NBER WORKING PAPER SERIES

THE LIMITS TO WAGE GROWTH:
MEASURING THE GROWTH RATE OF WAGES FOR RECENT WELFARE LEAVERS

David Card
Charles Michalopoulos
Philip K. Robins

Working Paper 8444
http://www.nber.org/papers/w8444

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
August 2001

We are grateful to Gordon Berlin, Ken Chay, James Powell, Saul Schwartz, and participants in the Berkeley Labor Lunch for comments. The views expressed herein are those of the authors and not necessarily those of the National Bureau of Economic Research, SDRC or MDRC or the sponsors of the Self Sufficiency Project.

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NBER Working Paper No. 8444
August 2001
JEL No. J30, I38

ABSTRACT

We study the rate of wage growth among welfare leavers in the Self Sufficiency Program (SSP), an experimental earnings subsidy offered to long-term welfare recipients in Canada. Single parents who started working in response to the SSP incentive are younger, less educated, and have more young children than those who would have been working regardless of the program. They also earn relatively low wages in their first few months of work: typically within $1 of the minimum wage. Despite these differences, their rate of wage growth is similar to other welfare leavers. We estimate that people who were induced to work by SSP experienced real wage growth of about 2.5 - 3 percent per year - a rate consistent with conventional measures of the return to experience for similar workers.

David Card
Department of Economics
549 Evans Hall, #3880
UC Berkeley
Berkeley, CA 94720
and NBER

Charles Michalopoulos
MDRC
16 East 34th Street
New York, NY 10016

Philip K. Robins
Department of Economics
University of Miami
Coral Gables, FL 33124

A key question for understanding the long-run impact of welfare reform policies is whether welfare leavers can anticipate rapid wage growth, or whether their labor market opportunities will improve only modestly over time (see, e.g., the review by Gottschalk, 2000.) This concern is especially relevant for reforms targeted at long-term recipients, who often lack the skills to obtain higher-wage jobs when they first re-enter the labor market. The Self Sufficiency Program (SSP), an experimental welfare reform begun in the mid-1990s in two Canadian provinces, offers a striking illustration of the issues. SSP provides an earnings subsidy for up to three years to long-term recipients who leave welfare and enter full-time work. The subsidy reduces welfare participation and raises employment: within15 months, the employment rate of single mothers who are offered the supplement is 10-15 percentage points higher than the rate of a randomly-assigned control group (Lin et al, 1998). Nevertheless, the wages associated with these jobs are low. Two-thirds of those who entered work because of the SSP supplement were earning within $1 of the minimum wage. In the absence of significant wage growth, many of those who were induced to work by SSP may return to welfare when their supplement payments end.

In this paper we present estimates of the rate of wage growth experienced by former welfare recipients who were induced to work by SSP. A key feature of SSP is that it is being evaluated by a randomized design: one half of a sample of long-term welfare recipients (the treatment group) was offered the earnings supplement while the other half was not. Despite the presence of a true control group, a serious methodological issue arises because wages are only observed for those who work. Thus, it is impossible to compare average wage growth for *everyone* in the SSP treatment group to average wage growth for *everyone* in the control group.[1] Moreover, since SSP leads some people to find jobs who would not be working in the absence of the program, workers in the treatment group are more disadvantaged than workers in the control group. Because of these issues, we narrow our focus to the problem of estimating wage growth conditional on having been induced to enter work. Even this measure of wage growth is only partially observed since some of those who enter work in response to the program incentive do not continue working.

In Section I we propose a simple procedure for estimating the rate of wage growth for members of the SSP treatment group who were induced to enter work by the financial incentives of the program. Under a restrictive but testable assumption -- namely, that the program has no effect on wage growth for those who would have been working in its absence -- we show that we can in principle measure the rate of wage growth for the induced treatment group. If the program affects wage growth of people who would have been working in its absence, the relative growth rate of wages for the induced group can still be identified provided that there is a subgroup of the experimental population whose employment rates are unaffected by the program. A final problem is potential selectivity bias arising from the fact that wage growth can only be measured for those who are working at some initial point *and* at some later date. We show how standard econometric methods can be used to reduce or eliminate this bias.

In Section II we analyze wage growth among welfare leavers in the SSP demonstration. Comparisons of the those who were induced to start working by SSP versus others who would

[1] 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).

have been working in the absence of the program show that the induced workers are less-educated, have less work experience, and have more young children. Induced workers also earn significantly lower wages than those who would have been working without the earnings supplement. Despite these differences, the two groups have similar wage growth. Results from several different approaches suggest that our estimates of wage growth are not substantially affected by selectivity biases, although our most general selectivity models are not very informative. After accounting for inflation, we conclude that individuals who were induced by the SSP program to enter work in the 12-14th month of the experiment had real wage growth of about 2.5-3.1 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. Measures of Wage Growth

a. Wage Growth of the Induced Program Group

Suppose that an incentive program is offered to a group of welfare participants (the treatment or program group, identified by P_i=1) beginning at date t=0. Define an initial reference date $s \geq 0$ and consider the subset of individuals in the group who are working at *s*. 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 induced program group, and denote membership in this group by an indicator IP_i=1. We refer to the former as the non-induced program group, and denote membership in this group by NP_i=1. Note that the distinction between the two groups is made with respect to a particular time period *and* a particular measure of work activity. In the case of SSP, one might argue that the induced program group should only include people who are working full time, since SSP earnings subsidies are only available to full time workers. Some of those who are responding to the program incentive may begin working part-time as a stepping stone to full time work, however, and we believe it is important to include them in the induced group. Thus, throughout this paper we define the induced program group to include all those who were working at the initial reference date and would not have done so in the absence of the program incentive.

Measurement of wage growth requires the specification of a time interval over which to observe changes in wages. Let f denote the ending date for the observation window ($f > s$), let w_{it} denote the log wage of individual i in period t, and let D_{it} be an indicator variable equal to 1 for those who are working in period t. Then the (average) growth rate of wages for the induced program group is:

(1) $$g = E[\,\Delta w_i \mid D_{is}=1, IP_i=1\,],$$

where $\Delta w_i = w_{if} - w_{is}$. We take g to be the primary object of interest. Note that by definition everyone in the induced program group is working in period s and (in principle) has a wage rate. However, part of the group may not be working at f. Thus, Δw_i is only partially observed. The average growth rate for the induced program group can also be defined conditional on a vector of observed characteristics x_i: $g(x_i) = E[\,\Delta w_i \mid x_i, D_{is}=1, IP_i=1\,]$.

Even ignoring the partial observability of end-period wages, a problem arises for the estimation of (1) because we cannot distinguish individuals who were induced to work in period s from others who would have been working in the absence of the incentive. If a valid control group is available, however, then it is straightforward to identify the size and characteristics of the induced program group by comparing workers in the program and control groups.[2] Specifically, the difference in employment rates in period *s* between the treatment (P_i=1) and control (P_i=0) groups is an estimate of the fraction of the program group that is induced to work. The relative fraction of induced versus non-induced workers is

$$\phi = \{ E[D_{is} \mid P_i=1] - E[D_{is} \mid P_i=0] \} / E[D_{is} \mid P_i=1],$$

which is simply the "treatment effect" of the incentive program on the employment rate in period *s*, divided by the overall employment rate of the program group in *s*.

Average wage growth for members of the program group who were working in period *s* is a weighted average of the growth rates for the induced and non-induced subgroups:

$$(2) \quad E[\Delta w_i \mid D_{is}=1, P_i=1] = \phi\, E[\Delta w_i \mid D_{is}=1, IP_i=1] + (1-\phi)\, E[\Delta w_i \mid D_{is}=1, NP_i=1].$$

In general neither expectation on the right-hand side of equation (2) is observable. If the program incentive has no effect on wage growth of the non-induced group, however, then the expected wage growth of the non-induced group can be inferred from the wage growth of the control group:

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

Under this assumption, the expected growth rate of wages for the induced program group is equal to the growth rate for members of the control group, plus a differential Δ:

$$(4) \quad g = E[\Delta w_i \mid D_{is}=1, P_i=0] + \Delta,$$

where

[2] A randomized control group is not strictly necessary to implement the methods we describe. A non-experimental comparison group (arising, for example, from a quasi-experiment) will work too.

$$\Delta = \{ E[\Delta w_i \mid D_{is}{=}1, P_i{=}1] - E[\Delta w_i \mid D_{is}{=}1, P_i{=}0] \} / \phi \ ,$$

is the difference in the growth rates of wages for the program and control groups, divided by the fraction of the program group that was induced to work by the program incentive. Wage growth for the induced program group can be estimated consistently provided that we can estimate the growth rate of wages for people in the control group who were working at *s*, and the difference in growth rates between people in the treatment and control groups who were working in *s*.

The key assumption underlying equation (4) -- that the incentive program has no effect on wage growth of its "windfall beneficiaries" -- may fail if the program causes people who would have worked anyway to choose different jobs or change their behavior in other ways. In this case it is still possible to estimate the growth rate of wages for the induced program group, provided that there is an identifiable subgroup for which the program has no employment effect, and provided that the program has the same effect on wage growth for all windfall beneficiaries. Specifically, for a subgroup that has the same employment rate in the treatment and control groups, the employed program group consists *entirely* of non-induced workers. A comparison of wage growth between program and control group members of this subgroup therefore provides an estimate of the effect of the program on the wage growth of non-induced workers. If there is evidence of differential wage growth between the treatment and control groups for this subgroup, the implied estimate of the effect of the program on non-induced workers can be used to appropriately modify equation (4).[3]

[3] Suppose that the difference in growth rates of wages between treatments and controls in the subgroup is h, and assume that the effect of the program on all non-induced workers is the same. Then the appropriate modification of (4) is: $g = E[\Delta w_i \mid D_{is}{=}1, P_i{=}0] + \Delta - h(1-\phi)/\phi$.

b. Selection Biases in Observed Wage Growth

If a wage in the final period *f* were observed for all those who were working in the initial period *s*, equation (4) would be directly estimable. In general, 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 rates of wage growth for members of the control group and the program group with characteristics x_i who were working in the starting period:

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

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

Using equation (4), wage growth for the induced program group is $g(x_i) = a(x_i) + (b(x_i)-a(x_i))/\phi(x_i)$. To estimate $g(x_i)$ we therefore need to estimate both the *difference* in wage growth between the treatment and control groups $(b(x_i)-a(x_i))$, and the growth rate of wages for the control group, $a(x_i)$.

Let S_0 and S_1 denote the selectivity biases in the observed wage changes of the control and treatment groups between periods *s* and *f*, relative to the true changes for all those who are working in *s*:

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

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

If people with faster wage growth are more likely to remain employed, then both $S_0(x_i)$ and $S_1(x_i)$ will be positive. The relative magnitude of these two terms, however, is less clear.[4] Observed

[4]Program group members in SSP are eligible for an earnings subsidy, which might lead one to expect a higher employment rate at later dates, conditional on work at the initial date, and a smaller selection bias in their observed wage changes. As we show below, however, the later employment rates of people in the program and control groups who were working at the initial date are very similar.

wage growth for those who are working in both *s* and *f*, conditional on treatment group status and the covariates, is:

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

This equation shows that a regression of observed wage growth on individual characteristics and their interactions with a treatment group indicator will recover a combination of the true expected wage growth terms and the selection biases. Any selection bias in the wage growth of control group members who are employed at the end period relative to the population that was employed at the initial period will confound the estimation of $a(x_i)$. Any differences in selection bias between the program and control groups will confound the estimation of $b(x_i) - a(x_i)$.

One approach to the estimation of equation (6) is to assume that the selection biases are negligible conditional on a sufficiently rich specification of the control variables (i.e., $S_0(x_i) = S_1(x_i) = 0$ for all x_i). This is similar in spirit to the assumption underlying the propensity score method developed by Rosenbaum and Rubin (1983) for the program evaluation problem.[5] Under this assumption, consistent estimates of $a(x_i)$ and $b(x_i)$ can be obtained by regressing observed wage growth on a flexible function of the covariates, interacted with a program group indicator. More generally, if the selection biases in observed wage growth for members of the program and control groups are equal, equation (6) shows that it is still possible to consistently estimate the difference in wage growth $b(x_i) - a(x_i)$, and thus the difference in wage growth between the induced program group and the non-induced group.

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

An alternative approach is to specify a model of wage growth and employment status that leads to specific functional forms for the selection bias terms. 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 (conditional on working in period *s*). In this model the selection bias terms are just the conditional expectations of ϵ_i, given that an individual is re-employed at the end date *f*: Next, assume that the probability that an individual is employed at the end date, conditional on working at the initial data, is given by a latent index model:

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

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

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.[6] Equations (7) and (8) imply that the selection biases take the form:

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

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

[6]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 $k_i = x_i\,\gamma + e_i$. Then the probability of work for members of the program group is $P(\ x_i(\alpha(1+S)-\gamma) - (e_i - (1+S)u_i) \geq 0)$.

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 depend on 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:

(9a) $C_0(r_0(x_i,\pi_0); \theta_0) = \theta_0\, n(r_0(x_i,\pi_0))/N(r_0(x_i,\pi_0)) = \theta_0\, \lambda(r_0(x_i,\pi_0))$

(9b) $C_1(r_1(x_i,\pi_1); \theta_1) = \theta_1\, n(r_1(x_i,\pi_1))/N(r_1(x_i,\pi_1)) = \theta_1\, \lambda(r_1(x_i,\pi_1))$,

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, ρ_p is the correlation between ϵ_i and η_{pi} for the control group (p=0) or program group (p=1), and $n(\cdot)$ and $N(\cdot)$ are the normal density and cumulative distribution function, respectively.[7] In this case the unconditional wage growth functions $a(x_i)$ and $b(x_i)$ can be estimated by Heckman's (1979) two-step procedure. 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:

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

where $\xi_i = \Delta w_i - E[\Delta w_i \mid x_i, D_{if}=1, P_i]$. Equation (10) can be estimated by ordinary least squares, yielding consistent estimates of conditional mean functions $a(x_i)$ and $b(x_i)$.

The recent econometrics literature has proposed a series of semi-parametric generalizations of the two-step estimation technique which relax the assumption that the joint

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

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 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 (10) to yield selection-corrected estimates of the *difference* in wage growth between program and control group members at each x_i. Identification of $a(x_i)$, however, requires identification of the intercept in the control function, which in turn requires that we observe mean wage growth for a group of individuals whose probability of employment is "close to 1" (Heckman, 1990). If the control functions are not the same, then even the estimation of the expected difference in wage growth between the program and control groups requires estimation of the intercepts of the control functions. In view of this fact, and the relatively small sample sizes available to study wage growth in the SSP sample, we do not try to implement semi-parametric control function methods in this paper.

II. Measuring Wage Growth in the SSP Experiment

In this section we use the methods described in Section I to estimate the wage growth of the induced program group in the SSP experiment. We begin with some background information on the Canadian income assistance program for low-income families and a brief overview of SSP. 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 rate of wage growth for the induced 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 (Courchene, 1994). Faced with rising welfare caseloads and changing attitudes toward work, Canadian policy makers have begun to search for innovations in the structure of income support programs that can reduce welfare dependency. One concern is that the Income Assistance (IA) program -- the main welfare program for non-disabled non-elderly adults and their families -- provides limited incentives for work.[8] As in U.S. under the old AFDC program, Income Assistance benefits are reduced dollar-for-dollar by the amount of any earnings above a modest disregard level.[9] The implicit 100 percent tax rate on earnings, coupled with the availability of other program benefits such as dental services and prescription drugs, reduce the incentives for people who have entered IA to work more than a few hours per week.

The Self Sufficiency Project was designed as a rigorous test of the effect of enhanced work incentives on the behavior of long-term IA recipients.[10] Under SSP, an individual who

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

[9]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.

[10]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 (2000) provide a survey of other recent financial incentive programs for welfare participants in the U.S.

leaves IA and finds a full time job (or combination of jobs) receives a supplement equal to one-half of the difference between his or her actual earnings and a target level set well above the level of IA benefits available to most families. The supplement raises the financial reward for leaving welfare 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 single parent 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 five years, and uses administrative records and specialized surveys to measure the effects of the program.

Table 1 presents a brief summary of the SSP experiment, including sample eligibility requirements and key features of the supplement program. Relative to 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. Under SSP, however, she would receive an additional supplement payment of $817 per month, raising the financial advantage of work versus IA to $972 per month. The reward to work 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 feature of SSP is its time-limited eligibility. Individuals who initiate supplement payments within the first year after random assignment can receive supplements for up to three years in any month that they are working full time and off IA. Program group members who fail to initiate an SSP payment within the first year lose all future eligibility. This feature has some implications for the incentive effects of the program. Most importantly, program group members have a strong incentive to start work within a year of being offered the program. Any work incentives in later years of the program are presumably confined to people who successfully gained eligibility within the first year.

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

Consistent with its generous financial incentives, SSP has significant behavioral effects on employment and welfare outcomes. A brief summary of these impacts is provided in Figure 1, which shows average monthly employment, earnings, and welfare participation rates for members of the treatment and control groups in the first 36 months of the experiment.[11] A prominent 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 into work. Relative to this underlying trend, members of the program group have accelerated rates of leaving IA and entering employment. As expected given the time-limited nature of SSP eligibility, the divergence in outcomes of the program and control groups peaks at 12-14 months after random assignment. After the first year the employment rate of the program

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

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, we decided to measure the effects of SSP on relative wage growth of people who were induced to enter work in months 12-14 of the experiment. The end period of our observation interval is 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 different subsamples of the SSP experiment. For reference, the first column reports the characteristics of the entire experimental population.[12] The group is over 95 percent single mothers, with an average age of 30. About one half have a child under 5. Nearly all sample members report prior work experience: indeed, the average is 7.4 years. Nevertheless, 73 percent did no work in the year prior to random assignment, and only 20 percent were working at the baseline.

Columns 2 and 3 show the characteristics of people who were working in months 12-14. In order to focus on people with a reasonably strong labor market attachment by one year after random assignment, we only count as “workers” people who reported positive hours of work in at least two of the three months between the 12th and 14th month. Among the control group, a total of 691 individuals, or 28.1 percent, reported positive hours in at least 2 months. Among the program group the corresponding number was 1,015 individuals, or 40.6 percent. Using the

[12] Due to data requirements, the data in Table 2 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, equally balanced between the program and control groups.

control group to estimate the behavior of the program group in the absence of SSP, these numbers imply that 68 percent of program group members who were working in months 12-14 would have been working in the absence of the program, while 32 percent were induced to work by the program incentives.

Relative to the overall experimental population, people who worked in months 12-14 are better-educated, have more work experience, are less likely to have preschool-age children, have more positive attitudes toward work, and were more likely to be working at the baseline interview. Interestingly, the differences between workers and the overall population is smaller for the program group than the controls. This is consistent with the observation that SSP induces some people to work who would not have done so in the absence of the subsidy. Formally, the average characteristics of workers in the program group are a weighted average of characteristics for the induced and non-induced program groups. Assuming that the mean characteristics of the non-induced group are the same as those of control group members who were working in months 12-14, the characteristics of the induced program group can be estimated using a formula similar to equation (4).[13]

The mean characteristics of the induced program group are shown in column 4 of Table 2. We also show the differences in characteristics between the induced and non-induced program groups in column 5. People in the induced program group are younger, less likely to hold a high school diploma, have less positive attitudes toward work, and are more likely to have young children than people who would have been working regardless of SSP. Perhaps the most striking

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[13]The assumption that the non-induced 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.

difference is the gap in employment rates prior to random assignment. The estimated baseline employment rate for the induced program group is 1.7 percent, compared to 52 percent for the non-induced group.

The method used in Table 2 to compare the pre-program characteristics of induced and non-induced workers can also be used to compare post-random assignment outcomes. Of particular interest are the characteristics of the jobs obtained by the induced program 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 induced program group.[14] An important caveat is that the interpretation of the estimates in column 3 as characteristics of the induced program group is only valid under the assumption that SSP has no effect on the non-induced workers in the program group.

Comparisons of the wage outcomes between workers in the program and control group suggest that the jobs obtained by the induced program group pay relatively low wages. For example, 14.4 percent of the program group report wages within 5 cents of the minimum wage, compared to 8.7 percent of the controls. The assumption that all of the additional jobs paying above the minimum wage in the program group are attributable to the induced program group leads to the inference that 27.3 percent of the induced 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.[15] The

[14]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.

[15]As indicated by the -8.1 percent entry in column 3 for the fraction of the induced 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.

remainder (27.2 percent) were paid $1-2 above the minimum, with virtually none paid more than $2 over the minimum. By comparison, close to 40 percent of control group workers in months 12-14 earned at least $2 above the minimum wage. The low relative wages of the induced program group may explain why these people would not have worked in the absence of SSP.

A similar tabulation of weekly hours suggests that the hours distribution of the induced 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-induced 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 causes some people who would have been working part-time in the absence of the program to shift to full-time work.

A key conclusion we draw from Table 3 is that former welfare recipients who are induced to work by SSP obtain jobs that pay in a narrow range above the minimum wage. The relatively low wage outcomes are particularly noteworthy because without the SSP supplement a minimum wage job is not a particularly attractive alternative to IA.[16] Thus, in the absence of significant wage growth, one might expect a sizeable fraction of the induced program group to eventually return to IA. A secondary conclusion is that SSP tends to raise the hours of people in the non-induced 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

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[16]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.

slower wage growth, SSP may have some affect on the average wage growth of the non-induced program group – an issue to which we now turn.

c. Does SSP Effect the Wage Growth of Non-induced Workers?

As noted in section I, the rate of wage growth of the induced program group can be readily estimated if it is assumed that SSP has no effect on the wages of people who would be working without the program. We also noted that this assumption can be tested by comparing the rates of wage growth in the treatment and control groups for a subgroup whose employment rate in months 12-14 is unaffected by the SSP incentive. 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. As motivation for this analysis, observe in row 1 of the table that the employment rate in months 12-14 for the subset of the program group who were working at the baseline is insignificantly different from the employment rate of the comparable controls. 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. Another way to see the same point is to recall from Table 2 that the fraction of the induced program group who were working at the baseline is essentially zero. Thus, people who were working at the baseline are nearly all in the non-induced group. A comparison of the growth rates of wages between the program and control groups in this subgroup therefore provides a test of the effect of SSP on the wage growth of the non-induced program group.

The entries in rows 2-4 of Table 4 suggest that treatment and control group members who were working at the SSP baseline have similar labor force attachment from months 12-14 to months 33-35, although the program group has slightly higher average hours per week. The mean log wage of the program group in months 12-14 is slightly lower than that of the controls, although the gap is insignificant (t-ratio =1). The average growth rate of log hourly wages over the period from months 12-14 to months 33-35 is about 7 percentage points for the control group and 5 percentage points for the program group, suggesting that if anything the induced program group has slower wage growth that the non-induced group. A detailed examination of the wage growth data, however, reveals a small number of outliers that potentially effect the comparison. A standard 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 very similar (and quite precisely estimated.) Row 11 shows that a similar conclusion emerges from the trimmed wage changes, constructed by censoring observations above the 95th percentile or below the 5th percentile of the pooled distribution.

Based on the results in Table 4 we conclude that the availability of SSP has no 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-induced workers, it is reassuring. Moreover, baseline workers account for 70 percent of all control group members who were working in months 12-14. Since the control group reproduces the behavior of the non-induced program group, we infer that baseline workers comprise about 70 percent of the non-induced group. At a minimum, then, we can conclude that SSP has no effect on wage growth for most of the non-induced group.

d. Wage Growth in the Program and Control Groups

To implement equation (4) we need to obtain estimates of both the difference in wage growth between the program and control groups of the SSP demonstration, and the average rate of wage growth for the control group. We begin by focusing on the differential in growth rates. Table 5 shows data on people in the two 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.[17] The entries in row 1 indicate that just over two-thirds of the treatments and controls who were working in months 12-14 were re-employed in months 33-35.[18] 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.

The third row of the table shows the mean log wages of the two groups in months 12-14. At this early stage of the SSP experiment workers in the program group had about 6 percent lower wages than those in the control group. This gap is consistent with the argument that people in the induced program group earn relatively low wages when they first enter the labor market. Indeed, if one assumes that 68 percent of program group would have had the same

[17]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.

[18]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.

wages as the control group in the absence of the program, then the mean log wage of the induced workers was about 1.78, or 20 percent below the mean wages of the non-induced group.

Row 4 shows the mean log wages in months 12-14 for the subset of workers who were re-employed in months 33-35. This subgroup is positively selected: the wage gap between all workers and those who were employed 21 months later is 4 percentage points in both the treatment and control 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 difference in mean wages between those who were re-employed in months 33-35 and those who were not. For both groups the 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 treatment group as for the controls. This is potentially reassuring, since many standard models of earnings dynamics imply that any selection bias in the growth rate of wages is proportional to the selection bias in the level of wages at the start of the interval.[19] In these models the equality of the differences between rows 3 and 4 for the treatment and control groups implies that the average selection biases in measured wage growth for the two groups are equal.

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[19]For example, if wages in months 12-14, wages in months 33-35, and the unobserved error component determining the probability of employment in months 33-35 are jointly normally distributed with the same covariance structure in the program and control groups, then if the two groups have the same selection bias in the levels of wages in month 12-14, they also have the same selection biases in the growth rate of wages. 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.

Row 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 growth in wages between months 12-14 and 33-35. 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 virtually disappears when medians, rather than means are analyzed, or when the observations are trimmed to eliminate the influence of extreme outliers. Taken at face value this similarity implies that the induced program group had virtually the same wage growth as those who would have been working in months 12-14 in the absence of SSP.

There are two limitations of the simple differences in wage growth between the program and control groups in Table 5. The first is that these estimates are based on specifications that assume a constant differential between members of the program and control groups, irrespective of their observed characteristics. 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, is equal to $S_0(x_i)$, 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 selectivity issue, we began by estimating a probit model for the probability of working in months 33-35, conditional on working in months 12-14. 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 show two interesting features. First, the predicted probabilities for the program group have virtually the same mean as the predicted probabilities for the control group, but somewhat less dispersion.[20] Second, the support of the distribution of predicted probabilities for the program group is contained in the support of the distribution for the control group.[21]

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) is restricted so that the control functions for the program and control group are the same. Then a person in the program group and a person in the control group with the same predicted probability of

[20]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.

[21]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 for program evaluation problems by Heckman, Ichimura, Smith and Todd (1998).

employment have the same selectivity biases their observed wage changes.[22] 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 re-employment in months 33-35. For simplicity, in this table and the remainder of the paper we use the trimmed wage growth measure described above. Row 1 reports data for 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 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).[23] 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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[22]This follows from the fact that the index $r_p(x_i,\pi_p)$ is an invertible function of the probability of employment: $Prob(D_{if}=1|x_i,...) = Prob(r_p(x_i,\pi_p)-\eta_{pi}>0) = F(r_p(x_i,\pi_p))$, where F is the distribution function of η_{pi}.

[23]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 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, 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 wage growth equation that includes the conventional (multivariate-normal) control functions given by equations (9a) and (9b). Table 7 reports estimation results for a series of models based on this approach. The first three columns report models that assume the same control function for the treatment and control groups, while the fourth and fifth columns report models with group-specific control functions. The specification in column 1 includes a set of 24 control variables, a dummy variable for the program group, and a conventional selection correction term. The model in column 2 generalizes this by including a full a set of interactions of the covariates with the program group dummy. We summarize the estimates from this specification by reporting 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 the probability of employment is based on the same set of 24 covariates fully interacted with program group status, the coefficient of the control function in this specification is identified by functional form alone. The model in column 3 is similar to the one in column 2, but it imposes some exclusion restrictions. Specifically, four covariates representing the effect of different seasons of the year are included in the employment probability model but excluded from the wage growth equation.[24] These exclusions are valid if jobs are harder to find in the winter months, but rates of pay do not vary over the seasons.

The specifications in columns 1 and 3 of Table 7 both yield small and statistically insignificant estimates of the selection parameter $\theta = \rho\sigma_{\epsilon}$ that indexes the correlation between wage growth and the probability of employment in months 33-35. 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.[25] These findings are consistent with the simple differences in mean wage growth in Table 5, which are valid under the assumption that $\theta=0$. By comparison, the specification in column 2 of Table 7 leads to a point estimate of θ 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 but imprecise. A concern with this specification is that the

[24]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 fall in the winter have significantly lower probabilities of employment in these months. This seasonality effect is stronger in New Brunswick.

[25]The model in column 3 imposes the restriction that the 4 seasonal terms are omitted from the wage growth equation. We tested this using a conventional F-test. The p-value of the test statistic is 0.21, providing little evidence against the exclusion restriction.

estimate of θ is so large that the implied estimate of ρ is bigger than 1 in absolute value.[26] 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.

Columns 4 and 5 present models that allow group-specific control functions. The model in column 4 is similar to the baseline specification in column 1, while the model in column 5 is similar to the one in column 3. Comparisons of parallel models with and without group-specific selection terms suggest that the data are not sufficiently rich to allow separate identification of the program-group-specific control functions and the difference in mean wage growth between the two groups. For example, comparing the specifications in columns 1 and 4, the introduction of group-specific control functions raises the standard error of the difference in mean wage growth from 0.014 to 0.043, but yields no improvement in the fit of the model. For what they are worth, the estimates in columns 4 and 5 suggest that there is some negative selection bias in the observed wage growth of the control group, and some positive selection bias in the observed wage growth of the program group, although the estimates are quite imprecise. Overall, the results in Table 7 are consistent with the hypothesis that there is no selectivity bias in the observed wage growth of either the program or control groups, although the estimates from the more general specifications are not particularly informative.

Based on the range of evidence in Tables 5, 6, and 7, we conclude that SSP program group members who were working in months 12-14 had about the same wage growth as the

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

members of the control group. Moreover, there do not appear to be any significant interactions between the observable baseline covariates and the difference in wage growth between the program and control groups. Thus, our best estimate is that people in the induced program group had about the same distribution of wage growth as people who would have been working without the SSP incentive. The estimates in Tables 5 show a mean growth rate in wages of 8.0 to 9.0 percent for both the program and control groups over the 21 month period from the 12th to 33rd months of the SSP experiment. Since there is no evidence of selectivity bias in the observed wage growth of either group, we conclude that the average wage growth of the induced program group was 8-9 percent. During the period from 1992 to 1996, the inflation rate averaged 2 percent per year. Thus, the induced program group in the SSP experiment had real wage growth of about 4.5 to 5.5 percent, or a growth rate of about 2.6 - 3.1 percent per year.

e. The Effect of Minimum Wage Increases

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.[27] Given the concentration of the induced 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

[27]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.

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 fit the wage growth models shown in Table 8. These models include a program group dummy, the percentage change in the minimum wage (which varies across people depending on their province and their date of enrollment in SSP), and an interaction of the minimum wage change with the program group dummy.

The estimates suggest a small 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 because minimum wages changes create ripple effects that raise wages of all low wage workers, and not just those closest to the old minimum (see Card and Krueger, 1995, chapter 5). Given these estimates, 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.[28]

f. Other Evidence on Wage Growth

The slow rate of real wage growth for people who were induced to enter work by the financial incentives of SSP suggests a limited role for work experience to boost the earnings of welfare leavers. In an effort to better understand this finding, we examined the cross sectional association between wages and previous work experience in the SSP population. Table 9 presents a series of conventional human capital earnings models, fit to the hourly wages of the SSP control and program group members 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 standard practice, all of these models include controls for education and gender, and a quadratic function of years of actual labor market experience as of the baseline. The estimates imply that for a group of workers with an average of 6.6 years of work experience (the mean for the SSP induced program group, as shown in Table 2), each additional year of work experience is associated with 1.5-2.8 percent higher real hourly wages. Members of the induced program group worked about 17 months, on average, over the period from months 12-14 to months 33-35. Thus, the cross-sectional estimates suggest they should have experienced real wage growth of 2-4 percent over this period, a little less than actually occurred. The difference is potentially

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[28]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 8.

explained by the effect of the minimum wage increases during the SSP experimental period.[29] Taking this factor into account, estimates of the wage growth for the induced program group are fairly close to the rates one would expect based on the cross-sectional return to experience in the SSP population.

Gottschalk (2000) has summarized the implications of several previous studies that provide estimates of the rate of wage growth of welfare leavers. He finds a range of estimates between 2 percent per year (in a study by Moffitt and Rangarajan, 1989) and 4.5 percent per year (in a study by Bartik, 1997). Although they do not focus on welfare leavers, Gladden and Taber (2000) estimate real wage growth rates of 3-4 percent per year of actual work for less-educated women for the first 10 years in the labor market. Presumably this is an upper bound on the rate of wage growth for a typical member of the SSP induced program group, who had 7 years of work experience and worked about 70 percent of the time in our observation window. This evidence is consistent with relatively modest rates of wage growth for low-skilled female workers in general, and for welfare leavers in particular.

III. Conclusions

This paper presents estimates of the rate of wage growth among former welfare recipients in the Self Sufficiency Project. We present a simple procedure for estimating the wage growth of the induced program group -- people who would not have been working in the absence of SSP -- using the observed growth rates of wages in the SSP treatment and control groups. The key

[29]Another partial explanation is that there is measurement error in reported experience that leads to a downward bias in the experience coefficients in Table 9.

requirement is that the SSP supplement has no effect on the wage growth of those who would have been working in its absence. Although this assumption cannot be fully tested, it can be evaluated for subgroups of the experiment, and it appears to be valid for the relatively large subset of the SSP population who were working at the baseline of the experiment.

Even under this assumption, a potential selectivity problem arises because wage growth is only measurable for people who are working at some initial point and are re-employed at a 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. Our interpretation of the results is that selection biases in the observed growth of wages are small, although our most general models are imprecise.

Based on the range of available evidence we conclude that SSP leads to wage growth among the induced program group that is very similar to the growth experienced by people who would have left welfare and entered work without the program's incentive. After accounting for inflation, we find that both groups had growth rates in the range of 2.5 to 3 percent per year. Up to a percentage point or more of this growth may have been attributable to minimum wage increases that occurred during the experiment. The range of estimated growth rates for SSP-induced welfare leavers, while modest, is in line with predictions based on the cross-sectional association of wages and labor market experience in the SSP population. Overall, the evidence from the SSP experiment suggests that welfare leavers who are induced to enter work can anticipate wage growth that is neither much faster nor much slower than the rate for other workers.

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Table 1: Key Features of the SSP Recipient Demonstration

A. Program Eligibility

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

· sample members drawn from IA registers in lower mainland British Columbia (including Vancouver) and southern New Brunswick (including Saint John, 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

		Working at Months 12-14 Only:			
				Induced Programs:	
	Overall Sample	Controls	Programs	Mean	Relative to Controls
Percent Female	95.6	96.7	96.1	94.8	-2.0
Percent Under Age 26	26.5 (0.6)	22.6 (1.6)	25.4 (1.4)	31.8 (5.7)	9.2 (6.8)
Percent without High School Diploma	53.6 (0.7)	39.5 (1.9)	43.2 (1.6)	51.4 (6.6)	11.9 (7.9)
Percent Never Married	48.7 (0.7)	47.8 (1.9)	48.2 (1.6)	49.1 (6.7)	1.4 (8.0)
Percent with Youngest Child Under 5	47.8 (0.7)	40.9 (1.9)	44.7 (1.6)	53.1 (6.7)	12.2 (8.0)
Average Years of Work Experience	7.4 (0.1)	9.6 (0.3)	8.7 (0.2)	6.6 (0.9)	-3.0 (1.1)
Average Months on IA in 3 Years Pre-Baseline	29.9 (0.1)	29.1 (0.3)	29.5 (0.3)	30.5 (1.1)	1.5 (1.3)
Percent Non-Workers in 12 Months Pre-Baseline	73.0 (0.6)	44.7 (1.9)	54.8 (1.6)	77.5 (6.7)	32.8 (8.0)
Percent Who Like Going to Work[a]	31.5 (0.7)	43.7 (1.9)	38.8 (1.5)	27.8 (6.6)	-15.9 (7.9)
Percent Working at Baseline	19.6 (0.6)	52.1 (1.9)	36.7 (1.5)	1.7 (6.5)	-50.4 (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 "induced 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	Induced Programs: Mean	Induced Programs: Relative to Controls
Average Hourly Wage	8.41 (0.24)	7.37 (0.11)	5.03 (0.65)	-3.38 (0.87)
Wage Distribution Relative To Minimum:				
Missing or Below Minimum	16.8 (1.4)	9.2 (0.9)	-8.1 (4.4)	-24.9 (5.5)
Minimum ± 5¢	8.7 (1.1)	14.4 (1.1)	27.3 (4.3)	18.6 (5.0)
Minimum + 5¢ to $1	18.1 (1.5)	29.2 (1.4)	54.2 (5.7)	36.1 (6.7)
Minimum + $1 to $2	17.8 (1.5)	20.7 (1.3)	27.2 (5.3)	9.4 (6.3)
Minimum + $2 or More	38.6 (1.9)	26.6 (1.4)	-0.6 (6.2)	-39.2 (7.5)
Average Weekly Hours	28.3 (0.6)	31.9 (0.4)	39.9 (1.9)	11.6 (2.3)
Weekly Hours Distribution:				
Weekly Hours Missing	1.9 (0.5)	1.7 (0.4)	1.2 (1.8)	-0.7 (2.1)
Weekly Hours < 29	46.0 (1.9)	25.5 (1.4)	-20.8 (6.2)	-66.8 (7.6)
29-31 Hours	7.2 (1.0)	15.9 (1.1)	35.4 (4.4)	28.1 (4.9)
31-40 hours	34.4 (1.8)	47.0 (1.6)	75.4 (6.5)	40.9 (7.8)
Over 40 Hours	10.4 (1.2)	10.0 (0.9)	8.9 (4.0)	-1.5 (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 “induced program group” in months 12-14, and the difference in means between the induced 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 season. Median regression is used in row 7.

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

Group	Number of Obs. C's	Number of Obs. P's	Mean Wage Growth C's	Mean Wage Growth P's	Difference in Wage Growth Raw	Difference in Wage Growth Adjusted(1)	Difference in Wage Growth 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 employment probability are obtained from probit model fit separately to program and control group members 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. Adjusted(1) difference in wage growth is 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) difference is obtained from a 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

	Single Control Function			Group-Specific Control Functions	
	(1)	(2)	(3)	(4)	(5)
1. Program Group Difference[a/]	-0.004 (0.014)	-0.044 (0.030)	-0.003 (0.118)	-0.048 (0.043)	-0.073 (0.142)
2. Coefficient of Control Function					
a. Control Group	-0.068 (0.072)	-0.437 (0.251)	-0.006 (0.115)	-0.075 (0.072)	-0.094 (0.199)
b. Treatment Group	--	--	--	0.015 (0.105)	0.038 (0.140)
3. Number of Included Control Variables	24	24	22	24	22
4. Program Group Interactions with Control Variables?	No	Yes	Yes	No	Yes
5. Estimated Residual Standard Error	0.200	0.200	0.200	0.200	0.200
6. R-squared	0.041	0.061	0.056	0.042	0.056

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: 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.

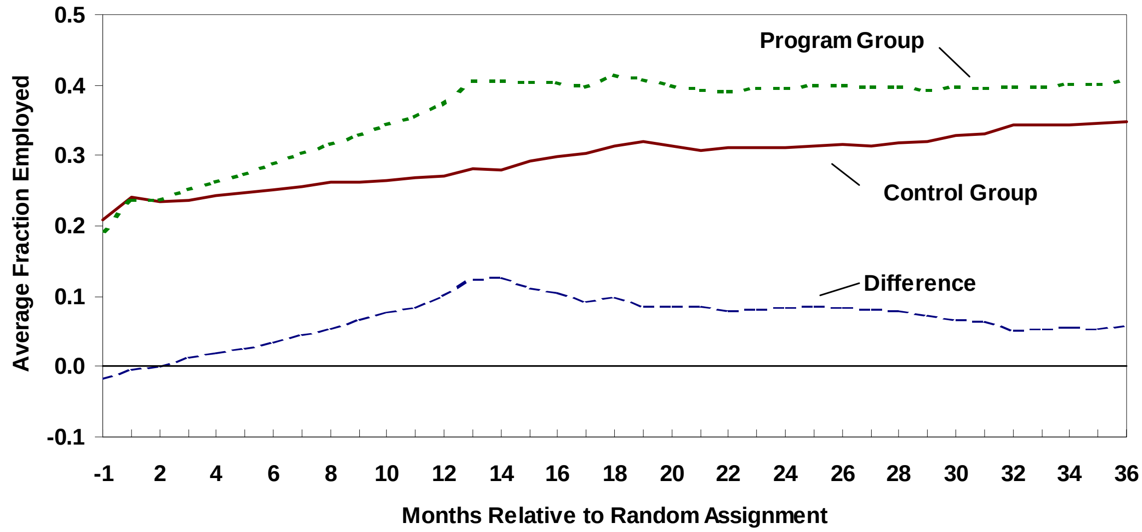
Table 9: Estimated Cross-Sectional Models for Log Hourly Wages

	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 (Coefficient 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 Rates**



b. **Montly Earnings**

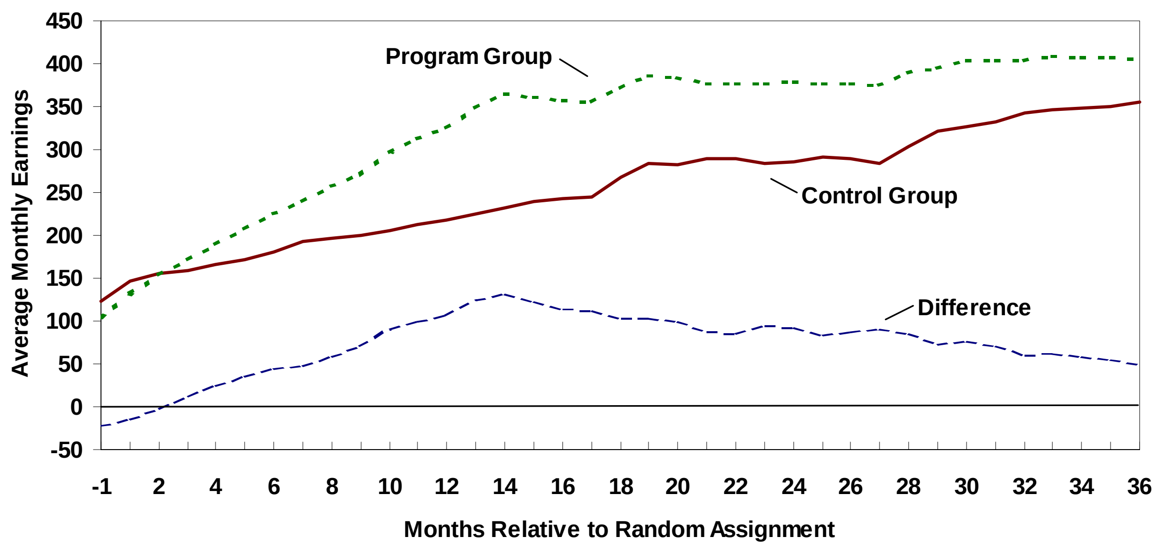


Figure 1: Average Outcomes of Control and Program Groups, continued

c. Monthly Welfare Participation Rates