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Educational Upgrading and Its Consequences Among Welfare Recipients:

Empirical Evidence From the Self-Sufficiency Project

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Executive Summary

This paper examines the extent and nature of educational upgrading among single parents on welfare. It also investigates whether policies that encourage income assistance (IA) recipients to exit welfare for full-time employment influence participation in educational activity. In addition, the paper analyzes the consequences of increases in educational attainment for employment, earnings, and reliance on welfare in this population.

To address these issues, we employ data from the Self-Sufficiency Project (SSP), a demonstration project designed to provide a rigorous test of a temporary earnings supplement. At the outset of the demonstration project, all participants were single parents with children who had been long-term IA recipients.

Our study is motivated by several considerations. First, relatively little is known about the extent to which those who exit welfare to enter the workforce do so by upgrading their formal education. A second motivation is associated with assessing the long-run consequences of programs that provide a financial incentive to exit welfare. Some welfare-to-work programs are designed to enhance the skills of IA recipients, and a central focus of evaluations of such programs is their impact on the employment and earnings of participants. In contrast, earnings supplementation programs provide a pure financial incentive to leave welfare. By raising the return to existing skills, such programs may reduce the incentive to invest in additional education. In addition, by encouraging full-time employment, they may reduce the time available for educational activity. If so, earnings supplementation policies may inadvertently reduce the long-run earnings capacity of welfare recipients.

There are four principal findings. First, there was a substantial amount of educational upgrading in this population. At baseline, more than one half of SSP participants had not completed secondary school. About 20 per cent of those who were high school dropouts at the beginning of the demonstration had completed secondary school by the end, four and a half years later. There was also substantial growth in post-secondary education. Over one fifth of those who had never enrolled in a community college or trade school program at baseline did so during the demonstration. There were also large increases in completion of college and trade school programs. Across all forms of schooling, there is evidence of a substantial amount of investment in acquiring additional education among these long-term welfare recipients.

Our second conclusion is that there was a substantial amount of mismeasurement of education in this population. This finding reinforces previous studies that conclude that self-reported education is subject to considerable measurement error. We deal with this situation by creating several samples that make different assumptions about the nature and extent of mismeasurement of education. These alternative samples allow us to check the sensitivity of our results to various forms of measurement error.

We also find that members of the SSP program group acquired less additional education during the period than did their counterparts in the control group. One possible explanation for this result is that the earnings supplement encouraged program members to exit welfare and take up full-time employment, thus providing less time for acquiring additional education. Consistent with this *time crunch* hypothesis, the difference in courses taken between program and control group members is greatest during the early part of the SSP demonstration when the employment gap between the two groups was largest.

The lower levels of human capital acquisition observed in the program group are most evident at the secondary school level. However, there is also evidence of less educational activity among community college programs. These results suggest that a financial incentive to exit welfare and take up full-time employment may, in addition to its demonstrated benefits in the form of hastening the exit from IA, also have an adverse side effect in the form of reduced investment in education.

Our final conclusion is that those who upgraded their education generally achieved larger gains in employment and wage rates than did their counterparts who did not acquire additional education. However, the impact of increases in education on welfare receipt is less clear. For this analysis we construct two samples of *potential upgraders*. The first consists of those who had not completed secondary school at baseline. We then compare the subsequent employment, wage rate, and welfare receipt outcomes of those who obtained a high school diploma with those who did not. The second sample consists of those who reported at baseline that they had never enrolled in a community college or trade school. We then compare the end-of-period employment and wage rate outcomes of those who enrolled in college or trade school with those who did not. We control for other influences using both linear regression and non-parametric matching estimation. We find striking differences between the *upgraders* and *non-upgraders* in both employment and wages. For example, our matching estimates imply employment gains of 13 percentage points associated with high school completion and 18 per cent wage gains associated with both secondary school completion and enrolment in a college or trade school program. Upgraders were also less likely to remain on welfare.

One interpretation of these results is that the much larger gains in employment and wages experienced by upgraders are a consequence of their investments in education during the period. However, an alternative interpretation is that there are unobserved factors such as ability and motivation that are correlated with both the propensity to make educational investments and wage and employment outcomes. We carry out a series of specification tests to assess the role of such unobserved factors. For the high school sample we find no evidence that unobserved factors account for differences in outcomes between upgraders and non-upgraders. A similar result is obtained for wage outcomes among the college/trade school sample. These results thus suggest that the positive wage and employment gains experienced by dropouts who completed secondary school, and the positive wage gains experienced by college and trade school enrollees, were a consequence of the educational investments made during the period. However, the specification tests suggest that the reduction in welfare use experienced by upgraders may be due to unobserved differences between upgraders and non-upgraders rather than to their additional education. Overall, our findings provide rather striking evidence that investments in formal education — such as completing secondary school — can yield significant benefits for single parents on welfare, a group that is of considerable policy interest.

Introduction

This paper examines the extent and nature of educational upgrading among single parents on welfare. It also investigates whether policies that encourage income assistance (IA) recipients to exit welfare for full-time employment influence participation in educational activity in this population. In addition to studying the extent of educational change, the paper also investigates the consequences of increases in educational attainment for employment, earnings, and welfare use among this population.

Our study is motivated by several considerations. First, relatively little is known about the extent to which those who exit welfare to enter the workforce do so by upgrading their formal education.[1] More generally, we need to understand more about the nature of changes in educational attainment that occur among the low-income population (for example, high school completion versus trade and vocational programs) and the consequences of such educational upgrading.

A second motivation is associated with assessing the long-run consequences of programs that provide a financial incentive to exit welfare. Some welfare-to-work programs are designed to enhance the skills and knowledge of IA recipients. In these circumstances a central focus of the evaluation of such programs is their impact on the employment prospects and earnings capacity of program participants. In contrast, earnings supplementation programs provide a pure financial incentive to leave welfare. By raising the return to existing skills, such programs may reduce the incentive to invest in additional education. In addition, by encouraging full-time participation in the workforce, they may reduce the time available for educational activity. If so, earnings supplementation policies may inadvertently reduce the long-run earnings capacity of welfare recipients.

To address these issues, we employ data from the Self-Sufficiency Project (SSP), a demonstration project designed to provide a rigorous test of a temporary earnings supplement. SSP was carried out in the provinces of British Columbia and New Brunswick during the 1990s. The impacts of the policy were evaluated using a random assignment design. Members of the program group were offered a generous earnings supplement if they left welfare to take full-time employment within a 12-month period. Those taking up the supplement offer could receive the earnings supplement for up to three years, provided they continued to meet the eligibility criteria. Program group members could also return to welfare at any time. Those randomly assigned to the control group received nothing from SSP — they could remain on welfare or enter the workforce.

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[1]Even our knowledge about the impact of the *level* of education (as opposed to changes in educational attainment) on the likelihood of exiting from welfare is limited. This situation exists principally because administrative data on IA programs — the main source of information for analyzing the determinants of the exit rate from welfare — often do not contain information on educational attainment. One exception is the province of New Brunswick, analyzed in the study by Barrett (2000). More recently, Coelli, Green, and Warburton (2004) use administrative IA data from the province of British Columbia matched to school records to study the impact of education on welfare receipt among those whose parents had received welfare. They conclude that high school graduation has a large impact on the likelihood of receiving welfare among this population.

The impact of the SSP financial incentive on educational attainment could be either positive or negative. Two factors that work in opposite directions are time and money. Members of the program group who took up the supplement earned substantially more than otherwise, and this additional income could be used to offset the costs of additional education. However, those receiving the supplement were required to work at least 30 hours per week and thus had less time to devote to acquiring additional education than did their counterparts who remained on IA. We refer to these channels of influence as the *money crunch* and *time crunch* hypotheses respectively.

The SSP research design may also have affected the timing of educational change. Individuals assigned to the program group had a powerful incentive to obtain full-time employment within the 12-month eligibility window and may have acquired further schooling in order to obtain a suitable job. However, while receiving the earnings supplement, those in the program group obtained little benefit from additional schooling. Because of the generous nature of the supplement, gains in market wages had little impact on total earnings. Thus program and control group members may have faced different incentives to acquire additional education during the 12-month eligibility window and the three-year supplement period. However, once eligibility for the supplement had expired, program and control group members faced similar incentives to invest in further education.

Our final motivation is related to improving our understanding of the consequences of increases in formal education among the low-income population. As economic activity becomes more knowledge-based, human capital formation is increasingly regarded as a central component not only of economic policy, but also of social policy. However, as discussed below, some observers question whether increases in educational attainment are worthwhile for the low-income population. Thus another objective of this paper is to assess the employment and earnings consequences of educational investments made by participants in the SSP demonstration as well as the consequences for welfare use.

The study has two components. The first part analyzes changes in educational attainment. Although we carry out a comprehensive examination of the various routes by which gains in formal education are achieved, particular focus is given to high school completion among secondary school dropouts and enrolment in trade school, vocational, and community college programs among both dropouts and high school graduates. We begin by estimating the impact of SSP on the various forms of educational upgrading. In order to obtain additional insight into the factors that may contribute to these overall net impacts, we also analyze the extent and nature of educational upgrading among subsets of the SSP program and control groups. Our study thus uses the random assignment feature of the SSP research design but also employs non-experimental methods.

The second part of the study analyzes the effects of increases in educational attainment on employment, earnings, and welfare receipt. We focus on two groups for which educational upgrading is quantitatively important: (i) high school dropouts who complete secondary school and (ii) high school dropouts and graduates who enrol in a non-university post-secondary program. As a comparison group for both groups, we use individuals who had the same level of initial formal schooling but who did not increase their educational attainment. For example, in the case of estimating the consequences of completing secondary school, the comparison group consists of high school dropouts who remained dropouts at the end of the sample period.[2] An important feature of this analysis is the availability of longitudinal data, which allows us to identify those individuals who increased their formal education over the sample period and those individuals who did not.

Our examination of SSP data indicates that increases in educational attainment among current and former welfare recipients are quantitatively important. For example, about 20 per cent of those who were high school dropouts at the time of the baseline survey had completed secondary school by the 54-month survey.[3] A similar amount of educational activity involved coursework in vocational and community colleges and trade schools. Approximately one fifth of those who were dropouts or high school graduates without further education at the baseline survey date had enrolled in such programs by the 54-month survey. The extent of educational upgrading suggests that this is a phenomenon worthy of investigation.

We also find that the offer of a financial incentive to exit welfare and take full-time employment reduced educational upgrading among SSP participants. The timing of these impacts is consistent with the time crunch hypothesis. Thus, encouraging full-time employment has potentially adverse consequences for the long-term earnings capacity of single parents on welfare.

Our final conclusion is that those who upgraded their formal education achieved large gains in employment and wages relative to their counterparts who did not acquire additional education. The large gains experienced by *upgraders* generally appear to be a consequence of their investments in education rather than being due to unobserved factors that are correlated with both the propensity to make educational investments and wage and employment outcomes. Our results suggest that gains in formal education are a potentially important source of growth in earnings capacity among single parents on welfare. However, it is less clear that acquiring additional education reduces the incidence of welfare receipt.

[2] For an example of this approach, see Blundell, Dearden, Goodman, and Reed (2000).

[3] Some of this change may be the result of mismeasurement of education. We devote considerable attention in the paper to dealing with measurement error in self-reported education.

Policy Background

The analysis in this study is relevant to a number of current policy concerns. Education, training, and skill formation have become prominent public policy issues in Canada and in many other countries. Several factors account for the increased attention being paid to the knowledge, skills, and competencies of the population and workforce. Technological change — especially advances in information and computer technologies — and the globalization of production have resulted in growing demand for highly skilled workers and changes in the nature of skills needed in the workplace. These same forces also appear to have contributed to widening inequality between more- and less-skilled workers in employment, wages, and other labour market outcomes. In addition, there is growing concern about future skills shortages, in part due to the fact that the leading cohorts of the well-educated baby boom generation are now approaching retirement age and are being replaced by the entry into the labour force of much smaller (though even-better-educated) cohorts. Finally, there has been a resurgence of interest in the determinants of long-term growth and greater emphasis on the importance of human capital in the creation of new knowledge and in the growth of living standards over time.

These factors explain the increased emphasis on skills and knowledge in economic policy. However, as economic activity becomes more knowledge-based and less dependent on natural resources and physical capital, human capital is also increasingly being viewed as a central component of social policy. Many of our current social programs were shaped during the expansion of the welfare state that took place during the early post-war period. As substantial changes to the economic and social environment have occurred, a major reassessment of these programs has been underway. Governments have begun to move away from passive income maintenance programs toward active labour market and social policies that facilitate adjustment to change, assist the jobless to find work, and encourage labour force participation. Associated with this shift has been greater emphasis on individual responsibility and on providing those in need of assistance with the opportunity to improve their economic situation — providing a hand up rather than a handout. Investing in the human capital of those with limited marketable skills is a key component of such an approach. As stated by Paul Martin, "Providing security and opportunity for Canadians in the future means investing in their skills, in their knowledge and capacity to learn . . . . Good skills are an essential part of the social safety net of the future" (Department of Finance Canada, 2002).

This philosophy was evident in the major changes that were introduced during the 1990s to Canada's social assistance programs. In many provinces a prominent feature of these reforms was greater emphasis on human capital and skills formation among welfare recipients. For example, as we discuss in more detail subsequently, both British Columbia and New Brunswick introduced income assistance (IA) reforms that provided incentives for — and in some cases compelled — welfare recipients to participate in education or skills upgrading.

Recent changes to Canada's IA programs thus reflect the belief that "good skills are an essential part of the social safety net." Nonetheless, there are dissenting views. Some are skeptical of this perspective, arguing that there is too much emphasis on the supply of skills and knowledge. One variant of this view holds that there is substantial underemployment of well-educated workers, that many individuals are overqualified for their jobs, and that their skills are underutilized (e.g. Lowe, 2000). Another variant — one that is particularly relevant to this study — holds that education and skills matter little among the low-income population. For example, in the US context, Lafer (2002) states that:

> *In the two-thirds of the labor market where college degrees are not required, the relationship between education and wages is extremely weak. Instead, the earnings of nonprofessional workers appear to be primarily determined by legal, institutional and political factors . . . .* (p. 4)
>
> *Federal employment policy remains committed to the proposition that education is the single most important requirement for Americans hoping to work their way out of poverty. This assumption, however, flies in the face of available evidence. The evidence indicates that education plays a relatively minor role in determining wages and that it cannot serve . . . as an effective strategy for restoring Americans' earning power.* (p. 46)

Thus there are sharply contrasting views about the value of additional education among the low-income population. One reason for these contrasting perspectives is the limited empirical evidence on the consequences of education for such individuals.[4] A key objective of this paper is to provide such evidence for a group for which there is substantial policy interest — single parents on welfare.

In both Canada and the United States, welfare programs were substantially restructured during the 1990s to place greater emphasis on work. At least partly as a result of these policy changes, welfare caseloads declined sharply in both countries, and many individuals leaving IA entered the workforce. However, it is less clear that those leaving welfare are better off as a result. In her survey of US evidence, Blank (2002) notes that "A disappointing aspect of the mandatory work programs is that they provide little evidence of increased income. In fact, increases in earnings appear to be entirely offset by losses in public assistance income." Similarly, Acs and Loprest (2004) state that "studies of welfare leavers indicate that working leavers are, for better or worse, entrenched in the low-wage labor market." Such findings raise several questions, including the following. How can these individuals progress out of low-wage jobs? Do they have alternatives to returning to IA if they lose their jobs? Our study examines the role of a key potential mechanism for escaping the low-wage labour market — gains in formal education.

Our results are also relevant to the design of welfare-to-work programs. One approach emphasizes moving recipients into jobs quickly, even if at low initial wages. This *work-first*

[4]There is a vast empirical literature on the returns to education; see, for example, Card (1999). While many recent studies focus on the returns to university programs, some Canadian studies focus on the consequences of additional secondary schooling and trade and vocational education. In his study of compulsory schooling laws in Canada, Oreopoulos (2003) finds that, over the period 1920 to 1970, students compelled to acquire additional secondary schooling benefited substantially. However, during much of this period, secondary school was a relatively advanced level of education. Using data from the past two to three decades, Parent (2001) and Boudarbat, Lemieux, and Riddell (2003) compare high school dropouts to high school graduates without additional education. They find earnings gains from high school completion of approximately 10 per cent for men and 10 to 15 per cent for women, after adjusting for work experience. This latter adjustment is important because older, more experienced workers are also less educated. Ferrer and Riddell (2002) also estimate gains to secondary school and college and trade school programs that are moderately large, albeit somewhat below those associated with university programs. All of these studies thus suggest that additional secondary schooling and non-university post-secondary education yield substantial real rates of return, at least on average. However, whether there are disadvantaged groups that receive little benefit from additional education remains an open question.

model reflects the view that welfare recipients can best acquire work habits and skills at the workplace. In contrast, the *human capital* approach provides more training and educational opportunities to IA recipients. As discussed by Blank (2002), evidence from careful evaluations of these approaches indicates that the work-first strategy increases earnings and reduces welfare receipt to a greater extent than does the human capital strategy, at least in the short run (two to three years after entry into the program). However, human capital strategies may be superior over longer time horizons. For example, using data from California's GAIN program — a program that adopted a variety of welfare-to-work strategies — Hotz, Imbens, and Klerman (2000) find that recipients who participated in human capital programs did as well as or better than those in work-first programs in years 7 to 9 after the program. In this paper we examine the consequences of a particular variant on the work-first strategy — one that provides a powerful financial incentive to obtain and maintain full-time employment. Our objective is not to contrast this policy with one that provides educational and training opportunities. Rather, we seek to understand whether welfare-to-work programs that focus on obtaining full-time employment may retard gains in formal education that would otherwise occur. Such a finding would add further complexity to the debate over alternative welfare-to-work approaches.

The Self-Sufficiency Project

The Self-Sufficiency Project (SSP) was designed to provide evidence on the effects of a financial incentive on long-term welfare recipients, defined as those who had been on income assistance (IA) for at least 12 of the past 13 months.[5] The study focused on single parents with children, the group with the lowest exit rates from welfare.[6] Among those who volunteered to participate in the demonstration project, one half was randomly assigned to the program group that was eligible for the earnings supplement. The remaining volunteers were assigned to the control group. Those in the program group were offered a financial incentive to leave welfare and take up full-time employment, defined as at least 30 hours per week.[7] The earnings supplement was calculated as one half of the difference between a target income level and the individual's labour market earnings. The financial incentive was quite generous, approximately doubling income from work for the typical participant and providing total income substantially higher than welfare benefits. In addition to providing a strong incentive to leave welfare and take up full-time employment, the lower implicit tax rate on market earnings under the SSP design provided marginal incentives that were much higher than conventional IA programs.

The SSP demonstration incorporated two important time limits. Members of the program group were given up to 12 months following random assignment to take up the earnings supplement (i.e. to obtain full-time employment). Once they had qualified, participants could continue to receive the supplement for three years. Receipt of the earnings supplement in any period was conditional on maintaining full-time employment during that period. At the end of the three-year period the supplement ended. At any point in time during the supplement period, program group members could return to IA. Members of the control group were treated no differently than other welfare recipients — they could remain on welfare or enter the workforce.

The demonstration project operated during the 1990s — a turbulent period in Canada's labour market. Random assignment took place between November 1992 and March 1995. Combining the 12-month window to establish eligibility and the maximum three-year duration of the supplement, the demonstration project continued until late 1996 to early 1999. Thus the experiment began during a period of unusually weak economic conditions, when the Canadian economy was beginning to recover from the prolonged recession of 1990 to 1992. Toward the end of the SSP demonstration, and especially after 1997, Canada's economy was expanding at a strong rate and the labour market was experiencing rapid employment growth and declining unemployment.

The experimental findings are summarized in Michalopoulos et al. (2002). More than one third of the program group obtained full-time employment and took up the earnings

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[5] For further details of the SSP demonstration and its consequences, see Michalopoulos et al. (2002).

[6] According to evidence from the provinces of British Columbia and Quebec reported in Barrett and Cragg (1998) and Lacroix (2000), single parents with children constitute 20 per cent to 25 per cent of the welfare population at any point in time. Single men and women without children constitute the largest group of IA recipients, followed by married parents without children. However, these groups also leave welfare at a more rapid rate.

[7] Full-time employment could be achieved by combining two or more part-time jobs.

supplement. During the SSP eligibility period, the program group experienced gains in earnings and employment and reduced welfare use relative to the control group. The largest impacts were observed during the first 12 to 15 months following random assignment. After this time the differences in outcomes between the program and control groups gradually narrowed. By the end of the three-year period of eligibility for the earnings supplement, the program/control group differences in employment, earnings, and welfare use were small.

Educational Upgrading: Nature, Extent, and Determinants

This part of the paper documents the nature and extent of education upgrading during the Self-Sufficiency Project (SSP) demonstration. The impacts of the SSP earnings supplement on education and the determinants of changes in educational attainment are also examined. The organization is as follows. The first section, "Survey Information on Educational Activity and Attainment," describes the education-related information provided by the SSP surveys. The next section, "Accounting for Measurement Error in Educational Upgrading," assesses the nature and extent of mismeasurement of educational attainment in the SSP data and proposes some methods for reducing the amount of measurement error. "The Impact of the SSP Earnings Supplement Offer on Educational Attainment" examines the impact of the SSP financial incentive on educational attainment and enrolment, taking account of mismeasurement of schooling. "Determinants of Educational Upgrading" analyzes the factors that determine whether or not an SSP participant upgrades her education, while "Legislative Changes and Educational Upgrading" investigates whether changes in welfare programs introduced during the SSP demonstration may have influenced the educational activities of welfare recipients.

SURVEY INFORMATION ON EDUCATIONAL ACTIVITY AND ATTAINMENT

Participants in the SSP demonstration were surveyed at baseline, and after 18, 36, and 54 months. These surveys contain two principal sources of information that can be used to characterize education upgrading. One source is based on the *enrolment and educational attainment* questions. Each survey asked whether the individual a) graduated from high school; b) was ever enrolled in a community college or trade school, and if so whether he or she obtained a certificate or diploma from that schooling; and c) was ever enrolled in a university and if so whether he or she obtained a university certificate, diploma, or degree from that schooling. The baseline and 18-month surveys contain additional information on the nature of college and trades schooling, but these questions were eliminated in the 36- and 54-month surveys. As will be discussed further below, we use only questions that were identical, or very similar, across all four surveys. Using the enrolment and attainment questions, we can follow an individual over time and characterize upgrading by enrolment in and/or completion of an educational program.

Table 1 lists the enrolment and educational attainment questions used in our analysis. With a few exceptions, the questions asked were quite consistent (if not identical) over the four surveys. One exception arises because the 18-month survey gave individuals a specific example (the graduate diploma equivalent) of a credential equivalent to a high school diploma, while the other three surveys did not. Another is due to the baseline survey question regarding enrolment in a college or trade school, which may be considered broader than the other three surveys due to the phrase "any other kind of school." Finally, the university enrolment questions in the baseline and 18-month surveys differ slightly from those in the 36-month and 54-month surveys. The former surveys ask "Have you ever been enrolled in a university?" whereas the latter two surveys ask "Have you been enrolled in university?"

Table 1: Educational Attainment and Enrolment Questions in SSP Surveys

Baseline	18 Month	36 Month	54 Month
High school diploma			
Did you obtain a high school graduation diploma or equivalent?	Have you obtained your high school graduation diploma or equivalent such as the graduate diploma equivalent?	Have you obtained a high school graduation diploma or equivalent?	Have you obtained a high school graduation diploma or equivalent?
College or trades enrolment			
Have you ever been enrolled in any other kind of school, community college, technical institute, trade or vocational school?	Have you ever been enrolled in a community college, technical institute, trade or vocational school?	Have you ever been enrolled in community college, technical institute, trade or vocational school?	Have you ever been enrolled in community college, technical institute, trade or vocational school?
College or trade school credential			
Have you received any certificates or diplomas as a result of this education?	Have you received any certificates or diplomas from these schools?	Have you received any certificates or diplomas as a result of this education?	Have you received any certificates or diplomas as a result of this education?
University enrolment			
Have you ever been enrolled in a university?	Have you ever been enrolled in a university?	Have you been enrolled in university?	Have you been enrolled in university?
University credential			
What degrees, certificates or diplomas have you received from a university?	What degrees, diplomas or certificates have you received from a university?	Have you received any degrees, certificates or diplomas from a university?	Have you received any degrees, certificates or diplomas from a university?

It is important to keep in mind, however, that despite the high degree of comparability in specific questions asked in the four surveys, there were potentially important differences in the overall structure of the education module across surveys. In particular, the overall number of questions asked in the education model, the order of these questions, and the nature of questions closely related to those in Table 1 changed over the four surveys — in some cases significantly.

The second possible characterization of education upgrading is to use the *coursework questions*, available from the 18- through 54-month surveys. These questions, which are summarized in Table 2, asked the individual whether he or she taken any work or non-work-related courses since the previous survey. Individuals who responded positively were then asked whether these courses were work-related or non-work (education) -related. In the case of the education-related courses, each survey then asked the individual to specify the objective of the course based on the following choices: a) courses taken towards a high school diploma or equivalent, b) courses taken towards an apprenticeship diploma, c) courses taken towards a trade/vocational diploma or certificate, d) courses taken towards a college diploma or certificate, e) courses taken towards a university degree, f) courses taken towards personal interest/life-skills, and g) courses taken towards other goals not listed. For the purposes of this paper, we ignore the final two choices.

Table 2: Coursework Questions in SSP Surveys

18 Month	36 Month	54 Month
Any courses taken		
n/a	Since your last interview on [last interview date], have you taken any courses, either work-related or non-work-related such as courses toward the completion of a high school diploma, college diploma, university degree, on-the-job training, trade certificate or other certificate or other courses?	Since your last interview on [last interview date], have you taken any courses, either work-related or non-work-related such as courses toward the completion of a high school diploma, college diploma, university degree, on-the-job training, trade certificate or other courses?
Non-work-related courses taken		
Since your last interview, have you taken any other courses that are not directly work-related such as courses towards the completion of a high school diploma, college diploma or university degree?	Were any of these courses taken towards the completion of a diploma, certificate or degree?	Were any of these courses taken towards the completion of a diploma, certificate or degree?
Non-work course objectives		
What have you taken these courses towards?	Have you taken these courses towards . . . ?	Have you taken these courses towards . . . ?
1. High school diploma or equivalent	1. High school diploma or equivalent	1. High school diploma or equivalent
2. An apprenticeship diploma	2. An apprenticeship diploma	2. An apprenticeship diploma
3. A trade/vocational diploma/certificate	3. A trade/vocational diploma/certificate	3. A trade/vocational diploma/certificate
4. College diploma/certificate	4. College diploma/certificate	4. College diploma/certificate
5. University degree	5. University degree	5. University degree
6. Personal interest/life-skills	6. Personal interest/life-skills	6. Personal interest/life-skills
7. Other	7. Other	7. Other
Work-related courses taken		
Have you taken any work-related training or education including correspondence courses, on-the-job training, apprenticeship training or other courses?	Were any of these courses specifically work-related, that is, they were taken as part of a job requirement or to improve your skills within a job?	Were any of these courses specifically work-related, that is, they were taken as part of a job requirement or to improve your skills within a job?

The structure of the coursework questions was identical in the 36-month and 54-month surveys. However, the structure of the 18-month survey differed somewhat from the other two surveys. Specifically, the 18-month survey contained a single lead question asking whether any non-work-related courses were taken, whereas in the subsequent surveys this lead question was split into two parts, the first asking about work-related or non-work-related courses.

ACCOUNTING FOR MEASUREMENT ERROR IN EDUCATIONAL UPGRADING

Previous research has found that education is frequently mismeasured (Griliches, 1977; Ashenfelter & Krueger, 1994; Kane, Rouse, & Staiger, 1999; Warburton & Warburton, 2004). Accordingly, particular attention is given to assessing the importance of measurement error in this setting. Our base sample is 4,371 individuals, which is the entire SSP population less those in the SSP Plus program (*N* = 246)[8] and those who did not respond to all four surveys (*N* = 1,361).[9]

We begin by examining secondary school courses taken and high school completion. The left-hand panel of Table 3 shows all possible combinations of responses to the high school completion question over time. The right-hand panel shows the number of responses for each combination. We have divided the responses into two main groups, referred to as *Consistent responses* and *Inconsistent responses*. Consistency here refers to responses that accord with the fact that educational attainment can increase but cannot decrease over time. For example, we label as inconsistent someone who stated at the baseline survey that he or she had graduated from high school but who responded to a later survey that he or she had not completed high school. The consistent responses are further broken down into *No upgrading* and *Upgrading* categories. Inconsistent responses are also separated into two subgroups. *Majority cases* are those in which one survey response differs from the responses to the other three surveys. In these circumstances the single outlier may be a coding error or response error. *Non-majority cases* are those for which the responses are evenly divided between Yes and No in a manner that is not logically consistent.

Beginning with the "Diploma Conferred?" column in Table 3, 3,501 individuals (or 80.1 per cent of the sample) reported no change in their high school diploma status over the entire SSP period, with these about evenly split between high school dropouts and high school graduates. The "Upgrading" numbers show that a substantial number completed a high school diploma — 436 individuals or about 10 per cent of the total sample, and over 20 per cent of the potential upgraders — those without a high school diploma at baseline in the "Consistent responses" section of the table.

The bottom panels of Table 3 show evidence of a substantial amount of measurement error in educational attainment. About eight per cent of the sample reported that their education declined. However, most of these involve cases where three of the four surveys match up.

The questions on courses taken can be used to provide additional information on changes in educational attainment. In general, the responses to the coursework questions tend to support our classification into consistent and inconsistent responses. Those who responded "Yes" throughout the period to the question about high school graduation had a very low incidence of coursework. Those who responded "No" throughout the sample period took more courses than their counterparts who were high school graduates at baseline, but much fewer than those who reported an increase in educational attainment. Perhaps most importantly, individuals who reported completion of secondary school during the SSP period — and reported having done so in a logically consistent manner — also reported having taken a substantial number of courses. The incidence of coursework is about 20 per cent among the group we classify as consistent upgraders versus 6 to 7 per cent among those who consistently responded "No" to the high school graduation question, and about 1 per cent among those who consistently responded "Yes." Furthermore, the consistent upgraders reported much higher incidence of coursework

[8]SSP Plus was a variant designed to test the combination of the SSP financial incentive and additional services such as resumé writing, job clubs, and employment counselling.

[9]We tested for and found no evidence of a relationship between non-response to one or more surveys and assignment to the SSP program group.

than the inconsistent responses — more than double the amount of coursework reported by the inconsistent majority cases and more than three times the amount reported by the inconsistent non-majority cases.

Table 3: Education Upgrading — High School (*N* = 4,371)

Possible Survey Responses				Number of Cases: Attainment Question	Coursework Questions		
					High School Courses	High School Courses	High School Courses
Baseline	**18 Month**	**36 Month**	**54 Month**	**Diploma Conferred?**	**Baseline to 18 Month**	**18 Month to 36 Month**	**36 Month to 54 Month**
Consistent responses							
No upgrading							
No	No	No	No	1,688	114	112	102
Yes	Yes	Yes	Yes	1,813	25	21	10
			Total	*3,501*	*139*	*133*	*112*
Upgrading							
No	No	No	Yes	155	20	32	34
No	No	Yes	Yes	113	30	46	5
No	Yes	Yes	Yes	168	33	13	5
			Total	*436*	*83*	*91*	*44*
Inconsistent responses							
Majority cases							
Yes	No	No	No	34	5	3	5
Yes	Yes	Yes	No	31	2	0	5
Yes	Yes	No	Yes	35	2	2	0
Yes	No	Yes	Yes	56	6	1	0
No	No	Yes	No	65	8	4	3
No	Yes	No	No	44	2	4	1
			Total	*265*	*25*	*14*	*14*
Non-majority cases							
Yes	Yes	No	No	12	0	0	0
Yes	No	Yes	No	10	1	0	1
Yes	No	No	Yes	17	0	2	0
No	Yes	Yes	No	15	2	1	1
No	Yes	No	Yes	15	0	2	3
			Total	*69*	*3*	*5*	*5*
Missing observations							
Total				100	10	7	2

Among the consistent upgraders, the timing of courses taken and reported high school completion also line up well. For example, those responding “No/No/No/Yes” to the graduation question report having taken most of their courses between the 18-month and 54-month surveys, whereas those responding “No/No/Yes/Yes” report having taken most of their courses between the baseline and 36-month surveys.

On the basis of this information, one way to reduce the amount of mismeasurement would be to drop the individuals with inconsistent responses from the sample.[10] Although this approach has much appeal — and is one that we employ — we cannot presume that all of the consistent responses are measured without error. We therefore also adopt a more stringent approach to the measurement of increases in education.

Our approach involves specifying decision rules that answer two questions. First, what is the most appropriate way to define *upgrading*? Second, how should we define the set of *potential upgraders* — the group that will be used to estimate the counterfactual? Our method employs two criteria and associated samples of upgraders that probably bound the truth. Both reduce the amount of measurement error relative to the full sample. One imposes very stringent requirements for treating the individual as having increased their educational attainment. In this sample, overstatement of increases in education is likely to be rare. However, the stringent requirements may also result in some true educational upgrading not being identified as such. The second, less restrictive set of requirements classifies more individuals as upgraders, but is also more susceptible to non-upgraders being incorrectly identified as having increased their level of education.

For both of these samples, we eliminate the inconsistent cases from Table 3, thus substantially reducing the extent of mismeasurement of education. In the restrictive case, an individual is classified as an upgrader if the educational attainment variables indicate upgrading *and* the coursework questions support the change in credentials. For example, in the case of secondary school completion, this means that an individual who indicated a change in his or her high school diploma status must also have reported that he or she (i) did educational coursework and (ii) that these courses were taken towards the objective of a high school diploma. In the absence of this confirming information from the coursework questions, we do not accept their claim that they completed secondary school during the SSP period. We also require confirming information for classification as a non-upgrader. A non-upgrader or comparator is an individual who never had a high school diploma (based on the educational attainment variables) and who did not take any high school courses according to the coursework questions.

The less restrictive sample does not impose the coursework requirement on either the upgraders or non-upgraders. Thus upgraders are those listed in the "Upgrading" sections of Table 3 and comparisons are the "No/No/No/No" individuals in the "No upgrading" section.

With these definitions in place, we end up with 119 high school upgraders in the restrictive case and 1,440 comparisons. As seen in Table 3, the numbers are 436 and 1,688 for the less restrictive sample. Imposing the course requirement clearly has a very large impact on the measured extent of high school upgrading: 7.6 per cent relative to 20.5 per cent. For the members of the potential comparison group, imposing the course requirement (i.e. the absence of coursework relating to a high school diploma) has a much smaller effect.

As a further check on our results, we also construct samples of upgraders and comparisons based on the high school coursework variables. In addition to assessing the

[10]We also estimated an instrumental variables model with the full sample, using the responses to coursework questions as instrumental variables (IV). However, the first stage regressions had little power, and the resulting IV estimates were very imprecise. Another option is to use the consistent and inconsistent responses and explicitly model the response errors. We are currently investigating this approach as a complement to the analysis pursued in this paper.

sensitivity of our findings to alternative specifications, this approach also reflects previous research on the consequences of human capital investments. In particular, many studies conclude that, on average, there are employment and earnings benefits associated with each incremental investment in human capital such as additional coursework or years of schooling (Card, 1999; Oreopoulos, 2003). This is so even when these additional investments do not result in completing an educational program (Kane & Rouse, 1995). This is not to claim that there is no premium associated with program completion. Evidence for both Canada and the US indicates that such *sheepskin effects* exist and are non-trivial in size (Ferrer & Riddell, 2002). Nonetheless, educational investments such as courses taken or years of schooling completed generally earn significant returns even when the relevant educational program is not completed. For this reason we do not wish to restrict our measures of upgrading to those in which the individual received a diploma or degree. Accordingly, we create a third sample of changes in secondary schooling based on courses taken. The comparison group consists of those who consistently responded "No" to the questions regarding high school completion and who reported not taking any high school courses. The upgraders are those who were high school dropouts at baseline and who reported taking secondary school courses (whether or not they subsequently graduated from high school). This sample results in 522 upgraders and 1,441 comparisons (the identical comparison group to our most restrictive sample). The reason for the increase in the number of upgraders (relative to the 436 in the less restrictive sample above) is that we do not impose consistency in the education attainment questions when we use the coursework questions alone. Taken together, the three samples cover all the bases, with one sample using the attainment questions alone to classify upgraders, one sample using the coursework questions to classify upgraders, and one sample — our most restrictive — using both.

Tables 4 and 5 show equivalent information for university and college/trade school programs. In these cases, we can examine both changes in degree/diploma status and in enrolment status. The latter may be a more appropriate measure given the time frame involved — particularly for university. Thus, under the "Attainment/Enrolment Questions" column we report the number of responses of the type indicated to both the graduation and enrolment questions.

Again, the data show evidence of considerable upgrading, with almost 50 individuals receiving a university degree and about 70 having entered university during the period. Table 5 suggests that a substantial number of welfare recipients pursue the non-university post-secondary route, with about 440 reporting the completion of a college or trade school program and over 370 enrolling during the period. Remarkably, over 40 per cent of those without a college diploma or trades certificate at baseline reported that they had completed a non-university post-secondary program by the end of the period. Over 20 per cent of those who had never enrolled in a college or trade school program at baseline reported that they did so during the SSP demonstration.

All three tables suggest that there is a moderate to substantial amount of mismeasurement of educational activity and attainment, despite the absence of proxy responses in the SSP surveys. One indicator of the extent of measurement error is the number of inconsistent responses as a proportion of total responses. By this indicator, university graduation exhibits the least error, with less than one per cent of total responses being inconsistent, followed by high school completion (7.6 per cent) and graduation from college or trade school (24 per cent). However, this indicator is potentially misleading in the case of university education, since the vast majority of respondents reported no activity over the period. For example, in the case of university, there are almost as many inconsistencies in degree holding as there are upgraders (41 versus 46) and triple the number of inconsistencies in enrolment relative to upgrading. Nonetheless, this indicator does suggest that the mismeasurement of college and trade school completion is particularly severe, with 24 per cent of the sample reporting inconsistencies. For college/trades, there are even 270 individuals who answered either "Yes/No/Yes/No" or "No/Yes/No/Yes" for completion or enrolment.

Table 4: Education Upgrading — University (*N* = 4,371)

Possible Survey Responses				Number of Cases				
				Attainment/ Enrolment Questions		Coursework Questions		
						University Courses	University Courses	University Courses
Baseline	18 Month	36 Month	54 Month	Degree Conferred?	Enrolled?	Baseline to 18 Month	18 Month to 36 Month	36 Month to 54 Month
Consistent responses								
No upgrading								
No	No	No	No	4,144	3,757	24	39	29
Yes	Yes	Yes	Yes	38	212	1	2	1
			Total	*4,182*	*3,969*	*25*	*41*	*30*
Upgrading								
No	No	No	Yes	12	32	6	11	8
No	No	Yes	Yes	17	16	9	11	6
No	Yes	Yes	Yes	17	23	10	5	1
			Total	*46*	*71*	*25*	*27*	*15*
Inconsistent responses								
Majority cases								
Yes	No	No	No	7	48	0	0	0
Yes	Yes	Yes	No	3	40	1	1	0
Yes	Yes	No	Yes	8	18	1	0	0
Yes	No	Yes	Yes	5	12	0	0	0
No	No	Yes	No	6	22	1	0	0
No	Yes	No	No	2	29	1	1	0
			Total	*31*	*169*	*4*	*2*	*0*
Non-majority cases								
Yes	Yes	No	No	1	20	0	0	0
Yes	No	Yes	No	0	11	0	0	0
Yes	No	No	Yes	3	12	0	2	0
No	Yes	Yes	No	1	11	1	1	1
No	Yes	No	Yes	5	6	2	2	2
			Total	*10*	*60*	*3*	*5*	*3*
Missing observations								
			Total	103	103	0	1	1

Table 5: Education Upgrading — College or Trades (*N* = 4,371)

Possible Survey Responses				Number of Cases: Attainment/ Enrolment Questions		Coursework Questions		
Baseline	18 Month	36 Month	54 Month	Diploma Conferred?	Enrolled?	College Courses Baseline to 18 Month	College Courses 18 Month to 36 Month	College Courses 36 Month to 54 Month
Consistent responses								
No upgrading								
No	No	No	No	586	1,352	25	133	144
Yes	Yes	Yes	Yes	2,178	1,121	31	104	88
			Total	*2,764*	*2,473*	*56*	*237*	*232*
Upgrading								
No	No	No	Yes	190	129	11	72	76
No	No	Yes	Yes	105	75	17	60	25
No	Yes	Yes	Yes	147	168	15	40	19
			Total	*442*	*372*	*43*	*172*	*120*
Inconsistent responses								
Majority cases								
Yes	No	No	No	160	217	4	15	21
Yes	Yes	Yes	No	147	260	6	20	27
Yes	Yes	No	Yes	112	161	4	23	19
Yes	No	Yes	Yes	95	83	7	37	14
No	No	Yes	No	98	90	4	23	23
No	Yes	No	No	108	130	4	17	8
			Total	*720*	*941*	*29*	*135*	*112*
Non-majority cases								
Yes	Yes	No	No	110	201	2	16	14
Yes	No	Yes	No	69	80	0	14	11
Yes	No	No	Yes	53	68	3	15	13
No	Yes	Yes	No	53	77	2	7	10
No	Yes	No	Yes	56	63	4	15	17
			Total	*341*	*489*	*11*	*67*	*65*
Missing observations								
Total				104	96	1	10	9

The incidence of inconsistent reporting of enrolment is greater than that of program completion, perhaps in part due to the previously discussed changes in the enrolment questions. For university enrolment, over five per cent of the sample gave inconsistent responses. At the college and trade school level, fully one third of responses were inconsistent.

The assessment of the extent of measurement error in the case of university programs is somewhat academic in nature because the number of individuals acquiring university education is too small to permit analysis. However, there are clearly a substantial number of welfare recipients and welfare leavers attending community college or trade school programs, as well as evidently a significant amount of mismeasurement of this activity. For this type of education we proceed in a fashion similar to that for secondary school. The principal difference in this case arises from the fact that we have information on program completion and enrolment as well as courses taken. To avoid presenting a proliferation of results on non-university post-secondary schooling, we focus on enrolment from the educational attainment and enrolment questions, and courses taken from the coursework questions. As in the case of secondary school, we begin by dropping from the sample the individuals with inconsistent responses to the relevant questions. In the case of enrolment, the comparison group of non-upgraders are those individuals who consistently answered "No" to the "Ever enrolled?" question. The upgraders are individuals who also responded "No" to the "Ever enrolled?" question at baseline, but subsequently reported that they had enrolled in a college or trade school. As before, we then create a more restrictive sample of upgraders in which the enrolment responses are recognized only when confirmed by responses to the questions about courses taken. Similarly, we restrict the comparisons to individuals who reported that they had never enrolled and that they had not taken courses from a college or trade school.

Using coursework information, we also construct two analysis samples that distinguish between courses taken at trade schools and community colleges. These are analogous to the high school sample based on secondary school courses rather than graduation. In the trade school sample, an upgrader is an individual who responded "No" to the "Ever enrolled?" question at baseline but reported having subsequently taken a trade school course. The comparison group is the same as the restrictive sample — those who reported never taking a trade school course. The second sample is the equivalent set of individuals based on responses to the question regarding courses taken from a community college. These samples allow us to examine whether community college and trade school programs play distinct roles within the broad and heterogeneous non-university post-secondary category.

THE IMPACT OF THE SSP EARNINGS SUPPLEMENT OFFER ON EDUCATIONAL ATTAINMENT

We now examine whether, after taking steps to minimize the extent of measurement error, there were differences between the program and control groups in acquiring additional schooling during the 54 months of the SSP demonstration. We focus on the two forms of educational change that are most prevalent among single parents on welfare: high school completion and enrolment in a program at a community college or trade school. Corresponding to these options, Table 6 shows the educational investments made by two groups of potential upgraders. For both groups we use only those who provided responses to the education-related questions that were consistent over time. The upper part of the table shows the evolution of high school completion among those who had not graduated from secondary school at baseline.[11] The bottom part of the table shows enrolment in college or trade school for those who reported at baseline that they had never enrolled.[12] These two examples of educational change illustrate behaviour that is examined for a wider range of groups in tables 7 and 8.

[11]In Table 4 these are the 436 individuals who upgraded to high school completion and the 1,688 individuals who did not complete secondary school, giving a total of 2,124 observations.

[12]In Table 6 these are the 372 individuals who enrolled for the first time during the SSP period and the 1,352 individuals who never enrolled, for a total of 1,724 observations.

Table 6: Changes in Educational Attainment Among SSP Participants

Education Type	Program Group	Control Group	Difference
High school dropout to high school graduate (*N* = 2,124)			
Baseline	0.0	0.0	
	(0.0)	(0.0)	—
18 month	0.070	0.088	0.018
	(0.008)	(0.008)	(0.011)
36 month	0.116	0.148	0.032
	(0.010)	(0.011)	(0.014)
54 month	0.182	0.228	0.046
	(0.011)	(0.012)	(0.017)
Enrolled in college or trade school (*N* = 1,724)			
Baseline	0.0	0.0	
	(0.0)	(0.0)	—
18 month	0.090	0.105	0.015
	(0.010)	(0.010)	(0.014)
36 month	0.133	0.150	0.017
	(0.011)	(0.012)	(0.016)
54 month	0.198	0.235	0.037
	(0.013)	(0.014)	(0.018)

Note: Both samples are restricted to those without the relevant education or enrolment at baseline and who provided consistent responses to the education-related questions.

For each of these groups, there is evidence of a substantial amount of educational change during the period. Among those who had not completed secondary school at the beginning of the demonstration, approximately 20 per cent graduated from high school by the end. In each interval between surveys, members of the control group exhibited a greater increase in high school completion than their counterparts in the program group. By the end of the SSP demonstration, high school completion had increased by 22.8 percentage points among the potential upgraders in the control group versus 18.2 percentage points in the program group, a differential of 4.6 percentage points.

The growth in educational activity at the college and trade school level is similar. Approximately one fifth of those who had never enrolled in college or trade school did so by the end of the SSP demonstration. In each interval between surveys, control group members displayed a greater propensity to engage in this kind of educational activity than did members of the program group. By the end of the period, college/trades enrolment had risen by 3.7 percentage points more in the control group than in the program group.

Of course, we cannot presume that the members of the program and control groups were randomly assigned to these subsets of the total SSP sample. Nonetheless, it is noteworthy that the average baseline characteristics of the two groups are very similar and are virtually never significantly different from each other, as would be the case if random assignment continued to hold. This similarity is evident in Table 7, which presents summary statistics for a selection of baseline characteristics used throughout our analysis using three high school samples discussed previously.[13] Sample 1, which deals with measurement error by dropping individuals with inconsistent responses, corresponds to the sample used in the upper part of Table 6. Sample 2 is the more restrictive version of the high school completion sample, and

[13]This similarity is also the case for other characteristics used in the analysis but is omitted from Table 7 for brevity.

Sample 3 is the sample based on reported coursework rather than program completion. In all three of these samples, the mean baseline characteristics of the program and control groups are very similar and rarely significantly different from each other.

Table 7: Baseline Summary Statistics — High School Upgrading Samples

	(1) Upgraders: Attainment Variable Comparison: Attainment Variable		(2) Upgraders: Attainment and Coursework Variables Comparison: Attainment and Coursework Variables		(3) Upgraders: Coursework Variables Comparison: Attainment and Coursework Variables	
Variable	**Program Group**	**Control Group**	**Program Group**	**Control Group**	**Program Group**	**Control Group**
Upgraded to high school	0.182	0.228	0.072	0.128	0.229	0.302
	(0.011)	(0.012)	(0.009)	(0.011)	(0.013)	(0.014)
British Columbia	0.482	0.506	.460	.473	0.462	0.480
	(0.016)	(0.015)	(0.018)	(0.018)	(0.016)	(0.016)
Female	0.948	0.953	0.951	0.947	0.951	0.955
	(0.007)	(0.006)	(0.008)	(0.008)	(0.007)	(0.007)
Age	31.79	31.51	32.30	32.19	31.93	31.48
	(0.256)	(0.260)	(0.291)	(0.308)	(0.264)	(0.270)
Single	0.516	0.492	0.491	0.498	0.504	0.503
	(0.015)	(0.015)	(0.018)	(0.018)	(0.016)	(0.016)
Married	0.020	0.023	0.024	0.024	0.023	0.023
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Separated/divorced/ widowed	0.464	0.485	0.485	0.478	0.473	0.474
	(0.015)	(0.015)	(0.018)	(0.018)	(0.016)	(0.016)
Working at baseline	0.161	0.152	0.156	0.157	0.154	0.147
	(0.011)	(0.011)	(0.014)	(0.013)	(0.012)	(0.011)
Likes to work	0.266	0.271	0.273	0.261	0.272	0.259
	(0.014)	(0.014)	(0.016)	(0.016)	(0.014)	(0.014)
Years pre-baseline work experience	6.45	6.49	6.48	6.53	6.30	6.27
	(0.193)	(0.197)	(0.221)	(0.233)	(0.198)	(0.201)
Number of children	1.72	1.73	1.74	1.75	1.73	1.74
	(0.027)	(0.028)	(0.031)	(0.033)	(0.028)	(0.029)
Age of youngest child	6.47	6.24	6.64	6.53	6.54	6.22
	(0.158)	(0.161)	(0.182)	(0.191)	(0.166)	(0.167)
IA receipt over 36 months pre-baseline	30.63	30.14	30.90	30.74	30.86	30.53
	(0.231)	(0.240)	(0.265)	(0.270)	(0.242)	(0.242)
Disability	0.204	0.216	0.210	0.223	0.204	0.213
	(0.012)	(0.013)	(0.014)	(0.015)	(0.013)	(0.013)
Born outside Canada	0.103	0.107	0.112	0.112	0.105	0.108
	(0.009)	(0.009)	(0.011)	(0.011)	(0.010)	(0.010)
Does not speak English	0.044	0.036	0.053	0.041	0.045	0.036
	(0.007)	(0.006)	(0.008)	(0.007)	(0.007)	(0.006)
Number of observations	1,050	1,074	802	797	966	997

Notes: Standard errors are in parentheses.
See text for discussion of the construction of the different samples.

Program/control group differences in educational activity are, however, evident in all three samples. Because of its stringent requirements, the more restrictive Sample 2 produces lower estimates of the extent of high school upgrading. However, the difference between program and control group members is even larger — 5.6 percentage points. Finally, although the scale is not comparable to samples 1 and 2, the coursework Sample 3 also produces a large and statistically significant gap between program and control group members in educational activity — in this case 7.3 percentage points. Thus all three samples, which deal with mismeasurement of education in different ways and capture different dimensions of human capital acquisition, suggest that the SSP financial incentive resulted in lower growth in education among the program group members than would otherwise have been the case.

Table 8 shows the equivalent average baseline characteristics and extent of educational upgrading for the four college and trade school samples. Again, note the high degree of similarity of the average baseline characteristics of the program and control groups. As was suggested in previous tables, there is evidence of a substantial amount of education-related activity in the community college and trade school categories. In the case of Sample 1, the behaviour of which was examined previously in Table 6, about one fifth of those who had never enrolled in college or trade school did so during the 54 months of the SSP demonstration. The coursework measures indicate a substantial amount of educational activity in both types of programs, with the rise in courses taken in trade school being particularly large. Differences between program and control group members are, however, less prominent at the non-university post-secondary level than was the case for secondary school. The base case Sample 1 yields a statistically significant differential of 3.7 percentage points, but the more restrictive sample indicates a smaller differential of 2.5 percentage points that is not significantly different from zero. Sample 3 shows that the program and control groups increased their coursework activity in trade school by equal (and large) amounts. However, as the means for the final sample (4) indicate, differences between program and control group members are observed in courses taken at community colleges. The gap in this measure of educational change is 4.7 percentage points and statistically significant.

In summary, our measures of educational upgrading at the secondary school and non-university post-secondary levels indicate that SSP participants made substantial additional investments in human capital during the demonstration. These measures also suggest that the SSP financial incentive resulted in less acquisition of additional schooling than would otherwise have been the case. The differential between the program and control groups is largest for high school activity, whether measured by enrolment or courses taken. With the exception of trade school coursework, the control group acquired additional non-university post-secondary education to a greater extent than did the program group, although the differences in behaviour between the two groups are not always statistically significant. We now examine whether these differences between program and control group members continue to be observed when we control for other influences on educational choices.

Table 8: Baseline Summary Statistics — College and Trades Upgrading Samples

Variable	(1) Upgraders: Enrolment Variables Comparison: Enrolment Variables		(2) Upgraders: Enrolment and Coursework Variables Comparison: Enrolment and Coursework Variables		(3) Upgraders: Coursework Variables/Trades Only Comparison: Enrolment and Coursework Variables		(4) Upgraders: Coursework Variables/College Only Comparison: Enrolment and Coursework Variables	
	Program Group	Control Group	Program Group	Control Group	Program Group	Control Group	Program Group	Control Group
Upgraded to college or trade school	0.198	0.235	0.111	0.136	0.372	0.372	0.187	0.234
	(0.013)	(0.014)	(0.011)	(0.012)	(0.015)	(0.015)	(0.013)	(0.014)
British Columbia	0.410	0.407	0.391	0.393	0.486	0.482	0.431	0.435
	(0.017)	(0.017)	(0.017)	(0.018)	(0.015)	(0.016)	(0.017)	(0.017)
Female	0.957	0.957	0.956	0.954	0.960	0.955	0.958	0.958
	(0.007)	(0.007)	(0.007)	(0.008)	(0.006)	(0.007)	(0.007)	(0.007)
Age	30.94	31.46	31.10	31.65	31.31	31.63	31.32	31.62
	(0.283)	(0.308)	(0.302)	(0.332)	(0.252)	(0.277)	(0.287)	(0.306)
Single	0.516	0.512	0.509	0.516	0.487	0.509	0.498	0.501
	(0.017)	(0.017)	(0.018)	(0.018)	(0.015)	(0.016)	(0.017)	(0.014)
Married	0.016	0.023	0.018	0.023	0.019	0.026	0.019	0.021
	(0.004)	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)
Separated/ divorced/widowed	0.468	0.465	0.473	0.462	0.494	0.468	0.483	0.479
	(0.017)	(0.017)	(0.018)	(0.018)	(0.015)	(0.016)	(0.017)	(0.017)
Working at baseline	0.169	0.179	0.171	0.179	0.189	0.186	0.175	0.185
	(0.013)	(0.013)	(0.013)	(0.014)	(0.012)	(0.013)	(0.013)	(0.014)
Likes to work	0.273	0.258	0.271	0.256	0.301	0.291	0.281	0.276
	(0.015)	(0.015)	(0.016)	(0.016)	(0.014)	(0.015)	(0.015)	(0.016)
Years pre-baseline work experience	6.19	6.59	6.26	6.64	6.74	7.18	6.47	6.80
	(0.207)	(0.217)	(0.219)	(0.234)	(0.194)	(0.208)	(0.213)	(0.221)
Number of children	1.68	1.69	1.67	1.71	1.67	1.66	1.67	1.67
	(0.029)	(0.031)	(0.030)	(0.034)	(0.026)	(0.028)	(0.029)	(0.031)
Age of youngest child	5.88	5.95	6.00	6.03	6.06	6.04	6.04	6.07
	(0.169)	(0.192)	(0.180)	(0.206)	(0.154)	(0.174)	(0.173)	(0.191)
IA receipt pre-baseline	30.04	29.64	30.04	29.79	29.88	29.53	30.05	29.55
	(0.266)	(0.283)	(0.282)	(0.297)	(0.244)	(0.261)	(0.267)	(0.284)
Disability	0.182	0.196	0.187	0.197	0.182	0.194	0.193	0.199
	(0.013)	(0.014)	(0.014)	(0.015)	(0.012)	(0.013)	(0.013)	(0.014)
Born outside Canada	0.122	0.132	0.124	0.131	0.133	0.140	0.133	0.141
	(0.011)	(0.012)	(0.012)	(0.012)	(0.011)	(0.011)	(0.012)	(0.012)
Does not speak English	0.043	0.043	0.048	0.048	0.036	0.039	0.042	0.043
	(0.007)	(0.007)	(0.008)	(0.008)	(0.006)	(0.006)	(0.007)	(0.007)
Number of observations	886	838	741	796	1,066	977	868	827

Notes: Standard errors are in parentheses.
See text for discussion of the construction of the different samples.

DETERMINANTS OF EDUCATIONAL UPGRADING

We now turn to a regression analysis of the determinants of upgrading. Tables 9 and 10 present the estimated marginal effects from probit models of educational change. The dependent variable equals 1 if the individual upgraded and 0 if he or she did not. We include as covariates a variety of individual and demographic characteristics, such as age, gender, marital status, number and age of children, immigrant status, disability status, and language proficiency. Also included are controls for work experience at the time of the baseline survey, receipt of IA during the previous three years, and attitudes toward work. In the case of secondary school (Table 9), we continue to find that members of the SSP control group were more likely to upgrade to a high school diploma relative to the program group members. However, the estimated magnitudes of the differences between program and control group members are somewhat smaller when we control for other influences on education. For example, in Sample 2 the differential is 5.0 percentage points, compared with the mean percentage point gap of 5.6 in Table 7.

Table 9: Estimated Marginal Effects for the Determinants of High School Upgrading

Variable	(1) Upgraders: Attainment Variable Comparison: Attainment Variable	(2) Upgraders: Attainment and Coursework Variables Comparison: Attainment and Coursework Variables	(3) Upgraders: Coursework Variables Comparison: Attainment and Coursework Variables
SSP control group	0.037** (0.017)	0.050*** (0.014)	0.068*** (0.014)
British Columbia	0.071*** (0.018)	0.004 (0.015)	0.021 (0.021)
Female	0.022 (0.040)	0.045 (0.028)	0.071 (0.048)
Age: 25 to 29	-0.046* (0.025)	-0.047*** (0.015)	-0.086*** (0.028)
Age: 30 to 39	-0.125*** (0.029)	-0.12*** (0.024)	-0.191*** (0.033)
Age: 40+	-0.136*** (0.027)	-0.099*** (0.014)	-0.222*** (0.026)
Married	-0.092* (0.043)	-0.052 (0.027)	-0.077 (0.058)
Separated/divorced/ widowed	0.009 (0.021)	0.009 (0.017)	0.041 (0.026)
Working at baseline	0.007 (0.024)	-0.017 (0.021)	-0.039 (0.028)
Likes to work	0.023 (0.020)	0.001 (0.016)	-0.006 (0.027)
Years pre-baseline work experience	0.012*** (0.004)	0.001 (0.003)	-0.001 (0.005)
Experience squared (*10)	-0.001 (0.001)	0.000 (0.000)	0.001 (0.002)

(continued)

Table 9: Estimated Marginal Effects for the Determinants of High School Upgrading (Cont'd)

Variable	(1) Upgraders: Attainment Variable Comparison: Attainment Variable	(2) Upgraders: Attainment and Coursework Variables Comparison: Attainment and Coursework Variables	(3) Upgraders: Coursework Variables Comparison: Attainment and Coursework Variables
One child	0.003	-0.008	0.008
	(0.026)	(0.023)	(0.031)
Two children	-0.017	0.006	-0.001
	(0.026)	(0.020)	(0.031)
Youngest child under 5 years	0.015	0.004	-0.011
	(0.023)	(0.020)	(0.027)
IA receipt previous 36 months: 24–35 months	0.004	-0.005	-0.006
	(0.024)	(0.021)	(0.027)
IA receipt previous 36 months: 36 months	-0.063***	-0.041**	-0.044*
	(0.024)	(0.020)	(0.027)
Disability	-0.021	-0.042**	-0.046*
	(0.022)	(0.016)	(0.024)
Born outside Canada	-0.012	-0.049**	-0.061*
	(0.032)	(0.019)	(0.034)
Does not speak English	-0.118***	a	-0.197***
	(0.034)		(0.034)
Log likelihood	-1,009.69	-447.16	-1,057.26
Number of observations	2,124	1,474	1,963

Notes: Standard errors are in parentheses.

Statistical significance is denoted by *** for 1 per cent level, ** for 5 per cent, and * for 10 per cent.

See text for discussion of the construction of the samples and dependent variables. In all cases, the dependent variable indicates upgrading to high school. The means of the dependent variables are as follows: (1) 0.205; (2) 0.100; (3) 0.266.

a: All 74 individuals in Sample 2 who indicated that they did not speak English equalled 0 for the dependent variable, and thus no effect can be identified.

Table 10: Estimated Marginal Effects for the Determinants of College and Trades Upgrading

Variable	(1) Upgraders: Enrolment Variables Comparison: Enrolment Variables	(2) Upgraders: Enrolment and Coursework Variables Comparison: Enrolment and Coursework Variables	(3) Upgraders: Coursework Variables/Trades Only Comparison: Enrolment and Coursework Variables	(4) Upgraders: Coursework Variables/College Only Comparison: Enrolment and Coursework Variables
SSP control group	0.033*	0.021	-0.001	0.048**
	(0.018)	(0.015)	(0.023)	(0.018)
British Columbia	0.144***	0.093***	0.354***	0.231***
	(0.022)	(0.019)	(0.023)	(0.023)
Female	0.028	0.014	0.080	0.088*
	(0.049)	(0.038)	(0.054)	(0.041)
Age: 25 to 29	-0.095***	-0.076***	-0.084***	-0.064**
	(0.024)	(0.015)	(0.034)	(0.027)

(continued)

Table 10: Estimated Marginal Effects for the Determinants of College and Trades Upgrading (Cont'd)

Variable	(1) Upgraders: Enrolment Variables Comparison: Enrolment Variables	(2) Upgraders: Enrolment and Coursework Variables Comparison: Enrolment and Coursework Variables	(3) Upgraders: Coursework Variables/Trades Only Comparison: Enrolment and Coursework Variables	(4) Upgraders: Coursework Variables/College Only Comparison: Enrolment and Coursework Variables
Age: 30 to 39	-0.132***	-0.103***	-0.138***	-0.128***
	(0.032)	(0.024)	(0.041)	(0.033)
Age: 40+	-0.104***	-0.083***	-0.198***	-0.124***
	(0.035)	(0.020)	(0.045)	(0.032)
Married	0.019	0.043	0.081	0.093
	(0.071)	(0.059)	(0.080)	(0.082)
Separated/divorced/ widowed	-0.009	-0.021	0.025	0.005
	(0.024)	(0.019)	(0.028)	(0.025)
Working at baseline	0.030	0.042**	0.040	0.042*
	(0.026)	(0.022)	(0.030)	(0.027)
Likes to work	0.075***	0.054***	0.164***	0.132***
	(0.024)	(0.019)	(0.027)	(0.025)
Years pre-baseline work experience	0.012***	0.013***	0.024***	0.017***
	(0.005)	(0.005)	(0.006)	(0.005)
Experience squared (*10)	-0.004*	-0.005**	-0.005***	-0.005**
	(0.002)	(0.002)	(0.002)	(0.002)
One child	-0.035	-0.032	0.080**	0.057*
	(0.030)	(0.025)	(0.037)	(0.034)
Two children	-0.039	-0.033	0.065*	0.036
	(0.029)	(0.022)	(0.037)	(0.035)
Youngest child under five years	0.039	0.012	0.023	-0.000
	(0.026)	(0.022)	(0.030)	(0.025)
IA receipt previous 36 months: 24–35 months	0.018	0.023	0.002	0.024
	(0.026)	(0.021)	(0.030)	(0.026)
IA receipt previous 36 months: 36 months	-0.026	-0.029	-0.026	-0.023
	(0.026)	(0.021)	(0.030)	(0.026)
Disability	-0.058**	-0.059***	-0.088***	-0.032
	(0.024)	(0.017)	(0.028)	(0.023)
Born outside Canada	-0.061**	-0.076***	-0.092**	-0.034
	(0.028)	(0.016)	(0.033)	(0.028)
Does not speak English	-0.171***	-0.042	-0.299***	a
	(0.024)	(0.039)	(0.032)	
Log likelihood	-812.77	-492.03	-1,130.85	-736.30
Number of observations	1,724	1,536	2,042	1,618

Notes: Standard errors are in parentheses.

Statistical significance is denoted by *** for 1 per cent level, ** for 5 per cent, and * for 10 per cent.

See text for discussion of the construction of the samples and dependent variables. In all cases, the dependent variable indicates upgrading to some type of college or trades schooling. The means of the dependent variables are as follows: (1) 0.216; (2) 0.123; (3) 0.372; (4) 0.209.

a: All 72 individuals in Sample 2 who indicated that they did not speak English equalled 0 for the dependent variable, and thus no effect can be identified.

The impacts of other factors on high school completion are largely as anticipated. Age exerts considerable influence. Younger dropouts were more likely to complete secondary school than their older counterparts. Single parents with a long history of reliance on IA prior to baseline and those who do not speak English were less likely to upgrade to a high school diploma. Pre-baseline work experience and living in British Columbia (BC) are positively associated with the probability of upgrading in the less restrictive Sample 1. With the exception of these latter two covariates, the results for high school upgrading are fairly consistent across the three samples.

Control group members were also more likely to enrol in a college or trades program, but only in the less restrictive of the two enrolment samples. Similarly, control group members took significantly more college courses than their counterparts in the program group but identical amounts of trade school courses. The differences between program and control group members in Table 10 are very similar to the mean differences shown in Table 8, when we did not control for other influences. This result is consistent with SSP participants being "as good as randomly assigned" to the program and control groups in the sub-samples used.

The results for enrolling in a college or trade school are broadly similar to those for high school. Older individuals were less likely to upgrade, while there was substantially more upgrading in BC than in New Brunswick. Individuals with higher labour force attachment — based on pre-baseline work experience — were more likely to enrol in a college or trades program. Attitudes toward work also exerted considerable influence. Finally, there is evidence — in both the high school and college/trades cases — that individuals who might face a learning disadvantage in the education system were less likely to upgrade. This group includes individuals with a disability, immigrants, and those who do not speak English. The latter results are systematically stronger in the case of college and trades programs.

For completeness, we also carried out an equivalent analysis of the determinants of work-related courses. There are no differences between program and control group members in the extent of such coursework. The other influences are similar to those that affect the likelihood of investing in formal education, as reported in tables 9 and 10.

LEGISLATIVE CHANGES AND EDUCATIONAL UPGRADING

During the 1990s, most Canadian provinces made significant reforms to their IA programs. In this section we examine whether the changes introduced in BC and New Brunswick could account for the finding that members of the SSP control group increased their educational attainment during the SSP period to a greater extent than did the program group.

Both provinces carried out welfare reforms that encouraged work and skills upgrading. These changes were principally directed at youths, single adults, and families without children. Nonetheless, single parents with children may also have been affected to some degree by these reforms. The new policies could have exerted more influence on the control group than on the program group because at the time these reforms were introduced, a larger proportion of the program group had already exited welfare in response to the SSP supplement offer.

The nature of the policy changes were as follows. In May 1995 New Brunswick passed the *Family and Income Security Act* which provided incentives for IA recipients to improve their education and skills. The inducements included generous subsidies for the education itself and subsidies for child care for those enrolled in schooling. However, welfare recipients were not compelled to engage in educational activities. In January 1996 BC introduced welfare-to-work programs that contained strong incentives to either work or enrol in skills upgrading, including high school completion. Under these new policies, which were eventually embodied in the *BC Benefits (Youth Works) Act* and the *BC Benefits (Income Assistance) Act*, IA recipients — with exemptions for some groups — were required to either find work or participate in skills upgrading activities in order to remain eligible for benefits. As part of this package, fully subsidized skills and education upgrading was provided by the province, although the number of available slots was limited and preference was given to those aged 24 and under. Other BC legislation passed around the same time — including the *Family Bonus Act*, *Healthy Kids Act*, and *Child Care Act* — created softer incentives by providing various benefits only to those either working or engaging in skills or educational upgrading. These incentives included subsidies to child care (*Child Care Act*), monetary benefits (*Family Bonus Act*), and health-care benefits (*Healthy Kids Act*).

These policy changes — especially those in BC — may have encouraged welfare recipients to acquire additional schooling. It is also possible that they may have exerted more influence on the SSP control group than on the program group. Random assignment ensured that the program and control groups had very similar characteristics at baseline. But after the supplement offer had been made, the characteristics of the two groups were no longer the same. In particular, following random assignment there was a significantly greater exit from IA among members of the program group than among those in the control group. Because the new policies affected those remaining on welfare, their impact may have been greater on the control group, which had a larger proportion of members still receiving IA.

To investigate the role of legislation, we take advantage of the fact that there were 22 different baseline interview dates, and different SSP participants became covered by the new legislation at different times relative to their date of random assignment. We therefore examine upgrading activity in the time intervals between each survey — that is, between the baseline and 18-month surveys, between the 18-month and 36-month surveys (conditional on not having upgraded prior to the 18-month survey), and so on. To test the role of legislation, we introduce an indicator variable for whether the individual became covered by the new policies (January 1996 in BC and May 1995 in New Brunswick) during the relevant interval between surveys. To illustrate, consider an individual in BC with a baseline interview date of January 1993. Between the baseline (January 1993) and 18-month point (July 1994), this individual was not affected by the January 1996 policy initiatives. Conversely, an individual in BC with a January 1995 baseline interview date would have been affected by the January 1996 legislation during the baseline to 18-month survey window. We then estimate educational upgrading regressions similar to those reported in tables 9 and 10, but where the dependent variable equals 1 for upgrading between the baseline and 18-month surveys only. The dummy variable indicating whether the individual was affected by the legislative change during that interval is added to the previous specification. If the legislation increased upgrading, and disproportionately affected the control group, this new "legislative coverage" variable should be positive and the estimated coefficient associated with the control group dummy should decrease. We then estimate the equivalent model for the 18-month to 36-month interval — conditional on not having upgraded to that point.[14]

In the case of high school completion (where the legislation is likely to have had the greatest impact, at least in BC), we allow for a six-month lag to complete the program requirements, since our upgrading variable measures high school graduation, rather than enrolling. We also carry out various robustness checks on this six-month lag definition (ranging from no lag to a one-year lag). The choice of time lag makes no difference to the findings.

Table 11 presents the results for the high school case for our two main samples (samples 1 and 2). The results indicate that the legislative changes did not lead to an increase in education upgrading. Furthermore, the previous finding that the control group was more likely to upgrade than the treatment group remains intact. Finally, although not shown, the same conclusion holds for college and trades enrolment, where there is also no evidence of an impact of the policy reforms.[15]

Table 11: Determinants of Upgrading to a High School Diploma — The Role of Legislation

Variable	Sample 1 — Less Restrictive Sample			
	Upgraded Between Baseline and 18 Months		Upgraded Between 18 Months and 36 Months: BC Only	
Control group	0.016	0.016	0.022	0.022
	(0.011)	(0.011)	(0.014)	(0.014)
Covered by change in law	-	-0.013	-	-0.002
		(0.016)		(0.025)
	Sample 2 — More Restrictive Sample			
	Upgraded Between Baseline and 18 Months		**Upgraded Between 18 Months and 36 Months: BC Only**	
Control group	0.018	0.018	0.010	0.010
	(0.008)	(0.008)	(0.010)	(0.010)
Covered by change in law	-	-0.011	-	0.004
		(0.011)		(0.019)

Notes: Standard errors are in parentheses.
Regressions include the same set of covariates as those reported in tables 9 and 10.

In interpreting these results, it is worthwhile noting that given the distribution of baseline dates and legislation dates, the 18- to 36-month window period is entirely identified from the policy changes in British Columbia (i.e. by the 18-month survey date, all participants in New Brunswick are affected by the legislation). Furthermore, the baseline to 18-month window is principally identified from the policy changes in New Brunswick — there are only 24 BC observations equal to 1 during this period. Thus the power of the test is limited by the fact that the baseline interview dates are not evenly distributed among the three time intervals in each province.

[14] There were no SSP participants who experienced these changes after the 36-month survey.

[15] For the analysis of upgrading between 18 and 36 months, we also allowed for a lagged response to the legislation; that is, the "legislative coverage" variable equals 1 if the individual was affected by the change in the period between the baseline and the 36-month survey. Doing so does not alter the results.

In summary, in the mid-1990s both BC and New Brunswick introduced changes to their IA programs that put greater emphasis on education and skills upgrading. Although the reforms were not directed specifically at single parents with children, they may nonetheless have influenced the behaviour of some SSP participants. We are able to test for such an influence by using the fact that the date of random assignment varied across SSP participants, so that some groups were affected by the legislation during different time periods (relative to random assignment) than were others. Our tests find no evidence of any impact of these policy changes on educational upgrading behaviour. However, the power of the tests is somewhat limited by the distribution of dates of random assignment relative to the timing of the policy changes. Thus we find no clear evidence that these legislative initiatives influenced educational behaviour, but we also cannot rule out some such influence.

The Consequences of Education Upgrading

We now investigate some outcomes of education upgrading. In order to avoid a proliferation of results, we use two of our previous samples in this section of the analysis. Sample 1 is the less restrictive case where we include all individuals other than those who gave inconsistent responses to the enrolment and educational attainment questions. Sample 2 is the more restrictive case where we require not only responses that are logically consistent across surveys, but we also require corroborating evidence from responses to the coursework questions. Our analysis focuses on three outcomes: employment, wages, and income assistance (IA) receipt.

Figures 1 through 6 present the evolution of employment rates, hourly wages, and IA receipt over the 54 months of the Self-Sufficiency Project (SSP) demonstration.[16] The pre-baseline data (12 months for employment and wages and 36 months for IA receipt) is also shown. The employment and IA receipt graphs contain the full upgrading samples used in the earlier analysis (for instance, $N = 2{,}124$ in the case of high school, Sample 1). The wage rate graphs, of course, are conditional on working, so the composition of the sample changes over time. It is worth emphasizing that, relative to previous studies of wage growth in the SSP demonstration such as Card, Michalopoulos, and Robins (2001), Connolly and Gottschalk (2002), Card and Hyslop (2005), and Riddell and Riddell (2004), we are left with few observations to work with in the case of wages. This is due to the low employment rates of individuals with limited educational attainment.

Beginning with the evolution of wages and employment (figures 1 to 4), the data indicates that upgraders experienced much larger wage gains than did long-term welfare recipients who did not upgrade. This is true for both high school completion and college/trades enrolment. For instance, hourly wages at the 54-month point were about \$2.50/hour greater for high school upgraders and \$1.80/hour higher for college and trades upgraders. For employment, a similar story emerges for high school completion, with upgraders being about 17 percentage points more likely to be employed by the end of the SSP period. For college/trade school enrolment, there is little action in employment rates.

Figures 5 and 6 illustrate that those who upgraded their education left IA at a more rapid rate than those who did not increase their educational attainment. Note, however, that those who subsequently upgraded their education were less likely to receive welfare prior to the beginning of the SSP demonstration. These differences in subsequent behaviour suggest that among the set of potential upgraders — high school dropouts in the case of Figure 5 — those who did increase their formal education had characteristics that made them less likely to rely on IA prior to the beginning of the SSP experiment. In order to isolate the impact of educational investments on IA receipt and other outcomes, we need to control for these characteristics.

[16]These plots use the less restrictive samples; the behaviour of the more restrictive samples is similar.

Figure 1: Employment Rates, High School Upgrading Sample 1

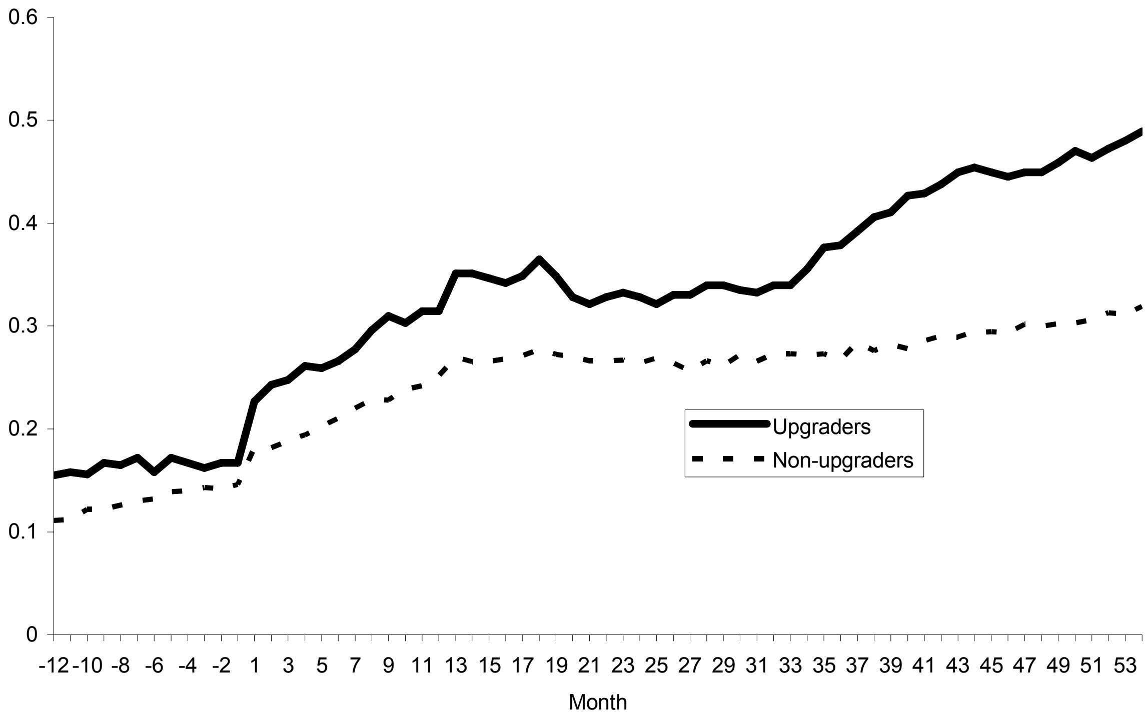


Figure 2: Hourly Wages, High School Upgrading Sample 1

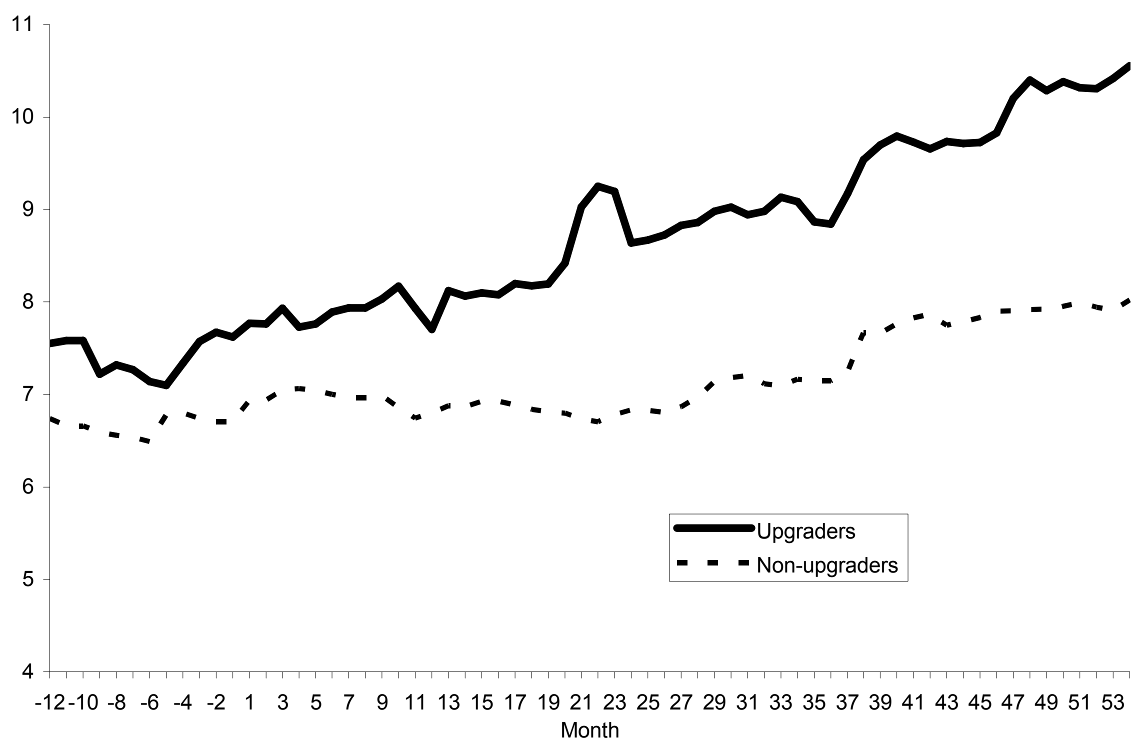


Figure 3: Employment Rates, College Upgrading Sample 1

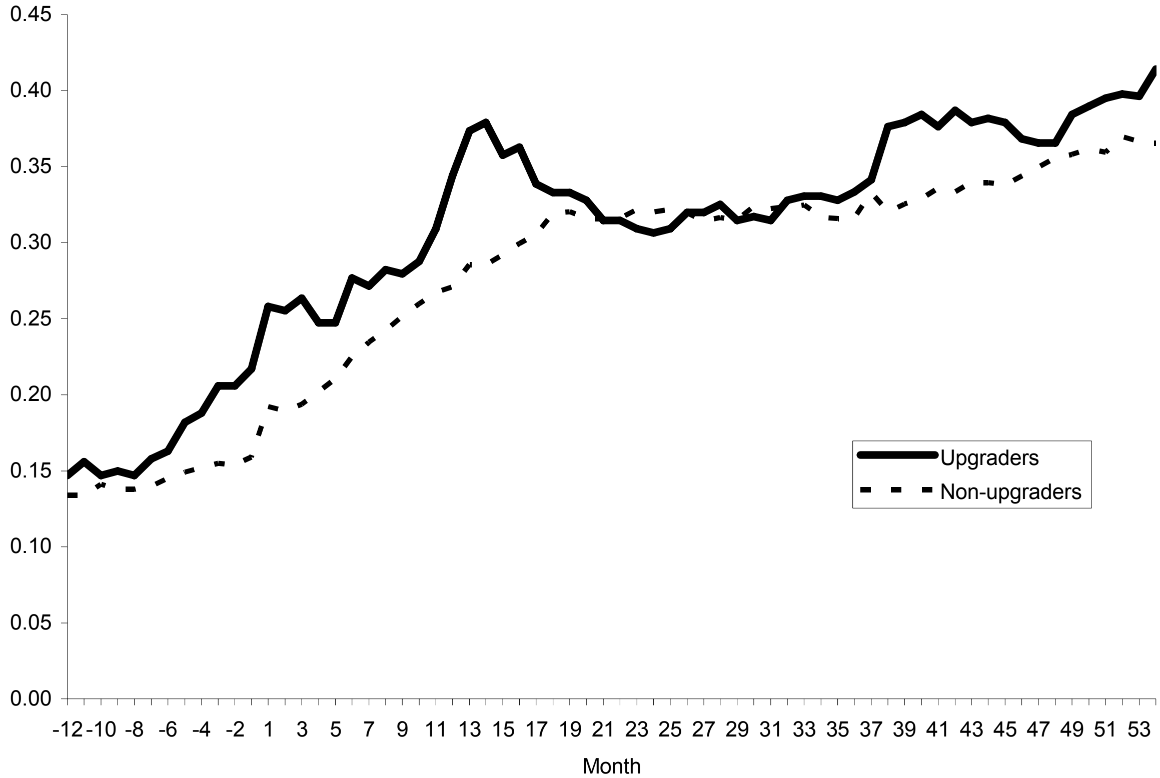


Figure 4: Hourly Wages, College Upgrading Sample 1

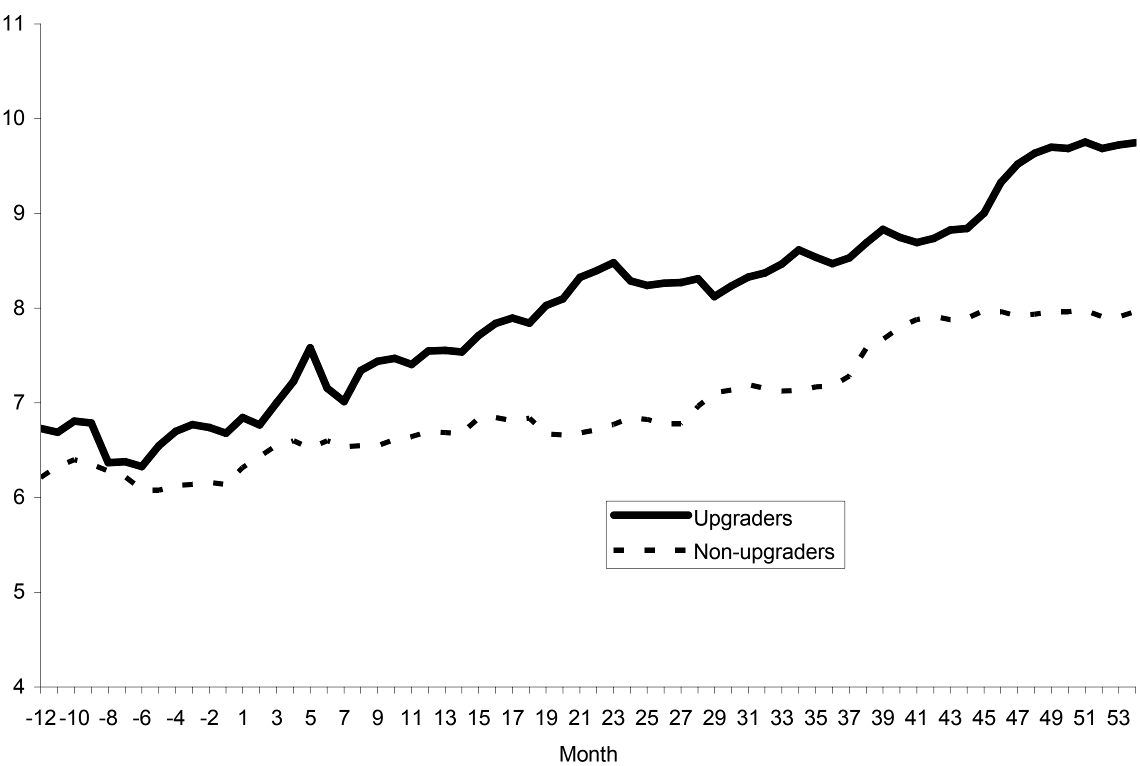


Figure 5: Income Assistance, High School Sample 1

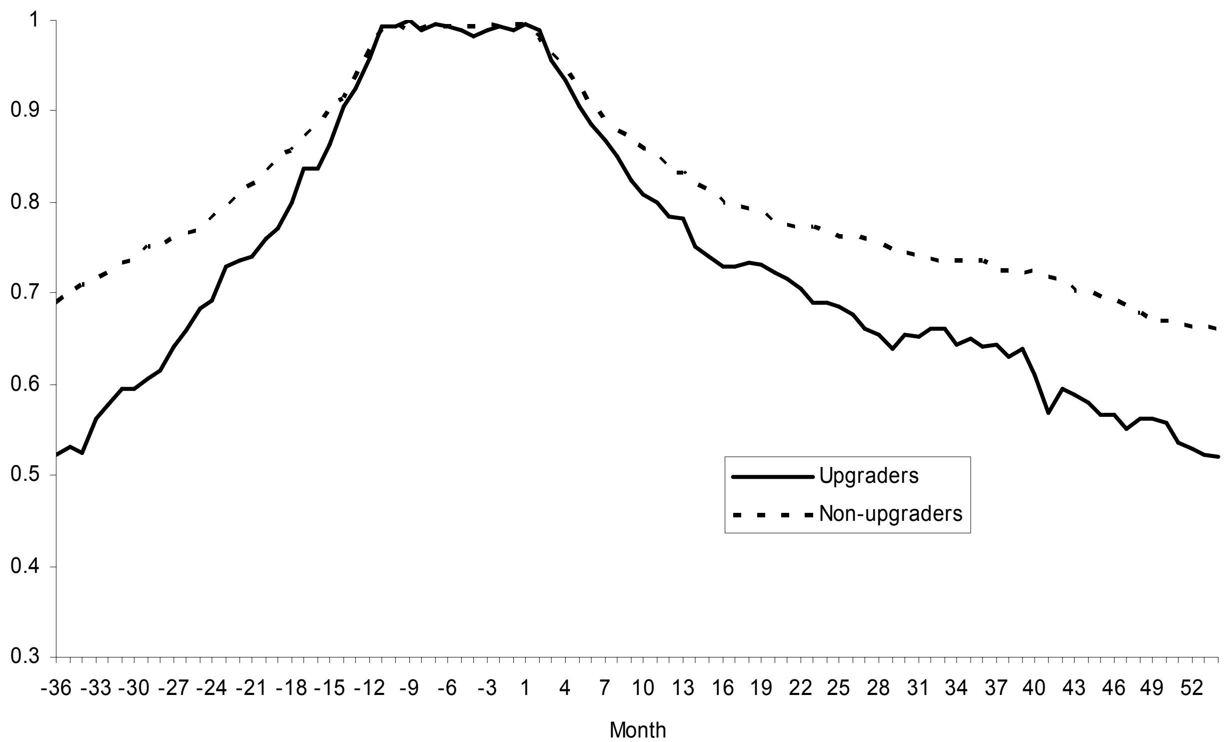
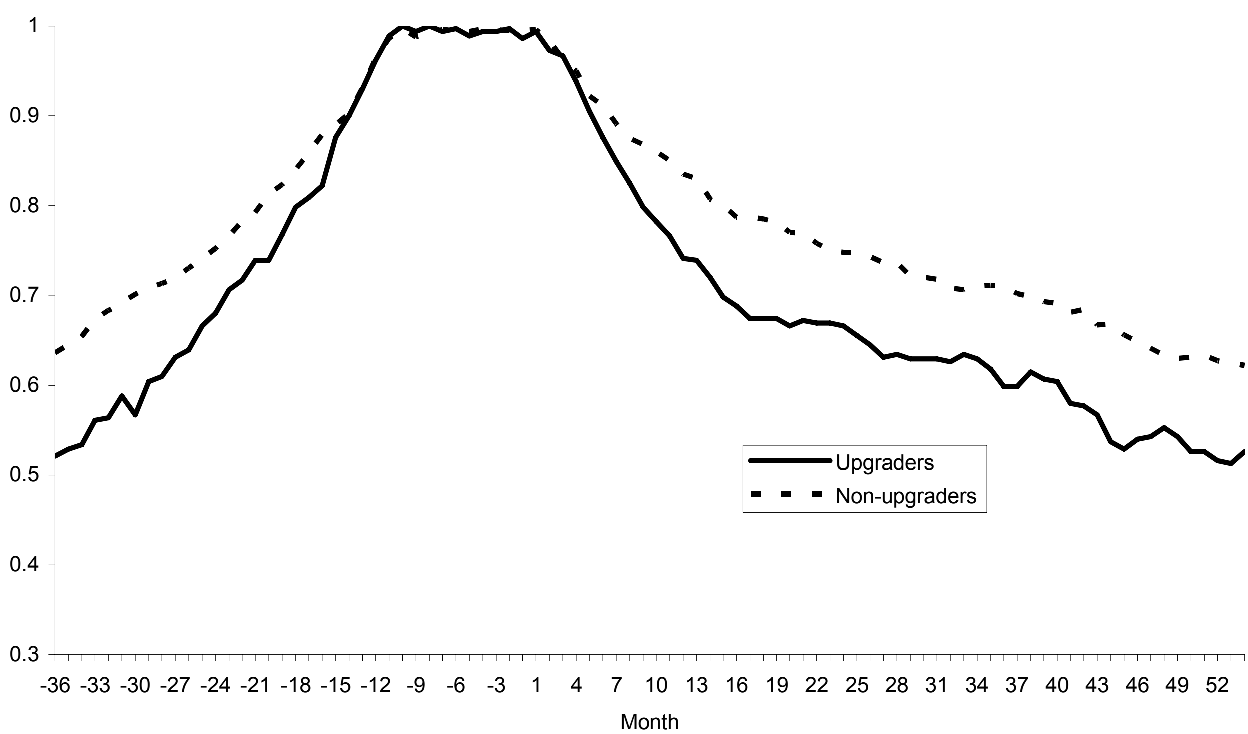


Figure 6: Income Assistance, College Sample 1



We now examine whether the changes in employment, wage rates, and IA receipt shown in the figures continue to hold when other factors are controlled for. Table 12 presents regression results for employment, Table 13 presents the wage regression estimates, and Table 14 contains the results for IA receipt. In each case, the dependent variable is measured at the 54-month point only. We are therefore asking the question: Is education upgrading correlated with better labour market outcomes by the end of the SSP demonstration? In addition to the estimates reported in the tables, all regressions also include other covariates used in this paper such as immigrant status, attitudes towards work, number and age of children, and so forth. These additional estimated coefficients are omitted from the tables because none was statistically significant.

Table 12: Estimated Marginal Effects for Employment Regressions

	High School Upgrading Samples		College and Trades Upgrading Samples	
Variable	**(1) Credential Variables**	**(2) Credential and Coursework Variables**	**(1) Credential Variables**	**(2) Credential and Coursework Variables**
Upgraded education	0.080***	0.129***	-0.001	0.006
	(0.031)	(0.040)	(0.035)	(0.044)
Female	0.032	-0.039	0.129	0.126*
	(0.058)	(0.071)	(0.063)*	(0.064)
Married at 54 months	0.108***	0.107***	-0.018	0.003
	(0.035)	(0.040)	(0.038)	(0.041)
Separated/divorced/ widowed at 54 months	0.026	0.013	0.001	0.012
	(0.032)	(0.035)	(0.038)	(0.040)
Age: 25 to 29	-0.011	-0.076*	-0.026	-0.041
	(0.041)	(0.043)	(0.046)	(0.048)
Age 30 to 39	-0.047	-0.078*	-0.053	-0.061
	(0.037)	(0.041)	(0.043)	(0.045)
Age: 40+	-0.145***	-0.102**	-0.076	-0.106*
	(0.043)	(0.043)	(0.066)	(0.060)
Worked at baseline	-0.116	-0.099	-0.126	-0.110
	(0.031)***	(0.035)***	(0.037)***	(0.040)***
Pre-baseline experience	0.014***	0.013**	0.012*	0.011*
	(0.005)	(0.006)	(0.006)	(0.007)
Pre-baseline experience squared (*10)	-0.003	-0.003	-0.001	-0.000
	(0.002)	(0.002)	(0.001)	(0.001)
SSP experience	0.032***	0.032***	0.034***	0.034***
	(0.002)	(0.002)	(0.003)	(0.003)
SSP experience squared (*10)	-0.003***	-0.002***	-0.003***	-0.003***
	(0.000)	(0.000)	(0.001)	(0.001)
Pre-baseline IA receipt: 24 to 35 months	-0.001	-0.024	-0.007	-0.011
	(0.033)	(0.038)	(0.039)	(0.041)
Pre-baseline IA receipt: 36 months	-0.003	-0.019	-0.034	-0.034
	(0.033)	(0.038)	(0.039)	(0.041)
Ashamed of being on welfare	0.030	0.017	0.100***	0.129***
	(0.034)	(0.038)	(0.043)	(0.047)
Disability	-0.042	-0.031	-0.066*	-0.065*
	(0.031)	(0.036)	(0.037)	(0.039)
Does not speak English	-0.018	-0.038	-0.044	-0.045
	(0.068)	(0.069)	(0.074)	(0.074)
Log likelihood	-755.86	-530.38	-592.49	-529.40
Number of observations	1,965	1,466	1,572	1,397

Notes: Standard errors are in parentheses.
Statistical significance is denoted by *** for 1 per cent level, ** for 5 per cent, and * for 10 per cent.
The dependent variable equals 1 if the individual was employed at 54 months, 0 if not.

Table 13: Estimated Coefficients for Wage Regressions

Variable	High School Upgrading Samples (1) Credential Variables	(2) Credential and Coursework Variables	College and Trades Upgrading Samples (1) Credential Variables	(2) Credential and Coursework Variables
Upgraded education	0.256***	0.164**	0.233***	0.318***
	(0.045)	(0.079)	(0.055)	(0.070)
Female	-0.199*	-0.202	-0.137	-0.126
	(0.113)	(0.140)	(0.139)	(0.146)
Married at 54 months	0.134***	0.194***	-0.017	0.007
	(0.052)	(0.067)	(0.059)	(0.065)
Separated/divorced/ widowed at 54 months	0.038	0.056	0.002	0.004
	(0.053)	(0.068)	(0.062)	(0.068)
Age: 25 to 29	0.057	0.079	-0.128*	-0.132*
	(0.069)	(0.092)	(0.075)	(0.082)
Age 30 to 39	-0.035	-0.073	-0.147**	-0.143**
	(0.062)	(0.080)	(0.069)	(0.073)
Age: 40+	-0.048	-0.096	-0.089	-0.096
	(0.075)	(0.095)	(0.085)	(0.092)
Pre-baseline experience	0.060***	0.050***	0.027***	0.025**
	(0.011)	(0.014)	(0.010)	(0.011)
Pre-baseline experience squared (*10)	-0.020***	-0.016***	-0.001**	-0.001**
	(0.004)	(0.001)	(0.000)	(0.000)
SSP experience	0.001	0.004	0.011*	0.014**
	(0.005)	(0.007)	(0.006)	(0.006)
SSP experience squared (*10)	0.000	-0.000	-0.001	-0.002*
	(0.000)	(0.001)	(0.000)	(0.000)
Pre-baseline IA receipt: 24 to 35 months	-0.063	-0.035	-0.011	-0.035
	(0.054)	(0.071)	(0.060)	(0.065)
Pre-baseline IA receipt: 36 months	-0.114**	-0.121*	-0.132**	-0.116*
	(0.055)	(0.069)	(0.060)	(0.065)
Disability	-0.019	-0.013	-0.123*	-0.130*
	(0.059)	(0.074)	(0.074)	(0.078)
Does not speak English	-0.254*	-0.219	-0.391**	-0.390**
	(0.160)	(0.190)	(0.175)	(0.179)
Constant	10.86***	10.86***	10.93***	10.88***
	(0.149)	(0.188)	(0.178)	(0.190)
R squared	0.15	0.11	0.12	0.13
Number of observations	656	460	551	489

Notes: Standard errors are in parentheses.
Statistical significance is denoted by *** for 1 per cent level, ** for 5 per cent, and * for 10 per cent.
The dependent variable is the log hourly wage at 54 months.

Table 14: Estimated Marginal Effects for IA Regressions

Variable	High School Upgrading Samples (1) Credential Variables	(2) Credential and Coursework Variables	College and Trades Upgrading Samples (1) Credential Variables	(2) Credential and Coursework Variables
Upgraded education	-0.080***	-0.058	-0.072**	-0.126***
	(0.029)	(0.041)	(0.034)	(0.046)
Female	-0.103*	-0.115**	-0.193***	-0.191***
	(0.050)	(0.052)	(0.054)	(0.057)
Married at 54 months	-0.455***	-0.490***	-0.465***	-0.478***
	(0.029)	(0.033)	(0.031)	(0.033)
Separated/divorced/ widowed at 54 months	-0.062**	-0.077**	-0.032	-0.042
	(0.029)	(0.033)	(0.035)	(0.037)
Age: 25 to 29	-0.020	0.040	-0.069	-0.063
	(0.040)	(0.043)	(0.046)	(0.049)
Age 30 to 39	0.006	0.038	-0.013	-0.033
	(0.035)	(0.039)	(0.040)	(0.043)
Age: 40+	0.004	0.052	-0.051	-0.079
	(0.041)	(0.044)	(0.050)	(0.054)
Worked at baseline	0.080**	0.037	0.094**	0.105***
	(0.032)	(0.037)	(0.038)	(0.040)
Pre-baseline experience	-0.001	0.001	0.002	0.001
	(0.005)	(0.005)	(0.005)	(0.006)
Pre-baseline experience squared (*10)	-0.003	-0.003	-0.003	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)
SSP experience	-0.021***	-0.020***	-0.022***	-0.023***
	(0.002)	(0.002)	(0.002)	(0.002)
SSP experience squared (*10)	0.002***	0.002***	0.002***	0.002***
	(0.001)	(0.001)	(0.001)	(0.001)
Pre-baseline IA receipt: 24 to 35 months	0.087***	0.105***	0.109***	0.098***
	(0.030)	(0.034)	(0.034)	(0.037)
Pre-baseline IA receipt: 36 months	0.122***	0.133***	0.174***	0.178***
	(0.030)	(0.034)	(0.034)	(0.037)
Ashamed of being on welfare	-0.045*	-0.049	-0.042	-0.051
	(0.029)	(0.032)*	(0.037)	(0.040)
Disability	0.048*	0.057*	0.045	0.044
	(0.029)	(0.032)	(0.035)	(0.037)
Does not speak English	0.016	0.067	0.017	0.025
	(0.064)	(0.063)	(0.072)	(0.072)
Log likelihood	-1,005.54	-726.83	-803.50	-711.99
Number of observations	2,124	1,559	1,723	1,536

Notes: Standard errors are in parentheses.
Statistical significance is denoted by *** for 1 per cent level, ** for 5 per cent, and * for 10 per cent.
The dependent variable equals 1 if the individual was receiving IA at 54 months.

The central result from tables 12 and 13 is that the very large impacts seen in the raw data hold up in a regression framework, but are more muted. For instance, in the more restrictive Sample 2, the 17 percentage point employment differential associated with high school upgrading in the raw data falls to 13 percentage points once we control for other influences. Similarly, the estimated impact declines from 13 to 8 percentage points in the less restrictive Sample 1. The estimates are still very substantial, particularly for wages. Estimated wage premiums associated with human capital investments are in the 20 to 30 percentage point range for both types of educational activities. There are also substantial employment gains associated with high school completion, but not with enrolment in college and trade school programs.

Other findings from the regressions are mainly ones that would be anticipated. Pre-baseline work experience is positively related (but with diminishing returns) to both the likelihood of being employed at 54 months and hourly wages at 54 months. Experience over the SSP period has a similar relationship in the employment regression, but this effect is small and not always statistically significant for wages.[17] Older workers were less likely to be employed at the end of the SSP period in the high school sample but not in the college and trades sample. Those with a longer history of IA receipt earned less at 54 months. Finally, we find negative wage effects for the disabled and for those without English language proficiency.

The estimated impacts of educational investments on IA receipt are similar to those for employment and wages: large differences between those who upgraded their education and those who did not continue to hold but are reduced in size when we control for other influences. Completing high school is associated with a reduction in IA receipt of six to eight percentage points, although this estimate is only statistically significant in the less restrictive Sample 1. Enrolment in college or trade school programs is associated with a reduction in welfare receipt of 7 to 13 percentage points. The most dramatic impact evident in Table 14 is the effect of being married at Month 54, which lowers IA incidence by almost 50 percentage points in both the high school and college/trade school samples, an impact that is very precisely estimated. Work experience during the SSP demonstration also has a strong negative effect on IA receipt, while those who had spent the most time on welfare prior to the beginning of SSP were also more likely to remain on IA at the end of the experiment. A surprising result is that single parents working at baseline are 8 to 10 percentage points more likely to be receiving IA at the 54-month point, although this estimate is not always statistically significant. Age does not affect IA receipt, but gender exerts an important influence. Attitudes toward welfare and disability status also have modest effects, although these are not precisely estimated and not always statistically significant.

The estimated impacts of educational upgrading are striking. One interpretation of these results is that the much larger gains in employment and wages experienced by upgraders, as well as the larger reductions in IA receipt, are a consequence of their investments in education during the period. However, an alternative interpretation is that the differences in outcomes are due, at least in part, to sample selection. Individuals choose whether or not to acquire additional schooling, and those who do so may differ from those who do not in ways that are correlated with subsequent labour market success.

We address this issue in two ways. The first approach uses propensity score matching estimation.[18] This method provides an alternative way of controlling for observable influences than that provided by linear regression. The second approach uses a series of specification tests to check for the possible influence of unobserved factors. These two approaches allow us to assess whether the more favourable outcomes experienced by those who acquired additional education are due to the additional education or due to other factors, either observed or unobserved.

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[17]This result may reflect the fact that there is little variation in experience in the wage regression.

[18]For recent surveys of these methods, see Imbens (2004) and Heckman and Navarro-Lozano (2004). Black and Smith (2004) provide an illustration of the application of these methods to educational choice in the form of college quality.

Matching estimation methods compare the outcomes experienced by those receiving a treatment (in this case, acquiring additional education) with a matched set of individuals who did not receive the treatment but who have a similar probability of receiving the treatment based on observed characteristics. Non-upgrader observations similar to upgrader observations with respect to the probability of upgrading serve as counterfactuals. These methods are appropriate when there is selection on observables, or the conditional independence (CI) assumption holds. This assumption requires that conditioning on observable factors removes all systematic differences in outcomes in the untreated state between those receiving and those not receiving the treatment. In these circumstances, after conditioning on observed covariates, the difference in mean outcomes can be attributed to the treatment.

The CI assumption is more likely to be valid when there is a rich set of covariates available that influence both educational change (the treatment) and labour market outcomes. This validity is arguably the case here, since we have a large number of demographic and individual characteristics including attitudinal variables such as "likes to work" that are unavailable in many data sets. We are also able to control for factors such as pre-baseline work experience and the individual's history of welfare receipt, variables that are often unavailable or crudely proxied in other data sets. Matching methods have been found to perform well when, in addition to the availability of a rich set of covariates, the *treatments* and *comparisons* receive the same survey instrument and operate in the same local labour markets (Heckman, Lalonde, & Smith, 1999). Both conditions hold in this setting.

Both matching and linear regression assume selection on observables. However, matching has two potential advantages over regression-based methods. First, it provides a semi-parametric approach, whereas linear regression assumes that *linearly* conditioning on observable characteristics is sufficient to remove all systematic differences between the two groups. The matching methods we employ combine a parametric probit model for the propensity scores with non-parametric matching on the estimated scores.

A second potential advantage of propensity score matching techniques is that they facilitate checking for common support. Common support refers to the set of conditioning variables for which there is positive density for the treatment and comparison groups, that is the values of the conditioning variables (or the propensity score) for which there exist treated as well as untreated individuals. Linear regression may mask failure of common support; identification outside the common support region comes from projections based on the assumed linear functional form.

We estimate propensity scores using a probit model. The conditioning variables are selected to include factors expected to influence the decision to acquire additional education as well as outcomes in the non-upgrading state (i.e. in the absence of educational change). The set of variables employed is the same as that used in the upgrading regressions discussed previously: gender, age, marital status, years of work experience at baseline, IA receipt over the past three years, number of children, and indicator variables for the SSP control group, BC, working at baseline, attitudes toward work, presence of a child under five years of age, disability status, immigrant status, and lack of proficiency in the English language. The estimated marginal effects of the probit model are reported in tables 9 and 10.

For propensity score matching to be valid, the outcomes must be mean independent of the treatment, after conditioning on the propensity score. One way to check whether this conditional independence property is violated is to carry out a balancing test, as described in Dehejia and

Wahba (2002). The balancing test is not informative about which conditioning variables to include in the model used to estimate the propensity score. However, it is informative about whether to include interaction terms and higher order terms in the specification. We report the results of such a test in Table 15 for the high school upgrading case.[19] The four columns show mean outcomes in the untreated state (i.e. at baseline) for those who subsequently obtained a high school diploma and those who did not. We condition on the propensity score by reporting the mean outcomes for intervals of the estimated propensity scores. None of the 18 pairs of outcomes is statistically significantly different at the five per cent level, and only one of 18 pairs is significantly different at the 10 per cent level. Thus the balancing test indicates that it is not necessary to include any interaction terms or additional higher order terms in the probit model.

Table 15: Balancing Test for High School Upgrading — Sample 1

	Upgraders		Non-upgraders	
Estimated Probability of Upgrading	**Mean Baseline Wages**	**Mean Baseline Employment**	**Mean Baseline Wages**	**Mean Baseline Employment**
0 to ≤0.05	-	-	1.31	0.065
			(0.405)	(0.037)
0.05 to ≤0.1	2.01	0.143	1.52	0.095
	(0.190)	(0.078)	(0.129)	(0.017)
0.1 to ≤0.15	1.59	0.135	1.72	0.132
	(0.261)	(0.048)	(0.080)	(0.019)
0.15 to ≤0.2	1.95	0.186	1.63	0.156
	(0.169)	(0.047)	(0.102)	(0.021)
0.2 to ≤0.25	1.89	0.239	1.82	0.184
	(0.119)	(0.052)	(0.097)	(0.024)
0.25 to ≤0.3	1.96	0.154	1.83	0.184
	(0.109)	(0.041)	(0.070)	(0.028)
0.3 to ≤0.35	1.89	0.145	1.92	0.142
	(0.194)	(0.041)	(0.053)	(0.031)
0.35 to ≤0.4	2.14	0.146	1.93	0.218
	(0.199)	(0.056)	(0.122)	(0.042)
0.4 to ≤0.45	1.76*	0.190	2.15*	0.194
	(0.132)	(0.088)	(0.150)	(0.072)
0.45+	2.08	0.500	2.01	0.75
	(0.333)	(0.189)	(0.192)	(0.164)

Notes: Standard errors are in parentheses.
*denotes that the two means are statistically different from each other at the 10 per cent level.

Table 16 checks for common support, again using five percentage point differences in the propensity scores. In our case, there is common support over the entire range. For example, in the case of high school completion and college/trade school enrolment in our less restrictive sample (Sample 1), in each interval there are potential comparators with the same propensity scores as those acquiring additional education. In no interval does common support fail. In fact, we are able to impose a one percentage point common support threshold for each of the samples; that is, there is never greater than a one percentage point difference between an upgrader's propensity score and a comparator's propensity score.

[19]The results for the case of college and trade school enrolment are very similar. We do not report the balancing test for IA receipt because virtually all of the sample was on IA at baseline.

Table 16: Common Support — The Distribution of Propensity Scores

Estimated Probability of Upgrading	Upgraders	Non-upgraders
High school upgrading, Sample 1		
0 to ≤0.05	2	55
0.05 to ≤0.1	21	287
0.1 to ≤0.15	47	315
0.15 to ≤0.2	69	300
0.2 to ≤0.25	68	255
0.25 to ≤0.3	82	207
0.3 to ≤0.35	80	138
0.35 to ≤0.4	38	100
0.4 to ≤0.45	24	25
0.45+	4	6
College and trade school enrolment, Sample 1		
0 to ≤0.05	2	133
0.05 to ≤0.1	14	194
0.1 to ≤0.15	28	226
0.15 to ≤0.2	53	196
0.2 to ≤0.25	49	199
0.25 to ≤0.3	62	162
0.3 to ≤0.35	52	78
0.35 to ≤0.4	40	68
0.4 to ≤0.45	31	45
0.45 to ≤0.5	23	31
0.5 to ≤0.55	12	12
0.55 to ≤0.6	5	3
0.6+	1	3

We implemented two variants of propensity score matching and applied these to our high school and college/trade school samples. The first method is *nearest neighbour* matching, which chooses for each member of the treatment group (i.e. each individual who upgraded his or her education) the individual in the comparison group of non-upgraders with the closest propensity score. The estimated impact of upgrading on employment and wages is then obtained by comparing the mean outcomes of the treated and untreated groups. The second method used was *kernel-based* matching, which estimates the counterfactual outcome on the basis of outcomes experienced by several individuals rather than a single nearest neighbour. The estimated counterfactual outcome is thus a kernel-weighted average of these individual outcomes, with the weights depending on the proximity of the propensity score to that of the treated individual. We report results using the Epanechnikov kernel estimator, which performed better than the Gaussian kernel estimator on the basis of a *leave-one-out* cross-validation procedure.[20] The cross-validation test also revealed that the nearest neighbour technique systematically performs worse than the kernel-based procedures (regardless of the type of kernel employed). In some

[20] As the name implies, this procedure omits one observation from the set of untreated individuals and uses the remaining observations to predict the counterfactual outcome for the omitted individual. The difference between the actual and predicted outcome provides an *in sample* prediction error for this observation. The procedure is then repeated for all untreated observations. Alternative matching methods can thus be compared on the basis of mean squared error. Black and Smith (2004) also found that the Epanechnikov kernel estimator outperformed the Gaussian kernel using this validation method (in addition to outperforming the nearest-neighbour technique). We thank Dan Black and Jeff Smith for providing their cross-validation algorithm.

cases, the mean squared error is 100 per cent higher for the nearest neighbour technique. However, we include these estimates in our tables for comparison purposes given that nearest neighbour matching remains a widely used technique.

Table 17 reports the matching estimation results. In the case of employment, for Sample 1 the estimated impact of high school completion is larger, with both the nearest neighbour and kernel matching estimators than the comparable regression-based estimate. The nearest neighbour and kernel estimates are not significantly different from each other, but both suggest that linear regression may understate the impact of educational change on employment. The estimates based on the more restrictive Sample 2 also suggest that completing high school leads to much higher employment. In this case the matching and regression estimates are similar in size and not significantly different from each other. Matching and standard regression also produce the same inferences regarding the effects of enrolling in college or trade school. All sets of estimates indicate that this form of educational activity has no significant impact on employment outcomes.

Table 17: Estimated Impact of Education Upgrading — Matching Estimators

Outcome by Education Type	Estimated Impact of Upgrading	
	Sample 1	**Sample 2**
High school completion		
Hourly wages at 54 months — Nearest neighbour	0.181*** (0.065)	0.069 (0.099)
Hourly wages at 54 months — Kernel (Epanechnikov)	0.178*** (0.041)	0.004 (0.077)
Number of observations {# upgraders}	656 {193}	460 {65}
Employed at 54 months — Nearest neighbour	0.147*** (0.047)	0.097* (0.061)
Employed at 54 months — Kernel (Epanechnikov)	0.125*** (0.029)	0.126*** (0.044)
Number of observations {# upgraders}	1,965 {416}	1,466 {150}
IA at 54 months — Nearest neighbour	-0.062* (0.036)	0.088 (0.074)
IA at 54 months — Kernel (Epanechnikov)	-0.088** (0.026)	0.023 (0.048)
Number of observations {# upgraders}	2,123 {435}	1,559 {159}
College and trade school enrolment		
Hourly wages at 54 months — Nearest neighbour	0.123*** (0.040)	0.151*** (0.077)
Hourly wages at 54 months — Kernel (Epanechnikov)	0.174*** (0.047)	0.247*** (0.065)
Number of observations {# upgraders}	551 {139}	489 {77}
Employed at 54 months — Nearest neighbour	-0.040 (0.048)	-0.039 (0.057)
Employed at 54 months — Kernel (Epanechnikov)	-0.017 (0.033)	-0.020 (0.041)
Number of observations {# upgraders}	1,572 {350}	1,397 {179}
IA at 54 months — Nearest neighbour	0.011 (0.057)	-0.037 (0.061)
IA at 54 months — Kernel (Epanechnikov)	-0.033 (0.031)	-0.050 (0.049)
Number of observations {# upgraders}	1,723 {372}	1,487 {189}

Note: Bootstrapped standard errors are in parentheses (based on 500 repetitions).

In contrast to the case of employment, the matching-based estimates for wages are uniformly lower than the regression-based estimates. This result is true for both high school completion and college/trade school enrolment. Nonetheless, for both types of educational upgrading, the matching-based estimated impacts are economically large.[21] For example, the kernel-based matching estimates imply wage gains of approximately 18 per cent for both high school completion and college/trade school enrolment.

Matching estimates of the impact of high school completion on IA receipt also yield similar results to the regression-based estimates. In the case of the less restrictive Sample 1, the kernel matching estimate implies a reduction in IA receipt of nine percentage points, almost identical to the regression-based estimate. With the smaller and more restrictive Sample 2, both methods produce insignificant impact estimates. However, matching estimation does produce different inferences regarding the impact of college and trade school enrolment. Specifically, the matching estimates are smaller in size than their regression-based counterparts and no longer significantly different from zero.

In summary, use of propensity score matching produces similar inferences to those obtained with linear regression. Matching estimation produces estimates of the impact of high school completion on employment that are the same size as or larger than those obtained with regression methods. Both matching and linear regression imply that enrolling in college or trade school does not increase employment. With respect to wages, the matching-based estimates are generally smaller in magnitude than their regression-based counterparts. Although the estimated effects of educational upgrading on wages are more muted when these semi-parametric methods are employed, the estimates nonetheless remain large in size and most are reasonably precisely estimated and statistically significant. Finally, matching estimates of the impact of high school graduation on IA receipt are similar to the regression-based estimates — a reduction in IA incidence of eight to nine percentage points. However, matching estimates of the impact of enrolment in college and trade school programs are smaller in size than the regression-based estimates and are not statistically significant.

Our final set of estimates explore the effect of timing: does it matter when you upgraded your education? The results thus far treat all upgraders in the same way, but one might anticipate that individuals who upgraded earlier in the sample period would be in a superior labour market position relative to those who upgraded at the end of sample period. Indeed, one possible reason why we do not find any upgrading effect for employment in the case of college and trades is that many of the relevant programs are two years in duration and thus there is insufficient time in our sample period for individuals to complete the education and find employment. Our final analysis therefore splits the upgrading indicator into three separate dummies: a) individuals who upgraded by the 18-month point (i.e. somewhere between the baseline and the 18-month survey); b) those who upgraded by 36 months, conditional on not having upgraded by Month 18; and c) those who upgraded during the 37- to 54-month interval. Table 18 presents the results. For brevity, we show only the estimates for Sample 1; the estimates were largely unchanged with Sample 2. For the wage regressions, the timing of the upgrading makes little difference. For both the high school and college samples, a hypothesis test that the three coefficients are equal cannot be rejected and thus it is appropriate to pool the three into a single dummy variable. For employment, as expected, the

[21]One exception is the estimates for high school completion using the restrictive Sample 2, which are imprecise and not significantly different from zero.

timing does appear to matter. The results suggest that individuals who upgraded prior to the 36-month survey are about 11 percentage points more likely to be employed at the end of our sample period, whereas individuals upgrading in the final phase are no more likely (actually less likely for college/trades) to be employed at Month 54 than individuals who did not upgrade. Oddly, for both high school and college/trades, no significant employment effect was found for individuals upgrading during the earliest phase of the sample period (prior to Month 18).

Table 18: The Timing of Upgrading — Estimates from Wage and Employment Regressions

Variable	High School Sample 1			College/Trade School Sample 1		
	Wages	Employment	IA	Wages	Employment	IA
Upgraded by 18-month survey	0.298 (0.063)***	0.067 (0.045)	-0.106 (0.046)**	0.317 (0.071)***	-0.003 (0.046)	-0.135 (0.048)***
Upgraded during months 19–36	0.233 (0.080)***	0.157 (0.059)***	-0.044 (0.052)	0.183 (0.094)**	0.169 (0.073)***	-0.031 (0.067)
Upgraded during months 37–54	0.269 (0.072)***	0.054 (0.040)	-0.085 (0.046)*	0.101 (0.103)	-0.106 (0.049)**	-0.020 (0.052)

Notes: Standard errors are in parentheses.
Statistical significance is denoted by *** for 1 per cent level, ** for 5 per cent, and * for 10 per cent.
The dependent variable for wage regressions is the log hourly wage at 54 months, and the dependent variable for employment regressions equals 1 if employed at the 54-month point. All other covariates are as noted in tables 14 and 15.
Sample 1 refers to the sample where only the educational attainment variables are used in defining upgrading.

Both linear regression and matching methods rely, of course, on the validity of the *selection on observables* assumption. Although we take advantage of the rich set of covariates available in the SSP data, it is possible that there are unobserved factors such as ability or motivation that are correlated with both the propensity to make educational investments and wage and employment outcomes. If so, our estimates of the impact of upgrading may overstate the true impact of educational change.

In order to address this issue, we carry out a series of specification tests. The intuition behind these pre-program tests is straightforward. Prior to receiving the treatment, the mean outcomes (after adjusting for other factors influencing these outcomes) of the treatment and comparison groups should not differ significantly from each other. Differences in mean adjusted pre-program outcomes would indicate the presence of unobserved differences between the two groups that also influence outcomes. We first carry out a series of pre-program tests using employment, wage rate, and IA receipt prior to and at baseline. For Sample 1, the mean unadjusted outcomes are plotted in figures 1 to 6. Note that the employment rates and mean wages of those who subsequently upgraded their education lie above those of non-upgraders throughout the pre-baseline period. However, in the high school upgrading sample, the differences between the two groups in mean pre-baseline wages and employment are never statistically significant. In the case of college and trade school enrolment, there are likewise no statistically significant differences in wages. However, college and trades enrollees (even though they had not enrolled yet) were about six percentage points more likely to be employed at baseline relative to the non-upgraders. This latter finding suggests the presence of observed or unobserved differences between enrollees and non-enrollees that are correlated with employment, but not wage rate, outcomes.

As figures 5 and 6 illustrate, there are significant differences in IA receipt between upgraders and non-upgraders during the pre-baseline period. The lower IA incidence among those who subsequently finished high school or enrolled in college or trade school programs indicates that upgraders may have observed or unobserved characteristics that make them less likely to rely on IA.

We next control for observable influences and test whether there are significant differences in mean adjusted outcomes. The covariates used for this purpose are the same as those used in the outcome equations discussed previously. These pre-program specification tests are reported in Table 19. For employment and wage rates, the first column reports the difference in mean regression-adjusted outcomes at baseline.[22] We also carry out further specification tests that take advantage of the fact that we observe educational changes and wage, employment, and IA receipt at each survey date. Thus, at the 18-month point we create a sample of potential upgraders that consists of all those who had not increased their education since baseline. Some of these individuals subsequently acquired additional education and some did not. We thus compare the mean 18-month outcomes of the *subsequent upgraders* to their non-upgrader counterparts. We then carry out equivalent tests at the 36-month point. An important advantage of these tests carried out at the 18-month and 36-month survey dates is that the proportion of the sample with non-zero employment and wage outcomes is growing over time. Thus the power of the tests is increasing over time.

Table 19: Specification Tests for Selection Bias

	High School Sample 1								
	Employment Measured at:			Wages Measured at:			IA Measured at:		
	Baseline	18 Months	36 Months	Baseline	18 Months	36 Months	36 Months Pre-baseline	18 Months	36 Months
Upgraded post-baseline	0.015 (0.013)	-	-	0.147* (0.088)	-	-	-0.104*** (0.028)	-	-
Upgraded post-18 months	-	0.010 (0.031)	-	-	0.097 (0.078)	-	-	0.004 (0.026)	-
Upgraded post-36 months	-	-	0.016 (0.037)	-	-	0.054 (0.100)	-	-	-0.018 (0.039)
College Sample 1									
Upgraded post-baseline	0.013 (0.012)	-	-	0.012 (0.073)	-	-	-0.040 (0.032)	-	-
Upgraded post-18 months	-	-0.046 (0.038)	-	-	0.085 (0.080)	-	-	-0.017 (0.032)	-
Upgraded post-36 months	-	-	-0.023 (0.046)	-	-	-0.069 (0.113)	-	-	-0.008 (0.026)

Notes: Standard errors are in parentheses.
Statistical significance is denoted by *** for 1 per cent level, ** for 5 per cent, and * for 10 per cent.

[22] The pre-baseline employment and wage rate data come from the baseline survey. We base the specification tests on the baseline levels because these are more likely to be accurately measured. Doing so also yields a larger sample due to greater non-response to questions about earlier experiences.

For the case of high school completion, we find no evidence of statistically significant differences between subsequent upgraders and non-upgraders in employment outcomes at the baseline, 18-month, and 36-month survey dates. We find a similar result for the college and trade school enrollee samples. In the case of wage outcomes, there is weak evidence of unobserved differences between upgraders and non-upgraders in the high school sample. Specifically, at baseline the adjusted mean wage of those who subsequently graduated from high school is about 15 per cent above that of non-upgraders, a difference that is statistically significant at the 10 per cent level but not at the 5 per cent level. Specification tests at the 18-month and 36-month surveys yield no significant differences in mean adjusted wage rates. Similarly, there are no significant differences in adjusted wages between enrollees and non-enrollees in the college and trade school sample. Overall, then, these specification tests indicate that there is no evidence of selection bias in our employment rate impact estimates and in our wage rate estimates for the college and trade school sample. There is, however, weak evidence of selection bias in the estimates of the impact of high school completion on wage rates.

For IA, we carried out specification tests at the 18-month and 36-month survey dates in a similar fashion to those for employment and wage rates. These tests indicate that there are no significant differences in regression-adjusted IA rates between those who upgraded their education after the 18-month survey and those who did not, and similarly for the 36-month survey. We also carried out specification tests using pre-baseline IA receipt.[23] However, although we observe pre-baseline IA receipt, we do not observe key covariates such as marital status, work experience, and so on prior to the baseline survey. Thus, this test needs to be interpreted with caution. The results indicate that for the college and trade school sample there are no significant differences in regression-adjusted IA receipt 36 months prior to the baseline survey between upgraders and non-upgraders. However, in the case of high school completion, the incidence of welfare receipt among those who completed secondary school after the baseline survey was about 10 percentage points lower than among those who did not subsequently complete high school. Although the results of this test need to be treated with caution, they do indicate that there may be unobserved factors that are correlated with both pre-baseline IA receipt and post-baseline propensity to complete secondary school. As a consequence, our impact estimates of high school completion on IA receipt may be biased upwards. The point estimate in Table 19 suggests that the extent of the bias may be in the order of 10 percentage points, which is approximately one half the difference between upgraders and non-upgraders in the raw data (see Figure 5).

In summary, our regression-based and matching estimates imply large gains in employment and wages for secondary school dropouts who completed high school during the SSP demonstration. Our specification tests find no evidence to support the view that the estimated employment gains can be attributed to unobserved differences between those who completed a high school diploma during the period and those who did not, and only weak evidence that the estimated wage gains may be partly attributable to such unobserved factors. These results thus suggest that the employment gains, and some — if not all — of the wage gains, were a consequence of the additional education acquired during the period. Similarly, the regression and matching models yield large estimated wage gains associated with enrolment in college and trade school programs. Our specification tests indicate that these

[23] At baseline, virtually all SSP participants were receiving IA, so there is no point testing for differences at that point in time.

impacts are not due to unobserved differences between enrollees and non-enrollees. The results thus suggest that the wage gains were a consequence of the additional educational activity.

Both our regression and matching estimates indicate that educational activity had no impact on employment rates among the college and trade school sample. The specification tests indicate that these estimates of zero impact are not biased due to the presence of unobserved differences between enrollees and non-enrollees. The absence of positive employment impacts for this group is one exception to the general finding that increasing formal education among single parents on welfare improves their labour market outcomes.

The results for IA receipt are less positive. Only in the case of the less restrictive high school sample are the regression-based and matching estimates statistically significant. However, the specification tests also show evidence of selection bias for this sample. Based on the point estimates, the magnitude of the selection bias appears to be similar in size to the impact estimates. Thus there is no clear evidence that high school completion or enrolment in college and trade school programs reduces welfare receipt in this population.

Conclusions

This paper examines the extent and nature of educational upgrading among participants in the Self-Sufficiency Project (SSP). At the outset of the demonstration project, all participants were single parents with children who had been long-term income assistance (IA) recipients. The paper also investigates the consequences of increases in educational attainment for their employment, earnings, and reliance on IA.

There are four principal findings. First, there was a substantial amount of educational upgrading in this population. At baseline, more than one half of SSP participants had not completed secondary school. During the 54 months of the SSP demonstration, the high school completion rate rose from about 46 per cent to approximately 55 per cent. About 20 per cent of those who were high school dropouts at the beginning of the demonstration had completed secondary school by the end of the period. There was also substantial growth in post-secondary education. Over one fifth of those who had never enrolled in a community college or trade school program at baseline did so during the demonstration. There were also large increases in the proportion of SSP participants who completed a college or trade school program. At the university level the increase in enrolment and graduation was even more dramatic in percentage terms, albeit from a very small initial level. Across all forms of schooling, there is evidence of a substantial amount of investment in additional education among these long-term welfare recipients.

Our second conclusion is that there was a substantial amount of mismeasurement of educational attainment and educational activity among this population. This finding reinforces previous research, which also concludes that self-reported education is subject to measurement error to an important extent. We deal with this situation in several ways. First, much of the analysis uses a sample that omits individuals who gave logically inconsistent responses to the four SSP surveys. Second, we take advantage of the rich structure of the education information in the SSP surveys by utilizing multiple sources of information on educational activity. In particular, we use the responses to questions about courses taken between surveys as corroborating evidence for self-reported educational attainment and enrolment. In this way we create several different samples that make different assumptions about the nature and extent of mismeasurement of education. These alternative samples allow us to check the sensitivity of our results to alternative forms of measurement error.

We also find that members of the SSP program group acquired less additional education during the period than did their counterparts in the control group. Although we cannot be certain about the mechanisms involved, the SSP financial incentive evidently resulted in less investment in additional education among members of the program group compared with the estimated counterfactual behaviour provided by the control group. One potential explanation for this result is that the SSP earnings supplement encouraged program members to exit welfare and take up full-time employment, thus providing less time for other activities, including acquiring additional education. Consistent with this time crunch hypothesis, the difference in courses taken between program and control group members is greatest during the early part of the SSP demonstration when the gap in employment between the two groups was largest. This difference narrows as the differential in employment rates diminishes.

The lower levels of human capital acquisition observed in the program group are most evident at the secondary school level. However, there is also evidence of less educational activity at the post-secondary level, especially among community college programs. These results suggest that providing a financial incentive to exit welfare and take up full-time employment may, in addition to its demonstrated benefits in the form of reducing reliance on IA, also have an adverse side effect in the form of reduced investment in education.

In the mid-1990s both British Columbia and New Brunswick introduced changes to their IA programs that put greater emphasis on education and skills upgrading. Although the reforms were not directed specifically at single parents with children, they may nonetheless have encouraged some SSP participants to acquire additional schooling. It is also possible that they may have exerted more influence on the SSP control group than on the program group. Random assignment ensured that the program and control groups had very similar characteristics at baseline. But after the supplement offer was made, the characteristics of the two groups were no longer the same. In particular, following random assignment there was a significantly greater exit from IA among members of the program group than among those in the control group. Because the new policies affected those remaining on welfare, their impact may have been greater on the control group, which had a larger proportion of members still receiving IA. We test for such an influence by utilizing the fact that the date of random assignment varied across SSP participants, so that some groups were affected by the legislation during different time periods (relative to random assignment) than were others. Our tests find no evidence of any impact of these policy changes on educational upgrading behaviour. However, the power of the tests is somewhat limited by the distribution of dates of random assignment relative to the timing of the policy changes. Thus we find no clear evidence that these legislative initiatives influenced educational behaviour, but we also cannot rule out some such influence.

Our final conclusion is that those who upgraded their education generally achieved larger gains in employment and wages than did their counterparts who did not acquire additional education. For this analysis, we construct two samples of potential upgraders. The first consists of those who had not completed secondary school at baseline. We then compare the subsequent employment and wage outcomes of those who obtained a high school diploma with those who did not. The second sample consists of high school dropouts and high school graduates who reported at baseline that they had never enrolled in a community college or trade school. We then compare the end-of-period employment and wage rate outcomes of those who enrolled in college or trade school with those who did not. In both cases we compare the simple average outcomes of the two groups as well as the outcomes after controlling for other influences on employment and wages. We control for other influences using both linear regression and non-parametric matching estimation. We find striking differences between the upgraders and non-upgraders in both employment and wages. For example, at the 54-month point, employment rates were about 13 percentage points higher among those who graduated from high school, while wages were about \$1.00 higher among high school completers and over \$2.00 higher among college/trade school enrollees. A substantial portion of these gains remains after accounting for other influences on labour market outcomes using linear regression and matching estimation. For example, our kernel matching estimates imply employment gains of 13 percentage points associated with high school completion and 18 per cent wage gains associated with both secondary school completion and enrolment in a college or trade school program.

One interpretation of these results is that the much larger gains in employment and wages experienced by upgraders are a consequence of their investments in education during the period. However, an alternative interpretation is that there are unobserved factors such as ability and motivation that are correlated with both the propensity to make educational investments and wage and employment outcomes. We carry out a series of specification tests to assess the role of such unobserved factors. For the high school sample we find no evidence that unobserved factors account for differences in outcomes between upgraders and non-upgraders. A similar result is obtained for wage outcomes among the college/trade school sample. These results thus suggest that the positive wage and employment gains experienced by dropouts who completed secondary school and the positive wage gains experienced by college and trade school enrollees are a consequence of the educational investments made during the period. Our findings thus provide rather striking evidence that investments in formal education — such as completing secondary school — can yield significant benefits for single parents on welfare — a group that is of considerable policy interest.

In contrast to the results regarding wages and employment, we do not find clear evidence that additional education reduced reliance on IA. Those who upgraded their formal education during the SSP demonstration were less likely to receive IA at the end of the SSP period than were their counterparts who did not upgrade their education. However, these differences in welfare receipt appear to be attributable to differences between upgraders and non-upgraders in observed and unobserved characteristics that are correlated with IA receipt.

Our results have a number of policy implications. One important implication relates to work-first policies that encourage IA recipients to exit welfare and enter the workforce. Our results suggest that providing financial or non-financial incentives to leave welfare and take up full-time employment may have adverse side effects on human capital investments among single parents on welfare. These negative consequences need to be weighed against any positive benefits of such policies.

The population studied here had relatively low levels of education. Little is known about the consequences of investments in formal education among this group. Our paper provides rather striking evidence that increases in education — such as completion of secondary school — can improve wage and employment outcomes among this population. This finding supports policies that emphasize human capital investments as a mechanism for increasing labour force attachment among welfare recipients and improving their labour market outcomes.

We also find evidence that there is a substantial amount of educational upgrading occurring among single parents on welfare. This finding suggests that we need to be careful when evaluating programs that promote education and training among welfare recipients. In particular, simple before-and-after comparisons are likely to overestimate the impact of the program. Our evidence indicates that a substantial amount of educational change takes place even in the absence of programs that promote investments in human capital.

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