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ABSTRACT

Do Earnings Subsidies Affect Job Choice?*

It is widely acknowledged that earnings subsidies promote employment by increasing rewards to labor market activity. This paper asks whether subsidies also affect job duration and wage growth. We provide an analytical framework that identifies causal links between earnings subsidies, job turnover, and wage growth. This framework highlights the importance of the form of the subsidy on the decision about the type of job to accept and, hence, its potential effect on within-job wage growth. The subsidy is predicted to increase job turnover and to affect between-job wage growth by affecting reservation wages. We use this framework to analyze the effects of the Canadian Self-Sufficiency Project (SSP). Consistent with the theory, we find that experimentals have shorter job duration and experience faster within-job and between-job wage growth than experimentals who continued to be eligible only for Income Assistance.

JEL Classification: J23, J38

Keywords: wage subsidy, job choice

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Do Earnings Subsidies Affect Job Choice?

The Impact of SSP Subsidies on Job Turnover and Wage Growth

1 Overview

It is widely acknowledged that earnings subsidies promote employment by increasing rewards to labor market activity. This paper addresses the question of whether subsidies also alter the types of jobs held (in terms of initial wages and wage growth) and the duration in these jobs.This question is especially important given the recent U.S. welfare reform that focuses on reducing the welfare rolls by requiring recipients to enter the labor market. If subsidies encourage low–income workers to find jobs with higher within–job wage growth or jobs with greater wage growth between jobs, then subsidies may lead to self–sufficiency.

While the analytical links between earnings subsidies and employment are well developed, the links between subsidies and the choice of jobs are less transparent. We, therefore, start by developing analytical links between the availability of an earnings subsidy and the type and duration of jobs individuals will accept. We then use data from a large earnings subsidy experiment to explore whether the replacement of an income assistance system by an earnings subsidy leads to differences in wage growth and job duration between experimentals and controls.

Our focus on the impact of earnings subsidies on wage growth and job duration stands in contrast to the previous literature that has focused primarily on the impact of earnings subsidies on labor supply decisions.[1] By increasing the rewards to work, earnings subsidies induce some eligible participants to join the labor market and encourage others to increase the number of hours worked. These predictions about the effect of a subsidy on labor supply have been strongly supported by the data.[2] By themselves, these labor supply effects will lead to higher earnings, but not necessarily to greater wage growth. Our empirical results indicate that experimentals who are eligible for an earnings subsidy experience faster within–job and between–job wage growth than do controls who continue to be covered by the original income transfer system.

This paper contains six sections. We start in Section 2 by developing a framework that provides the analytical links between wage subsidies and the decisions that affect wage growth. Section 3 presents the econometric issues and Section 4 presents details of our data and results. Concluding remarks are in Section 5.

2 Analytical Framework

This section provides the conceptual links between program attributes and the economic factors that affect both the decision to search for a better job and the decision of which offer to accept.

[1] An exception is Heckman et al. (2002), which explores the impact of the Earned Income Tax Credit (EITC) on wage growth through its effect on human capital investment and learning–by–doing.

[2] See Card et al. (2000).

2.1 Within–job Wage Growth

In this section, we consider whether offering a wage or earnings supplement can affect the choice between jobs with different wage profiles. In order to isolate the effects of wage growth on job choice, consider two jobs that have different wage paths (i.e., starting wages and expected wage growth), but are similar in terms of hours and expected job duration.[3] Suppose workers choose between jobs on the basis of their expected wage streams. The question we ask is whether the introduction of a wage supplement affects the choice of jobs. For example, if a job with a high initial wages but low wage growth has a higher present value than a job with a low starting wage but high wage growth, then it will be chosen in the absence of a subsidy. Can the availability of a wage subsidy reverse this job choice? If so, then the program induces workers to choose higher wage growth jobs.

Appendix A shows that the effect of the subsidy on job choice depends on the form of the mapping of pre–subsidy wages into post–subsidy wages. If the transformation is convex (concave) the person is more (less) likely to accept a job with wage growth when the supplement is available.

In the special case of a linear supplement, the wage subsidy has no affect on the threshold. Constant wage jobs that would be accepted before the subsidy will still be accepted after the subsidy. Therefore, the linear subsidy has no affect on the choice of jobs. Intuitively, the job with low starting wages and high wage growth will have a larger subsidy in the early periods and a smaller subsidy in the later periods. The job with low wage growth and higher starting wages will have a smaller supplement early on, but a larger supplement in the later periods when the wage in the high wage growth job starts exceeding the wages in the job with lower wage growth. If the subsidy is a linear function of pre–subsidy earnings, the lower subsidies in the later periods will just offset the higher subsidies in the earlier periods.[4] This is analytically equivalent to the result in the public finance literature that income–maximizing taxpayers are indifferent to the timing of income if the tax system is proportional.

As we will show, the Self–Sufficiency Project (SSP) subsidy is effectively linear, but the Income Assistance (IA) transfer program it replaces is highly concave.[5] This implies that IA discourages workers from taking jobs with low starting wages but high wage growth. Since SSP does not have this disincentive, SSP experimentals are expected to be more likely than IA controls to take jobs with low initial wages, but high wage growth.

2.2 Job Turnover and Between–job Wage Growth

Earnings subsidies may also affect job duration and between–job wage growth by altering: (1) the decision to search for a new job, (2) the type of search (search on the job or while unemployed), and (3) the reservation wage. Changes in the decision to search and the type of search will affect job duration. The intuition for the relationship between subsidies and the decision to search on the job or while unemployed is straightforward. Subsidies increase

[3] Wage and earnings supplements are analytically equivalent if hours are fixed.

[4] This prediction would have to be modified to take discounting or risk aversion into account, but the principle is the same.

[5] The EITC is also a concave mapping of pre-subsidy earnings into post-subsidy earnings.

the cost of searching while unemployed since refusing a wage offer and continuing to search results in losing both the offered wage and the subsidy. This increase in the cost of search increases the probability of accepting an offer and possibly continuing to search while on the job.

The effect of a wage subsidy on the expected wage gain between jobs, however, cannot be signed. As we will show, the subsidy increases the benefits of search by increasing the value of each subsidized offer, but the expected wage gain between jobs can increase or decrease. To show this, we introduce an earnings supplement into the framework developed in Burdett (1978)'s classic article on the choice between full–time search and on–the–job search. In order to focus on essentials, we use a standard search framework in which agents are assumed to pay a fixed price to obtain draws from a known wage offer distribution. They must then decide whether to accept that wage or continue to search.[6]

Let $f(w)$ be the distribution of wage offers and let c_{fts} and c_{ojs} be the out–of–pocket costs of obtaining an offer while searching full–time (while unemployed) and while searching on the job, respectively. We follow Burdett (1978) in considering the case where out–of–pocket search costs are lower when searching full–time than when searching while holding another job (i.e., $c_{fts} < c_{ojs}$).[7]

We start by considering the decision, in the absence of a wage subsidy, whether to search and, if so, whether to search full–time or on the job. Burdett (1978) shows that there are three ranges of wages with different optimal decisions. In the absence of the subsidy, the marginal benefit of search is given by:

$$H(w^*) = \frac{1}{r}\int_{w^*} (w - w^*) f(w)\, dw, \tag{1}$$

where r is the interest rate. As shown in Figure 1, marginal benefits decline with higher reservation wages since $\frac{\partial H(w^*)}{\partial w^*} = -\frac{1}{r}(1 - F(W^*)) < 0$ for all distributions. The marginal cost of full–time search is given by the sum of the out–of–pocket costs, c_{fts}, and the opportunity cost of not working, w^*. This is shown as the upward–sloping cost function, $c_{fts} + w^*$. Since the cost of searching while working, c_{ojs}, is independent of w^*, the cost of on–the–job search is shown as a horizontal line. These two cost functions determine two thresholds, w_1^* and w_2^*. The first threshold, w_1^*, separates the region of full–time search from on–the–job search. At this point, the costs of the two search methods are equal:

$$c_{ojs} = w_1^* + c_{fts}. \tag{2}$$

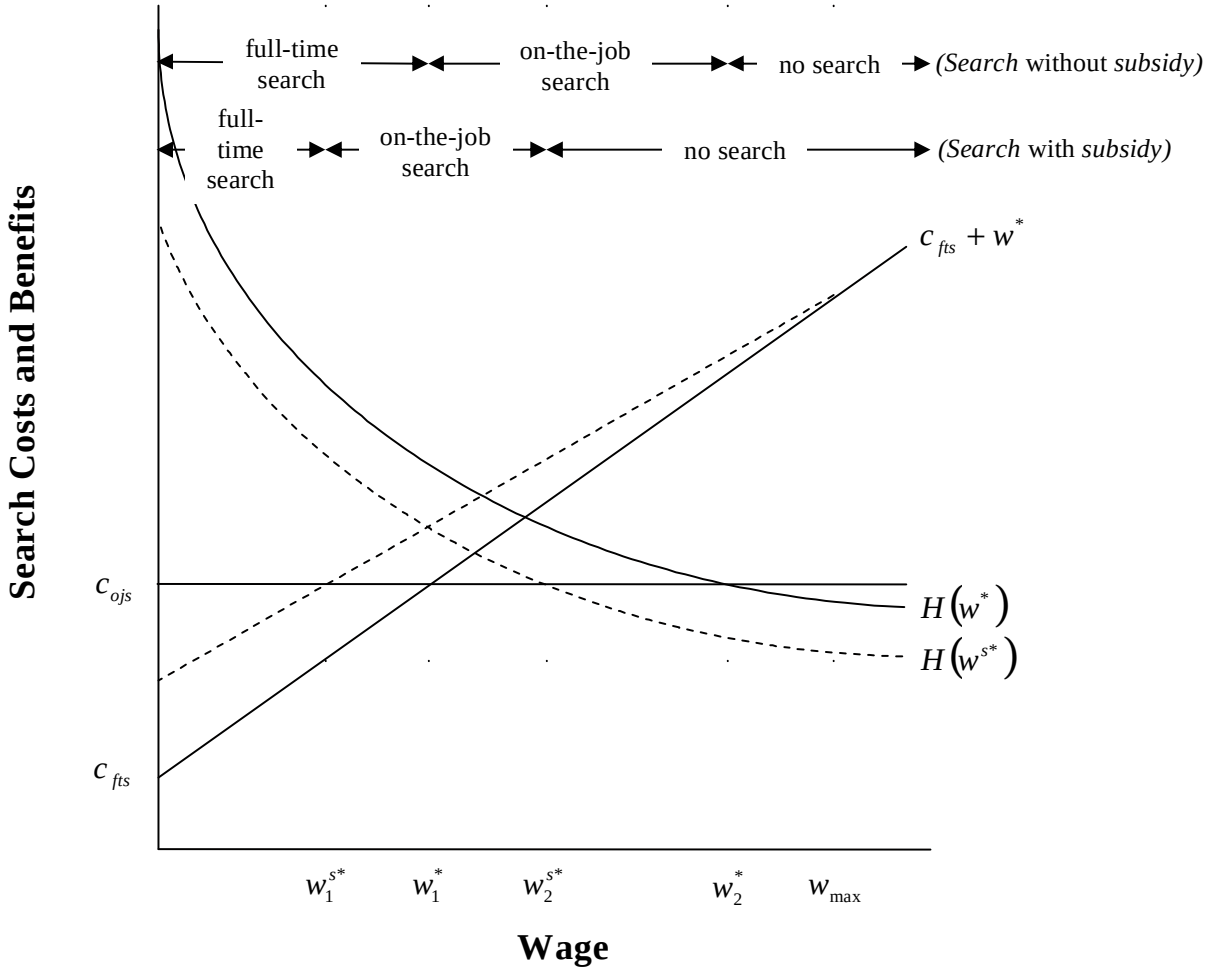
Full–time search is less costly than on–the–job search below this threshold. Above this threshold, the agent would search on the job rather than forego the offered wage.

The decision whether to continue searching depends on the marginal costs and benefits of search. Search stops when wage offers exceed w_2^*, which is the point where the marginal

[6]Implicitly these models assume that wages are constant in each job. While it would be possible to allow jobs to be described by slopes and intercepts, as was done in the previous section, this would add considerable complexity. Connolly and Gottschalk (2004b) analyze search over both slopes and intercepts using a dynamic programming framework that accounts for the probabilistic nature of leaving a job with wage growth. This model could be modified to analyze the effect of earnings subsidies, but at the cost of a substantial increase in analytical complexity without adding insight to the simple point we are making.

[7]If $c_{fts} > c_{ojs}$, then full-time search is never an optimal choice.

Figure 1: The Impact of a Wage Subsidy on Full-time and On-the-Job Search



costs and benefits of further on–the–job search are equal. This threshold is determined by the implicit expression:

$$c_{ojs} = \frac{1}{r} \int_{w_2^*} (w - w_2^*) f(w) \, dw. \tag{3}$$

In summary, Burdett shows that offers below w_1^* are refused and the agent continues to search full time, with a reservation wage of w_1^*. Wage offers between w_1^* and w_2^* are accepted, but the agent continues to search while on the job with a reservation wage equal to the current wage, w_c^*. Wage offers above w_2^* are accepted and the agent no longer searches.[8]

[8]Note that this model predicts that no agent would voluntarily quit to search full time unless the costs of search or the wage offer distribution changed. This, of course, does not mean that agents may not decide to voluntarily quit for other reasons. Inasmuch as the subsidy increases the opportunity costs of not working, it is expected to decrease these voluntary transitions to non–employment.

2.2.1 The Impact of a Wage Subsidy

Now consider the impact of introducing a wage subsidy on the decision to search full time or on the job.[9] Let $s(w)$ be the post–subsidy wage, which depends on the pre–subsidy wage, w. The cost of full–time search is increased since the opportunity cost of refusing an offer in order to continue full–time search now also includes the foregone subsidy that would have been received had the offer been accepted. This increase in the cost of full–time search is shown as an upward shift in the cost function for full-time search in Figure 1. Since on–the–job search does not involve an opportunity cost in terms of foregone wages, there is no change in this cost function. The increase in the cost of full–time search relative to on–the–job search reduces the threshold for searching on the job to w_1^{s*}, as illustrated in Figure 1. Thus, the earnings subsidy is predicted to increase the propensity to search while holding a job.[10]

Now consider the impact of the subsidy on the decision whether to search on the job or to stop searching. The benefits of search are decreased by the subsidy as long as the subsidy declines with wages, but is still positive at w_2^*.[11] The intuition for this result is that both the acceptable offers and the threshold are subsidized. However, since the threshold is lower than any acceptable wage, it receives a larger subsidy. As a result, the post–subsidy gain over the threshold is reduced. This reduction in the benefit of search is shown by the dashed benefit schedule in Figure 1. The decrease in the benefit of search lowers the threshold from w_2^* to w_2^{s*}. As a result, some offers which would have led to further on–the–job search are now accepted without further search.

A direct implication of the reduction in w_1^* and w_2^* is that wage subsidies are predicted to decrease job duration. Let the hazard of leaving a job paying w_c^* be given by $h(w_c^*) = \int_{w_c^*} f(w)\,dw$. Integrating over all possible jobs with on–the–job search (i.e., $w_1^* < w_c^* < w_2^*$) yields the hazard for a random person searching on–the–job: $h(w_1^*, w_2^*) = \frac{\int_{w_1^*}^{w_2^*} h(w_c^*) f(w_c^*) dw_c^*}{\int_{w_1^*}^{w_2^*} f(w_c^*) dw_c^*}$. It is straightforward to show that a decline in w_1^* and w_2^* increases $h(w_1^*, w_2^*)$ and, hence, decreases job duration. The intuition for this result is that persons who formerly searched while unemployed now search while holding low–paying jobs (i.e., those with $w_1^{s*} < w_c^* < w_1^*$).

[9] For analytical simplicity, we assume that the subsidy does not alter the wage offer distribution. However, if the subsidy is only available for full–time jobs, as is the case with the SSP, and if the wage offer distribution for these jobs is different than for part–time jobs, then this is yet another way in which the program may affect search.

[10] If the lower support of the wage offer distribution is set by the minimium wage, then w_1^{s*} can never drop below the minimum wage. If w_1^* is already at the minimum wage then the subsidy will not affect the reservation wage.

[11] The change in the benefit of search is given by:

$$\begin{aligned}
\Delta H(w^*) &= H(w^*) - H(w^{s*}) \\
&= \frac{1}{r}\int_{w^*} (w - w^*) f(w)\,dw - \frac{1}{r}\int_{w^*} (s(w) - s(w^*)) f(w)\,dw \\
&= \frac{1}{r}\int_{w^*} \{[w - s(w)] - [w^* - s(w^*)]\} f(w)\,dw < 0.
\end{aligned}$$

Both terms in brackets are negative, but if the subsidy declines with wages, the first term is smaller in absolute value than the second. Hence, the gains from search are reduced by the wage subsidy.

Given their low wages, they have higher hazards of leaving these jobs than persons with higher wages who searched on the job in the absence of the wage subsidy. At the other end of the wage spectrum, persons who would have searched on the job now no longer find it worthwhile to search (i.e., those with $w_2^{s*} < w_c^* < w_2^*$). Individuals with high wages have the lowest probability of finding jobs that led to a job exit. The wage subsidy, therefore, increases the number of workers with high hazards of job exits and reduces the number with low hazards. As a result, the subsidy is predicted to increase the mean hazard and, hence, decrease expected job duration.

While it is possible to sign the impact of the wage subsidy on the two relevant thresholds and job duration, the impact on between–job wage gain cannot be signed.[12] Therefore, earnings subsidies may either increase or decrease between–job wage gains depending on the wage offer distribution. Intuitively, the reduction in both w_1^* and w_2^* means that lower wages are accepted by persons searching on the job. This implies that on–the–job search occurs further down in the wage offer distribution. Some persons with low offers who would have searched while unemployed in the absence of a wage subsidy instead accept these offers and search on the job. In addition, some persons with higher offers who would have searched on the job in the absence of a wage subsidy now no longer search. The result is that persons searching on the job have lower average wages than in the absence of a subsidy. Since the current wage is the on–the–job reservation wage, this shift in the distribution of persons searching on the job lowers both the reservation wage and the mean acceptable offer. Whether the reservation wage or the mean acceptable offer decreases more depends on the form of the wage offer distribution.

In summary, we have shown that a wage subsidy is predicted to induce some persons to switch from full–time search to on–the–job search and some people to stop searching for a better job. This is the result of the subsidy increasing the opportunity cost of not accepting a wage which would make the person eligible for a subsidy and of the subsidy lowering the benefits of search. In addition we have shown that a wage subsidy is predicted to decrease job duration as persons initially accept jobs with low wages which are later dominated by better offers. While predictions can be made about the type of search and job duration, there is no general prediction about whether the subsidy will lead to larger or smaller mean change in wages between jobs. That remains an empirical issue.

2.3 Application to the Self-Sufficiency Project

While our analytical results are based on a generic earnings subsidy, we apply these to a specific demonstration project that was instituted in two Canadian provinces. Since our empirical work requires a certain familiarity with the basic structure of this experiment, we start with a brief description of this demonstration project.[13]

The Canadian Self-Sufficiency Project (SSP) was designed to determine the impact of instituting an earnings supplement for welfare recipients in order to reduce the reliance on long-term Income Assistance (IA). The hope was that the subsidy would not only lead to an increase in labor supply, but also to jobs with higher wages. The key attribute of the

[12] Proof that the impact of the subsidy on expected between–job wage change depends on the form of the wage offer distribution is available on request.

[13] For a full description of this program see Michalopoulos et al. (2002).

program is that it provides time–limited income supplements to individuals who work full time and do not collect IA. The supplement was offered to a random sample of single parents in New Brunswick and British Columbia, aged 19 or over, who had been on IA for at least 12 of the previous 13 months. In order to collect the supplement, individuals in the selected experimental group were required to work full time (at least 30 hours a week) in one or more jobs within 12 months of becoming eligible for the program.[14] The supplement was offered for a period of three years to each eligible individual and set so that most eligible families would find work to be financially preferable to continued receipt of IA. In addition to the supplement, program participants were provided with informational sessions on the rules and benefits of the supplement.[15]

Supplement payments were based on earnings and were 50 percent of the difference between the individual's monthly earnings and a target earnings level each pay period. In 1993, the target earnings for an individual in New Brunswick was $2,500 per month or $625 per week. A person working 30 hours per week at $6 per hour would, therefore earn $180 per week and receive a supplement of $222, which is the equivalent of subsidized wage of over $13 per hour. The target earnings in British Columbia was $3,083, which would result in a $295 subsidy for someone working 30 hours per week at the minimum wage of $6, which would result in an effective wage of close to $16. The subsidy would, therefore, more than double the individual's earnings.[16] She would, however, face a 50 percent benefit reduction against any increase in earnings, since the subsidy is equal to half the difference between actual earnings and the target earnings level.

Each eligible individual had 12 months from the time of eligibility to begin working full time and to start collecting SSP payments. These individuals could claim the supplement for a maximum of 36 consecutive months, starting the month they began collecting supplemental payments, but only during those months they were employed at least 30 hours. Those assigned to the SSP group could return to IA and/or cease working full time, but they could not collect the supplement during those months. They could resume receipt of the supplement in any month they work full time during the three–year period.

2.3.1 Comparison of the SSP Wage Subsidy to the IA Transfer System

Our application requires that we compare the job choice of experimentals who were eligible for the SSP wage supplement with the job choice made by controls who were only eligible for the IA income transfer. The fact that both controls and experimentals were eligible for income transfers adds a level of complexity.

We start by considering the impact of the SSP subsidy on the job choice of experimentals. Under the SSP supplement a wage, w, is supplemented by half the difference between the wage and the target wage, $w_{\max}$. The supplemented wage, $s(w)$, is given by:

[14] An eligible job is one that is covered by Unemployment Insurance and pays at least the minimum wage. In 1993, the minimum wage was $5 and $6 in New Brunswick and British Columbia, respectively. Employers are not informed of an individual's SSP status.

[15] While participants were also offered some limited auxiliary services, these constituted a very small part of the program.

[16] The supplement payments are treated as regular income for tax purposes and are not affected by unearned income or by the income of a spouse or partner.

$$s(w) = \left\{ \begin{array}{ll} = w + \left(\frac{w_{\max} - w}{2}\right) \quad = \frac{1}{2}(w + w_{\max}), & \text{if } w < w_{\max} \\ = w, & \text{if } w \geq w_{\max} \end{array} \right\}. \tag{4}$$

The supplemented wage is, therefore, a linear function of the unsupplemented wage, w, up to $w_{\max}$ and is equal to w above that threshold. Since few experimentals can expect to have wages rise above \$20 per hour, which is roughly the wage necessary to exceed the the earnings maximum for recipients working 30 hours a week, the subsidy is effectively linear. This implies that the SSP supplement does not affect the choice of jobs compared to an unsubsidized job.[17]

Since the control group is eligible for IA transfers, we must also consider the impact of this program on job choice. IA recipients can earn up to \$200 per month without a reduction in benefits. Earnings above this level are, however, subject to a 100 percent benefit reduction rate. This implies that post–IA earnings are a concave function of pre–transfer earnings. This discourages IA recipients from taking jobs with low initial wages but high wage growth. This is a direct result of the fact that the wage gains are fully taxed once monthly earnings rise above \$200.[18]

Our analytical framework predicts that the replacement of IA by SSP may induce experimentals to take jobs with wage growth that would be less attractive to controls who received IA. It should, however, be kept in mind that this is because SSP eliminates a disincentive in the IA program, not because SSP itself has a positive incentive. In other words, if SSP were made available to individuals who did not receive other transfers it would not be expected to affect job choice.

2.4 Behavioral and Compositional Effects

The preceding sections have developed the analytical basis for a behavioral response to wage subsidies that can alter job choice and job duration of working recipients. Subsidies may also alter the composition of workers, which can also result in observed differences between experimentals and controls. Since wage subsidies induce some individuals to start working, the subsidy may also change the composition of the working population. The new labor market entrants may face different demand conditions or they may have a different set of skills resulting in a different set of wage offer distributions or offers of jobs with different expected durations. These compositional changes could also lead to differences in observed outcomes.

The empirical results we present in the following sections, therefore, should be interpreted as the reduced form effects of the combined behavioral and compositional effects. While we do not attempt to model the change in composition of workers, we provide some auxiliary estimates for a subset of experimentals and controls who exhibited labor force attachment prior to the availability of the subsidy. Since this subset of experimentals and controls already

[17] An exception to this statement is for jobs whose expected duration exceeds the subsidized period of three years. It can be shown that for these jobs, the subsidy makes the job with wage growth more attractive.

[18] Consider a person who is offered a 5 hour per week job at \$10 an hour and another job with the same hours that pays \$9 in the first month and \$11 in the following month. In the constant wage job, earnings are \$200 in both 4–week months, so the individual receives full IA benefits. In the job with wage growth, earnings are \$220 in the second month, so IA benefits are reduced.

exhibited labor force attachment prior to SSP, results for these groups should be less affected by compositional effects.

3 Empirical Methodology

In our empirical work, we start by presenting descriptive tables on control/experimental differences in job duration and wage growth both within jobs and between jobs. Since wage growth reflects returns to tenure, returns to experience, and improved job match, we also estimate standard log wage equations in order to describe the factors underlying the observed differences in wage growth within and between jobs. For example, experimentals may have higher wage growth between jobs for two conceptually different reasons. Changing jobs may lead to larger increase in the job match component. Alternatively, experimentals may lose less in terms of foregone returns to job–specific tenure when switching jobs if they have lower returns to tenure.

This section describes the method used to estimate returns to tenure, experience, and job match, explicitly accounting for the potential endogeneity of the job match component. It is well known that tenure in the current job may be correlated with the unobserved job match component of the current match. (See Altonji and Williams (1997), and Topel (1991) among others.) Intuitively, a person with longer tenure on a job will have more to give up when moving to a new job since the person will lose the rewards to job–specific tenure obtained on the job. Therefore, these agents require a higher job match component in the new job in order to switch. Likewise, match quality improves as agents move to better jobs.

The following standard log wage model with person– and match–specific error components can be used to explain our method for dealing with this endogeneity:

$$\begin{array}{ll} Y_{ijt} & = \beta_x X_{ijt} + \beta_T T_{ijt} + \varepsilon_{ijt}, \quad \text{and} \\ \varepsilon_{ijt} & = \phi_{ij} + \mu_i + \upsilon_{ijt}, \end{array} \tag{5}$$

where X_{ijt} is accumulated labor market experience, T_{ijt} is tenure for person i in job j in period t, μ_i is a person–specific error component, and ϕ_{ij} is a job match–specific component. We follow Connolly and Gottschalk (2004a)'s modification of Topel (1991) to deal with the endogeneity of ϕ_{ij}.

Taking first differences of equation (5) eliminates μ_i, the person–specific error component, so that:

$$\begin{array}{lcl} \Delta Y_{ij} & = & \beta_x \Delta X_{ij} + \beta_T \Delta T_{ij} + \Delta\varepsilon_{ij}, \text{ and} \\ \Delta\varepsilon_{ij} & = & \Delta\phi_{ij} + \Delta\upsilon_{ij}. \end{array} \tag{6}$$

This framework can be used to analyze wage changes within jobs (where Δ is defined in terms of the difference between t and $t+1$) or wage changes between jobs (where Δ is defined in terms of the difference between the wage in job j at time t and and the wage in job j' at time t'). The issue is how to model $\Delta\phi_{ij}$, which is the source of the potential endogeneity.

Since the job match component is job–specific, $\Delta\phi_{ij} = 0$ while on the same job. Therefore, both the person–specific component, μ_i, and the match–specific component, ϕ_{ij}, are eliminated when estimating within–job wage growth. This fact is exploited by both Altonji and Williams (1997) and Topel (1991).

Between–job wage changes are more complicated since they involve a change in the job match component across jobs. As Connolly and Gottschalk (2004a) show, a standard Burdett (1978) search model, in which workers either search full–time or search on the job, has direct implications for estimation. First consider a person who searches while holding a job. In order for this person to leave her current job and move to another job, she must be compensated for the lost returns to tenure in her current job. This raises her reservation wage. As a result, the expected change in the match component is higher for a person who has longer tenure in her previous job. On the other hand, individuals who search while unemployed do not have the option of staying in the current job. For them, the reservation wage does not depend on the tenure in the previous job.

The preceeding argument implies the following linear approximations to the change in the match component:

$$\Delta\phi_{ij} = \left\{ \begin{array}{ll} 0, & \text{within jobs} \\ \alpha_1 + \alpha_T \widetilde{T}_{i,j-1} + \Delta\eta_{ij}, & \text{if } jj = 1 \\ \alpha_2 + \Delta\eta_{ij}, & \text{if } jj = 0 \end{array} \right\}, \tag{7}$$

where $jj = 1$ if the transition is directly job–to–job and $jj = 0$ if there is an intervening spell of non–employment. $\widetilde{T}_{i,j-1}$ is the total accumulated tenure in the previous job. The first line of equation (7) indicates that the match component does not change while on the same job. The next two lines indicate the between–job changes in the match component depend on tenure in the previous job for persons who search while on the job ($jj = 1$), but not those who do not have a job while searching ($jj = 0$).

Substituting the first line of equation (7) into equation (6) yields the standard within–job wage change equation ($\Delta\phi_{ij} = 0$ and $\Delta X_{ij} = \Delta T_{ij} = 1$):

$$\begin{array}{rl} \Delta Y_w & \equiv Y_{ijt} - Y_{i,j,t-1} \\ & = (\beta_X + \beta_T) + \Delta\upsilon_{ij}. \end{array} \tag{6a}$$

This equation, which is identical to Topel (1991)'s first stage estimator, shows that changes in wages within jobs reflect the sum of returns to experience and tenure. β_X and β_T are, however, not separately identified.

The change in starting wages across jobs identifies returns to tenure, experience, and the change in job match. Since tenure is equal to zero at the start of both jobs, $\Delta T_{ij} = 0$. The change in experience is equal to tenure on the previous job plus the one–unit gain in experience at the start of the new job. Therefore, $\Delta X_{ij} = \widetilde{T}_{i,j-1} + 1$, where $\widetilde{T}_{i,j-1}$ is the completed tenure in job $j-1$**.** Substituting the second and third lines in equation (7) into equation (6) yields the change in starting wages for persons who search on the job ($jj = 1$) and those who search while not employed ($jj = 0$):

$$\begin{array}{rll} \Delta Y_s & \equiv Y_{ij0} - Y_{i,j-1,0} & \\ & = (\alpha_1 + \beta_X) + (\alpha_T + \beta_X)\,\widetilde{T}_{i,j-1} + \big(\Delta\eta_{ij} + \Delta\upsilon_{ij}\big), & \text{if } jj = 1, \text{ and} \\ \Delta Y_s & = (\alpha_2 + \beta_X) + \beta_X \widetilde{T}_{i,j-1} + \big(\Delta\eta_{ij} + \Delta\upsilon_{ij}\big), & \text{if } jj = 0. \end{array} \tag{6b}$$

The change in starting wages for individuals with an intervening spell of non–employment ($jj = 0$) identifies β_X and α_2. With β_X identified, the within–job wage change (equation (6a))

identifies β_T. The remaining parameters, α_1 and α_T, are then identified from the change in starting wages of persons who move directly from one job to the next ($jj = 1$).

Equations (6a) and (6b) form our estimating equations. These equations are estimated separately for experimentals and controls.

4 Results

4.1 Data

The SSP data we use include wage and job histories for persons who were randomly assigned to the experimental and control groups. This randomization allows us to measure the differences in wage growth and turnover of the 2,827 individuals assigned to the IA control group and the 2,858 individuals assigned to the SSP experimental group.[19] The SSP data include the key variables necessary to identify when respondents change jobs, as well as the wage changes both while working for the same employer and when moving to a new employer.[20] Individuals were interviewed at three points during the project: at the time of random assignment (baseline); 18 months after random assignment; and 36 months after random assignment.[21] During these interviews, respondents were asked questions about their jobs and earnings histories. In the baseline survey, individuals were asked how long they had worked at a paid job or business since the age of 16. This provides a retrospective measure of previous experience that is not available in most other data sets and allows us to calculate labor market experience directly rather than having to rely on potential experience (age, minus education, minus six).[22] Data from our primary analysis file is used to measure the duration of jobs and to see whether these differ between controls and experimentals.

Starting and ending wages were not recorded until after the 18–month interview, so we are limited to the sub-sample of jobs that began after the 18th month of the survey when examining wage growth.[23] Jobs that satisfy this criterion are included in our wage sample. Since we are concerned about the generalizability of the results based on this sample, we explore whether this sample gives results similar to the full sample when the outcomes are available for both samples.[24] Specifically, we explore whether this sample restriction would

[19] We exclude the SSP-plus group, which received additional services.

[20] Since a substantial number of respondents held two or more jobs at the same time, we follow the primary job, which is defined as the job with the greatest number of hours worked in any given month.

[21] Data from the 54–month follow–up was not available when we undertook this project.

[22] To calculate experience, we take the baseline measure of monthly experience and increment by one for each month of observed employment. Tenure is measured similarly by counting the months since the respondent started working for the employer.

[23] We measure monthly within–job wage growth between the start of a job and the last observed month of the job.

[24] Card et al. (2000) uses the change in wages between the average wage in the 12th through 14th months of the experiment and the average wage in the 33rd to 35th months. Their measure of within-job wage growth is affected by an oddity in the way the data set was constructed. Wages in months 12 to 14 are obtained from the 18-month follow-up survey that reports the average wage in each job spell. (A job spell is an uninterrupted period working for the same employer.) This average wage is assigned to all observations in that spell. This eliminates all wage growth within a job spell. If a respondent was in the same job spell in the start and end periods used by Card et al. (2000), then this measure would report no wage growth since

have affected our conclusions about experimental/control differences in employment and job duration, which can be observed for the full sample.

As discussed earlier, differences between experimentals and controls may reflect compositional as well as behavioral effects of the SSP subsidy. In order to explore the importance of compositional effects, we also construct a sample of persons working at baseline, when the SSP supplement was not available. To the extent that SSP was not a factor inducing these individuals to work, experimental/control differences in this subsample do not reflect compositional effects. We compare the results for this sub–sample of persons working at baseline to results based on the broader sample to see if controlling for compositional effects in this way affects our conclusions.

4.2 Summary Statistics

Tables 1a and 1b present summary statistics for the full sample and for the wage sample.[25] Table 1a shows the characteristics of the 5,685 individuals in our full sample used to examine employment and job duration. Among the 2,858 experimentals, 1,888, or roughly two–thirds did not take up the subsidy.[26] As can be seen in columns 2 and 3, the control and combined experimental groups (including those who took up the program and those who did not) closely resemble one another. For both groups, the women average just under 32 years of age and roughly 14 percent speak French. Almost 50 percent have never married, and they have an average of 1.7 children. Both experimentals and controls have low education, with roughly 55 percent having less than a high school degree.

Both groups have substantial previous labor market experience, but few were working at the baseline interview. Roughly 5 percent had no previous work experience and the average months of previous experience is just under 90 months. This indicates that these females already had more than seven years of experience at the baseline. However, only 19 percent were working at the baseline interview and almost two–thirds of these were working part–time. Thus, both controls and experimentals were only marginally attached to the labor market at the baseline interview.

The summary statistics in Table 1a indicate that the experimental and control groups in our primary sample are similar on the basis of observed demographic and labor market characteristics. Table 1b shows the characteristics of the 2,444 respondents in our wage sample. Not surprisingly, this sample was somewhat more likely to have been working at the baseline interview and was considerably less likely to have no work experience or not to be looking for work at the baseline. These differences, however, affect controls as well as experimentals. As a result, the experimental/control differences in baseline characteristics

the wage in both periods would be the same average wage in the spell.

[25] The "wage sample" refers to that subsample of jobs for which there are both starting and ending wages available.

[26] Among those assigned to the experimental group, roughly two-thirds did not receive a subsidy because they either did not work full time within the first 12 months or they qualified but did not apply. Looking at the "no take up" group, however, shows that only four percent of those who were eligible yet did not take up the program worked full-time during the first twelve months. Therefore, the dominant reason for not taking up the program is not qualifying, rather than qualifying but failing to apply for the program. The top three reasons for not taking up the program were that: the respondent could not find a job (32.7 percent); personal responsibilites interfered (15.2 percent); and health problems precluded full–time employment (14.0 percent).

Table 1a: Summary Statistics at Baseline Interview, *Full Sample*

	All	Control (IA)	Experimental		
			SSP Eligible	Took Up SSP	Did Not Take Up SSP
	(1)	(2)	(3)	(4)	(5)
Demographic					
Age	31.9	31.9	31.9	31.0	32.4
	(0.11)	(0.15)	(0.15)	(0.25)	(0.20)
Speaks English	0.97	0.97	0.97	0.99	0.96
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Speaks French	0.13	0.14	0.13	0.15	0.12
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Never Married	0.48	0.48	0.48	0.48	0.48
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Number of Kids	1.68	1.68	1.67	1.61	1.71
	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)
Less Than High School	0.55	0.55	0.54	0.43	0.60
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
High School Graduate	0.45	0.45	0.46	0.57	0.40
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Employment					
Initial Experience (months)	88.92	89.93	87.91	102.42	80.50
	(1.05)	(1.48)	(1.48)	(2.58)	(1.79)
Employed	0.19	0.19	0.18	0.32	0.11
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Employed Full Time	0.07	0.07	0.06	0.13	0.02
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
Employed Part Time	0.12	0.12	0.12	0.19	0.09
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
No Previous Work Experience	0.05	0.06	0.05	0.01	0.07
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Not Employed and Not Looking	0.58	0.57	0.59	0.41	0.68
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
# Individuals	5,685	2,827	2,858	970	1,888

NOTE: Standard errors in parentheses.

Table 1b: Summary Statistics at Baseline Interview, *Wage Sample*

	All	Control (IA)	Experimental		
			SSP Eligible	Took Up SSP	Did Not Take Up SSP
	(1)	(2)	(3)	(4)	(5)
Demographic					
Age	30.8	30.8	30.8	30.8	30.9
	(0.15)	(0.23)	(0.21)	(0.27)	(0.33)
Speaks English	0.99	0.99	0.99	0.99	0.99
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Speaks French	0.15	0.16	0.15	0.16	0.13
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Never Married	0.50	0.50	0.51	0.50	0.51
	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)
Number of Kids	1.60	1.58	1.62	1.59	1.67
	(0.02)	(0.02)	(0.02)	(0.03)	(0.04)
Less Than High School	0.44	0.42	0.45	0.42	0.49
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
High School Graduate	0.56	0.58	0.55	0.58	0.51
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Employment					
Initial Experience (months)	96.62	98.86	94.76	101.03	85.89
	(1.53)	(2.28)	(2.07)	(2.81)	(2.99)
Employed	0.30	0.33	0.28	0.34	0.20
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Employed Full Time	0.11	0.12	0.10	0.14	0.05
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Employed Part Time	0.19	0.21	0.18	0.20	0.15
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
No Previous Work Experience	0.02	0.02	0.02	0.01	0.03
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Not Employed and Not Looking	0.44	0.41	0.47	0.41	0.55
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
# Individuals	2,444	1,103	1,341	788	553

NOTE: Standard errors in parentheses.

are similar in the wage sample and the full sample. The largest difference is in those who are not working or looking for work at the baseline interview. For controls, 41 percent are not in the labor market; 47 percent of experimentals are not attached. For all other measures the differences between experimentals and controls are small and of roughly the same magnitude as in the full sample.

While experimentals resemble controls, there are striking differences between those experimentals who took up the program and those who did not. Tables 1a and 1b show that experimentals who took up the program (column 4), had substantially more attachment to the labor market than those who did not take up the program (column 5). In our wage sample (Table 1b), those who took up the program had 101 months of prior work experience at the baseline survey compared to 86 months for those experimentals who did not take up the program. Likewise, the proportion working at the baseline is 34 percent for the takeup group, but only 20 percent for those who did not take up the benefits in spite of being eligible. This is comparable to the full sample shown in Table 1a. These data clearly indicate that the decision to take up the program was not random; it is consistent with persons who are more likely to gain from the program also being more likely to take advantage of the program.

While the contrast between the experimentals who took up the program and the controls does not yield an unbiased estimate of the impact of the program on a random individual, it does answer another interesting question: what is the expected impact of the subsidy on persons who would decide to participate in a program if it were made available?

We, therefore, provide two contrasts when comparing experimentals and controls. The first is the mean difference between controls and all persons assigned to the experimental group, including experimentals who did not receive a supplement at any point. Contrast between the controls and all experimentals, including the group that did not take up the subsidy, gives the average treatment effect, where the treatment is interpreted as making a supplement available to a random group of welfare recipients. The average treatment effect is, therefore, a mixture of the effect on those who took up the supplement and those who did not.

The second contrast is between the controls and those experimentals who participated in the program, as evidenced by receiving the supplement. Since the decision to take up the subsidy is likely to be influenced by the expected benefits of the program, those who took up the program are likely to have higher expected gains from the program than a randomly chosen person who is offered the program. In terms used in the evaluation literature, comparing outcomes of controls with outcomes of experimentals who took up the program yields the impact of the treatment on the treated.[27]

[27] The standard distinction can be made in terms of the OLS framework. Let $\Delta w_i = \alpha_i T_i + \epsilon_i$ be the impact of the treatment on the change in wages of person i and $T_i^* = g(X_i) + \nu_i$ be the latent treatment. Assume $E(\epsilon_i\ \nu_i) = 0$. If T_i is a random treatment, then this also ensures $E(\nu_i\ \alpha_i) = 0$, so $E(\hat{\alpha}_{ols}) = E(\alpha_i)$, which is the average treatment effect. However, if agents choose T_i on the basis of α_i, then $E(\nu_i\ \alpha_i\ |T_i) \neq 0$. In that case, $E(\hat{\alpha}_{ols}) = E(\alpha|\alpha > \alpha^*)$, where α^* is determined by $Pr(\nu_i > -g(X_i))$. In this case of self–selection, OLS yields the conditional mean of α_i, which is known as the effect of the treatment on the treated (i.e., those with $\alpha_i > \alpha^*$). Note that if $E(\epsilon_i|T_i) \neq E(\epsilon_i)$, then OLS will not yield the effect of the treatment of the treated. This endogeneity is, however, conceptually different from the self-selection of those with the most to gain from the treatment (i.e., $\alpha_i > \alpha^*$). Note that in our case, where the dependent variable is the change in wage, all person–specific, time–invariant unobservables are already differentiated out.

It should be noted that Card et al. (2000) use a different contrast. The authors ask whether those who worked full time after the 12–month qualifying period, but who would not have worked full time in the absence of the program, are different from the controls. Given this question, the treated are only those experimentals induced to increase their hours as a result of the program. This is a subset of those who actually took up the program.[28] Since the decision to work full time is also endogenous, the treatment effect found in Card et al. (2000) should also be interpreted as the effect of the treatment on the treated, but now the treatment is more narrow than just taking up the program. Any effect of the program on those who would have worked full time in absence of the program is not included in the effect of the treatment on the treated. These two papers are, therefore, estimating two different treatment effects.[29]

4.3 Differences in Employment

We start by confirming that our full sample and our wage sample show the same positive employment effects of earnings subsidies as found in previous studies. Tables 2a and 2b show the number of months worked after the baseline survey for members of the full sample and the wage sample, respectively. These summary statistics are shown for controls and experimentals (who are further disaggregated into those who took up the program and those who did not). These data show that both samples are consistent with previous studies that find that the SSP supplement increased the probability that former welfare recipients would take full–time jobs. The top panel of Table 2a shows that for control group members in the full sample, the average number of months of work through the 36–month follow–up period is 10.6 months. The following two panels show that roughly half of this is full–time work (5.6 months). Experimentals, however, worked substantially more and the difference largely reflects an increase in full–time work. For the experimental group, the average number of months worked is 12.9 months (versus 10.6) and roughly two–thirds of that is full–time work (8.6 months). This indicates that the program increased the average number of months worked by 2.3 months and the number of months of full–time work by 3.0 months. Not unexpectedly, experimentals who took up the program show substantially larger increases in work.

Table 2b shows that experimentals in the wage sample worked 1.7 months more than controls and that the average number of months of full–time work was 4.6 months higher for experimentals than controls. In spite of having smaller sample sizes in the wage sample than in the full sample, we can still reject the null hypothesis of no increase in overall employment and no increase in full–time employment at conventional levels. These differences indicate that limiting the sample to the subset of jobs that can be used to study wage growth does not obscure the employment–increasing impact of the program.

[28] It is, however, unclear whether they include those who did not take up the program in the potential pool of persons who may have been "incentivised".

[29] While our division between those who took up the program and those who did not is observable, their division between the "incentivised" group and those who would have worked in the absence of the program is not observable. This requires an identity assumption. The assumption they make is that the wage growth of those who would have worked full time is not affected by the program. Our analytical model suggests that this group could also have been affected.

Table 2a: Observed Employment, *Full Sample*

	All	Control (IA)	Experimental		
			SSP Eligible	Took Up SSP	Did Not Take Up SSP
	(1)	(2)	(3)	(4)	(5)
All Jobs					
Average Employment (months)	11.78	10.62	12.92	24.90	6.77
	(0.18)	(0.25)	(0.26)	(0.36)	(0.25)
Average Number of Jobs per Individual	1.07	0.96	1.18	2.11	0.70
	(0.02)	(0.02)	(0.02)	(0.04)	(0.02)
Average Number of Jobs per Worker	1.78	1.71	1.84	2.14	1.52
	(0.02)	(0.02)	(0.03)	(0.04)	(0.03)
Full-time Jobs					
Average Employment (months)	7.11	5.58	8.62	20.50	2.51
	(0.15)	(0.19)	(0.23)	(0.38)	(0.14)
Average Number of Jobs per Individual	0.62	0.50	0.73	1.57	0.30
	(0.01)	(0.02)	(0.02)	(0.04)	(0.01)
Average Number of Jobs per Worker	1.56	1.48	1.62	1.79	1.28
	(0.02)	(0.03)	(0.03)	(0.04)	(0.03)
Part-time Jobs					
Average Employment (months)	4.67	5.04	4.30	4.40	4.25
	(0.12)	(0.18)	(0.16)	(0.27)	(0.21)
Average Number of Jobs per Individual	0.45	0.45	0.45	0.54	0.40
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Average Number of Jobs per Worker	1.31	1.30	1.32	1.35	1.30
	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)
# Individuals	5,685	2,827	2,858	970	1,888
# Jobs	6,079	2,705	3,374	2,045	1,329
# Full-time Jobs	3,514	1,425	2,089	1,522	567
# Part-time Jobs	2,565	1,280	1,285	523	762

NOTE: Standard errors in parentheses.

Table 2b: Observed Employment, *Wage Sample*

	All	Control (IA)	Experimental		
			SSP Eligible	Took Up SSP	Did Not Take Up SSP
	(1)	(2)	(3)	(4)	(5)
All Jobs					
Average Employment (months)	22.46	21.52	23.23	27.85	16.64
	(0.24)	(0.36)	(0.31)	(0.32)	(0.48)
Average Number of Jobs per Individual	1.93	1.85	2.00	2.26	1.63
	(0.02)	(0.03)	(0.03)	(0.04)	(0.04)
Average Number of Jobs per Worker	1.94	1.85	2.01	2.26	1.64
	(0.02)	(0.03)	(0.03)	(0.04)	(0.04)
Full-time Jobs					
Average Employment (months)	14.10	11.57	16.18	23.09	6.33
	(0.26)	(0.37)	(0.35)	(0.39)	(0.36)
Average Number of Jobs per Individual	1.16	1.00	1.29	1.68	0.73
	(0.02)	(0.03)	(0.03)	(0.04)	(0.03)
Average Number of Jobs per Worker	1.65	1.56	1.72	1.88	1.34
	(0.02)	(0.03)	(0.03)	(0.04)	(0.04)
Part-time Jobs					
Average Employment (months)	8.36	9.96	7.05	4.77	10.31
	(0.23)	(0.36)	(0.28)	(0.30)	(0.50)
Average Number of Jobs per Individual	0.77	0.85	0.71	0.58	0.90
	(0.02)	(0.02)	(0.02)	(0.03)	(0.04)
Average Number of Jobs per Worker	1.35	1.35	1.36	1.36	1.36
	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
# Individuals	2,444	1,103	1,341	788	553
# Jobs	4,723	2,039	2,684	1,780	904
# Full-time Jobs	2,829	1,103	1,726	1,322	404
# Part-time Jobs	1,894	936	958	458	500

NOTE: Standard errors in parentheses.

4.4 Differences in Job Duration

Our analytical model predicts that experimentals are more likely to search on the job than are controls and that they will change jobs more often. The former is a direct consequence of the fact that the opportunity cost of searching while not employed is higher for experimentals than for controls since experimentals forego the earnings subsidy when they search while not employed. The result of accepting some lower–paying jobs in order to search while working leads to a higher probability that the current wage will be dominated by a new wage offer. This would lead to shorter job duration.[30]

Cox proportional hazard models of the competing risk of exiting the current job to move directly to another job or exiting to non–employment are shown in Tables 3a and 3b for the full sample and the wage sample, respectively. In both tables, columns 5 through 8 present coefficient estimates for exits directly to other jobs. Column 5 of Table 3a shows that the odds ratio of leaving a job to move directly to another job is 22 percent higher for experimentals than controls and this difference is significant at conventional levels. When demographic controls are added, the coefficient increases slightly and remains highly significant. When the contrast is between controls and experimentals who took up the program (columns 7 and 8), the coefficients nearly double to 0.42 indicating that the experimentals who took up the program were much more likely than controls to leave their current jobs to take another job. These four columns, therefore, strongly support the hypothesis that workers who receive SSP have higher job turnover than IA recipients.

An increase in job changes is consistent with the behavioral predictions provided earlier. It should, however, be kept in mind that these experimental/control differences might not be due to a behavioral change, but to a compositional change in which persons who started working because of the SSP subsidy had shorter job durations. To investigate this possibility, we restrict the sample to only those individuals who were working at the time of the baseline survey. The same basic patterns hold for this restricted sample. (See Appendix B.) Since these individuals already had a commitment to work before the SSP subsidy became available, it is less likely that these results reflect compositional changes.

While the Burdett (1978) model of on–the–job search implies that individuals will not quit voluntarily to search while unemployed, voluntary quits may occur for other reasons, such as increased family obligations or geographic relocation. The availability of a subsidy will, however, increase the foregone earnings if the person quits, which implies that experimentals are less likely to make such transitions. Columns 9 to 12 of Table 3a offer support for this prediction. The hazard of exiting to non–employment is lower for experimentals than controls in all four columns, although the differences are not significant at conventional levels in column 9.

Finally, Table 3b shows that experimentals in our wage sample also have higher hazards of making a job–to–job transitions. The point estimates are somewhat smaller than for the full sample, but the smaller sample size lowers the precision of these estimates. As a result, the coefficients in column 10 as well as in column 9 are not significant, although the differ-

[30] An indirect implication is that experimentals will accept lower wages than controls in their first jobs after the program is started. While we do not have starting wages for jobs that ended before the 18th month, we do have the mean wage in the first job. Mean wages in the first jobs of experimentals were 13 percent lower than for controls, which is a statistically significant difference.

Table 3a: Cox Proportional Hazard Models of Job Duration, *Full Sample*

	All Job Exits				Job to Job Exits				Exits to Unemployment			
	Control v. SSP		Control v. Take Up		Control v. SSP		Control v. Take Up		Control v. SSP		Control v. Take Up	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
In SSP Group	0.018	0.018	-0.014	-0.015	0.218 ***	0.233 ***	0.415 ***	0.417 ***	-0.055	-0.059 *	-0.198 ***	-0.200 ***
	(0.029)	(0.029)	(0.033)	(0.033)	(0.055)	(0.055)	(0.059)	(0.059)	(0.035)	(0.035)	(0.041)	(0.041)
Female	———	0.071	———	0.039	———	0.055	———	-0.007	———	0.090	———	0.067
	———	(0.078)	———	(0.087)	———	(0.145)	———	(0.153)	———	(0.094)	———	(0.107)
Speaks French	———	0.063	———	0.070	———	0.183 ***	———	0.157 **	———	0.019	———	0.031
	———	(0.040)	———	(0.044)	———	(0.071)	———	(0.076)	———	(0.048)	———	(0.054)
Never Married	———	0.105 ***	———	0.110 ***	———	0.013	———	0.013	———	0.133 ***	———	0.153 ***
	———	(0.030)	———	(0.034)	———	(0.057)	———	(0.061)	———	(0.037)	———	(0.042)
Number of Children	———	-0.014	———	-0.011	———	-0.108 ***	———	-0.085 **	———	0.009	———	0.009
	———	(0.019)	———	(0.022)	———	(0.037)	———	(0.041)	———	(0.023)	———	(0.027)
High School Graduate	———	-0.020	———	-0.039	———	0.189 ***	———	0.168 ***	———	-0.110 ***	———	-0.135 ***
	———	(0.029)	———	(0.033)	———	(0.056)	———	(0.061)	———	(0.035)	———	(0.041)
# Exits	4,798		3,740		1,374		1,169		3,300		2,489	

NOTES:
(1) Standard errors in parentheses.
(2) Coefficient estimates are significant at the 10%(*), 5%(**), or 1%(***) levels.

Table 3b: Cox Proportional Hazard Models of Job Duration, *Wage Sample*

	All Job Exits				Job to Job Exits				Exits to Unemployment			
	Control v. SSP		Control v. Take Up		Control v. SSP		Control v. Take Up		Control v. SSP		Control v. Take Up	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
In SSP Group	0.024	0.023	0.006	0.003	0.210 ***	0.222 ***	0.369 ***	0.373 ***	-0.053	-0.059	-0.173 ***	-0.180 ***
	(0.034)	(0.034)	(0.037)	(0.038)	(0.061)	(0.061)	(0.064)	(0.064)	(0.042)	(0.042)	(0.048)	(0.048)
Female	———	0.020	———	0.013	———	0.031	———	0.007	———	0.037	———	0.040
	———	(0.095)	———	(0.107)	———	(0.166)	———	(0.179)	———	(0.119)	———	(0.137)
Speaks French	———	0.055	———	0.091 *	———	0.164 **	———	0.173 **	———	0.005	———	0.043
	———	(0.046)	———	(0.050)	———	(0.078)	———	(0.081)	———	(0.058)	———	(0.064)
Never Married	———	0.095 ***	———	0.105 ***	———	-0.008	———	-0.002	———	0.128 ***	———	0.149 ***
	———	(0.036)	———	(0.040)	———	(0.063)	———	(0.066)	———	(0.044)	———	(0.050)
Number of Children	———	-0.015	———	-0.023	———	-0.098 **	———	-0.093 **	———	0.006	———	-0.003
	———	(0.023)	———	(0.026)	———	(0.042)	———	(0.045)	———	(0.028)	———	(0.032)
High School Graduate	———	0.013	———	-0.004	———	0.129 **	———	0.098	———	-0.057	———	-0.074
	———	(0.035)	———	(0.039)	———	(0.062)	———	(0.066)	———	(0.043)	———	(0.048)
# Exits	3,511		2,862		1,142		1,005		2,294		1,803	

NOTES:
(1) Standard errors in parentheses.
(2) Coefficient estimates are significant at the 10%(*), 5%(**), or 1%(***) levels.

ences between controls and experimentals transitioning through unemployment (columns 11 and 12) and the differences for all job–to–job exits continue to be significant (columns 5 through 8).

4.5 Differences in Wage Growth

In Section 2.1, we show that the behavioral effect on within–job wage growth depends on the functional form of the wage subsidy or transfer system. If a person is indifferent between two jobs in the absence of transfers, then she would prefer the job with lower wage growth if the post–transfer wage is a concave function of the pre–subsidy wage, but she would be indifferent if the subsidy were linear. While the SSP subsidy is linear in its effective range, the IA transfer system strongly discourages taking jobs with high wage growth since the wage gains are subject to a 100 percent marginal tax rate above monthly earnings of $200. This implies that experimentals who are eligible for the SSP subsidy, would have higher within–job wage growth than the controls, who are only eligible for the IA transfer. The SSP subsidy may also affect between–job wage growth, though the effect of the subsidy depends on the functional form of the wage offer distribution. Since we have no priors on the functional form of this distribution, the effect of the subsidy on between–job wage growth remains an empirical question. In addition to these behavioral factors, the SSP subsidy may affect within– and between–job wage growth by changing the composition of workers.

4.5.1 Differences in Means

Table 4a shows mean within– and between–job wage growth for our wage sample. In order to reduce the compositional effects of the subsidy, Table 4b restricts the sample to experimentals and controls who were working at the baseline interview. Since the group of individuals experiencing an intervening spell of non–employment between jobs includes persons who quit or were involuntarily terminated, we separate between–job wage growth into direct transitions from one job to the next and transitions with an intervening spell of non–employment.

The top panel of each table shows monthly within–job wage growth. The point estimate of the mean monthly wage growth for experimentals in Table 4a is .0031, which is considerably higher than the .0013 for controls. Similar patterns are shown in Table 4b, which includes only persons working at the baseline interview. Within–job wage growth is .0020 for experimentals and .0014 for control group members who had already demonstrated the willingness to work before the program was instituted. This is consistent with our prediction that the SSP wage subsidy does not discourage participants from finding jobs with wage growth while the IA transfer system has strong disincentives to accepting these jobs.

While the analytical model does not have strong predictions on the effects of the earnings subsidy on between–job wage growth, the bottom panel of Tables 4a and 4b indicate that the experimentals have substantially larger wage growth when transitioning directly from one job to the next than do the controls (.050 versus .012 in Table 4a and .092 versus .024 in Table 4b). Not surprisingly, experimentals have smaller growth in wages than controls when there is an intervening spell of non–employment since they forego the subsidy as well as the wage while unemployed. The fact that Table 4b gives results similar to Table 4a indicates that

Table 4a: Mean Monthly Log Wage Growth, *Wage Sample*

	All	Control (IA)	Experimental		
			SSP Eligible	Took up SSP	Did Not Take Up SSP
	(1)	(2)	(3)	(4)	(5)
Within-job Wage Changes					
Within-job Wage Change	0.0022	0.0013	0.0031	0.0031	0.0030
	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)
Between-job Wage Changes					
All Between-job Wage Change	0.0215	0.0199	0.0227	0.0240	0.0193
	(0.009)	(0.014)	(0.012)	(0.012)	(0.027)
Job-to-Job Exits	0.0342	0.0121	0.0504	0.0562	0.0303
	(0.013)	(0.022)	(0.017)	(0.020)	(0.034)
Intervening Spell of Non-employment	0.0116	0.0256	0.0005	-0.0053	0.0129
	(0.013)	(0.019)	(0.017)	(0.018)	(0.039)

NOTE: Standard errors in parentheses.

Table 4b: Mean Monthly Log Wage Growth, *Wage Sample, employed at baseline interview*

	All	Control (IA)	Experimental		
			SSP Eligible	Took up SSP	Did Not Take Up SSP
	(1)	(2)	(3)	(4)	(5)
Within-job Wage Changes					
Within-job Wage Change	0.0017	0.0014	0.0020	0.0034	-0.0010
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Between-job Wage Changes					
All Between-job Wage Change	0.0205	0.0061	0.0346	0.0397	0.0126
	(0.018)	(0.021)	(0.030)	(0.030)	(0.093)
Job-to-Job Exits	0.0639	0.0244	0.0925	0.0921	0.0957
	(0.025)	(0.029)	(0.038)	(0.041)	(0.084)
Intervening Spell of Non-employment	-0.0221	-0.0071	-0.0417	-0.0470	-0.0289
	(0.026)	(0.029)	(0.046)	(0.034)	(0.132)

NOTE: Standard errors in parentheses.

these patterns are still present when focusing on a subsample for whom the compositional effects are expected to be small.

4.5.2 Differences in Returns to Tenure, Experience, and Job Match

In this section we use the methodology developed earlier to estimate returns to experience (β_x), tenure (β_T), and the coefficients in the job match equations (α_1, α_2 and α_T) for experimentals and controls. Using this framework requires additional assumptions, but has the offsetting benefit of allowing us to interpret the experimental findings in the context of standard wage equations. For example, the larger between–job wage changes for experimentals than controls shown in Tables 4a and 4b does not necessarily indicate that experimentals are finding better matches than controls when moving to new jobs. Since experimentals have higher hazards of making a job transition, they have accumulated less tenure by the time they change jobs. As a result they lose less in foregone returns to job–specific tenure. Therefore, the larger between–job wage growth of experimentals may partially reflect this factor. Put another way, the unconditional means of between–job wage changes shown in Tables 4a and 4b do not hold tenure constant.

Tables 5a and 5b present the estimated parameters of equations (6a) and (6b) for our wage sample and for the sample of persons working at baseline, respectively.[31] Columns 1 and 2 of each table show estimated coefficients for experimentals and controls combined. Columns 3a, 4a, 5a, and 6a show the coefficients for controls while columns 3b, 4b, 5b, and 6b present differences in coefficients for experimentals and controls.[32] F statistics are shown at the bottom of each column. These provide joint tests of the hypotheses that experimentals and controls have the same coefficients on: (1) all variables; (2) tenure, experience, and experience squared (β_T, β_X, and β_{X^2}); (3) the job match coefficients for those moving directly from one job to the next (α_1 and α_T); and (4) the full set of job match coefficients (α_1, α_2, and α_T).[33]

Column 2, which combines experimentals and controls, indicates that while wages grow with tenure,they do not depend on experience. Wages also increase when workers make transitions to new jobs. When the transition includes an intervening spell of non–employment, the improvement in job match leads to a 4.2 percent increase in wages. Direct job–to–job transitions result in a 2.2 percent wage gain plus an additional .3 percent gain for each month of previous tenure.[34] The latter is consistent with the theory that workers will only accept jobs that have a sufficiently high job match component to compensate for the lost returns to tenure in the previous job.

Column 4a presents coefficients in equations (6a) and (6b) for controls while column 4b presents the differences in coefficients for those eligible for the SSP subsidy. The coefficients on tenure and experience in column 4a indicate that control group members do not

[31] All regressions include a set of control variables. These include gender, Canadian native, French-speaking, residing in British Columbia, high school graduate at baseline, mother is a high school graduate, not working or looking for work at baseline, change in part time status, and year dummies.

[32] These are coefficents on the interaction of an SSP dummy with the variables in equations (6a) and (6b).

[33] While we test for the joint significance of all job match coefficients as well as the joint significance of the job match coefficients for transitions directly to other jobs (α_1 and α_T), these statistics are not reported in the table.

[34] The F statistic indicates that these two coefficients are jointly significant.

Table 5a: Parameter Estimates, *Wage Sample*

	All		Control v. SSP				Control v. Take Up			
			No Covariates		With Covariates		No Covariates		With Covariates	
	No Covariates	With Covariates	Coefficients	Interaction with SSP	Coefficients	Interaction with SSP	Coefficients	Interaction with Take Up	Coefficients	Interaction with Take Up
	(1)	(2)	(3)		(4)		(5)		(6)	
Change Equation										
Tenure x 10 ^{2}	0.251 **	0.268 **	0.032	0.488 **	0.061	0.430 *	0.032	0.621 **	0.077	0.579 *
	(0.103)	(0.106)	(0.113)	(0.221)	(0.115)	(0.227)	(0.113)	(0.286)	(0.112)	(0.307)
Experience x 10^{2}	-0.134	-0.154	0.001	-0.326	0.004	-0.332	0.001	-0.513	0.017	-0.531
	(0.127)	(0.133)	(0.122)	(0.263)	(0.126)	(0.271)	(0.122)	(0.333)	(0.126)	(0.354)
Experience^{2} x 10 ^{4}	0.027	0.019	0.019	0.015	0.001	0.031	0.019	0.068	0.000	0.086
	(0.025)	(0.026)	(0.019)	(0.053)	(0.019)	(0.054)	(0.019)	(0.072)	(0.019)	(0.074)
Job Match Equation										
Job-to-Unemployment-to-Job Transition	0.037 *	0.042 **	0.046	-0.012	0.060 *	-0.029	0.046	-0.009	0.065 **	-0.021
	(0.019)	(0.020)	(0.030)	(0.040)	(0.031)	(0.042)	(0.030)	(0.042)	(0.031)	(0.044)
Job-to-Job Transition	0.032	0.022	0.007	0.042	-0.003	0.038	0.007	0.053	0.003	0.057
	(0.022)	(0.023)	(0.035)	(0.044)	(0.040)	(0.048)	(0.035)	(0.046)	(0.040)	(0.050)
Previous Tenure After Job-to-Job Transition	0.280	0.343	0.030	0.623	0.078	0.671	0.030	0.753	0.063	0.711
	(0.203)	(0.222)	(0.209)	(0.389)	(0.218)	(0.428)	(0.209)	(0.486)	(0.216)	(0.511)
F(All Interactions)	-------	-------	-------	2.19	-------	1.89	-------	2.91	-------	2.61
Probability	-------	-------	-------	0.03	-------	0.06	-------	0.00	-------	0.01
F(Tenure and Experience)	-------	-------	-------	3.90	-------	2.90	-------	5.29	-------	4.54
Probability	-------	-------	-------	0.01	-------	0.03	-------	0.00	-------	0.00
F(Job-to-Job Interactions)	-------	-------	-------	3.25	-------	2.78	-------	3.28	-------	2.74
Probability	-------	-------	-------	0.04	-------	0.06	-------	0.04	-------	0.06
F(Job Match Interactions)	-------	-------	-------	2.32	-------	2.19	-------	2.30	-------	2.05
Probability	-------	-------	-------	0.07	-------	0.09	-------	0.08	-------	0.11
# Individuals	1,496	1362.00	-------	1,496	-------	1,362	-------	1,120	-------	1,018
# Observations	2,426	2189.00	-------	2,426	-------	2,189	-------	1,884	-------	1,690
R^2	0.027	0.04	-------	0.040	-------	0.052	-------	0.056	-------	0.070

NOTES:

(1) Standard errors in parentheses.

(2) Coefficient estimates are significant at the 10%(*), 5%(**), or 1%(***) levels.

(3) Additional covariates include: gender, canadian native, french-speaking, resides in British Columbia, high school graduate at baseline, mother is a high school graduate, not working or looking for work at baseline, and year dummies.

Table 5b: Parameter Estimates, *Wage Sample, employed at baseline interview*

	All		Control v. SSP				Control v. Take Up			
			No Covariates		With Covariates		No Covariates		With Covariates	
	No Covariates	With Covariates	Coefficients	Interaction with SSP	Coefficients	Interaction with SSP	Coefficients	Interaction with Take Up	Coefficients	Interaction with Take Up
	(1)	(2)	(3)		(4)		(5)		(6)	
Change Equation										
Tenure x 10 ^{2}	0.236 *	0.270 **	-0.028	0.580 **	0.033	0.494 **	-0.028	0.685 **	0.032	0.630 *
	(0.123)	(0.118)	(0.154)	(0.230)	(0.139)	(0.221)	(0.154)	(0.337)	(0.139)	(0.359)
Experience x 10 ^{2}	-0.120	-0.151	0.010	-0.277	-0.026	-0.237	0.010	-0.363	-0.034	-0.315
	(0.135)	(0.133)	(0.162)	(0.252)	(0.147)	(0.248)	(0.162)	(0.342)	(0.149)	(0.363)
Experience ^{2} x 10 ^{4}	0.008	0.003	0.030	-0.063	0.021	-0.054	0.030	-0.034	0.023	-0.041
	(0.019)	(0.018)	(0.019)	(0.039)	(0.017)	(0.039)	(0.019)	(0.050)	(0.017)	(0.051)
Job Match Equation										
Job-to-Unemployment-to-Job Transition	0.068	0.069	0.041	0.058	0.023	0.116	0.041	0.007	0.021	0.038
	(0.042)	(0.046)	(0.047)	(0.091)	(0.047)	(0.105)	(0.047)	(0.083)	(0.047)	(0.096)
Job-to-Job Transition	0.073 **	0.054	-0.003	0.113 *	-0.034	0.132 *	-0.003	0.143 **	-0.033	0.165 **
	(0.035)	(0.038)	(0.040)	(0.064)	(0.035)	(0.067)	(0.040)	(0.066)	(0.035)	(0.071)
Previous Tenure After Job-to-Job Transition	0.138	0.199	-0.039	0.675	0.100	0.522	-0.039	0.333	0.100	0.201
	(0.236)	(0.239)	(0.252)	(0.495)	(0.239)	(0.508)	(0.252)	(0.486)	(0.236)	(0.502)
F(All Interactions)	-------	-------	-------	2.42	-------	2.27	-------	2.33	-------	2.10
Probability	-------	-------	-------	0.01	-------	0.02	-------	0.02	-------	0.04
F(Tenure and Experience)	-------	-------	-------	3.72	-------	2.87	-------	3.77	-------	3.06
Probability	-------	-------	-------	0.01	-------	0.04	-------	0.01	-------	0.03
F(Job-to-Job Interactions)	-------	-------	-------	4.05	-------	3.76	-------	3.24	-------	3.21
Probability	-------	-------	-------	0.02	-------	0.02	-------	0.04	-------	0.04
F(Job Match Interactions)	-------	-------	-------	2.80	-------	2.92	-------	2.15	-------	2.18
Probability	-------	-------	-------	0.04	-------	0.03	-------	0.09	-------	0.09
# Individuals	388	362.00	-------	388	-------	362	-------	321	-------	299
# Observations	594	547.00	-------	594	-------	547	-------	508	-------	464
R^2	0.092	0.12	-------	0.131	-------	0.157	-------	0.160	-------	0.194

NOTES:

(1) Standard errors in parentheses.

(2) Coefficient estimates are significant at the 10%(*), 5%(**), or 1%(***) levels.

(3) Additional covariates include: gender, canadian native, french-speaking, resides in British Columbia, high school graduate at baseline, mother is a high school graduate, not working or looking for work at baseline, and year dummies.

have significant returns to either tenure or experience. This is consistent with the very low within–job wage growth for this group shown in Tables 4a and 4b. Experimentals, however, had higher within–job wage growth than controls, which is consistent with the significant difference in returns to tenure between experimentals and controls shown in column 4b. Experimentals, however, do not have higher returns to experience than controls. Thus, the larger within–job wage growth of experimentals found earlier is a result of greater returns to tenure, not experience, among experimentals.

Higher returns to tenure for experimentals compared to controls, however, implies that experimentals have more to lose in terms of lost returns to job–specific skills when moving to new jobs, unless they also move to jobs with a sufficiently high job match component to offset the lost returns to tenure. The jointly significant values of α_1 and α_T in column 4b indicate that experimentals have larger increases in the job match component than controls when moving job–to–job.[35] The positive estimates of α_T indicate that increases in the job match component are larger for experimentals who have greater tenure in their previous job. This is consistent with experimentals only moving when the increase in the match component is large enough to compensate for the lost returns to tenure. Experimentals also have a higher value of α_1, indicating that even without compensation for lost returns to tenure, experimentals have an increase in the job match component that is 3.5 percent higher than controls.

In summary, we find that there are large and significant differences in the underlying parameters that generate within– and between–job wage growth. Experimentals are in jobs with higher returns to tenure than are controls. The greater between–job wage gains of experimentals reflects the fact that experimentals have larger improvements in the job match component and this is sufficient to make up for the lost returns to tenure in the previous job. The fact that we obtain similar patterns in Tables 5a and 5b indicates that these differences in coefficients hold even when we use the sample which is expected to have small compositional effects.

5 Conclusions

We started this paper by asking whether wage or earnings supplements can affect job choice and job duration. The answer to this question is of particular interest given the recent emphasis being placed on work as an alternative to welfare. If earnings supplements can increase wage growth as well as labor market activity, then this program has dual benefits.

Our analytical framework indicates that there are sound economic reasons to believe that a wage or earnings subsidy can affect the type of job a person accepts and the length of time the person stays in each job. Subsidies are expected to induce individuals to accept offers and to search for better jobs while employed, since the opportunity cost of searching while unemployed is increased by the foregone subsidy. If individuals initially accept jobs with lower wages in order to get the subsidy, they are more likely to find a new job that dominates the initial job they accepted; this leads to shorter expected job durations. Whether this

[35] While α_1 and α_T are not each significantly higher for experimentals than controls, the F statistic of 2.78 leads us to reject the hypothesis that these two coefficients are both the same for experimentals and controls.

makes jobs with lower starting wages but higher wage growth more attractive than jobs with flatter wage profiles depends on the structure of the subsidy. Wage or earnings subsidies can also affect between–job wage growth, but the sign of the effect depends on the form of the wage offer distribution. These behavioral effects can be countered or reinforced by compositional effects of the subsidy if the workers induced to enter the labor market by the subsidy face different wage offer distributions or different job opportunities.

Our empirical work shows that experimentals in the SSP have shorter job durations and are more likely to move directly to other jobs than are controls. Experimentals are, however, less likely to leave their jobs and become unemployed. The jobs accepted by experimentals have higher within–job wage growth than the jobs accepted by controls. This is reinforced by higher between–job wage growth for experimentals. These patterns are largely consistent with the behavioral predictions of the analytical framework. While these patterns could also be the result of compositional effects of the program, we find similar results for a subsample designed to minimize compositional effects.

Appendix A

Consider the choice between job A, which has a wage profile given by $w(t) = w_A(t)$ and job B, which pays a constant wage, $w(t) = w_B$, over the same T periods.[36] The constant wage, $\widetilde{w}_A$, that produces an expected wage stream equal to that of job A satisfies the following condition:

$$T\widetilde{w}_A = \int^T w_A(t)\,dt \equiv W_A(T). \tag{A1}$$

Solving for the constant wage equivalent to job A yields the threshold value:

$$\widetilde{w}_A = \frac{W_A(T)}{T}.^{37} \tag{A2}$$

An income–maximizing worker would choose job B over job A only if the constant wage in job B exceeds the constant wage equivalent of job A ($w_B < \widetilde{w}_A$).

To see the effect of an earnings supplement on the choice between jobs, let the wages of both job A and job B be supplemented according to the function $s(w(t))$, which maps pre-subsidy wages, $w(t)$, into the post-subsidy wages, $w^s(t) = s(w(t))$. The question we ask is whether the supplement raises or lowers the constant wage equivalent of job A. If the subsidy changes the threshold that separates acceptable from unacceptable constant wage jobs, then the availability of the supplement affects the choice of jobs.

Similar to equation (A2), the constant wage equivalent to the supplemented stream of wages from job A is given by:

$$\widetilde{w}_A^s = \frac{W_A^s(T)}{T}, \tag{A3}$$

where:

$$W_A^s(T) = \int^T w_A^s(t)\,dt. \tag{A4}$$

In order to see the impact of the supplement on the choice of jobs, we compare the constant wage equivalent of the subsidized job A, $\widetilde{w}_A^s$, to the subsidized value of the constant wage equivalent for job A, $s(\widetilde{w}_A)$. If the subsidy was such that $\widetilde{w}_A^s = s(\widetilde{w}_A)$, then the subsidy has no effect on the choice between job A and job B since the relative threshold does not change. If, however, $\widetilde{w}_A^s > s(\widetilde{w}_A)$, then the supplement raises the constant wage equivalent threshold and the person is more likely to accept a job with wage growth (job A) when the supplement is available.

Since $\widetilde{w}_A$ is the mean of $w_A(t)$, comparing $s(\widetilde{w}_A)$ with $\widetilde{w}_A^s$ requires that we compare the transformation of a mean with the mean of the transformed variable, $\widetilde{w}_A^s$. Using Jensen's inequality, we know that the mean of the transformed variable, $\widetilde{w}_A^s$, is greater than (less than) the transformation of the mean, $\widetilde{w}_A$, if the transformation is convex (concave).Therefore, if

[36] Allowing for discounting, risk aversion, or aversion to intertemporal changes in wages would complicate notation without affecting the results.

[36] If T is unknown but its distribution, $v(T)$, is known, then agents are assumed to compare the expected wage stream in the two jobs. In terms of equation (A2), the equivalent wage streams are given by $\widetilde{w}_A = \int \frac{W_A(T)}{T} v(T)\,dT$.

the transformation is convex the person is more likely to accept a job with wage growth (job A) when the supplement is available.

Appendix B

Table B1: Cox Proportional Hazard Models of Job Duration, *Wage Sample, employed at baseline interview*

	All Job Exits				Job to Job Exits				Exits to Unemployment			
	Control v. SSP		Control v. Take Up		Control v. SSP		Control v. Take Up		Control v. SSP		Control v. Take Up	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
In SSP Group	0.075	0.073	0.109 *	0.101	0.253 ***	0.249 ***	0.389 ***	0.380 ***	-0.064	-0.066	-0.135	-0.141
	(0.059)	(0.059)	(0.064)	(0.064)	(0.089)	(0.090)	(0.093)	(0.094)	(0.080)	(0.081)	(0.090)	(0.090)
Female	———	-0.070	———	-0.126	———	-0.112	———	-0.222	———	-0.058	———	-0.040
	———	(0.179)	———	(0.201)	———	(0.257)	———	(0.275)	———	(0.249)	———	(0.295)
Speaks French	———	-0.072	———	-0.006	———	-0.013	———	0.078	———	-0.102	———	-0.067
	———	(0.083)	———	(0.088)	———	(0.121)	———	(0.124)	———	(0.115)	———	(0.124)
Never Married	———	0.098	———	0.092	———	0.109	———	0.076	———	0.067	———	0.084
	———	(0.063)	———	(0.068)	———	(0.094)	———	(0.099)	———	(0.086)	———	(0.094)
Number of Children	———	-0.032	———	-0.031	———	-0.074	———	-0.060	———	-0.031	———	-0.030
	———	(0.040)	———	(0.043)	———	(0.060)	———	(0.065)	———	(0.054)	———	(0.060)
High School Graduate	———	0.133 **	———	0.082	———	0.106	———	0.039	———	0.139	———	0.109
	———	(0.063)	———	(0.068)	———	(0.094)	———	(0.099)	———	(0.086)	———	(0.094)
# Exits	1,155		994		516		463		621		517	

NOTES:
(1) Standard errors in parentheses.
(2) Coefficient estimates are significant at the 10%(*), 5%(**), or 1%(***) levels.

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