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The Effects of Human Capital and Earnings Supplements on Income Assistance Dependence in Canada

The Self-Sufficiency Project

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Abstract

This paper uses unique data from a social experiment that took place in Canada in the 1990s to analyze the effects of educational attainment, work experience, labour market training, and earnings supplements on income assistance (IA) dependence. The main objective of the empirical analysis is to estimate the effects of these factors on both exit and re-entry rates in IA. The empirical results suggest that, contrary to conventional wisdom, formal education has no significant causal effect on either the exit rates or the re-entry rates. Work experience is found to significantly reduce the length of welfare spells and the risk of returning to IA. Finally, economic incentives are important, as receiving a generous earnings supplement significantly increases the probability of leaving IA and reduces the probability of returning to IA.

Executive Summary

This paper analyzes the effects of human capital and earnings supplements on the dynamics of welfare use in Canada using administrative data on welfare spells combined with survey information from the Self-Sufficiency Project (SSP) Applicant study. The SSP for welfare applicants offered a substantial earnings supplement to new welfare applicants who remained on welfare for one year and, in the subsequent year, left welfare for full-time employment. Supplement recipients could receive the supplement for up to three years during the months in which they worked full time.

The results in this paper document that there is substantial educational upgrading among welfare applicants in the years following their entry into welfare. For example, 35 per cent of those who had less than a high school diploma at the initial interview upgraded their education during the following six years. Among high school graduates, educational upgrading was even more common. This contrasts with the common assumption in research that education levels do not change over time. It is possible that educational upgrading is a more important determinant of welfare exits than the observed level of education at the time of entry into welfare. Consequently, assuming that education is constant may overestimate (underestimate) the effects of variables that are positively (negatively) correlated with educational upgrading (such as initial educational attainment) and would therefore lead to erroneous policy conclusions.

The substantial educational upgrading observed in the data may also imply that educational programs for welfare applicants will have a number of “windfall” gainers: those who appear to have been helped to upgrade their education / job skills by an education program but would have upgraded their education / job skills even without the program. As a consequence, the education program is less effective than it would appear if one assessed the program by simply looking at the education levels of participants before and after the program. As a result, it may be useful to re-examine the cost-effectiveness of educational programs for welfare recipients in light of these windfall effects. In addition, one should be skeptical of educational programs for welfare recipients that justify their results on the basis of simple before-and-after comparisons.

The SSP Applicant data also reveals that there are no significant differences in skill enhancements obtained at educational institutions between the respondents in the control group and the respondents who eventually received the earnings supplement. Thus, despite higher opportunity costs of obtaining education for respondents receiving earnings supplements, the data suggests that for this group of applicants, earnings supplements do not carry a cost in terms of lowering formal education. The policy implication is that earnings supplements for some welfare recipients do not reduce their education / job skills and, consequently, long-term earnings (other factors held equal).

Conventional wisdom asserts that individuals with more education tend to leave welfare more quickly and re-enter welfare more slowly. It is thus conventionally concluded that increased formal education caused the reduced welfare usage, and, consequently, programs to increase the formal educational levels of current welfare recipients would lead them to leave welfare earlier and return to welfare less frequently. This paper shows that this conventional wisdom is false. Higher formal education does not cause lower welfare use. Instead, favourable unobserved characteristics — such as labour-market ability, motivation, and preferences — cause some welfare clients to have both higher levels of education and shorter, less frequent spells on welfare. Consequently, a policy aimed at improving the educational attainments of former welfare recipients may have only limited effects on the reliance on income assistance.

The effects of work-related training showed the expected signs but were generally not significant at conventional levels. Furthermore, work experience, in particular full-time work experience, significantly increased exit rates and reduced re-entry rates. This positive effect may be due to skills upgrading on the job or to changes in preferences and labour-market attachment during the employment tenure. These results are quite intuitive as they suggest that specific skills have larger effects on welfare exits and re-entries than general training (such as high school English, social studies, history, and mathematics) given the labour market in which these individuals generally compete.

The results also indicate that economic incentives matter. The provision of a generous earnings supplement significantly reduces time spent on welfare, both by increasing the probability of leaving income assistance and by reducing the risk of returning to welfare. These positive effects are however limited to the time periods when respondents received the supplement. While time limits are likely to reduce the long-term impacts, even short-term interventions, combined with work requirements, may have long-lasting effects through the positive effects associated with the additional work experience obtained during the period when the policy was in effect.

Introduction

Since the mid-1990s both Canada and the United States have experienced large declines in the number of welfare recipients. The reduction in welfare caseloads in both countries has primarily been attributed to improved economic conditions over the period and to changes in the welfare system.[1] A major reason for reforming the welfare system, both in Canada and in the United States, was to encourage transitions from welfare to work. One way to achieve this is to provide economic incentives in order to make work more worthwhile. Alternatively, policy-makers can provide training opportunities to welfare recipients in order to increase their labour-market skills and consequently their wage offers. Most of the existing work on evaluating the welfare reforms has focused on the effects of providing economic incentives (e.g. Blank, 2002; Card & Hyslop, 2005; Fortin, Fougère, & Lacroix, 2002; Fortin, Lacroix, & Drolet 2004; Fortin, Lacroix, & Thibault, 1999; Meyer & Rosenbaum, 2001; Michalopoulos, Robins, & Card, forthcoming; Moffitt, 1999; Stewart & Dooley, 1999; and Zabel, Schwartz, & Donald, 2004) while less attention has been paid to the effects of increasing participants' stock of labour-market skills.[2]

It is possible that even if economic incentives, such as lowering the implicit tax rates on earnings and changing the benefit levels, may increase the transition rates out of welfare, the implied reductions in welfare use may be short-lived. This will be true if former recipients find employment in minimum-wage jobs with little or no skill production (and wage growth) and if the incentive package is not permanent. On the other hand, temporary policy interventions may have long-term effects if the employment experiences generate new skills — or change preferences and attitudes — so that even when the short-term economic incentives are terminated, former recipients find themselves more attached to the labour market and therefore less likely to return to welfare. Other forms of skills improvement, such as educational upgrading or participation in training activities, may also reduce welfare caseloads by increasing the likelihood of both higher wage offers and better job matches. Considering the constantly changing nature of the labour market, with technological changes and increasing demand for skilled workers, it is likely that the production of skills, either on the job or off the job, will be an essential ingredient in any successful policy aiming at reducing welfare use.

Existing research that has focused on economic incentives, usually measured by the maximum benefit levels or by the implicit tax rates, has very often found that such incentives

[1]The United States reform of 1996 — *The Personal Responsibility and Work Opportunity Reconciliation Act* — imposed, among other things, time limits on the receipt of welfare and denied non-citizens who arrived after 1996 the right to receive most types of public assistance. In Canada welfare is a provincial responsibility, although the federal government assumes a portion of the program costs. In 1996 the *Canada Health and Social Transfer Act* replaced the Canada Assistance Plan, which implied a substantial reduction in the dollar value of the federal transfers to the provinces.

[2]Although results from previous studies indicate the importance of education in reducing time on welfare, this has not been the primary focus of the analysis. One exception is Barrett (2000), who analyzed the effect of education on welfare dependence in Canada. Few studies, if any, have investigated the effects of other forms of skill production, such as job training, on welfare use. The large number of studies devoted to evaluating active labour-market programs that exist (see for instance the survey of empirical findings in Heckman, Lalonde, & Smith, 1999) are typically not set in the context of welfare dependence. A number of recent studies have also evaluated the effects of time limits on welfare use (e.g. Grogger, 2002, 2003, 2004; and Grogger & Michalopoulos, 2003).

matter. Studies using Canadian data include Fortin et al. (1999) who report that the exit rate from welfare in Quebec decreases as the implicit tax rate increases and Stewart and Dooley (1999) who find that "higher welfare benefits are strongly associated with a lower hazard" (p. S61). The final report on the Canadian Self-Sufficiency Project (SSP) for welfare applicants by Ford, Gyarmati, Foley, and Tattrie (2003) finds that, from the second year onwards, SSP significantly reduced welfare participation and welfare payments and increased full-time employment.[3] Recently, Michalopoulos et al. (forthcoming), using data from the SSP Applicant study surveys through the first 30 months, provide further evidence on the positive effects of financial incentives in the form of earnings supplements on full-time employment and income. Zabel et al. (2004), using data from the SSP Recipient study, report significant short-term effects of the income supplement on both welfare durations and non-welfare durations. They also find that there is a positive long-term effect of the income supplement on employment rates for the subgroup of program group members who received the supplement (the "take-up" group). Finally, Card and Hyslop (2005), also using data from the SSP Recipient study, find that while the earnings supplement reduced welfare use in the short term, there were only limited long-term impacts from the supplement on either wages or welfare use.

Most previous research on welfare use includes controls for educational attainment and generally reports strong and significant effects of education on the exit rate from welfare. However, as argued by Barrett (2000), the common treatment of educational attainment as a single continuous variable measuring years of schooling is restrictive, since it does not allow for non-linear effects of education. Barrett (2000), using administrative records on welfare use in New Brunswick for the period 1986–1993, finds that the linearity assumption can be rejected and that higher education is associated with higher exit rates for women, while education beyond high school is not associated with higher exit rates for men. In addition to the limited treatment of educational attainment, most previous work often ignores other forms of human capital acquisitions, such as work-related training and work experience. Moreover, as far as I know, all existing work on the effects of education and other forms of human capital assume that measures of human capital or labour-market skills are exogenous and uncorrelated with unobservable effects, such as labour-market ability and preferences. This is obviously a very strong assumption that is unlikely to hold in any empirical analysis. The data in this paper will allow me to formally address this endogeneity issue and to obtain causal effects of education on welfare utilization.

While most of the earlier work on the dynamics of welfare use has focused on the exit rate out of welfare, a few recent studies have also provided estimates of the re-entry rates into welfare (see Barrett & Cragg, 1998; Card & Hyslop, 2005; Fortin et al., 1999; Stewart & Dooley, 1999; and Zabel et al., 2004 for studies using Canadian data). It is important to consider both exit and re-entry probabilities in order to accurately measure total time on

[3]The Self-Sufficiency Project consists of three different studies: The SSP Recipient study, SSP Plus, and the SSP Applicant study. The SSP Recipient study targeted individuals who were single parents, who were 19 years of age or older, who received income assistance (IA) payments when first interviewed, and who — at this interview — had received IA payments at least 11 months of the prior 12 months. SSP Plus focused on a small group of IA recipients in New Brunswick who were offered a range of employment services in addition to the earnings supplement. Finally, the SSP Applicant study targeted a group of people in British Columbia who had recently been approved to receive IA after having been away from the IA program for at least six months.

welfare and also to correctly assess the impact of human capital and other observable characteristics on total exposure towards the welfare system. For instance, a single spell model that considers only the exit rate from welfare may seriously underestimate the effect of education and other forms of human capital if these characteristics not only increase the likelihood of leaving welfare, but also prevent former welfare recipients from returning to welfare. Thus, it is important to recognize the possibility that individuals who have left welfare may soon fall back into welfare use, after controlling for observed characteristics. Furthermore, focusing only on single spells of welfare, as opposed to multiple spells, may significantly underestimate the total time spent on welfare. While long single welfare spells obviously imply that a substantial fraction of time is spent receiving IA, repeated welfare spells with intermittent periods of no IA receipt also lead to substantial periods on welfare. It is also necessary to allow for correlation between exit and re-entry probabilities to accurately estimate total time spent in poverty. For example, a person who has experienced a long welfare spell and then re-enters welfare may be likely to experience another long welfare spell. Moreover, it may be the case that individuals with particularly high exit rates also have low re-entry rates. In both these cases, assuming independence between the transition rates will bias the results (this form of bias has been referred to as "dynamic self-selection bias" by Ham & Lalonde, 1996, and others), and the empirical specification in this paper will address this issue.

In the previous literature on welfare dynamics, there is generally a limited treatment of the exit state. In most cases, data limitations prevent a deeper analysis of the destination state and of the process leading up to an exit to a particular state. Blank (1989), using data from the Seattle/Denver Income Maintenance Experiments for the period 1970–1976, distinguishes three (out of many) reasons for leaving welfare (Aid to Families with Dependent Children [AFDC]): marriage, increase in earnings while remaining single, and other reasons (including increases in non-earned income). She finds that the hazard rate associated with leaving welfare via marriage is the lowest and the probability of leaving AFDC through increases in earnings is the greatest. However, her model assumes independence between the hazard rates and that they are not affected by unobserved heterogeneity. In the empirical analysis in this paper, I will estimate a model that builds on Blank's (1989) paper but that also attempts to allow for unobserved heterogeneity and correlations between the different hazard rates.

The analysis presented in this paper is based on data from the Self-Sufficiency Project's Applicant study. This data set consists of about 3,000 single parents from British Columbia who started a new welfare spell between February 1994 and February 1995.[4] There are a number of properties with this data set that make it suitable for the analysis of welfare dependence. First, information on monthly welfare use is obtained from provincial IA records, ensuring a high degree of accuracy on the time pattern of welfare use. Secondly, the provincial IA records were supplemented with information obtained during four interviews over a six-year period, providing details on educational attainment, work-related training, work experience, and other characteristics of the survey respondents. Thirdly, as opposed to most existing administrative data on welfare use, the respondents remain in the data after the initial welfare spell has been completed. This provides an opportunity to analyze the

[4]Thus, the SSP Applicant study is a flow sample as opposed to many other data sets used for analyzing welfare durations, including the SSP Recipient study, which are stock samples.

possibility of re-entering welfare as well as the reasons for leaving welfare. Finally, respondents were initially randomly assigned into either a program group, whose members could eventually become eligible for a substantial income supplement conditional on taking up full-time employment, or into a control group, whose members could not receive the income supplement. However, many respondents who were assigned to the program group never took up the treatment, either because they left welfare before they became potentially eligible or because they were unable to secure full-time employment within the 12-month "take-up" period.[5] This particular feature of the data necessitates non-experimental methods to correctly assess the effect of the supplement on durations of both welfare and non-welfare spells.

The objectives with this paper are (i) to provide a detailed description of the human capital stock (including formal education, training, and work experience) among welfare participants at the beginning of the survey (the baseline interview); (ii) to provide a description on human capital accumulation over the sample period (72 months); (iii) to estimate the effects of different types of accumulated skills on both exit and re-entry rates using a methodology that controls for unobserved heterogeneity, allows for dependence between the different transition rates, and accounts for possible endogeneity in the decisions to take-up the earnings supplement; (iv) to estimate the effect of income supplements on total time on welfare and compare this with the effects of accumulated human capital; and (v) to jointly estimate the probability of welfare exits, distinguishing between three possible "reasons": marriage, increased earnings, and other reasons.

The empirical results suggest that educational attainment may not be a significant determinant of either the exit rate from IA or the re-entry rate. In a specification that assumes that educational attainment is independent of unobserved effects, education beyond high school is found to significantly increase the probability of leaving IA. However, in a less restrictive specification where education is allowed to be correlated with the unobserved effects, the estimates associated with educational attainment are not significant at conventional levels. This suggests that, even if educational attainments are strongly correlated with welfare use, those who are more likely to leave welfare (and less likely to re-enter welfare) are also more educated. Thus, education serves as a sorting device and it has little or no causal effect on the probability of leaving (or re-entering) IA. Considering other forms of human capital, the results indicate that labour-market training (such as work-related correspondence courses, on-the-job training, and apprenticeship training) has a positive, but only marginally significant, effect on the probability of leaving IA. The duration of current full-time employment spells significantly reduces the risk of returning to IA. It is also found that economic incentives matter. The provision of a generous earnings supplement significantly increases the probability of leaving IA and reduces the probability of returning to IA. To gauge the economic importance of earnings supplements and labour-market skills (either through training or experience), the estimates from the most general model specification were used to generate counterfactual outcomes for hypothetical control group respondents. Holding everything (that is observed) constant, provision of an earnings

[5]To become potentially eligible for the income supplement, program group members first had to remain on welfare for at least 12 months. After having received welfare for 12 months, they would start receiving the earnings supplement if they found full-time employment within the next 12 months. Respondents could receive the income supplement for a maximum of 36 months, and they also had the option to give up the supplement at any time and return to welfare.

supplement for 36 months reduced total time on welfare over a six-year period by up to 43 per cent. Provision of work-related training, assuming it was completed during the first year, reduced welfare use by about 8 per cent over a six-year period. Assuming that everyone worked full time the last five years over the six-year period, it reduced welfare use by almost 11 per cent. While these figures are highly dependent on conditions and assumptions made for the simulation exercise and should be used with care, they provide some insight into the magnitudes and effects of the estimated parameters from a relatively complex empirical model.

For the specification that distinguishes between different destination states, the results indicate that educational attainment is not significantly related to the exit rate out of welfare, regardless of the destination state. Work experience is found to increase the likelihood of leaving welfare because of increases in earnings or for unknown reasons, but it has no significant impact on exit because of marriage. Program group members are more likely to leave welfare because of increases in earnings than control group members.

The remainder of the paper is organized as follows: a detailed description of the data is provided in the second section, the empirical model is presented in the third section, the results are shown in the fourth section, and a summary is provided in the fifth section.

Data

THE SSP APPLICANT STUDY

The data analyzed in this paper is taken from the Self-Sufficiency Project (SSP) Applicant study, which consists of a sample of 3,315 single parents from the province of British Columbia. The respondents, who all started a fresh welfare spell between February 1994 and February 1995, were drawn at random from provincial income assistance (IA) records.[6] In order to qualify for the Applicant study, applicants had to be single parents 19 years of age or older and had to have not received IA payments in the previous six months.

Following the baseline interview, half of the sample was randomly assigned to a program group that could potentially receive an earnings supplement in their second year. The other half of the sample formed a control group that could not receive the earnings supplement. In order to become eligible for the earnings supplement, applicants in the program group had to remain on welfare for at least 12 months. Program group members who received income assistance for less than 12 months were consequently not eligible for subsequent supplements. We shall refer to this group as "not-eligible" program members in the remainder of this paper. In order for an eligible program group member to receive the earnings supplement, that person had to find full-time employment (an average of at least 30 hours per week over a four-week or monthly period) within the next 12 months and stop receiving welfare payments. Those eligible who found full-time employment within this time window will be referred to as "take-up" program members, while those who were not able to secure full-time employment will be referred to as "no-take-up" program members. The supplement was set to half the difference between a participant's employment earnings and an "earnings benchmark" which was determined by SSP, and in 1994 it equalled $37,500. The supplement was available for a maximum of three years, and it was reduced by 50 cents for every dollar of increased earnings. Unearned income, earnings of other family members, and number of children did not affect the amount of the supplement. Finally, a recipient could decide to return to income assistance at any time as long as he or she gave up the supplement.[7]

Participants in the Applicant study were followed for up to six years, with follow-up interview surveys at approximately 12, 30, 48, and 72 months after the baseline interview. Of the 3,315 respondents in the baseline interview, 9 did not report valid information on educational attainment at baseline and were excluded from the sample. Furthermore, due to sample attrition, not all of the original sample participants completed all subsequent follow-up surveys and only 2,006 completed all succeeding follow-up interviews forming a balanced panel. The analysis in this paper is based on this balanced panel with some additional sample

[6]The sample drawn at random from the provincial IA records originally consisted of 4,214 single parents. Of these, 832 sampled individuals were not included in the Applicant study, because they did not complete a baseline interview or they did not sign an informed consent form agreeing to be part of the study. In addition, 59 individuals were removed from the Applicant study after having completed the baseline interview, because they had not been off IA for enough months or they were already off welfare before they completed the baseline interview. Finally, eight additional respondents were removed after the baseline interview, because they asked to be removed from the study.

[7]For additional details about the SSP Applicant study and the SSP earnings supplement, see Ford et al. (2003).

exclusions. From this reduced sample, 97 additional individuals were excluded because they were not IA recipients at the time of the baseline interview and 4 respondents with missing information on their marital history were also excluded. Finally, 413 respondents who provided inconsistent reports on their educational attainments over the six-year period were also excluded.[8] After these exclusions, the sample consisted of 1,492 applicants who were observed for a period of 72 months.

Information on IA benefits was obtained from administrative records, whereas other information — such as educational attainment, work-related training, employment, work experience, marital status, age and number of children, and immigrant status — was obtained from the surveys. Table 1 presents characteristics of respondents at the baseline interview. The entries in the top panel show the characteristics for the original sample (excluding the nine individuals with invalid records on education at baseline), while the figures in the second panel show the same information for the sample that is used for the subsequent empirical analysis. Despite reducing the original sample by over 50 per cent, the characteristics of the sample respondents remain quite similar. The fraction that belongs to the take-up group is somewhat higher in the sample used in this paper (15.2 per cent as opposed to 11.8 per cent). The proportions of control group members and no-take-up members are virtually the same, while the full sample has a higher fraction of not-eligible members than the reduced sample. It is possible that one reason for the difference is a higher incidence of leaving the province among this latter group. Regarding observable characteristics, the reduced sample has slightly more women and slightly less immigrants and persons of First Nations ancestry than the original sample, but otherwise the average values of the selected variables in Table 1 are virtually identical. As can be seen, nearly all respondents were female (over 90 per cent), 25 to 30 per cent were born outside Canada, and most respondents had one or two children.

Table 1: Characteristics of Respondents to the SSP Applicant Study at Baseline

	Control Group	Program Group		
		Take-Up	No-Take-Up	Not-Eligible
		Full Sample		
Sample size	1,660	390	546	708
Observed characteristics				
Age	32.4	32.0	32.4	33.3
	(0.2)	(0.4)	(0.3)	(0.3)
Female (%)	0.916	0.908	0.912	0.876
	(0.007)	(0.015)	(0.012)	(0.012)

(continued)

[8]This relatively large degree of measurement errors in educational attainments has also been documented by Riddell and Riddell (2004) for the SSP Recipient study. A regression of invalid reports on program status and other observable characteristics reveal that the measurement errors are uncorrelated with program status but positively correlated with immigrant status and baseline education. Most of the inconsistent reports appear at the first follow-up interview. In an earlier version of the paper, respondents with inconsistent information on changes in education were retained in the sample and their responses were "corrected" (i.e. it was assumed that education cannot depreciate). The main results on the effect of educational attainment were similar to those reported in this version of the paper.

Table 1: Characteristics of Respondents to the SSP Applicant Study at Baseline (Cont'd)

	Control Group	Program Group		
		Take-Up	No-Take-Up	Not-Eligible
	Full Sample			
First Nations ancestry (%)	0.042	0.033	0.044	0.047
	(0.005)	(0.009)	(0.009)	(0.008)
Immigrant (%)	0.307	0.295	0.368	0.243
	(0.011)	(0.023)	(0.021)	(0.016)
Number of children	1.74	1.74	1.77	1.60
	(0.02)	(0.04)	(0.04)	(0.03)
	Sample Used for Estimation			
Sample size	740	227	243	282
Observed characteristics				
Age	32.3	31.9	32.6	33.5
	(0.3)	(0.5)	(0.5)	(0.5)
Female (%)	0.941	0.925	0.942	0.922
	(0.009)	(0.018)	(0.015)	(0.016)
First Nations ancestry (%)	0.032	0.035	0.033	0.025
	(0.007)	(0.012)	(0.011)	(0.009)
Immigrant (%)	0.266	0.242	0.325	0.238
	(0.016)	(0.029)	(0.030)	(0.025)
Number of children	1.75	1.71	1.74	1.62
	(0.03)	(0.06)	(0.06)	(0.05)

Source: Calculations based on baseline survey data from the SSP Applicant study.

Notes: Nine respondents who did not report valid answers to the questions on educational attainment at baseline were excluded from the full sample. The sample used for estimation consists of respondents who participated in all follow-up surveys (12-month, 30-month, 48-month, and 72-month surveys), conditioning on IA receipt at baseline, and with four respondents excluded because of missing information on marital history. Finally, 413 individuals with inconsistent reports on educational attainment were excluded.

Standard errors are in parentheses.

DESCRIPTION OF EDUCATION AND WORK EXPERIENCE OF SURVEY RESPONDENTS

Table 2 presents information on educational attainment at the baseline interview for the different samples described above.[9] In this paper, I consider four mutually exclusive and exhaustive educational categories: less than high school, high school only, completed vocational school, and attended university. The first category includes respondents who had not obtained a high school graduation diploma or equivalent, while the second category includes persons who had obtained such a diploma but had not acquired any further schooling. The third category is defined to include respondents who had a high school diploma who attended a community college, technical institute, or a trade or vocational school and who also received a vocational diploma. Finally, the last category includes high school graduates who attended an educational institution and who were taking this education towards a college diploma or a university degree.

[9]The variables used to obtain the distribution of educational attainments at the baseline interview are BED9, BED10, BED15, and BED16.

Table 2: Educational Attainment at Baseline for Respondents to the SSP Applicant Study

	Control Group	Program Group		
		Take-Up	No-Take-Up	Not-Eligible
		Full Sample		
Sample size	1,660	390	546	708
Respondent has				
Less than high school	0.432	0.372	0.527	0.385
	(0.012)	(0.025)	(0.021)	(0.018)
High school only	0.211	0.254	0.214	0.190
	(0.010)	(0.022)	(0.018)	(0.015)
Completed vocational school	0.166	0.203	0.128	0.200
	(0.009)	(0.020)	(0.014)	(0.015)
Attended university	0.191	0.172	0.130	0.225
	(0.010)	(0.019)	(0.014)	(0.016)
		Sample Used for Estimation		
Sample size	740	227	243	282
Respondent has				
Less than high school	0.447	0.379	0.527	0.351
	(0.018)	(0.032)	(0.032)	(0.028)
High school only	0.226	0.273	0.239	0.245
	(0.015)	(0.030)	(0.027)	(0.026)
Completed vocational school	0.149	0.185	0.107	0.170
	(0.013)	(0.026)	(0.020)	(0.022)
Attended university	0.178	0.163	0.128	0.234
	(0.014)	(0.025)	(0.021)	(0.025)

Source: Calculations based on baseline survey data from the SSP Applicant study.

Notes: Nine respondents who did not report valid answers to the questions on educational attainment at baseline were excluded from the full sample. The sample used for estimation consists of respondents who participated in all follow-up surveys (12-month, 30-month, 48-month, and 72-month surveys), conditioning on IA receipt at baseline, and with four respondents excluded because of missing information on marital history. Finally, 413 individuals with inconsistent reports on educational attainment were excluded.

Standard errors are in parentheses.

The distribution of educational attainment between the original sample and the reduced sample used in this paper is quite similar, and none of the differences is significant at conventional significance levels. This provides an additional indication that the sample selections made in this paper are not systematically related to important characteristics of the respondents. Focusing on the reduced sample, 44.7 per cent of the control group respondents had not completed high school, 22.6 per cent had high school as their highest level of education, 14.9 per cent had completed vocational school in addition to completing high school, and 17.8 per cent had graduated from high school and had attended college or university. Among the program group respondents, there is substantial variation in educational attainment, where the take-up group and the not eligible group had acquired more formal education at the baseline interview than the no-take-up group. High school drop-out rates were 37.9 per cent for the take-up group and 35.1 per cent for the not-eligible group. These rates are lower than those of the control group and substantially lower than the drop-out rate for the no-take-up group (52.7 per cent). More respondents in the take-up group and the not-eligible group had completed vocational school and attended college or university than respondents in the control group and the no-take-up group.

The differences in educational attainment within the program group are generally significant and suggest that program group status may be endogenous. In other words, those who eventually received the income supplement (the take-up group) are not randomly selected from the overall program group, and this may contaminate the initial randomization of respondents into control and program groups. An implication of this is that simple comparisons in welfare and/or employment rates between control and program groups may not represent causal average treatment effects. In the subsequent empirical analysis, I will model the duration of welfare and non-welfare spells jointly with the determination of eligibility and take-up status to account for the potential endogeneity of program group status.

At this stage, it is also useful to compare educational attainment among SSP applicants with schooling attainment in the population. Using data from Statistics Canada's School Leavers' Follow-up Survey (SLFS), which provides detailed information on educational attainment among young individuals residing in British Columbia in 1995, I calculated the proportions of individuals in each of the four categories described above. According to SLFS, 14.9 per cent had not completed high school, 21.4 per cent had high school only, 10.3 per cent had completed vocational school, and 53.4 per cent had attended college or university. While the SLFS is not representative of the overall population, and is instead representative of only young adults (in their early 20s), these figures indicate that, not surprisingly, the educational attainment among SSP applicants is substantially below those of the population.

In Table 3, accumulated work experience at the baseline interview is shown for the two different samples of the SSP Applicant study.[10] The average number of years worked (not distinguishing between full-time employment and part-time employment) is virtually the same in the two samples. Again, focusing on the reduced sample, average work experience ranges from 8.5 years for the no-take-up group to 11.2 years for the not-eligible group. Breaking down work experience by educational attainment shows that more educated respondents have acquired more work experience. This positive correlation between work experience and education is observed for all program group categories shown in Table 3.

Table 3: Work Experience at Baseline for Respondents to the SSP Applicant Study

	Control Group	Program Group		
		Take-Up	No-Take-Up	Not-Eligible
	Full Sample			
Sample size	1,660	390	546	708
Years worked	9.6	9.8	8.5	11.2
	(0.2)	(0.3)	(0.3)	(0.3)

(continued)

[10]The variable used to obtain accumulated work experience at the baseline interview is BEMPYRS.

Table 3: Work Experience at Baseline for Respondents to the SSP Applicant Study (Cont'd)

	Control Group	Program Group		
		Take-Up	No-Take-Up	Not-Eligible
	Full Sample			
Years worked by educational attainment at baseline				
Less than high school	8.0	9.2	7.6	10.0
	(0.2)	(0.6)	(0.4)	(0.5)
High school only	9.1	8.3	8.6	9.8
	(0.3)	(0.5)	(0.6)	(0.6)
Completed vocational school	11.6	11.1	9.8	12.8
	(0.4)	(0.7)	(0.8)	(0.6)
Attended university	12.0	11.9	10.9	13.1
	(0.4)	(0.9)	(0.8)	(0.6)
	Sample Used for Estimation			
Sample size	740	227	243	282
Years worked	9.5	9.8	8.5	11.2
	(0.2)	(0.3)	(0.3)	(0.3)
Years worked by educational attainment at baseline				
Less than high school	8.1	9.0	7.9	10.7
	(0.4)	(0.7)	(0.7)	(0.8)
High school only	9.1	8.5	8.7	9.8
	(0.5)	(0.7)	(0.8)	(0.8)
Completed vocational school	11.4	10.6	10.2	11.8
	(0.6)	(1.0)	(1.1)	(0.9)
Attended university	11.8	12.3	12.3	14.1
	(0.6)	(1.0)	(1.2)	(1.0)

Source: Calculations based on baseline survey data from the SSP Applicant study.

Notes: Nine respondents who did not report valid answers to the questions on educational attainment at baseline were excluded from the full sample. The sample used for estimation consists of respondents who participated in all follow-up surveys (12-month, 30-month, 48-month, and 72-month surveys), conditioning on IA receipt at baseline, and with four respondents excluded because of missing information on marital history. Finally, 413 individuals with inconsistent reports on educational attainment were excluded.

Standard errors are in parentheses.

DESCRIPTION OF CHANGES IN HUMAN CAPITAL OF SURVEY RESPONDENTS

It is common practice in most of the previous work on welfare persistence to assume that educational attainment is constant over time. Usually, the controls for education are measured when an individual enters the survey and is then assumed not to change. However, as shown in tables 4 and 5 (which are based on the reduced sample), the educational attainment of respondents to the SSP Applicant study change considerably over the six-year period they were observed. At the baseline interview, 44.7 per cent of the control group respondents had not completed a high-school diploma. For the program group, high school drop-out rates are 37.9 per cent for the take-up group, 52.7 per cent for the no-take-up group, and 35.1 per cent for the not-eligible group. At the 72-month follow-up survey, these figures had dropped substantially for all groups. At this survey, that is six years after the baseline interview, the proportion of respondents who had not completed high school had decreased to 27.8 per cent for the control group, 22.5 per cent for the take-up group, 35.8 per cent for the no-take-up group, and 25.5 per cent for the not-eligible group. The reduction is largest for the take-up group (40.6 per cent) and smallest for the not-eligible group (27.3 per cent). The proportions who had attended college or university increased significantly over the six-year period, and for the take-up and no-take-up groups, the proportions who had attended college or university more than doubled over this period. Thus, the entries in Table 5 indicate existence of significant educational upgrading among SSP applicants.[11] While both control and program group members increased their education, the increase was largest for the take-up program group.

Table 4: Educational Attainment at Baseline and Follow-Up Surveys, Based on Reduced Sample (Sample Size = 1,492)

	Control Group		Program Group					
			Take-Up		No-Take-Up		Not-Eligible	
			At Baseline					
Respondent has								
Less than high school	0.447	(0.018)	0.379	(0.032)	0.527	(0.032)	0.351	(0.028)
High school only	0.226	(0.015)	0.273	(0.030)	0.239	(0.027)	0.245	(0.026)
Completed vocational school	0.149	(0.013)	0.185	(0.026)	0.107	(0.020)	0.170	(0.022)
Attended university	0.178	(0.014)	0.163	(0.025)	0.128	(0.021)	0.234	(0.025)
			At 12-Month Survey					
Less than high school	0.380	(0.018)	0.322	(0.031)	0.473	(0.032)	0.287	(0.027)
High school only	0.200	(0.015)	0.225	(0.028)	0.222	(0.027)	0.230	(0.025)
Completed vocational school	0.172	(0.014)	0.203	(0.027)	0.111	(0.020)	0.167	(0.022)
Attended university	0.249	(0.016)	0.251	(0.029)	0.193	(0.025)	0.316	(0.028)
			At 30-Month Survey					
Less than high school	0.353	(0.018)	0.269	(0.029)	0.444	(0.032)	0.280	(0.027)
High school only	0.153	(0.013)	0.198	(0.027)	0.218	(0.027)	0.174	(0.023)
Completed vocational school	0.199	(0.015)	0.233	(0.028)	0.119	(0.021)	0.206	(0.024)
Attended university	0.296	(0.017)	0.300	(0.030)	0.218	(0.027)	0.340	(0.028)
			At 48-Month Survey					
Less than high school	0.330	(0.017)	0.251	(0.029)	0.416	(0.032)	0.273	(0.027)
High school only	0.138	(0.013)	0.181	(0.026)	0.193	(0.025)	0.135	(0.020)
Completed vocational school	0.222	(0.015)	0.238	(0.028)	0.132	(0.022)	0.234	(0.025)
Attended university	0.311	(0.017)	0.330	(0.031)	0.259	(0.028)	0.358	(0.029)
			At 72-Month Survey					
Less than high school	0.278	(0.016)	0.225	(0.028)	0.358	(0.031)	0.255	(0.026)
High school only	0.159	(0.013)	0.141	(0.023)	0.206	(0.026)	0.142	(0.021)
Completed vocational school	0.231	(0.016)	0.269	(0.029)	0.173	(0.024)	0.227	(0.025)
Attended university	0.331	(0.017)	0.366	(0.032)	0.263	(0.028)	0.376	(0.029)

Source: Calculations based on baseline survey data from the SSP Applicant study.

Notes: Nine respondents who did not report valid answers to the questions on educational attainment at baseline were excluded from the full sample. The sample used for estimation consists of respondents who participated in all follow-up surveys (12-month, 30-month, 48-month, and 72-month surveys), conditioning on IA receipt at baseline, and with four respondents excluded because of missing information on marital history. Finally, 413 individuals with inconsistent reports on educational attainment were excluded. Standard errors are in parentheses.

[11] Riddell and Riddell (2004) also find evidence for substantial increases in educational attainment for a different SSP population (the Recipient study).

In Table 5, the changes in educational attainment between the baseline interview and the last follow-up interview (at Month 72) are presented conditioning on baseline education. For respondents with less than high school at baseline, between 30 and 40 per cent had increased their educational attainment by the 72-month survey. Among those who upgraded their education, 17.2 per cent of control group members had completed high school only, 12.4 per cent had completed vocational school, while 8.2 per cent had attended university. For the program group, these numbers are 16.3, 14.0, and 10.5 per cent for the take-up group; 13.3, 13.3, and 5.5 per cent for the no-take-up group; and 11.1, 8.1, and 8.1 per cent for the not-eligible group. The second panel of Table 5 shows that educational upgrading is larger among respondents who had high school only at baseline than among high school dropouts. Overall, conditioning on high school or less at baseline, it appears as if the take-up group was most likely to upgrade their schooling, while the no-take-up and the not-eligible groups were least likely to invest in education. It is thus possible that the income supplement had a positive effect not only on employment rates (which will be shown below), but also on educational upgrading. However, the reported standard errors are quite large and the differences between the control group and the take-up group are generally not significant at conventional levels.

Table 5: Educational Attainment at the Last Follow-Up Survey by Educational Attainment at Baseline, Based on Reduced Sample (Sample Size = 1,492)

	Control Group	Program Group		
		Take-Up	No-Take-Up	Not-Eligible
Less than high school at baseline				
Less than high school	0.622	0.593	0.680	0.727
	(0.027)	(0.053)	(0.041)	(0.045)
High school only	0.172	0.163	0.133	0.111
	(0.021)	(0.040)	(0.030)	(0.032)
Completed vocational school	0.124	0.140	0.133	0.081
	(0.018)	(0.038)	(0.030)	(0.028)
Attended university	0.082	0.105	0.055	0.081
	(0.015)	(0.033)	(0.020)	(0.028)
High school only at baseline				
High school only	0.365	0.290	0.569	0.420
	(0.037)	(0.058)	(0.066)	(0.060)
Completed vocational school	0.383	0.323	0.276	0.406
	(0.038)	(0.060)	(0.059)	(0.060)
Attended university	0.251	0.387	0.155	0.174
	(0.034)	(0.062)	(0.048)	(0.046)
Completed vocational school at baseline				
Completed vocational school	0.600	0.690	0.346	0.583
	(0.047)	(0.072)	(0.095)	(0.072)
Attended university	0.400	0.310	0.654	0.417
	(0.047)	(0.072)	(0.095)	(0.072)

Source: Calculations based on baseline survey data from the SSP Applicant study.

Notes: Nine respondents who did not report valid answers to the questions on educational attainment at baseline were excluded from the full sample. The sample used for estimation consists of respondents who participated in all follow-up surveys (12-month, 30-month, 48-month, and 72-month surveys), conditioning on IA receipt at baseline, and with four respondents excluded because of missing information on marital history. Finally, 413 individuals with inconsistent reports on educational attainment were excluded.

Standard errors are in parentheses.

Table 6 shows years of work experience at the last follow-up survey as well as the difference in work experience between this survey and the baseline survey (the baseline values were presented in Table 3). The average number of years worked (not distinguishing between full-time employment and part-time employment) at the last follow-up survey is highest for the not-eligible group (15.1 years) and lowest for the no-take-up group (10.8 years). The take-up group had on average accumulated 13.8 years, which is significantly higher than the corresponding value for the control group (12.3 years). Breaking down accumulated work experience by educational attainment shows that more educated respondents generally acquire more work experience. This positive correlation between work experience and education was also observed at the baseline interview and holds for both control and program group respondents.

Table 6: Years of Work Experience at the Last Follow-Up Survey and Difference Between Last Follow-Up Survey and Baseline Survey, Based on Reduced Sample (Sample Size = 1,492)

	Control Group	Program Group		
		Take-Up	No-Take-Up	Not-Eligible
	At last follow-up interview (Month 72)			
Years worked	12.3	13.8	10.8	15.1
	(0.3)	(0.4)	(0.5)	(0.5)
Years worked by educational attainment at baseline				
Less than high school	10.6	13.0	9.4	14.0
	(0.4)	(0.8)	(0.7)	(0.9)
High school only	12.0	12.5	11.2	13.3
	(0.6)	(0.7)	(0.8)	(0.9)
Completed vocational school	14.4	14.8	12.3	16.1
	(0.7)	(0.9)	(1.1)	(1.0)
Attended university	15.1	16.7	15.0	18.0
	(0.6)	(1.1)	(1.3)	(1.1)
	Difference between last follow-up interview and baseline			
Years worked	2.8	4.1	1.9	3.7
	(0.1)	(0.1)	(0.1)	(0.1)
Years worked by educational attainment at baseline				
Less than high school	2.5	4.0	1.5	3.3
	(0.1)	(0.1)	(0.1)	(0.2)
High school only	2.9	4.0	2.4	3.5
	(0.1)	(0.2)	(0.2)	(0.2)
Completed vocational school	3.0	4.1	2.1	4.2
	(0.2)	(0.2)	(0.5)	(0.3)
Attended university	3.3	4.4	2.7	3.9
	(0.2)	(0.2)	(0.4)	(0.2)

Source: Calculations based on baseline survey data from the SSP Applicant study.

Notes: Nine respondents who did not report valid answers to the questions on educational attainment at baseline were excluded from the full sample. The sample used for estimation consists of respondents who participated in all follow-up surveys (12-month, 30-month, 48-month, and 72-month surveys), conditioning on IA receipt at baseline, and with four respondents excluded because of missing information on marital history. Finally, 413 individuals with inconsistent reports on educational attainment were excluded. Standard errors are in parentheses.

The lower panel of Table 6 shows the amount of work experience generated between the baseline survey and the last follow-up survey. The largest value is reported for the take-up group (4.1 years) and the lowest is found for the no-take-up group (1.9 years). The value for the control group is 2.8 years while it is 3.7 years for the not-eligible group. The program group differences are significant at conventional levels and clearly show that the treatment was effective on those who were treated, while those who for some reason (such as lack of ability to find full-time employment or strong preferences against work) did not take up the supplement worked significantly less than any other group in the sample. The 4.1 years for the take-up group correspond to approximately two thirds of the six-year period, leaving two years that most used to establish eligibility for the income supplement (one year receiving income assistance and up to one year to find a full-time position). When breaking down the differences in work experience over the sample period by baseline educational levels, we still observe a positive correlation between education and work experience, but it is less pronounced than the one found at the baseline survey. This is especially true for the not-eligible and the take-up groups, for whom the differences in work experience for those who attended university are not significantly different from that for the high-school dropouts.

The SSP Applicant study includes detailed information on employment activities during the period of the study, and monthly information on full-time and part-time employment status is available. In Table 7, I present the accumulation of months of full-time and part-time employment since the baseline interview — at each follow-up survey — by program status category.[12] At the 12-month survey, the average accumulated months of part-time employment are not significantly different across the four groups and range between 1.7 and 2 months. On the other hand, looking at the history of full-time employment at this survey, we observe more dispersion and significant differences, with the highest value for the not-eligible group and lowest for the no-take-up group. Thus, many in the not-eligible group took up full-time employment before the 12-month income assistance eligibility period had ended, indicating that for these respondents, the potential for a substantial income supplement did not cause them to defer starting a job. The effect of the income supplement on full-time employment rates is clearly illustrated in the second panel of Table 7, which shows accumulated employment months at the second follow-up interview which took place 30 months after the baseline interview. At this point in time, full-time experience is highest for the take-up group, and while not significantly higher than that for the not-eligible group, it is significantly above the values for both the control group and the no-take-up group. At this interview, there are again no significant differences across the four groups in part-time employment experience. The difference in full-time experience can also be observed at the third follow-up interview (conducted 48 months after baseline) and at the last follow-up interview.

[12]In the SSP applicant survey, full-time employment is defined as working 30 or more hours in at least one week during the month.

Table 7: Accumulated Months of Full-Time and Part-Time Employment at the Follow-Up Surveys, Based on Reduced Sample (Sample Size = 1,492)

	Control Group	Program Group		
		Take-Up	No-Take-Up	Not-Eligible
	Months of experience at first follow-up survey (Month 12)			
Part-time	1.8 (0.1)	1.7 (0.2)	1.7 (0.2)	2.0 (0.2)
Full-time	2.2 (0.1)	2.2 (0.3)	0.6 (0.1)	4.1 (0.3)
	Months of experience at second follow-up survey (Month 30)			
Part-time	4.4 (0.3)	4.0 (0.4)	4.3 (0.5)	5.2 (0.5)
Full-time	6.8 (0.4)	12.8 (0.5)	2.2 (0.4)	11.6 (0.6)
	Months of experience at third follow-up survey (Month 48)			
Part-time	7.0 (0.4)	5.8 (0.6)	6.8 (0.7)	7.8 (0.7)
Full-time	13.0 (0.6)	25.8 (0.7)	5.8 (0.6)	20.4 (1.0)
	Months of experience at last follow-up survey (Month 72)			
Part-time	10.8 (0.4)	8.5 (0.8)	11.1 (1.0)	11.5 (0.9)
Full-time	23.2 (0.8)	40.9 (1.1)	12.2 (1.1)	32.7 (1.5)

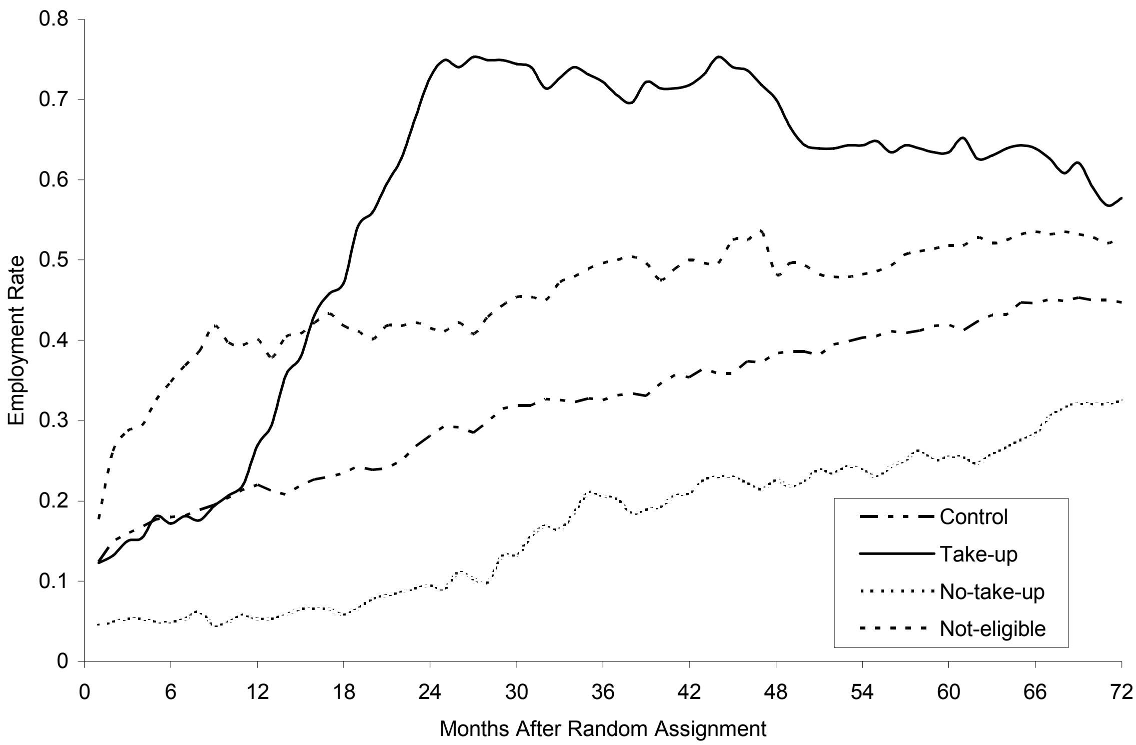
Source: Calculations based on baseline survey data from the SSP Applicant study.

Notes: Nine respondents who did not report valid answers to the questions on educational attainment at baseline were excluded from the full sample. The sample used for estimation consists of respondents who participated in all follow-up surveys (12-month, 30-month, 48-month, and 72-month surveys), conditioning on IA receipt at baseline, and with four respondents excluded because of missing information on marital history. Finally, 413 individuals with inconsistent reports on educational attainment were excluded.

Standard errors are in parentheses.

Recently, a few papers have reported that SSP (using the recipients data that sampled individuals who had received social assistance for at least 11 months out of the 12 months preceding random assignment) may only have had limited long-term effects on full-time employment rates for the program group (see Card & Hyslop, 2005, and Foley, 2004). Indeed, descriptive analysis using this data source shows a convergence in full-time employment rates for the control and the program groups 52 months after random assignment. Zabel et al. (2004), using the same SSP sample, show however that a significant difference between the take-up group and the control group exists at Month 52 (the difference is around 25 percentage points) but that the difference is decreasing over time after a peak at around 12 months after random assignment. A similar pattern is observed for the sample used in this paper. Figure 1 shows full-time employment rates by program status category. Consistent with the incentives embedded in the income supplement, there is a rapid increase in employment rates for the take-up group between 12 and 24 months after random assignment. The time-limit effect of the supplement is also evident in the figure as employment rates for this group decline when the supplement begins to expire (occurring between months 42 and 48). Following this decline, there is a slight reduction until Month 72 where the full-time employment rate equals 58 per cent. For the control group, employment rates increase linearly from around 10 per cent one month after random assignment to about 45 per cent at Month 72. The difference between the take-up group and the control group peaks around two years after random assignment and is then reduced. However, a significant difference remains at Month 72. For the no-take-up group, the development over time is similar to that of the control group but with lower employment levels. Finally, for the not-eligible group, employment rates increase rapidly during the first nine months after baseline and then slowly increase to about 53 per cent at Month 72.

Figure 1: Monthly Full-Time Employment Rates, by Program Group Category, Based on Reduced Sample (Sample Size = 1,492)



Source: Calculations based on baseline survey data from the SSP Applicant study.

Note: Nine respondents who did not report valid answers to the questions on educational attainment at baseline were excluded from the full sample. The sample used for estimation consists of respondents who participated in all follow-up surveys (12-month, 30-month, 48-month, and 72-month surveys), conditioning on IA receipt at baseline, and with four respondents excluded because of missing information on marital history. Finally, 413 individuals with inconsistent reports on educational attainment were excluded.

The SSP Applicant study also contains information on work-related training, such as on-the-job training and apprenticeship training. Table 8 presents information on the fraction of survey respondents who had completed any work-related training at the first and the last follow-up survey.[13] The completion rates vary both over time, across program group categories, and across educational attainment. At the 12-month survey, 10 per cent of the control group members and the take-up group members had completed any form of work-related training. This is higher than for the no-take-up group (7 per cent) but lower than the not-eligible group (15.6 per cent).

[13]The variables used to obtain completion rates of work-related training are FED6_2, FED16, FED18, SED2, SED3, SED9_2, TED2, TED3, TED9_2, LED2, LED3, and LED9_2.

Among high school drop-outs, the completion rates range between 5.5 per cent for the no-take-up group and 16.2 per cent for the not-eligible group. With a few exceptions, the completion rates are higher among those who had acquired more schooling at baseline, but there are no significant differences across educational categories, regardless of program group status. At the 72-month survey, between 37 and 54 per cent had completed some work-related training. Again, the lowest rate is observed for the no-take-up group and the highest for the not-eligible group. For the take-up group, 47 per cent had completed some form of work-related training, which is about four percentage points higher than for the control group. At this survey, there are significant differences in completion rates between high school dropouts and those with at least a high school diploma for all groups except the no-take-up group, indicating a possible positive correlation between training and education.

Table 8: Completed Work-Related Training at the First and Last Follow-Up Survey, Based on Reduced Sample (Sample Size = 1,492)

	Control Group	Program Group		
		Take-Up	No-Take-Up	Not-Eligible
	At first follow-up survey (Month 12)			
Completed training	0.097 (0.011)	0.097 (0.020)	0.070 (0.016)	0.156 (0.022)
Completed training by educational attainment at baseline				
Less than high school	0.085 (0.015)	0.081 (0.030)	0.055 (0.020)	0.162 (0.037)
High school only	0.084 (0.022)	0.097 (0.038)	0.034 (0.024)	0.072 (0.031)
Completed vocational school	0.145 (0.034)	0.095 (0.046)	0.115 (0.064)	0.208 (0.059)
Attended university	0.106 0.027	0.135 (0.057)	0.161 (0.067)	0.197 (0.049)
	At last follow-up interview (Month 72)			
Completed training	0.428 (0.018)	0.471 (0.033)	0.374 (0.031)	0.535 (0.030)
Completed training by educational attainment at baseline				
Less than high school	0.344 (0.026)	0.419 (0.054)	0.375 (0.043)	0.394 (0.049)
High school only	0.479 (0.039)	0.532 (0.064)	0.293 (0.060)	0.536 (0.060)
Completed vocational school	0.491 (0.048)	0.405 (0.077)	0.385 (0.097)	0.625 (0.071)
Attended university	0.523 (0.044)	0.568 (0.083)	0.516 (0.091)	0.682 (0.058)

Source: Calculations based on baseline survey data from the SSP Applicant study.

Notes: Nine respondents who did not report valid answers to the questions on educational attainment at baseline were excluded from the full sample. The sample used for estimation consists of respondents who participated in all follow-up surveys (12-month, 30-month, 48-month, and 72-month surveys), conditioning on IA receipt at baseline, and with four respondents excluded because of missing information on marital history. Finally, 413 individuals with inconsistent reports on educational attainment were excluded.

Standard errors are in parentheses.

DESCRIPTION OF TIME SPENT ON IA AMONG SURVEY RESPONDENTS

The IA records in the SSP Applicant study exhibit the typical pattern of duration dependence in welfare spells. Table 9 provides information on the distribution of initial IA spells for the 740 control group respondents and for the 243 respondents in the no-take-up group.[14] The monthly information on IA was used to calculate Kaplan–Meier survival probability functions. For control group members, 683 spells end during the six-year survey period while 57 spells (7.7 per cent) are right censored, while for the no-take-up group, 214 spells are uncensored and 29 spells (11.9 per cent) are right censored. Consistent with findings from previous studies, most spells last a relatively short period of time. The empirical survival function values in Table 9 show that of all spells for the control group, 64.6 per cent last at least six months and 33.6 per cent last at least two years. For the no-take-up group, these figures are 95.9 per cent and 64.2 per cent respectively. The figures for the control group are similar to those reported by Blank (1989), Barrett (2000), and Fortin et al. (2004) and are somewhat lower than those reported by Miller and Sanders (1997).

To illustrate how the exit rates vary with education, Figure 2 plots the Kaplan–Meier survival probability functions for the first IA spell, by educational attainment, at the baseline interview for the control group.[15] There are some substantial differences in the probabilities of leaving welfare across the educational categories, with high school dropouts having the lowest probabilities of exiting welfare and those who have attended university having the highest exit probabilities. The convex shape of the survival functions for all educational categories suggest existence of negative duration dependence regardless of educational attainment. However, as is well-recognized, the observed negative duration dependence may also reflect individual differences that persist through time. That is, even in the absence of duration dependence, we might observe that the exit rates reduce with time spent on welfare, because those who have been on welfare for a long time are disproportionally represented by those least likely to leave welfare. Thus, the empirical survival functions in Figure 2 should not be taken as evidence of a causal relationship between time spent on welfare and the exit rates from welfare. The empirical models presented in the next section will attempt to control for unobserved individual differences that persist through time and thus provide an opportunity to infer the degree of “true” duration dependence.

[14]The analysis in Table 9 is restricted to the control and no-take-up groups, since their initial IA spells can potentially last 72 months, whereas for the other two program groups the initial spells are by construction either less than 12 months (for the not-eligible group) or between 12 and 24 months (for the take-up group).

[15]The survival functions in Figure 2 are restricted to the control group only as the sample sizes for the no-take-up group when broken down by educational attainments are small. Moreover, as mentioned previously, the initial spells for the two other program groups are by construction either less than 12 months (for the not-eligible group) or between 12 and 24 months (for the take-up group) and therefore less informative in this context.

Table 9: Kaplan–Meier Survival Functions for Initial IA Spells for Respondents in the Control Group and in the No-Take-Up Group (Sample Size = 967)

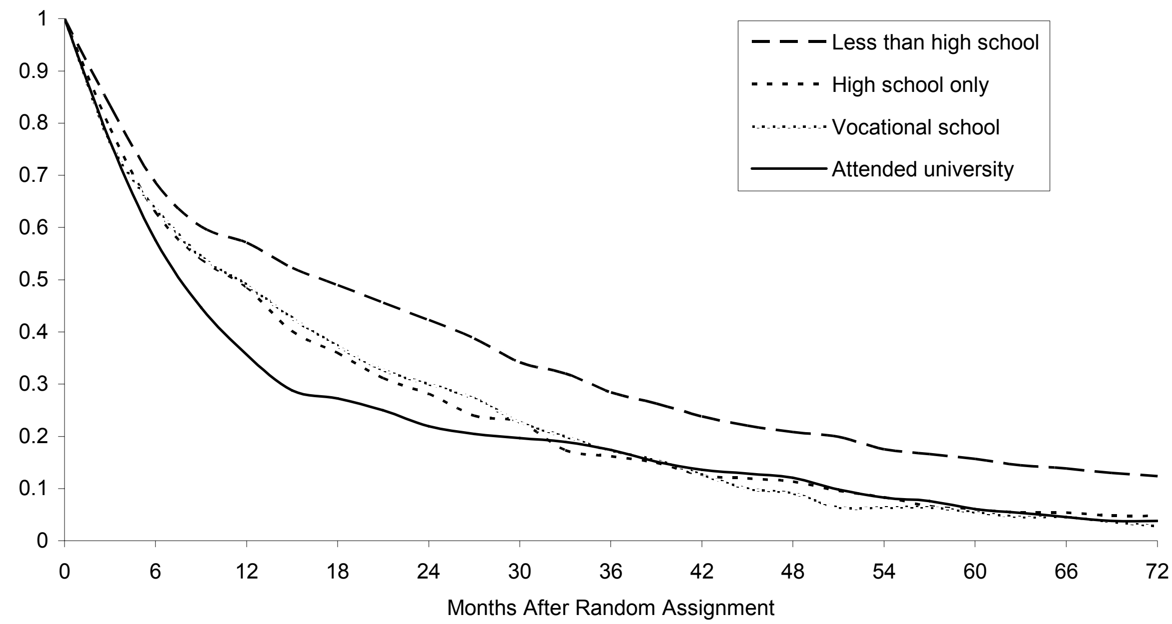
	Control Group		Program Group No Take-Up	
Time (Months)	Survival Function	Standard Error	Survival Function	Standard Error
0	1	n.a.	1	n.a.
3	0.801	0.015	0.984	0.008
6	0.646	0.018	0.959	0.013
9	0.551	0.018	0.938	0.015
12	0.501	0.018	0.881	0.021
15	0.439	0.018	0.823	0.025
18	0.404	0.018	0.770	0.027
21	0.368	0.018	0.704	0.029
24	0.336	0.017	0.642	0.031
27	0.304	0.017	0.572	0.032
30	0.273	0.016	0.510	0.032
33	0.246	0.016	0.424	0.032
36	0.220	0.015	0.391	0.031
39	0.201	0.015	0.337	0.030
42	0.178	0.014	0.296	0.029
45	0.164	0.014	0.267	0.028
48	0.154	0.013	0.247	0.028
51	0.138	0.013	0.214	0.026
54	0.122	0.012	0.198	0.026
57	0.112	0.012	0.185	0.025
60	0.103	0.011	0.165	0.024
63	0.093	0.011	0.156	0.023
66	0.089	0.011	0.144	0.023
69	0.081	0.010	0.128	0.021
72	0.077	0.010	0.119	0.021

Source: Calculations based on baseline survey data from the SSP Applicant study.

Note: Nine respondents who did not report valid answers to the questions on educational attainment at baseline were excluded from the full sample. The sample used for estimation consists of respondents who participated in all follow-up surveys (12-month, 30-month, 48-month, and 72-month surveys), conditioning on IA receipt at baseline, and with four respondents excluded because of missing information on marital history. Finally, 413 individuals with inconsistent reports on educational attainment were excluded.

As mentioned in the introduction, respondents to the SSP Applicant study remain in the data after the initial IA spell has ended. This is generally not the case for most existing administrative data on welfare use and provides an excellent opportunity to study total time on welfare, which acknowledges the possibility that many former welfare recipients do not permanently leave welfare. In Table 10, the distribution of months of IA receipt during the six-year sample period is presented, by program status category. By construction, no one in the take-up or no-take-up groups experiences six months or less of IA receipt. For the two non-eligible groups, 18.4 per cent of the control group members received IA for six months or less while for the not-eligible group, this figure is 41.1 per cent.

Figure 2: Kaplan–Meier Survival Functions for Initial IA Spells, by Educational Attainment, for Respondents in the Control Group (Sample Size = 740)



Source: Calculations based on baseline survey data from the SSP Applicant study (sample size = 1,492).

Note: Nine respondents who did not report valid answers to the questions on educational attainment at baseline were excluded from the full sample. The sample used for estimation consists of respondents who participated in all follow-up surveys (12-month, 30-month, 48-month, and 72-month surveys), conditioning on IA receipt at baseline, and with four respondents excluded because of missing information on marital history. Finally, 413 individuals with inconsistent reports on educational attainment were excluded.

The entries in Table 10 also show that almost a quarter of the no-take-up group received IA for almost the whole sample period (more than 66 months). For the control group, this fraction is 15.4 per cent, while it is close to zero for the remaining two groups (take-up and not-eligible). The lower part of Table 10 also shows the average number of months on welfare (out of 72) and indicates significant differences across the four program status groups. The average time on welfare is 32 months for the control group, 24 months for the take-up group, 44 months for the no-take-up group, and finally, 14 months for the not-eligible group. The last entries in Table 10 show the average number of welfare spells over the six-year period as well as the fraction of respondents with more than one spell. The group with most spells is the not-eligible group (3.4 spells) while the no-take-up group has the lowest average number of spells (2.7 spells). However, the differences across the four groups are not significant. Furthermore, around 50 per cent of all respondents return at least once to IA after having left the initial IA spell, and this proportion is similar across the four groups. Overall, Table 10 indicates that welfare is a relatively permanent state for the no-take-up group, while the not-eligible group uses IA on a more temporary basis.

Because respondents to the SSP Applicant study remain in the data after the initial IA spell has ended, it is also possible to examine reasons for leaving welfare. In tables 11a and 11b, the characteristics of respondents are presented depending on the reasons for leaving welfare by program group status.[16] The entries are based on durations of initial IA spells measured in months. Of the 1,492 first spells, 86 (or six per cent) are right censored, meaning that they were ongoing at Month 72 after the baseline interview.[17] The entries in the first four columns of Table 11a show that the most common ending reason is increases in earnings. This is true regardless of program group status, and the proportions range from 50 per cent (no-take-up group) to 81 per cent (take-up group). The average durations of the first spells that end with earnings increases are around 18 months for both the control and the take-up group. For the no-take-up group, the average duration is significantly longer (30 months) while it is significantly shorter for the not-eligible group (7 months). Table 11a also shows the educational distribution for those whose initial welfare spell ended because of earnings increases. This distribution is similar to, and not significantly different from, that presented in Table 2.

In the last four columns of Table 11a, characteristics of respondents who left welfare because changes in marital status are presented. Between 5 and 11 per cent end welfare for this reason, with the lowest proportion recorded for the take-up group and the highest proportion for the not-eligible group. The average durations of the first spells that end with marital status changes are similar to those found for those who ended IA through earnings increases. Considering the educational distribution among these welfare leavers, the control, take-up, and not-eligible groups show a distribution similar to that found in Table 2, while the no-take-up group members who leave IA via marriage appear less educated than other no-take-up group members.

The first four columns of Table 11b show descriptive statistics for those respondents who left welfare for other reasons (such as changes in family composition or increases in household income not due to increases in respondents' own earnings). The proportions that fall into this category range from 14 per cent for the take-up group to 31 per cent for the no-take-up group. These figures are similar to those reported in Blank (1989), who also has information on spells for a period of 72 months. The average duration of these initial IA spells are similar to those found for respondents who left because of earnings increases. Moreover, the educational attainment is also similar to the attainments of those who ended welfare through earnings increases. Finally, the last four columns show the characteristics of respondents in the control and no-take-up groups whose initial spells are right censored. These respondents have lower educational attainment and less work experience than the other group members.

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[16]The entries in tables 11a and 11b were derived by studying changes in marital status and earnings within six months *before* a spell ended as well as within six months *after* it had ended. Administrative rules imply that welfare recipients may continue to receive IA for a few months after they took up employment and, consequently, changes that occurred *before* the spell ended must be considered to accurately code the ending reason. Also, since administrative and survey data rarely combine perfectly, a six-month span was used. Following Blank (1989), who also uses a six-month span, changes in marital status were coded first, and if there was no change in marital status, earnings changes were coded. If neither marital status nor earnings changed, the spell was coded as ending because of other reasons. Initially, I also considered changes in the number of children as a separate destination state, but since less than 0.5 per cent experienced such changes, these were merged with exits because of other reasons. Questions on ending reasons for those who left IA within 12 months after random assignment were administered at the first follow-up survey, and the proportions in each category were similar to those reported in tables 11a and 11b.

[17]As mentioned above, none of the first spells for the take-up or not-eligible groups is right-censored by construction.

Table 10: Distribution of IA Spells Using Monthly Data, Based on Reduced Sample (Sample Size = 1,492)

	Control Group		Program Group					
			Take-Up		No Take-Up		Not Eligible	
Time (Months)	Proportion	Standard Error	Proportion	Standard Error	Proportion	Standard Error	Proportion	Standard Error
6	0.184	0.014	0.000	n.a.	0.000	n.a.	0.411	0.029
12	0.105	0.011	0.075	0.018	0.058	0.015	0.273	0.027
18	0.092	0.011	0.300	0.030	0.049	0.014	0.085	0.017
24	0.089	0.010	0.282	0.030	0.107	0.020	0.053	0.013
30	0.062	0.009	0.132	0.023	0.119	0.021	0.050	0.013
36	0.068	0.009	0.079	0.018	0.091	0.018	0.011	0.006
42	0.061	0.009	0.057	0.015	0.082	0.018	0.028	0.010
48	0.045	0.008	0.040	0.013	0.070	0.016	0.021	0.009
54	0.039	0.007	0.004	0.004	0.053	0.014	0.025	0.009
60	0.046	0.008	0.013	0.008	0.058	0.015	0.021	0.009
66	0.055	0.008	0.004	0.004	0.091	0.018	0.014	0.007
72	0.154	0.013	0.013	0.008	0.222	0.027	0.007	0.005
Average months	32.30	0.89	24.05	0.76	44.12	1.30	13.82	0.94
Average number of spells	2.98	0.10	2.92	0.18	2.67	0.17	3.41	0.18
Fraction with more than one spell	0.55	0.02	0.52	0.03	0.47	0.03	0.59	0.03

Source: Calculations based on baseline survey data from the SSP Applicant study.

Note: Nine respondents who did not report valid answers to the questions on educational attainment at baseline were excluded from the full sample. The sample used for estimation consists of respondents who participated in all follow-up surveys (12-month, 30-month, 48-month, and 72-month surveys), conditioning on IA receipt at baseline, and with four respondents excluded because of missing information on marital history. Finally, 413 individuals with inconsistent reports on educational attainment were excluded.

Table 11a: Characteristics of Welfare Recipients Whose Spells Ended Through Earnings Increases or Changes in Marital Status, Based on Reduced Sample (Sample Size = 1,492)

	Exits Due to Changes in Earnings				Exits Due to Changes in Marital Status			
	Control Group	Program Group			Control Group	Program Group		
		Take-Up	No-Take-Up	Not-Eligible		Take-Up	No-Take-Up	Not-Eligible
Frequency	417	183	122	195	65	12	16	30
Average duration of first spells	18.1	17.8	29.8	7.3	15.8	14.9	31.3	4.4
	(0.8)	(0.5)	(1.5)	(0.7)	(1.0)	(1.4)	(3.2)	(0.6)
Human capital at baseline								
Less than high school	0.415	0.393	0.484	0.374	0.385	0.417	0.688	0.267
	(0.024)	(0.036)	(0.045)	(0.035)	(0.061)	(0.149)	(0.120)	(0.082)
High school only	0.242	0.262	0.238	0.236	0.231	0.250	0.125	0.233
	(0.021)	(0.033)	(0.039)	(0.030)	(0.053)	(0.131)	(0.085)	(0.079)
Completed vocational school	0.156	0.180	0.115	0.164	0.185	0.167	0.125	0.300
	(0.018)	(0.028)	(0.029)	(0.027)	(0.048)	(0.112)	(0.085)	(0.085)
Attended university	0.187	0.164	0.164	0.226	0.200	0.167	0.063	0.200
	(0.019)	(0.027)	(0.034)	(0.030)	(0.050)	(0.112)	(0.063)	(0.074)
Years of work experience	10.0	9.7	9.1	11.9	7.4	11.9	6.9	9.3
	(0.3)	(0.5)	(0.7)	(0.5)	(0.8)	(1.9)	(0.9)	(1.2)

Source: Calculations based on monthly information on welfare use from the SSP Applicant study.

Notes: Nine respondents who did not report valid answers to the questions on educational attainment at baseline were excluded from the full sample. The sample used for estimation consists of respondents who participated in all follow-up surveys (12-month, 30-month, 48-month, and 72-month surveys), conditioning on IA receipt at baseline, and with four respondents excluded because of missing information on marital history. Finally, 413 individuals with inconsistent reports on educational attainment were excluded.

Welfare spells are measured in months.

Standard errors are in parentheses.

Table 11b: Characteristics of Welfare Recipients Whose Spells Ended for Unknown Reasons or for Those With Right Censored Spells, Based on Reduced Sample (Sample Size = 1,492)

	Exits Due to Unknown Reasons				Censored			
	Control Group	Program Group			Control Group	Program Group		
		Take-Up	No-Take-Up	Not-Eligible		Take-Up	No-Take-Up	Not-Eligible
Frequency	201	31	76	57	57	0	29	0
Average duration of first spells	17.1	15.8	27.2	3.8				
	(1.3)	(1.0)	(1.7)	(0.3)				
Human capital at baseline								
Less than high school	0.458	0.258	0.539	0.316	0.719	n.a.	0.586	n.a.
	(0.035)	(0.080)	(0.058)	(0.062)	(0.060)	n.a.	(0.093)	n.a.
High school only	0.214	0.355	0.276	0.281	0.140	n.a.	0.207	n.a.
	(0.029)	(0.087)	(0.052)	(0.060)	(0.046)	n.a.	(0.077)	n.a.
Completed vocational school	0.149	0.226	0.079	0.123	0.053	n.a.	0.138	n.a.
	(0.025)	(0.076)	(0.031)	(0.044)	(0.030)	n.a.	(0.065)	n.a.
Attended university	0.179	0.161	0.105	0.281	0.088	n.a.	0.069	n.a.
	(0.027)	(0.067)	(0.035)	(0.060)	(0.038)	n.a.	(0.048)	n.a.
Years of work experience	9.7	9.0	8.5	11.2	6.8	n.a.	10.3	n.a.
	(0.5)	(1.0)	(0.8)	(1.2)	(0.8)	n.a.	(1.6)	n.a.

Source: Calculations based on monthly information on welfare use from the SSP Applicant study.

Notes: Nine respondents who did not report valid answers to the questions on educational attainment at baseline were excluded from the full sample. The sample used for estimation consists of respondents who participated in all follow-up surveys (12-month, 30-month, 48-month, and 72-month surveys), conditioning on IA receipt at baseline, and with four respondents excluded because of missing information on marital history. Finally, 413 individuals with inconsistent reports on educational attainment were excluded.

Welfare spells are measured in months.

Standard errors are in parentheses.

Empirical Specification

In this section, I will first present a hazard model that considers multiple spells that I will use to asses the impact of observable characteristics — in particular the effect of education, training, work experience, and treatment status — on both welfare exits and re-entries. In the second subsection, I will present a discrete-time competing risks model that jointly estimates the exit rates to marriage, earnings increases, and exits because of other reasons, attempting to control for unobserved characteristics that might generate dependence between the destination-specific exit rates.

A DISCRETE–TIME MULTIPLE SPELL HAZARD MODEL

The empirical model presented in this section has similarities to those presented by Ham and Lalonde (1996), Eberwein, Ham, and Lalonde (1997), Meghir and Whitehouse (1997), Stevens (1999), Devicienti (2001), Cappellari and Jenkins (2002), Biewen (2003), Hansen and Wahlberg (2004), and Zabel et al. (2004). I assume that the hazard rate for individual *i* for *leaving* welfare at time *t* can be specified as

$$\lambda_{it}^{EW}(d_{i,t-1} \mid \mu_i^{EW}) = \Phi(\mu_i^{EW} + \mathbf{X}_{i,t-1}\beta^{EW} + \gamma^{EW}(d_{i,t-1})) \qquad (1)$$

where μ_i^{EW} is an unobserved, time-invariant, individual-specific effect (representing ability, motivation, preferences, etc.); $\mathbf{X}_{i,t-1}$ is a vector containing observable characteristics, including indicators for program group category; $\gamma^{EW}(d_{i,t-1})$ is a function designed to capture duration dependence; $d_{i,t-1}$ represents the duration length at *t-1*; and $\Phi(.)$ represents the standard normal cdf. In addition to the variables describing program group membership, which will be described later, $\mathbf{X}_{i,t-1}$ includes information on age at baseline, gender, immigrant status, First Nations ancestry status, educational attainments (high school only, completed vocational school, attended college or university), work experience at baseline, duration of current part-time employment spell, job training, marital status, duration of current marriage spell, and presence of a child less than 5 years old. All variables, except age at baseline, gender, immigrant status, First Nations ancestry status, and work experience at baseline, are time-varying.

As mentioned previously, it is important to consider both exit and re-entry probabilities in order to accurately measure total time on welfare and also to correctly assess the effects of income supplement and human capital on total exposure towards the welfare system. For example, a single spell model that considers only the exit rate from the initial welfare spell may seriously underestimate the effect of both the income supplement and human capital if these characteristics not only increase the likelihood of leaving welfare, but also reduce the probability that former welfare recipients return to welfare. Thus, it is important to recognize the possibility that individuals that have left welfare may return to welfare, after controlling for observed and unobserved characteristics. I assume that the hazard rate for individual *i* for *re-entering* welfare at time *t* can be specified as

$$\lambda_{it}^{RW}(d_{i,t-1} \mid \mu_i^{RW}) = \Phi(\mu_i^{RW} + \mathbf{Z}_{i,t-1}\beta^{RW} + \gamma^{RW}(d_{i,t-1})) \tag{2}$$

where $\mathbf{Z}_{i,t-1}$ contains the same variables as in $\mathbf{X}_{i,t-1}$ but also includes a measure of the length of current full-time employment spells.[18] $\gamma^{RW}(d_{i,t-1})$ is a function designed to capture duration dependence, $d_{i,t-1}$ represents the duration length of the non-welfare spell at *t-1* and $\Phi(.)$ represents the standard normal cdf. Because the hazard rates in equations (1) and (2) explicitly depend on welfare and non-welfare durations, we need to condition on the state in which a person is initially observed. As opposed to the general case, where the initial state is likely to be endogenous, this is less of a problem in this paper given the construction of the data.[19]

To distinguish between the three different categories of program group members (take-up, no-take-up, and not-eligible), the following variables have been defined:

$$\begin{aligned} PROG_i &= 1 \text{ if i} \in \text{Program group} \\ &= 0 \text{ otherwise} \end{aligned}$$

$$\begin{aligned} ELIG_{it} &= 1 \text{ if i} \in \text{Program group and t} \geq \text{t}^{\text{el}} \\ &= 0 \text{ otherwise} \end{aligned}$$

$$\begin{aligned} TAKEUP_{it} &= 1 \text{ if i} \in \text{Program group and t} \geq \text{t}^{\text{ta}} \\ &= 0 \text{ otherwise} \end{aligned}$$

where t^{el} is the month at which a respondent in the program group becomes potentially eligible for the Self-Sufficiency Project (SSP) income supplement (that is, the respondent has remained on welfare for at least 12 months) and t^{ta} is the month at which a potentially eligible respondent in the program group starts receiving the SSP income supplement. These

[18]The reasons for excluding current full-time employment spells in $\mathbf{X}_{i,t-1}$ are (i) few respondents working full time are eligible for welfare and (ii) as mentioned above, administrative rules imply that some welfare recipients may continue to receive income assistance for a few months after they took up employment, potentially overstating the effects of full-time employment on exit rates.

[19]One advantage with the SSP Applicant sample is that the start date of initial welfare spells is observed for all respondents. However, as is true with virtually all data used for empirical analysis, the history of welfare and non-welfare spells prior to the baseline interview is generally unobserved. Thus, even if there is no left censoring of the initial welfare spell, we do not observe the entire process that generated the sample at baseline and therefore the initial state may still be endogenous.

variables can then be used to infer the treatment effect (τ^{EW}) of income supplements on welfare exit rates:

$$\tau^{EW} = \beta_P^{EW} * PROG_i + \beta_{TA}^{EW} * TAKEUP_{it} * 1(t^{ta} \leq t \leq t^{ta} + 36)$$

as well as the treatment effect on welfare re-entry rates (τ^{RW}):

$$\tau^{RW} = \beta_P^{RW} * PROG_i + \beta_{TA}^{RW} * TAKEUP_{it} * 1(t^{ta} \leq t \leq t^{ta} + 36)$$

As mentioned by Card and Hyslop (2005) and Zabel et al. (2004), the structure of SSP may have generated "pre-incentive" effects. That is, there may be an initial program effect on the welfare hazard rates because program group members who had remained on welfare for at least 12 months after random assignment had to find full-time employment within the next 12 months. The variables defined above can also be used to assess this pre-incentive treatment effect (η^{EW}) of income supplements on welfare exit rates:

$$\eta^{EW} = \beta_P^{EW} * PROG_i + \beta_{EL}^{EW} * ELIG_{it} * 1(t^{el} \leq t \leq t^{el} + 12)$$

Using the transition rates defined above, we can define the contributions to the likelihood for all respondents. For illustration purposes, it may be useful to consider the contributions for four different groups, where all contributions are conditional on unobserved effects:

1. Respondents whose initial welfare spell is right censored (lasting 72 months):

$$L_i(\mu_i^{EW}) = \prod_{t=2}^{T} (1 - \lambda_{i,t}^{EW}(d_{i,t-1} \mid \mu_i^{EW})$$

2. Respondents whose initial welfare spell lasts for t^w months and then they remain off welfare until T:

$$L_i(\mu_i^{EW}, \mu_i^{RW}) = \left[\lambda_{i,t^w}^{EW}(d_{i,t^w-1} \mid \mu_i^{EW}) \prod_{t=2}^{t^w-1} (1 - \lambda_{i,t}^{EW}(d_{i,t-1} \mid \mu_i^{EW})\right]$$
$$\prod_{t=t^w+1}^{T} (1 - \lambda_{i,t}^{RW}(d_{i,t-1} \mid \mu_i^{RW})$$

3. Respondents who experience *k* welfare spells and *k-1* non-welfare spells and who are on welfare at T (i.e. the last, right censored spell is a welfare spell):

$$L_i(\mu_i^{EW}, \mu_i^{RW}) = \prod_{m=1}^{k-1} \left\{ \left[\lambda_{i,t^m}^{EW}(d_{i,t^m-1} \mid \mu_i^{EW}) \prod_{t=2}^{t^m-1} (1 - \lambda_{i,t^m}^{EW}(d_{i,t^m-1} \mid \mu_i^{EW})\right] \right\}$$
$$\prod_{t=t^k+1}^{T} (1 - \lambda_{i,t^k}^{EW}(d_{i,t^k-1} \mid \mu_i^{EW})$$
$$\prod_{j=1}^{k-1} \left\{ \left[\lambda_{i,t^j}^{RW}(d_{i,t^j-1} \mid \mu_i^{RW}) \prod_{t=2}^{t^j-1} (1 - \lambda_{i,t^j}^{RW}(d_{i,t^j-1} \mid \mu_i^{RW})\right] \right\}$$

4. Respondents who experience *k* welfare spells and *k* non-welfare spells and who are not on welfare at T (i.e. the last, right censored spell is a non-welfare spell):

$$L(\mu_i^{EW},\mu_i^{RW}) = \prod_{m=1}^{k-1}\left\{\left[\lambda_{i,t^m}^{RW}(d_{i,t^m-1}\mid\mu_i^{RW})\prod_{t=2}^{t^m-1}(1-\lambda_{i,t^m}^{RW}(d_{i,t^m-1}\mid\mu_i^{RW})\right]\right\}$$
$$\prod_{t=t^k+1}^{T}(1-\lambda_{i,t^k}^{RW}(d_{i,t^k-1}\mid\mu_i^{RW})$$
$$\prod_{j=1}^{k-1}\left\{\left[\lambda_{i,t^j}^{EW}(d_{t^j-1}\mid\mu_i^{EW})\prod_{t=2}^{t^j-1}(1-\lambda_{i,t^j}^{EW}(d_{i,t^j-1}\mid\mu_i^{EW})\right]\right\}$$

In order to obtain the unconditional contributions in each case, the likelihood functions must be integrated over the support of the unobserved effects. Thus, to empirically implement the model, I need to specify the stochastic nature of unobserved heterogeneity. I choose to formulate a finite mixture model, which allows for unobserved heterogeneity in a flexible way without imposing a parametric structure, following Heckman and Singer (1984). Specifically, I assume that the unobserved heterogeneity components follow a factor structure:

$$\begin{aligned}\mu_i^{EW} &= \alpha^{EW} + \kappa^{EW}\theta_i\\ \mu_i^{RW} &= \alpha^{RW} + \kappa^{RW}\theta_i\end{aligned} \tag{3}$$

where θ_i follows a discrete distribution with a finite number of mass points *(M)*.[20] Note that in the absence of unobserved effects, μ_i^{EW} reduces to α^{EW} and μ_i^{RW} reduces to α^{RW}. The parameters κ^{EW} and κ^{RW} allow for correlation between μ_i^{EW} and μ_i^{RW}. Identification requires some normalizations, and I choose to set $\alpha^{EW}=0$ and $\kappa^{EW}=1$. The distribution parameters are defined as

$$\sum_{m=1}^{M} p_m = 1 \;\; and \;\; p_m \geq 0,\;\; m = 1,2,\ldots,M \tag{4}$$

and they are estimated using a logistic transformation:

$$p_m = \frac{\exp(q_m)}{\sum_{l=1}^{M}\exp(q_l)} \tag{5}$$

[20]This factor structure is common in empirical work; see for instance Ham and Lalonde (1996).

with the normalization that $q_M = 0$. Given the distributional assumptions of the unobserved heterogeneity components, the contribution to the likelihood function for a given individual, *i*, is

$$\log L_i = \log \sum_{m=1}^{M} p_m L_i(\mu_i(m)) \tag{6}$$

where $\mu_i = \left\{\mu_i^{EW}, \mu_i^{RW}\right\}$ and $L_i(\mu_i(m))$ is the likelihood function, conditional on the unobserved effects, as described above.

In this paper, I set M = 2. Generally, a low dimensionality has been found sufficient in many studies of mixture models (e.g. Ham & Lalonde, 1996; Eberwein et al., 1997; Stevens, 1999; Cameron & Heckman, 2001; Hansen & Lofstrom, 2001; Card & Hyslop, 2005; Hansen & Wahlberg, 2004; and Zabel et al., 2004).

While the specification of the unobserved effects in equation (3) allows for correlation between μ_i^{EW} and μ_i^{RW}, it assumes that the effects are uncorrelated with the observable characteristics that are included in $\mathbf{X}_{i,t-1}$ and $\mathbf{Z}_{i,t-1}$. For instance, this assumption implies that education and other forms of human capital are uncorrelated with such unobserved characteristics as labour-market ability and preferences for work. Clearly, this assumption is restrictive and unlikely to hold in the present context. Moreover, a violation to this assumption will yield estimates that are inconsistent and unreliable. To address this potential misspecification, I will use the fact that, for many respondents in this sample, human capital changes over time and formulate a version of the "correlated random effects" model (see Chamberlain, 1980; Mundlak, 1978; and Wooldridge, 2002, forthcoming). In this approach, the unobserved effects $\left(\mu_i^{EW}, \mu_i^{RW}\right)$ are assumed to be linearly related to a selection of observed regressors in $\mathbf{X}$ (or $\mathbf{Z}$) as follows:

$$\begin{aligned} \mu_i^{EW} &= \alpha^{EW} + \mathbf{X}^*_i \lambda^{EW} + \kappa^{EW} \nu_i \\ \mu_i^{RW} &= \alpha^{RW} + \mathbf{Z}^*_i \lambda^{RW} + \kappa^{RW} \nu_i \end{aligned} \tag{7}$$

where $\mathbf{X}^*_i$ and $\mathbf{Z}^*_i$ are row vectors of individual averages over time of all time-varying human capital variables in $\mathbf{X}$ and $\mathbf{Z}$, respectively. λ^j *(j=EW,RW)* is a vector of parameters to be estimated, and ν_i is an error term assumed to be independent of $\mathbf{X}^*_i$, $\mathbf{Z}^*_i$, $\mathbf{X}_{i,t-1}$, and $\mathbf{Z}_{i,t-1}$.

Finally, as was shown in the previous section, those who eventually received the income supplement (the take-up group) are not randomly selected from the overall program group. This fact may contaminate the initial randomization of respondents into control and program groups, and in order to estimate the effect of receiving the supplement, the treatment group indicators defined above ($ELIG_{it}$ and $TAKEUP_{it}$) must be treated as endogenous. A similar endogeneity problem was encountered by Eberwein et al. (1997) in their paper on the impact of classroom training on employment. However, in their case, both control and program group members had access to training, and they could therefore use the randomly assigned treatment group variable as a predictor for the endogenous variable indicating participation in classroom training. In the SSP Applicant study however, only program group members could eventually receive the earnings supplement, and thus the random assignment into control and program groups cannot be used in addressing this endogeneity issue. Instead, identification will rely on functional form assumptions and I assume that the hazard of potential eligibility can be specified as

$$\lambda_{it}^{EL}(d_{i,t-1} \mid \mu_i^{EL}) = \Phi(\mu_i^{EL} + \widetilde{\mathbf{X}}_{i1}\beta^{EL} + \gamma^{EL}(d_{i,t-1}))$$

where $\widetilde{\mathbf{X}}_{i1}$ is a vector of observable characteristics at baseline (age, gender, immigrant status, First Nations ancestry status, educational attainments [high school only, completed vocational school, attended college or university], and work experience) and μ_i^{EL} is an unobserved, time-invariant, individual-specific effect that determines the probability of remaining on welfare during the 12-month eligibility period.

The hazard of initiating the income supplement is similarly defined:

$$\lambda_{it}^{TA}(d_{i,t-1} \mid \mu_i^{TA}) = \Phi(\mu_i^{TA} + \widetilde{\mathbf{X}}_{i1}\beta^{TA} + \gamma^{TA}(d_{i,t-1}))$$

where μ_i^{TA} is an unobserved, time-invariant, individual-specific effect that determines the probability of taking up the earnings supplement. By allowing μ_i^{EL} and μ_i^{TA} to be correlated with μ_i^{EW} and μ_i^{RW}, the endogeneity issue is addressed and I will assume that the four unobserved effects follow a factor structure:

$$\begin{aligned}
\mu_i^{EW} &= \alpha^{EW} + \mathbf{X}^*_i\lambda^{EW} + \kappa^{EW}\nu_i \\
\mu_i^{RW} &= \alpha^{RW} + \mathbf{Z}^*_i\lambda^{RW} + \kappa^{RW}\nu_i \\
\mu_i^{EL} &= \alpha^{EL} + \kappa^{EL}\nu_i \\
\mu_i^{TA} &= \alpha^{TA} + \kappa^{TA}\nu_i
\end{aligned} \tag{10}$$

where ν_i follows a discrete distribution with a finite number of mass points *(M)*. The parameters κ^{RW}, κ^{EL}, and κ^{TA} allow for correlation between the unobserved effects. As mentioned above, identification requires some normalizations, and I choose to set $\alpha^{EW} = 0$ and $\kappa^{EW} = 1$. The distribution parameters are estimated using a logistic transformation, and the contribution to the likelihood function for a given individual, *i*, is

$$\log L_i = \log \sum_{m=1}^{M} p_m L_i(\mu(m)) \tag{11}$$

where

$$\mu(m) = \left\{\mu^{EW}(m), \mu^{RW}(m), \mu^{EL}(m), \mu^{TA}(m)\right\}$$

and

$$L_i(\mu\,(m)) = L(\mu^{EW}, \mu^{RW})\left\{ f_{EL}\left(t^{EL} \mid \mu_i^{EL}\right)^{PROG_i} f_{TA}\left(t^{TA} \mid \mu_i^{TA}\right)^{PROG_i * ELIGIBLE_i} \right\} \quad (12)$$

where $L(\mu^{EW}, \mu^{RW})$ is defined above, *ELIGIBLE$_i$* equals one for respondents who are potentially eligible for the earnings supplement and equals zero otherwise, and

$$f_j\left(t^j \mid \mu_i^j\right) = \lambda_{it}^j\left(t^j \mid \mu_i^j\right)\prod\nolimits_{s=1}^{t^j-1}\left[1-\lambda_{is}^j\left(s \mid \mu_i^j\right)\right] \text{ if uncensored}$$

$$= \prod\nolimits_{s=1}^{t^j}\left[1-\lambda_{is}^j\left(s \mid \mu_i^j\right)\right] \text{ if right censored}$$

where $j=EL,TA$.

A DISCRETE-TIME COMPETING RISKS MODEL

In addition to examining total time on welfare and how the exit and re-entry rates are related to observable characteristics, it is interesting to investigate why welfare participants leave welfare. Reasons for leaving welfare include changes in household composition (both changes in marital status and changes in number of children), increases in earned income, and changes in other income. In this paper, I will follow Blank (1989) and restrict attention to the first welfare spell and consider the following destination states: change in marital status, earnings increase, and other reasons.[21]

To jointly estimate the hazard rates to the different destination states using a competing-risks framework, I specify the probability of a transition from welfare to state *j (j=1,2,3)* for marriage, earnings increase, and other reasons) for individual *i* at time *t* as

$$\lambda_{it}^j(d_{i,t-1} \mid \mu_i^j) = \Phi(\mu_i^j + \mathbf{X}_{i,t-1}\beta^j + \gamma^j(d_{i,t-1})) \quad (13)$$

where μ_i^j is an unobserved, time-invariant, individual-specific effect (representing ability, motivation, preferences, etc.), $\mathbf{X}_{i,t-1}$ is a vector containing observable characteristics, $\gamma^j(d_{i,t-1})$ is a function designed to capture duration dependence, and $\Phi(.)$ the standard normal cdf. The assumptions regarding the unobserved heterogeneity are the same as those presented above for the multiple spell framework (summarized in equations [4], [5], and [7]). Thus, the unobserved effects are allowed to be correlated with human capital and this setup

[21] Details on how the destination states were defined can be found in the data section above. Multiple welfare spells were not considered in this context since the number of destination states (including returns to welfare) becomes very large even for a limited number of transitions. For example, there are 27 possible destination states after the first three transitions. While it would be of great interest to learn more about the recidivism rates depending on reasons for exiting welfare, such an analysis is beyond the scope of this paper.

will also allow for dependence between the destination states. The correlation between states will be determined by the factor loading parameters (*κ^i, j=1,2 2)*and the likelihood function is constructed using sample information on duration of first welfare spells as well as information about the exit states. Given the definition of the transition rates and the assumption regarding the unobserved effects, the unconditional contribution to the likelihood function for respondents who are still receiving welfare at the end of the observation period is

$$L_i^c = \sum_{m=1}^{M} p_m \left\{ \left[\prod_{s=1}^{T} \left(1-\lambda_{is}^{MA}\right)\left(1-\lambda_{is}^{EA}\right)\left(1-\lambda_{is}^{OT}\right) \right] f_{EL}\left(t^{EL} \mid \mu_i^{EL}\right)^{PROG_i} \right\} \tag{14}$$

while for respondents who leave welfare at time t^* and go to destination state j, the likelihood contribution is

$$L_i = \sum_{m=1}^{M} p_m \left\{ \left[\lambda_{it^*}^{j} \prod_{s=1}^{t^*-1} \left(1-\lambda_{is}^{MA}\right)\left(1-\lambda_{is}^{EA}\right)\left(1-\lambda_{is}^{OT}\right) \right] f_{EL}\left(t^{EL} \mid \mu_i^{EL}\right)^{PROG_i} \right\} \tag{15}$$

where λ_{is}^{MA} denotes the hazard rate for exits to marriage, conditional on unobservable effects μ_i^{MA}, λ_{is}^{EA} denotes the conditional hazard rate for exits because of increase in earnings, and finally, λ_{is}^{OT} denotes the conditional hazard rate for exits because of other reasons. As for the multiple spell model presented above, the unobservable effects (μ_i^j) are allowed to be correlated with time-varying human capital (education, work-related training, and duration of current part-time employment spell). Moreover, the eligibility decision is endogenized in the same fashion as for the multiple spell specification. However, since the take-up effect is not identified for first welfare spells (by construction, this effect can only be observed for respondents who re-enter welfare during the take-up period), the variable $TAKEUP_{it}$ is excluded from the set of covariates. Finally, as in the multiple spell specification, M is set to 2.

Empirical Results

In this section, I will discuss present results on exit and re-entry rates in welfare use. In order to illustrate the effects of work experience, work-related training, and earnings supplements on total time on welfare, I will generate counterfactual outcomes for the control group respondents using estimates from the most general model specification. This is followed by a discussion of the results for the competing risks model. Finally, indications on how well the empirical models fit the observed data on welfare use are presented.

RESULTS FROM A MULTIPLE SPELL HAZARD MODEL OF WELFARE EXITS AND RE-ENTRIES

I report results from maximizing the likelihood function in equation (12) above, under different assumptions regarding the unobservable effects, in tables 12 to 14. The entries in Table 12 show a selection of estimates associated with the hazard of leaving IA, while the entries in Table 13 show the corresponding estimates for the hazard of returning to IA. Table 14 presents the distribution of unobservable effects. The remaining estimates are presented in tables A.1–A.3 in the Appendix.

Starting with estimates for the exit hazard in Table 12, Model 1 refers to a model specification where welfare and non-welfare spells are assumed to be uncorrelated.[22] The estimates obtained under this assumption indicate that education has a significant effect on the probability of leaving welfare. The effect is non-linear and larger for having attended university than for having completed high school. This result is similar to that reported for women by Barrett (2000). Completion of work-related training also significantly increases the probability of leaving welfare. The results also suggest that the duration of current part-time employment spells and work experience at baseline are positively correlated with the exit rate. The treatment effects in this specification are: $\hat{\tau}^{EW}$ = 1.522 and $\hat{\eta}^{EW}$ = -0.109, and both effects are significant at conventional levels. These estimated treatment effects suggest that the exit rate is substantially higher for the "take-up" group during the take-up or entitlement period compared with the control group. Moreover, the exit rate during the period prior to taking-up the earnings supplement (which can be up to a maximum of 12 months for potentially eligible respondents) is somewhat lower than that for the control group respondents. While a large positive effect during the take-up period has also been reported by Card and Hyslop (2005) and Zabel et al. (2004), the negative "pre-incentive" treatment effect contrasts the results reported in Zabel et al. (2004). Their model specification differs from the one used in this paper, in particular the variable definitions differ, and this may explain some of this difference. Another, and perhaps more important, reason for the difference is underlying differences in SSP Recipient study and SSP Applicant study.

[22] A reduced version of Model 1 that considered only exits from initial welfare spells yielded results that were similar to those for Model 1 in Table 12, although the magnitudes of the coefficients were generally larger.

Table 12: Multiple Spell Model Estimates for Human Capital and Program Status Variables on Hazard Rates From Welfare

	Model 1		Model 2		Model 3		Model 4	
Variable	**Est.**	**s.e.**	**Est.**	**s.e.**	**Est.**	**s.e.**	**Est.**	**s.e.**
Human capital at t-1								
High school only	0.087*	0.037	0.071	0.053	-0.073	0.090	0.078	0.096
Completed vocational school	0.064	0.040	0.127*	0.052	0.008	0.093	0.058	0.098
Attended university	0.204*	0.036	0.208*	0.047	0.001	0.067	0.057	0.068
Completed work-related training	0.262*	0.053	0.291*	0.066	0.136*	0.075	0.101	0.074
Duration of current full-time employment spell	n.a.		n.a.		n.a.		n.a.	
Duration of current part-time employment spell	0.009*	0.003	0.008*	0.004	0.004	0.004	0.008*	0.004
Work experience (years) at baseline	0.021*	0.003	0.033*	0.005	0.031*	0.005	0.015*	0.004
Program status at t-1								
Take-up	1.525*	0.072	1.684*	0.083	1.709*	0.082	1.800*	0.096
Eligible	-0.106*	0.055	-0.139*	0.061	-0.121*	0.061	-0.067	0.064
Treatment	-0.002	0.031	-0.027	0.043	-0.018	0.043	-0.034	0.041
Treatment effects								
Entitlement effect	1.522*	0.070	1.657*	0.084	1.691*	0.082	1.755*	0.092
Pre-incentive effect	-0.109*	0.052	-0.166*	0.067	-0.138*	0.066	-0.101	0.060

Notes: *denotes significance at the five per cent level. "Est." denotes parameter estimates, while "s.e." denotes estimated standard errors.

Based on estimation results from a multiple spell hazard model of welfare exits and re-entries. Model 1 assumes that welfare and non-welfare spells are uncorrelated. Model 2 allows unobservables in welfare and non-welfare spells to be correlated (dynamic self-selection). Model 3 extends Model 2 by allowing human capital variables to be correlated with unobservable effects. Model 4 extends Model 3 by allowing the eligibility and take-up decisions to be endogenous.

All model specifications also include controls for age, gender, First Nations ancestry status, immigrant status, indicator for presence of children less than 5 years old, indicator for marital status, duration of current marriage, log(duration), and its square. The associated estimates for these control variables are presented in Table A.1 in the Appendix. In all specifications controlling for unobservable effects, M = 2. The log-likelihood values are -12,544 for Model 1, -12,514 for Model 2, -12,486 for Model 3, and finally, -13,750 for Model 4.

The entries in the third and fourth columns of Table 12 refer to a specification that allows for "dynamic self-selection" by modeling the correlation between unobservable effects in both welfare and non-welfare spells. The estimates show that completion of a vocational education or attending university is associated with a higher likelihood of leaving welfare compared with having less than high school or having completed high school only. Similar to the results for Model 1, the completion of work-related training is associated with higher exit rates from welfare. The treatment effects in this specification are similar to those reported above, $\hat{\tau}^{EW} = 1.657$ and $\hat{\eta}^{EW} = -0.166$, and both are significantly different from zero at conventional levels.

Table 13: Multiple Spell Model Estimates for Human Capital and Program Status Variables on Hazard Rates From Non-welfare

	Model 1		Model 2		Model 3		Model 4	
Variable	**Est.**	**s.e.**	**Est.**	**s.e.**	**Est.**	**s.e.**	**Est.**	**s.e.**
Human capital at t-1								
High school only	-0.020	0.058	-0.022	0.060	0.213	0.124	0.298*	0.141
Completed vocational school	0.082	0.058	0.104	0.059	0.050	0.136	0.092	0.152
Attended university	-0.033	0.053	-0.030	0.055	-0.017	0.075	-0.060	0.089
Completed work-related training	-0.117*	0.061	-0.141*	0.065	-0.187*	0.090	-0.106	0.105
Duration of current full-time employment spell	-0.020*	0.003	-0.021*	0.003	-0.023*	0.004	-0.025*	0.006
Duration of current part-time employment spell	-0.008*	0.004	-0.008*	0.004	0.001	0.005	0.002	0.006
Work experience (years) at baseline	-0.010*	0.005	-0.008	0.005	-0.008	0.006	-0.012*	0.006
Program status at t-1								
Take-up	-0.146	0.080	-0.183*	0.085	-0.179*	0.086	-0.990*	0.278
Treatment	-0.039	0.047	-0.036	0.048	-0.035	0.048	-0.203*	0.058
Treatment effects								
Entitlement effect	-0.185*	0.077	-0.218*	0.082	-0.214*	0.083	-1.193*	0.276

Notes: *denotes significance at the five per cent level. "Est." denotes parameter estimates, while "s.e." denotes estimated standard errors. Based on estimation results from a multiple spell hazard model of welfare exits and re-entries. Model 1 assumes that welfare and non-welfare spells are uncorrelated. Model 2 allows unobservables in welfare and non-welfare spells to be correlated (dynamic self-selection). Model 3 extends Model 2 by allowing human capital variables to be correlated with unobservable effects. Model 4 extends Model 3 by allowing the eligibility and take-up decisions to be endogenous.

All model specifications also include controls for age, gender, First Nations ancestry status, immigrant status, indicator for presence of children less than 5 years old, indicator for marital status, duration of current marriage, log(duration), and its square. The associated estimates for these control variables are presented in Table A.2 in the Appendix.

Models 1 and 2 rely on the questionable assumption that the unobservable effects (such as preferences and labour-market ability) are uncorrelated with educational attainments, training, and work experience. Model 3, whose exit parameters are shown in columns 5 and 6, relaxes this assumption, and the estimates for educational attainments are no longer significantly different from zero at conventional levels. This suggests that the positive effects of education found for models 1 and 2 are spurious and do not represent any causal effects of education on the hazard rate from welfare. Instead, it appears that those with more education have unobserved (to the econometrician) traits that make them more likely to leave welfare early. This result contrasts virtually all previously documented effects of education on welfare exit probabilities and suggests that the conventional wisdom that education reduces time on welfare is generally based on biased and inconsistent parameter estimates. While allowing for correlation between human capital and unobservable effects has substantial impacts on the effects of education, it has less of an impact on the effect of work-related training. The coefficient associated with training is smaller than those found for models 1 and 2, but it remains positive and significant (the associated p-value is 0.070). Duration of part-time employment has a positive but insignificant effect, while accumulated years of work experience at baseline have a positive and significant effect. Finally, the treatment effects in this specification are similar to those reported above: $\hat{\tau}^{EW}$ = 1.691 and $\hat{\eta}^{EW}$ = -0.138, with standard errors 0.082 and 0.066, respectively.

Table 14: Multiple Spell Model Estimates for the Distribution of Unobservable Effects

	Model 2		Model 3		Model 4	
Parameter	**Est.**	**s.e.**	**Est.**	**s.e.**	**Est.**	**s.e.**
Constant						
α^{EW}	0.0		0.0		0.0	
α^{RW}	-1.258*	0.193	-1.190*	0.204	-0.817*	0.213
α^{EL}	n.a		n.a		-0.448	0.789
α^{TA}	n.a		n.a		-1.560	12.150
Factor loading						
κ^{EW}	1.0		1.0		1.0	
κ^{RW}	-0.162*	0.082	-0.118	0.082	0.018	0.041
κ^{EL}	n.a		n.a		4.493*	0.311
κ^{TA}	n.a		n.a		0.022	6.977
θ_1	-1.574*	0.161	-1.733*	0.165	-1.39*	0.157
θ_2	-0.720*	0.158	-0.873*	0.160	-0.137	0.151
q_1	-0.155	0.198	-0.067	0.192	1.227*	0.098

Notes: *denotes significance at the five per cent level. "Est." denotes parameter estimates, while "s.e." denotes estimated standard errors.

Based on estimation results from a multiple spell hazard model of welfare exits and re-entries. Model 1 assumes that welfare and non-welfare spells are uncorrelated. Model 2 allows unobservables in welfare and non-welfare spells to be correlated (dynamic self-selection). Model 3 extends Model 2 by allowing human capital variables to be correlated with unobservable effects. Model 4 extends Model 3 by allowing the eligibility and take-up decisions to be endogenous.

All model specifications also include controls for age, gender, First Nations ancestry status, immigrant status, indicator for presence of children less than 5 years old, indicator for marital status, duration of current marriage, log(duration), and its square. The associated estimates for these control variables are presented in Table A.1 in the Appendix. In all specifications controlling for unobservable effects, M=2. The log-likelihood values are -12,544 for Model 1, -12,514 for Model 2, -12,486 for Model 3, and finally, -13,750 for Model 4.

The last set of results in Table 12 refers to a model specification similar to that of Model 3 but with the difference that the decisions to become potentially eligible for the earnings supplement and to take up the supplement are both considered endogenous. As was shown in the data section above, those who eventually received the supplement are very different in terms of their human capital (both stock at baseline and changes over the survey period) from those who qualified by becoming potentially eligible but failed to obtain full-time employment within the required timeframe. It is likely that there is not only a selection on observables, but also on unobservables, implying that both decisions should be considered endogenous. The results regarding education, training, and work experience from this general model specification (Model 4) are quite similar to those of Model 3. Specifically, education does not have a significant impact on the exit rate. The training effect is smaller but remains positive. However, the significance level has dropped and the estimate is now borderline significant (the p-value equals 0.17). The effect of work experience at baseline is also lower, less than half of that found in Model 3, but remains significant at common significance levels. The treatment effects in this specification are $\hat{\tau}^{EW}$ = 1.755 and $\hat{\eta}^{EW}$ = -0.101. The "take-up" effect is in the same range as those obtained using more restrictive models above and precisely estimated. However, the “pre-incentive” effect is closer to zero compared with the effects reported above and not significantly different from zero.

The effects of human capital and earnings supplements on the re-entry rates are shown in Table 13. In the most restrictive model specification (Model 1), educational attainments have no significant effects on the probability of returning to welfare. Relaxing the exogeneity assumption on education (models 3 and 4) implies that respondents who have completed vocational school or attended university do not have lower re-entry rates than respondents who never completed high school. However, the re-entry rate for respondents who obtain a high school diploma but no additional education is estimated to be greater than the re-entry rate for high school drop-outs. The estimated coefficients for work-related training are negative across all model specifications but only significant for the more restrictive models (models 1–3). For the exit rates, the duration of a current full-time employment spell (measured in months) is added to the set of covariates, and the effect of this variable on the re-entry rate is virtually the same across models and precisely estimated. The estimate is numerically small and close to zero (it varies between -0.025 and -0.020) because of the unit of measurement and does not mean that the economic impact of full-time employment is negligible. While full-time employment appears to reduce the risk of returning to welfare, perhaps through learning, part-time employment does not have any significant effect on the re-entry hazard in the most general model specifications. Accumulated work experience at baseline, measured in years, reduces the risk of returning to welfare, but the estimate is marginally significant for the most general model (p-value equals 0.064). Finally, the estimated treatment effect ($\hat{\tau}^{RW}$) suggests that the probability of returning to welfare is lower for the “take-up” group during the take-up or entitlement period compared with the control group. The estimated effect is -0.214 in Model 3, with a standard error of 0.083, and -1.193 in Model 4, with a standard error of 0.276. The difference between these two estimates rests entirely on the different assumptions made regarding the endogeneity of the eligibility and take-up decisions. The pattern of a larger treatment effect (in absolute terms) in Model 4 compared with Model 3 suggests that respondents who are less likely to leave welfare are more likely to become potentially eligible for the supplement and also to receive the supplement. This is consistent with the structure of the SSP Applicant study and with the pattern in the data, described in the data section above, which distinguished between three types of program group members: those who left IA before becoming potentially eligible (the not-eligible group), those who became potentially eligible but never took-up the supplement (the no-take-up group), and those who became potentially eligible and took-up the supplement (the take-up group). The specification in Model 3 ignores the sorting into these three groups, treats the allocation of respondents into groups as exogenous, and consequently underestimates the treatment effect.

The estimated distributions of unobserved heterogeneity for models 2–4 are presented in Table 14. For Model 2, the estimate of the factor-loading parameter is negative (-0.162) and significant. This implies that the correlation between the unobserved factors determining the duration of welfare and non-welfare spells is negative and suggests that those who are less likely to leave welfare are more likely to return to welfare once they have left welfare. The estimate of the factor-loading parameter in Model 3 is negative but insignificant. This suggests that, once the portion of unobservable effects that are correlated with human capital has been controlled for, the remaining share is not correlated across welfare and non-welfare spells. For Model 4, the factor-loading parameter κ^{RW} is positive but not significant. This model specification contains two additional factor-loading parameters, one for the eligibility hazard (κ^{EL}) and one for the take-up hazard (κ^{TA}). The former is positive and significant while the latter is positive and insignificant. The positive value of κ^{EL} suggests that those who are less likely to leave welfare are more likely to become potentially eligible for the earnings supplement.

While the estimates reported above indicate the direction of the effect of changes in observable characteristics on the hazard rates, the relatively complicated nature of the model makes it difficult to assess the impact of these characteristics on overall welfare use. One way to illustrate the impact of the provision of earnings supplements and training opportunities on expected total time on welfare over a specific time period is to simulate counterfactual experiments for hypothetical respondents.[23] The parameter estimates from the most general model (Model 4) along with the stochastic assumptions made on the unobservables are used to generate values of the latent variables that underlie the hazard functions. Specifically, the two following latent variables

$$I_{it}^{EW} = \hat{\mu}_i^{EW} + \mathbf{X}_{i,t-1}\hat{\beta}^{EW} + \hat{\gamma}^{EW}(d_{i,t-1}) + \varepsilon_{it}^{EW}$$

and

$$I_{it}^{RW} = \hat{\mu}_i^{RW} + \mathbf{X}_{i,t-1}\hat{\beta}^{RW} + \hat{\gamma}^{RW}(d_{i,t-1}) + \varepsilon_{it}^{RW}$$

were generated for each t = 1,…,72, holding the observable characteristics constant. $\hat{\mu}_i^{EW}$ and $\hat{\mu}_i^{RW}$ were estimated as

$$\sum_{j=1}^{2}\left(\hat{\nu}_j + \mathbf{X}^*{}_i\hat{\lambda}^{EW}\right) * \hat{\pi}_j$$

and

$$\sum_{j=1}^{2}\left(\hat{\alpha}^{RW} + \mathbf{Z}_i^*\hat{\lambda}^{RW} + \hat{\kappa}^{RW}\hat{\nu}_j\right) * \hat{\pi}_j$$

respectively. Finally, 740 values of ε_{it}^{EW} and ε_{it}^{RW} were obtained for each t = 1,…,72, from independent and identically distributed (i.i.d.) random draws from a standard normal distribution. A transition from welfare occurs whenever I_{it}^{EW} is positive and a re-entry into welfare occurs whenever I_{it}^{RW} is positive.

Since formal education has no significant impact on either transition rate, the simulation analysis will focus on the effects of full-time employment, work-related training, and

[23]Welfare histories are simulated for respondents who have not completed a high school degree at Month 72 and are 33 years old at baseline, continuously single, born in Canada, female, not of First Nations ancestry, have no children under 5 years of age, and have no work experience at baseline.

earnings supplement. The simulation outcomes are shown in Table 15. Initially, welfare outcomes were generated assuming that the respondents belong to the control group, never complete any training, and are never employed full time over the six-year period. The expected time on welfare under these assumptions, presented in column 1, is 37.8 months, just over three years. To quantify the effect of the SSP earnings supplement on time spent on welfare, the assumptions from the initial simulations were retained but with the difference that respondents became potentially eligible for the earnings supplement 12 months after baseline and took up the supplement 24 months after baseline. It was assumed that they received the supplement for 36 months, the full duration of the SSP supplement. The welfare histories generated by this experiment, shown in column 2, yielded an average time on welfare equal to 21.4 months, a reduction of about 43 per cent. Reducing the take-up period from 36 months to 12 months reduced the average time on welfare to 26 months, corresponding to a reduction of 31 per cent. Thus, not surprisingly and consistent with previous findings, the earnings supplement significantly reduces welfare use and suggests that economic incentives are important. Obviously, the longer the earnings supplement is in effect, the larger will the reduction in welfare use be. The estimates regarding the effects of the supplement suggest that when the supplement ceases to exist, re-entry rates increase and exit rates decrease. This suggests that there may only be limited long-term effects of time-limited economic incentives.

Table 15: Simulation of Welfare Use Based on Estimates From a Multiple Spell Hazard Model of Welfare Exits and Re-entries (Model 4 in Tables 12–14)

	Belongs to the Control Group, Never Employed After Baseline and Never Completes Training (1)	Same as (1) but With $PROG_i = 1$, $ELIG_{it} = 1$ for $11 < t < 23$, $TAUP_{it} = 1$ for $23 < t < 61$ (2)	Same as (1) but With $train_{it} = 1$ for $t > 11$ (3)	Same as (1) but Works Full-Time From Month 12 and Onwards (4)	Same as (2) but Works Full-Time From Month 24 and Onwards (5)
Months on welfare	37.8	21.4	34.6	33.8	19.8
Reduction compared with (1) (%)	-	-43.0	-8.5	-10.6	-56.0

Notes: The entries show expected total months on welfare over a period of six years (72 months) as a function of program group status, full-time employment, and training. Entries are calculated for an individual who has not completed a high school degree at Month 72 and is 33 years old at baseline, continuously single, born in Canada, female, not of First Nations ancestry, has no children under 5 years of age, and has no work experience at baseline.

It may also be interesting to compare the effects of a time-limited earnings supplement with the effects of increasing the skill levels of respondents. Basically, labour-market skills can be acquired through formal education (general type of training), work-related training (combination of general and job-specific training), and by learning on the job (job-specific training). This latter category is generally unobserved and may be approximated by information on employment. In this paper, it is possible that a portion of the positive effects on welfare use from full-time employment is due to learning (other possibilities for the positive effects are changing preferences and wage growth not linked to learning).

To assess the effects of work-related training, outcomes were simulated holding observable characteristics identical to those that generated the initial simulated outcomes (reported in column 1), with the exception of completion of work-related training. It was assumed that respondents had completed training 12 months after baseline, and the average time on welfare in this case (reported in column 3) is 34.6 months, a reduction of 8.5 per cent. Thus, training is found to have a relatively small effect and the parameter estimates for training both in the exit and the re-entry hazards were not significant at conventional levels. This result is consistent with much of the previous literature devoted to evaluate labour-market effects of training.

In order to estimate the effects of full-time employment (learning), outcomes were again simulated holding observable characteristics identical to those that generated the initial simulated outcomes (reported in column 1), with the exception of the duration of the current full-time employment spell. Instead, it was assumed that respondents started to work full time 12 months after baseline and remained employed full time until the end of the survey period. The average time on welfare under these assumptions, shown in column 4, is 33.8 months, which corresponds to a reduction of about 11 per cent. It should be noted that it is assumed that respondents had no work experience at baseline, and given the estimates of baseline experience on both exit and re-entry rates, the overall effect of work experience is greater than the 11 per cent reduction, since that effect is solely attributed to full-time employment during the six-year period.

Finally, given the structure of the SSP earnings supplement, which was conditional on working full time, it is worth emphasizing that the total effect of SSP is due both to the treatment effect (holding everything else constant) and to the effect of full-time employment. Thus, a positive externality of the earnings supplement is the work experience gained by those who took up the supplement, which was shown to have a significant effect on reducing the risk of returning to welfare. Combining these two elements (supplement for 36 months combined with full-time employment from 36 months and onwards) in the simulation exercise yields an average time on welfare of 19.8 months, a reduction of 56 per cent compared with the initial scenario where it was assumed that respondents were never exposed to the supplement and never worked full time during the six-year period.

To summarize, these results, although only illustrative and highly dependent on the assumptions made, show that both labour market skills and economic incentives can reduce welfare use. However, the effects of economic incentives depend on the duration of the subsidies and skill improvements via learning (and, to some extent, training) may have greater effects in the very long term.

RESULTS FROM A COMPETING RISKS HAZARD MODEL OF WELFARE EXITS

I report results from a three-way competing risks model in Table 16. The table presents estimates associated with human capital, program group status, and the distribution of unobserved heterogeneity on exit rates from initial welfare spells. The remaining parameter estimates are presented in Table A.4 in the Appendix. Following Blank (1989), I consider three alternative hazard rates: one for individuals who leave welfare because of marriage, one for those who leave welfare through an earnings increase, and finally one for those who remain single, but leave welfare for other reasons than increase in their earnings. Details on the definition of exit states are provided in the data section above.

Table 16: Estimation Results From a Competing Risks Hazard Model of Welfare Exits

Exit to:	Marriage		Earnings Growth		Other	
Variable	**Est.**	**s.e.**	**Est.**	**s.e.**	**Est.**	**s.e.**
Human capital						
High school only	-0.117	0.207	-0.128	0.092	0.127	0.111
Completed vocational school	0.105	0.215	0.117	0.092	0.150	0.106
Attended university	0.236	0.203	0.055	0.082	-0.067	0.097
Completed work-related training	-0.395	0.312	0.017	0.079	0.121	0.099
Duration of current part-time employment spell	0.010	0.010	0.011*	0.005	0.002	0.005
Work experience (years) at baseline	-0.008	0.010	0.018*	0.004	0.013*	0.005
Program status at t-1						
Eligible	-0.140	0.136	-0.382*	0.057	-0.220	0.074
Treatment	0.057	0.085	0.212*	0.037	0.118*	0.045
Unobserved heterogeneity:						
Constant $\left(\alpha^j\right), j = earning, other$	0.0		0.947	0.521	0.609	0.522
Factor loading $\left(\kappa^j\right), j = earning, other$	1.0		1.140*	0.131	1.101*	0.130
θ_1	-3.677*	0.401				
θ_2	-2.243*	0.366				
q_1	1.046*	0.070				
Log-likelihood value	-8,558					

Notes: *denotes significance at the five per cent level. "Est." denotes parameter estimates, while "s.e." denotes estimated standard errors.

Based on estimation results from a competing risks model that allows for (i) correlated risks, (ii) endogenous human capital variables, and (iii) endogenous eligibility decisions. The model also includes controls for age, gender, First Nations ancestry status, immigrant status, indicator for presence of children less than 5 years old, log(duration), and its square. The associated estimates for these control variables are presented in Table A.4 in the Appendix.

The likelihood of leaving welfare through marriage is not significantly affected by educational attainments or by completion of work-related training. Furthermore, program group status does not have a significant effect on this reason for leaving welfare. For exits due to increases in earnings, however, the entries in Table 16 suggest that work experience, both accumulated experience at baseline and the duration of the current part-time employment spell, significantly increase the likelihood of leaving welfare through increases in earnings (either due to an increase in hours of work or an increase in the hourly wage rate). The results also indicate that program group members are more likely than control group members to leave welfare through earnings increases. However, the estimate associated with $ELIG_{it}$ is negative (and significant), suggesting that the probability of leaving welfare via increases in earnings for program group members is lower during the eligibility period than during the period preceding the eligibility period. This result is driven by the fact that in the SSP Applicant study a majority of the program group leave welfare because of earnings increases before becoming potentially eligible for the earnings supplement (the not-eligible group).

The third destination state, labeled "exits for other reasons," consists of exits because of changes in welfare eligibility, not due to changes in marital status or increased earnings. This includes for instance those who have increases in non-earned income (including changes in the spouse's earned income), those whose children leave home, and those who do not report wage or earnings information. Exits to this absorbing state are positively related to accumulated work experience at baseline but not significantly related to education or work-related training. As for exits due to earnings increases, program group members are more likely than control group members to leave welfare because of "other" reasons.

Estimates for the distribution of unobservable effects reveal that the two support points for the finite distribution are significantly different from each other. Furthermore, the sign and significance of the factor-loading parameters indicate that spells are positively correlated across destination states.

Regarding the effects of other observable characteristics on different types of exit reasons, shown in Table A.4 in the Appendix, few are significant for exits due to changes in marital status. The only estimates that are significant at reasonable levels are those associated with the indicators for First Nations ancestry and immigrant status. The former is positive while the latter is negative, suggesting that the probability of leaving welfare because of marriage is highest for First Nations respondents and lowest for immigrants. There is evidence in favour of positive duration dependence. For exits because of earnings increases, age, gender, and immigrant status have negative and significant effects. There is evidence on significant effects of the duration of the current welfare spell on exit probabilities due to increases in earnings as indicated by the significant coefficients on the logarithm of duration of the current spell and its square. The positive sign on the former and negative sign on the latter mean that the estimated hazard rate has an inverted "U" shape, where the exit rates increase early in the spell and then start to decline. This result is consistent with the findings in Blank (1989) and may suggest that job and earnings opportunities as well as job-seeking activities change with time spent on welfare. Finally, regarding exits due to "other" reasons, the effects of age and duration dependence are similar to those found for exits via earnings increases.

To summarize, it appears that the process of leaving welfare is different depending on the reasons for the exits. Exits through changes in marital status are independent of work experience and program group status while the opposite is true for exits to the remaining destination states. Moreover, education and work-related training are not significantly related to any of the exit hazards. Finally, the exit rates are positively correlated with time spent on welfare.

EVALUATING HOW THE EMPIRICAL MODELS FIT ACTUAL DATA

To evaluate how the empirical models fit observed frequencies of welfare spells, predicted frequencies can be compared with the observed ones. In Table 17, I report predicted and observed frequencies for each six-month period, as well as average number of months, for the multiple spell model and for the competing risks model. I also report a goodness of fit measure, which is intended as an informal indication of the capacity of the models to fit the data.[24] While the statistic reported in Table 17 for the multiple spell model is

[24]The measure is calculated as $\sum_{c=1}^{24} \frac{\left(n_c - \hat{n}_c\right)^2}{\hat{n}_c}$ where n_c and $\hat{n}_c$ are, respectively, the observed and predicted frequency in each class. While this measure is indicative of the ability of the estimated model to fit actual distribution of welfare spells, it is not a completely reliable statistic, since its distribution is unknown and it does not account for the fact that the predicted frequencies are based on estimated parameters.

relatively high (59.8) compared with critical values of a chi-square distribution, a comparison of observed and predicted frequencies reveals a reasonably good fit. The predicted distribution follows the observed one reasonably well, but tends to overestimate the fraction of respondents receiving welfare for less than a year. The last two columns of Table 17 show the predicted and observed distributions of first welfare spells from the competing risks model as well as the value of the goodness of fit measure. The model fit is very good and the value of the statistic is 16.1, which suggests that the null hypothesis of no statistical differences between the observed and predicted distributions cannot be rejected at conventional levels (the p-value is 0.19).

Table 17: Predictions of Welfare Use From a Multiple Spell Hazard Model and a Competing Risks Hazard Model, Compared With the Observed Distributions of Welfare Use, Based on Reduced Sample (Sample Size = 1,492)

	Multiple Spell Model[a]		Competing Risks Model[b]	
Time (Months)	**Observed**	**Predicted**	**Observed**	**Predicted**
Less than 6	0.169	0.222	0.305	0.312
7–12	0.125	0.121	0.174	0.152
13–18	0.115	0.109	0.125	0.135
19–24	0.115	0.095	0.097	0.105
25–30	0.080	0.064	0.077	0.074
31–36	0.062	0.052	0.054	0.042
37–42	0.058	0.038	0.037	0.038
43–48	0.044	0.042	0.026	0.023
49–54	0.034	0.031	0.019	0.026
55–60	0.038	0.037	0.014	0.016
61–66	0.046	0.044	0.011	0.011
67–72	0.116	0.145	0.063	0.066
Average months	29.5	29.1	20.3	20.2
Goodness of fit measure	59.8		16.1	

Notes: [a]Predictions are based on estimates from a multiple spell model that allows for (i) correlation between welfare and non-welfare spells, (ii) endogenous human capital variables, and (iii) endogenous eligibility and take-up decisions (Model 4 in tables 12–14).

[b]Predictions are based on estimates from a competing risks model that allows for (i) correlation across destination states, (ii) endogenous human capital variables, and (iii) endogenous eligibility take-up decisions (Model in Table 16).

Finally, the importance of using multiple spells as opposed to single spells is also illustrated in Table 17. The single spell measure indicates that 30.5 per cent of the respondents experienced six months or less of IA receipt and that the average time on welfare equals 20.3 months. When considering multiple spells (i.e. the possibility that some of those who leave IA will return to welfare within the sample period), only 16.9 per cent of the respondents received IA for six months or less. Using this measure of time on welfare, the average time is 29.5 months. Thus, it is quite clear that focusing only on single welfare spells may substantially underestimate the total time of welfare receipt.

Conclusions

This paper analyzes the effects of income supplements and human capital on the dynamics of welfare use in Canada using administrative data on welfare spells combined with survey information from the Self-Sufficiency Project (SSP) Applicant study. The paper also investigates the effects of human capital and other observable characteristics on the reasons for leaving initial (in the data) welfare spells.

There are five main findings. First, using information from four follow-up surveys spanning a six-year period, it was found that a significant fraction of the sample increased their educational attainments over this period. For instance, at the last follow-up survey, the proportion of respondents who had completed high school was 72 per cent, an increase of 26 per cent compared with the high school completion rates at the baseline interview. The data also revealed substantial increases in the proportions who had completed vocational school and attended college or university. Significant differences in educational attainments, both at the baseline interview and at the 72-month follow-up survey, were found across the four treatment groups. At both interviews, the not-eligible and take-up groups had the highest proportion of high school graduates while the no-take-up group had the lowest proportion. However, the difference between the control group and the take-up group was not significant.

A second finding is that there was substantial upgrading of skills through work-related training, such as on-the-job training and apprenticeship training. At the first follow-up survey, 10 per cent had completed any form of work-related training. At the 72-month survey, this proportion had increased to 45 per cent of the respondents. The completion rates appear to be positively correlated with educational attainment and they also differed depending on treatment group status. The highest completion rates were found for the not-eligible and take-up groups, while the no-take-up group was least likely to have completed any work-related training by the 72-month survey.

A third conclusion is that, contrary to conventional wisdom, there appears to be no causal effects of formal education on either welfare exit rates or re-entry rates. Instead, the spurious correlations found both in the data and in the previous literature seem to be driven by sorting on unobservables, such as labour-market ability and preferences. The effects of work-related training had the expected signs, but were generally not significant at conventional levels. While education is not significantly related to welfare use, work experience (in particular full-time work experience) significantly increases exit rates and reduces re-entry rates. This may be due to skills upgrading on the job by learning and to changes in preferences and labour-market attachment.

The results also indicate that economic incentives matter. The provision of a generous earnings supplement significantly reduced time on welfare, both by increasing the probability of leaving income assistance (IA) and by reducing the risk of returning to IA. However, the positive effects were limited to the time periods when respondents received the supplement. A simple simulation exercise of welfare outcomes showed that, while holding everything else constant, the longer the earnings supplement is in effect, the larger will the reduction in welfare use be. Some concerns about the long-term effects of the SSP supplement have recently been raised (e.g. Card & Hyslop, 2005, and Foley, 2004), and it has been suggested that the supplement encourages employment in minimum-wage jobs, where there is little wage growth, and once the supplement period expires, respondents return to welfare to a large extent. This pattern is consistent with the results in this paper. However, the relatively strong positive effects of the duration of full-time employment spells mitigate the re-entry rates for former SSP recipients.

A final conclusion is that educational attainment is not significantly related to the exit rate out of welfare, regardless of the destination state. Work experience is found to increase the likelihood of leaving welfare because of increases in earnings or for unknown reasons and to reduce time on welfare. Program group members are more likely to leave welfare because of increases in earnings than are control group members. There are also indications that the exit rate out of initial welfare spells is positively correlated with time spent on welfare.

To summarize, the results show that both labour-market skills and economic incentives can reduce welfare use. Policies that would likely reduce welfare caseloads by significant numbers would aim to combine short-term economic incentives with provision of training and employment opportunities that would improve the labour-market skills of welfare recipients over a longer term.

Appendix

Table A.1: Estimates Associated With Personal Characteristics, Duration Dependence, and Correlated Random Effects on Hazard Rates From Welfare Using a Multiple Spell Framework

	Model 1		Model 2		Model 3		Model 4	
Variable	**Est.**	**s.e.**	**Est.**	**s.e.**	**Est.**	**s.e.**	**Est.**	**s.e.**
Personal characteristics								
Age at baseline	-0.016	0.003	-0.021	0.005	-0.021	0.005	-0.016	0.004
Female	0.053	0.059	0.055	0.080	0.061	0.078	-0.031	0.077
First Nations ancestry	0.246	0.083	0.407	0.147	0.441	0.129	0.284	0.109
Immigrant	-0.146	0.033	-0.251	0.049	-0.256	0.049	-0.177	0.041
Married at t-1	0.658	0.070	0.822	0.084	0.823	0.084	0.804	0.085
Duration of current marriage at t-1	-0.017	0.004	-0.017	0.004	-0.017	0.004	-0.024	0.005
Any child less than 5 years old present at t-1	-0.091	0.033	-0.128	0.043	-0.123	0.043	-0.096	0.041
Duration dependence								
ln(duration)	-0.229	0.042	-0.255	0.048	-0.251	0.048	0.141	0.064
ln(duration) squared	0.016	0.010	0.056	0.012	0.061	0.012	-0.026	0.013
Correlated random effects								
High school only	n.a.		n.a.		0.221	0.108	0.018	0.111
Completed vocational school	n.a.		n.a.		0.056	0.102	-0.044	0.105
Attended university	n.a.		n.a.		0.437	0.119	0.346	0.115
Completed work-related training	n.a.		n.a.		0.356	0.077	0.270	0.073
Duration of current part-time employment spell	n.a.		n.a.		0.011	0.006	0.004	0.005

Notes: *denotes significance at the five -per cent level. "Est." denotes parameter estimates, while "s.e." denotes estimated standard errors.

Based on estimation results from a multiple spell hazard model of welfare exits and re-entries. Model 1 assumes that welfare and non-welfare spells are uncorrelated. Model 2 allows unobservables in welfare and non-welfare spells to be correlated (dynamic self-selection). Model 3 extends Model 2 by allowing human capital variables to be correlated with unobservable effects. Model 4 extends Model 3 by allowing the eligibility and take-up decisions to be endogenous. All model specifications also include controls for age, gender, First Nations ancestry status, immigrant status, indicator for presence of children less than 5 years old, indicator for marital status, duration of current marriage, log(duration), and its square. In all specifications controlling for unobservable effects, M = 2. The log-likelihood values are -12,544 for Model 1, -12,514 for Model 2, -12,486 for Model 3, and finally, -13,750 for Model 4.

Table A.2: Estimates Associated With Personal Characteristics, Duration Dependence, and Correlated Random Effects on Hazard Rates From Non-welfare Using a Multiple Spell Framework

	Model 1		Model 2		Model 3		Model 4	
Variable	**Est.**	**s.e.**	**Est.**	**s.e.**	**Est.**	**s.e.**	**Est.**	**s.e.**
Personal characteristics								
Age at baseline	0.006	0.005	0.006	0.005	0.005	0.005	0.005	0.006
Female	-0.214	0.082	-0.188	0.084	-0.192	0.084	-0.198	0.108
First Nations ancestry	-0.187	0.137	-0.257	0.147	-0.277	0.151	-0.369	0.211
Immigrant	0.046	0.051	0.048	0.053	0.056	0.054	0.024	0.065
Married at t-1	-0.173	0.092	-0.174	0.094	-0.164	0.095	-0.269	0.118
Duration of current marriage at t-1	-0.003	0.004	-0.003	0.004	-0.004	0.004	0.001	0.005
Any child less than 5 years old present at t-1	0.071	0.052	0.107	0.054	0.095	0.054	-0.021	0.067

(continued)

Table A.2: Estimates Associated With Personal Characteristics, Duration Dependence, and Correlated Random Effects on Hazard Rates From Non-welfare Using a Multiple Spell Framework (Cont'd)

	Model 1		Model 2		Model 3		Model 4	
Variable	**Est.**	**s.e.**	**Est.**	**s.e.**	**Est.**	**s.e.**	**Est.**	**s.e.**
Duration dependence								
ln(duration)	-0.757	0.056	-0.762	0.058	-0.768	0.058	-0.826	0.068
ln(duration) squared	0.105	0.014	0.106	0.015	0.109	0.015	0.107	0.018
Correlated random effects								
High school only	n.a.		n.a.		-0.295	0.145	-0.420	0.167
Completed vocational school	n.a.		n.a.		0.053	0.147	0.010	0.164
Attended university	n.a.		n.a.		-0.012	0.130	0.082	0.151
Completed work-related training	n.a.		n.a.		0.059	0.101	-0.022	0.120
Duration of current full-time employment spell	n.a.		n.a.		0.003	0.003	-0.001	0.004
Duration of current part-time employment spell	n.a.		n.a.		-0.024	0.009	-0.025	0.010

Notes: *denotes significance at the five per cent level. "Est." denotes parameter estimates, while "s.e." denotes estimated standard errors.

Based on estimation results from a multiple spell hazard model of welfare exits and re-entries. Model 1 assumes that welfare and non-welfare spells are uncorrelated. Model 2 allows unobservables in welfare and non-welfare spells to be correlated (dynamic self-selection). Model 3 extends Model 2 by allowing human capital variables to be correlated with unobservable effects. Model 4 extends Model 3 by allowing the eligibility and take-up decisions to be endogenous. All model specifications also include controls for age, gender, First Nations ancestry status, immigrant status, indicator for presence of children less than 5 years old, indicator for marital status, duration of current marriage, log(duration), and its square. The associated estimates for these control variables are presented in Table A.1 in the Appendix. In all specifications controlling for unobservable effects, M = 2. The log-likelihood values are -12,544 for Model 1, -12,514 for Model 2, -12,486 for Model 3, and finally, -13,750 for Model 4.

Table A.3: Estimates for Eligibility and Take-Up Hazard Rates in the Multiple Spell Model

	Eligibility		Take-Up	
Variable	**Est.**	**s.e.**	**Est.**	**s.e.**
Human capital at baseline				
High school only	-0.134	0.123	0.265	0.105
Completed vocational school	0.119	0.138	0.356	0.128
Attended university	0.188	0.134	0.282	0.130
Work experience (years)	0.017	0.011	0.011	0.011
Personal characteristics				
Age at baseline	-0.007	0.009	-0.017	0.010
Female	-0.173	0.203	-0.202	0.183
First Nations ancestry	0.097	0.302	0.141	0.280
Immigrant	-0.194	0.124	-0.087	0.101
Duration dependence				
ln(duration)	-0.157	0.397	0.437	0.237
ln(duration) squared	1.594	0.142	-0.122	0.077

Note: Based on the Model 4 specification described in Table 12. "Est." denotes parameter estimates, while "s.e." denotes estimated standard errors.

Table A.4: Estimates Associated With Personal Characteristics, Duration Dependence, and Correlated Random Effects on Hazard Rates From Welfare Using a Competing Risks Framework

Exit to:	Marriage		Earnings Growth		Other	
Variable	**Est.**	**s.e.**	**Est.**	**s.e.**	**Est.**	**s.e.**
Personal characteristics						
Age at baseline	-0.013	0.009	-0.013	0.004	-0.012*	0.004
Female	-0.013	0.219	-0.162*	0.070	0.173	0.107
First Nations ancestry	0.373*	0.186	0.182	0.101	0.261*	0.113
Immigrant	-0.180	0.099	-0.191*	0.040	-0.089	0.048
Any child less than 5 years old present at t-1	0.041	0.091	-0.027	0.039	-0.073	0.049
Duration dependence						
ln(duration)	0.840*	0.139	1.100*	0.060	0.732*	0.064
ln(duration) squared	-0.135*	0.029	-0.186*	0.013	-0.108*	0.014
Correlated random effects						
High school only	0.209	0.231	0.300*	0.105	-0.094	0.129
Completed vocational school	0.044	0.227	-0.079	0.097	-0.139	0.112
Attended university	-0.004	0.338	0.332*	0.133	0.311*	0.154
Completed work-related training	-0.018	0.165	0.240*	0.068	0.080	0.087
Duration of current part-time employment spell	0.012	0.010	-0.013	0.006	0.021*	0.005

Notes: *denotes significance at the five per cent level. "Est." denotes parameter estimates, while "s.e." denotes estimated standard errors. Based on estimation results from a competing risks model that allows for (i) correlated risks, (ii) endogenous human capital variables, and (iii) endogenous eligibility decisions. Est. denotes parameter estimates while s.e. denotes estimated standard errors. The model also includes controls for age, gender, First Nations ancestry status, immigrant status, indicator for presence of children less than 5 years old, log(duration), and its square.

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