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Do Earnings Subsidies Affect Job Choice?
The Impact of SSP Supplement Payments on Wage Growth

The Self-Sufficiency Project

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The Authors

Abstract

This paper asks whether wage or earnings supplements encourage participants to move into jobs with greater wage growth or to change jobs more often in order to raise their wages. It provides an analytical framework that identifies the key causal links between earnings subsidies and wage growth. This framework highlights the fact that the incentive effects of the subsidy depend crucially on the form of the subsidy. A linear subsidy, like that offered in the Self-Sufficiency Project, is predicted to have no effect on within-job wage growth but is predicted to increase the hazard of moving from one job to the next in order to increase between-job wage growth. This paper finds that these predictions are consistent with the data. The major effect of the SSP supplement is that it increases the probability that a worker will move from one job to another and that the resulting between-job wage gains will be larger as a result of larger gains in the job match component.

Overview

It is widely acknowledged that earnings subsidies promote employment by increasing rewards to labour market activity. The question this paper addresses is whether subsidies also alter the preference for different types of jobs and duration in these jobs. Specifically, do subsidies affect the decision to continue searching for better jobs? Do they alter the trade-off between jobs with low starting wages and high wage growth versus jobs that have higher starting wages but less wage growth? The answers to these questions are 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 increase labour market activity, then this policy tool has dual benefits.

While the analytical links between earnings subsidies and employment are obvious, the links between subsidies and the choice of jobs is less transparent. This paper begins by developing an analytical framework that identifies the key causal mechanisms between earnings subsidies and the choice of jobs and their duration. This framework highlights the importance of the form of the subsidy on the decision to accept certain types of jobs. The focus is specifically on the effects of the subsidy on the trade-off between jobs with low initial wages but high wage growth by asking whether the subsidy alters the trade-off between these two aspects of a job. The paper then turns to the effect of earnings subsidies on wage gains that come from moving to better jobs to determine if subsidies lead to shorter job duration by encouraging “job hopping” as a means to higher wages. Then data from a large earnings supplement experiment is used to explore whether participants who received a supplement exhibited different patterns of wage growth and job duration.

The focus of this paper on the impact of earnings supplements on wage growth and job duration stands in contrast to the previous literature that has focused primarily on the impact of earnings supplements on labour supply decisions. By increasing the rewards to work, earnings supplements induce some eligible participants to join the labour market and encourage others to increase the number of hours worked. These predictions about the effect of the supplement on labour supply have been strongly supported by the data.[1] By themselves, these labour supply effects would lead to higher earnings, but not necessarily to wage growth. This analysis shows that earnings supplements also lead to choices that increase wage growth but that the primary mechanism is movement across a series of jobs, each with a higher starting wage.

This paper contains seven sections. The first section briefly describes the Self-Sufficiency Project (SSP). The second section presents the analytical model used to motivate the empirical work. The third section presents the econometric issues, and the fourth section presents details of the data used. Results are presented in the fifth section, and concluding remarks are in the final section.

[1] See Card, Michalopoulos, & Robins (2001).

Background on the Self-Sufficiency Project

While the analytical results presented in this paper are based on a generic earnings subsidy, they are applied to a specific demonstration project that was instituted in two provinces. Since this application requires a certain familiarity with the basic structure of this experiment, this section briefly describes this demonstration project.

The 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).[2] The hope was that the supplement would not only lead to an increase in labour supply, but also to jobs with higher wages. The key attribute of the program is that it provides time-limited income supplements to individuals who work full time and do not collect IA.

The supplement was provided to 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 and who worked full-time (at least 30 hours a week) in one or more eligible jobs.[3] 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 information sessions on the rules and benefits of the supplement.[4]

Supplement payments were based on earnings and were 50 per cent of the difference between the individual's monthly earnings and a target earnings level each pay period. In 1993 the monthly target earnings for an individual in New Brunswick was $2,500, translating to a maximum supplemented wage of $19.23 for someone working 30 hours per week. The target earnings in British Columbia was $3,083, or $23 per hour.[5] A person earning the minimum wage of $5.00 per hour working 30 hours per week would, therefore, receive a supplement of over $7.00 in New Brunswick, effectively more than doubling her wage.[6] She[7] would, however, face a 50 per cent implicit tax rate against any increase in earnings, since the supplement 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

[2]The IA system has large work disincentives since it imposes a 100 per cent benefit reduction rate on any earnings over a small disregard.

[3]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.

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

[5]The target earnings level was designed to provide adequate income support while creating a positive work incentive. It was adjusted for inflation in subsequent years.

[6]The minimum wage in British Columbia in 1993 was $6.00. 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.

[7]Feminine pronouns are used in this paper because more than 95 per cent of single parents who have received income assistance for at least a year — the target group for SSP — are women.

maximum of 36 consecutive months, starting the month they began collecting supplement payments, but only during those months they were employed full time.[8] 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 worked full time during the three-year period.

[8]An individual who works 30 hours or more per week at one or more jobs is considered to be working full time.

Predicted Impact

Previous research has primarily focused on the predicted impact of wage or earnings subsidies on the propensity to work. Such subsidies may, however, have an effect on wage growth if the structure of the program affects the choice between jobs with different wage profiles. It may also affect the costs and benefits of finding a job with a higher stream of earnings. The objective of this section is to provide conceptual links between program attributes and the economic factors that may affect the decision to search for a better job as well as the decision of which offer to accept. This provides the analytical links to the empirical work that contrasts the outcomes of program group members and control group members in the Self-Sufficiency Project (SSP).

WITHIN-JOB WAGE GROWTH

This section considers whether offering a wage or earnings subsidy can affect the choice between jobs with different wage profiles.[9] For simplicity, consider the choice between Job A, which has a wage profile given by

$$w = g(t), \quad 0 < t < T,\ g^{'}(t) > 0, \tag{1}$$

and Job B, which pays a constant wage over the same T periods.

Consider the constant wage equivalent to Job A when there is neither a wage subsidy nor an income-tested transfer system.[10] The unsubsidized equivalent to Job A, $\tilde{w}$, satisfies the following condition:

$$T\widetilde{w} = \int^{T} g(t)dt \equiv G(T). \tag{2}$$

Solving for the constant wage equivalent yields the threshold value

$$\widetilde{w} = \frac{G(T)}{T}^{11} \tag{3}$$

Therefore, $\tilde{w}$ is the average wage over the duration of Job A. This yields the threshold that separates the constant wage jobs the individual would accept over Job A (i.e. those with $w > \tilde{w}$).

[9] A wage supplement is equivalent to an earnings supplement if an offer consists of a wage profile $g(t)$ and a fixed number of hours, H. If hours are fixed for a job, then the constant wage equivalent is the wage a person would have to receive to have the same total earnings over the life of the job working H hours period at wage $\tilde{w}$.

[10] Allowing for discounting, risk aversion or aversion to intertemporal changes in wages would complicate notation without affecting the results. Appendix A shows that allowing the wage subsidy to be limited to a fixed period, which may be shorter than T, does not affect the results.

[11] 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 3, the equivalent wage streams are given by $\widetilde{w} = \int \frac{G(T)}{T} v(T)dT$.

The following paragraphs turn to the effect of a wage subsidy or transfer on the choice of jobs. Since the choice between jobs will depend on the parameters of the wage supplement and of the transfer system, each is introduced in turn.

Impact of Wage Subsidies and Income-Tested Transfers

General Framework

To see the effect of an earnings subsidy on the choice between jobs, again consider the constant wage equivalent of Job A. Now, instead, let the wages of both Job A and the constant wage job be subsidized according to function $w_s(w)$, which maps pre-subsidy wages into the post-subsidy wages, w_s. The question asked is whether the subsidy raises or lowers the constant wage equivalent of Job A. If it changes the threshold that separates acceptable from unacceptable constant wage jobs, then the availability of the subsidy will affect the choice of jobs.

The constant wage equivalent to the subsidized stream of wages from Job A satisfies

$$T\tilde{w}_s = \int^{T} w_s(g(t))dt \equiv W_s(T), \tag{4}$$

which implies

$$\tilde{w}_s = \frac{W_s(T)}{T}. \tag{5}$$

In order to see the impact of the subsidy on the choice of jobs, $\tilde{w}_s$ can be compared with $\tilde{w}$, the constant wage equivalent to Job A in the absence of a subsidy. If $\tilde{w}_s = w_s(\tilde{w})$, then if a person was indifferent between Job A and a job paying $\tilde{w}$ before the subsidy, she would also be indifferent between Job A and a constant wage job paying $\tilde{w}_s$ after the subsidy. If $\tilde{w}_s > w_s(\tilde{w})$, then the subsidy raises the constant wage equivalent threshold and the person is more likely to accept a job with wage growth when the subsidy is available.

Since $\tilde{w}$ is the mean of w, comparing $w_s(\tilde{w})$ with $\tilde{w}_s$ requires that the transformation of a mean is compared with the mean of the transformed variable, $\tilde{w}_s$. Using Jensen's inequality, it is known that the mean of the transformed variable, $\tilde{w}_s$, is greater than the transformation of the mean, $\tilde{w}$, if the transformation is convex, and is less than the transformation of the mean, $\tilde{w}$, if the transformation is concave. Since the transformation is the mapping of pre-subsidized wages into post-subsidized wages, this indicates that the constant wage equivalent will be no lower after the subsidy unless the transformation is concave. Intuitively, if the post-subsidy wage is an increasing multiple of the pre-subsidy wage, then the subsidy makes high wage growth jobs relatively more attractive. The agent will, therefore, increase the threshold for the constant wage equivalent job she would accept compared with the threshold in the absence of the subsidy. As a result of this convex transformation, the job with wage growth becomes more attractive.[12] In the special case of a linear subsidy, the wage subsidy has no affect on the threshold ($\tilde{w}_s = \tilde{w}$). Constant wage jobs that would be accepted before the subsidy will still be accepted after the subsidy. Therefore, the subsidy has no affect on the choice of jobs in the case of a linear subsidy.

Illustrative Example

To illustrate this result, consider two jobs that have the same duration but different wage paths. For simplicity, consider the value of these two jobs when there is no discounting and no risk aversion. The first job pays $10 per hour in each period. The second job starts with a wage of $5 per hour during the first half of the job and $15 during the second half. Since these two jobs yield the same total earnings, it is assumed that the worker would be indifferent between the job with the constant wage and the job with wage growth. Now consider the choice between these two jobs if the wage is subsidized by half the difference between the offered wage and $20. This is a linear subsidy since the subsidy decreases by a constant amount as wages increase (i.e. the subsidy declines by $0.50 for each $1.00 increase in the wage until the maximum of $20 is reached). The first job now yields $15 per hour (i.e. $10 plus the $5 subsidy). The second job has an effective wage of $12.50 during the first half of the job (the $5 wage plus the $7.50 subsidy) and $17.50 during the second half (the $15 wage plus the $2.50 subsidy). Therefore, both jobs have higher effective wages as a result of the subsidy but the subsidy does not change their relative rank since they now both pay a total of $30 over the two periods. This linear subsidy, therefore, does not make the high wage growth job more or less attractive.

Now consider a different set of jobs. The first pays $18 during its duration and the other pays $12 during the first half of the job and $24 during the second half. For these two jobs the subsidy is no longer linear since wage increases above $20 are not subsidized. The two jobs are still valued equally without the subsidy. However, under the subsidy the job with wage growth is preferred. The constant wage job would yield an effective wage of $19 per period ($18 plus the $1 subsidy) or $38 over the two periods. In contrast the job with wage growth would have an effective wage of $16 ($12 plus the $4 subsidy) during the first half of the job and an unsubsidized wage of $24 during the second half. Since the job with wage growth yields $40 over the two periods, it is made more attractive by the wage subsidy.

Comparison of the SSP Wage Supplement With the IA Transfer System

This analysis requires that the job choice of program group members who are eligible for the SSP wage supplement is compared with the job choice made by control group members who are eligible for the income assistance (IA) income transfer. The fact that both control and program group members are eligible for income transfers adds a level of complexity.[13]

[12] Note that in the case of the SSP supplement the convexity is only introduced by the ceiling on earnings. As long as earnings are below this ceiling, post-supplement wages increase by half of the increase in pre-supplement wages. Once the ceiling is exceeded there is a dollar for dollar increase since earnings are no longer subject to the implicit 50 per cent tax rate.

[13] In order to focus on essentials, labour supply effects of transfers and wage supplements are ignored.

First consider the impact of the IA transfer on the job choice of control group members (IA recipients). The same logic as developed above is used for the earnings supplement. Let $w_{tr}(w)$ be a mapping of pre-transfer wages, w, into the post-transfer wages, w_{tr}, under the IA system. The identical logic used above is followed to obtain the constant wage equivalent to Job A after the transfer:

$$\tilde{w}_{tr} = \frac{W_{tr}(T)}{T}. \tag{6}$$

The threshold separating the constant wage jobs, $\tilde{w}_{tr}$, is the transfer the recipient would accept given the choice of Job A when both jobs are subject to the transfer system. The effect of the supplement again depends on the concavity of $w_{tr}(w)$. Transfer recipients who are working under IA can earn up to $200 per month without a reduction in benefits.[14] Earnings above this level are subject to a 100 per cent benefit reduction rate. The transfer system is, therefore, a linear mapping of pre-transfer earnings into post-transfer earnings for persons who do not expect to exceed the disregard. For them, the transfer system does not alter the preference for jobs since they will be able to keep all the benefits of a job with wage growth. On the other hand, for those recipients who expect to earn more than the disregard, the mapping is highly concave. This makes jobs with higher wage growth less attractive since all the benefits of growth past the disregard would be taxed away by the transfer system.

Now consider the impact of the SSP earnings supplement on the job choice of program group members. Under the SSP supplement available to the program group members, a wage, w, is supplemented by half the difference between the wage and the target wage, $w_{\max}$. The supplemented wage, $w_s(w)$, is given by

$$\begin{aligned} w_s(w) &= w + \left(\frac{w_{\max} - w}{2}\right) = .5\left(w + w_{\max}\right) \text{ if } w < w_{\max}, \\ &= w \text{ if } w \geq w_{\max}. \end{aligned} \tag{7}$$

The supplemented wage, w_s, is, therefore, a linear function of the unsupplemented wage, w, up to $w_{\max}$ and is equal to w above that threshold. Thus, for persons who do not expect their wages to rise above $w_{\max}$, the supplement has no impact on the preference for jobs with wage growth. Those who expect to earn more than the maximum will face a convex mapping which will make jobs with wage growth more attractive.[15] Intuitively, they will be eligible for

[14] In New Brunswick there was a disregard of $200 per month and a 100 per cent tax rate in the IA program. When the experiment started in British Columbia, there was a "flat" disregard of $200 per month (with a 100 per cent tax rate) plus an "enhanced" disregard of 25 per cent of earnings above the $200. This enhanced disregard could only be used for 12 of every 36 months. In January 1996 the flat disregard was dropped and the 25 per cent disregard could be used for only 12 months over the recipient's lifetime.

[15] Appendix A shows that if the job lasts longer than the supplemented period $(T > T_o)$, then the supplement is predicted to raise the threshold, which implies that the supplemented workers are again more likely to accept the job with wage growth. The intuition for this result is that the program supplements the low wages at the beginning of the job where wages start low and increase. The supplement decreases as wages grow. Thus, if the job is not supplemented over its full duration, it is better to be in a job with wage growth since the supplement would already be low when the supplement is eliminated.

supplements when they start their jobs with low initial wages and will benefit from the wage growth after the supplement is phased out. Since few program group members can expect to have wages rise above $20 per hour, which is roughly the wage that would have to be received to exceed the target earnings, this factor is expected to have relatively little impact.[16] This leads to the general prediction that wage growth within jobs is not expected to be affected by the SSP supplement.

Since both IA and SSP are linear in their effective ranges, it is not expected that either program will have a substantial effect on within-job wage growth. If there is an effect, it will come from IA recipients who foresee working past the disregard in the job with wage growth (and would, therefore, prefer the job with constant wages) or from SSP recipients who expect to earn more than the target earnings on the job with wage growth. Both are expected to be sufficiently unlikely as to have a negligible effect on the choice of jobs.

BETWEEN-WAGE JOB GROWTH

Earnings subsidies may also affect between-job wage growth by altering the decision to search for a new job, the type of search (search on the job or while unemployed), and the reservation wage. The intuition for the relationship between wage subsidies and the decision to search on the job or while unemployed is straightforward. Earnings subsidies increase the cost of refusing a wage offer and continuing to search since the person would receive the subsidy as well as the wage if she accepted the job. 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 is, however, not as easily signed. The subsidy increases the benefits of search by increasing the value of each subsidized offer, but the expected wage gain between jobs depends on the functional form of the wage offer distribution.

To put this somewhat more formally, the standard results in Burdett's (1978) classic article on the choice between full-time search and on-the-job search is modified by introducing an earnings subsidy into that framework. In order to focus on essentials, a standard search framework is used 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 without recall.[17]

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. In order to allow for the possibility that full-time search is the optimal

[16] Recall from page 3 that the 1993 target wage level was $19.23 per hour in New Brunswick and $23 per hour in British Columbia.

[17] 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 (2002) analyze search over both slopes and intercepts using a dynamic programming framework that takes account of 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 being made.

choice, it is assumed that the search costs are lower when searching full time than when searching while holding another job (i.e. $C_{fts} < C_{ojs}$).

Search in the Absence of a Wage Subsidy

First consider the decision whether to search and, if so, whether to search full time or on the job in the absence of a wage subsidy. Burdett (1978) shows that there are three ranges of wages with different optimal decisions. This is shown in Figure 1. In the absence of the subsidy, the marginal benefit of search is given by

$$H(w^*) = \frac{1}{r}\int_{w^*}(w - w^*)f(w)dw, \tag{8}$$

which is downward sloping since $\frac{\partial H(w^*)}{\partial w^*} = -\frac{1}{r}\left(1 - F\left(W^*\right)\right) < 0$ 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:

$$C_{ojs} = w_1^* + C_{fts}. \tag{9}$$

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.

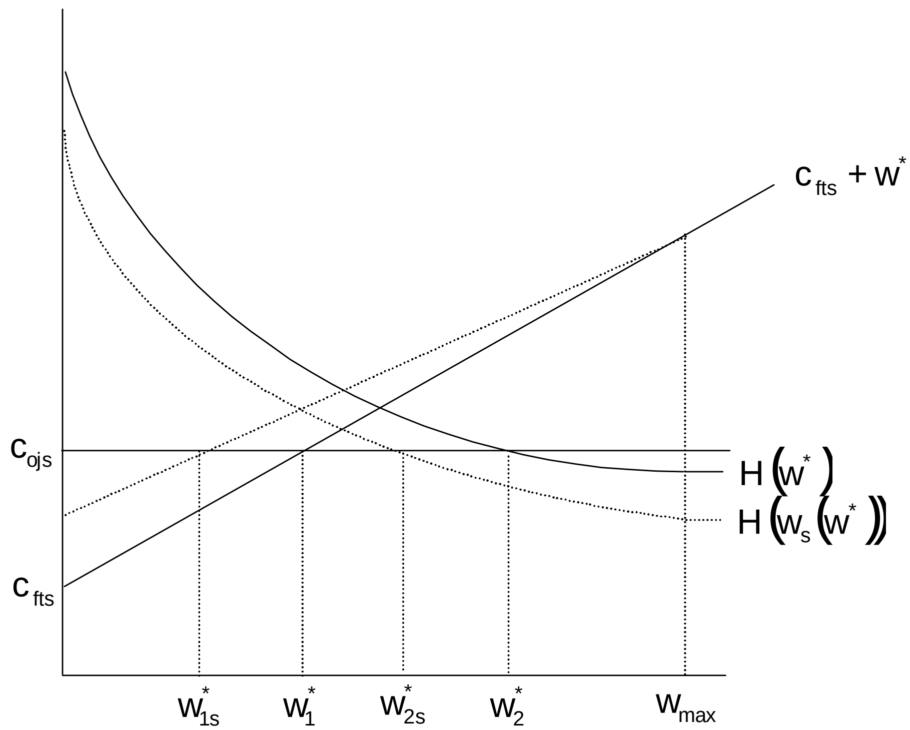
The decision whether to search depends on the marginal costs and benefits of search. Search stops when wage offers exceed w_2^*, which is the point where the marginal costs and benefits of further on-the-job search are equal. This threshold is determined by the implicit expression

$$C_{ojs} = \frac{1}{r}\int_{w_2^*}(w - w_2^*)f(w)dw. \tag{10}$$

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.[18]

[18] 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.

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



Search With a Wage Subsidy in Place

First consider the impact of introducing a wage subsidy, $w_s(w)$, on the decision whether to search full time or on the job. 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_{1s}^*, as illustrated in Figure 1. Thus, the earnings subsidy is predicted to increase the propensity to search while holding a job.[19]

[19] If the lower support of the wage offer distribution is set by the minimium wage, then w_{1s}^* 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.

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^*.[20] 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_{2S}^*, which is defined by the implicit expression

$$C_{ojs} = \frac{1}{r}\int_{w_{2s}^*} (w_s(w) - w_{2S}^*) f(w) dw. \tag{11}$$

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^* reduces $h(w_1^* w_2^*)$ and, hence, increases 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 $w_{1s}^* < w_c^* < w_1^*$). 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 $w_{2s}^* < w_c^* < w_2^*$). Individuals with high wages have the lowest probability of finding jobs that lead to a job exit. The wage subsidy, therefore, increases the number of persons 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 and, thus, remains an empirical question. To see the predicted impact on the expected wage gain, define $G(w_c^*)$ to be the difference between the expected wage and the current wage for a person with a wage of w_c^*:

$$G(w_c^*) = E(w - w_c^* | w > w_c^*). \tag{12}$$

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

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

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

The expected gain for a random individual who is searching on the job (i.e. $w_1^* < w_c^* < w_2^*$) is given by

$$\tilde{G}(w_1^* w_2^*) = \frac{\int_{w_1^*}^{w_2^*} G(w_c^*) f(w_c^*) dw_c^*}{\int_{w_1^*}^{w_2^*} f(w_c^*) dw_c^*}. \tag{13}$$

As shown in the previous section, the wage subsidy decreases both w_1^* and w_2^*. The impact of introducing a wage subsidy is, therefore, given by taking the total differential:

$$d\tilde{G}(w_1^* w_2^*) = \frac{\partial \tilde{G}}{\partial w_1^*} dw_1^* + \frac{\partial \tilde{G}}{\partial w_2^*} dw_2^*. \tag{14}$$

Without further assumptions about the form of $f(w)$ the sign of this expression may be positive or negative. Therefore, earnings subsidies may either increase or decrease between-job wage gains depending on the wage offer distribution.[21] 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 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 acceptable offer decreases more depends on the form of the wage offer distribution.

In summary, this analysis shows 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 cease searching altogether. 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 the analysis shows that a wage subsidy is predicted to decrease job duration as persons initially accept jobs with low wages that 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.

[21] In the special case where the wage offer distribution is exponential, $\tilde{G}$ is independent of w_1^* and w_2^*.

Empirical Methodology

The empirical work presented in this paper begins with descriptive tables on control group / program group differences in hazard rates to see whether job duration is shorter for those eligible for the subsidy, as suggested by the theory. Differences in within-job and between-job wage growth for program group and control group members are then examined.

Since wage growth reflects returns to tenure, returns to experience, and improved job match, these fundamentals are estimated in order to understand the factors underlying the observed differences in wage growth within and between jobs. For example, program group members may have higher wage growth between jobs for two conceptually different reasons. Changing jobs may lead to a larger increase in the job match component as suggested by the theory. Alternatively, program group members may lose less in terms of forgone returns to job-specific tenure when switching jobs if they have lower returns to tenure. Since the latter would be inconsistent with a linear subsidy, it is important to distinguish between these two possible explanations.[22] It is only by obtaining the underlying parameters of the wage generating process that the factors that affect the descriptive summary measures can be understood.

The empirical model used to estimate the underlying parameters takes explicit account of 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. Intuitively, a person with longer tenure on a job will have more to give up when moving to a new job since the new job will not reward the job-specific tenure obtained on the previous job. Therefore, these agents require a higher job match component in order to switch jobs. Likewise, match quality improves as agents move to better jobs. Based on these observations, the literature in this area has largely used the following standard log wage model with person- and match-specific error components:

$$Y_{ijt} = \beta_x X_{ijt} + \beta_T T_{ijt} + \varepsilon_{ijt}, \text{ and} \tag{15}$$

$$\varepsilon_{ijt} = \phi_{ij} + \mu_i + \upsilon_{ijt}, \tag{16}$$

where X_{ijt} is accumulated labour 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.

A simple model of on-the-job search predicts that a person with T_{ijt} periods of tenure in job j will accept an offer if the job match component in the new job exceeds the sum of the match component in the current job plus the foregone returns to tenure in the current job.

[22] The linear subsidy is not expected to affect within-job wage growth, which is the sum of returns to tenure and returns to experience.

Let ϕ_{ij}^* be the resulting reservation value for job j. Then

$$\phi_{ij}^* = \phi_{ij-1} + \beta_T T_{ij-1}. \tag{17}$$

The expected value of accepted offers is, therefore, given by $E\left(\phi_{ij} \mid \phi_{ij} > \phi_{ij}^*\right)$. This expectation increases with tenure since the rewards to job-specific tenure are lost when moving to a new job. It also increases with the number of previous successful job matches if each successive job must have a higher wage than the previous job. This implies that one should condition on the number of previous accepted offers, $j-1$, as well as previous tenure, T_{ijt}.

Following the literature, a linear approximation to conditional expectations of ϕ_{ij} is taken:[23]

$$\phi_{ij} = \alpha_0 + \alpha_1 (j-1) + \alpha_T T_{ijt} + \eta_{ij}. \tag{18}$$

Substituting Equation 18 into Equation 16 and rearranging terms yields

$$Y_{ijt} = (\alpha_0 - \alpha_1) + \beta_x X_{ijt} + \widetilde{\beta}_T T_{ijt} + \alpha_1 j + \varepsilon_{ijt}, \tag{19}$$

where $\varepsilon_{ijt} = \eta_{ij} + \mu_i + \upsilon_{ijt}$ and $\widetilde{\beta}_T = \beta_T + \alpha_T$. The estimated returns to tenure, $\widetilde{\beta}_T$, will, therefore, include both the direct effect of tenure on wage growth within the job, β_T, plus the indirect effect through the improvement in job match, α_T. Since β_T cannot be identified separately from α_T, the previous literature is followed in noting that when returns to tenure are referred to both the direct impact of tenure and the indirect impact through improved job match are implicitly included (i.e. β_T and α_T).

Topel (1991) is followed in estimating the combined impact of experience and tenure ($\beta_x + \beta_T$) by taking differences of Equation 15 for periods in which the respondent is in the same job. The within-job estimator is given by

$$\Delta_w = Y_{ijt+1} - Y_{ijt} = (\beta_x + \beta_T) T_{ijk} + \Delta\upsilon_{ij}, \tag{20}$$

where the difference is taken between the starting wage and the last observed wage in the job when the worker's tenure is T_{ijk}. Estimating Equation 20 by least squares yields consistent estimates of $\beta \equiv \beta_x + \beta_T$. While not separately identifying returns to experience from returns to tenures, $\hat{\beta}$ does yield an unbiased estimate of within-job wage growth.

Two different measures of wage changes across jobs are used to identify the underlying parameters. The first uses the wage change from the last observed period in job j to the first

[23] Note that Topel (1991) and Altonji and Williams (1997) include previous experience rather than the previous number of jobs in Equation 18.

period in job $j+1$:

$$\Delta_b = Y_{i,j+1,0} - Y_{ijs} = [\beta_x + \alpha_1] - \widetilde{\beta}_T \widetilde{T}_{ij} + \Delta\eta_{ij} + \Delta\varepsilon_{ij}, \tag{21}$$

where $\widetilde{T}_{ij}$ is total tenure at the end of job j and Y_{ijs} is the wage in the last period of job j. The intercept captures the returns to experience from the additional period of work, β_x, plus the improved job match associated with making a transition, α_1.

Changes in starting wages are also used to identify the parameters. Taking the difference in wages at the beginning of jobs j and $j+1$ yields

$$\Delta_s \equiv Y_{i,j+1,0} - Y_{ij0} = \alpha_1 + \beta_x \left(X_{i,j+1,0} - X_{ijo}\right) + \Delta\eta. \tag{22}$$

Since $X_{i,j+1,0} - X_{j0} = \widetilde{T}_{ij} + 1$, this can be rewritten as

$$\Delta_s = \alpha_1 + \beta_x \left(\widetilde{T}_{ij} + 1\right) + \Delta\eta. \tag{23}$$

Therefore, only a measure of completed tenure in the previous job is needed, rather than a direct measure of lifetime labour market experience to estimate Equation 23.[24] Since tenure is zero at the start of each job, the change in starting wages is not affected by $\widetilde{\beta_T}$. This allows β_x and α_1 to be identified from Equation 23.

The parameters in equations 20, 21, and 23 are estimated using least squares. Since these equations include the same parameters (β_x, $\widetilde{\beta}_T$, and α_1), these three equations are estimated simultaneously and impose these cross-equation constraints. White's correction for heteroscedasticity is used in constructing standard errors.

[24] However, in the application of this model, a measure of the change in experience squared is needed.

Data and Summary Statistics

The SSP data used in this paper include wage and job histories for persons who were randomly assigned to the program and control groups between November 1992 and February 1995, resulting in 2,827 and 2,858 individuals assigned to the IA (control) and SSP (program) groups, respectively.[25] The SSP data set includes 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 for a subset of these respondents.[26] 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. 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 labour market experience directly rather than having to rely on potential experience (age, minus education, minus six).[27]

Data from the primary analysis file is used to measure the duration of jobs and to see whether these differ between control group members and program group members as suggested by the theory. Starting and ending wages were not recorded until after the 18-month interview, so the analysis is limited to the subsample of jobs that began after the 18th month when examining differences between program group members and control group members in wage growth.[28] Inference can, therefore, be made only to the jobs starting at least 19 months after the start of eligibility.[29] Since the generalizability of the results concerning wage growth to the wider population of all jobs is a concern, the implications of this sample limitation are explored. Specifically, whether this sample restriction would have affected the conclusions about program group / control group differences in employment and job duration, which can be observed for the full sample is explored.

[25]The SSP Plus group, which received additional services, is excluded.

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

[27]To calculate experience, the baseline measure of experience is taken and turned into a monthly measure. This measure is then incremented by one for each month of observed employment. Tenure is measured similarly by counting the months since the respondent started working for the employer.

[28]Wage growth is measured between the start of a job and either its end or the last wage reported in the 36-month interview.

[29]Card et al. (2001), use 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 asks 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., then this measure would report no wage growth since the wage in both periods would be the same average wage in the spell.

Tables 1a and 1b present summary statistics for the full sample and for the wage sample.[30] Table 1a shows that among the program group members, 1,888, or roughly two thirds did not up take the supplement.[31] As can be seen in columns 2 and 3, the control and combined program 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 per cent speak French. Almost 50 per cent have never married, and they have an average of 1.7 children. Both program group members and control group members have low education, with roughly 55 per cent having less than a high school diploma.

Both groups have substantial previous labour market experience, but few were working at the baseline interview. Only five per cent had no previous work experience and the average months of previous experience is 88 months. This indicates that these females already had more than seven years of experience. However, only 19 per cent were working at the baseline interview and almost two thirds of these were working part time. Thus, both control group members and program group members were only marginally attached to the labour market at the baseline interview.

These summary statistics in Table 1a indicate that the program and control groups in the primary sample are similar on the basis of observed demographic and labour market characteristics. Table 1b shows the characteristics of the 1,461 respondents in the 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. These differences, however, affect control group members as well as program group members. As a result, the program group / control group differences in baseline characteristics are similar in the wage sample and the full sample. The only major difference is in initial experience where control group members have substantially more experience at baseline than control group members. For all other measures the differences between program group members and control group members are small and of roughly the same magnitude as in the full sample.

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[30] The "wage sample" refers to that subsample of jobs for which there are both starting and ending wages available (i.e. those jobs that start after the 18th month).

[31] Among those assigned to the program group, roughly two thirds did not receive a supplement because they either did not find a full-time job within the first 12 months or they qualified but did not apply. Looking at the "no takeup" group, however, shows that only four per cent of those who were eligible yet did not take up the program worked full time during the first 12 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 per cent), personal responsibilites interfered (15.2 per cent), and health problems precluded full-time employment (14.0 per cent).

Table 1a: Summary Statistics, All Individuals

	All	Control Group (IA)	Program Group: SSP Eligible	Program Group: Took Up SSP	Program Group: Did Not Take Up SSP
	(1)	(2)	(3)	(4)	(5)
Individual characteristics					
Age	31.9	31.9	31.9	31.0	32.4
	(0.11)	(0.16)	(0.16)	(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)
Baseline characteristics					
Demographic characteristics					
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)
Job characteristics					
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)
Number of individuals	5,685	2,827	2,858	970	1,888

Note: Standard errors are in parentheses.

ALTERNATIVE PROGRAM GROUP / CONTROL GROUP CONTRASTS

While program group members resemble control group members, there are striking differences between those program group members who took up the program and those who did not. Tables 1a and 1b show that program group members who took up the program (column 4), had substantially more attachment to the labour market than those who did not take up the program (column 5). In the full sample (Table 1a) those who took up the program had 96 months of prior work experience at the baseline survey compared with 76 months for those program group members who did not take up the program. Likewise, the proportion working at the baseline is 31 per cent for the take-up group but only 15 per cent for those who did not take up the benefits in spite of being eligible. These data clearly indicate that the decision to take up the program was not random and is consistent with persons who are more likely to gain from the program being more likely to take advantage of the program.

Table 1b: Summary Statistics, Wage Sample

				Program Group	
	All (1)	Control Group (IA) (2)	SSP Eligible (3)	Took Up SSP (4)	Did Not Take Up SSP (5)
Individual characteristics					
Age	30.0	30.1	30.0	30.0	29.9
	(0.20)	(0.28)	(0.28)	(0.40)	(0.39)
Speaks English	0.99	0.99	0.99	0.99	0.99
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
Speaks French	0.15	0.16	0.15	0.16	0.14
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Baseline characteristics					
Demographic characteristics					
Never married	0.52	0.52	0.53	0.52	0.54
	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)
Number of kids	1.59	1.59	1.59	1.55	1.63
	(0.02)	(0.03)	(0.03)	(0.04)	(0.04)
Less than high school	0.42	0.42	0.43	0.40	0.45
	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)
High school graduate	0.58	0.58	0.58	0.60	0.55
	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)
Job characteristics					
Initial experience (months)	88.47	90.96	86.17	96.36	76.38
	(1.91)	(2.72)	(2.67)	(3.98)	(3.52)
Employed	0.23	0.24	0.23	0.31	0.15
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Employed full time	0.09	0.09	0.09	0.14	0.04
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Employed part time	0.15	0.15	0.14	0.17	0.11
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
No previous work experience	0.02	0.02	0.02	0.01	0.03
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Not employed and not looking	0.49	0.48	0.51	0.42	0.59
	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)
Number of individuals	1,461	700	761	374	387

Note: Standard errors are in parentheses.

While the contrast between the program group members who took up the program and the control group members 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 on persons who would decide to participate in a program if it were made available?

Two contrasts are, therefore, provided when comparing program group members and control group members. The first is the mean difference between control group members and all persons assigned to the program group, including program group members who did not receive a supplement at any point. Contrast between the control group members and all program group members, including the no-takeup group, 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 control group members and those program group members who participated in the program, as evidenced by receiving a supplement. Since the decision to take up the supplement 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 control group members with outcomes of program group members who took up the program yields the impact of the treatment on the treated.[32]

It should be noted that Card et al. (2001) use a different contrast. They 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 control group members. Given this question, the treated are only those program group members induced to increase their hours as a result of the program. This is a subset of those who "took up" the program.[33] Since the decision to work full time is also endogenous, their treatment effect 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 publications are, therefore, estimating two different treatment effects.[34]

[32] 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 this paper, where the dependent variable is the change in wage, all person-specific time-invariant unobservables are already differentiated out.

[33] 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."

[34] While this 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. The analytical model presented in this paper suggests that this group could also have been affected.

Program Impact

This section begins by presenting tabular evidence on program group / control group differences in employment and in wage growth. Since these summary measures do not control for other relevant factors, the estimated model is then considered.

IMPACT ON EMPLOYMENT

The full sample and the wage sample used in this paper show the same positive employment effects of earnings subsidies as found in previous studies. Tables 2a and 2b show the numbers of months worked after the baseline survey for members of the full sample and the wage sample respectively. These summary statistics are shown for control group members and program group members (who are further disaggregated into those who took up the program and those who did not). These data, based on the 18- and 36-month surveys, show that both samples are consistent with previous studies showing that the supplement offered by the Self-Sufficiency Project (SSP) increased the probability that former welfare recipients would take full-time jobs. The bottom 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 15.1 months. The top panel shows that roughly half of this is full-time work (7.7 months). program group members, however, worked substantially more and the difference largely reflects an increase in full-time work. For the program group, the average number of months worked is 17.5 months (versus 15.1) and 61 per cent of that is full-time work (10.7 months). This indicates that the program increased the average number of months worked by 2.4 months and the number of months of full-time work by 3.0 months. Not unexpectedly program group members who took up the program show substantially larger differences. The mean expected increase for someone who took up the program would be 17.1 months of employment by the 36th month.

While the wage sample imposes an additional non-experimental restriction, this constraint does not alter the conclusion that program group members worked more than control group members. Table 2b shows that program group members in the wage sample worked 1.6 months more than control group members, and that the average number of months of full-time work was 2.6 months higher for program group members than control group members. In spite of having considerably smaller sample sizes in the wage sample than in the full sample, the null of no increase in overall employment at the 0.05 level can still be rejected and the null of no increase in full-time employment can be rejected at the 0.01 level. The difference between program group members who took up the program and control group members is 9.3 months and highly significant in spite of the small sample size. These differences indicate that limiting the sample to the subset of jobs that can be used to study wage growth reduces, but does not obscure, the employment-increasing impact of the program.

Table 2a: Observed Employment, All Individuals

	All	Control Group (IA)	Program Group		
			SSP Eligible	Took Up SSP	Did Not Take Up SSP
	(1)	(2)	(3)	(4)	(5)
Full-time jobs					
Total number of full-time jobs	4,464	1,877	2,587	1,832	755
Observed employment (months)	9.23	7.72	10.72	24.80	3.49
	(0.20)	(0.27)	(0.30)	(0.57)	(0.20)
Average number of jobs per individual	0.79	0.66	0.91	1.89	0.02
	(0.01)	(0.02)	(0.02)	(0.04)	(0.37)
Average number of jobs per worker	1.23	1.10	1.34	1.91	0.78
	(0.02)	(0.03)	(0.03)	(0.04)	(0.03)
Part-time jobs					
Total number of part-time jobs	3,310	1,650	1,660	692	968
Observed employment (months)	7.05	7.36	6.74	7.42	6.40
	(0.20)	(0.29)	(0.27)	(0.45)	(0.34)
Average number of jobs per individual	0.58	0.58	0.58	0.71	0.02
	(0.01)	(0.02)	(0.02)	(0.03)	(0.47)
Average number of jobs per worker	0.91	0.96	0.86	0.72	1.00
	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)
All jobs					
Total number of jobs	7,774	3,527	4,247	2,524	1,723
Observed employment (months)	16.28	15.08	17.47	32.22	9.89
	(0.27)	(0.39)	(0.39)	(0.63)	(0.39)
Average number of jobs per individual	1.37	1.25	1.49	2.60	0.03
	(0.02)	(0.03)	(0.03)	(0.05)	(0.86)
Average number of jobs per worker	2.13	2.06	2.20	2.64	1.77
	(0.02)	(0.03)	(0.03)	(0.05)	(0.04)
Number of individuals	5,685	2,827	2,858	970	1,888

Note: Standard errors are in parentheses.

Table 2b: Observed Employment, Wage Sample

	All (1)	Control Group (IA) (2)	Program Group: SSP Eligible (3)	Program Group: Took Up SSP (4)	Program Group: Did Not Take Up SSP (5)
Full-time Jobs					
Total number of full-time jobs	2,457	1,037	1,420	998	422
Observed employment (months)	13.75	12.40	14.99	23.20	7.05
	(0.38)	(0.55)	(0.52)	(0.74)	(0.46)
Average number of jobs per individual	1.68	1.48	1.87	2.67	0.05
	(0.04)	(0.05)	(0.05)	(0.07)	(0.99)
Part-time jobs					
Total number of part-time jobs	1,656	805	851	398	453
Observed employment (months)	9.77	10.29	9.29	8.87	9.68
	(0.38)	(0.56)	(0.51)	(0.71)	(0.73)
Average number of jobs per individual	1.13	1.15	1.12	1.06	0.06
	(0.03)	(0.04)	(0.04)	(0.06)	(1.05)
All jobs					
Total number of jobs	4,113	1,842	2,271	1,396	875
Observed employment (months)	23.52	22.70	24.27	32.07	16.73
	(0.48)	(0.70)	(0.66)	(0.87)	(0.82)
Average number of jobs per individual	2.82	2.63	2.98	3.73	0.07
	(0.04)	(0.06)	(0.06)	(0.08)	(2.11)
Number of individuals	1,461	700	761	374	387

Note: Standard errors are in parentheses.

IMPACT ON TRANSITION TYPES AND JOB DURATION

The analytical model predicts that program group members are more likely to search on the job than are control group members 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 program group members than control group members since program group members forego the earnings supplement 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 new wage offers. This would lead to shorter job duration.[35]

[35] An indirect implication is that program group members will accept lower wages than control group members in their first jobs after the program is started. While starting wages for jobs that started before the 18th month are not available, the mean wage in the first job is known. Mean wages in the first jobs of program group members were 13 per cent lower than for control group members, which is a statistically significant difference.

The predictions about job duration and exit type are explored using Cox proportional hazard models of the competing risk of exiting the current job to move directly to another job or exiting to non-employment. The first four columns of tables 3a and 3b present coefficient estimates for exits directly to other jobs.[36] These estimates can be used to test the prediction that the wage supplement increases the probability of moving directly to another job.[37] Column 1 of Table 3a shows that the odds ratio of leaving a job to move directly to another job is 27 per cent higher for program group members than control group members and this difference is significant at all conventional levels. When demographic controls are added, the coefficient increases slightly and remains highly significant. When the contrast is between control group members and program group members who took up the program (columns 3 and 4), the coefficients nearly double to 0.48 indicating that the program group members who took up the program were much more likely than control group members to leave their current jobs to take another job. These four columns, therefore, give strong support to the prediction that the subsidy decreases the expected duration of on-the-job search.

While the Burdett 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 supplement will, however, increase the foregone earnings if the person quits, which implies that program group members are less likely to make such transitions. Columns 5 to 8 of Table 3a offer support for this prediction. The hazard of exiting to non-employment is lower for program group members than control group members in all four columns, though the differences are significant at conventional levels only for the contrast between control group members and program group members who took up the program.

Finally, Table 3b shows that program group members in the wage sample also have higher hazards of making a job-to-job transition. 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 it is only the contrast between the program group members who took up the program and the control group members that is statistically significant at conventional levels.

[36] Exits to non-employment are treated as censored observations.

[37] The latter can be viewed as competing risk models, where exit to another job and exits to non-employment are the competing risks. In each case exits to the alternative state is treated as a censored spell.

Table 3a: Cox Proportional Hazard Models of Job Duration, All Jobs

	All Job Exits				Job to Job Exits				Exits to Unemployment			
	Control vs. SSP		Control vs. Take Up		Control vs. SSP		Control vs. Take Up		Control vs. SSP		Control vs. Take Up	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
In SSP group	0.036	0.036	-0.002	-0.001	0.268 ***	0.287 ***	0.479 ***	0.487 ***	-0.047	-0.053	-0.210 ***	-0.212 ***
	(0.029)	(0.030)	(0.034)	(0.034)	(0.058)	(0.058)	(0.061)	(0.061)	(0.035)	(0.035)	(0.041)	(0.041)
Female	---------	0.041	---------	-0.018	---------	0.072	---------	-0.007	---------	0.030	---------	-0.021
	---------	(0.078)	---------	(0.087)	---------	(0.154)	---------	(0.162)	---------	(0.092)	---------	(0.104)
Speaks French	---------	0.021	---------	0.021	---------	0.167 **	---------	0.134 *	---------	-0.034	---------	-0.029
	---------	(0.040)	---------	(0.045)	---------	(0.074)	---------	(0.079)	---------	(0.048)	---------	(0.054)
Never married	---------	0.108 ***	---------	0.118 ***	---------	-0.003	---------	0.016	---------	0.139 ***	---------	0.154 ***
	---------	(0.031)	---------	(0.035)	---------	(0.059)	---------	(0.064)	---------	(0.036)	---------	(0.042)
Number of children	---------	-0.043 **	---------	-0.039 *	---------	-0.133 ***	---------	-0.113 ***	---------	-0.017	---------	-0.016
	---------	(0.019)	---------	(0.022)	---------	(0.039)	---------	(0.042)	---------	(0.022)	---------	(0.026)
High school graduate	---------	-0.085 ***	---------	-0.096 ***	---------	0.105 *	---------	0.089	---------	-0.160 ***	---------	-0.180 ***
	---------	(0.030)	---------	(0.034)	---------	(0.059)	---------	(0.064)	---------	(0.035)	---------	(0.040)
Number of jobs	7,073		5,569		7,073		5,569		7,073		5,569	
Number of exits	4,657		3,647		1,244		1,073		3,354		2,529	
Number of observations	85,861		68,708		85,861		68,708		85,861		68,708	

Note: Standard errors in parentheses are indicated as follows: *= significant at 10 per cent; ** = significant at 5 per cent; *** = significant at 1 per cent.

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

	All Job Exits				Job to Job Exits				Exits to Unemployment			
	Control vs. SSP		Control vs. Take Up		Control vs. SSP		Control vs. Take Up		Control vs. SSP		Control vs. Take Up	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
In SSP group	0.097	0.091	0.034	0.038	0.177	0.180	0.345 **	0.344 **	0.080	0.069	-0.095	-0.093
	(0.069)	(0.069)	(0.082)	(0.082)	(0.135)	(0.135)	(0.150)	(0.150)	(0.083)	(0.083)	(0.103)	(0.103)
Female	---------	-0.188	---------	-0.264	---------	-0.351	---------	-0.418	---------	-0.102	---------	-0.164
	---------	(0.168)	---------	(0.190)	---------	(0.302)	---------	(0.333)	---------	(0.211)	---------	(0.245)
Speaks French	---------	0.068	---------	0.058	---------	0.517 ***	---------	0.445 **	---------	-0.112	---------	-0.124
	---------	(0.093)	---------	(0.109)	---------	(0.160)	---------	(0.181)	---------	(0.119)	---------	(0.143)
Never married	---------	0.197 ***	---------	0.237 ***	---------	0.025	---------	0.065	---------	0.232 ***	---------	0.304 ***
	---------	(0.073)	---------	(0.085)	---------	(0.142)	---------	(0.158)	---------	(0.088)	---------	(0.105)
Number of children	---------	0.087 **	---------	0.130 **	---------	-0.014	---------	0.011	---------	0.104 **	---------	0.158 ***
	---------	(0.044)	---------	(0.051)	---------	(0.092)	---------	(0.104)	---------	(0.052)	---------	(0.061)
High school graduate	---------	-0.080	---------	-0.102	---------	0.100	---------	0.078	---------	-0.162 *	---------	-0.198 **
	---------	(0.070)	---------	(0.082)	---------	(0.139)	---------	(0.156)	---------	(0.084)	---------	(0.100)
Number of jobs	1,778		1,338		1,778		1,338		1,778		1,338	
Number of Exits	854		631		224		180		586		417	
Number of Observations	10,567		8,091		10,567		8,091		10,567		8,091	

Note: Standard errors in parentheses are indicated as follows: *= significant at 10 per cent; ** = significant at 5 per cent; *** = significant at 1 per cent.

IMPACT ON WAGE GROWTH

Recall that the "Within-Job Wage Growth" section of this paper (see pages 5–9), shows that the effect of the wage subsidy on within-job wage growth depends on the functional form of the wage subsidy. If a person is indifferent between two jobs before the subsidy, then she would prefer the job with higher wage growth if the post-transfer wage is a convex function of the pre-subsidy wage, but she would be indifferent if the subsidy is linear. While both the SSP supplement and income assistance (IA) transfers have non-linear sections, the argument is that both are linear in their effective ranges. This implies that no significant differences in within-job wage growth between program group members and control group members should be expected. Turning to between-job wage growth, it is shown that the effect of the subsidy depends on the functional form of the wage offer distribution. Since there are no priors on the functional form of this distribution, the effect of the supplement on between-job wage growth remains an empirical question.

Tabular Evidence

Table 4 shows mean wage growth within jobs and between jobs for program group members and control group members. Since the group of individuals experiencing an intervening spell of non-employment between jobs includes persons who quit or were involuntarily terminated, between-job wage growth is separated into direct transitions from one job to the next and transitions with an intervening spell of non-employment.

The top panel shows monthly within-job wage growth. The point estimates of the means for program group members (0.0028) and control group members (0.0027) are very similar. Even when the program group members are restricted to those taking up the program, wage growth is still very similar to that of the control group members (0.0025). This is consistent with the prediction mentioned earlier that this wage supplement will not affect within-job wage growth. Program group members perceived that they would continue to receive the supplement in their new jobs, whether or not they experienced wage growth, which is a reasonable presumption given that the target earnings in the SSP would not be reached until wages in a 30 hour a week job rose to $20.

The bottom panel, however, indicates that the program group members have substantially larger wage growth between jobs than do the control group members. The average wage change between jobs for a member of the control group is 0.008, or less than one per cent. For program group members, the corresponding mean wage growth between jobs is 0.044. Thus, program group members have between-job wage growth that is more than five times as large as that of the control group members. This reflects larger wage gains both in job-to-job transitions and in transitions that include an intervening spell of non-employment. It should, however, be noted that the small number of job changes yields sufficiently large standard errors that even these large differences could have occurred by chance.

Table 4: Mean Monthly Wage Growth (in Log Wages)

			Program Group		
	All (1)	Control Group (IA) (2)	SSP Eligible (3)	Took Up SSP (4)	Did Not Take Up SSP (4)
Within-job wage changes					
Within-job wage change	0.0028	0.0027	0.0028	0.0025	0.0033
	(0.0011)	(0.0015)	(0.0017)	(0.0027)	(0.0017)
Between-job wage changes					
All between-job wage change	0.0279	0.0081	0.0442	0.0245	0.0804
	(0.0219)	(0.0290)	(0.0322)	(0.0409)	(0.0518)
Job-to-job exits	0.0242	0.0038	0.0409	0.0393	0.0446
	(0.0356)	(0.0424)	(0.0547)	(0.0689)	(0.0877)
Intervening spell of non-employment	0.0315	0.0123	0.0476	0.0070	0.1068
	(0.0259)	(0.0399)	(0.0340)	(0.0374)	(0.0630)

Note: Standard errors are in parentheses.

ESTIMATED MODEL

This section uses the methodology developed earlier to estimate returns to experience (β_x), tenure ($\widetilde{\beta}_T$), and improved job match (α_1).[38] In this framework, within-job wage growth is the sum of returns to tenure and experience. Between-job wage growth is the difference between improved job match and the lost returns to job-specific tenure. Using this framework requires additional assumptions but has the offsetting benefit of allowing the experimental findings to be interpreted in the light of an explicit model. For example, the larger between-job wage changes for program group members shown in Table 4 does not necessarily indicate that program group members are finding better matches than control group members when moving to new jobs. Since program group members have higher hazards of leaving their current jobs for other jobs, they have accumulated less tenure by the time they change jobs. As a result, they lose less in forgone returns to job-specific tenure. Therefore, the larger between-job wage growth of program group members may partially reflect their lower tenure as well larger improvements in job match. Put another way, the unconditional mean of between-job wage changes shown in Table 4 does not hold tenure constant. As a result it ignores the forgone returns to tenure and understates the improvement in job match.

Table 5 presents the estimated parameters. Columns 1 and 2 show coefficients for all persons (i.e. program group and control group members combined). Column 1 shows the returns to tenure, experience, experience squared, and improved job match. Column 2 adds the change in wage associated with going from a part-time job to a full-time job. Columns 3 and 4 add a set of interactions that test the differences between program group and control group members. The remaining two columns test differences between control group members and the program group members who took up the program. At the bottom of each column are the F-statistics used for joint tests of program group / control group differences.

[38]Throughout this section the previous literature in referring to $\widetilde{\beta}_T$ as returns to tenure is followed. However, the reader should keep in mind that this term differs from β_T if α_T is non-zero.

Table 5: Parameter Estimates

	All		Control vs. SSP		Control vs. Take Up	
	(1)	(2)	(3)	(4)	(5)	(6)
Tenure	0.005 **	0.004 **	0.003	0.002	0.003	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Experience	-0.001	-0.001	-0.002	-0.002	-0.002	-0.002
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
Experience ^{2} *10 ^{3}	-0.003	-0.003	0.003	0.003	0.003	0.003
	(0.010)	(0.010)	(0.011)	(0.011)	(0.011)	(0.011)
Job Match	0.057 ***	0.054 ***	0.022	0.017	0.022	0.017
	(0.019)	(0.019)	(0.026)	(0.026)	(0.026)	(0.026)
Full Time	---------	-0.052 *	---------	-0.070	---------	-0.070
	---------	(0.027)	---------	(0.046)	---------	(0.046)
SSP*Tenure	---------	---------	0.004	0.004	---------	---------
	---------	---------	(0.004)	(0.004)	---------	---------
SSP*Experience	---------	---------	0.002	0.002	---------	---------
	---------	---------	(0.006)	(0.006)	---------	---------
SSP*(Experience ^{2} *10 ^{3})	---------	---------	-0.010	-0.010	---------	---------
	---------	---------	(0.018)	(0.018)	---------	---------
SSP*Job match	---------	---------	0.065 *	0.069 *	---------	---------
	---------	---------	(0.037)	(0.037)	---------	---------
SSP*Fulltime	---------	---------	---------	0.030	---------	---------
	---------	---------	---------	(0.055)	---------	---------
Takeup*Tenure	---------	---------	---------	---------	0.007	0.007 *
	---------	---------	---------	---------	(0.004)	(0.004)
Takeup*Experience	---------	---------	---------	---------	0.001	0.001
	---------	---------	---------	---------	(0.006)	(0.006)
Takeup*(Experience ^{2} *10 ^{3})	---------	---------	---------	---------	-0.013	-0.013
	---------	---------	---------	---------	(0.017)	(0.018)
Takeup*Job Match	---------	---------	---------	---------	0.070	0.075 *
	---------	---------	---------	---------	(0.042)	(0.042)
Takeup*Fulltime	---------	---------	---------	---------	---------	0.054
	---------	---------	---------	---------	---------	(0.054)
F(experience term)	0.45	0.40	-------	-------	-------	-------
Prob	0.64	0.67	-------	-------	-------	-------
F(tenure)	6.15	5.63	-------	-------	-------	-------
Prob	0.01	0.02	-------	-------	-------	-------
F(tenure and experience terms)	2.28	2.09	-------	-------	-------	-------
Prob	0.08	0.10	-------	-------	-------	-------
F(interactions)	-------	-------	1.05	1.00	1.28	1.23
Prob	-------	-------	0.38	0.42	0.27	0.29
F(experience interactions)	-------	-------	0.24	0.21	0.53	0.51
Prob	-------	-------	0.79	0.81	0.59	0.60
F(tenure interactions)	-------	-------	1.20	1.22	2.64	2.80
Prob	-------	-------	0.27	0.27	0.10	0.09
F(tenure and experience interactions)	-------	-------	0.56	0.55	1.22	1.27
Prob	-------	-------	0.64	0.65	0.30	0.28
F(job match interaction)	-------	-------	3.11	3.57	2.69	3.15
Prob	-------	-------	1.05	1.00	1.28	1.23
Number of jobs	892	892	892	892	720	720
R-squared	0.0109	0.0152	0.0148	0.0198	0.0156	0.0206

Notes: Standard errors are in parentheses.
Coefficient estimates are significant at the 10 per cent (*), 5 per cent (**)
or 1 per cent (***) levels.

Column 1 captures two key factors affecting wage growth of these former welfare recipients. First, tenure leads to higher wages but accumulated experience does not. The significant coefficient on tenure, but not experience, implies that all within-job wage growth reflects returns to tenure.[39] Since returns to tenure are lost when switching to a new job, the positive returns to tenure do not contribute to higher wages in subsequent jobs. Second, there are large and significant gains to switching jobs. In terms of the analytical framework, the expected wage gains above the reservation wage are large. Column 1 indicates that the average increase in the job match component is 5.7 per cent.[40] This large and statistically significant coefficient implies that wages would grow by 5.7 per cent when finding an acceptable new job were it not for the loss of returns to tenure.

Column 2 adds a dummy variable that is equal to one when going from a part-time to a full-time job. This statistically significant negative coefficient of 0.052 indicates that, on average, there is a premium paid to part-time work for the jobs held by the combined program and control groups. This somewhat surprising result is consistent with a similar finding in the Survey of Income and Program Participation (SIPP) data for less-educated women.[41]

Columns 3 and 4 test whether the program group members had jobs with different rewards to tenure, experience, or job match than control group members. The F-statistics on the interactions between the experimental dummy and tenure, experience, and experience squared are not significant at conventional levels. This implies that even after controlling for experience and tenure, the within-job wage growth (which is the sum of returns to experience and tenure) is not different between program group members and control group members. The fact that those eligible for earnings supplements had similar within-job wage growth to those who did not is consistent with SSP providing a linear mapping between pre- and post-supplement earnings.

The coefficient of 0.069 on the interaction between the job match component and the experimental dummy is large and statistically significant at conventional levels. Control group members experience a 1.7 per cent increase in the job match component when they move to a new job while the corresponding figure for program group members is 8.6 per cent. This is consistent with the large differences in between-job wage growth in Table 4, but now it can be seen that the difference reflects differences in the improvement in the job match component, not differences in lost returns to tenure.

The results of this statistical model give insight into the factors affecting the within- and between-job wage growth reported earlier. The statistical model shows that returns to tenure and experience are not statistically different for program group members and control group members. Since within-job wage growth is the sum of these statistically insignificant coefficients, it is not surprising that the difference in within-job wage growth in Table 4 is also insignificant. The large difference in between-job wage growth in Table 4 is consistent with the large gains from improved job match shown in Table 5. The fact that estimated returns

[39] A joint test that the coefficients on experience and experience squared are both zero cannot be rejected at conventional levels.

[40] The improvement in job match is given by α_1 in the empirical specification. This is the constant found in the equation reflecting the change in starting wages and part of the constant (along with β_x) in the between-job wage change equation.

[41] See Connolly & Gottschalk (2000).

to tenure are small and insignificant means that between-job wage growth is not diminished from losing the returns to accumulated tenure.[42]

In summary, the analysis reveals that there are large and significant differences in some but not all of the underlying parameters that generate within- and between-job wage growth. Program group members are in jobs with similar returns to tenure and experience as control group members, though when the contrast is limited to program group members who took up the program there is a statistically significant difference in returns to tenure. The largest differences are, however, in the change in the job match component. All program group members experience substantially larger increases in wages when moving to new jobs as a result of the change in this component.

EXPERIMENTAL VERSUS NON-EXPERIMENTAL FINDINGS

The results in this paper are largely non-experimental in the sense that they require assumptions beyond random assignment. The only purely experimental finding is that job duration is shorter for program group members than control group members.[43] This finding is consistent with the prediction that the supplement lowers the reservation wage for the initial job, which makes it more likely that program group members would move quickly to new jobs with higher wages. The findings on wage growth presented in this paper are consistent with this prediction, but these findings are based on the subset of jobs for which there is information on starting and ending wages. Since this selection is dictated by a shortcoming in the data, not by random assignment, this analysis of wage growth is non-experimental.

The use of the wage sample requires that the selection has the same affect on mean wage growth of the control group members as it has on the program group members. This is weaker than requiring that the selection has no affect on either group. In fact, the analysis shows that the proportion employed at the baseline interview is substantially higher in the wage sample than in the full sample. The proportion employed is, however, higher for control group as well as program group members. As a result, program group / control group differences are similar in the two samples. This carries over to job duration where program group members have shorter durations than control group members, both in the wage and full samples. While it is not possible to know whether the program group / control group differences in wage growth

[42] The insignificant difference in between-job wage growth can be reconciled with the statistically significant difference in job match by recognizing that the sampling variance of between-job wage growth depends on the sampling variances of both the estimates of the job match component, $\hat{\alpha}$, and the sampling variance of estimates of returns to tenure, $\hat{\beta}_T$:

$$
\begin{aligned}
var(\hat{\Delta}_b|T_i) &= var(\hat{\alpha} - \hat{\beta}_T T_i) && (24)\\
&= var(\hat{\alpha}) + T_i^2\, var(\hat{\beta}_T) + T_i\, cov(\hat{\alpha}, \hat{\beta}_T). && (25)
\end{aligned}
$$

Thus, the variance of within-job wage growth can be large even if the job match component is estimated precisely.

[43] Since the Cox proportional models estimated in this paper assume that control group and program group members have the same baseline hazard, Kaplan-Meier duration models that are fully non-parametric are also estimated. These models also show statistically significant higher hazards of job-to-job transitions for program group members than control group members.

would also be similar in the full sample as in the wage sample, this is at least supporting evidence that selection is not affecting the results.

The final set of estimates move from conditional means to estimates of parameters of standard log wage equations. This requires further assumptions about the form of observables and unobservables in the wage equation. Following a long line of research, the focus here is on the potential endogeneity of the job match component. As the model indicates, persons are more likely to stay in jobs that offer good matches with their skills, which would lead to a spurious correlation between wages and experience or tenure. Therefore, all the equations are estimated in first differences, which eliminates the job match component as well as all unmeasured characteristics of the individual that do not vary with time. While differencing eliminates the most obvious source of endogeneity, it does not ensure that all forms of endogeneity have been eliminated. The cost of this non-experimental approach is that it requires the further assumptions. The benefit is that it allows for an estimation of the underlying parameters that generate within- and between-job wage growth.

Conclusions

This paper began by asking whether wage or earnings subsidies 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 subsidies can increase wage growth as well as increasing labour market activity, then this program has dual benefits.

The analytical framework presented in this paper indicates that there are sound economic reasons to think that a wage or earning subsidy could affect the type of job a person accepts and the length of time the person stays in each job. Whether the subsidy 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 program. Jobs with low starting wages but high wage growth receive larger subsidies in the initial periods but smaller subsidies in the later periods when wages are high. Hence, the subsidy makes these jobs more or less attractive depending on how fast the subsidy declines as earnings increase. If the subsidy diminishes rapidly as earnings rise, then this makes jobs with wage growth less attractive. It would be better to take a job with a flatter wage profile in order to avoid having the subsidy cut back sharply as wages grow. Conversely, if the subsidy program is designed so that post-subsidy earnings rise more quickly than pre-subsidy earnings, then the program makes the high wage growth job more attractive. The intermediate case is a subsidy that is neutral with respect to wage growth. It can be argued that the Self-Sufficiency Project (SSP) is largely neutral to wage growth. It is, therefore, not expected to affect within-job wage growth.

Wage and earnings subsidies can also affect the decision of whether to accept a job and continue to search while employed rather than continuing to search while unemployed. The intuition here is clear. Subsidies increase the opportunity cost of not taking a job. Therefore, participants are more likely to search for a better job while employed than while unemployed. As a result of being less demanding in the choice of their initial jobs, participants are more likely to find that new opportunities dominate their current jobs. Hence they are more likely to switch jobs and raise their wages in the process.

More formally, this paper shows that wage subsidies can affect wage growth but that the effects depend on the structure of the subsidy and the functional form of the wage offer distribution. If the subsidy is a concave mapping of pre-subsidy wages into post-subsidy wages, then this will make jobs with wage growth less attractive since the period of high wages will yield a disproportionately small subsidy. Convex mappings make jobs with wage growth more attractive, and linear programs have no affect on the choice of jobs. The subsidy is expected to have an effect on search behavior and the resulting between-job wage growth. Specifically, the subsidy is expected to increase on-the-job search and to alter the expected change in wages when switching jobs. The direction of the change, however, depends on the shape of the wage offer distribution.

These predictions are explored using two samples from SSP. The first includes all members of the program and control groups.[44] Information on the starting and end dates for all jobs is available for this sample. It can, therefore, be used to test the prediction that SSP increases job-to-job mobility and thus reduces job duration. While the primary sample includes information on starting and ending dates for all jobs, it includes information on starting and ending wages only for the subset of jobs that started after the 18-month interview. The analysis is, therefore, limited to this subsample when examining wage changes. While this is a serious limitation imposed by the data, it should be borne in mind that the conceptual model is as applicable to this subset of jobs as to jobs that started earlier. Therefore, if there is an impact of the program, it would be expected to be found in this limited sample as well as in the larger sample.

The analysis shows that program group members are more likely to leave their jobs and move directly to other jobs than are control group members. Program group members are, however, less likely to leave their jobs and become unemployed. This is consistent with the theoretical predictions. The program induces program group members to be less demanding about their first jobs so they are more likely to be successful in finding better jobs once they start searching on their new jobs. A member of the program group is, however, less likely to leave a job without another job lined up since the opportunity cost of becoming unemployed is higher as a result of the supplement.

This paper's findings about wage changes are also consistent with the theory. Wage growth within jobs is nearly identical for program group members and control group members. This is consistent with the structure of the SSP supplement which makes it neutral with respect to within-job wage growth. Between-job wage growth is, however, substantially higher for program group members than control group members. The statistical model estimated in this paper shows that this largely reflects program group members experiencing larger increases in the quality of the job match when moving to new jobs.

In summary, wage or earnings subsidies can be designed to affect wage growth and job turnover. The design of the SSP supplement did not lead to greater within-job wage growth, but it did lead workers to increase their wages through more job transitions, often to higher-paying jobs. Furthermore, the wage gains from these transitions were higher for program group members than control group members.

[44]The SSP Plus sample is excluded since it received auxilliary services.

Appendix A

This appendix shows that when the subsidy is not available for the full duration of the job, then $\tilde{w}_S$ may increase or decrease. The subsidy is, however, still predicted to increase the probability that the job with wage growth will be chosen. Consider a job that is expected to last through period, T, which is beyond the end of the subsidized period T_o (i.e., $T_o > T$). Again, the constant wage equivalent, $\widetilde{w}_s$, that has the same value as the job with wage growth, is found.

Since the subsidy is available only through $T_o < T$, the expression for each job can be written as the subsidized wage that would be earned if the subsidy were available for the full duration of the job, minus the subsidy that would not be received between T and T_o because the job outlasted the subsidy:

$$
\begin{aligned}
& T\widetilde{w}_s - [.5(w_{\max} - \widetilde{w_s})\,(T - T_o)] \\
= \;& .5\int_o^T (w_{\max} + g(t))dt - \left[.5\int_{T_o}^T (w_{\max} - g(t))dt\right] && \text{(A-1)} \\
= \;& .5(Tw_{\max} + G(T)) - \left[.5\int_{T_o}^T (w_{\max} - g(t))\,dt\right], && \text{(A-2)}
\end{aligned}
$$

where the first terms on the left and right sides are the flows if the subsidy were available for all T months. The terms in brackets subtract out the subsidy between T_o and T. Dividing by T, recognizing that $G(T)/T$ is equal to $\tilde{w}$, and simplifying yields the following:

$$
\widetilde{w}_s = .5\,(w_{\max} + \widetilde{w}) + (.5/T)\int_{T_o}^T (g(t) - \tilde{w}_s)\,dt. \qquad \text{(A-3)}
$$

If $T \leq T_0,$ then the second expression on the right is equal to zero. Whether $\widetilde{w}_s$ increases or decreases when $T_o > T$ depends on the sign of the second term on the right. The integral on the right-hand side is the difference between the average wage in the wage growth job and the average wage in the non-wage growth job in the period after the subsidy is removed, $(T - T_o)$. Therefore, this expression is always positive if wages grow monotonically.

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