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Measuring Wage Growth Among Former Welfare Recipients

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ABSTRACT

We study the rate of wage growth among long-term welfare recipients in the Self Sufficiency Program (SSP) who were induced by the financial incentives of the program to enter the work force. We find that single parents who began working in response to the SSP incentive are younger, less educated, have more young children, and have less positive attitudes toward work than those who would have been working regardless of SSP. They also earn relatively low wages in the first few months of work: typically within $1 of the minimum wage. Despite these differences, their rate of wage growth is similar to other workers. After adjusting for inflation and potential effects of rising minimum wages, we estimate that people who were induced to enter work by SSP experienced real wage growth of about 1.6 to 2.6 percent per year – a range that is consistent with the cross-sectional relationship between wages and labor market experience in the control group of the experiment, and with the existing literature on wage growth of less-skilled workers.

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A key question for understanding the long run impact of welfare reform policies is whether welfare recipients who find jobs will experience rapid wage growth within their first few years of work, or whether their labor market opportunities will improve only modestly over time. This concern is especially relevant for programs targeted at long-term welfare recipients, since long-term recipients often lack the skills to immediately obtain higher-wage jobs. The Self Sufficiency Program (SSP), a welfare reform program currently being evaluated in two Canadian provinces, offers a striking illustration of the issues. SSP provides a generous three-year earnings subsidy to long-term welfare recipients who leave welfare and enter full-time work. The program has significant incentive effects: within 15 months, single parents who are offered the subsidy have a 10-15 percentage point higher full-time employment rate than members of a randomly-assigned control group (Lin et al, 1998). Nevertheless, the wage rates associated with these jobs are relatively low. Two-thirds of those who entered work because of the SSP supplement were earning no more than $1 above the minimum wage; most of the rest were earning between $1 and $2 above the minimum wage. In the absence of significant wage growth, it is possible that many of those who took advantage of the SSP supplement will return to welfare at the end of their SSP eligibility period.

This paper attempts to measure the rate of wage growth experienced by members of the SSP treatment group in the first three years of the program.[1] A key methodological problem is that wage growth can only be measured for those who enter the labor market. Thus, it is impossible to compare average wage growth for *all* members of the SSP program group to average wage growth for *all* members of the control group. Moreover, since SSP induces some

[1]This is sometimes referred to as the rate of "wage progression".

people to find jobs who would not be working in the absence of the program, the subset of workers in the program group includes more relatively disadvantaged workers than the subset of workers in the control group. A further complexity is that wage growth is only observed for individuals who enter the labor market *and* are observed working at some later date. Because of these partial observability problems, any analysis of wage growth must rely on assumptions that are stronger than the (very weak) assumptions needed to draw valid inferences on fully observable outcomes in a randomized experiment, such as employment or welfare participation.[2]

Part I of this paper introduces a conceptual measure of *relative* wage growth for welfare reform programs like SSP that cause some people to enter the labor market who would not have been working otherwise. The proposed measure is the average change in wages over a fixed time period for individuals who are offered the program and are induced to work at the start of the period, minus the change for those who would have been working even without the program incentive. Under a restrictive but potentially testable assumption – namely, that the presence of the program has no effect on wage growth for those who would have been working in the absence of the program – the rate of relative wage growth for the "incentivized program group" can be inferred from the differences in wage growth between the program and control groups in a randomized evaluation setting.[3] Standard econometric methods are also presented that may reduce or eliminate selectivity biases in the measured wage growth of those who are employed

[2]A similar problem arising in analyzing the effect of training programs on the duration of employment or unemployment, even when training is randomly assigned. See Ham and Lalonde (1996).

[3]We are aware that "incentive" is a noun and use the term "incentivized" as a convenient shorthand.

both at the beginning *and* end of the observation window, relative to the population who worked at the start of the window.

Part II of the paper uses data from the first 36 months of the SSP experiment to examine the rate of relative wage growth among former long-term welfare recipients who were incentivized to leave welfare and enter work within the first year of the program. A comparison of characteristics of the incentivized program group relative to those who would have been working without the SSP incentive shows that the incentivized group is less-educated, has less work experience, and has less positive attitudes toward work. Members of the incentivized program group also earn significantly lower wages than those who would have been working in the absence of SSP. Despite these differences, their *wage growth* is similar to the wage growth of former welfare recipients who would have been working without SSP. Both groups experience about 8 percent nominal wage growth over the period from the 12th to 33rd month of the experiment. Results from several different approaches suggest that the measured wage growth of the SSP program and control groups is not differentially affected by selectivity biases, although our most general selectivity models are essentially uninformative. After accounting for inflation and possible effects of rising minimum wages, we conclude that individuals who were incentivized by the SSP program to enter work in the 12-14th month of the experiment had real growth of about 1.6-2.6 percent per year over the next 21 months. Although modest, this rate is comparable to predictions based on the cross-sectional association between wages and labor market experience in the SSP population., and with other research on the wage growth of low-skilled workers.

I. Defining and Measuring Relative Wage Growth

To begin our analysis it is useful to pose a precise definition of wage growth in the context of an incentive program like SSP that causes some people to find jobs who would not be working in the absence of the program. Suppose that a program is offered to a subset of individuals (the "program group", identified by P_i=1) beginning at date t=0 and continuing for periods t=1,2,...T. Define an initial reference date $s \geq 0$ and a final reference date $f > s$, and consider the subset of individuals in the program group who are working at the initial reference date. Conceptually, this subset can be divided into two groups: those who would have been working in period s even in the absence of the program, and those who would not. We refer to the latter as the "incentivized program group", and denote membership in this group by an indicator IP_i=1.[4] We refer to the former as the "non-incentivized program group", and denote membership in this group by NP_i=1.

A useful definition of relative wage growth is the rate of growth of wages from s to f for the incentivized program group, relative to the growth experienced by individuals who would have been working in period s in the absence of the program. If there is a randomized control group available (as we shall assume) the latter is just the wage growth experienced by members of the control group (denoted by P_i=0) who were working in s.[5] Let w_{it} denote the log wage of individual i in period t, and let E_{it} be an indicator variable equal to 1 for those who are working in period t. Then the relative wage growth of the incentivized program group is:

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[4]Note that the incentivized program group has to be defined relative to a particular time period <u>and</u> a particular outcome (e.g. working, off welfare, working full time). Since individuals are only eligible for SSP payments if they work full time, one might prefer to define the incentivized program group as those who were working full time in months 12-14. We discuss this choice below.

[5]In principle the rate of relative wage growth can be measured by comparisons with a non-experimental comparison group.

(1) $\Delta_{s,f} = E[\, w_{if} - w_{is} \mid E_{is}=1, IP_i=1 \,] - E[\, w_{if} - w_{is} \mid E_{is}=1, P_i=0 \,].$

The rate of relative wage growth can also be defined conditional on a set of observable characteristics x_i:

(1a) $\Delta_{s,f}(x_i) = E[\, \Delta w_i \mid x_i, E_{is}=1, IP_i=1 \,] - E[\, \Delta w_i \mid x_i, E_{is}=1, P_i=0 \,],$

where $\Delta w_i = w_{if} - w_{is}$. Note that x_i must refer to characteristics that are observed prior to any behavioral impact of the program.

There are two problems in estimating equation (1) or (1a). The first, which we defer for the moment, is that wages in the final period *f* are only observed for those who are working in that period. Average wage growth for the subset of people who were employed at both the beginning and end of the observation period may overstate or understate the growth in potential wages among those who were employed in the beginning period. The second is that we cannot directly identify the subset of program group members who were incentivized to work in period *s*.

Although program group members who are working in period *s* cannot be individually classified as incentivized (IP_i=1) or non-incentivized (NP_i=1), it is possible to identify the relative size and characteristics of these two groups by comparing workers in the program and control groups. Let $h(x_i)$ represent the probability that an individual with characteristics x_i would be working in period *s* in the absence of the program, and let h represent the mean of $h(x_i)$ over the experimental population. Similarly, let $g(x_i)$ represent the probability that an individual with characteristics x_i would not work in period *s* in the absence of SSP but would work with the SSP incentive, and let g represent the mean of $g(x_i)$ in the experimental population.[6] Note that $h(x_i)$ is estimable by the employment rate of individuals with characteristics x_i in the control group, while

[6]Note that $0 \le h(x_i) \le 1$, $0 \le g(x_i) \le 1$, and $0 \le h(x_i) + g(x_i) \le 1$.

$g(x_i)$ is estimable by the difference in employment rates between individuals with characteristics x_i in the program and control groups. The fraction of incentivized workers among all those in the program group with characteristics x_i who are working in period *s* is therefore

$$\phi(x_i) = g(x_i)/(\, h(x_i)+g(x_i)\,)\,,$$

which can be estimated for any particular subgroup, or for the overall experimental population. Moreover, the distribution of characteristics x_i in the incentivized program group has density

$$f(x_i \mid IP_i=1) = g(x_i)/g \times f(x_i),$$

where $f(x_i)$ is the unconditional density of characteristics in the experimental population. Thus, it is possible to compute the mean characteristics of the incentivized program group (or any other sample statistic for x_i) by computing a weighted average over the program group, using the relative weight $g(x_i)/g$.

Average wage growth for members of the program group with characteristics x_i who were working in *s* is just a weighted average of the growth rates for the incentivized and non-incentivized subgroups. Thus

(2) $E[\Delta w_i \,|x_i, E_{is}=1, P_i=1] = \phi(x_i)\, E[\Delta w_i \,|x_i, E_{is}=1, IP_i=1] + (1-\phi(x_i))\, E[\Delta w_i \,|x_i, E_{is}=1, NP_i=1].$

In general neither expectation on the right-hand side of equation (2) is observable. If the expected wage growth of the non-incentivized program group is equal to the expected wage growth of control group members who were employed in period s, however, then

(3) $E[\, \Delta w_i \mid x_i, E_{is}=1, NP_i=1 \,] = E[\, \Delta w_i \mid x_i, E_{is}=1, P_i = 0 \,].$

In this case, equation (2) implies that the relative wage growth of the incentivized program group is

(4) $\Delta_{s,f}(x_i) = \{\, E[\Delta w_i \mid x_i, E_{is}=1, P_i=1 \,] - E[\Delta w_i \mid x_i, E_{is}=1, P_i=0 \,] \,\} \,/\, \phi(x_i),$

which is simply the difference in the growth rates of the program and control groups, divided by the fraction of the program group who were incentivized by the program.

The assumption underlying equations (3) and (4) -- that that program has no effect on the wage growth of windfall beneficiaries -- may fail if the availability of the program causes people in the program group who would have worked anyway to find different jobs than they would have held in the absence of the program. In the case of SSP, for example, the offer of the wage supplement leads some people to work full time who would not have done so in the absence of the program (see below). If these jobs tend to offer slower or faster wage growth than the jobs that would have been otherwise selected, SSP will affect the rate of growth of wages for non-incentivized program group.[7] While such behavior can never be ruled out, the assumption that the wage growth of windfall beneficiaries is unaffected by the program is an obvious starting point. Moreover, if there is an identifiable subgroup G with $g(x_G) \approx 0$, it is testable. Specifically, for a subgroup with no program impact on the employment rate in period *s*, the employed program group consists entirely of non-incentivized workers. A comparison of wage growth between program and control group members with characteristics x_G therefore provides a test of the effect of the program on the wage growth of non-incentivized workers.

If a wage in the final period *f* were observed for all those who were working in the reference period *s*, equation (4) would be directly estimable. More generally, however, all that is observable is the mean change in wages for those who were employed in both *s* and *f*. Let $a(x_i)$ and $b(x_i)$ denote the expected rate of wage growth (or potential wage growth) for members of the

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[7]Of course those who switch from part time to full time in response to the program are "incentivized". However, we restrict our definition of incentivized to refer to the decision to work.

control group and the program group with characteristics x_i who were working in the starting period:

$$a(x_i) = E[\Delta w_i \mid x_i, E_{is}=1, P_i=0]$$

$$b(x_i) = E[\Delta w_i \mid x_i, E_{is}=1, P_i=1] .$$

The difference $b(x_i)-a(x_i)$ -- the excess wage growth of members of the program group with characteristics x_i -- is the key component of equation (4). Let S_0 and S_1 denote the selectivity biases for the control group and the program group in estimating the true wage changes from *s* to *f* using the observed wage changes for those who are employed in *f*:

(5a) $$S_0(x_i) = E[\ \Delta w_i \mid x_i, E_{is}=1, P_i=0\ ] - E[\ \Delta w_i \mid x_i, E_{is}=1, E_{if}=1, P_i=0\ ] ,$$

(5b) $$S_1(x_i) = E[\ \Delta w_i \mid x_i, E_{is}=1, P_i=1\ ] - E[\ \Delta w_i \mid x_i, E_{is}=1, E_{if}=1, P_i=1\ ] .$$

Using these expressions, expected wage growth conditional on working in the final period is:

(6) $$E[\Delta w_i \mid x_i, E_{is}=1, E_{if}=1, P_i] = a(x_i) + P_i(b(x_i)-a(x_i)) + S_0(x_i) + P_i\,(S_1(x_i)-S_0(x_i)) .$$

The difference in observed wage growth between program and control group members therefore includes both the true program effect and a relative selection bias term S_1-S_0. If people with faster potential wage growth are more likely to remain employed, then one would expect both $S_0 \geq 0$ and $S_1 \geq 0$. Whether this leads to a positive or negative bias in the estimate of relative wage growth, however, depends on the relative magnitudes of S_0 and S_1.

One approach to the issue of potential selection bias is to assume that these biases are negligible, conditional on a rich enough specification of the observable control variables. This is essentially the approach followed in the literature based on the propensity score method developed by Rosenbaum and Rubin (1983).[8] Under this assumption, consistent estimates of equation (6) can be obtained by regressing observed wage growth on a flexible function of the covariates, interacted with a program group indicator. The average difference in wage growth between the program and control groups can be obtained by evaluating the estimated interaction terms at the mean values of the covariates for the program group.[9] More generally, this approach will be valid if the selection biases in observed wage growth for members of the program and control groups with the same covariates are equal (i.e. if $S_1(x_i) \approx S_0(x_i)$).

An alternative approach is to posit a structural model of the joint determination of wage growth and employment status that leads to specific functional forms for the selection bias functions. To proceed along these lines, assume that an individual who is employed in *s* with characteristics x_i has potential wage growth:

(7) $\Delta w_i = (1-P_i)\, a(x_i) + P_i\, b(x_i) + \epsilon_i$

where ϵ_i is an unobserved component that is independent of x_i. Under this model, the selection bias terms are just the conditional expectations of the unobserved wage growth components, given that an individual is employed at the end date *f*:

$$S_0(x_i) = E[\epsilon_i | x_i, E_{if}=1, P_i=0]$$

$$S_1(x_i) = E[\epsilon_i | x_i, E_{if}=1, P_i=1] .$$

Next, assume that the probability that an individual with characteristics x_i is employed at the end date, conditional on working in *s*, is given by a latent index model:

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[8] This is sometimes referred to as the assumption of "selection on the observables". See Heckman, Ichimura, Smith, and Todd (1998) for further discussion.

[9] Evaluating the interaction terms at the mean characteristics of the program group provides an estimate of the average program effect on the program group itself.

(8a) $\quad \text{Prob}(E_{if}=1 | x_i, E_{is}=1, P_i=0) = \text{Prob}(r_0(x_i,\pi_0) - \eta_{0i} > 0),$

(8b) $\quad \text{Prob}(E_{if}=1 | x_i, E_{is}=1, P_i=1) = \text{Prob}(r_1(x_i,\pi_1) - \eta_{1i} > 0),$

where $r_p(x_i, \pi_p)$ for P=0 or 1 is a scalar index that depends on the parameters π_p (e.g. $r_p(x_i, \pi_p) = x_i\pi_p$), and η_{0i} and η_{1i} are continuous random variables that incorporate unobserved taste and labor market opportunity factors.[10] The combination of equations (7) and (8) implies that the selection biases take the form:

(9a) $$S_0(x_i) = E[\epsilon_i | \eta_{0i} < r_0(x_i,\pi_0)] = C_0(r_0(x_i,\pi_0); \theta_0)$$

and

(9b) $$S_1(x_i) = E[\epsilon_i | \eta_{1i} < r_1(x_i,\pi_1)] = C_1(r_1(x_i,\pi_1); \theta_1),$$

where C_0 and C_1 are control functions (Heckman and Robb, 1985) that depend only on the indexes $r_p(x_i,\pi_p)$ and on the unknown parameters θ_0 and θ_1 that describe the joint distribution of ϵ_i, η_{0i}, and η_{1i}.

In the benchmark case in which ϵ_i, η_{0i}, and η_{1i} are normally distributed (Heckman, 1979), the control functions are:

(10a) $$C_0(r_0(x_i,\pi_0); \theta_0) = \theta_0\, n(r_0(x_i,\pi_0))/N(r_0(x_i,\pi_0)) = \theta_0\, \lambda(r_0(x_i,\pi_0))$$

(10b) $$C_1(r_1(x_i,\pi_1); \theta_1) = \theta_1\, n(r_1(x_i,\pi_1))/N(r_1(x_i,\pi_1)) = \theta_1\, \lambda(r_1(x_i,\pi_1)),$$

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[10] A simple labor supply model might give rise to these equations. For example, suppose that available wage opportunities are given by $w_i = x_i\alpha + u_i$, and that in the absence of the program an individual works if $w_i \geq c_i$, where $c_i = x_i\beta + v_i$ represents a reservation wage that depends on child care costs, preferences, etc. In this case the probability of work for members of the control group is $P(x_i(\alpha-\beta) - (v_i-u_i) \geq 0)$. Suppose that the program provides a proportional wage subsidy of s, but requires individuals to work full time, leading to a reservation wage $k_i = x_i\beta' + e_i$. Then the probability of work for members of the program group is $P(x_i(\alpha(1+s)-\beta') - (e_i - (1+s)u_i) \geq 0)$.

where $\theta_0 = \rho_0\sigma_\epsilon$, $\theta_1 = \rho_1\sigma_\epsilon$, σ_ϵ is the standard deviation of the unobserved wage growth component[11], and ρ_p is the correlation between ϵ_i and η_{pi} for the control group (p=0) or program group (p=1). Assuming joint normality, the unconditional wage growth functions $a(x_i)$ and $b(x_i)$ can be estimated using the two-step procedure developed by Heckman (1979). In the first step a probit model is fit to data on individuals who were working in period *s* for the event that they are employed in *f*. This provides estimates of the index functions $r_0(x_i,\hat{\pi}_0)$ and $r_1(x_i,\hat{\pi}_1)$ which can then be substituted into a second stage model for wage growth between periods *s* and *f* (for those who are observed working in the later period):

$$(11) \quad \Delta w_i = a(x_i) + P_i(b(x_i)-a(x_i)) + (1-P_i)\,\theta_0\lambda(r_0(x_i,\hat{\pi}_0)) + P_i\,\theta_1\lambda(r_1(x_i,\hat{\pi}_1) + \xi_i,$$

where $\xi_i = \Delta w_i - E[\Delta w_i \mid x_i, E_{if}=1, P_i]$. This equation can be estimated by ordinary least squares, yielding estimates of the difference in wage growth between people in the program and control groups with characteristics x_i, $\hat{b}(x_i)-\hat{a}(x_i)$, which can be averaged across the distribution of the covariates in the program group.[12] An alternative to the two-step approach is to simultaneously fit the probit model for the probability of employment and the model for observed wage growth among those who are employed at the end date using maximum likelihood techniques.

The recent econometrics literature has proposed a series of semi-parametric generalizations of the two-step estimation technique that relax the assumption that the joint distribution of ϵ_i, η_{0i}, and η_{1i} is known (e.g. Powell, 1987, Robinson, 1988, Ahn and Powell, 1993, Newey, 1997). Under the assumptions that the control functions for the program group

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[11]In general ϵ_i can have a different variance in the program and control groups.

[12]Note that equation (11) can be estimated even if the control variables x_i enter in an unrestricted fashion in both the unconditional wage growth functions ($a(x_i)$ and $b(x_i)$, and in the employment probability indexes $r_0(x_i,\pi_0)$ and $r_1(x_i,\pi_1)$.

and the control group are the same, and that there is at least one covariate that affects the probability of employment but does not directly affect wage growth, these methods can be applied to equation (11) to yield selection-corrected estimates of the difference in wage growth between program and control group members at each x_i. In the context of an incentive program like SSP, however, it may be unreasonable to assume that the selection biases in the observed wage changes of the program and control groups are the same, since the program directly affects the relative economic value of employment versus non-employment. If separate control functions are introduced for the two program groups, then it is necessary to separately identify the difference in the intercepts of the control functions and the difference in the intercepts of the wage growth equation between the program and control groups in order to estimate the expected difference in wage growth between the program and control groups. In principle, the intercept of the control function is identified in a semi-parametric procedure by evaluating the wage growth equation for individuals whose probability of selection is "close to 1" (e.g. Heckman, 1990). Given the relatively small sample sizes available to study wage growth in the SSP sample, however, we do not try to implement semi-parametric methods with separate control functions for the program and control groups.

II. Measuring Wage Growth in the SSP Experiment

In this section we use the methods described in Section I to estimate the relative wage growth of program group members in the SSP experiment. We begin with a brief overview of the SSP experiment and some background information on the Canadian income assistance program for low-income families. We then turn to a detailed examination of the labor market outcomes of individuals in the control and program groups of the experiment who were working during the 12th to 14th month of the program. Finally, we turn to the problem of measuring the relative rate of wage growth for the incentivized program group over the period from the 12th to the 35th month of the SSP experiment.

A. Income Assistance Programs and the SSP Experiment

During the 1970s and 1980s, Canada, like many other countries, experienced large increases in spending on income support programs for low-income families.[13] Faced with rising welfare caseloads and changing attitudes toward the employment of mothers, Canadian policy makers have recently begun to search for innovations in the structure of income support programs that can reduce welfare dependency and increase labor market attachment. A key concern is that the Income Assistance (IA) program -- the primary welfare program for non-disabled non-elderly adults and their families -- provides limited financial incentives for recipients to work.[14] Indeed, IA benefits are reduced dollar-for-dollar with any earnings (or other income) above a modest "disregard" level.[15] The implicit 100 percent tax rate on earnings, coupled with the availability of other program benefits (such as dental services and prescription drugs) creates strong financial

[13]For example, combined federal and provincial spending on income assistance programs for low-income families under the Canada Assistance Plan rose close to 300 percent over the 1980s (Courchene, 1994).

[14]See Human Resources and Development Canada (1993) for a detailed inventory and description of income support programs in Canada.

[15]Income assistance programs are operated at the provincial level, but share several important features across most provinces, including a dollar-for-dollar benefit reduction rate.

disincentives for individuals who have entered the IA system to work more than a few hours per week, or to leave IA.

In this context, the Self Sufficiency Project (SSP) was conceived as a rigorous test of enhanced financial incentives on the welfare participation and labor market behavior of long-term single-parent IA recipients.[16] 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 target level set well above the level of IA benefits available to most families. The SSP supplement increases the financial reward for leaving IA and entering work. Moreover, since supplement payments are reduced by only 50 cents per dollar of additional earnings, SSP provides a stronger marginal incentive for work than conventional IA.

The SSP evaluation is based on a randomized design: one half of a group of long-term IA recipients from two sites (in British Columbia and New Brunswick) were offered the SSP supplement, the other half were assigned to a control group. The demonstration follows both groups for a period of 5 years after random assignment, and utilizes data from both administrative records and specialized surveys to infer the effects of the program.[17]

Table 1 presents a brief summary of the SSP Recipient Experiment, including information on the sample eligibility requirements and key features of the supplement program. Relative to

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[16]SSP was conceived and funded by Human Resources and Development Canada. See Lin et al (1998) for a comprehensive description of the program and results from the first 18 months of the experiment. Blank, Card, and Robins (1999) provide a survey of other recent financial incentive programs for welfare participants in the U.S.

[17]The overall SSP demonstration includes three experiments: the main one outlined in the text; a second experiment designed to measure the effect of the availability of SSP on the probability that new entrants to the IA system stay on welfare long enough to become eligible for SSP (see Berlin et al, 1998); and a third small scale experiment designed to test a combination of SSP financial incentives and employment-related services (Quets et al, 1999).

other financial incentive reforms (such as those tested by different U.S. states in the early 1990s), SSP is quite generous. For example, a single mother in New Brunswick with one child received a maximum monthly IA grant of $712 in 1994. Her gross income if she were to leave IA and enter full time work at the minimum wage would be $867 per month -- a gain of only $155 per month for working 40 hours per week.[18] Under the SSP program, however, she would receive an additional supplement payment of $817 per month, raising the income advantage of work versus IA to $972 per month. The relative reward to work versus welfare under SSP is smaller when taxes and transfers are taken into account, but is still relatively large (see Lin et al, 1998, Table G.1).

An important characteristic 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, however, lose all future eligibility. The latter feature has some implications for the composition of the incentivized program group under the SSP program. Most importantly, anyone whose labor market outcomes in later years of the experiment are affected by the program was presumably working within the first year after random assignment.[19] Program group members who only began working in the

[18]This calculation ignores payroll and income taxes, and the potential loss of other benefits (prescription drugs and dental care) which further lower the return to work.

[19]One could argue that the incentivized program group only includes those who were working full time within a year of random assignment. This definition may be too strict, since some people could have started work in anticipation of receiving a supplement, but failed to meet the full time work requirement to initiate a supplement payment.

second or third year after random assignment were never eligible for SSP, and should not have been much affected by the program.

B. Program Impacts and Characteristics of Workers in Months 12-14

Consistent with the generosity of its financial incentives, SSP has significant behavioral effects on employment and welfare outcomes. A brief summary of these impacts is provided by Figure 1, which shows average monthly employment, earnings, and welfare participation by members of the control and program groups for the first 36 months of the experiment.[20] A interesting 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 accelerated rates of leaving IA and entering employment. As expected given the time-limited nature of SSP eligibility, however, the divergence in outcomes of the program and control groups peaks at 12-14 months after random assignment. After this point, the employment rate of the program group is roughly constant, while the rate of the control group continues to rise. A similar pattern appears in earnings and IA participation, although for these outcomes the program group continues to experience modest changes after months 12-14.

In view of SSP's eligibility rules, and the resulting pattern of program effects, we decided to measure the effects of SSP on relative wage growth of the incentivized program group among those who were working in months 12-14 after random assignment. In the notation of section I,

[20]Outcomes for the month just prior to random assignment are plotted as month -1 in the figures. See Lin et al (1998) for a information on the program impacts.

the initial period *s* for our observation interval is the 12th to 14th month of the experiment. We also define the end period of the observation interval as months 33-35, which are the latest months for which labor market data are currently available.

Table 2 shows a variety of characteristics of individuals in the program and control groups who were working in months 12-14. In order to focus more directly on people who had a reasonably strong labor market attachment by one year after random assignment, we only include people who reported positive hours of work in at least two of the three months between the 12th and 14th month. For comparative purposes, column 1 of the table reports the characteristics for all program and control group members in the SSP Recipient experiment.[21] The experimental population is over 95 percent single mothers, and tends to have relatively low education. About one-half of the sample have at least one child under the age of 5. Virtually all of the sample report some work experience: indeed, the average years of work experience is 7.4 years. Nevertheless, 73 percent had not worked in the year prior to the baseline interview and only 20 percent were working at the baseline.

Columns 2 and 3 report the characteristics of the subsets of the control and program groups who were working in months 12-14. 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

[21]Due to data requirements, the data in Table 1 and the rest of the paper 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.

provides a valid 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.

Relative to the overall experimental population, the subsets of the control and program groups who worked in months 12-14 are better-educated, have more work experience, are less likely to have preschool-age children, have a more positive attitude toward work, and were more likely to be working at the baseline interview. Consistent with the observation that SSP causes some people who would not have worked in months 12-14 to enter the labor market, the differences in characteristics between workers in the program group and the overall experimental population are smaller than the corresponding differences between workers in the control group and the overall population. Formally, the average characteristics of workers in the program group are a weighted average of characteristics for the incentivized and non-incentivized program groups. Assuming that the mean characteristics of the non-incentivized group are the same as those of control group members who were working in months 12-14, the mean characteristics of the incentivized program group can be estimated by dividing the difference in means between the working program group and working control group by the estimated fraction of the working program group who are incentivized (32 percent).[22]

Use of this formula yields the estimated mean characteristics for the incentivized program group shown in column 4 of Table 2, and estimates of the mean differences in characteristics

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[22]The assumption that the non-incentivized program group have the same characteristics as the worker in the control group will hold under random assignment, since all of the characteristics under consideration in Table 2 are pre-program characteristics collected in the baseline interview.

between incentivized and non-incentivized workers shown in column 5. As one might expect, the incentivized program group is a little younger, a little less likely to hold a high school diploma, has less positive attitudes toward work, and is more likely to have young children than the group who would have been working in months 12-14 regardless of SSP. More remarkable is the fact that the mean baseline employment rate for the incentivized program group is close to 0 (only 1.7 percent), whereas among the non-incentivized group it is over 50 percent.

While Table 2 shows only the differences in the pre-program characteristics of different subgroups, similar methods can be used to compare post-random assignment outcomes. Of particular interest are the characteristics of the jobs obtained by the incentivized program group, relative to the jobs of the non-incentivized group. Table 3 presents data on the wages and hours outcomes of the program and control groups in months 12-14, along with estimates of the distributions of these outcomes among the incentivized program group.[23] An important caveat to this table is that the interpretation of the derived estimates for the incentivized program group is only valid under the assumption that SSP has no effect on the non-incentivized workers in the program group.[24]

A comparison of wage outcomes between workers in the program and control group in Table 3 suggest 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

[23]We define the wage in months 12-14 as the simple weighted average of the available wages for each of the three months, and hours per week similarly.

[24]As in the discussion of equation (4), the assumption that the post-random assignment outcomes of the non-incentivized program group are the same as those of the control group may be problematic if the availability of SSP causes the non-incentivized program group to change their behavior.

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.[25] 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.

A similar tabulation of the weekly hours of the program and control groups suggests that the hours distribution of the incentivized program group is largely concentrated between 30 and 40 hours per week. There is also strong evidence that the availability of SSP affects the hours choices of the non-incentivized program group. Specifically, the -20.8 percent entry in column 3 for the "under 29 hours per week" category arises because there is a smaller total number of people in the program group working part-time than in the control group, even though the size of the working population in the program group is bigger. This is presumably attributable to the fact that SSP induces some people who would have been working part-time in the absence of the program to shift to full-time work. Given this behavior, the hours distribution in the third column of Table 3 is actually a mixture of the distribution for the incentivized group (as we have defined

[25]As 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.

it) and the distribution for people who would have worked part-time in the absence of SSP. Clearly, people in both groups tended to work between 30 and 40 hours per week.

The key conclusion we draw from Table 3 are that former welfare recipients who are incentivized to work by SSP obtain jobs that pay within a narrow range above the minimum wage, and work between 30 and 40 hours per week. The relatively low wage outcomes are particularly noteworthy because without the SSP supplement a minimum wage job is not an particularly attractive alternative to IA.[26] Thus, in the absence of significant wage growth, one might expect a sizeable fraction of the incentivized program group to eventually return to IA.

A secondary conclusion is that SSP tends to raise the hours of people in the non-incentivized program group who would have worked part-time (under 30 hours per week) in the absence of the program. To the extent that jobs with longer hours lead to systematically faster or slower wage growth, SSP may therefore have some affect the average wage growth of the non-incentivized program group – an issue to which we now turn.

C. Does SSP Effect the Wage Growth of Non-incentivized Workers?

As discussed in section I, the rate of relative wage growth of the incentivized program group can be inferred from the observed difference in the relative wage growth of the program and control groups, provided that SSP has no effect on the wage growth of those who would have been working without the program. Table 4 presents some direct evidence on this question, based on the outcomes of individuals who were working at the baseline of the SSP experiment.

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[26]In New Brunswick in 1994 the monthly IA grant of $712 for a single mother with one child was equivalent to 32.9 hours of work per week at the minimum wage. In British Columbia the monthly grant of $982 was equivalent to 37.8 hours per week at the minimum wage.

As motivation for this analysis, observe in row 1 of Table 4 that the employment rate in months 12-14 for the subset of the program group who were working at the baseline is only slightly higher than the employment rate of the comparable control group. This small differential implies that only a minor fraction (6 percent) of the program group who were working at the baseline would not have been working in months 12-14 in the absence of SSP.[27] Thus, a comparison of the growth rates of wages between the program and control groups over the period from months 12-14 to months 33-35 provides an approximate test of the effect of SSP on the wage growth of its windfall beneficiaries.

Comparing labor market outcomes over the next 21 months, the program and control groups have similar cumulative months of work, although the program group has slightly higher average hours per week (as might be expected from Table 3). The employment rates of the two groups in months 33-35 are similar, and in fact have converged slightly from months 12-14. The mean log wage of the program group in months 12-14 is slightly lower than that of the controls, although the difference is not statistically significant. In months 33-35, the differential is only slightly wider and is still statistically insignificant. The average growth rate of log hourly wages over the period from months 12-14 to months 33-35 (for those who are employed in the end period) is about 7 percentage points for the control group and 5 percentage points for the program group. The difference is small (-2 percentage points) and far from statistically significant, suggesting that overall wage growth for the program group is not much affected by SSP.

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[27]Another way to see the same point is to recall from Table 2 that the fraction of the incentivized program group who were working at the baseline is close to zero. Thus, most of the program group who were working in months 12-14 and were also working at the baseline are windfall beneficiaries of SSP.

Close examination of the wage growth data shows the presence of a small number of potential outliers in both the program and control groups. One way to reduce the influence of these observations is to consider medians, rather than means. Another way is to "trim" or censor the large changes. Row 10 shows that median growth rates for the program and control groups are about equal. Moreover, controlling for observed characteristics in a median regression does not affect the comparison. Row 11 shows that similar conclusions emerge from trimmed wage growth data, constructed using a lower censoring point of -0.35 and an upper censoring point of 0.5. (These correspond to roughly the 5th and 95th percentiles of the distribution of changes in the control group). As with the medians, the trimmed mean wage changes of the program and control group are relatively precisely estimated, and are very similar: with 1 percentage point of each other.

Based on the analysis in Table 4 we believe it is reasonable to conclude that the availability of SSP has little systematic effect on the wage growth of individuals who were working at the experimental baseline. While this finding does not rule out the possibility that SSP affects the wage growth of other non-incentivized beneficiaries of the program, it is reassuring. Moreover, the subgroup of people who were working at the baseline is a significant fraction of <u>all</u> windfall beneficiaries of the program: of the 691 members of the overall control group who were working in months 12-14, 493 were working at the baseline. Thus, baseline workers comprise about 70 percent of the non-incentivized program group. At a minimum, then, we can infer that SSP has little or no effect on wage growth for a majority of the non-incentivized group.

D. Simple Comparisons of Wage Growth in the Program and Control Groups

Table 5 summarizes some key labor market outcomes for individuals in the SSP program and control groups who were working in months 12-14. As in Table 4, we present means for the control group, means for the program group, and both raw and regression-adjusted differences in means between the two.[28]

As shown in row 1, just over two-thirds of those who were working in months 12-14 were also working in months 33-35.[29] The unadjusted difference in re-employment rates between the program and control groups is small and statistically insignificant. Taking account of the different characteristics of the control and program groups who were working in months 12-14, however, the program group has a slightly higher employment rate. The two groups also have similar amounts of cumulative labor market experience between months 12 and 33. In each case, those who were employed in months 12-14 worked in about 80 percent of the following months.

Row 3 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 3, which show that the extra employment attributable to the incentive effect of the SSP program is concentrated at relatively low wages. Indeed, if one assumes that 68 percent of program group workers would

[28]A total of 24 control variables are used in the regression model, including 2 dummy variables for education, labor market experience and its square, indicators for province, gender, number and age of children, labor market status at the baseline, two dummy variables measuring attitudes toward work, an indicator for whether months 33-35 occur in the winter, and interactions of most of the controls with province.

[29]Again, we define employment in months 33-35 as having reported positive hours in 2 or more of the 3 months. Results based on other definitions are similar.

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 1.78, or 20 percent below the mean wages of those who would have worked even without the SSP subsidy.

Row 4 of Table 5 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. By comparison, as shown in row 9, mean log wages of those who were subsequently not working are about 10 percentage points lower than the overall average. Row 10 shows the gap in mean wages between those who were re-employed in months 33-35 and those who were not. For both program groups this differential is about 13 percent. We conclude from these patterns that conditioning on employment status in months 33-35 introduces about the same selection bias in observed wages in months 12-14 for the program group as for the controls. This is potentially reassuring, since under certain assumptions the selection bias in the level of wages in months 12-14 for the subset of workers who were subsequently employed in months 33-35 is proportional to the selection bias in their measured wage growth.[30] Under these assumptions the similarity of the program group differences in rows

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[30]Proportionality holds if wages in months 12-14, wages in months 33-35, and the unobserved variables determining the probability of employment in months 33-35 are all normally distributed with the same covariance structure in the program and control groups. To see this, let w_1 and w_2 denote wages in months 12-14 and 33-35, respectively, and let z denote a normally distributed index such that w_2 is observed if $z>0$. Finally, let σ_t denote the standard deviations of w_t $(t=1,2)$, let ρ_{tz} denote the correlations of w_t and z, and let $\lambda=E(z|z>0)$. Then the selectivity bias in w_1, given that w_2 is observed, is $\sigma_1\rho_{1z}\lambda$, and the selectivity bias in w_2-w_1 given w_2 is observed is $(\sigma_2\rho_{2z}-\sigma_1\rho_{1z})\lambda$. Clearly, if the covariance parameters are the same in the program and control groups and the mean selectivity bias in w_1 given that w_2 is observed is the same for the two groups, then the mean of λ is the same. In this case the mean of the selectivity bias in w_2-w_1 given that w_2 is observed, is the same for the two groups.

3 and 4 of Table 5 implies that the mean selection bias in measured wage growth for the two program groups is equal.

Row 5 of Table 5 shows mean log wages in months 33-35 for the subsets of the program and control group who were working then, while row 6 shows the mean changes in log wages between months 12-14 and 33-35 for the same groups. The data indicate a somewhat slower average growth rate of wages in the program group than the control group, although the gap is far from statistically significant. As shown in rows 7 and 8, however, this gap closes when medians, rather than means are analyzed, or when the wage growth observations are trimmed to eliminate the influence of extreme outliers. Indeed, the trimmed wage growth data show virtually identical wage growth rates in the program group and control group. Taken at face value this similarity implies that the incentivized program group had virtually the same wage growth as those who would have been working in months 12-14 even in the absence of SSP.

We have also performed the comparisons in Table 5 using an alternative definition of the group of interest that recognizes the full time work requirement of SSP. Specifically, we compared wage growth of members of the program and control groups who worked full time in at least 2 months during the 12th to 14th months of the experiment. The results are very similar to those in Table 5, with small and statistically insignificant differences in re-employment rates between program and control group members who worked full time in months 12-14, lower wages among the program group than the program group in months 12-14 and months 32-24, and statistically indistinguishable wage growth rates for the two groups.

E. More Detailed Comparisons of Wage Growth

There are two limitations of the simple estimates of relative wage growth shown in Table 5. The first is that these estimates are based on specifications that assume a constant program effect. In the notation of section I, the assumption is that $b(x_i)$, the expected wage growth for the program group, conditional on the observed covariates x_i, differs from $a(x_i)$, the expected wage growth for the control group, by only an intercept shift. The second is that the estimates are only valid if $S_1(x_i)$, the selection bias in measured wage growth for the program group, equals $S_0(x_i)$, the selection bias in measured wage growth for the controls.

With respect to the first issue, a simple procedure is to estimate a fully-interacted regression specification that includes all the covariates and their interactions with the program group dummy, and then evaluate the interactions at the mean characteristics of the program group. Using the trimmed wage growth measure, this procedure leads to an estimated difference in expected wage growth between the program and control groups of 0.003 (with a standard error of 0.013), which is very similar to the adjusted estimate shown in row 8 of Table 5. Moreover, none of the 24 interactions terms between the program dummy and the covariates is individually significant, and an F-test that the interaction terms are jointly insignificant has a probability value of 0.67. We infer that the differences in wage growth between the program and control group are relatively similar across subgroups.

To address the issue of selectivity bias, we began by estimating a probit model for the probability of working in months 33-35, conditional on working in months 12-14 (and reporting a valid wage). We fit the model separately to the program and control groups, using the full set of 24 covariates used to estimate the adjusted wage growth differentials in Table 5. We then used the model to predict the probability of employment in months 33-35 for members of the program and control groups. The distributions of predicted probabilities for the subsets of the two groups who actually worked in months 33-35 (and reported a valid wage) are graphed in Figure 2. The distributions have two key features. First, the predicted probabilities for the program group have virtually the same mean as the predicted probabilities for the control group, but less dispersion.[31] Second, (and related to the first) the support of the distribution of predicted probabilities for the program group is contained in the support of the distribution for the control group.[32]

These two features have potentially important implications for assessing the relative selection biases in the observed wage changes of the program group versus the control group. Specifically, suppose that the structural model given by equations (7) and (8) in Section I is restricted so that the control functions for the program and control group are the same:

$$C_p(r_p(x_i,\pi_p);\theta_p) \;=\; C(\, r_p(x_i,\pi_0);\theta) \,, \quad \text{for } p=0 \text{ or } 1 \,.$$

In addition suppose that the distribution functions of the latent random variables (η_{0i} and η_{1i}) in the employment probability models for the two program groups are the same.[33] Then an individual in the program group with a given probability of employment has the same selectivity bias in her observed wage change as an individual in the control group with the same probability

[31]The mean and standard deviation of the predicted probability of being re-employed in months 33-35 for those in the control group who were re-employed are 0.688 and 0.144, respectively. The corresponding mean and standard deviation for the program group are 0.691 and 0.101, respectively.

[32]The importance of checking the comparability of the support of the distributions of the probabilities of selection between the program and control (or comparison) groups is emphasized by Heckman, Ichimura, Smith and Todd (1998).

[33]These properties will be true in a conventional multivariate normal model if the correlation between the unobserved component of wage growth and the unobserved component of the the probability of employment are the same in the program and control groups.

of employment.[34] In this case, comparisons of wage growth between program and control group members with similar probabilities of employment in months 33-35 will abstract from any selectivity biases.

Table 6 presents a set of comparisons of wage growth between program and control group members with similar values of the predicted probability of employment. For simplicity, in this table and the remainder of the paper we use the trimmed wage growth measure described above. Row 1 displays the overall program and control groups. As noted in Table 5, the two groups have very similar growth rates in wages, with a raw difference of only 0.002. The adjusted(1) difference in row 1 represents the difference in growth rates, controlling for a basic set of baseline covariates and a set of dummy variables indicating the decile of the predicted probability of employment (from the probit models described above).[35] This estimate is only slightly smaller than the unadjusted difference, suggesting that differences in wage growth between program and control group members with similar predicted probabilities of employment are small. The adjusted(2) difference is obtained from a similar model that includes a richer set of 24 control variables (the same set of variables used to form the predicted probabilities of employment within each program group). This estimate is slightly more negative than either the other differences, but is not significantly different from 0.

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[34]This follows from the fact that the index $r_p(x_i,\pi_p)$ is an invertible function of the probability of employment: $\text{Prob}(E_{if}=1|x_i,\ldots) = \text{Prob}(r_p(x_i,\pi_p)-\eta_{pi}>0) = F(r_p(x_i,\pi_p))$, where F is the distribution function of η_{pi}.

[35]The deciles are assigned from the pooled sample of program and control group members who reported valid wage growth data (1100 observations).

The remaining rows of Table 6 report similar comparisons between program and control group members whose predicted probabilities of employment fall in specific decile ranges. Across the decile groups there is some variation in the average growth rates of wages for the two program groups, but little indication of a systematic pattern. As shown in the bottom row of the table, weighted averages of the decile-specific program-group differences based the distribution of the program group are similar to those reported in row 1, but slightly more negative. If one maintains the hypothesis of identical control functions for the program and control groups, the entries in the bottom row of Table 6 suggest that the simple difference in the growth rate of wages between the program and control groups may be slightly upward biased. In light of the modest sample sizes, however, it is very difficult to draw precise inferences, and one could plausibly conclude that the selection biases for the program and control group are equal, leading to little or no bias in the simple difference of observed growth rates.

An alternative way to evaluate the potential effect of selection bias is to estimate a model for wage growth that includes the estimated value of the control function as an additional covariate. Under the assumption that all unobserved variables are normally distributed, with the same joint distributions in the program and control groups, the appropriate control function for an individual with predicted employment probability $\hat{p}$ is

(12) $\rho\sigma_\epsilon \, n(\, N^{-1}(\, \hat{p})\,)\, /\, \hat{p},$

where σ_ϵ is the standard deviation of the unobserved component of wage growth (ϵ_i), ρ is the correlation of ϵ_i with the unobserved component of the probability of employment (η_i), $n(z)$ is the normal density at z, and $N^{-1}(p)$ is the inverse normal cumulative distribution function at p.

Table 7 reports estimation results for three alternative specifications based on this approach. The first specification assumes that the true wage growth equation is identical for the program and control groups, apart from an intercept shift. The explanatory variables in the wage growth equation for this specification includes a set of control variables, a dummy variable for the program group, and the control function given by (12). The second model allows a completely unrestricted specification of the wage growth equations for the program and control groups. Thus, the wage equation includes all the control variables and a full a set of interactions of the controls with the program group dummy. The entry in row 1 of the table for this specification is the mean difference in expected wage growth between the program and control groups, evaluated at the characteristics of the program group. Since the probit model used to estimate $\hat{p}$ is based on the same set of covariates, fully interacted with program group status, the coefficient of the control function in this specification is identified by functional form alone. Finally, the model in column 3 is similar to that in column 2, except that 4 covariates (based on the interaction of season, location, and control group status) are assumed to effect the probability of re-employment, but to have no direct effect on wage growth.[36] This specification is valid if people for whom the last months of the sample fall in winter have the same potential wage growth as other people, but have a harder time finding a job in those months. As in column 2, the program group dummy is fully interacted with the included control variables, and the entry in row 1 is the

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[36]Specifically, the excluded variables are an indicator for the event that months 33-35 occur in the winter, and its interactions with province and program group. People for whom months 33-35 are in the winter have a significantly lower probability of employment in these months than others in the sample. This seasonality effect is stronger in New Brunswick.

mean difference in expected wage growth between the program and control groups, evaluated at the characteristics of the program group.

The specifications in columns 1 and 3 of Table 7 both yield small and statistically insignificant estimates of the coefficient $\rho\sigma_\epsilon$. They also give estimates of the selection-corrected difference in wage growth between the program group and the control group that are very close to 0. These findings are consistent with the simple specification reported in row 7 of Table 5, which is valid under the assumption that $\rho\sigma_\epsilon=0$. By comparison, the specification in column 2 of Table 7 leads to a point estimate of $\rho\sigma_\epsilon$ that is large and negative, and an estimate of the difference in mean wage growth between the program and control groups that is relatively large in magnitude, although not significantly different from zero. A potential problem with this specification is that the estimate of $\rho\sigma_\epsilon$ is so large that the implied estimate of ρ is bigger than 1 in absolute value.[37] In view of this fact, and the very weak basis for the identification of the coefficients in this specification, we believe that the estimates in column 2 should be given less credence than those in columns 1 and 3.

A potential criticism of the estimates in Tables 6 and 7 is the implicit assumption that the unobserved determinants of wage growth and the probability of employment have the same joint distribution in the program and control groups. As noted in Section I, this assumption can be relaxed by introducing separate control functions for the two groups. In the context of the

[37]The estimated standard deviation of the wage growth residual, σ_ϵ, is 0.200. Thus the implied estimate of ρ is -2.19.

models used in Table 7, this amounts to introducing program-group-specific coefficients on the control function.[38]

Table 8 reports estimation results for three specifications that allow for program-group specific selectivity corrections. The first model, in column 1, assumes that the effects of the covariates on expected wage growth for the program and control groups are identical, apart from an intercept shift. The second and third specifications allow for program-group-specific effects of all the included covariates, but exclude the seasonal variables from the wage equation. As noted above, this exclusion restriction is valid if potential wage growth has no seasonal component. The results in columns 1 and 2 of Table 8, like those in Table 7, are obtained using a simple two-step approach in which the estimated control function is included as an additional explanatory variable in the wage growth equation. In contrast the results in column 3 are obtained from a maximum likelihood procedure applied separately to the two program groups. This procedure simultaneously estimates a probit model for the probability of employment in months 33-35, and the wage growth equation, conditional on employment.[39] In the maximum likelihood model ρ and σ_ϵ are treated as free parameters: we report their product in rows 2a and 2b of the table for comparability with the two-step estimates.

The results from these three specifications are fairly similar. In all three cases, the estimates suggest that wage growth is slightly slower for members of the control group who are more likely to be re-employed in months 33-35, and slightly faster for members of the program

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[38]Recall that the first-stage probit model used to estimate the probabilities used to construct the index function is estimated separately for the two program groups.

[39]The likelihood function for this model is well-known. See e.g. Amemiya, 1985, pp. 385-386).

group who are more likely to be re-employed. The selection correction coefficients are imprecisely estimated, however, and are not significantly different from each other. The estimated differences in expected wage growth between the program and control groups are all negative, and relatively large in magnitude, but very imprecise. The source of the imprecision is the difficulty of separately identifying both program-group-specific control functions, and differences in mean wage growth for the two groups. For example, comparing the specifications in column 1 of Table 7 and column 1 of Table 8, the introduction of program group-specific coefficients for the control function causes the standard error of the difference in mean wage growth to rise from 0.014 to 0.043, although it does not significantly improve the fit of the model. Similarly, comparing the specification in column 3 of Table 7 with the specification in column 2 of Table 8, the introduction of program group-specific coefficients for the control function leads to a rise in the standard error of the wage growth differential (from 0.118 to 0.142) and a big change in the estimated wage growth differential (from -0.003 to -0.073), but no significant improvement in fit. We conclude that the results from the more general specifications are consistent with the restrictive model which assumes similar control functions for the program and control groups, but are not particularly informative

E. Interpretation

The estimation results reported in Tables 5-8 point to three conclusions. First, under the assumption that selection biases in the measured wage growth of individuals in the SSP program and control groups are the same (i.e. $S_0(x_i) = S_1(x_i)$), relatively precise estimates of the differential in average wage growth between the program groups can be obtained from a simple specification that controls for the covariates (possibly with interactions for program group status). Such estimates imply very small differences in average wage growth between the program group and the control group. Second, under the more general assumption that the selectivity bias for an individual with observed wage growth varies with her probability of selection (i.e. her probability of employment in months 33-35), somewhat less precise estimates can be obtained by comparing wage growth between individuals in the program and control groups with similar predicted employment probabilities. Two different implementations of this idea (including the traditional two-step estimation method proposed by Heckman (1979)) point to small differences in average wage growth between the program and control groups, and little or no selection bias in the measured wage changes of those who were employed in months 33-35 relative to the overall population (Tables 6 and 7). Finally, under the most general alternative considered in this paper, selection biases may differ between members of the program and control group with the same probability of selection. Estimates from a parametric version of this case (Table 8) are quite imprecise. Although they do not reject the simpler specifications, they underscore the limitations of the available data in choosing between alternative models of the selection process.

On balance, we believe that the evidence is consistent with a view that members of the SSP program group who were employed in months 12-14 experience the same wage growth as members of the control group. This in turn implies that incentivized program group members – those who would not have been working in months 12-14 in the absence of SSP – had about the same wage growth as members of the control group who were working then. For both groups, the estimates in Table 6 show an 8.0 percent growth rate over the period from the 12^{th} to 33^{rd} months of the SSP experiment. During the period from 1992 to 1996, the inflation rate averaged about 2 percent per year (or a rise of about 3.5 percent over 21 months. Thus, both groups had real wage growth of about 4.5 percentage points over the period from months 12-14 to months 33-35 -- an annual real wage growth rate of 2.6 percent per year.

One potentially important influence on the rate of growth of wages of low-wage workers is the level of the minimum wage (see Card and Krueger, 1995, and DiNardo, Fortin, and Lemieux, 1996). During the period covered by the first 36 months of the SSP Recipient experiment, the minimum wage rose from $5.00 to 5.50 per hour in New Brunswick, and from $6.00 to 7.00 per hour in British Columbia.[40] Given the concentration of the incentivized program group's wages in a narrow range above the minimum wage, it is possible that these increases differentially affected the observed wage growth of the SSP program group. To investigate this issue, we calculated the changes in the province-specific minimum wage over the period from the 12th to 33rd month for each person in the SSP experiment, and constructed the comparisons in Table 9. We classify individuals into 5 categories: those who had no change in the minimum wage over the calendar period corresponding to their 12th to 33rd months; and those with minimum wage changes of roughly 5, 8, 10, and 15 percent. The fourth column of Table 9 shows the average wage changes for program and control group members in each category, while the fifth column shows the differences in wage growth between program and control group members in each category. There is some evidence that average wages grew faster for people who experienced larger increases in the minimum wage, although the differences across categories

[40]The minimum wage in New Brunswick was $5.00 from late 1992 to January 1996, rose to 5.25 on January 1 1996, and to 5.50 on July 1 1996. The minimum wage was in British Columbia was $6.00 from late 1992 to March 1995, rose to 6.50 in March 1995, and to $7.00 in October 1995.

are not statistically significant.[41] More importantly, there is no systematic correlation between the size of minimum wage increases and the mean difference in wage growth between the program and control groups.

A further investigation of what role, if any, minimum wage increases play in the relative wage growth of the SSP program group versus the control group is presented in Table 10. The regression models in this table include a program group dummy, the percentage change in the minimum wage, and an interaction of the minimum wage change with the program group dummy as explanatory variables for observed wage growth between the 12-14th and 33-35th months of the SSP experiment. The specifications in columns 4-6 also include a set of 24 baseline covariates. The estimates in columns 2,3, 5, and 6 show a modest effect of minimum wage increases on individual wage growth, although the measured effect is never statistically significant (in column 2 the t-ratio for the minimum wage coefficient is 1.52, which has a probability value of 0.12). The magnitude of the effect implies that a 10 percent increase in the minimum wage leads to a 1.2 to 2.0 percentage point higher wage in months 33-35, relative to the case of no increase in the minimum. As shown by the interaction coefficients in the third row of the table, however, the effect is very similar for members of the program and control groups. This may be a little surprising, since the program group includes more people whose wages are closer to the minimum wage, and one might have thought this would lead to a bigger effect of the minimum wage on the program group. Nevertheless, there is no evidence of a differential effect, perhaps indicating that minimum wages changes create ripple effects that raise wages of all low wage workers, and not

[41]An F-test for the joint significance of 4 dummies indicating the range of the minimum wage increase has a probability value of 0.40. This test statistic is less significant if additional controls are added.

just those closest to the old minimum. Given the estimates in Table 10, we conclude that minimum wage increases may have accounted for up to 1.7 percentage points of average wage growth for both the program and control groups between months 12-14 and 33-35 (or 1 percent per year), but had very little effect on the relative wage growth of the program groups.[42]

After accounting for inflation and the possible effects of the minimum wage, the average wage growth of the SSP program and control groups was therefore between 1.6 percent and 2.6 percent per year. Using the framework introduced in Section I, we can infer that the incentivized program group experienced the same growth rate as control group workers -- presumably in the range of 1 to 3 percent per year. Interestingly, this range of growth rates is very similar to the rate implied by the cross sectional association between wages and previous work experience in the SSP population. Appendix Table 1 presents a series of conventional human capital earnings models, fit to the hourly wages of SSP control and program group workers who were working at the baseline, and to the hourly wage outcomes of control group workers at 12-14, 24-27, and 33-35 months after the baseline. Following conventional practice, all of these models include controls for education and gender, and a quadratic function of measured years of labor market experience as of the baseline.[43] The estimates imply that for a group of workers with an average of 6.6 years of work experience (the mean for the SSP incentivized program group, as shown in Table 2), each additional year of work experience is associated with 1.5-2.6 percent higher real hourly wages. The rates of actual wage growth of the incentivized program group are therefore

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[42]The upper bound of 1.7 percentage points comes from multiplying the average percentage increase in the minimum wage (8.3 percent) by 0.2, the coefficient from column 3 of Table 10.

[43]The SSP baseline questionnaire asked each person how many years she had worked for pay over her life. We use this as our measure of experience.

quite comparable to the rates one would expect for a similar group of workers who were working without the SSP incentive. In addition, the rates are fairly similar to those estimated by Gladden and Taber (2000) in a recent study of wage growth among less-skilled workers in the U.S.

Conclusions

This paper attempts to measure the rate of wage growth among former welfare recipients in the Self Sufficiency Project who were induced by the financial incentives of SSP to enter the work force in the 12^{th} through 14^{th} months of the experiment. We develop a set of assumptions under which the relative wage growth of the incentivized program group can be inferred from the observed difference in wage growth between the program and control groups of the experiment. The key requirement is that the program has no effect on the wage growth of those who would have been working even in its absence (even if it leads to some changes in their hours of work). Although this assumption cannot be fully tested, it can be evaluated for subgroups of the experiment, and seems to be valid for the relatively large subgroup of the SSP population who were working at the baseline of the experiment.

Even under this assumption, a potential selectivity problem arises in evaluating the effects of SSP on the wage growth of program participants, because wage growth is only observable for those who are re-employed at some later date. We use several different techniques, including simple comparisons between subgroups with similar probabilities of selection, and conventional two-step selection correction methods, to evaluate the potential magnitude of any selection biases. Selection models based on the assumption that members of the SSP program and control groups with similar probabilities of selection have similar selection biases point to small differences in average wage growth between the program and control groups, and little or no selection bias in the measured wage changes of those who were employed in months 33-35 relative to the overall population. More generally, however, selection biases may differ between members of the program and control group with the same probability of selection. Estimates under this assumption are quite imprecise, although they do not reject the simpler specifications.

Based on the range of the evidence we conclude that SSP leads to wage growth among the incentivized program group that is very similar to the growth experienced by members of the control group, who would have left welfare and entered work without the program's incentive. After accounting for inflation, and possible effects of raises in the minimum wage, we find that both groups had growth rates of wages in the range of 1.6 to 2.6 percent per year. This range, while modest, is quite comparable with predictions based on the cross-sectional association of wages and labor market experience in the SSP population.

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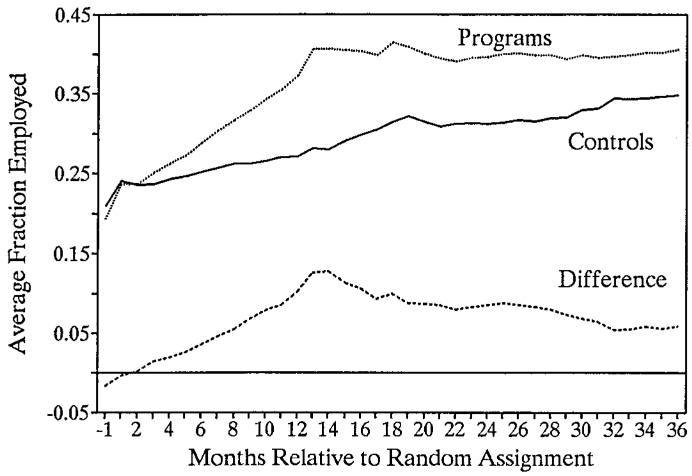
Appendix Table 1: Estimated Models for Log Hourly Wages

	Controls And Programs At Baseline (1)	Controls Only: At Baseline (2)	Months 12-14 (3)	Months 24-27 (4)	Months 33-35 (5)
Female	-0.041	-0.051	-0.243	-0.285	-0.280
	(0.149)	(0.226)	(0.128)	(0.107)	(0.100)
No High School Diploma	-0.125	-0.117	-0.188	-0.194	-0.197
	(0.049)	(0.068)	(0.043)	(0.039)	(0.038)
Some Post-Secondary Schooling	0.114	0.141	0.145	0.139	0.129
	(0.049)	(0.070)	(0.044)	(0.039)	(0.038)
Age at Baseline	-0.014	-0.016	-0.001	-0.002	-0.007
	(0.004)	(0.006)	(0.004)	(0.003)	(0.003)
Years Worked as of Baseline	0.025	0.028	0.033	0.020	0.018
	(0.011)	(0.018)	(0.011)	(0.010)	(0.009)
Years Worked-Squared (Coeff x 1000)	-0.055	-0.011	-0.905	-0.367	-0.139
	(0.365)	(0.636)	(0.399)	(0.362)	(0.349)
R-Squared	0.080	0.115	0.207	0.198	0.202
Number Observations	926	465	655	728	790
Marginal Value of Additional Year of Work Assuming 6.6 Years Experience (percent)	2.4	2.8	2.1	1.5	1.6

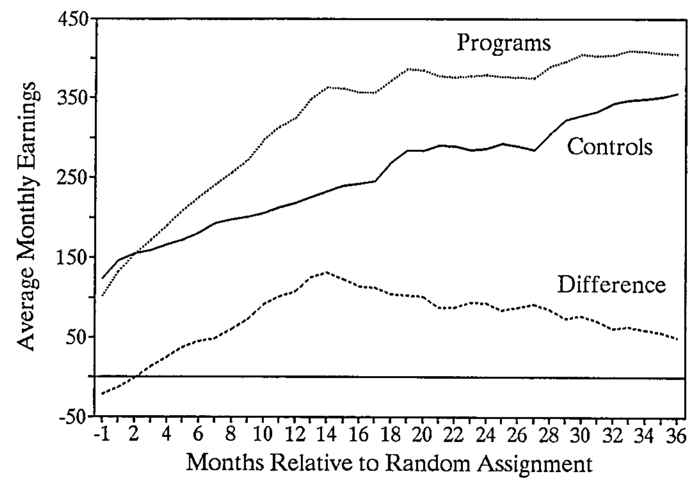
Notes: Standard errors in parentheses. Dependent variable is log of hourly wages in time period indicated in column heading. Model in column 1 is fit to observations for wage reported in baseline survey for members of program and control group who were working at the baseline. Models in columns 2-4 are fit to wages for control group only. All models also include an indicator for province of residence, and a linear trend term representing the calendar month of the wage observation, relative to July 1995.

Figure 1: Average Outcomes of Control and Program Groups

A. Monthly Employment Rate



B. Monthly Earnings



C. Monthly IA Participation Rate

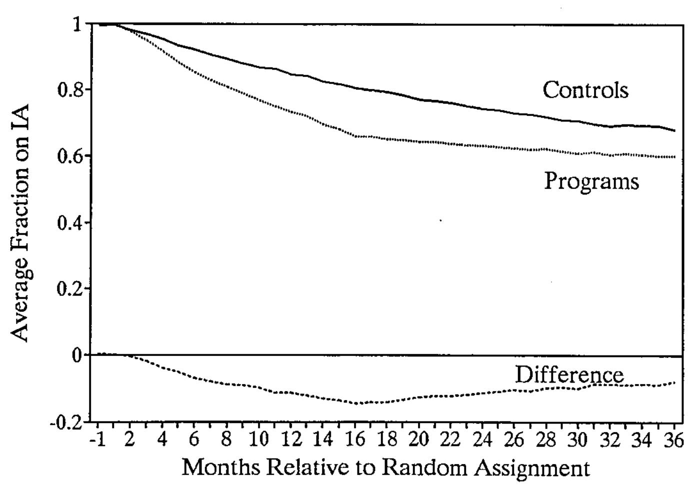


Figure 2: Distributions of Employment Probabilities

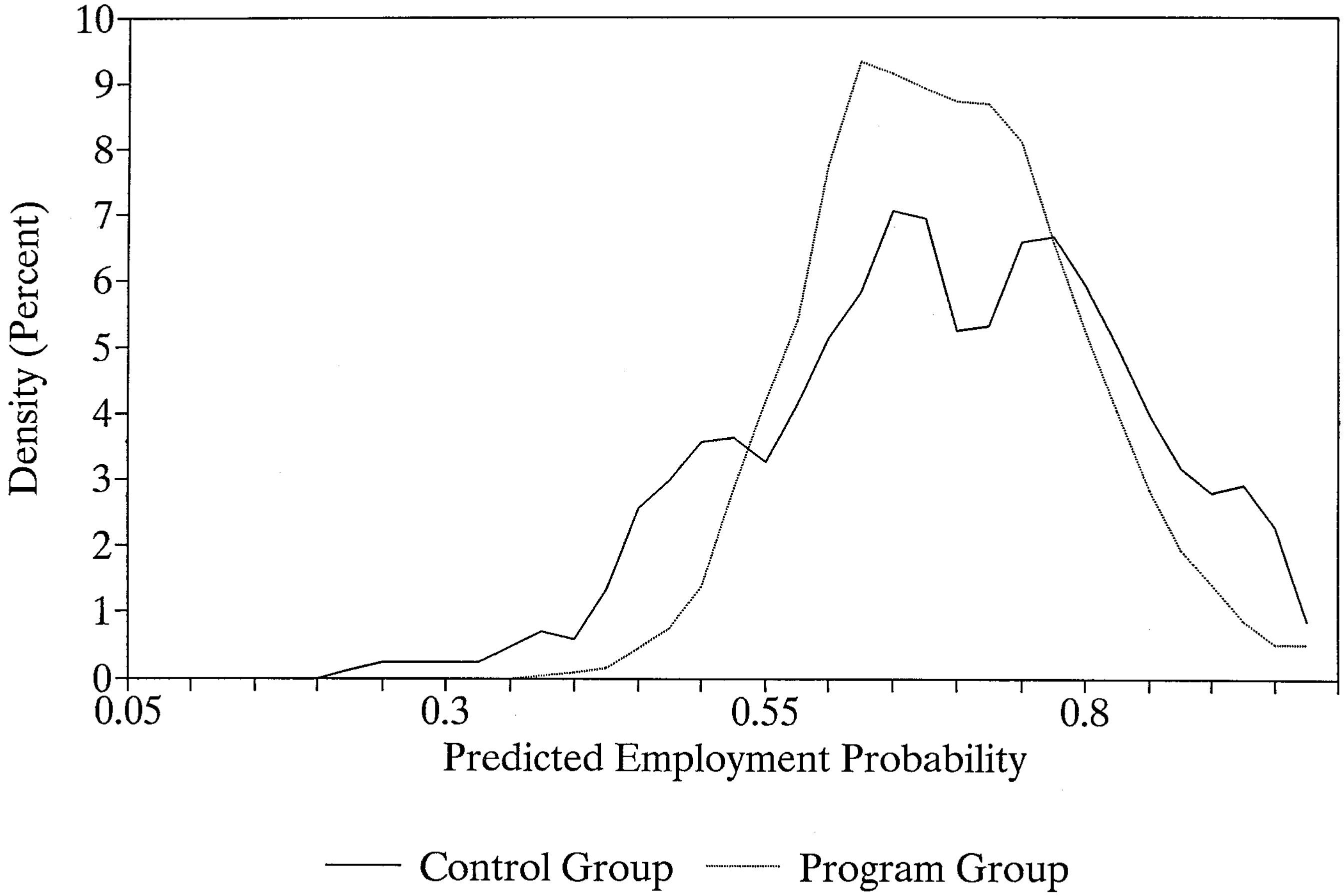


Table 1: Key Features of the SSP Recipient Demonstration

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, Moncton, and Fredrickton)

· 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: Comparison of Baseline Characteristics of Individuals Who Were Working in Months 12-14 in the Program and Control Groups

	Overall Sample	Working at Months 12-14 Only: Controls	Programs	Incentivized Programs: Mean	Incentivized Programs: Relative to Controls
Percent Female	95.6	96.7	96.1	94.8	-2.0
	(0.3)	(0.7)	(0.6)	(2.5)	(3.0)
Age Distribution:					
Percent Under 26	26.5	22.6	25.4	31.8	9.2
	(0.6)	(1.6)	(1.4)	(5.7)	(6.8)
Percent 26-34	38.6	39.1	40.6	43.9	4.8
	(0.7)	(1.9)	(1.5)	(6.5)	(7.9)
Percent Over 34	34.9	38.3	34.0	24.3	-13.9
	(0.7)	(1.9)	(1.5)	(6.4)	(7.7)
Percent without High School Diploma	53.6	39.5	43.2	51.4	11.9
	(0.7)	(1.9)	(1.6)	(6.6)	(7.9)
Percent Never Married	48.7	47.8	48.2	49.1	1.4
	(0.7)	(1.9)	(1.6)	(6.7)	(8.0)
Percent with Youngest Child Under 5	47.8	40.9	44.7	53.1	12.2
	(0.7)	(1.9)	(1.6)	(6.7)	(8.0)
Average Years of Work Experience	7.4	9.6	8.7	6.6	-3.0
	(0.1)	(0.3)	(0.2)	(0.9)	(1.1)
Average Months on IA in 3 Years Pre-Baseline	29.9	29.1	29.5	30.5	1.5
	(0.1)	(0.3)	(0.3)	(1.1)	(1.3)
Percent Whose Parents Were IA Recipients	25.2	21.9	21.9	22.1	0.2
	(0.6)	(1.6)	(1.3)	(5.6)	(6.8)
Percent Non-Workers in 12 Months Pre-Baseline	73.0	44.7	54.8	77.5	32.8
	(0.6)	(1.9)	(1.6)	(6.7)	(8.0)
Mean Earnings/Month in 12 Months Pre-Baseline	97.2	246.0	176.7	20.1	-225.9
	(3.5)	(14.4)	(9.7)	(45.4)	(56.7)
Like Going to Work[a]	31.5	43.7	38.8	27.8	-15.9
	(0.7)	(1.9)	(1.5)	(6.6)	(7.9)
Percent Working at Baseline	19.6	52.1	36.7	1.7	-50.4
	(0.6)	(1.9)	(1.5)	(6.5)	(7.9)
Sample Size	4,961	691	1,015	--	--

Notes: Standard errors in parentheses. Column 1 shows mean characteristics for the pooled sample of programs and controls (N=4,961 individuals who were in the Baseline and 36 Month follow-up interviews). Columns 2 and 3 show the mean characteristics of program and control group members who were working in months 12-14 after random assignment. "Work" is defined as having positive hours in 2 or more of the 3 months. Columns 4 and 5 show characteristics of the "incentivized program group" in months 12-14 and the difference in their characteristics relative to the control group who were working then.
[a]Percent who respond that they agree with the statement "I like going to work".

Table 3: Comparison of Wage and Hours Distributions of Individuals Who Were Working in Months 12-14 in the Program and Control Groups

	Controls	Programs	Incentivized Programs: Mean	Incentivized Programs: Relative to Controls
Average Hourly Wage	8.41	7.37	5.03	-3.38
	(0.24)	(0.11)	(0.65)	(0.87)
Wage Distribution Relative To Minimum:				
Missing or Below Minimum	16.8	9.2	-8.1	-24.9
	(1.4)	(0.9)	(4.4)	(5.5)
Minimum ± 5¢	8.7	14.4	27.3	18.6
	(1.1)	(1.1)	(4.3)	(5.0)
Minimum + 5¢ to $1	18.1	29.2	54.2	36.1
	(1.5)	(1.4)	(5.7)	(6.7)
Minimum + $1 to $2	17.8	20.7	27.2	9.4
	(1.5)	(1.3)	(5.3)	(6.3)
Minimum + $2 or More	38.6	26.6	-0.6	-39.2
	(1.9)	(1.4)	(6.2)	(7.5)
Average Weekly Hours	28.3	31.9	39.9	11.6
	(0.6)	(0.4)	(1.9)	(2.3)
Weekly Hours Distribution:				
Weekly Hours Missing	1.9	1.7	1.2	-0.7
	(0.5)	(0.4)	(1.8)	(2.1)
Weekly Hours < 29	46.0	25.5	-20.8	-66.8
	(1.9)	(1.4)	(6.2)	(7.6)
29-31 Hours	7.2	15.9	35.4	28.1
	(1.0)	(1.1)	(4.4)	(4.9)
31-40 hours	34.4	47.0	75.4	40.9
	(1.8)	(1.6)	(6.5)	(7.8)
Over 40 Hours	10.4	10.0	8.9	-1.5
	(1.2)	(0.9)	(4.0)	(4.9)

Notes: Standard errors in parentheses. Columns 1 and 2 show the means for program and control group members who were working in months 12-14 after random assignment. "Work" is defined as having positive hours in 2 or more of the 3 months. Columns 3 and 4 show means for the "incentivized program group" in months 12-14, and the difference in means between the incentivized program group and the control group.

Table 4: Comparison of Labor Market Outcomes of Program Group and Control Group Members Who Were Working at Baseline

	Mean Outcomes		Difference:	
	Control Group	Program Group	Raw	Adjusted
1. Percent Working in Months 12-14	73.0 (2.0)	78.0 (1.9)	5.0 (2.8)	5.4 (2.7)
2. Percent Working in Months 33-35	65.3 (2.1)	67.5 (2.1)	2.2 (3.0)	2.7 (3.0)
3. Cumulative Months Worked (Months 12-33)	15.3 (0.4)	15.5 (0.4)	0.3 (0.5)	0.4 (0.5)
4. Average Monthly Hours (Months 12-33)	20.1 (0.7)	22.0 (0.7)	1.9 (1.0)	2.2 (0.9)
5. Average Number of Months Of SSP (Months 12-33)	0.0	8.7 (0.4)	--	--
6. Mean Log Hourly Wage Month 12-14	1.98 (0.03)	1.94 (0.02)	-0.04 (0.04)	-0.03 (0.04)
7. Mean Log Hourly Wage Month 12-14 for those Working in Months 33-35	2.02 (0.04)	1.98 (0.03)	-0.04 (0.04)	-0.03 (0.04)
8. Mean Log Hourly Wage in Months 33-35	2.10 (0.03)	2.05 (0.03)	-0.06 (0.05)	-0.05 (0.04)
9. Mean Growth in Log Hourly Wages from Month 12-15	0.07 (0.03)	0.05 (0.03)	-0.02 (0.04)	-0.03 (0.04)
10. Median Growth in Log Hourly Wages from Month 12-15	0.05 (0.01)	0.05 (0.01)	0.00 (0.01)	0.00 (0.01)
11. Mean Growth in Log Hourly Wages from Month 12-15 (Trimmed)[a/]	0.08 (0.01)	0.07 (0.01)	-0.01 (0.02)	0.00 (0.02)

Notes: Standard errors in parentheses. Tabulations are based on subsamples of 493 control group members and 477 program group members who were employed at the baseline interview. Entry in column 3 is simple difference between outcomes of program group and control group. Adjusted difference in column 4 is from a regression model that includes controls for province, education, labor market experience, number/age of children, gender, and whether the individual was working full time or part time at baseline. Median regression is used in row 10.

[A/]Wage growth less than -0.35 is set to -0.35; wage growth greater than 0.50 is set to 0.50. 12 percent of wage growth observations for the control group are trimmed; 11 percent of wage growth observations for the program group are trimmed.

Table 5: Comparisons of Labor Market Outcomes of Program and Control Group Members Who Worked in Months 12-14

	Mean Outcomes		Difference:	
	Control Group	Program Group	Raw	Adjusted
1. Percent Working in Months 33-35	67.1 (1.8)	68.3 (1.5)	1.1 (2.3)	4.5 (2.3)
2. Cumulative Months Worked (Months 12-35)	17.1 (0.2)	17.1 (0.2)	0.0 (0.3)	0.5 (0.3)
3. Mean Log Hourly Wage Months 12-14	1.97 (0.02)	1.91 (0.01)	-0.06 (0.03)	-0.04 (0.02)
4. Mean Log Hourly Wage Months 12-14 for those Working in Months 33-35	2.01 (0.03)	1.95 (0.02)	-0.06 (0.03)	-0.04 (0.03)
5. Mean Log Hourly Wage in Months 33-35	2.11 (0.03)	2.00 (0.02)	-0.11 (0.04)	-0.08 (0.03)
6. Mean Growth in Log Hourly Wages, Months 12-14 to Months 33-35	0.09 (0.02)	0.05 (0.02)	-0.04 (0.03)	-0.04 (0.03)
7. Median Growth in Log Hourly Wages, Months 12-14 to Months 33-35	0.06 (0.01)	0.07 (0.01)	0.01 (0.01)	0.00 (0.01)
8. Mean Growth in Log Hourly Wages, Months 12-14 to Months 33-35, Trimmed	0.08 (0.01)	0.08 (0.01)	0.00 (0.01)	0.00 (0.01)
Addendum				
9. Mean Log Hourly Wage in Months 12-14 for Non-Workers in Months 33-35	1.87 (0.04)	1.82 (0.03)	-0.05 (0.05)	-0.06 (0.04)
10. Difference in Wages in Months 12-14, Workers vs. Nonworkers in Months 33-35	0.14 (0.05)	0.12 (0.03)	-0.01 (0.06)	--

Notes: Standard errors in parentheses. Tabulations are based on subsamples of 691 control group members and 1015 program group members who worked in at least 2 of months 12-14. Raw difference is simple difference between outcomes of program group and control group. Adjusted difference is the coefficient of an indicator for the program group from a regression model that controls for province, education, labor market experience, number/age of children, gender, labor market status at the baseline interview, attitudes toward work, and seasonals. Median regression is used in row 7.

Table 6: Differences in Wage Growth by Predicted Probability of Employment in Months 33-35

	Number of Obs.		Mean Wage Growth		Difference in Wage Growth		
Group	C's	P's	C's	P's	Raw	Adjusted(1)	Adjusted(2)
All	429	671	0.082 (0.010)	0.084 (0.007)	0.002 (0.013)	-0.006 (0.014)	-0.012 (0.014)
By Decile of Predicted Probability of Employment in Months 33-35:							
1	74	36	0.024 (0.025)	0.132 (0.041)	0.108 (0.048)	0.072 (0.055)	0.060 (0.069)
2	34	76	0.092 (0.038)	0.113 (0.019)	0.021 (0.043)	-0.017 (0.045)	-0.026 (0.067)
3	27	83	0.132 (0.046)	0.073 (0.021)	-0.058 (0.051)	-0.059 (0.035)	-0.075 (0.062)
4	39	71	0.104 (0.038)	0.091 (0.021)	-0.013 (0.043)	-0.016 (0.047)	-0.034 (0.052)
5	39	71	0.096 (0.037)	0.074 (0.018)	-0.022 (0.041)	-0.056 (0.044)	-0.069 (0.046)
6	29	81	0.115 (0.033)	0.081 (0.025)	-0.034 (0.031)	-0.002 (0.049)	-0.004 (0.055)
7	34	76	0.095 (0.031)	0.106 (0.023)	0.011 (0.038)	0.008 (0.044)	0.015 (0.052)
8	42	68	0.073 (0.035)	0.043 (0.022)	-0.030 (0.041)	-0.032 (0.043)	-0.007 (0.049)
9	51	59	0.068 (0.027)	0.084 (0.023)	0.016 (0.036)	-0.001 (0.042)	-0.033 (0.046)
10	60	50	0.098 (0.029)	0.058 (0.030)	-0.039 (0.042)	-0.005 (0.054)	0.047 (0.057)
Weighted Average of Difference in Growth Rates (Using Program Group Distribution)			0.095 (0.011)	0.084 (0.006)	-0.010 (0.012)	-0.016 (0.013)	-0.019 (0.016)

Notes: Standard errors in parentheses. Deciles of predicted probability of employment are obtained from probit model fit separately to program and control groups who were employed in months 12-14 with valid wage. Models include 24 covariates. Deciles are defined over pooled subsample of program and control group who have valid wage growth data. Adjusted(1) differences in wage growth of programs and controls are obtained from a regression model, fit by decile, that includes controls for gender, province, number and age of children and baseline labor force status. Adjusted(2) differences are obtained from regression model that includes adjusted(1) controls, plus measures of attitudes toward work, indicators for season, and interactions of key covariates with province. Regression models used in row 1 (for overall sample) also include dummy variables indicating the decile of the predicted employment probability.

Table 7: Selection-Corrected Models for Wage Growth from Months 12-14 to Months 33-35

	Specification: (1)	(2)	(3)
1. Program Group Difference[a/]	-0.004 (0.014)	-0.044 (0.030)	-0.003 (0.118)
2. Coefficient of Control Function	-0.068 (0.072)	-0.437 (0.251)	-0.006 (0.115)
3. Number of Included Control Variables	24	24	22
4. Program Group Interactions with Control Variables?	No	Yes	Yes
5. Estimated Residual Standard Error	0.200	0.200	0.200
6. R-squared	0.041	0.061	0.056
7. F-test for Excluded Control Variables And Interactions (P-value)	--	--	0.205

Notes: Standard errors in parentheses. All models are fit to subsample of 429 control group members and 671 program group members who were employed in months 12-14 and 33-35, and reported valid wages for both periods. Model in column 1 include 24 covariates plus single indicator for program group. Model in column 2 includes 24 covariates, fully interacted with program group status. Models in column 3 includes 22 covariates, fully interacted with program group status. All models also include program group dummy and control function based on first-stage probit models for the probability of employment in months 33-35, fit to the samples of control and program group members who worked in months 12-14. The probit models include 23 covariates and are fit separately to the program and control groups.

[a/]In interacted models, predicted difference in wage growth between program and control groups is evaluated at the mean characteristics of the program group.

Table 8: Selection-Corrected Models for Wage Growth from Months 12-14 to Months 33-35, with Program-Group Specific Correction

	Two-Step Estimates:		MLE Fit by Program Group
	(1)	(2)	(3)
1. Program Group Difference[a/]	-0.048 (0.043)	-0.073 (0.142)	-0.082 (0.121)
2. Coefficient of Control Function:			
a. Control Group	-0.075 (0.072)	-0.094 (0.199)	-0.034 ρ=-0.161 (0.856)
b. Program Group	0.015 (0.105)	0.038 (0.140)	0.127 ρ=0.616 (0.175)
3. Number of Included Control Variables	24	22	22
4. Program Group Interactions with Control Variables?	No	Yes	Yes
5. Estimated Residual Standard Error	0.200	0.200	C's: 0.211 P's: 0.207
6. R-squared	0.042	0.056	--

Notes: Standard errors in parentheses. Models in columns 1 and 2 are fit by ordinary least squares to wage growth data for program and control group members who were employed in months 33-35, and reported valid wage data. Model in column 3 is fit by maximum likelihood to data on the probability of employment in months 33-35, together with data on wage growth conditional on employment. See notes to Table 7 and text.

[a/] In interacted models, predicted difference in wage growth between program and control groups is evaluated at the mean characteristics of the program group.

Table 9: Changes in Minimum Wages and Differences in Wage Growth

	Number of Observations	Percent in BC	Average Change in Minimum Wage	Average Wage Growth (Pooled)	Difference in Wage Growth
All	1110	46.6	8.9	8.3 (0.6)	0.2 (1.3)
By Range of Minimum Wage Changes:					
No Change	197	27.4	0.0	6.3 (1.4)	3.6 (2.8)
\$5.00 to 5.25, \$5.25 to 5.50	75	0.0	4.7	6.2 (2.1)	-0.2 (4.4)
\$6.00 to 6.50 \$6.50 to 7.00	176	100.0	7.5	9.4 (1.7)	-3.7 (3.4)
\$5.00 to 5.50	369	0.0	9.5	9.0 (1.0)	-1.3 (2.0)
\$6.00 to 7.00	283	100.0	15.4	8.9 (1.3)	2.7 (2.7)

Notes: Standard errors in parentheses. Tabulations based on sample of 429 control group members and 671 program group members who were employed in months 12-14 to 33-35 (and reported valid wages for both periods). Minimum wage change is percent change in the log of the nominal minimum wage from month 12 to month 33. Average wage growth is the percentage change in the log of the wage from months 12-14 to months 33-35 for both program and control groups. Difference in wage growth is the difference in average percentage wage growth between program and control groups.

Table 10: Estimated Wage Growth Models, Including Minimum Wage Variables

	Specification:					
	(1)	(2)	(3)	(4)	(5)	(6)
Program Group Dummy	0.002 (0.012)	0.002 (0.012)	0.005 (0.024)	0.000 (0.013)	0.000 (0.013)	0.000 (0.025)
Change in Minimum Wage	--	0.179 (0.117)	0.199 (0.192)	--	0.115 (0.131)	0.115 (0.200)
Change in Minimum Wage × Program Group	--	--	-0.033 (0.243)	--	--	0.000 (0.245)
Number of Other Control Variables	0	0	0	24	24	24
R-squared	0.000	0.002	0.002	0.040	0.041	0.041

Notes: Standard errors in parentheses. Regression models are fit to sample of 429 control group members and 671 program group members who were employed in months 12-14 to 33-35 (and reported valid wages for both periods). Change in minimum wage variable (row 2) is the change in the log of the nominal minimum wage from month 12 to month 33. Control variables included in models 4-6 are same as those included in Adjusted(2) specifications in Table 6.