

SRDC Working Paper Series 04-02

**New Evidence from the Self-Sufficiency Project on the Potential of
Earnings Supplements to Increase Labour Force Attachment Among
Welfare Recipients**

The Self-Sufficiency Project

Kelly Foley

February 2004

This paper is part of the Social Research and Demonstration Corporation's program of analysis for the Self-Sufficiency Project (SSP) sponsored by the federal Department of Human Resources and Skills Development (HRSD). The Self-Sufficiency Project is sponsored by HRSD. This paper was produced for the Social Research and Demonstration Corporation (SRDC). The opinions expressed herein are the author's and do not necessarily reflect those of SRDC or HRSD.

The Social Research and Demonstration Corporation is a non-profit organization and registered charity with offices in Ottawa, Vancouver, and Sydney, Nova Scotia. SRDC was created specifically to develop, field test, and rigorously evaluate social programs. SRDC's two-part mission is to help policy-makers and practitioners identify social policies and programs that improve the well-being of all Canadians, with a special concern for the effects on the disadvantaged, and to raise the standards of evidence that are used in assessing social policies and programs. As an intermediary organization, SRDC attempts to bridge the worlds of academic researchers, government policy-makers, and on-the-ground program operators. Providing a vehicle for the development and management of complex demonstration projects, SRDC seeks to work in close partnership with provinces, the federal government, local programs, and private philanthropies.

Copyright © 2004 by the Social Research and Demonstration Corporation

Table of Contents

Tables and Figures	iv
Acknowledgements	v
Abstract	vii
Introduction	1
Existing Evidence on Work Incentives in Social Assistance Policies	3
An Economic Model of Job Search Within the Self-Sufficiency Project	5
The Self-Sufficiency Project	5
Job Search With the Supplement Offer	6
Empirical Strategy	9
Data and Results	14
Effects on Full-Time Employment	14
Effects on Job Loss or Job Leaving	16
Sensitivity of Results	19
Conclusion	23
Appendix A: Key Features of the SSP Earnings Supplement	25
Appendix B: Propensity Score Matching Technique	27
References	33

Tables and Figures

Table		Page
1	Characteristics of Program and Control Group Members in the Constrained and Unconstrained Groups	12
2	Effects on Job Loss and Job Leaving	17
3	Effects on Job Loss and Job Leaving (Using Recipient Propensity Score)	21
B.1	Results from Logit Estimation of Propensity to Be Unemployed for More Than 12 Months	29
B.2	Effect of the Constraint Within the Control Group	31
Figure		
1	Full-Time Employment Rates, by Months From Random Assignment	6
2	Full-Time Employment Rates, for the Constrained Group	15
3	Full-Time Employment Rates, for the Unconstrained Group	15

Acknowledgements

This paper has greatly benefited from advice and comments from Nicole Fortin. The author would also like to thank Reuben Ford, Doug Tattrie, Pierre-Yves Yanni, and two anonymous referees for helpful comments.

Abstract

This paper examines possible long-run effects of the Self-Sufficiency Project (SSP) on labour force attachment. SSP offered welfare recipients a generous but temporary earnings supplement, contingent on full-time work in the first year. Because this requirement limited the job search, participants might have made lower-quality job matches and then voluntarily left their jobs.

Estimates suggest the time limit decreased full-time work for those who had taken up full-time work primarily from voluntary job leaving. The time limit's effect was isolated by comparing program group members who were constrained and unconstrained by the time limit to their control group counterparts. Constrained participants were identified using propensity score matching.

Introduction

Recent reforms of social assistance systems, particularly in Canada, the United States, and the United Kingdom, have been preoccupied with welfare-to-work transitions. Generally, evaluations of recent reforms have reported gains in labour supply. In the United States Bloom and Michalopoulos (2001) synthesize the effects from 29 different experimental reform initiatives and report evidence that providing a financial work incentive can be an effective way to encourage labour force participation among welfare recipients. Blank, Card, and Robins (1999) review the impacts of both experimental and non-experimental programs. Their findings are consistent — financial incentives can increase work among welfare recipients. Similarly, Blundell and Brewer (2000) summarize evidence from three studies of reforms in the United Kingdom that had positive effects on labour supply. It is less clear, however, from existing research how well recent reforms have performed in terms of encouraging labour force attachment in the long run among former welfare recipients.

In Canada the Self-Sufficiency Project (SSP), a random assignment experiment, tested the effect of an earnings supplement on the labour supply of long-term welfare recipients. SSP was among the most generous reforms in North America and had the potential to double earned income from minimum wage work. At its peak, the program impact on full-time employment was 15 percentage points, representing a doubling of the proportion of welfare recipients working full time (Michalopoulos et al., 2002).

Over time, however, particularly after the temporary earnings supplement was no longer available, the impact declined to zero. That a very generous, and initially highly effective financial incentive has no long-run impacts raises some important questions about whether it is possible to improve labour force attachment with financial incentives alone.

An important feature of SSP was that supplement receipt was contingent on finding full-time work in the first year of the program. This feature effectively places a limit on the duration of participants' job search. An application of job-search models (Lippman & McCall, 1976; Mortensen, 1986) suggests that limiting job search could lead to lower-quality job matches. A low-quality job match could subsequently lead to a participant leaving the job voluntarily.

This paper attempts to isolate the effect of the time constraint on job search by estimating a difference-in-difference model. The program effect (the difference between control and program group members) among those who were constrained by the time limit was compared with the program effect among those who were not constrained. Propensity score matching was used to determine whether program group members were constrained by the 12-month time limit.

The estimates suggest that making supplement receipt contingent on full-time work within one year leads participants who do work to later cease working full time. Furthermore, most people who were no longer working full time left their jobs voluntarily. These findings support the conclusion that lower-quality job matches arose from the time constraint on the job search.

In the next section of the paper (“Existing Evidence on Work Incentives in Social Assistance Policies”), the existing evidence on financial incentives in the literature is reviewed. SSP is described and experimental results presented to illustrate the pattern of impacts on full-time employment over time. The third section (“An Economic Model of Job Search Within the Self-Sufficiency Project”) discusses ways in which SSP might affect job search. The empirical strategy, which compares constrained and unconstrained program group members with their counterparts in the control group, is laid out in the fourth section (“Empirical Strategy”). Results are presented in the fifth section (“Data and Results”), which includes estimates of the effects of the 12-month take-up constraint on full-time employment. Finally, the paper provides evidence of effects on job loss and leaving and examines the sensitivity of these results.

Existing Evidence on Work Incentives in Social Assistance Policies

The effects of welfare programs on labour supply are well documented in the literature. Moffit's (1992) seminal review of US research along with several Canadian studies (Allen, 1993; Charette & Meng, 1994; Dooley, 1999) provide strong evidence of the negative work incentives generated by transfer programs.

In response to the evidence of work disincentives, income security policy has evolved to encourage labour supply among recipients of government transfers. Saez (2000) demonstrates that optimal programs provide negative marginal tax rates when participants are at the extensive margin. In other words, income is transferred most efficiently to individuals supplying no labour when work is a prerequisite for the transfer.

In the United States one of the largest transfer programs, the Earned Income Tax Credit (EITC), follows this scheme by providing refundable tax credits to the working poor. A similar program called the Working Families' Tax Credit (WFTC) exists in the United Kingdom.

Empirical evidence suggests that supplementing work is effective in encouraging labour force participation among low-income individuals. Eissa and Leibmann (1996) estimate the effect of EITC on labour supply using a difference-in-difference approach. The labour supply of single mothers was compared with that of a control group of women without children before and after the implementation of a substantial increase in the EITC benefits in 1986. Eissa and Leibmann find that the higher benefits, obtained only when working, increased single mothers' labour supply by 2.8 percentage points.

Estimates of the effect of the WFTC on employment among lone parents are somewhat lower. Impacts of the UK tax credit range from 1.6 to 2.3 per cent (Blundell & Brewer, 2000). In Canada the experimental evaluation of SSP showed large impacts on labour supply. After 18 months SSP had more than doubled full-time employment among those offered an earnings supplement (Michalopoulos et al., 2000).

While earned income transfers appear to have positive effects on participation, effects on work effort (i.e. the number of hours worked) may be negative. Scholz (1996) estimates that recipients reduced their work effort by an average of 11 hours annually because of EITC.¹ Because transfers may reduce hours among those already working, the SSP earnings supplement was contingent on full-time work. Similarly, the WFTC required at least 16 hours of work and rewarded full-time work with a small bonus.

Little is known about the effects on labour force attachment of transfer programs. Labour force attachment in this context refers to maintaining full-time work consistently, in contrast to cycling between welfare and work. The capacity to improve labour force attachment is relevant in terms of reducing reliance on government transfers either as in-work or

¹Estimates of the effects of earned income credits on hours have not been consistent in the literature. See MaCurdy, Green, and Paarsch (1990) for a discussion of methodologies employed and the corresponding implied elasticities.

out-of-work benefits. Many transfer programs now either have time limits on receipt or phase out benefits as recipient income increases. To sustain the labour supply after transfers are no longer made implies the need for labour force attachment.

Combining transfers with employment services is one potential method to improve labour force attachment. A companion study to SSP, called “SSP Plus”, added employment services to the supplement and found that the services incrementally increased full-time employment beyond the life of the supplement (Michalopoulos et al., 2002).

In the absence of any supplements, services, or programs, earned income transfers could potentially improve long-run employment outcomes if participants experienced wage growth. Connolly and Gottschalk (2003) consider whether SSP may have affected participants’ likelihood of experiencing within-job wage growth relative to between-job wage growth. They found that participants are more likely to experience wage growth by switching jobs. Their results imply that the initial job match may not be as important as subsequent job switching.

Other research has shown that wage growth has been relatively negligible in SSP. Card and Michalopoulos (2001) estimate that those who worked because of SSP experienced wage growth of about 2.5 to 3 percentage points, a rate that is similar to other welfare leavers. This result is particularly important in the context of the very low wages typically earned by former recipients. Low wages, with little chance of progression, are not suggestive of long-term self-sufficiency.

An Economic Model of Job Search Within the Self-Sufficiency Project

THE SELF-SUFFICIENCY PROJECT

The Self-Sufficiency Project (SSP), a random assignment experiment implemented in British Columbia and New Brunswick, was designed to reduce poverty while simultaneously lessening dependence on government transfers. The central feature of SSP was a generous earnings supplement offered to lone parents who had received income assistance (IA) for at least one year. Participants who were randomly assigned to the program group were informed that if they found full-time employment within one year, their earnings would be supplemented for each of the following 36 months in which they worked full time. Details of SSP are described in Appendix A.

SSP was implemented as a random assignment experiment. A random sample of long-term welfare recipients were sent a letter asking them to volunteer to be part of the SSP study. Volunteers were then randomly assigned to either a control group or a program group. Members of the program group were offered the supplement while members of the control group were not.

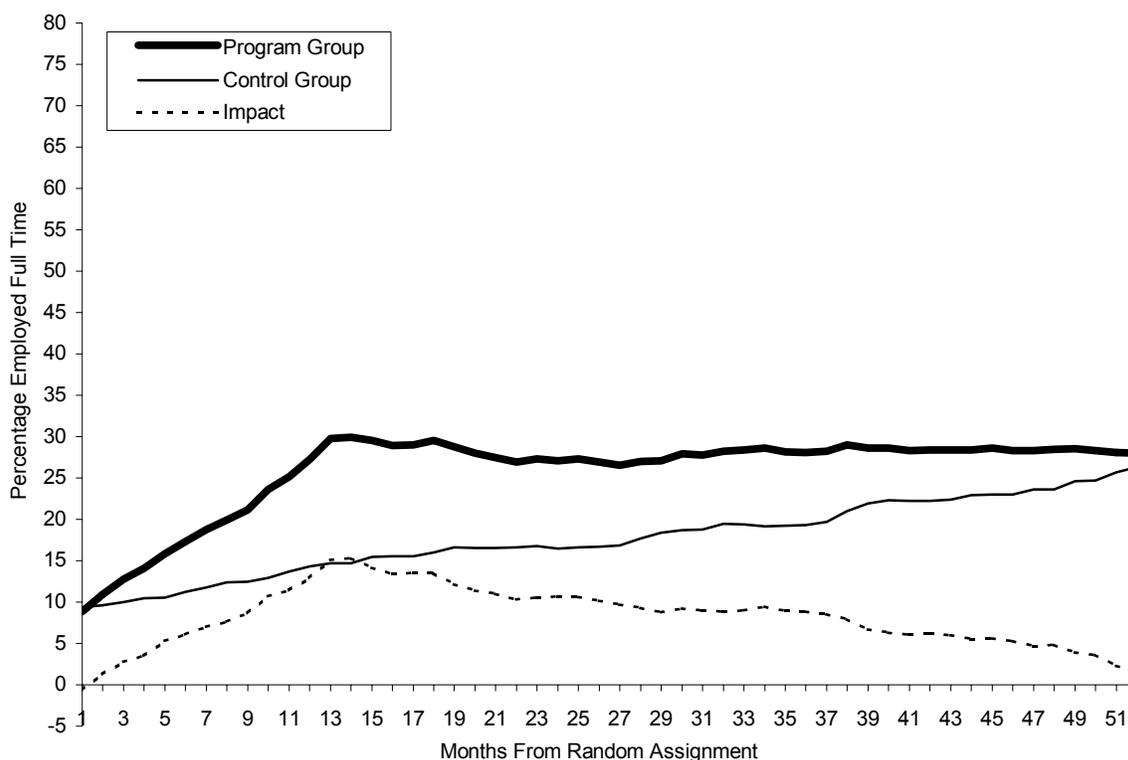
This paper focuses on a program feature, often referred to as the “supplement take-up window,” which made supplement receipt contingent on initiation of the supplement within the first year of the program. Initiation of the supplement meant leaving IA and taking up full-time work.

Participants were followed for 54 months. Over the course of the follow-up, the size of SSP’s impacts changed (Michalopoulos et al., 2002). Figure 1 shows the impacts on full-time employment for 52 months after random assignment. In the first year, when participants could initiate the supplement, the impact on full-time employment increased dramatically. In Month 14, SSP had increased full-time employment by over 15 percentage points. After the 12-month take-up window had closed, the impacts began to decline. After 52 months, program group members were only 1.6 percentage points more likely to be working full time — a difference that was not statistically significant.

Michalopoulos et al. (2002) note that the declining impacts may have resulted from two different effects. First, control group members eventually leave IA and “catch up” to the program group. Second, full-time work may have decreased among program group members who had been receiving the supplement, once the supplement was no longer available.

In Figure 1, full-time employment appears fairly stable for the program group in the later stages of the program. However, the program group is composed of two distinct subgroups: those who received the supplement (called “takers”) and those who did not (called “non-takers”). While full-time employment among non-takers increases over time, like the control group, these gains in full-time employment are offset by the program group takers. The combined effect of non-taker and taker behaviour is reflected in Figure 1.

Figure 1: Full-Time Employment Rates, by Months From Random Assignment



Sources: Calculations from baseline survey data and 18-month, 36-month, and 54-month follow-up survey data.

This paper moves beyond the experimental results discussed in Michalopoulos et al. (2002) by assessing the possibility that voluntary job leaving in the program group resulted from a lower-quality job match. Evidence of job leaving can only be suggestive of job quality; however, other measures of match quality are unfortunately unavailable.²

JOB SEARCH WITH THE SUPPLEMENT OFFER

SSP might affect the quality of job matches by affecting the duration of a participant's initial job search. In simple job search models, unemployed workers maximize, by choice of a reservation wage, their expected wealth with an optimal stopping strategy (Lippman & McCall, 1976). The optimal reservation wage will have the property that unemployed persons will stop searching when the best job offer in that period is at least as high as the reservation wage. As long as participants' preferences do not change (e.g. the birth of child) the reservation wage should remain constant. Offers arrive as a function of the quality and intensity of participants' job search as well as the participants' characteristics (e.g. their skills).

By offering an earnings supplement, SSP effectively lowers program group members' reservation wages. All else being equal, participation in the program should increase the

²Although the surveys did ask about reasons why someone lost or left a job, response rates were far too low. Moreover, most respondents to that question reported "other" as a reason.

probability that program group members work. The availability of the supplement may also increase the intensity of search since participants may believe they have a better chance of finding a job with wages that, combined with the supplement, will be higher than their reservation wage.

In order to receive the supplement, program group members needed to take up full-time work within one year of random assignment. Effectively, this meant there was a cost — loss of the supplement — associated with search durations exceeding 12 months. This constraint on search duration suggests that offers would be accepted with the supplement. In the absence of the supplement, it would have been optimal to wait for the next and possibly better offer.³ Constraining search duration in this way might lower the quality of the job matches made by program group members.

³The concept of constrained job search is analogous to job search with liquidity constraints in Mortensen (1986).

Empirical Strategy

Since the theory suggests there are two program effects, the critical task is to estimate separately the effect of receiving the supplement payments and the effect of the time constraint. First, a difference-in-difference method is used to isolate the effect of the time constraint.⁴ Second, to test whether the time constraint led to lower-quality job matches, the paper examines its impact on voluntary job leaving.

In the Self-Sufficiency Project (SSP), participants were randomly assigned to either the program or control group. This means that the difference $E(Y_i|X_i, P_i = 1) - E(Y_i|X_i, P_i = 0)$ provides a consistent estimate of the average treatment effect, which can be defined as θ . In this expression, P_i is an indicator variable taking on the value 1 if a participant was assigned to the program group, Y_i is an employment outcome, and X_i is a vector of characteristics.

For SSP, the treatment effect may result from either the supplement income or the 12-month take-up constraint. The parameter θ can therefore be decomposed into ϕ , the effect of participation, and δ the constraint effect. Previously, these two effects have not been estimated separately. Decomposing the impact is important, because the estimate of θ represents an *average* effect. Examining average effects might mask offsetting effects.

Offsetting effects among program group members might have resulted, because members differed in their pre-program labour force attachment. This might have critically affected the extent to which participants were constrained by the time limit on supplement initiation. Some program group members would have quickly left welfare and found full-time work in the absence of the program. Because these individuals were unconstrained by it, the time limit should have had no effect on their behaviour.

Other members of the program group had far lower levels of labour force attachment prior to random assignment. These members might have searched far longer for a job had they not been offered the supplement only if they found work within one year. For this group, a behavioural impact, specifically a lower-quality job match, is expected.

Using the assumption that unconstrained program group members were unaffected by the time limit, an estimate of δ can be obtained using the following double difference:

$$E(Y_i|X_i, P_i = 1, Con_i = 1) - E(Y_i|X_i, P_i = 0, Con_i = 1) - \\ E(Y_i|X_i, P_i = 1, Con_i = 0) - E(Y_i|X_i, P_i = 0, Con_i = 0)$$

where Con_i equals one if a participant was constrained by the 12-month take-up window.

The difference-in-difference estimate of the effect of the 12-month take-up constraint will be consistent only if certain conditions are met. First, the untreated search should be unaffected. This means that members of the control group should be unaffected by the program, and those who are defined as unconstrained should be unaffected by the time limit.

⁴Card and Sullivan (1988) provide a description of difference-in-difference estimates of training impacts.

Because control group members received no services or supplements associated with SSP, it is unproblematic to assume that they are not affected by the program. However, since unconstrained group members are assigned, based on their observable behaviour, the assignment is not perfect. There is a chance that unconstrained program group members were still affected by constraint.

The second condition requires that the treatment must be independent of participants' pre-treatment employment status. This is guaranteed by random assignment. The final condition is an extension of the second and requires that the treatment be independent of participants' pre-random assignment characteristics within the group of participants who were constrained and the group who were unconstrained.

Strong assumptions are required to guarantee that the above conditions are met. Once the analysis moves beyond the comparison of randomly assigned groups, it is unlikely that an unbiased estimate can be found. Non-experimental methodologies, such as the matching technique used in this paper, can be employed to mitigate potential biases. However, the possibility of such biases means that results cannot be interpreted as causal effect but merely suggests what might be occurring in reality.

Because they were not offered the supplement and were therefore unaffected by the time constraint, the search behaviour of control group members is optimal in the absence of the treatment. The availability of a randomly assigned control group means that it is possible to observe directly, within that group, those who would have been constrained by the time limit had they been offered the supplement.⁵ For the analysis in this paper, control group members were assigned to an unconstrained group, C^{un} , if they found full-time work within one year of random assignment and a constrained group, C^{con} , if they did not.

Untreated search behaviour is not directly observed in the program group because members' behaviour has been affected by the supplement offer. This paper exploits the availability of the control group, which can act as a proxy for the state where program group members did not receive the supplement offer, by constructing similar groups, P^{un} and P^{con} , for program group members. Following Dehejia and Wahba (1998), a propensity score-matching technique is used to construct the constrained and unconstrained subgroups for the program group. The details of the technique are outlined in Appendix B.

The propensity score technique estimates the probability or propensity to have been constrained by the time limit. Assignment to the constrained or unconstrained group was accomplished by matching program group members to control group members based on their propensity scores.

In a first-best world, the effect of the time constraint would be estimated when the treatment was independent of the participants' pre-random assignment characteristics within the group of participants who were constrained and the group who were unconstrained. While matching is unlikely to result in perfect independence, which is an untestable hypothesis, it is possible to assess the quality of that matching.

⁵Assigning control group members to constrained and unconstrained groups in this way implicitly assumes that the program participation effect (separate from the time constraint effect) affects only *whether* welfare recipients work full time, and not *when* they first work full time. The results from relaxing this assumption are explored in a later section.

One way to gauge the quality of the propensity score estimate is to examine the extent to which participants' actual search behaviour can be predicted. Within the control group, 1,863 or 78 per cent of members remained unemployed for more than one year after random assignment. If control group members were ranked by their propensity scores, about 84 per cent of the participants with the top 1,863 scores remained unemployed for more than one year. This suggests that the propensity score was reasonably successful at predicting untreated search behaviour.

There were 506 control group members (for whom the propensity score was not missing) who found a full-time job within the first year of the program. This 22 per cent represents the expected proportion of a randomly drawn sample who would work full time within one year. In contrast, within the control group, of the 506 members with the greatest propensity to work in the first year, according to their predicted score, 48 per cent actually took up full-time employment in the first year.

Another way to assess the propensity score match is to examine the extent to which program and control group members, within the constrained and unconstrained groups, are similar in their observable characteristics. Table 1 presents average baseline characteristics for all four groups. In Column 3, the difference between the characteristics of P^{con} and C^{con} are reported. For the most part, the constrained program group members are similar to their control group counterparts. There is a statistically significant difference in only two characteristics. Members of the P^{con} were 3.5 percentage points more likely to have a high school education and 3.1 percentage points less likely to have reported that they could not work because of a lack of child care.

Balancing members' characteristics within the unconstrained group was not achieved as successfully, as illustrated in Column 6 of Table 1. Members of the P^{con} group were more likely to face barriers with employment. They were also more likely to be male, to be married, and to have children 12 years of age and older. None of these factors was, however, a significant predictor of the probability of finding full-time work within the first year (from Appendix Table B.1).

Members of the P^{un} group also had, on average, one additional year of prior work experience when compared to members of the C^{un} group. While previous employment history significantly predicted whether participants worked within the first year of the program, the magnitude of the imbalance, in this case, is quite small.

This test of the similarity of characteristics suggests that the matching procedure was reasonably successful in identifying appropriate members of the P^{un} and P^{con} groups. However, because the characteristics are not perfectly balanced, the effects of the time limit are estimated with two specifications, one that adjusts for baseline characteristics and one that does not. If it were possible to ensure that $P_i * Con_i$ had no effect on Y_i prior to random assignment, the following effect would be estimated in the unadjusted specification:

$$Y_i = \alpha + \gamma Con_i + \phi P_i + \delta P_i * Con_i.$$

While this specification makes use of random assignment in assuming that the effect of P_i on Y_i prior to random assignment is zero, it does not control for possible selection biases associated with assignment into either the constrained or unconstrained groups. To mitigate the possibility that $P_i * Con_i$ has an effect on Y_i prior to random assignment, an adjusted model

is also estimated. If it were possible to control for observable and unobservable characteristics the specification would be:

$$Y_i = \alpha + x_i\beta + \gamma Con_i + \phi P_i + \delta P_i * Con_i,$$

where x_i is a vector of baseline characteristics including age, gender, speaks English, less than high school education, First Nations ancestry, physical or emotional problem reported, age of youngest child, couldn't work at baseline because of a lack of adequate child care, couldn't work at baseline because of family responsibility, full-time work greatest need at baseline, and years of prior employment.

Table 1: Characteristics of Program and Control Group Members in the Constrained and Unconstrained Groups

Characteristic	Constrained Group (Untreated Search > 12 months)			Unconstrained Group (Untreated Search <= 12 months)		
	Program Group (1)	Control Group (2)	Difference (3)	Program Group (4)	Control Group (5)	Difference (6)
Propensity score	0.95	0.95	0.00	0.42	0.42	0.00
Personal characteristics						
Female	95.7	95.8	-0.2	94.9	97.4	-2.5*
Age of sample member at random assignment						
25–29	21.0	20.9	0.1	21.9	22.3	-0.4
30–39	38.4	38.8	-0.5	37.6	38.1	-0.5
40–49	15.5	16.0	-0.5	17.0	13.4	3.6
50 and older	2.1	2.5	-0.4	1.7	1.9	-0.2
Less than high school education	54.8	58.2	-3.5*	43.9	39.1	4.8
Reported First Nations ancestry	10.0	9.8	0.2	8.7	7.8	1.0
Immigrated less than 5 years ago	2.6	2.9	-0.3	2.5	1.7	0.8
Immigrated 5 or more years ago	9.4	10.5	-1.1	7.9	6.0	1.9
Lives in urban region	85.1	83.6	1.5	79.4	80.3	-0.9
Speaks English	97.3	96.8	0.4	99.4	98.7	0.8
Barriers to work						
Reported physical condition that limited activity ^a	26.9	28.0	-1.0	21.0	17.6	3.4
Reported emotional condition that limited activity ^b	7.8	8.4	-0.6	6.6	3.2	3.4**
At risk of depression ^c	47.7	48.8	-1.1	41.2	38.9	2.3
Could not work because of lack of child care	14.8	17.9	-3.1**	6.4	3.4	3.0**
Could not work because of illness	17.7	16.4	1.3	8.0	3.2	4.7***
Could not work because of family responsibilities	23.9	25.0	-1.1	9.1	6.2	2.9
Employment history						
Ever worked for pay	94.0	93.0	1.1	98.9	99.1	-0.2
Years of prior employment	6.8	7.0	-0.2	9.5	8.7	0.9*
Not employed, looking for work	23.1	23.2	-0.1	24.5	23.9	0.6
Neither employed nor looking for work	65.7	65.9	-0.2	25.4	24.8	0.6
Full-time employment greatest need at random assignment ^d	26.2	25.3	0.9	38.0	43.0	-5.0

(continued)

Table 1: Characteristics of Program and Control Group Members in the Constrained and Unconstrained Groups (Cont'd)

Characteristic	Constrained Group (Untreated Search > 12 months)			Unconstrained Group (Untreated Search <= 12 months)		
	Program Group (1)	Control Group (2)	Difference (3)	Program Group (4)	Control Group (5)	Difference (6)
Family structure						
Family received welfare while growing up	25.1	25.2	0.0	19.6	24.1	-4.5
Never married	48.2	48.4	-0.2	45.6	51.2	-5.7*
Number of children at random assignment	1.7	1.7	0.0	1.6	1.6	0.0
Age of youngest child at random assignment						
6–11	25.7	24.6	1.0	28.1	28.4	-0.3
12 and older	19.8	18.3	1.6	24.6	19.6	5.0*
Sample size	1,860	1,860		528	528	

Sources: Calculations based on baseline survey data and IA administrative records.

Notes: Two-tailed t-tests were applied to differences between the outcomes for the program and control groups.

Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

Rounding may cause slight discrepancies in sums and differences.

^aThis group includes sample members who indicated having a long-term physical condition or health problem that limited the kind or amount of activity they could do at any of the following: at home, at school, at work, or in other activities such as travel, sports, or leisure.

^bThis group includes sample members who indicated having a long-term emotional condition or health problem that limited the kind or amount of activity they could do at any of the following: at home, at school, at work, or in other activities such as travel, sports, or leisure.

^cSample members were considered to be at risk of depression if they scored 3 or more (out of a possible total score of 12) on an abridged version of the CES-D (Center for Epidemiology Scale-Depression).

^dThe precise question on the baseline survey was: “At present, which of these best describes your greatest need?” Sample members were allowed to choose among immediate full-time employment, immediate part-time employment, education or training, or something else.

Data and Results

The Self-Sufficiency Project (SSP) data are derived from three sources: survey data, income assistance (IA) administrative records, and the SSP Program Management Information System (PMIS). Participants completed surveys conducted by Statistics Canada at baseline, 18, 36, and 54 months after random assignment. The surveys collected information about participants' family composition, demographic characteristics, employment history, job search, education, income and expenditures, child-care use and personal attitudes. Provincial administrative records provided information about participants' monthly IA use and IA payments. Data on supplement take-up and monthly supplement payments were obtained from the SSP PMIS.

The sample used for the analysis of the 12-month take-up window included the 2,460 program group members and 2,392 control group members who responded to the 54-month recipient follow-up survey.⁶

EFFECTS ON FULL-TIME EMPLOYMENT

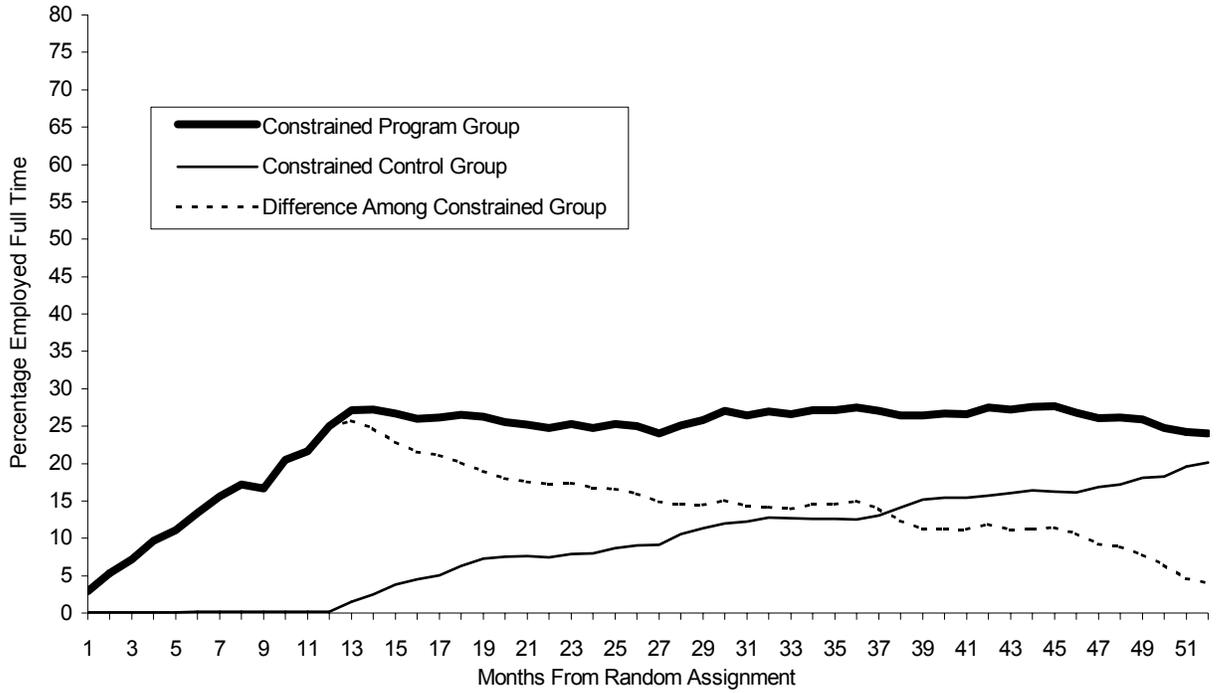
Experimental results presented in Figure 1 show that impacts on full-time employment declined over the course of the follow-up. In this section, results are presented that decompose the impacts for those who were constrained and unconstrained by the supplement take-up window. Figure 2 shows the effect on full-time employment rates for those participants who were expected to have untreated job search durations greater than 12 months.

None of the members of the C^{con} group, by definition, worked full time during the first 12 months of the program. This is why the thin solid line in Figure 2, which shows full-time employment for this group, remains at zero until Month 12. After Month 12 these control group members gradually began to take up full-time work. In contrast, full-time employment increased rapidly in the first year for constrained program group members. After the first year, full-time employment remained fairly constant for the duration of the follow-up. The combination of these two trends meant that the effect of the supplement offer, which was as large as 28 percentage points in Month 12, fell to less than 5 percentage points by Month 52.

The employment patterns for program and control group members for whom the 12-month supplement take-up window was not binding were substantially different than those observed in Figure 2. Figure 3 shows full-time employment in months 1 to 52 for groups P^{un} and C^{un} . Even at random assignment, Month 1, it is clear that the members of the P^{un} and C^{un} groups have very different employment outcomes than members of the constrained groups. In Month 1 full-time employment among participants who were expected to find full-time employment within the first year was around 30 to 40 per cent. In contrast, full-time employment for the constrained participants was below 5 per cent when the program began. These different employment outcomes no doubt reflect the lower levels of education and labour force experience among constrained group members.

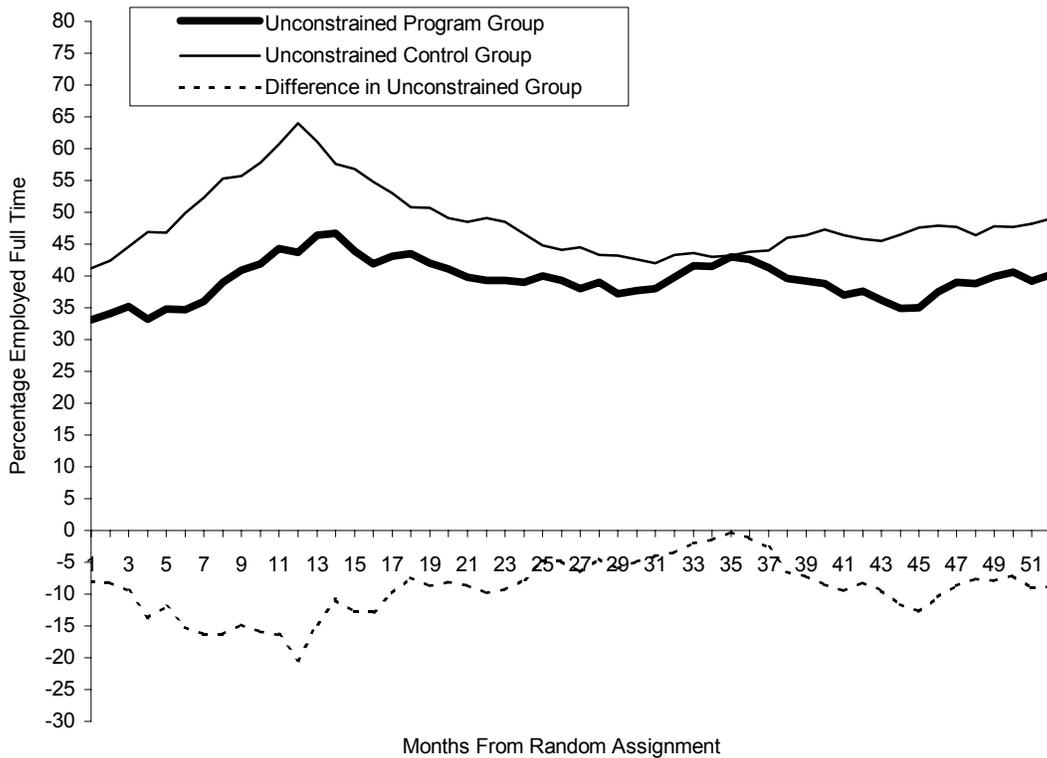
⁶See Michalopoulos et al. (2002: Appendix A) for a discussion of any bias introduced by differential sample attrition.

Figure 2: Full-Time Employment Rates, for the Constrained Group (Untreated Job Search of More Than 12 Months)



Sources: Calculations from baseline survey data and 18-month, 36-month, and 54-month follow-up survey data.

Figure 3: Full-Time Employment Rates, for the Unconstrained Group (Untreated Job Search of Less Than 12 Months)



Sources: Calculations from baseline survey data and 18-month, 36-month, and 54-month follow-up survey data.

As Figure 3 shows, employment among members of the P^{un} group was lower than that among their counterparts in the control group. This finding could result from a strong income effect. A fairly large proportion of members of the P^{un} would have become eligible for the supplement in the first month of the program without any adjustment in their behaviour. Once initiated, the supplement could be collected in any of the 36 subsequent months in which the program group member worked full time. While this is consistent with the evidence that income transfers can reduce work effort (Saez, 2000), the magnitude of the effect seems implausibly large.

The difference observed in Figure 3 more likely results from the selection procedure used to define the unconstrained control group. By definition of the unconstrained group, *all* control group members worked full time in at least one of the first 12 months after random assignment. In contrast, not all program group members worked full time, because they were matched to control group members based on their observable characteristics. This would imply that there are unobservable characteristics in the unconstrained program group that make them less likely to work full time than their counterparts in the control group.

EFFECTS ON JOB LOSS OR JOB LEAVING

While there is substantial evidence that the take-up window had the desired employment effects during the first year, this paper seeks to understand whether those employment gains were made at a cost in terms of subsequent job leaving or loss. To explore that question, this section considers only people who worked after random assignment. In the previous section, results on *employment* outcomes were reported. Employment refers to an average across all the jobs held by participants. This means a participant working for two employers, each for 15 hours a week, would be considered to have full-time employment.⁷ The ensuing analysis presents outcomes for a single job. Examining a job outcome, rather than an employment outcome permits analysis of job leaving and job loss. Job loss is assumed to be involuntary and includes permanent or temporary layoffs, and dismissal. In contrast, leaving a job or quitting a job is voluntary.

In Table 2, the estimated effects of the 12-month take-up window on job loss and job leaving are reported. Panel A reports estimates of the proportion of participants who left or lost their final full-time job at any time during the follow-up. This panel includes all those participants who held at least one full-time job at any point during the follow-up. The last job held is examined, as opposed to the first job or any interim jobs, to distinguish between voluntary job leaving and job switching.

Job leaving and job loss in the years when the supplement became unavailable are reported in Panel B. Participants who held a full-time job in any of months 36 to 52 are included. The first two panels in Table 2 differ in that Panel A provides a general sense of the total decrease in full-time work resulting from the program, while Panel B provides some indication of the decline in work stemming from supplement loss.

⁷It would appear that holding more than one job at the same time was not common. Foley and Schwartz (2003) estimate that about 14 per cent of the program group members and 10 per cent of the control group members held more than one job when they first left IA.

Table 2: Effects on Job Loss and Job Leaving

Row		Constrained Group (Untreated Search > 12 Months)			Unconstrained Group (Untreated Search <= 12 Months)			Difference in Difference (3) - (6)
		Program Group (1)	Control Group (2)	Difference (3)	Program Group (4)	Control Group (5)	Difference (6)	(7)
Unadjusted Estimates (%)								
Panel A: Last full-time job at any time during the follow-up								
1	Lost job	24.5	22.4	2.1	26.5	26.3	0.2	1.9
2	Left job	29.3	20.9	8.4***	23.2	26.7	-3.5	11.9***
3	Either lost or left job	53.8	43.3	10.5***	49.7	53.0	-3.3	13.8***
	Sample size^a	557	557		476	476		
Panel B: Last full-time job when the supplement was no longer available								
4	Lost job	15.5	18.0	-2.5	20.2	16.1	4.1	-6.5***
5	Left job	23.3	17.7	5.6**	14.7	16.1	-1.5	7.1***
6	Either lost or left job	38.8	35.7	3.2	34.9	32.3	2.6	0.6
	Sample size^b	421	421		328	328		
Adjusted Estimates (%)^c								
Panel C: Last full-time job at any time during the follow-up								
7	Lost job	24.5	22.1	2.4	25.1	26.7	-1.5	3.9
8	Left job	29.1	21.0	8.1***	23.8	26.3	-2.5	10.6***
9	Either lost or left job	53.6	43.1	10.5***	48.9	52.9	-4.0	14.5***
	Sample size^a	557	557		476	476		
Panel D: Last full-time job when the supplement was no longer available								
10	Lost job	15.4	17.9	-2.5	19.4	16.2	3.3	-5.8***
11	Left job	23.4	17.5	5.9**	15.2	15.5	-0.4	6.2**
12	Either lost or left job	38.8	35.4	3.3	34.6	31.7	2.9	0.4
	Sample size^b	421	421		328	328		

Sources: Calculations based on baseline, 18-month, 36-month, and 54-month surveys.

Notes: Two-tailed t-tests were applied to differences between the outcomes for the program and control groups.

Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

Rounding may cause slight discrepancies in sums and differences.

F-tests were applied to difference-in-difference estimates.

^aSample includes those who worked in at least one full-time job during the follow-up.

^bSample includes those who worked in at least one full- or part-time job during months 36 to 52.

^cEstimates are adjusted for the following baseline characteristics: age, gender, speaks English, less than high school education, First Nations ancestry, physical or emotional problem reported, age of youngest child, couldn't work at baseline because of a lack of adequate child care, couldn't work at baseline because of family responsibility, full-time work greatest need at baseline, and years of prior employment.

In Table 2, Column 3 shows the effect of the program among constrained participants, and Column 6 shows the effect among unconstrained participants. The difference of these two effects, reported in Column 7, is the estimate of the effect of the time limit. The estimates in Table 2 show that over the course of the follow-up SSP increased job loss or job leaving for constrained group members. Row 3 of Panel A shows that for program group members who were constrained by the time limit, the supplement offer increased their chances of having lost or left their last full-time job by nearly 11 percentage points. Among unconstrained program group members, the supplement offer had a negative but not significant effect of 3.3 percentage points.

This is a difference of nearly 14 percentage points, which suggests that the time constraint increased the chances that program group members, who took up full-time work, were not working full time by the end of the follow-up. When the program began, constrained group members had lower levels of labour force attachment relative to participants who were unconstrained by the time limit. Because of this difference, one interpretation of the finding suggests that imposing a time limit, which accelerated the transition from welfare to work, could lead to negative long-run employment outcomes for the group of relatively disadvantaged persons who work.

Rows 1 and 2 in Panel A indicate that the decline in full-time work arising from the time limit was virtually all voluntary. In Row 1, the estimates show that SSP did not have a significant effect on job loss among either unconstrained or constrained participants. Row 2 shows that the effect of the supplement offer on job leaving for those unconstrained by the time limit was not statistically significant. In contrast, constrained program group members were 8.4 percentage points more likely than their counterparts in the control group to have left their last full-time job.

The results in Panel A support the theory that imposing a time limit on supplement take-up reduces the quality of job matches made by participants who left welfare for work. The supportive evidence comes from the finding that the termination of work was voluntary. Voluntarily leaving a job implies that the utility derived from the job was lower than the participant's reservation utility. The job search process should reduce the incidence of voluntary job leaving by matching employers and employees so utility in the job is always higher than the reservation utility.

One important way that in-work utility might fall below reservation utility is when supplemented work became ineligible for SSP supplement (i.e. when the 36-month period of supplement eligibility elapsed). To consider this possible effect, Panel B examines only those participants who worked full time in months 36 to 52.

Panel C shows that SSP did not increase the chances that those who worked full time in months 36 to 52 subsequently left full-time work. As reported in Row 6, the effect of the supplement offer on the proportion who either left or lost their job was insignificant for both the constrained and unconstrained groups. This zero net effect is a result, however, of offsetting effects and illustrates the value of decomposing the average effects.

Within the constrained group, the effect of the supplement offer on job loss was negative and insignificant, as Row 4 shows. For unconstrained group members, SSP increased, by an insignificant amount, the chances of having lost their last job in months 36 through 52. Differencing these two results produces a significant effect of the time limit. The last column of Row 4 shows that the 12-month supplement take-up window decreased the chances of involuntary job loss by 6.5 percentage points.

The incidence of voluntary job leaving grew, however, because of the time constraint. The constrained program group members were nearly six percentage points more likely to have left their final job in months 36 through 52. The effect among unconstrained group members was small and negative. This implies that during the time when the supplement was becoming unavailable, those who reduced the duration of their job search were more likely to leave their jobs voluntarily.

Again, evidence of voluntary job leaving suggests a lower-quality job match. In the case of Panel B, the time limit increased job leaving in the time when the supplement was becoming unavailable to those who had initiated it. Because the samples include participants who did not take up the supplement, Panel B does not provide direct evidence that participants left their jobs because the supplement was no longer available. However, the results do support the idea that the time limits led to lower-quality job matches.

The estimates in panels A and B are repeated in panels C and D with adjustment for covariates. Adjusted estimates are presented, because within the constrained and unconstrained groups, some significant differences were found in the baseline characteristics of program and control group members. These differences may lead to inconsistent estimates. To mitigate the possible confounding effects of the baseline differences, the estimates in panels C and D were adjusted for participants' characteristics at baseline. The covariates employed included baseline age, gender, speaks English, high school education attainment, First Nations ancestry, physical or emotional problem reported, age of youngest child, couldn't work at baseline because of a lack of adequate child care, couldn't work at baseline because of family responsibility, full-time work greatest need at baseline, and years of prior employment.

The similarity of the results in panels C and D to those presented in panels A and B implies the results are reasonably robust. In Panel C, Row 12 shows that the time limit increased the chance that participants either lost or left their job by 14.5 percentage points. This result is very close to the unadjusted estimate of 13.8 percentage points in Panel A. In the adjusted estimates, the decline in full-time work is again concentrated in voluntary job leaving. In Panel C, the time limit is estimated to have increased the probability of voluntary job leaving by 10.6 percentage points. This estimate is slightly smaller than the unadjusted estimate but still very similar in size.

Panel D presents adjusted estimates of the effects of the time limit in the years when the supplement eligibility period was expiring. As was similarly observed in Panel C, adjusting for baseline characteristics reduces the size of the estimated effect. The difference between the adjusted and unadjusted effects is, however, quite small. The direction of the effects is the same: the time limit reduced the chances of losing a job and increased the chances of leaving a job.

SENSITIVITY OF RESULTS

In order to estimate the results in Table 2, strong assumptions were required. The findings also reflect three kinds of selection effects: selection into full-time work, selection into the constrained group, and selection into the unconstrained group. Examining how sensitive these results are to the methodology used to estimate them is a valuable exercise.

To begin, it is possible that the results occur simply by chance or by definition of the constrained and unconstrained groups. To investigate this possibility, the methodology was applied using only the control group. The control group was randomly divided into two equally sized groups. Each group was divided into constrained and unconstrained members using the same methodology as above. Because the sample size was fairly small, this procedure was repeated 30 times and the averages were taken.

The control group received no treatment, so evidence of any difference between the two randomly chosen control groups would suggest that the results found above were spurious or the result of the selection into constrained and unconstrained groups. The differences between the two control groups were small and statistically insignificant within both the constrained and unconstrained groups, as was the difference-in-difference estimate. These results are shown in Appendix Table B.2. The fact that there are no effects when the comparisons are made among only control group members suggests that findings in Table 2 do not result purely from chance or from the selection procedure alone.

The selection of program group members into constrained and unconstrained group members was undertaken using a propensity score estimated in an outside sample from the SSP applicant study. Although steps were taken to ensure that the applicants chosen for the sample were similar to the recipient sample members, it is possible that the use of the applicant sample affected the results in Table 2.

A second propensity score was estimated using a randomly chosen 10 per cent sample of recipient participants.⁸ The results from using that propensity score to assign program group members into the constrained and unconstrained groups are presented in Table 3. The large increases in the chances of job leaving remain statistically significant when the recipient propensity score is employed. This is true for any full-time jobs as well as full-time jobs held at the end of the follow-up period.

The magnitude of the results on job loss does differ: the recipient propensity scores produce more positive estimates. In Table 2, the positive estimated time constraint effect on any job loss is substantially smaller than in Table 3. In the unadjusted results, using the applicant propensity score, the estimate is 1.9 percentage points, whereas with the recipient score it is 13.4 percentage points. For jobs at the end of the follow-up, the effects are less negative when using the recipient propensity score. The estimated effect of the time constraint is -1.8 percentage points with the recipient propensity score and -6.5 with the applicant propensity score.

Although use of the applicant sample to estimate the propensity score appears to have resulted in lower estimates of the effect of the time constraint on job loss, the sign of the effects remains the same. Moreover, job leaving also appears to be importantly influenced by the constraint.

⁸That 10 per cent of the sample was then excluded from the main analysis.

Table 3: Effects on Job Loss and Job Leaving (Using Recipient Propensity Score)

Row	Constrained Group (Untreated Search > 12 Months)			Unconstrained Group (Untreated Search <= 12 Months)			Difference in Difference (3) - (6) (7)	
	Program Group (1)	Control Group (2)	Difference (3)	Program Group (4)	Control Group (5)	Difference (6)		
Unadjusted Estimates (%)								
Panel A: Last full-time job at any time during the follow-up								
1	Lost job	22.7	12.7	10.1	20.8	24.1	-3.3	13.4***
2	Left job	32.0	22.2	9.8	22.7	23.3	-0.6	10.4***
3	Either lost or left job	54.7	34.8	19.9**	43.5	47.4	-3.9	23.8***
	Sample size^a	93	93		109	109		
Panel B: Last full-time job when the supplement was no longer available								
4	Lost job	7.3	11.1	-3.8	13.4	15.4	-2.0	-1.8
5	Left job	33.3	18.1	15.2*	16.0	12.8	3.2	12.0***
6	Either lost or left job	40.6	29.2	11.4	29.4	28.2	1.2	10.2**
	Sample size^b	74	74		83	83		
Adjusted Estimates (%)^c								
Panel C: Last full-time job at any time during the follow-up								
7	Lost job	21.1	13.8	7.2	21.0	23.4	-2.4	9.7 **
8	Left job	34.4	21.5	12.8*	22.1	23.4	-1.3	14.2 ***
9	Either lost or left job	55.5	35.4	20.1**	43.1	46.9	-3.8	23.9***
	Sample size^a	93	93		109	109		
Panel D: Last full-time job when the supplement was no longer available								
10	Lost job	8.1	10.2	-2.1	13.8	14.9	-1.1	-1.0
11	Left job	34.3	18.6	15.7*	15.2	12.8	2.4	13.3***
12	Either lost or left job	42.4	28.8	13.6	29.0	27.7	1.3	12.3***
	Sample size^b	74	74		83	83		

Sources: Calculations based on baseline, 18-month, 36-month, and 54-month surveys.

Notes: Two-tailed t-tests were applied to differences between the outcomes for the program and control groups.

Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

Rounding may cause slight discrepancies in sums and differences.

F-tests were applied to difference-in-difference estimates.

^aSample includes those who worked in at least one full-time job during the follow-up.

^bSample includes those who worked in at least one full- or part-time job during months 36 to 52.

^cEstimates are adjusted for the following baseline characteristics: age, gender, speaks English, less than high school education, First Nations ancestry, physical or emotional problem reported, age of youngest child, couldn't work at baseline because of a lack of adequate child care, couldn't work at baseline because of family responsibility, full-time work greatest need at baseline, and years of prior employment.

Conclusion

Although the offer of a generous but temporary earnings supplement more than doubled full-time work among long-term welfare recipients, this effect did not persist beyond the period in which participants could receive the supplement. Understanding why the program impacts attenuated could lead to policy innovations that improve the program's effectiveness. This paper focuses on the role of one program feature — the 12-month supplement take-up window — and how that feature might have contributed to the decline in impacts.

Job search models suggest that because the Self-Sufficiency Project (SSP) required participants to initiate the supplement by taking up full-time work within one year, they might make a lower-quality job match. The loss of quality would result because the take-up constraint reduced reservation wages by more than the supplement payment alone. Without any countervailing effect that might improve the quality of the job search, a lower reservation wage could lead to a lower quality match.

Even if the 12-month take-up window had reduced the quality of job matches, this loss would need to be weighed against the benefits of the constraint. Requiring participants to initiate the supplement by working full time within one year of random assignment served two purposes. First, the constraint made it more difficult for participants to experience a windfall. This means that more participants had to change their planned behaviour to receive the supplement and should have reduced costs relative to benefits. Second, the constraint accelerated the transition from welfare to full-time employment. In the first year of the program, full-time employment rates were substantially higher as a result of the 12-month supplement take-up window.

Evidence was found that these employment gains were accompanied by less full-time work later in the follow-up. Because of the constraint, program group members who worked full time during the follow-up were more likely than their control group counterparts to have stopped working at their last full-time job. Similar evidence of declines in the proportion working among those participants who were working full time in the last years of the program was not found. However, those who did stop working full time were more likely to have left their jobs rather than have lost their job as a result of the 12-month take-up window.

This paper has demonstrated that the take-up window is a critical design feature with particular effects on program outcomes. This suggests that there is scope for improving the design by selecting a different constraint. A possible design improvement could include a more restrictive constraint for participants with greater labour force attachment, in particular those currently working full time. This might reduce the level of “windfall” and make the program more cost effective. This evidence also implies that further program enhancements should focus on supplemental services that improve job search to counteract the effect of reducing reservation wages. Increasing human capital among welfare recipients would seem to be an important conduit to increase the number and quality of job matches. Services that provide job search information could also improve match quality. Findings from SSP Plus, which did provide such job search assistance, further suggest that this is a promising avenue for future research.

Appendix A: Key Features of the SSP Earnings Supplement

Full-time work requirement. Supplement payments were made only to eligible single parents who worked at least 30 hours per week and who left income assistance.

Substantial financial incentive. The supplement equalled half the difference between a participant's earnings and an "earnings benchmark." During the first year of operations, the benchmark was \$30,000 in New Brunswick and \$37,000 in British Columbia. The benchmark was adjusted over time to reflect changes in the cost of living and the generosity of income assistance. The supplement was reduced by 50 cents for every dollar of increased earnings. Unearned income (such as child support), earnings of other family members, and number of children did not affect the amount of the supplement. The supplement roughly doubled the earnings of many low-wage workers (before taxes and work-related expenses).

One year to take advantage of the offer. Individuals could sign up for the supplement if they found full-time work within the year after random assignment. If they did not sign up during that year, they could never receive the supplement.

Three-year time limit on supplement receipt. Individuals could collect the supplement for up to three calendar years from the time they first began receiving it, as long as they were working full time and not receiving income assistance.

Appendix B: Propensity Score Matching Technique

This paper employs a difference-in-difference method to estimate the effect of the Self-Sufficiency Project (SSP) 12-month supplement take-up constraint on job leaving. For the estimator, it is necessary to create four different groups: a constrained and unconstrained group within each of the control and program groups. These groups are called C^{con} , C^{un} , P^{con} , and P^{un} respectively. The untreated optimal search behaviour within the control group is directly observable. Consequently, the constrained control group is defined as all those members of the control group who did not work full time in the first 12 months of the program. The unconstrained control group is composed of those remaining control group members who worked full time in the first year.

Since untreated optimal search behaviour is not directly observable in the program group, another method is required to create the program constrained and unconstrained groups. Individuals are assigned to either P^{con} or P^{un} by matching program group members to members of C^{con} or C^{un} based on their probability or propensity to have been constrained by the time limit. This method of matching is called “propensity score matching”. The method of matching program group members to C^{un} is identical to that of C^{con} ; therefore, for expositional ease, this appendix uses assignment to P^{con} as an example.

The matching methodology first calculates a propensity score for program and control group members that measures the probability that they were constrained by the supplement take-up time limit. The propensity score is calculated in the following steps.

First, coefficients for the determinants of optimal search durations of at least 12 months are estimated. These coefficients can be estimated only by using members of the control group since program group members’ behaviour was affected by the supplement offer. Kemple and Snipes (2001) suggest that using the same sample to estimate the propensity score and to estimate the program effects will lead to over-fitted values and, therefore, biased estimates. Possible solutions include using bootstrapped standard errors or a randomly generated subsample to estimate the propensity and subsequently exclude the subsample from the main analysis.

Fortunately, a sample of control group members is available from a companion study called the “SSP Applicant study.” The Applicant study differs from the Recipient study in two important ways. First, participants in the Applicant study lived in British Columbia, whereas the Recipient sample was drawn from both British Columbia and New Brunswick. This should not affect the suitability of the applicant sample, however, since results from both the 36- and 54-month Recipient follow-up surveys have suggested that the impacts were not statistically different in the two provinces (Michalopoulos et al., 2000, 2002).

The second important difference between the Recipient and Applicant study samples is that Recipients had all been on income assistance (IA) for at least 12 months at random assignment. In contrast, Applicants had received IA for no more than one month prior to random assignment. To make the samples more comparable in this respect, only members of

the Applicant control group who remained on IA for at least 12 months after random assignment were used to estimate the propensity score.

Using this sample of control group members in the Applicant study, the log odds of remaining unemployed for at least 12 months were estimated using a logit model. The coefficients from this model were then exported to Recipient data. By applying the coefficients to all members of the Recipient sample and taking an anti-logit, the propensity score was obtained for both control and program group members.

After calculating the propensity scores, the next step in creating the P^{con} group was to match program group members to the control group members who did not work full time in the first year of the program. There are many strategies available to perform propensity score matching.⁹ Following Dehejia and Wahba (1998), program group members were matched to members in C^{con} using stratified nearest-neighbourhood matching, with replacement.

In general, matching is performed by minimizing a distance metric. If the metric is the Euclidean distance then the objective is

$$\min_{m(.)} D = \frac{1}{n} \sum_{i=1}^n d(i, m(i))$$

where $d(i, C^{>12}) = \left| p_i - \frac{1}{|C^{>12}|} \sum_{j \in C^{>12}} p_j \right|$ and $|C^{>12}|$ is the number of control group members

who remained out of work for more than 12 months. Program group members are indexed by i and control group members are indexed by j ; p is the propensity score. When members of C^{con} are exactly identical to matched members of P^{con} , then $D = 0$.

Because the employment and job-search status of participants at random assignment would fundamentally affect the duration of their optimal search, the matching approach used here was stratified by employment and job-search status. Members of C^{con} and the program group were divided into four mutually exclusive groups: those, at random assignment, who were employed full time, employed part time, unemployed and looking for work, and unemployed but not looking for work. Matches for members of C^{con} with a particular baseline employment and job-search status were made only within the same category of program group members.

The first step in the matching procedure estimated coefficients for the determinants of optimal search behaviour. The coefficients were estimated in the sample of Applicant control group members who collected IA in all of months 1 to 12. The coefficients were obtained by regressing baseline characteristics on a dummy variable indicating whether the respondent worked full time in any of months 13 to 24, using a logit model. To obtain the coefficient for group P^{con} , the dependent variable took on the value 1 if the respondent did not work full time in months 13 to 24. For P^{un} , the dependent variable took on the value 1 if the respondent did work full time in any of months 13 to 24. The results of the model are reported in Appendix Table B.1.

⁹Heckman, Ichimura, and Todd (1997) and Bloom, Michalopoulos, Hill, and Lei (2002) assessed the matching methodology's ability to eliminate bias associated with non-experimental estimates. Heckman et al. concluded that matching methods using comparison groups from similar labour markets can substantially reduce non-experimental bias, while Bloom et al. concluded that matching techniques cannot replicate results from randomized experiments.

Table B.1: Results From Logit Estimation of Propensity to Be Unemployed for More Than 12 Months (Applicant Sample)

Covariate	Coefficient (Standard Error)
Personal characteristics	
Female	-0.58 (0.36)
Age of sample member at random assignment ^a	
25–29	0.34 (0.32)
30–39	1.18*** (0.39)
40–49	1.70*** (0.57)
50 and older	1.51 (0.95)
Less than high school education	0.15 (0.20)
Reported First Nations ancestry	0.06 (0.32)
Immigrated less than 5 years ago	0.33 (0.34)
Immigrated 5 or more years ago	0.32 (0.24)
Lives in urban region	0.44 (0.29)
Speaks English	-0.83 (0.53)
Barriers to work	
Reported physical condition that limited activity ^b	0.30 (0.27)
Reported emotional condition that limited activity ^c	0.16 (0.37)
At risk of depression ^d	-0.02 (0.18)
Could not work because of lack of child care	-0.20 (0.24)
Could not work because of illness	0.12 (0.36)
Could not work because of family responsibilities	0.42 (0.25)

(continued)

Table B.1: Results From Logit Estimation of Propensity to Be Unemployed for More Than 12 Months (Applicant Sample) (Cont'd)

Covariate	Coefficient (Standard Error)
Employment history	
Ever worked for pay	-0.74 (0.77)
Years of prior employment	-0.09*** (0.02)
Working part time at random assignment	0.40 (0.48)
Not employed, looking for work	0.73 (0.46)
Neither employed nor looking for work	1.52*** (0.48)
Full-time employment greatest need at random assignment ^e	-0.46** (0.20)
Family structure	
Family received welfare while growing up	0.28 (0.26)
Never married	-0.01 (0.24)
Number of children at random assignment	-0.08 (0.11)
Age of youngest child at random assignment ^f	
6–11	0.11 (0.24)
12 or older	-0.10 (0.34)
Sample size	902

Sources: Calculations based on baseline survey data.

Notes: Statistical significance levels are indicated as * = 10 per cent; ** = 5 per cent; *** = 1 per cent.

^aReference group includes those aged 19 to 24.

^bThis group includes sample members who indicated having a long-term physical condition or health problem that limited the kind or amount of activity they could do at any of the following: at home, at school, at work, or in other activities such as travel, sports, or leisure.

^cThis group includes sample members who indicated having a long-term emotional condition or health problem that limited the kind or amount of activity they could do at any of the following: at home, at school, at work, or in other activities such as travel, sports, or leisure.

^dSample members were considered to be at risk of depression if they scored 3 or more (out of a possible total score of 12) on an abridged version of the CES-D (Center for Epidemiologic Studies–Depression) scale.

^eThe precise question on the baseline survey was: “At present, which of these best describes your greatest need?” Sample members were allowed to choose among immediate full-time employment, immediate part-term employment, education or training, or something else.

^fReference group includes those with children aged under 6 years.

The propensity scores were predicted using a set of covariates describing the participants' personal characteristics, any barriers to work they might face, their employment history, and their family structure. As Table B.1 shows, most covariates were not significant predictors of whether the participant remained unemployed for more than 12 months.¹⁰ Rubin and Thomas (1996) suggest that for the purposes of creating a propensity score, a fuller model is preferred to a parsimonious specification. Despite their lack of significance, the coefficients have the expected sign.

Table B.2: Effect of the Constraint Within the Control Group

Row		Constrained Group (Untreated Search > 12 months)			Unconstrained Group (Untreated Search ≤ 12 months)			Difference in Difference (3) - (6) (7)
		Program Group (1)	Control Group (2)	Difference (3)	Program Group (4)	Control Group (5)	Difference (6)	
Unadjusted Estimates (%)								
Panel A: Last full-time job at any time during the follow-up								
1	Lost job	21.8	26.4	-4.6	23.5	25.6	-2.1	-2.5
2	Left job	24.6	26.1	-1.5	25.0	26.1	-1.2	-0.3
3	Either lost or left job	46.4	52.5	-6.1	48.4	51.8	-3.3	-2.8
	Sample size^a	285	285		236	236		
Panel B: Last full-time job when the supplement was no longer available								
4	Lost job	17.1	15.7	1.4	18.9	15.2	3.8	-2.3
5	Left job	15.2	15.1	0.1	14.7	15.3	-0.6	0.7
6	Either lost or left job	32.3	30.8	1.5	33.7	30.5	3.2	-1.7
	Sample size^b	250	250		161	161		
Adjusted Estimates (%)^c								
Panel C: Last full-time job at any time during the follow-up								
7	Lost job	23.9	22.4	1.5	23.6	23.0	0.6	0.9
8	Left job	25.5	20.6	4.9	25.4	20.8	4.6	0.3
9	Either lost or left job	49.5	43.0	6.5	49.0	43.8	5.2	1.3
	Sample size^a	285	285		236	236		
Panel D: Last full-time job when the supplement was no longer available								
10	Lost job	15.0	18.2	-3.1	15.2	18.5	-3.3	0.2
11	Left job	19.0	17.5	1.5	19.0	17.5	1.6	-0.1
12	Either lost or left job	34.0	35.6	-1.6	34.2	36.0	-1.8	0.1
	Sample size^b	250	250		161	161		

Sources: Calculations based on baseline, 18-month, 36-month, and 54-month surveys.

Notes: Two-tailed t-tests were applied to differences between the outcomes for the program and control groups.

Rounding may cause slight discrepancies in sums and differences.

F-tests were applied to difference-in-difference estimates.

^aSample includes those who worked in at least one full-time job during the follow-up.

^bSample includes those who worked in at least one full- or part-time job during months 36 to 52.

^cEstimates are adjusted for the following baseline characteristics: age, gender, speaks English, less than high school education, First Nations ancestry, physical or emotional problem reported, age of youngest child, couldn't work at baseline because of a lack of adequate child care, couldn't work at baseline because of family responsibility, full-time work greatest need at baseline, and years of prior employment.

¹⁰Stewart and Dooley (1999) find that the duration of welfare spells was longer for younger, never married, poorly educated, and unemployable lone mothers. The transition to work is also found to be inversely related to the number and age of children.

References

- Allen, D. (1993). Welfare and the family: The Canadian experience. *Journal of Labour Economics*, 11, S201–S223.
- Blank, R., Card, D., & Robins, P. (1999). *Financial incentives for increasing work and income among low-income families* (Joint Center for Poverty Research Working Paper 69). Chicago: Joint Center for Poverty Research, Northwestern University, University of Chicago.
- Bloom, D., & Michalopoulos, C. (2001). *How welfare and work policies affect employment and income: A synthesis of research*. New York: Manpower Demonstration Research Corporation.
- Bloom, H., Michalopoulos, C., Hill, C., & Lei, Y. (2002). *Can non-experimental comparison group methods match the findings from a random assignment evaluation of mandatory welfare-to-work programs?* New York: Manpower Demonstration Research Corporation.
- Blundell, R. & Brewer, M. (2000). *Tax and benefit reform in the UK: Making work pay*. Mimeo. Draft peer review for the European Commission.
- Card, D., & Michalopoulos, C. (2001). *The limits to wage growth: Measuring the growth rate of wages for recent welfare leavers* (National Bureau of Economic Research Working Paper 8444). Cambridge, MA: National Bureau of Economic Research.
- Card, D., & Sullivan, D. (1988). Measuring the effect of subsidized training programs on movements in and out of employment. *Econometrica*, 56.
- Charette, M., & Meng, R. (1994). The determinants of welfare participation of female heads of household in Canada. *Canadian Journal of Economics*, 27, 290–306.
- Connolly, H., & Gottschalk, P. (2003). *Do earnings subsidies affect job choice? The impact of SSP Supplement payments on wage growth* (SRDC Working Paper Series 03-02). Ottawa: Social Research and Demonstration Corporation.
- Dehejia, R. H., & Wahba, S. (1998). *Propensity score matching methods for non-experimental causal studies* (National Bureau of Economic Research Working Paper 6829). Cambridge, MA: National Bureau of Economic Research.
- Dooley, M. (1999). The evolution of welfare participation among Canadian lone mothers from 1973–1991. *Canadian Journal of Economics*, 32, 589–612.
- Eissa, N., & Liebman, J. B. (1996). Labor supply response to the Earned Income Tax Credit. *Quarterly Journal of Economics*, 111, 605–637.
- Foley, K., & Schwartz, S. (2003). Earnings supplements and job quality among former welfare recipients: Evidence from the Self-Sufficiency Project. *Relation Industrielles/Industrial Relations*, 48 (3).
- Heckman, J., Ichimura, H., & Todd, P. E. (1997). Matching as an econometric evaluation estimator: Evidence from evaluating a job training program. *The Review of Economic Studies*, 64, 605–654.
- Kemple, J. J., & Snipes, J. C. (2001). *A regression-based strategy for defining subgroups in a social experiment* (Working Papers on Research Methodology). New York: Manpower Demonstration Research Corporation.

- Lippman, S. A., & McCall, J. J. (1976). Job search in a dynamic economy. *Journal of Economic Theory*, 12, 365–390.
- MaCurdy, T., Green, D., & Paarsch, H. (1990). Assessing empirical approaches for analyzing taxes and labor supply. *Journal of Human Resources*, 25, 415–490.
- Michalopoulos, C., Card, D., Gennetian, L. A., Harknett, K., & Robins, P. K. (2000). *The Self-Sufficiency Project at 36 months: Effects of a financial work incentive on employment and income*. Ottawa: Social Research and Demonstration Corporation.
- Michalopoulos, C., Tattrie, D., Miller, C., Robins, P. K., Morris, P., Gyarmati, D., Redcross, C., Foley, K., & Ford, R. (2002). *Making work pay: Final report on the Self-Sufficiency Project for long-term welfare recipients*. Ottawa: Social Research and Demonstration Corporation.
- Moffit, R. (1992). Incentive effects of the U.S. welfare system: A review. *Journal of Economic Literature*, 30, 1–61.
- Mortensen, D. T. (1986). Job search and labor market analysis. In O. Ashenfelter & R. Layard. (Eds.) *Handbook of labor economics*. New York: Elsevier Science Publishers.
- Rubin, D. B., & Thomas, N. (1996). Matching using estimated propensity scores: Relating theory to practice. *Biometrics*, 52, 249–264.
- Saez, E. (2000). *Optimal income and transfer programs: Intensive versus extensive labor supply responses* (National Bureau of Economic Research Working Paper 7708). Cambridge, MA: National Bureau of Economic Research.
- Scholz, J. K. (1996). In-work benefits in the United States: The Earned Income Tax Credit. *Economic Journal*, 106, 156–169.
- Stewart, J., & Dooley, M. (1999). The duration of spells on welfare and off-welfare among lone mothers in Ontario. *Canadian Public Policy*, 25, S47–S72.