of applying for welfare are unwilling to stay on income assistance for an additional 9-12 months to get the supplement, it seems unlikely that people who would not otherwise enter welfare would be attracted to enter the system just to receive IA.

V. The Effects of SSP on New Applicants

Although the primary purpose of the Applicant Experiment is to measure the delayed leaver effect, this experiment also provides an opportunity to evaluate the behavioral impact of SSP on a different group of welfare recipients: those who received IA for just the minimum amount of time to be eligible. This is a particularly useful exercise because if SSP were made a permanent feature of the IA system, eventually all SSP participants would be of this type.

At present information is available on the outcomes of the program and control groups in the

Applicant Experiment for up to 36 months after random assignment. Since applicants had to stay on IA for a year before becoming eligible for SSP, this means that data are currently available for a period of up to two years after SSP was made available to eligible program group members. Figure 6 shows the fractions of the two groups receiving Income Assistance over the first 36 months of the experiment. At month 13, the program group has a roughly 3 percentage point higher IA participation rate, consistent with the results in Table 6. Within a few months, however, the fraction of the program group on IA drops significantly relative to the controls. This is attributable to the effect of SSP: by month 25 about 18 percent of the program group were receiving SSP payments. Since only 60 percent of the program group were actually eligible for SSP, however, the participation rate among the eligible subgroup is about 30 percent – slightly above the takeup rate at a comparable point for the program group in the Recipient Experiment.²³

Table 7 summarizes some key outcomes for the control and program groups of the Applicant Experiment in the period 24-30 months after random assignment (or roughly 18 months after SSP-eligible program group members were notified of their eligibility). The upper panel shows labor market and program participation results in months 25-27, while the lower panel shows the components of incomes and government cost, averaged over the 6-month period from months 25 to 30. The effects of SSP on the employment and IA participation rates of the applicant sample are comparable to its effects on longer-term recipients: employment is raised by about 12 percentage points (a little bigger than the 9.7% employment effect in year 2 of the Recipient Experiment) and IA participation is lowered by 12 percentage points (about the same as the effect in year 2 of the Recipient Experiment). However, the effect on earnings is much larger for the applicants: \$242 per month versus just over \$100 in the second year of the Recipient Experiment. Keeping in mind that only 60 percent of the program group in the Applicant Experiment were eligible for SSP, these impacts are quite remarkable. If one assumes that the offer of SSP has no

²³In the 12th month of the Recipient Experiment the fraction of the program receiving supplement payments was about 20 percent.

effect on the behavior of the people in the program group who left IA before establishing eligibility, the employment and IA impacts *per eligible program group member* are almost twice as large in the Applicant Experiment as the Recipient Experiment, while the earnings impacts are four times larger!

The results in the lower panel of Table 7 are also quite remarkable. Although the program group gets more in SSP supplement payments than it saves in IA costs, it also pays considerably higher taxes than the control group, and this higher tax revenue more than recovers the government's costs. Thus, SSP increases the net incomes of the program group while saving the government money on transfer costs. As in the Recipient Experiment, the added income of the program group leads to a sizeable reduction in the poverty rate.

The key to the relatively favorable cost implications of SSP for the new applicant population is the high level of earnings among those who were induced by the SSP incentive to enter full time work. Program group members with higher earnings get a smaller SSP supplement payment, and pay higher taxes. Looking at the additional hours and additional earnings of program group members relative to controls it is possible to compute the "marginal wage" associated with the extra work of the program group. In the Applicant Experiment the program group worked 20 extra hours per month relative to the controls in months 25-27 after random assignment, and earned \$242 more, implying a marginal wage of \$12.10 per hour. By comparison, in the Recipient Experiment the program group worked 18.4 extra hours per month relative to the controls in months 12-14 after random assignment, and earned \$122 more, implying a marginal wage of only \$6.60 per hour.²⁴

The higher wage rates enjoyed by members of the program group in the Applicant Experiment are partially explainable by a number of factors. First, this group is better educated, and has more recent job experience than program group members in the Recipient Experiment. Second, the Applicant Experiment

²⁴Note that the period 12-14 months after notification of eligibility for SSP occurs in months 25-27 of the Applicant Experiment.

was conducted solely in the Vancouver area, whereas about one-half of the Recipient Experiment sample are drawn from the relatively low wage province of New Brunswick. Even after accounting for these factors, and attempting to control for the amount of time in the welfare system, the relatively high earnings gains of the program group in the Applicant Experiment are not fully explainable (see Michalopoulos, Robins, and Card, 1999). The most likely explanation is that people who have only been on welfare for a relatively short period of time are substantially different than those who have spent longer on the rolls (or are viewed as different by potential employers). Regardless of the explanation, the relatively favorable picture of SSP for recent welfare applicants is largely attributable to the wage opportunities available to people in this population who are induced by the supplement to find a full time job.

VI. Wage Growth Among Welfare Leavers

A key question for a time-limited intervention like SSP is whether the program will have a lasting effect. This is especially relevant for the Recipient Experiment, since as was just noted, the impacts on hours of work and earnings in this experiment suggest that the wages obtained by long-term welfare recipients “incentivized” by SSP are relatively low.²⁵ Proponents of SSP have expressed the hope that the work experience gained by members of the program group in the three years of supplement eligibility will lead to significant wage growth. To the extent that such wage growth actually occurs, there is some possibility that SSP will have a lasting effect. In the absence of wage growth, many of those who took advantage of the SSP supplement may be forced to return to welfare at the end of their eligibility period.

In this section I present some evidence on the pace of wage growth for members of the program group in the Recipient Experiment who were induced to enter work by the SSP incentive. In view of SSP’s eligibility rules, and the pattern of impacts in Figure 4, the starting point for measuring wage

²⁵I apologize for the use of the word “incentivized”, but I have been unable to come up with an efficient alternative.

growth is taken to be the period just after the close of the 1-year window that program group members had to find a full time job and initiate a supplement payment (i.e. months 12-14 after random assignment). The end point is taken to be months 33-35, which are the latest for which labor market data are currently available.

Table 8 shows a variety of characteristics of individuals in the Recipient Experiment who were working in months 12-14.²⁶ The first two columns present data for the subsets of the workers in the control and program groups, respectively. Among the control group, a total of 691 individuals, or 28.1 percent of the population, reported positive hours in at least 2 months between months 12 and 14. Among the program group the corresponding number was 1,015 individuals, or 40.6 percent of the program group. Since under random assignment the behavior of the control group provides an estimate of what the program group would have done in the absence of SSP, these numbers imply that 32 percent of all those in the program group who were working in months 12-14 were incentivized by SSP, while 68 percent would have been working even in the absence of the program.

An interesting pattern shown by the characteristics in rows 1-5 is that members of the control group who were working in months 12-14 seem to have better “skill characteristics” than members of the program group (i.e., higher education, fewer young children, more work experience, and stronger labor market attachment in the period prior to random assignment). This is consistent with the notion that SSP causes some people to enter the labor market who would not have otherwise been working. Using the fact that workers in the program group consist of people who would have worked even in the absence of SSP, and those who were incentivized by SSP, it is possible to derive the average characteristics of the incentivized program group: these are shown in the third column of the Table. Perhaps the most interesting feature of the incentivized group is that fact that virtually none of the group was working at the

²⁶In order to focus more directly on people who had a reasonably strong labor market attachment by one year after random assignment, “working” is defined as reporting positive hours of work in at least two of the three months between the 12th and 14th month.

baseline interview.

While the top few rows of Table 8 show the pre-program characteristics of the control, program, and incentivized program groups, similar methods can be used to compare post-random assignment outcomes. Of particular interest are the wages of the jobs obtained by the incentivized program group, relative to the jobs of the non-incentivized group. Data on the wage outcomes, measured relative to the minimum wage in the appropriate SSP site, are presented in the bottom rows of the table.²⁷ Note that an implicit assumption used to derive the wage distribution of the incentivized program group is that the presence of SSP has no effect on wages of people who would have been working without the supplement.

A comparison of wage outcomes for the program and control groups indicates that the jobs obtained by the incentivized program group pay relatively low wages. For example, 14.4 percent of the program group report wages within 5 cents of the minimum wage, compared to 8.7 percent of the controls. The assumption that all of the additional above-minimum-wage jobs in the program group are attributable to the incentivized program group leads to the inference that 27.3 percent of the incentivized program group earned within 5 cents of the minimum wage, and another 54.2 percent earned from 5 cents to a \$1 above the minimum.²⁸ The remainder (27.2 percent) were paid \$1-2 above the minimum, with virtually none paid more than \$2 over the minimum wage. By comparison, close to 40 percent of control group workers in months 12-14 earned at least \$2 above the minimum wage. The relatively low wages of the incentivized program group potentially explain why these people would not have been working in the absence of SSP.

²⁷This normalization helps adjust for the fact that overall wage levels in New Brunswick are about 20% below those in Vancouver. The minimum wage in British Columbia was 20-30% higher than in New Brunswick over the demonstration period.

²⁸As indicated by the negative entry in column 3 for the fraction of the incentivized program group earning missing or sub-minimum wages, this strict interpretation is probably incorrect. SSP is only available to paid employees who earn at least the minimum wage. This requirement may lead some people who would be working in the absence of SSP to take slightly different jobs -- for example, hourly-rated versus piece-rate jobs.

What happened to wages of the program and control groups over the next two years? An answer is provided in Table 9, which shows wage outcomes from months 12-14 to months 33-35 for the program and control groups. As shown in the first row of this table, just over two-thirds of those who were working in months 12-14 were also working in months 33-35.²⁹ The difference in re-employment rates between the program and control groups is small and statistically insignificant. This is reassuring, since if a much higher fraction of either group was employed in the later period, one would be concerned about differential selection biases in the observed wage growth patterns of the two groups.³⁰ The second row shows the mean log wages of the two groups in months 12-14. At this early stage of the SSP experiment, the program group workers have about 6 percent lower wages than the control group. This gap is consistent with the wage distributions in Table 8, which show that the extra employment attributable to the incentive effect of SSP is concentrated at relatively low wages. Indeed, if one assumes that 68 percent of program group workers would have had the same wages as the control group in the absence of the program, then the mean log wage of the incentivized workers was about 20 percent below the mean wages of those who would have worked even without the SSP subsidy.

The third row of Table 9 shows the mean log wages in months 12-14 for the subset of workers who were subsequently re-employed in months 33-35. This subgroup is positively selected: the gap in wages between all workers and those who were re-employed 21 months later is 4 percentage points in both program groups. The similarity in the extent of selection bias among those who were re-employed in the two groups is again reassuring. Finally, the last three rows show three different estimates of wage growth for those who were employed in months 12-14 and 33-35. The first is just the average of measured wage

²⁹Again, employment in months 33-35 is defined as reporting positive hours in 2 or more of the 3 months. Results based on other definitions are similar.

³⁰Card, Michalopoulos, and Robins (1999) presents a detailed discussion of selectivity bias issues in the measurement of wage growth. Roughly speaking, many models of selection bias imply that if the fraction of the sample with observed data is the same in two groups, the degree of selection bias is the same.

growth; the second is mean growth, calculated after censoring a small number of “outliers”; and the third is median wage growth. Mean wage growth is a little slower for the program group than the control group, although the gap is far from statistically significant. (The t-ratio is 1.3). As shown in rows 6 and 7, however, the differential in growth rates completely disappears when medians, rather than means are analyzed, or when the wage growth observations are trimmed to eliminate the influence of extreme outliers. Based on these results, I conclude that the program and control groups had virtually identical wage growth rates over the second and third years of the Recipient Experiment.

As in Table 8, the difference in mean wage growth between the program and control groups can be translated into an estimate of the rate of wage growth for the incentivized program group. The key assumption needed to justify this exercise is that the presence of SSP has no effect on wage growth for people who would have been working without the supplement. Although this is not fully testable, some direct evidence is provided by comparing wage growth rates for individuals in the program and control groups who were working at the baseline of the SSP experiment. In Table 8 it was observed that only 1.7 percent of the incentivized program group were working at the baseline: thus, virtually all those who were working at the baseline are in the non-incentivized group. Comparisons of wage growth for people who were working at the baseline shows very small and statistically insignificant differences between the program and control groups, suggesting that the availability of SSP has no effect on wage growth for those who would have been working in months 12-14 even without the supplement.

Assuming then that SSP has no effect on the non-incentivized program group, one can conclude from the results in Table 9 that the incentivized and non-incentivized members of the Recipient Experiment program group have similar and very modest wage growth rates: approximately 6-8 percent over two years.

Some of this is attributable to inflation (3-4 percentage points) and a minor fraction may also be

attributable to rises in the minimum wage that took place over the period of the demonstration.³¹ After accounting for these two factors the average wage growth of the incentivized program group was about 2 percent per year. Interestingly, this growth rate is very close to the rate implied by the cross sectional association between previous work experience and wages at the baseline interview of the SSP experiment.³² It is also fairly similar to the rate of growth of wages with respect to experience estimated by Gladden and Taber (2000) in a recent study of wage growth among less-skilled workers in the U.S., using data from the National Longitudinal Survey of Youth. On balance, it seems that people who were induced by SSP to leave welfare and enter full-time work experienced about the same wage growth rates that would be predicted from a conventional analysis of age-earnings profiles, ignoring any special characteristics of welfare leavers. Unfortunately for the welfare leavers, this growth rate is quite modest.

VII. Summary and Implications for Welfare Reform

Although the SSP demonstration is still underway, I believe that the results so far suggest a number of important conclusions about the characteristics and behavior of single parent welfare recipients that are relevant to the general issue of welfare reform.

1. A sizeable fraction of long-term welfare recipients will respond to enhanced financial incentives for work, even if these incentives are restricted to full-time workers. Such incentive programs significantly increase the net incomes of working poor families, although they may also raise total transfer costs. The

³¹The effect of minimum wage increases is examined in some detail in Card, Michalopoulos, and Robins (1999). Contrary to my expectations, rises in minimum wages do not seem to directly explain much of the measured wage growth. I suspect that when minimum wages rise, so do wages “just above” the minimum, making it very hard to isolate the minimum wage effect.

³²A series of conventional human capital earnings functions were fit to data on wages at the experimental baseline. Following conventional practice, these models included controls for education and gender, and a quadratic function of measured years of labor market experience as of the baseline. The estimates imply that the rate of growth of wages per year of work experience is 2.4%, at the mean level of experience in the SSP population.

income gains of participants exceed the rise in government costs by a ratio of at least 3 to 1.

2. Long-term welfare recipients who are induced by financial incentives to leave welfare and enter work earn relatively low wages, and have only modest wage growth. In light of these low wage opportunities the decision to remain on welfare in the absence of incentives makes “economic sense”. Moreover, the slow pace of wage growth for welfare leavers suggests that simply encouraging work will have only a limited effect on future earning power.

3. Shorter-term welfare recipients who leave welfare face significantly better wage opportunities than their longer-term counterparts. In view of this fact, financial incentive programs focused on shorter-term recipients may be more effective at encouraging work, and may have more favorable cost implications for the government. Indeed, the offer of SSP to recent welfare applicants who stay on the rolls for a year leads to a small reduction in government transfer costs (relative to the existing welfare system), and at the same time leads to substantial increases in income.

4. The offer of a financial incentive plan to those who have been in the welfare system for a year leads to only a modest slow-down in the rate of leaving welfare. Even taking into account this delayed-leaver effect, SSP is cost-effective relative to conventional welfare.

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Table 1: Key Features of the SSP Recipient Demonstration

A. Program Eligibility

- single parents who have received Income Assistance (IA) for at least 12 months
- sample members drawn from IA registers in lower mainland British Columbia (including Vancouver) and southern New Brunswick (including Saint John)
- sample members randomly assigned between November 1992 and February 1995
- 2,859 single parents assigned to program group; 2,827 assigned to control group

B. Program Features

- supplement payments are available to program group members who are not receiving IA and who work at least 30 hours per week (over a four-week or monthly accounting period)
- supplement recipients must earn at least the minimum wage (\$5.00 per hour in New Brunswick in 1993; \$6.00 per hour in British Columbia in 1993)
- supplement payment is 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
- supplement payment is not affected by unearned income, or income of spouse/partner
- supplement payments are treated as regular income for income tax purposes
- supplement payments are available for up to 36 months from time of first payment. Supplement is only available to program group members who successfully initiate their first supplement payment within one year of random assignment
- program group members can return to IA at any time. Supplement payments are re-established if an eligible program group member leaves IA and meets the full-time hours requirement
- employers are not informed of SSP status. Program group members apply for supplement payments by mailing in copies of pay stubs (which show weekly hours)

Table 2: Characteristics of Single Parent Long-term Welfare Recipients

	All	British Columbia	New Brunswick
<u>Personal Characteristics:</u>			
Percent Female	95.6	95.5	95.7
Percent Under Age 25	26.5	23.0	30.1
Percent High School Dropouts	53.5	53.3	53.8
Percent Immigrants	13.3	23.4	2.8
Percent with Physical Work Limitation	25.2	25.9	24.5
Percent Raised by Single Parent	39.6	43.0	36.0
<u>Family Structure:</u>			
Percent with Only 1 Child	51.9	47.6	56.4
Percent Never Married	48.7	43.5	54.1
<u>Work and Welfare Experience:</u>			
Percent Ever Worked	94.7	95.3	94.1
Average Years of Work Experience	7.4	8.1	6.6
Percent Working at Baseline	19.6	18.9	20.2
Average Earnings/Week if Working	135.3	161.5	112.9
Average Number of Months On IA in Last 3 Years	29.9	28.8	30.9
Sample Size	4,961	2,537	2,424

Table 3: Labor Market and Program Participation Outcomes in First Three Years of Recipient Experiment

	Control Group	Program Group	Difference
<u>Percent Employed:</u>			
Year 1	25.3	29.7	4.4
Year 2	30.4	40.1	9.7
Year 3	33.1	39.9	6.8
<u>Percent Employed Full Time:</u>			
Year 1	11.4	18.0	6.6
Year 2	15.8	28.5	12.7
Year 3	18.3	27.9	9.5
<u>Average Monthly Earnings:</u>			
Year 1	183	232	48
Year 2	264	370	106
Year 3	324	396	71
<u>Percent Receiving IA:</u>			
Year 1	91.7	85.4	-6.3
Year 2	78.9	65.9	-12.9
Year 3	70.7	61.3	-9.4
<u>Percent Receiving IA or SSP:</u>			
Year 1	91.7	94.0	2.4
Year 2	78.9	86.5	7.6
Year 3	70.7	80.6	9.9
<u>Average Monthly IA/SSP Payment:</u>			
Year 1	792	851	59
Year 2	689	779	90
Year 3	593	682	89

Notes: Entries are annual averages of monthly outcomes. Samples include 2,458 members of control group and 2,503 members of program group. All differences in column 3 are all statistically significant at the 1% level.

Table 4: Effects of SSP on Income and Government Cost

	Control Group	Program Group	Difference
<u>Months 13-15:</u>			
Average Earnings	222	347	124
Average SSP Payments	0	196	196
Average IA Payments	723	621	-103
Average Income Taxes	14	53	39
Average Net Transfers	709	764	55
Average After Tax Income	932	1111	179
Percent Below Poverty Threshold	84.8	77.5	-12.2
<u>Month 36:</u>			
Average Earnings	348	406	58
Average SSP Payments	0	156	156
Average IA Payments	573	506	-67
Average Income Taxes	61	94	33
Average Net Transfers	757	814	56
Average After Tax Income	1198	1301	153
Percent Below Poverty Threshold	86.0	77.0	-9.4

Notes: Income taxes include federal and provincial income taxes as well as payroll taxes, and are estimated using tax schedules and data on income and family structure. All differences in column 3 are statistically significant at the 1% level.

Table 5: Differences in the Effect of SSP and in the Fraction of Windfall Beneficiaries

	Percent of Program Group Receiving SSP	<u>Full-Time Employment Rate:</u>			Estimated Percent Windfalls
		Control Group	Program Group	Difference	
All	23.0	14.0	29.3	15.2	33.9
<u>By Education:</u>					
High School Dropouts	17.5	10.1	23.6	13.5	22.9
High School Graduates	29.3	18.8	35.9	17.1	41.6
<u>By Labor Force Status at Baseline:</u>					
Employed Full Time at Baseline	56.8	50.8	64.2	13.4	76.4
Employed Part Time at Baseline	37.9	23.7	46.9	23.2	38.8
Looking for Work at Baseline	26.0	15.1	32.0	16.9	35.0
Not in Labor Force at Baseline	14.0	7.0	20.5	13.5	3.6

Notes: Estimated percent of windfall beneficiaries in fifth column is

Table 6: Percent of Recent Applicants Leaving Income Assistance in Each Month, and Cumulative Percent Still on Welfare

Month	Percent Leaving IA in Month:		Percent Still On IA Continuously Since Entry		
	Controls	Programs	Controls	Programs	Difference
1	0.5	1.0	99.5	99.0	-0.6
2	3.7	4.2	95.8	94.8	-1.0
3	8.3	8.3	87.5	86.5	-1.0
4	8.8	7.6	78.7	78.9	0.2
5	6.2	5.5	72.5	73.4	0.9
6	5.2	4.9	67.2	68.5	1.3
7	4.7	4.2	62.5	64.3	1.8
8	3.5	3.5	59.0	60.9	1.9
9	2.8	2.9	56.2	58.0	1.8
10	3.2	2.6	53.0	55.4	2.4
11	2.6	2.9	50.4	52.5	2.2
12	2.3	1.8	48.1	50.8	2.7
13	1.6	2.0	46.6	48.8	2.2
14+	46.6	48.8	--	--	--

Addendum: Percent Receiving IA in 12 of 13 months since start of IA spell

Controls: 54.3
 Programs: 56.9
 Difference: 2.6

Notes: samples include 1,667 control group members and 1,648 program group members. None of the differences in column 5 are statistically different at the 5 percent level.

Table 7: Summary of the Impacts of SSP on the Labor Market and Program Participation Status, and on Components of Income and Government Cost - Applicant Experiment

	Control Group	Program Group	Difference
<u>A. Labor Market and Program Participation (Months 25-27):</u>			
Percent Employed	42.8	54.9	12.1
Percent Employed Full Time	28.7	41.2	12.5
Average Monthly Earnings	610	853	242
Percent Receiving IA	49.6	37.7	-12.0
Percent Receiving IA or SSP	49.6	56.0	6.4
<u>B. Components of Income and Government Cost (Months 25-30):</u>			
Average Earnings	613	836	223
Average SSP Payments	0	154	154
Average IA Payments	449	352	-97
Average Income Taxes	115	193	78
Average Net Transfers	600	571	-29
Average After Tax Income	1355	1529	174
Percent Below Poverty Threshold	68.5	57.2	-11.3

Notes: Samples include 1,430 members of the program group and 1,422 members of the control group of the SSP Applicant Experiment. All differences in column 3 except the difference in average net transfers are statistically significant at the 1% level. The difference in net transfers has a sampling error of \$26.

Table 8: Comparison of Members of Control Group, Program Group, and Incentivized Program Group Who Were Working in Months 12-14 of Recipient Experiment

	Control Group	Program Group	Incentivized Program Group
<u>Individual Characteristics:</u>			
Percent High School Dropouts	39.5	43.2	51.4
Percent with Child Under 5	40.9	44.7	53.1
Average Years of Work Experience	9.6	8.7	6.6
Percent Who Worked in Year Before Baseline	55.3	45.2	22.5
Percent Working at Baseline	52.1	36.7	1.7
<u>Hourly Wage Outcomes:</u>			
Percent with Missing or Sub-minimum Wage	16.8	9.2	-8.1
Percent within 5 cents of Minimum Wage	8.7	14.4	27.3
Percent from 5 cents to \$1 Above Minimum Wage	18.1	29.2	54.2
Percent from \$1 to \$2 Above Minimum Wage	17.8	20.7	27.2
Percent More than \$2 Above Minimum Wage	38.6	26.6	-0.6
Sample Size	691	1,015	--

Notes: Incentivized program group refers to members of the program group who would not have been working in months 12-14 in the absence of SSP. Wage distribution for this group are estimated by assuming that availability of SSP has no effect on wage outcomes of those who would have been working in the absence of SSP.

Table 9: Comparisons of Wage Growth Between Months 12-14 and Months 33-35 of SSP Recipient Experiment

	Control Group	Program Group	Difference
Percent Working in Months 33-35	67.1	68.3	1.1
Mean Log Hourly Wage in Months 12-14	1.97	1.91	-0.06*
Mean Log Hourly Wage in Months 12-14, if Working in Months 33-35	2.01	1.95	-0.06*
Mean Log Hourly Wage in Months 33-35	2.11	2.00	-0.11*
Mean Log Wage Growth from Months 12-14 to 33-35	0.09	0.05	-0.04
Mean Log Wage Growth from Months 12-14 to 33-35 (Outliers Censored)	0.08	0.08	0.00
Median Log Wage Growth from Months 12-14 to 33-35	0.06	0.07	0.01

Notes: Differences in column 3 indicated with * are statistically significant at the 5% level. Other differences are not statistically significant.

Appendix Table 1: Knowledge of SSP and IA Program Rules in the SSP Applicant Experiment

A. Knowledge of SSP Program (Program Group Only)

	Percent of Program Group
1. Responded "yes" to question: "...were you informed that you would be eligible for SSP?"	75.1 (1.1)
2a. Without prompting responded that SSP offers extra extra money if I get a job (or similar language)	55.2 (1.3)
2b. With or without prompting responded that SSP offers extra money if I get a job (or similar language)	77.5 (1.1)
3. Responded to question "How long does someone have to be on IA to receive money from SSP..."	
One year from first IA check	51.9 (1.3)
Some other specified time	17.1 (1.0)
Don't Know	31.0 (1.2)
4a. Without prompting responded that someone must do the following to receive SSP:	
Find a job	61.2 (1.2)
Leave IA	25.7 (1.1)
Work at least 30 hours per week	37.9 (1.2)
Enroll in school or training	13.2 (0.9)
4b. Without or without prompting knew that that someone must do the following to receive SSP:	
Find a job	83.3 (1.0)
Leave IA	67.6 (1.2)
Work at least 30 hours per week	72.6 (1.1)

Note: table continues. See notes at end of table.

Table 2, continued

B. Knowledge of IA Program (Program and Control Group)

	Overall	Program Group	Control Group
<u>Knowledge of IA Program</u>			
5. Responded to question: "Can people earn money without affecting their IA benefit"			
Yes	55.5 (0.9)	55.6 (1.3)	55.5 (1.3)
No	30.7 (0.8)	30.8 (1.2)	30.7 (1.2)
Don't know	13.7 (0.6)	13.5 (0.9)	13.9 (0.9)
6. Responded "yes" to previous question, and know the maximum amount is \$200 per month	25.7 (0.8)	25.1 (1.1)	26.3 (1.1)
7. Responded to question: "If someone leaves IA for a full-time job, are there services or additional benefits they can apply for"			
Yes	55.6 (0.9)	54.3 (1.3)	57.0 (1.3)
No	21.7 (0.7)	23.1 (1.1)	20.3 (1.0)
Don't know	22.6 (0.8)	22.5 (1.1)	22.7 (1.1)
8. Sample Size	3,055	1,528	1,527

Notes: Standard errors in parentheses. Tabulations based on responses to 12 Month Survey.

Figure 1: Welfare Recipients as a Percent of Total Population

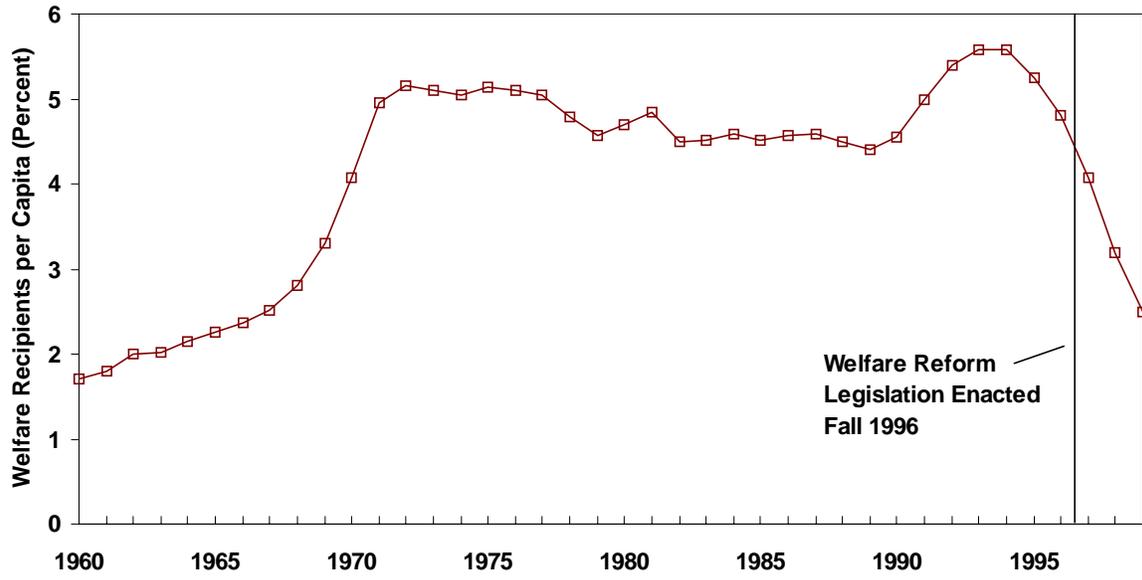


Figure 2: Budget Constraint of Conventional Welfare System

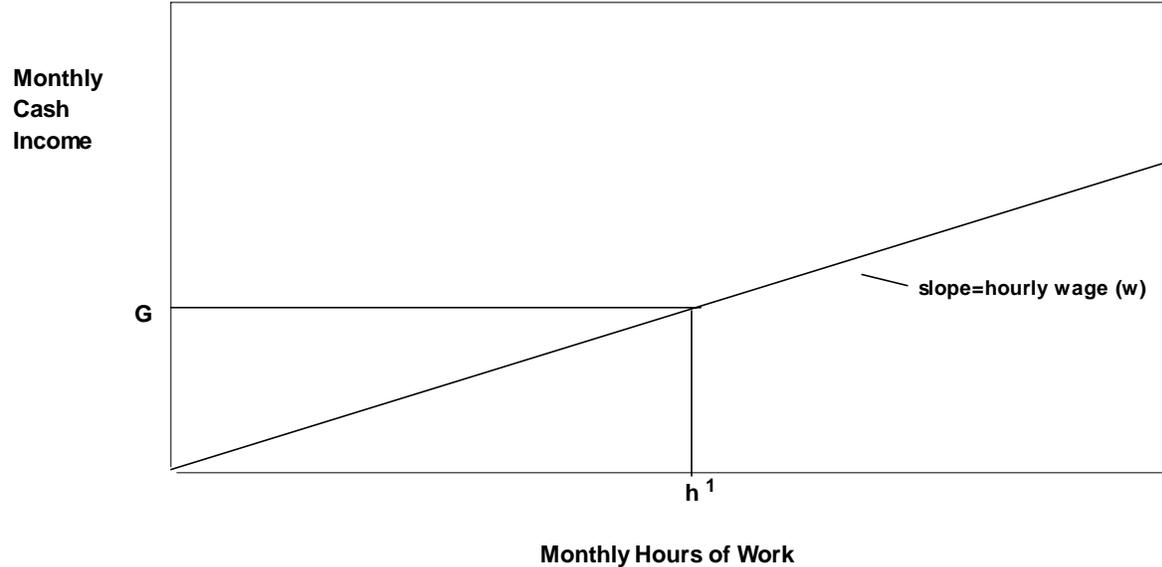


Figure 3: Budget Constraint with Financial Incentives

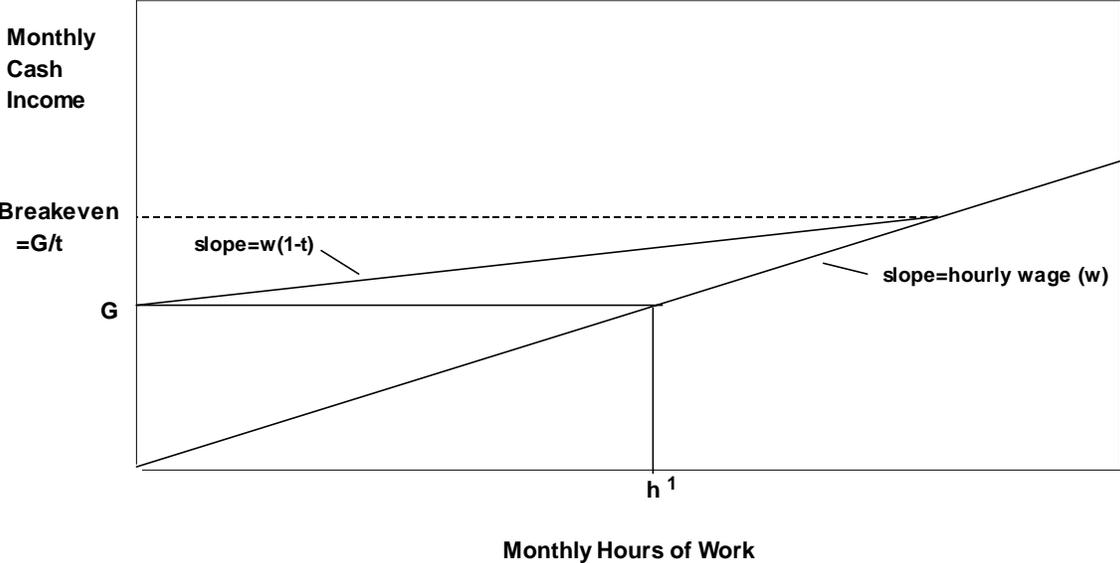


Figure 4a: Monthly Employment Rates

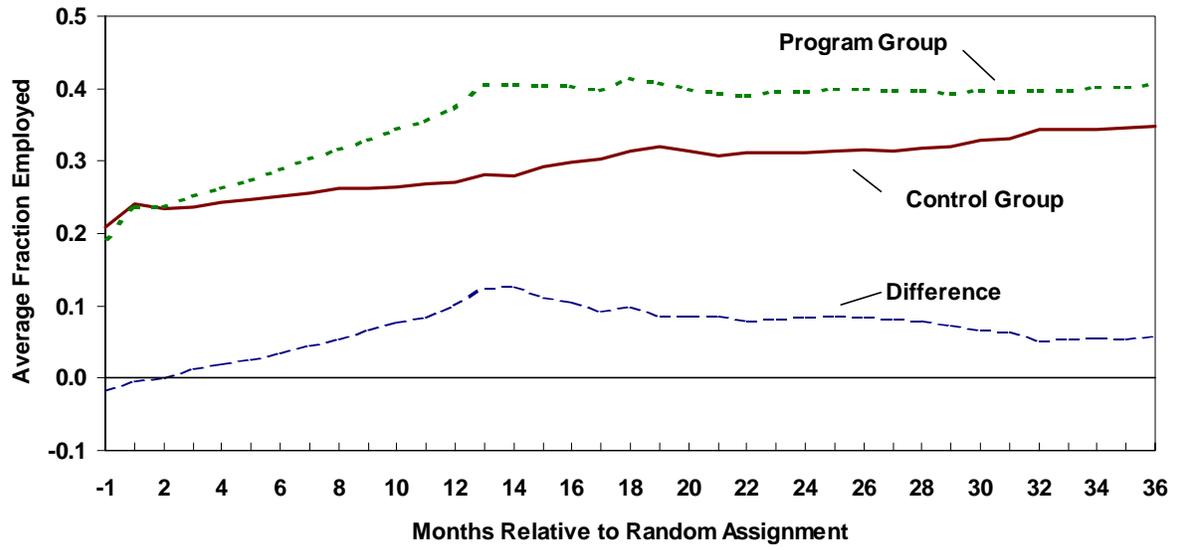


Figure 4b: Montly Earnings

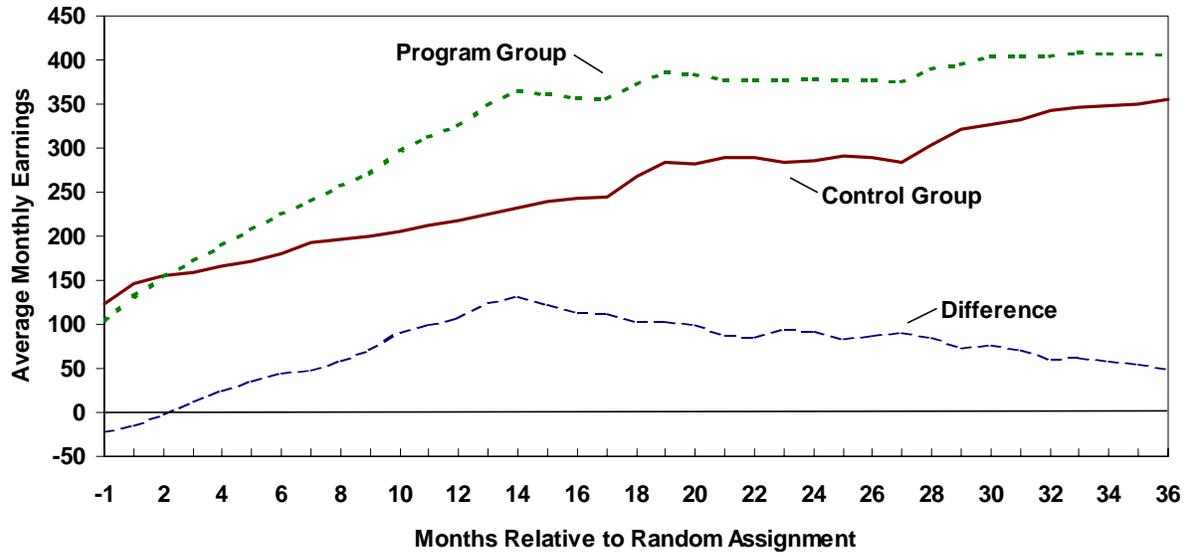


Figure 4c: Monthly Welfare Participation Rates

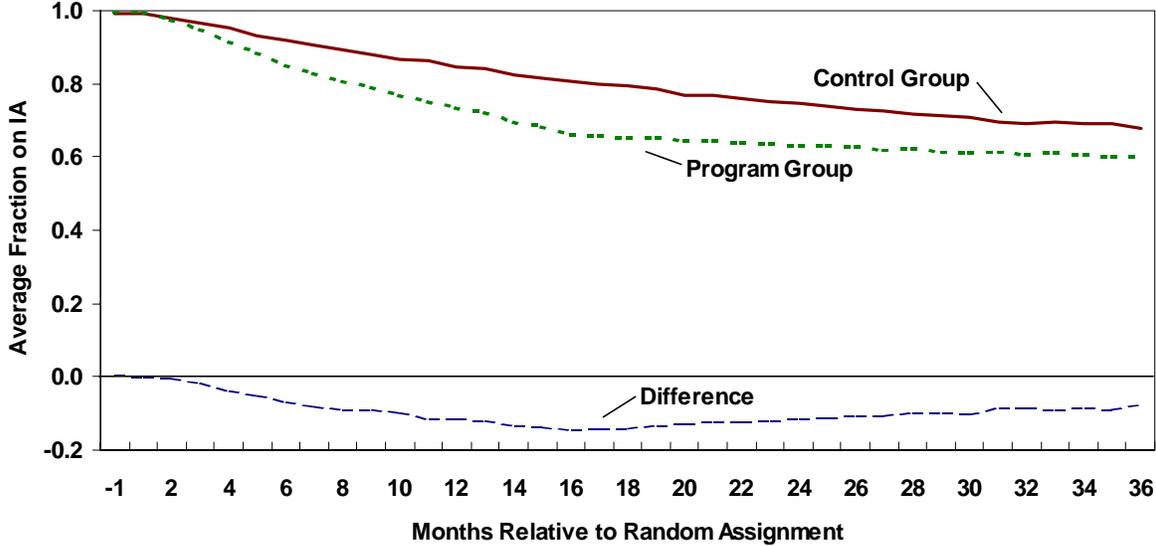


Figure 5: Percent of Applicants Still on Income Assistance

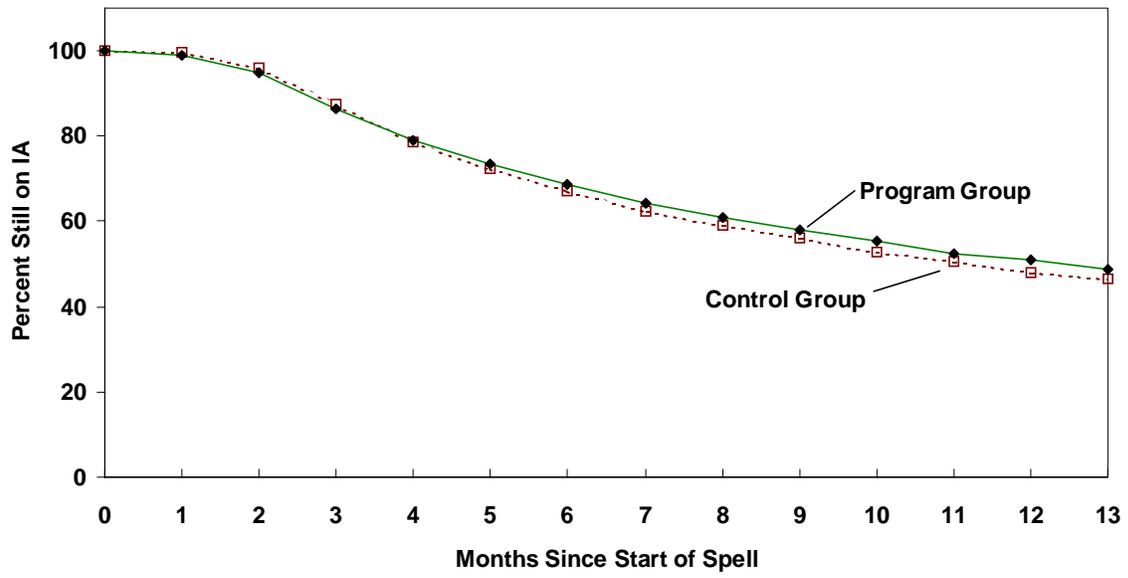


Figure 6: Welfare Participation Rates in Applicant Experiment

