Post-secondary education in Canada: can ability bias explain the earnings gap between college and university graduates?

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Abstract. Using the Canadian General Social Survey we compute returns to post-secondary education relative to high school. Unlike previous research using Canadian data, our data set allows us to control for ability selection into higher education. We find strong evidence of positive ability selection into all levels of post-secondary education for men and weaker positive selection for women. Since theability selection is stronger for higher levels of education, particularly for university, the difference in returns between university and college or trades education decreases slightly after accounting for ability bias. However, a puzzling large gap persists, with university-educated men still earning over 20% more than men with college or trades education. JEL classification: J24, J31, I2, C31

L'éducation post-secondaire au Canada : est-ce que le différentiel d'habileté peut expliquer les écarts de gains entre les diplômés des collèges et des universités? En utilisant les résultats de l'Enquête sociale générale au Canada, on calcule les rendements sur l'éducation post-secondaire par rapport à ceux sur l'éducation secondaire. Contrairement aux travaux antérieurs sur les données canadiennes, les données utilisées ici permettent de tenir compte des différentiels d'habileté. Les résultats révèlent un fort différentiel positif d'habileté pour les hommes à tous les niveaux de l'éducation post-secondaire, et un moindre différentiel pour les femmes. Puisque ce différentiel est plus fort pour les plus hauts niveaux d'éducation, particulièrement pour l'université, la différence de rendements entre universités et collèges ou écoles de métiers décroît quand on tient compte du différentiel d'habileté. Cependant un large écart résiduel demeure et laisse perplexe : les hommes avec une éducation universitaire gagnent encore plus de 20% de plus que ceux qui ont fréquenté les collèges et les écoles de métiers après normalisation pour le niveau d'habileté.

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1. Introduction

Canada spends a tremendous amount of resources on post-secondary education.[1] Consequently, the share of Canadian population with post-secondary education is the largest among OECD countries. Encompassed within this successful statistic is the fact that Canada also has the largest share of non-university post-secondary education such as trades and community colleges. This in turn raises the obvious question whether there are potential differences in the return to human capital investments between university and non-university post-secondary degrees.

The very small Canadian literature looking at returns to different types of post-secondary education reports large gaps between the returns to university and the returns to college or trades education, with university degrees appearing to be much more rewarding than community college degrees or trade certificates (see Ferrer and Riddell 2002; Boothby and Drewes 2006). However, this literature does not account for the possibility that the estimated returns to education may suffer from the selection bias that arises when the choice of education is related to unobserved characteristics, for example, innate ability, which also affect earnings. Controlling for ability bias has a twofold importance. First of all, we need to have a better understanding of the true, unbiased returns to education. Second, we would like to know how much of the university-college earnings differential is due to ability selection and how much of the differential still persists even after ability is controlled for.

A major contribution of this paper is to provide selection-corrected estimates of the returns to schooling using a data set rich in information on personal background: the 1994 Canadian General Social Survey (GSS). Our results show that, once we correct for selection bias, the returns to post-secondary education are lower than the OLS numbers indicate, especially for men. In our preferred specification, the university returns relative to high school decrease from 0.42 to 0.34 for men and from 0.42 to 0.39 for women.[2] Likewise, after ability correction, college returns drop from 0.19 to 0.14 for men and from 0.22 to 0.18 for women. Since we do find evidence of ability selection into all levels of post-secondary education, the differential between university and college returns stays large even after accounting for ability selection, at about 0.23 for men and 0.21 for women. Thus, a second related contribution of this paper is to document the substantial

1 Post-secondary education represents studies pursued beyond high school. We focus on non-university degrees, comprising trades certificates and community college diplomas, and university bachelor's degrees.

2 In what follows we provide arguments for and some statistical evidence of why, given the sample characteristics and the selection mechanism into post-secondary education, our preferred specification is propensity score matching using log hourly wages as outcome. Throughout we report returns as log wage differentials. Usually the log coefficients are a good approximation for percentage differences, but when the coefficients are large, this approximation becomes less accurate, and the percentage difference can be exactly calculated as the exponentiated log coefficient minus 1.

gap that remains between returns to university and non-university post-secondary education, even after correcting for ability.

Goldberg and Smith (2008) provide an up-to-date survey of the returns to education literature in which the bias-correction methods are classified as belonging to two groups: 'selection on observables,' such as propensity score matching, and 'selection on unobservables,' such as Instrumental Variables (IV) or the Heckman correction estimator. While both approaches rely on the availability of good data, they differ in the properties these data need to satisfy in order for identification to be achieved. Selection on observables estimators rely on the availability of variables correlated with both ability and educational attainment, while selection on unobservables estimators rely on assumptions on the error term, perhaps together with the availability of instruments orthogonal to ability but correlated with the education outcome.

In US studies, ability-related measures such as the Armed Force Qualifying Test (AFQT) or the Scholastic Assessment Test (SAT) scores are the ideal candidates for the selection on observables approach. Comparing results in Kane and Rouse (1995) – who control for AFQT ability in the National Longitudinal Survey of Youth (NLSY) – with results in Heckman, Lochner, and Todd (2006) – who use US Census data where such ability measures are not available – Goldberg and Smith (2008) conclude that there is evidence of a significantly large ability bias in the OLS returns to education.[3] Furthermore, Carneiro, Heckman, and Vytlacil (2003) provide OLS returns to education in the NLSY both with and without conditioning on AFQT scores; including the ability measures decreases the OLS returns to a year of education from 0.099 to 0.076, magnitudes that are very similar to our own findings.[4]

Unfortunately, ability measures such as AFQT or SAT scores are not available in Canadian data at the same time as labour market outcomes such as wages or employment. Our study fills this gap by using family background information from the 1994 Canadian GSS to correct for ability selection into post-secondary education. The correction strategies implemented here, in particular propensity score matching, lead to results for the Canadian context that are very similar to the ones obtained in the literature for US data.

Within previous work that has investigated the returns to education for Canada (e.g., Vaillancourt 1995; Burbidge, Magee, and Robb 2002), the only two Canadian studies we are aware of that address the distinction between university and non-university post-secondary education are Ferrer and Riddell (2002) and

3 Kane and Rouse (1995) investigate explicitly the returns to two-year colleges in the U.S. (associate degree) compared with more traditional four-year colleges (bachelor's). In their AFQT-controlled regression, they find that the gains from an associate degree are 0.207 for men and 0.188 for women, while the gains from a bachelor's degree are 0.338 and 0.331, respectively.

4 If we assume that a college degree is equivalent to two extra years of schooling after high school and a university degree to four extra years, this implies OLS-biased returns of 0.198 and 0.396 for college and university degrees, and bias-corrected ones of 0.152 and 0.304 in the Carneiro, Heckman, and Vytlacil (2003) study.

Boothby and Drewes (2006). Ferrer and Riddell (2002) use the 1996 Canadian Census to separate the effect of schooling measured as years spent in an educational institution from the effect of credentials measured as the type of degree obtained. Related to non-university post-secondary education, they find that trades and college degrees are characterized by returns that are only slightly higher than the returns to high school degrees and lower than the returns to university degrees. Similar results are reported by Boothby and Drewes (2006), who use four waves of Census data spanning 1981 to 1996 to look at the trends in the returns to post-secondary education over this period. These two are the main studies we relate our findings to.[5]

Finally, we document that the widely accepted finding of higher OLS returns for university educated women holds only when the outcome is measured in terms of weekly or yearly earnings, as is the case with studies using Census data where no hourly wage measure is readily available. When we use our preferred measure, hourly wages, there is no OLS differential in the returns to university between men and women.[6] The higher weekly wage returns for women, as documented, for instance, by Ferrer and Riddell (2002) and Boothby and Drewes (2006), result from a labour supply issue, since university educated women work more hours per week than the average: 39.3 hours a week, compared with 35.7 hours a week for women with high-school education.

The paper is organized as follows. Section 2 describes the GSS data we use in this analysis. Section 3 refers to the identifying assumptions and properties of the bias correction procedures we implement here: selection on observables (propensity score matching) and selection on unobservables (Heckman bivariate normal model).[7] Section 4 discusses our findings. We start by placing our OLS results within the rest of the literature, and then we present returns to post-secondary education corrected for selection bias. We conclude in section 5.

5 In other US studies, as surveyed by Grubb (2002), returns between 20% to 30% from an associate degree and an additional 10% to 20% from a bachelor's degree are documented. More recently, Marcotte et al. (2005) report a gain between 5% to 8% from attending a full year of community college in a sample of youth from the 2000 National Education Longitudinal Survey (NELS).Nevertheless, evidence from other studies suggests that the returns to education widen with experience and the evaluation of post-secondary education impacts should also include older cohorts.

6 From our OLS computation, the returns to university education are 0.416 for men and 0.420 for women when the outcome of interest is hourly wages, and 0.413 for men and 0.486 for women with weekly wages.

7 Within the selection on unobservables framework, we have also attempted to implement an IV estimator based on sibling/family composition instruments. Unfortunately, the IV identification conditions have failed, and as such we have dropped the IV approach from our main results. The IV analysis is available in a separate appendix OL-D available on line. Appendices OL-A to OL-E are linked to this article at the CJE journal archive http://economics.ca/cje/en/archive.php. We use the prefix 'OL-' to indicate material provided in the on-line archive.

2. Sample characteristics

In this paper we use the General Social Survey (GSS) 1994 - Cycle 09. The GSS was established by Statistics Canada in 1985 with the purpose of gathering data on social issues and on the living conditions of Canadians over time. Every year Statistics Canada collects information on a cross section of individuals. The information collected varies year by year depending on the focus of the particular GSS Cycle. Examples of topics covered by the GSS are health, time use, victimization, education, work and retirement, family and friends, social support, access to and use of information technology. Usually the topics are repeated every six years; however, not all topics are repeated regularly, and some new topics are added later on and others are dropped. Unfortunately, the latter is the case for the survey cycles of interest for us, the GSS on Education, Work, and Retirement, which were conducted in 1989 (cycle 4) and 1994 (cycle 9) and then terminated.

Our choice to use the GSS on Education, Work, and Retirement, rather than another Canadian data set with information on educational attainment, is motivated by the fact that it contains much richer information on individual and family characteristics than other data sets. Most important for our study, the GSS collects information on the educational and working history of individuals, the composition of their current family (spouses and children), the composition of their family of origin (parents and siblings), and the education of parents. As detailed in the next section, we can use this set of rich background information to characterize the educational choice of individuals and build a proper counterfactual of what would have been the labour market outcome for an individual, had he or she made different educational choices. This in turn will allow us to assess the bias in OLS estimates when factors outside of the econometrician's control, such as unobserved innate ability, influence both the educational choice and the labour market outcomes of an individual.

To define educational attainment, we use the survey variable 'highest degree, diploma or certificate completed.' The categories considered in our analysis are high-school diploma ('high school'), diploma or certificate from trade or vocational schools ('trades'), diploma or certificate from community college ('college'), and bachelor's degree ('university'). We drop from the main sample individuals who report not having a high-school diploma and individuals reporting having degrees above bachelor's. High-school drop-outs include a few individuals who, despite not having graduated from high school, have acquired either trades or college education. We do not believe high-school drop-outs should belong to the post-secondary analysis and thus we drop them from the sample.[8] There are 165 such individuals in the trades group, or 24.5% of all trades observations, and 91 individuals in the college group, or 10.4% of all college observations. Similarly, we drop individuals with degrees above bachelor's to keep our analysis

8 Unlike the U.S., Canada includes OECD statistics within the post-secondary education group.

comparable with the previous Canadian and international literature. Like the case of high-school drop-outs, not using postgraduates comes at the cost of smaller sample sizes; sample statistics including all education categories are presented in table OL-B1 from appendix OL-B, available on line (linked to this article at the CJE journal archive http://economics.ca/cje/en/archive.php). This appendix also reports the returns to education when high-school dropouts with trades or college certificates are included in the analysis, as well as when university postgraduates are included. (Note there are no changes to the baseline high-school group.) Adding high-school drop-outs with post-secondary degrees will slightly decrease the returns to trades and college relative to high school, since these drop-outs have labour market outcomes somewhere above those of high-school graduates but below those of trades and college. Reciprocally, including university postgraduates increases the OLS returns relative to high school as well as the ability correction. Other than the differences in magnitudes and the slighlty weaker bias correction for men with trades education and stronger bias correction for men with university education when postgraduates are included, not much else changes when we use the entire post-secondary sample, as illustrated by the results in appendix OL-B.

The GSS cycle 9 collected information on 11,876 persons 15 years old or older, reporting for a population of about 22 million residents in the 10 provinces of Canada. We restrict our analysis to the working-age population, 17 to 65 years old, who have completed high school in Canada, are currently not attending any school, and report positive earnings (and are therefore employed or self-employed) for at least part of the survey year. Considering the restrictions mentioned above, our sample reduces to a total of 3,274 observations, 1,586 for men and 1,688 for women.

Table 1 summarizes the education data for men and women. The second column shows the number of observations in each schooling category, while the third reports the corresponding population equivalent. Each observation is weighted by sample survey weights to ensure the statistics are representative at the national level. The fourth column reports the share of each category in the total population. Our numbers are within the range of statistics reported by OECD (2005) for Canada using 2002 data, also reproduced in Riddell (2002, table 5). For the full sample, the percentage attending high school, non-university post-secondary education (trades and college), and university are reported to be 34%, 41%, and 24%, respectively. Our high-school percentage is slightly higher and the non-university post-secondary education percentage slightly lower. There are two potential reasons for this: the first is that from 1994 to 2002 the percentage of individuals with trades and college degrees could have further increased; the second is that, while the OECD statistics include high-school drop-outs with trades or college degrees, we have eliminated them from our sample.

For men, more than one-third of high-school graduates (37%) decide to acquire non-university post-secondary education (trades or community college) after high school; only half that amount (17%) enrol in university. The fraction

TABLE 1
Summary statistics: educational attainment

	N. obs.	Population	Share
Men			
High school	709	1,506,857	0.45
Trades	277	532,721	0.16
College	312	710,174	0.21
University	288	579,725	0.17
Total	1,586	3,329,477	1.00
Women			
High school	663	1,239,394	0.40
Trades	232	394,742	0.13
College	470	883,911	0.28
University	323	589,292	0.19
Total	1,688	3,107,339	1.00

NOTES: Authors' calculations using GSS 1994 data. Referenced population: 17 to 65 years old, having completed high school in Canada, not currently attending any school, and reporting positive labour earnings for at least part of the survey year. High-school drop-outs and university post graduates are not included in calculations. Population totals are computed by applying GSS survey weights to sample observations.

of women who participate in non-university post-secondary education (41%) is even higher than for men. However, compared with men, more women enroll in college education rather than trades.[9] Both men and women are more likely to choose community college over university (although this is no longer true for men if university postgraduate degrees are also considered).

To some extent, the educational choices made by individuals can be better understood by looking at the earnings differences between trades, college, and university for women and for men. For all individuals we use the reported main working activity to derive three measures of labour earnings: hourly, weekly, and yearly wages. Because the survey allows people to choose how to report the frequency of their earnings (hourly, weekly, yearly, etc.), when the reported frequency is different from hourly or annually we compute wages using information on the number of weeks worked during the year and the usual number of hours worked in a week. Summary statistics on earnings data are reported in table 2.

Columns (1), (3), and (5) report the average hourly wages and weekly and yearly earnings for each education category; the associated standard errors are in columns (2), (4), and (6). While the college versus trades differential is substantial

9 We emphasize that we restrict our analysis to working individuals. As such, it may be possible that the educational attainment in the sample of working women differs from the educational attainment of women in general, to the extent that there could be systematic differences between the educational choices of women who are in the labour force compared with those out of the labour force.

TABLE 2
Summary statistics: wages by education categories

	Hourly wage (1)	Std. err. (2)	Weekly wage (3)	Std. err. (4)	Yearly wage (5)	Std. err. (6)
Men						
High school	15.07	0.28	657.15	13.17	33495	682.3
Trades	17.76	0.46	770.96	17.76	39705	918.4
College	17.52	0.43	762.82	18.52	38636	979.9
University	24.02	0.67	1030.21	29.04	52796	1453.4
Total sample	17.55	0.22	762.85	9.87	38984	507.9
Women						
High school	11.82	0.22	431.71	10.05	21878	499.3
Trades	12.11	0.34	422.95	14.49	20696	786.9
College	14.74	0.33	517.77	12.60	26192	642.0
University	17.68	0.44	680.62	20.34	35233	1027.5
Total sample	13.84	0.17	505.41	7.37	25498	371.2

NOTES: Authors' calculations using GSS 1994 data; see table 1 notes.

for women (a college-educated woman earns more than a trades-educated one by, on average, more than \$5,000 per year), for men there is little difference. Thus, it makes sense that women are more likely to get a college education rather than a trades one, while the proportions should be more similar for men. It is also apparent, even from these simple summary statistics, that university seems to be more rewarding than any other post-secondary educational alternative for both genders – even more so for men than for women. Nevertheless, the average unconditional financial gain is not the only factor that determines the education decision; if that were the case, everyone would opt for a university education. The focus of our paper is to disentangle the various factors that determine the choice of higher education and to correctly measure the monetary gain from higher education after accounting for selection into education on the basis of ability.

Our data set provides relatively good information on the individual's working history. People were asked to report the date when they completed their highest degree and also when they started their first full-time job after graduation, as well as any full-time jobs they might have held prior to or during their studies. We use this information to build an experience variable that takes into account the time individuals spend looking for a job after graduation. This is a more precise measure than the often used potential experience one (which is age minus years of schooling minus 6).[10] We also have access to all the socio-economic variables usually used in Mincerian wage regressions, such as province of residence, union status, and family demographics.

10 Although our measure is a better approximation for overall job market experience, it still falls short of the true experience measure. While we expect that for men our measure is going to be very close to actual experience, we are aware that this may not be the case for women, who tend to move in and out of the labour force much more often than men do.

Perhaps the most relevant feature of our data set comes from a set of questions asking individuals about their family background. As detailed in section 3, we use this information in two different ways to correct for selection bias: (i) selection on observables (propensity score matching) and (ii) selection on unobservables (Heckman correction). To successfully implement either methodology we need information on individuals that can explain their educational choice in such a way that, conditional on this information, the educational choice can be considered random.

3. Estimation methodologies

To identify the causal effect of education on earnings we use the standard wage equation framework, which accounts for observed characteristics that influence the productivity of a worker. We can summarize the wage equation by $Y = \beta X + \delta D + \epsilon$, where the left-hand-side variable Y represents log earnings; X is a collection of individual characteristics that in our analysis include labour market experience, union status, province of residence, marital status, and the presence of kids at home; D is the education variable; and ϵ is an unobserved nuisance parameter. The focus of our research is on the coefficient δ as a measure of returns to education. In order to compare our results with the previous literature we first perform a multinomial analysis, where D is a collection of three indicator variables, one for each education category above high school: trades, college, and university.[11]

As is well known in the returns to education literature (see, e.g., Card 1999 for a review), a simple ordinary least squares (OLS) regression may not identify the true returns to education δ. If any component of the unobservables ϵ (such as, e.g., innate ability) is correlated with both the education variable D and the earnings variable Y, the OLS coefficients will be biased and inconsistent. The main methods used in the literature to correct for selection bias – selection on observables and selection on unobservables – differ on the identification assumptions, and on how the selection correction is implemented. In what follows we detail these procedures.

3.1. Selection on observables: matching

The assumption here is that the researcher has access to some variables Z correlated with both ability and educational attainment, so that, conditional on these variables, assignment to education can be considered random. Formally, this

11 The multinomial analysis corresponds to a constrained wage equation which assumes that all the Xs have the same effect on individuals with different educational levels (i.e., all the βs are constrained to be equal across educational groups). Running separate regressions for each education category relative to high school is equivalent to a multinomial analysis where the education indicators are interacted with all other productivity characteristics X, therefore allowing for heterogeneity in the returns to productivity characteristics by education.

identification assumption, called Conditional Independence Assumption (CIA) or strong unconfoundedness, states that $Y_1, Y_0 \perp D \mid Z$, where Y_1 is the treatment outcome (earnings for individuals with post-secondary education), Y_0 is the outcome without treatment (high-school earnings), D is the treatment indicator (education dummy) and Z is the set of variables that account for selection into education. One reason for the popularity enjoyed by propensity score matching, the estimator we implement here, is due to Rosenbaum and Rubin (1983). They show that, as long as Z satisfies CIA, then rather than matching on the multi-dimensional vector Z, matching can be performed on a scalar index $P(Z)$. Such an index $P(Z)$ is the predicted probability of attending higher education conditional on the characteristics Z, $P(D = 1 \mid Z)$.

The best candidate for Z in our data set is parental education. Parental education is strongly correlated with the educational attainment of children: on average, better-educated parents produce better-educated children. Better-educated parents are higher than average ability individuals within their age cohort. If we assume intergenerational transfer of innate abilities, the children of higher-ability parents should also be of higher ability within their own age cohort. Therefore, parental education is also a good predictor for both children's education and children's innate ability. Because we have access to a data set where we can control for parental education, using selection on observables to control for selection bias is a very good methodological choice.

The orthogonality condition in the CIA is too strong for identification, since a weaker, mean-independence version is needed to uncover unbiased returns to post-secondary education: $E[Y_0 \mid Z, D = 0] = E[Y_0 \mid Z, D = 1]$ and $E[Y_1 \mid Z, D = 1] = E[Y_1 \mid Z, D = 0]$. The former mean condition suffices to identify the returns to post-secondary education for the educated, or the 'treatment on the treated (TT)' parameter: $\Delta^{TT} = E[Y_1 - Y_0 \mid D = 1] = E[Y_1 \mid Z, D = 1] - E[Y_0 \mid Z, D = 1]$, while the latter is needed to identify the returns to post-secondary education for high-school educated individuals, or the 'treatment on the untreated (TU)' parameter: $\Delta^{TU} = E[Y_1 - Y_0 \mid D = 0] = E[Y_1 \mid Z, D = 0] - E[Y_0 \mid Z, D = 0]$. The main parameter of interest identifies the returns to education for a random individual in the population, or what the literature calls the average treatment (AT) effect $\Delta^{AT} = E[Y_1 \mid Z] - E[Y_0 \mid Z]$, and it is thus the right parameter to use in the comparison of returns with the uncorrected OLS ones; we are grateful to a referee for making this point.

The propensity score matching estimator is given by

$$\Delta^{AT} = \frac{1}{N}\left\{\sum_{i\in D_1}\left[Y_{1i} - \sum_{j\in D_0} w_1(i, j)Y_{0j}\right] + \sum_{j\in D_0}\left[\sum_{i\in D_1} w_0(i, j)Y_{1i} - Y_{0j}\right]\right\}.$$

N is the number of observations; $i \in D_1$ and $j \in D_0$ indicate treated and control individuals; $w_1(i, j)$ is the weight placed on the jth control observation in constructing the counterfactual for the ith treatment observation, and $w_0(i, j)$ is the weight placed on the ith treatment in constructing the counterfactual for the jth control.[12] In Kernel matching the weights are given by

$$w_1(i, j) = \frac{G_{ij}}{\sum_{k \in D_0} G_{ik}}$$

and

$$w_0(i, j) = \frac{H_{ij}}{\sum_{k \in D_1} H_{kj}},$$

where

$$G_{ik} = G\left(\frac{P(X_i) - P(X_k)}{a_{n1}}\right)$$

and

$$H_{kj} = H\left(\frac{P(X_j) - P(X_k)}{a_{n0}}\right)$$

are kernels with bandwidth parameters a_{n1} and a_{n0}.[13]

Bandwidth selection is an important issue in kernel matching. We pick the functional form of the kernels G and H and the optimal bandwidths a_{n1} and

12 AT is a weighted average of the TT and TU parameters:

$$\begin{aligned}
\Delta^{AT} &= E[Y_1 \mid Z] - E[Y_0 \mid Z] \\
&= \{E[Y_1 \mid Z, D = 1]P(D = 1) + E[Y_1 \mid Z, D = 0]P(D = 0)\} \\
&\quad - \{E[Y_0 \mid Z, D = 1]P(D = 1) + E[Y_0 \mid Z, D = 0]P(D = 0)\} \\
&= \{E[Y_1 \mid Z, D = 1] - E[Y_0 \mid Z, D = 1]\}\ P(D = 1) \\
&\quad + \{E[Y_1 \mid Z, D = 0]) - E[Y_0 \mid Z, D = 0]\}\ P(D = 0) \\
&= \Delta^{TT} P(D = 1) + \Delta^{TU} P(D = 0).
\end{aligned}$$

The first summation term in curly brackets comes from the TT estimator, while the other one comes from the TU estimator:

$$\Delta^{TT} = \frac{1}{n_1} \sum_{i \in D_1} \left[Y_{1i} - \sum_{j \in D_0} w(i, j) Y_{0j} \right],$$

and

$$\Delta^{TU} = \frac{1}{n_0} \sum_{j \in D_0} \left[\sum_{i \in D_1} w_0(i, j) Y_{1i} - Y_{0j} \right],$$

where n_1 is the number of treated persons $i \in D_1$ and n_0 is the number of untreated controls in $j \in D_0$), and $\Delta^{ATE} = \frac{n_1}{N}\Delta^{TT} + \frac{n_0}{N}\Delta^{TU}$.

13 In nearest neighbour matching the weights are $w_1(i, j) = 1$ for the control that is closest to i in terms of $P(Z)$ and zero for all the other controls, and $w_0(i, j) = 1$ for the treatment that is closest to j and zero for the other treatments. Similarly, k nearest neighbours matching can be defined with weights $1/k$ and, respectively, 0.

a_{n0} in a cross-validation optimization procedure that minimizes mean squared error, MSE (see Racine and Li 2005 for details). In the cross-validation procedure we follow recent work by Bergemann, Fitzenberger, and Speckesser (2005) and Galdo, Smith, and Black (2007) by assigning higher weight in the MSE to observations more likely to be someone's closest neighbour and zero weight to observations who are never anyone's closest neighbour and are thus unlikely to participate in the computation of the counterfactual in estimation. While our main results come from kernel matching estimators with optimal bandwidth, we also report sensitivity results to different bandwidths (0.01, 0.05, 0.10, and 0.50) using the Epanechnikov kernel, as well as results from one, five, and ten nearest neighbours.

Although we cannot test the identification assumption that conditioning on the set of characteristics Z makes assignment to education random, we can test one implication of this assumption, namely, whether conditional on the propensity score $P(Z)$, the Z have the same distribution in the treatment and in the control group. Such a test is called the balancing score test. The literature offers more variants for the balancing score test; because it is insensitive to the particular form of the matching estimator, we use here the polynomial version introduced by Smith and Todd (2005). For every possible Z we estimate a polynomial in $P(Z)$ and interactions between D and powers of $P(Z)$. Once we have conditioned by the polynomial in $P(Z)$, further conditioning by the treatment indicator D and interactions between D and the polynomial in $P(Z)$ should not be informative.[14]

One final concern regarding the biases that may arise when using propensity score matching (or other regression techniques such as OLS) is the issue of common support. To compare similar persons, the distribution of the propensity scores for people who go on to post-secondary education and for those who do not should have overlapping support. We achieve common support by imposing a min-max condition, where we drop observations with a propensity score below the maximum of the two minima or above the minimum of the two maxima of the propensity scores. This is the most commonly used, as well as the easiest, way to impose common support.[15]

14 While there is no guidance as for the order of the polynomial, we follow Smith and Todd (2005) by picking a quartic in $P(Z)$. Formally, for each component of the vector Z we can estimate the regression

$$\begin{aligned} Z_k = {} & \beta_0 + \beta_1 \widehat{P(Z)} + \beta_2 \widehat{P(Z)}^2 + \beta_3 \widehat{P(Z)}^3 + \beta_4 \widehat{P(Z)}^4 + \beta_5 D \\ & + \beta_6 D\widehat{P(Z)} + \beta_7 D\widehat{P(Z)}^2 + \beta_8 D\widehat{P(Z)}^3 + \beta_9 D\widehat{P(Z)}^4 + \epsilon. \end{aligned}$$

The variable Z_k is balanced if we do not reject the null $H_0 : \beta_5 = \beta_6 = \beta_7 = \beta_8 = \beta_9 = 0$.

15 No overlapping common support creates fewer problems for kernel matching than for nearest neighbour matching or for OLS because observations outside the common support receive a very low weight in kernel matching anyway. We nevertheless impose common support in all non-parametric estimations, including kernel matching.

3.2. Selection on unobservables: Heckman correction estimator

The second approach implemented here solves the selection problem by making explicit assumptions about the correlation between the endogenous regressor D and the error term ϵ. Methods belonging to this class of correction are the Heckman correction model and the Instrumental Variables (IV) approach.

We discuss here the selection on unobservables estimators within the framework of control function estimators. which solve the selection problem by imputing the non-zero conditional mean of the error term as a missing variable. By adding and subtracting this term from the original wage equation we obtain

$$Y = \beta X + \gamma D + E[\epsilon \mid X, D] + (\epsilon - E[\epsilon \mid X, D]),$$

where the term in parentheses has now become a well-behaved mean-zero error term. $E[\epsilon \mid X, D]$ represents the control function that, when added to the outcome equation, leads to unbiased coefficient estimates. For the IV estimator, in the first-step education D is regressed on all exogenous variables X as well as on the instrument Z, which must be orthogonal on ability (but correlated with education): $D = \gamma_1 Z + \gamma_2 X + u$. In the second step, the wage equation is estimated by replacing the education indicator D with its prediction from the first-stage, or, equivalently, by supplementing the second-stage wage regressors X and D with the residual from the first-stage regression, $Y = \beta X + \delta D + \pi \widehat{u} + \epsilon$; in this equivalent representation $\pi \widehat{u}$ is the control function. In the Heckman correction framework, the first-stage participation equation is given by $D^* = \gamma Z + \eta$, where D^* is a latent variable (e.g., utility from education) such that we observe $D = 1$ when $D^* > 0$ and $D = 0$ otherwise. Z is assumed to be orthogonal on ϵ, and it can include any or all of the exogenous X. The extra term can be written as $E[\epsilon \mid X, D] = E[\epsilon \mid X, D, Z] = E[\epsilon \mid X, Z, D = 1]D + E[\epsilon \mid X, Z, D = 0](1 - D)$. The Heckman correction model further assumes joint normality for the distribution of the two error terms $(\epsilon, \eta)\tilde{}N(0, 0, \sigma, 1, \rho)$, where the variance of η is normalized to 1, and σ and ρ are the standard deviation of ϵ and the correlation of ϵ and η, respectively. Then, $E[\epsilon \mid X, Z, D = 1] = E[\epsilon \mid X, Z, \eta > -\gamma Z)$, and using properties of the truncated normal mean $E[\epsilon \mid X, Z, D = 1] = \rho\sigma\lambda$, where $\lambda = \phi(Z\gamma)/\Phi(Z\gamma)$, with ϕ and Φ the p.d.f and the c.d.f of the standard normal distribution, and λ is the inverse Mills ratio, or selection hazard. Similarly, we obtain $E[\epsilon \mid X, Z, D = 0] = \rho\sigma\widetilde{\lambda}$, where $\widetilde{\lambda} = \phi(Z\gamma)/[1 - \Phi(Z\gamma)]$. The Heckman procedure therefore eliminates the selection bias by imputing the extra term $\rho\sigma(\lambda D + \widetilde{\lambda}(1 - D))$, where $\lambda D + \widetilde{\lambda}(1 - D)$ is computed from the first-stage probit and $\rho\sigma$ is estimated as a coefficient in the second-stage wage equation. The true impact of post-secondary education on earnings is computed from corrected wage equation as $\Delta = E[Y \mid D = 1] - E[Y \mid D = 0] = \delta + \rho\sigma(\lambda - \widetilde{\lambda})$.

Unlike IV, the Heckman correction model only relies on exclusion restrictions to the extent that the joint normality assumption is violated (see, e.g., Little and Rubin 2002 or the survey in Puhani 2000). In fact, the identification of the Heckman correction model can rely solely on the non-linearity of the inverse Mills ratio coming from the joint normality distribution assumption; in contrast, the success of the IV estimator hinges exclusively on the availability of a good instrument Z. This may explain why the Heckman estimator performed reasonably well in correcting for selection into schooling, while IV failed. We tried to implement the IV estimation, using as instruments the province of high school and the family composition of an individual (number and gender of siblings and order of birth), but, as we report in on-line appendix OL-B, our identification strategy failed because of the weakness of the instruments available.[16]

There are two final issues to discuss; they apply to both selection on observables and selection on unobservables estimators. The first issue concerns standard errors. Other than OLS, obtaining correct standard errors is a non-trivial issue, because of the extra variation induced by the two-stage estimation procedures. For IV we have closed-form standard errors that account for the variation due to the first-stage derived regressor. For matching and for the Heckman correction procedures we compute standard errors in a bootstrap procedure with 300 repetitions. The second issue refers to estimators that rely on first-stage estimations of probit models (such as matching and Heckman correction models). We do not implement a multinomial procedure to compute joint probabilities; instead, we get consistent marginal probabilities of attending one level of education versus another directly from the binary probability estimators.[17]

4. Estimation results

4.1. Comparing our OLS results to the previous literature

Since we use different data from Ferrer and Riddell (2002) and Boothby and Drewes (2006), we first check to what extent the choice of the data set drives the difference between our results and those in the previous literature. In table 3 we summarize the results obtained by Boothby and Drewes (2006) (first column) and by Ferrer and Riddell (2002) (second column) and we compare them with our own results from two different specifications. Reported throughout are log wage coefficients. Column (3) reports results from an OLS analysis where we impose

16 We thought that these variables could be good instruments, since geographical variables could affect the cost of education because of policy differences across provinces, and that sibling composition could influence educational attainment, since it is easier to finance post-secondary education for one child than for many children. Nevertheless, the arguments against the appropriateness of our instruments seem to have prevailed. Geographical variables could be endogenous, owing to mobility, and family composition and income might influence early childhood investments in cognitive and non-cognitive skills, which in turn correlates with adult outcomes even conditional on education. Indeed, this may be one of the reasons why the IV estimator failed to account for any positive selection into education, similar to conclusions found for the U.S. by Butcher and Case (1994).

17 For papers that extend binary correction models to the multinomial case see, for instance, Imbens (2000), Lechner (2001), and Plesca and Smith (2007).

TABLE 3
Comparison of OLS returns to post-secondary education in Canada

Log wage coefficient	Boothby et al.[a] (1)	Ferrer et al.[b] (2)	Restricted[c] (3)	Unrestricted[d] (4)
Men				
Trades	0.108		0.110	0.156
College	0.156		0.137	0.193
Trades and College	0.142	0.125	0.124	0.175
University	0.377	0.373	0.393	0.411
Women				
Trades	0.034		0.015	0.026
College	0.184		0.158	0.211
Trades and College	0.153	0.158	0.106	0.147
University	0.473	0.494	0.448	0.410

a Boothby and Drewes (2006, table 4), Census data 1995. Results reported here in log-differences rather than percentages for consistency.
b Authors' calculation from Ferrer and Riddell (2002), Census data 1995. Results from their specifications (3) and (4) as reported in table 3, using average education duration from table 1.
c Authors' estimation based on GSS 1994. Log-weekly wages; potential experience used instead of actual experience; sample restricted to FTFY workers (≥30 hours/week, ≥48 weeks/year).
d Authors' estimation based on GSS 1994. Log-hourly wages; GSS-defined measure of experience; full sample as defined in footnotes to table 1.

restrictions, trying to get as close as possible to the OLS specification and sample restrictions, as in Ferrer and Riddell (2002) and Boothby and Drewes (2006). We restrict the analysis to men and women working full time and full year (FTFY), we use potential experience (age minus years of schooling minus 6) rather than our more accurate experience measure, and, like them, we use log-weekly wages as dependent variable. Column (4) has results from our preferred OLS specification, which does not impose FTFY, uses our better experience measure, and looks at log-hourly wages as a more direct measure of human capital.

Not surprisingly, if we define our sample and variables to be similar to the sample and variables used in Ferrer and Riddell (2002) and Boothby and Drewes (2006), we obtain very similar results.[18] Nevertheless, when we use our preferred unrestricted specification, we depart slightly from the original results. For men our returns are higher at every level of education, although the relative differences remain stable, and for women the returns to university are lower.

While we do not have a conclusive explanation for the larger observed returns for men, we conjecture it could be our more precise measure of experience, which accounts for gaps in experience until the first full-time job is obtained. The potential experience measure used in Ferrer and Riddell (2002) and Boothby

18 However, small differences remain, such as somewhat lower returns to all levels of education compared with high school for women. Given the order of magnitude of our standard errors, the difference may be simply due to random errors.

and Drewes (2006) considers the initial time spent looking for the first job after graduation as full-time experience, even when this is not the case, and this could potentially account for higher returns to experience at the expense of lower returns to schooling in their specification.

For women, our lower returns to university are driven by the different measure of earnings employed. In our preferred specification we focus on hourly wages, while both Ferrer and Riddell (2002) and Boothby and Drewes (2006) use weekly wages, since the Census data provide no equivalent measure of hourly wages. There is little variation in hours worked by men, but women with university education have a higher labour supply than the average (for statistics on hours worked, see table OL-A1 in the on-line appendix OL-A). The returns to weekly wages captures the extra effect of hours worked, which is not present when hourly wages are used instead. This is the main reason we find similar OLS returns to university for men and women, unlike other Canadian studies, which find higher OLS returns for women.

Overall the information in table 3 indicates that, although we use a different data set and a close but different year in our estimation, our results can be easily reconciled with the results obtained by previous literature using Census data. In this sense we are confident that the use of our data set, which is significantly richer in personal and family information than the Census, provides comparable results that can be generalized to the whole Canadian population.

4.2. Bias-corrected returns to education

Table 4 summarizes the main results of our analysis, separately for men and women. Investigating hourly wages returns, the first column reports the multinomial OLS results (which were presented in the last column of table 3). The second column of table 4 reports returns to post-secondary schooling using the same simple OLS procedure but performing separate regressions for each level of schooling above high school relative to high school only (full estimation results are reported in table A1 in the appendix). The returns to schooling from the three separate regressions are very close to the multinomial case, lending further support to the bivariate analysis. It is more convenient for us to focus on separate regressions by education groups in order to easily implement the procedures that correct for selection bias and compare their results with OLS.

Columns (3) and (4) contain the bias corrected returns: column (3) from propensity score matching, and column (4) from the Heckman correction procedure. Given the very rich information on family background and the identifying assumptions for the three correction procedures, we believe propensity score matching is the method best suited to correct for selectivity in our data set. This is further confirmed by the first- and second-stage statistical properties of the matching and Heckman estimators, as discussed in section 4.3. Given that our

TABLE 4
Returns to education using log hourly wages

	Multinomial OLS (1)	OLS (2)	Matching (3)	Heckman (4)
Men				
Trades	0.156	0.145	0.123	0.111
	(0.032)	(0.032)	(0.033)	(0.040)
College	0.193	0.191	0.140	0.188
	(0.031)	(0.031)	(0.030)	(0.031)
University	0.411	0.416	0.345	0.404
	(0.031)	(0.033)	(0.045)	(0.044)
Women				
Trades	0.026	0.043	0.001	−0.002
	(0.032)	(0.032)	(0.036)	(0.039)
College	0.211	0.217	0.177	0.164
	(0.026)	(0.027)	(0.032)	(0.033)
University	0.410	0.420	0.390	0.345
	(0.029)	(0.030)	(0.031)	(0.046)

NOTES: Authors' estimations. Standard errors in parenthesis. Reference group: High-school graduates. Sample as defined in table 1. University comprises bachelor's only. Matching Average Treatment (AT) impacts, results for TT and TU impacts are also available in on-line table OL-A2. Kernel matching with optimal bandwidths listed in on-line table OL-A3.

preferred methodology is matching, we focus our substantive discussion on the bias-corrected matching results in column 3.[19]

The top panel of table 4 provides returns to education corrected for ability selection bias for men and the one on the bottom for women. The bias correction procedure decreases the returns to post-secondary education when compared with OLS returns, indicating a positive ability selection into higher education. As in the OLS case, with the exception of women with trades education, all the results are statistically significant.

For men, the university bias corrected returns go down to 0.345 compared with the 0.416 OLS returns, while the college returns go down to 0.140 compared with the 0.191 OLS returns. For women returns to college go down to 0.177 compared with the 0.217 OLS returns, while for university they go down to 0.390 compared with 0.420 in the OLS case. Therefore, our results suggest that the correction for ability bias is slightly stronger at the university level than at the college level for men, while for women it is the opposite: the positive selection into post-secondary education is slightly lower for university than for college, and lower than for men. Moreover, once ability selection is accounted for, in contrast

19 The matching results come from kernel matching with optimal kernel and bandwidth reported in appendix table OL-A3. Sensitivity results to other bandwidth choices are reported in tables OL-A6 and OL-A7. A more detailed discussion on kernel and bandwidth choice appears in section 4.3.

TABLE 5
Direct estimation of university-trades and university-college returns

	Men		Women	
	OLS	Matching	OLS	Matching
Trades				
Difference in relative impacts	0.272	0.222	0.377	0.389
	(0.037)	(0.047)	0.040	0.045
Coefficient from direct estimation	0.252	0.228	0.378	0.391
	(0.039)	(0.039)	(0.037)	(0.045)
College				
Difference in relative impacts	0.226	0.205	0.203	0.213
	(0.037)	(0.046)	(0.037)	(0.041)
Coefficient from direct estimation	0.233	0.206	0.199	0.200
	(0.038)	(0.043)	(0.031)	(0.034)

NOTES: Log-wage differentials reported here as an approximation to percentage differences; the exact percentages can be obtained as the exponential of the log differential minus 1. 'Difference in relative impacts' is computed as the impact of university relative to high school minus the impact of trades (or college) relative to high school. 'Coefficient from direct estimation' comes from the analysis on a subsample of university and trades (respectively, university and college). Standard errors (bootstrapped or closed-form for OLS estimation) in parentheses. Sample as defined in table 1. Kernel matching with optimal bandwidths listed in on-line table OL-A3.

to the OLS case, the 'true' returns to college and university become higher for women than for men. These higher returns for women are consistent with recent trends in post-secondary education, which have seen female enrolments overshoot male ones starting in the mid-1990s, as documented, for instance, in Christofides, Hoy, and Yang (2006). Finally, for women the returns to trades education remain very low, presumably because they acquire different skills than men do with this type of education, begging the question of why so many women choose this type of education at all.

For men, accounting for selection bias not only lowers the returns to post-secondary education, but, because the ability selection into university is highest, it also decreases to some extent the large gap between returns to university and college. This gap increases very slightly for women, for whom the ability selection does not appear higher for university than it is for college. Table 5 summarizes the magnitude of the gap in returns between university and trades (top panel) and the gap between university and college (bottom panel). The first line, 'Difference in relative impacts,' reports the straightforward subtraction of the returns to trades (respectively college) from the returns to university, as given in table 4, where all returns are reported relative to the same baseline, high school. The second line, 'Coefficient from direct estimation,' provides sensitivity to this computation by directly estimating the returns of university relative to trades (respectively, college). We can see that the advantage of having a university education over a college education is reduced by about 2 percentage points for men when ability bias is taken into account, while it increases by about 1 percentage point for women. At the end of the day, a substantial gap between university and college of about 20 percentage points still remains for both men and women. This gap seems too high to justify the preference that many Canadians show for trades or college education over university.

Despite different methodology and sample, our bias corrected returns are similar to the ones in US studies, for instance, Carneiro, Heckman, and Vytlacil (2003) and Kane and Rouse (1995), who use the NLSY sample to control for ability selection by including in the wage regression the AFQT battery of test scores, together with a rich set of personal characteristics. Especially for men, the bias correction we find is very similar to the correction that can be obtained from the NLSY sample by controlling for AFQT scores as a measure of ability.[20]

The Heckman bias correction for the hourly wage outcome is negligible for men with college or university education and positive for men with trades education and for women with all levels of post-secondary education. The statistical properties of the Heckman estimator discussed below confirm that the bias correction is not significant for men with college or university education. As a consequence of the small ability bias detected by the Heckman procedure for men, the returns to all levels of post-secondary education are smaller for women than for men when the Heckman correction is applied.

4.3. Statistical properties of matching and Heckman estimators

We can rely on statistical tests to verify, to a certain extent, the appropriateness of each correction method. For matching, the first step requires computing the propensity score that combines in an index measure the covariates Z conditional on which assignment to education can be considered random. This is done in a probit estimation reported in the appendix tables A2 and A3 for men and women, respectively.

20 This result corroborates the idea that including parental background in the propensity score can proxy for other more direct measures of ability (not available in Canadian data). To check to what extent parental background can lead to similar ability bias correction as AFQT in the NSLY, we replicated our analysis on an NLSY sample. We used the same year as in the Canadian analysis, 1993, and imposed the same data restrictions (the equivalent of a Canadian college degree is an associate degree in the U.S.). Table OL-E1 from on-line appendix OL-E shows that for men we obtain no significant bias reduction for the returns to an associate degree (relative to the OLS coefficient) from propensity score matching – neither when using AFQT nor when using parental background in the propensity score. Nevertheless, for university-educated males we obtain very similar (statistically the same) bias reduction: from 0.397 (OLS) down to 0.322 for matching with AFQT, or down to 0.336 for matching with parental background. The results for women are less encouraging. We find similar bias reduction with AFQT and with parental background for the treatment on the treated (TT) parameter, but not so for the treatment on the untreated (TU) parameter, when propensity score matching using parental education fails to identify a negative ability bias. There might be many reasons why the results for women are inconclusive; for instance, NSLY follows a cohort of women of very similar age, around 32 years old in 1993. This is an age when fertility decisions are very important for women and may affect our results as long as the fertility choice differs across education groups.

4.3.1. Probit coefficients

Besides the role of the probit model in providing the metric to compare participants and non-participants, the probit results are relevant from a substantive point as well, giving us an idea about what determines selection into post-secondary education. For both men and women, parental education matters, especially for the decision whether to attend university or not. Individuals whose mothers or fathers have any education other than university are less likely to participate in university education.[21] Having more sisters and brothers decreases the probability of a university education for men; the same story applies to men with trades or college education and to women in all post-secondary education categories, but the coefficients are not statistically significant (except for women with trades education, where the sister dummies are once again significant). In general, living in a province other than Ontario increases the probability of a trades education, and, with few exceptions (not statistically significant) decreases the probability of college or university education for both men and women.

4.3.2. Common support

Related to the first-stage probit analysis, common support does not seem to pose a major problem. The bottom panels of tables A2 and A3 indicate that rather few post-secondary education (treatment) observations – ranging from none for men with trades or college education up to at most 5.1% for women with university education – are lost due to lack of common support. More observations are off support for the high-school (control) group, but since the high-school sample is larger, percentage-wise the observations lost to common support are fewer than 10% (and usually lower than that) in all cases.

4.3.3. Balancing score tests

The results from the polynomial version of the balancing score test are available in on-line appendix OL-A, tables OL-A4 and OL-A5. Since for each covariate Z the null hypothesis tested is that extra conditioning on the participation variable should provide no further information, a successful balancing score should show a small F-test statistic and a large P-value, preferably more than 0.10. Reassuringly, these tests indicate that balancing is almost always achieved in our specification. One instance where balancing is not achieved comes from the analysis on the expanded sample, including high-school drop-outs with trades or college certificates. These results are available from on-line appendix OL-B, tables OL-A4 and OL-A5. Since we do not believe high-school drop-outs should be part of our analysis on post-secondary returns, the balancing score test results serve as a further indication that high-school drop-outs with post-secondary degrees are

21 In the university stream, the coefficients for father's education are statistically significant, but not so for mother's education, except for mothers who are high-school dropouts. In the other education streams parental education coefficients also are not significant, except for men, who are more likely to enrol in trades or college education if their fathers also had a trades or college education, all else equal.

different from other post-secondary graduates; for them, our education selection model may not be appropriate.

4.3.4. Sensitivity to propensity score specification

In theory there should exist an optimal set of covariates in the propensity score (‘minimal relevant information set’ (e.g., Heckman and Navarro-Lozano 2004)) such that CIA is satisfied. Leaving out some relevant characteristics can lead to biases, but so can adding irrelevant ones. In particular, it is not advisable to include in the propensity score covariates that are good candidates as instrumental variables; because they are uncorrelated with the outcome (other than through their correlation with education), such covariates may further erode common support. We performed sensitivity analysis on the covariates entering the propensity score by (i) omitting sibling information from the propensity score and (ii) adding immigrant status to the baseline specification.[22] Since all of our covariates are discrete variables, we did not have to investigate specifications where covariates entered non-linearly. On-line appendix OL-C provides in table OL-C1 matching returns to education using the sensitivity specifications, while Probit estimation results, common support, and balancing tests for the sensitivity specifications are available from tables OL-C2 to OL-C10. Substantively, the results do not change when the sensitivity specifications are employed in the propensity score. The common support neither deteriorates much nor improves with the sensitivity specifications. If anything, support seems slightly worse for the trades and college analysis and perhaps slightly better for the university analysis with the first sensitivity specification. Balancing seems slightly worse for the second sensitivity specification. Overall, we can conclude that (i) there is no reason to suspect our baseline propensity score specification is not the correct one, and (ii) returns to education are stable to small changes in the propensity score.

4.3.5. Sensitivity to kernel bandwidths

One final relevant aspect related to the matching procedure regards the bandwidth selection in kernel estimation or, similarly, the number of neighbours in the nearest neighbour estimation in the second stage of the propensity score matching procedure. Our main results come from kernel estimation where the kernel and bandwidth have been selected in a leave-one-out cross-validation procedure that minimizes the root mean squared error (RMSE) in a fashion that penalizes bad matches. The optimal kernel and bandwidth choices for each education category are reported in on-line appendix table OL-A3. While the propensity score matching literature has documented significant sensitivity to the bandwidth choice in some instances, this does not seem to be our case. On-line appendix tables OL-A6 and OL-A7 report returns to education from kernel propensity score matching

22 If siblings were good instruments in the IV sense, then we should not have included them in the propensity score. Nevertheless, since siblings do not make good instruments, we leave them in the main specification.

with different bandwidths in kernel, as well as different numbers of neighbours in nearest neighbour matching. All results are very close to those from kernel matching with optimal bandwidths: matching overall appears to be a stable and appropriate bias correction procedure. Further evidence that the bandwidth choice has little impact on the matching results is presented in on-line appendix tables OL-C11 to OL-C14, which report sensitivity to bandwidth choice for all other specifications discussed here.

4.3.6. Heckman estimators

As in matching, the first step in the Heckman correction procedure is a probit model for education choice.[23] Tables A4 and A5 report the first-stage probit regression and the second-stage log-hourly wage regression corrected for selection, for men and women, respectively.[24] For all specifications the estimated coefficient $\rho\sigma$ in front of the correction term $\lambda D + \widetilde{\lambda}(1 - D)$ is negative, therefore indicating a positive ability selection into all categories of post-secondary education for both men and women. The coefficient $\rho\sigma$ in front of the correction term λ is reported in the middle of tables A4 and A5. While it is significant for men with trades education and for women with all categories of post-secondary education, it is not significant for men with college or university education. This is consistent with the Heckman corrected results reported in table 4, which indicate a substantial correction for the returns of men with trades education and for women with all levels of post-secondary education, but little change in the returns of men with college and university education compared with the biased OLS results.[25]

4.4. Returns to education for weekly wages

For comparability with studies using Census data where the outcome is by necessity the weekly wage, we repeat the analysis on weekly wages. The results reported in table 6 are consistent with the story told so far. For men, for whom hours worked do not vary substantially across education, there are only few differences between the returns to log-weekly wages compared with the returns to log-hourly wages reported in table 4. The same positive ability selection is identified, resulting in smaller bias-corrected returns relative to the OLS ones. Notably, for men the ability correction is larger for university than for college, shrinking

23 While in matching probit is one of the options available for the first step estimation – for instance, logit is also a popular choice to which matching results are not sensitive – the Heckman correction relies on the normality assumption embedded in the probit.

24 While we tried to argue that the number of siblings could work as exclusion restriction, it failed as an instrument in the IV procedure. Other variables such as parental education may determine both the post-secondary education decision and the wage outcome and are also not good exclusion restrictions. A specification that did not include parental education in the first step produced very similar results.

25 Also note that the coefficient on the education dummy from the second-stage wage equation does not represent the returns to education, which should instead be computed as $E[Y \mid D = 1] - E[Y \mid D = 0] = \delta + \rho\sigma(\lambda - \widetilde{\lambda})$.

TABLE 6
Returns to education using log weekly wages

	Multinomial OLS (1)	OLS (2)	Matching (3)	Heckman (4)
Men				
Trades	0.169	0.156	0.116	0.094
	(0.036)	(0.037)	(0.035)	(0.037)
College	0.215	0.212	0.150	0.205
	(0.035)	(0.035)	(0.034)	(0.039)
University	0.404	0.413	0.297	0.362
	(0.035)	(0.038)	(0.053)	(0.050)
Women				
Trades	0.028	0.038	−0.018	−0.043
	(0.048)	(0.049)	(0.054)	(0.070)
College	0.206	0.218	0.174	0.146
	(0.039)	(0.041)	(0.040)	(0.052)
University	0.491	0.486	0.454	0.385
	(0.044)	(0.045)	(0.043)	(0.063)

NOTES: Authors' estimations. Standard errors in parentheses. Reference group: high-school graduates. Sample as defined in table 1. Matching Average Treatment (AT) impacts. Optimal bandwidths for kernel matching reported in on-line table OL-A3.

the gap between the two to about 15%. The opposite is true for women, for whom the bias-corrected gap between university and college returns increases to about 28%. One possible explanation comes from the labour supply side, since college-educated men work on average one hour more each week than university-educated ones, while college-educated women work three and a half hours less than their university-educated counterparts (see the labour supply statistics from on-line table OL-A1).

Measuring the outcome in terms of weekly wages adds the labour supply factor to the productivity differential reflected in the hourly wages. Nevertheless, it answers a relevant question about returns to education insofar as the labour supply differential itself is an outcome of education choices.[26] The returns to university education are much higher for women than for men, even before correction for ability selection. This finding is consistent with the accepted status quo in the Canadian literature, where higher OLS returns for women using weekly wages have been documented.

4.5. *The rate of return to post-secondary education*

We also calculate the internal rate of return to college and university education in a similar fashion to Drewes (2006). The internal rate of return is given by the value of the discount rate that makes the relative gain from post-secondary

26 Similar results on yearly wages and yearly labour supply are available from the authors.

TABLE 7
Simulated internal rates of returns to college and university

		Men		Women	
		College	University	College	University
Drewes (2006)[a]		0.1180	0.1140	0.1140	0.1280
LSE[b]	OLS	0.1143	0.1223	0.1265	0.1413
	Matching	0.0833	0.1015	0.1025	0.1331
NO-LSE[c]	OLS	0.1096	0.1125	0.1258	0.1054
	Matching	0.0783	0.0916	0.1018	0.0971

a Drewes (2006, table 8).
b,c Authors' calculations based on equation 1. Lifelong earnings path of individuals computed based on OLS coefficients from the regressions reported in table A1 for men and women, respectively. Hourly wages by level of education and experience are calculated first, then expected yearly earnings are computed by multiplying hourly wages by total hours worked. Labour supply effects are embedded in total hours worked as follows: *b*: Labor Supply Effects: hourly wages ∗ average hours worked by education level; *c*: No-labour supply effects: hourly wages ∗ total average hours worked.

education compared with high school equal to its costs, that is, the r that solves

$$\sum_{t=0}^{45} \frac{E_t^i - E_t^h}{(1+r)^t} - C = 0, \tag{1}$$

where E_t^i are the expected earnings at time t by persons with education $i = c, u$ for college and university, respectively, and C is the direct cost of education, mainly fees.[27] In table 7 we report the internal rates of return to college and university education for men and women, as implied by our OLS and matching corrected regressions.

While Drewes (2006), whose results we reproduce in the beginning of our table, finds that the rates of return for college and university are not very dissimilar and therefore might explain the high enrolment in college education in Canada, we find a larger difference between the two. When we account for the labour supply effects (LSE) of post-secondary education, we find that the OLS internal rate of return for college education is 11.43% for men and 12.65% for women, while for university it is 12.23% for men and 14.13% for women. The matching internal rates of return are lower than the OLS ones, more so for men than for women, consistent with matching accounting for selection bias into education. Accounting for LSE, both the uncorrected (OLS) and corrected (matching) rates of return are higher for university than for college. If we do not account for the effect of education on labour supply (NO-LSE) and focus instead on the returns

27 Our computation differs from the one made by Drewes (2006) in that we explicitly discount the costs, so that $C = \sum_{t=0}^{n} c_t/(1+r)^t$ with $n = 2$ for college and $n = 4$ for university.

to productivity, we find lower internal rates of return for university-educated women, given their labour supply response to university education.

5. Conclusion

Using a data set rich in information on personal background, the 1994 Canadian General Social Survey (GSS), we provide selection-corrected estimates of the return to schooling. We perform the estimation separately for men and women to allow for a different selection mechanism and wage process for each sex. Among the estimators implemented to correct for selection bias we find propensity score matching to be the most reliable one, given identification assumptions and available data. By contrast, we find the IV estimator performed very poorly, despite the fact that ex ante the instruments had seemed plausible enough.

Substantively, our results show positive ability selection into post-secondary education, especially for men. In other words, part of what the OLS estimator identifies as returns to education actually represents returns to innate ability. Once we correct for ability selection bias, the returns to post-secondary education are lower than the OLS numbers would indicate. For women, propensity score matching indicates a bias correction of a smaller magnitude than for men, which we conjecture may be related to women's selection into labour force participation.

Once we correct for ability selection bias, the university returns relative to high school decrease slightly more than the returns to college or trades for both sexes. Because we find evidence of ability selection into all levels of post-secondary education, albeit of different magnitudes, the gap between the returns to trades or college and university decreases only slightly once ability selection is corrected for. Therefore, ability selection can account only for some of the differential returns between university and non-university post-secondary education degrees. The large gap that remains perhaps may be due to differences in human capital technology between community colleges and universities.

We also compute internal rates of return to education, and we find somewhat lower rates of return for college compared with university, even after accounting for ability differences. We conclude that the high enrolment in community colleges in Canada represents a puzzle not justified by the lower returns generated by a non-university post-secondary education. Moreover, while internal rates of return can be illustrative of the education investment decisions faced by individuals, the computation is very sensitive to more or less ad hoc constraints imposed by researchers, in particular those relating to the duration of study and the direct costs of education.

To consistently explain how students select themselves into different educational programs and how human capital gets accumulated, we need the discipline brought about by a full estimation of a structural model. Such a model should account for the present value of costs and benefits of human capital accumulation in a heterogeneous population. This is our next line of research.

Appendix: Additional tables

TABLE A1
OLS analysis, log hourly wages

	Trades		College		University	
Wage equation	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.
Men						
Education dummy	0.145	0.032	0.191	0.031	0.416	0.033
Marital status: married	0.145	0.043	0.174	0.041	0.130	0.043
Marital status: div./wd./sep.	0.101	0.058	0.158	0.061	0.062	0.061
Province: NFL	−0.265	0.062	−0.324	0.068	−0.354	0.071
Province: PEI	−0.224	0.106	−0.252	0.103	−0.243	0.094
Province: NS	−0.150	0.061	−0.139	0.061	−0.172	0.065
Province: NB	−0.176	0.069	−0.188	0.060	−0.183	0.072
Province: QUE	−0.076	0.046	−0.052	0.043	−0.057	0.045
Province: MAN	−0.150	0.061	−0.158	0.059	−0.180	0.058
Province: SAS	−0.109	0.063	−0.167	0.065	−0.139	0.065
Province: ATA	−0.047	0.052	−0.084	0.050	0.002	0.053
Province: BC	0.036	0.049	−0.011	0.049	−0.005	0.050
Immigrant	−0.044	0.067	0.000	0.067	−0.163	0.064
Experience	0.032	0.004	0.032	0.004	0.039	0.005
Experience squared	−0.001	0.000	−0.001	0.000	−0.001	0.000
Children at home: 1	−0.023	0.048	−0.035	0.048	0.002	0.052
Children at home: 2	−0.001	0.044	−0.029	0.045	−0.004	0.048
Children at home: 3 or more	0.014	0.063	−0.039	0.063	0.029	0.064
Union/collective agreement	0.279	0.029	0.259	0.029	0.218	0.030
Constant	2.183	0.047	2.189	0.046	2.170	0.048
N. obs.	901		929		906	
R-squared	0.276		0.272		0.342	
Women						
Education dummy	0.043	0.032	0.217	0.027	0.420	0.030
Marital status: married	0.060	0.037	0.066	0.034	0.080	0.035
Marital status: div./wd./sep.	0.054	0.048	0.109	0.043	0.062	0.047
Province: NFL	−0.386	0.073	−0.289	0.064	−0.205	0.076
Province: PEI	−0.189	0.082	−0.183	0.074	−0.190	0.082
Province: NS	−0.264	0.061	−0.280	0.061	−0.250	0.054
Province: NB	−0.204	0.067	−0.249	0.051	−0.212	0.060
Province: QUE	−0.097	0.047	−0.084	0.039	−0.062	0.043
Province: MAN	−0.149	0.062	−0.178	0.053	−0.156	0.057
Province: SAS	−0.226	0.056	−0.245	0.056	−0.229	0.058
Province: ATA	−0.059	0.050	−0.140	0.045	−0.090	0.046
Province: BC	0.015	0.048	0.045	0.045	0.007	0.047
Immigrant	0.076	0.067	0.004	0.056	−0.021	0.058
Experience	0.030	0.004	0.034	0.004	0.035	0.004
Experience squared	−0.001	0.000	−0.001	0.000	−0.001	0.000
Children at home: 1	−0.009	0.039	−0.023	0.036	0.004	0.038
Children at home: 2	−0.005	0.043	−0.004	0.037	−0.008	0.042
Children at home: 3 or more	0.099	0.064	0.020	0.062	0.074	0.063
Union/collective agreement	0.277	0.031	0.254	0.027	0.234	0.029
Constant	2.054	0.047	2.057	0.042	2.038	0.044
N. obs.	786		1011		876	
R-squared	0.270		0.305		0.378	

NOTES: The reference group is high-school graduates for each estimation. Sample as defined in table 1.

TABLE A2
Propensity score matching, men

Education equation	Trades Coef.	Trades Std. err.	College Coef.	College Std. err.	University Coef.	University Std. err.
Mother educ. trades, college	−0.081	0.268	0.213	0.270	−0.166	0.237
Mother educ. high school	−0.169	0.253	0.086	0.261	−0.243	0.224
Mother educ. below high school	−0.382	0.247	−0.094	0.256	−0.398	0.221
Father educ. trades, college	0.542	0.255	0.436	0.232	−0.441	0.199
Father educ. high school	0.056	0.256	0.217	0.230	−0.556	0.193
Father educ. below high school	0.250	0.232	0.114	0.210	−0.954	0.174
Age 20–24 years old	5.189	0.346	5.742	0.315	4.955	0.322
Age 25–29 years old	5.690	0.329	6.121	0.305	5.946	0.286
Age 30–34 years old	5.932	0.321	6.127	0.302	6.154	0.279
Age 35–39 years old	5.950	0.321	5.862	0.306	6.024	0.282
Age 40–44 years old	5.964	0.327	6.168	0.308	6.226	0.287
Age 45–49 years old	5.983	0.338	6.122	0.320	6.270	0.295
Age 50–54 years old	5.453	0.402	6.047	0.353	6.311	0.327
Age 55–59 years old	5.574	0.361	5.742	0.345	6.015	0.320
High school province NFL	0.976	0.202	−0.205	0.223	−0.277	0.247
High school province PEI	0.513	0.387	−0.108	0.364	0.391	0.337
High school province NS	0.690	0.209	0.012	0.196	−0.237	0.219
High school province NB	0.368	0.234	0.272	0.184	−0.244	0.243
High school province QUE	0.553	0.156	−0.012	0.130	−0.102	0.140
High school province MAN	0.168	0.220	−0.654	0.211	−0.042	0.182
High school province SAS	0.575	0.199	−0.471	0.216	0.080	0.189
High school province ATA	0.626	0.191	−0.041	0.170	−0.103	0.187
High school province BC	0.629	0.174	−0.271	0.168	−0.375	0.181
Birth rank first-born	−0.046	0.112	−0.066	0.106	0.013	0.110
Siblings 1 sister	0.169	0.133	0.068	0.129	−0.153	0.130
Siblings 2 sisters	−0.111	0.155	0.129	0.140	−0.336	0.146
Siblings 3 sisters	−0.108	0.147	−0.204	0.144	−0.406	0.147
Siblings 1 brother	−0.073	0.134	−0.256	0.123	−0.346	0.129
Siblings 2 brothers	−0.061	0.153	−0.165	0.143	−0.223	0.148
Siblings 3 brothers	−0.020	0.154	−0.202	0.148	−0.338	0.157
Constant	−6.749	0.465	−6.386	0.432	−5.011	0.381
N. obs. & support	Off	On	Off	On	Off	On
Untreated (high school)	30	615	14	631	42	584
Treated (other education)	0	256	0	284	4	257
Total	30	871	14	915	46	841

NOTES: The reference group is high-school graduates for each estimation. Sample as defined in table 1.

TABLE A3
Propensity score matching, women

	Trades		College		University	
Education equation	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.
Mother educ. trades, college	−0.311	0.363	0.041	0.309	−0.096	0.278
Mother educ. high school	−0.432	0.351	−0.252	0.301	−0.370	0.269
Mother educ. below high school	−0.349	0.343	−0.254	0.297	−0.769	0.266
Father educ. trades, college	0.238	0.305	−0.086	0.222	−0.445	0.213
Father educ. high school	0.313	0.301	−0.095	0.217	−0.608	0.216
Father educ. below high school	0.124	0.275	−0.449	0.195	−1.031	0.186
Age 20–24 years old	5.665	0.385	6.489	0.333	6.231	0.516
Age 25–29 years old	5.998	0.381	6.890	0.324	6.952	0.508
Age 30–34 years old	5.889	0.378	6.820	0.320	6.679	0.507
Age 35–39 years old	6.025	0.378	6.652	0.320	6.634	0.507
Age 40–44 years old	5.778	0.383	6.645	0.324	6.574	0.510
Age 45–49 years old	5.918	0.395	6.819	0.331	6.324	0.528
Age 50–54 years old	6.013	0.423	6.911	0.354	7.008	0.532
Age 55–59 years old	5.458	0.454	6.552	0.361	6.687	0.541
High school province NFL	1.116	0.242	0.302	0.209	0.342	0.249
High school province PEI	0.122	0.326	−0.343	0.259	−0.174	0.316
High school province NS	0.520	0.228	−0.516	0.209	0.255	0.198
High school province NB	0.279	0.255	0.282	0.168	0.279	0.210
High school province QUE	0.338	0.176	0.011	0.124	−0.025	0.150
High school province MAN	0.288	0.230	−0.226	0.179	−0.016	0.199
High school province SAS	0.485	0.192	−0.770	0.182	−0.444	0.199
High school province ATA	0.252	0.211	−0.482	0.170	−0.239	0.187
High school province BC	0.126	0.199	−0.761	0.167	−0.846	0.206
Birth rank first-born	0.088	0.122	−0.030	0.102	0.082	0.113
Siblings 1 sister	−0.238	0.146	−0.075	0.122	−0.045	0.144
Siblings 2 sisters	−0.425	0.166	−0.210	0.135	−0.124	0.154
Siblings 3 sisters	−0.328	0.167	−0.102	0.137	−0.174	0.163
Siblings 1 brother	−0.044	0.144	−0.019	0.118	−0.019	0.131
Siblings 2 brothers	−0.007	0.163	−0.021	0.133	−0.005	0.151
Siblings 3 brothers	0.132	0.158	0.119	0.135	0.073	0.155
Constant	−6.429	0.556	−6.091	0.469	−5.594	0.606
N.Obs. & support	Off	On	Off	On	Off	On
Untreated (High school)	34	553	34	553	52	501
Treated (Other education)	1	198	11	413	14	275
Total	35	751	45	966	66	776

NOTES: The reference group is high-school graduates for each estimation. Sample as defined in table 1.

TABLE A4
Heckman correction estimator, men, log hourly wages

	Trades		College		University	
	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.
EDUCATION EQUATION (1st stage)						
Mother education: trades, college	−0.078	0.268	0.214	0.270	−0.168	0.237
Mother education: high school	−0.167	0.253	0.088	0.261	−0.244	0.224
Mother education: below high school	−0.380	0.246	−0.091	0.256	−0.398	0.220
Father education: trades, college	0.542	0.255	0.434	0.232	−0.441	0.199
Father education: high school	0.057	0.256	0.215	0.230	−0.555	0.193
Father education: below high school	0.251	0.232	0.112	0.210	−0.954	0.174
Age: 20–24 years old	5.192	0.346	5.739	0.494	5.371	0.279
Age: 25–29 years old	5.694	0.329	6.119	0.490	6.362	0.239
Age: 30–34 years old	5.939	0.320	6.126	0.489	6.569	0.229
Age: 35–39 years old	5.956	0.321	5.858	0.493	6.439	0.233
Age: 40–44 years old	5.970	0.327	6.169	0.495	6.642	0.239
Age: 45–49 years old	5.984	0.338	6.114	0.500	6.688	0.248
Age: 50–54 years old	5.458	0.402	6.038	0.524	6.434	0.276
Age: 55–59 years old	5.570	0.361	5.726	0.517	6.699	0.327
High school province: NFL	0.976	0.202	−0.202	0.222	−0.277	0.246
High school province: PEI	0.510	0.385	−0.120	0.363	0.395	0.336
High school province: NS	0.693	0.209	0.008	0.196	−0.237	0.219
High school province: NB	0.369	0.235	0.277	0.183	−0.244	0.243
High school province: QUE	0.555	0.156	−0.011	0.130	−0.102	0.140
High school province: MAN	0.170	0.220	−0.651	0.211	−0.041	0.182
High school province: SAS	0.576	0.199	−0.473	0.216	0.080	0.189
High school province: ATA	0.629	0.191	−0.041	0.170	−0.103	0.187
High school province: BC	0.631	0.174	−0.271	0.168	−0.376	0.181
Siblings: 1 sister	0.177	0.132	0.080	0.128	−0.155	0.129
Siblings: 2 sisters	−0.098	0.151	0.151	0.136	−0.340	0.143
Siblings: 3 or more sisters	−0.092	0.141	−0.179	0.138	−0.410	0.142
Siblings: 1 brother	−0.066	0.133	−0.248	0.122	−0.348	0.128
Siblings: 2 brothers	−0.050	0.151	−0.149	0.141	−0.227	0.144
Siblings: 3 or more brothers	−0.005	0.149	−0.180	0.144	−0.341	0.154
Constant	−6.789	0.454	−6.430	0.557	−5.418	0.334
Hazard = $\rho\sigma$	−0.199	0.079	−0.023	0.078	−0.027	0.055
WAGE EQUATION (2nd stage)						
Marital status: married	0.142	0.042	0.174	0.041	0.130	0.043
Marital status: div./wd./sep.	0.097	0.057	0.157	0.060	0.061	0.061
Province: NFL	−0.336	0.070	−0.320	0.068	−0.350	0.071
Province: PEI	−0.253	0.110	−0.252	0.102	−0.242	0.093
Province: NS	−0.201	0.066	−0.139	0.061	−0.170	0.065
Province: NB	−0.189	0.071	−0.189	0.060	−0.179	0.072
Province: QUE	−0.104	0.049	−0.051	0.042	−0.053	0.046
Province: MAN	−0.156	0.062	−0.152	0.062	−0.180	0.058
Province: SAS	−0.151	0.067	−0.162	0.067	−0.138	0.064
Province: ATA	−0.081	0.055	−0.082	0.050	0.004	0.052
Province: BC	−0.003	0.053	−0.009	0.049	−0.003	0.050
Immigrant	−0.044	0.066	0.000	0.066	−0.162	0.063
Experience	0.027	0.005	0.032	0.004	0.038	0.005
Experience squared	0.000	0.000	−0.001	0.000	−0.001	0.000
Children at home: 1	−0.029	0.048	−0.035	0.048	0.002	0.052
Children at home: 2	−0.012	0.044	−0.029	0.044	−0.004	0.047
Children at home: 3 or more	0.002	0.062	−0.038	0.062	0.030	0.063
Union/Collective agreement	0.281	0.028	0.259	0.028	0.218	0.030
Education dummy	0.457	0.130	0.227	0.127	0.454	0.085
Constant	2.161	0.050	2.178	0.058	2.162	0.050

NOTES: The reference group is high-school graduates for each estimation. Sample as defined in table 1.

TABLE A5
Heckman correction estimator, women, log hourly wages

	Trades		College		University	
	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.
EDUCATION EQUATION (1st stage)						
Mother education: trades, college	−0.303	0.363	0.040	0.309	−0.091	0.277
Mother education: high school	−0.425	0.351	−0.253	0.301	−0.368	0.269
Mother education: below high school	−0.339	0.343	−0.256	0.297	−0.765	0.266
Father education: trades, college	0.242	0.306	−0.085	0.222	−0.453	0.213
Father education: high school	0.307	0.301	−0.095	0.217	−0.614	0.216
Father education: below high school	0.121	0.276	−0.447	0.195	−1.045	0.185
Age: 20–24 years old	5.622	0.384	6.477	0.386	6.285	0.263
Age: 25–29 years old	5.959	0.380	6.877	0.382	7.007	0.245
Age: 30–34 years old	5.847	0.377	6.807	0.380	6.731	0.242
Age: 35–39 years old	5.985	0.377	6.638	0.382	6.692	0.241
Age: 40–44 years old	5.743	0.382	6.628	0.386	6.636	0.249
Age: 45–49 years old	5.883	0.394	6.802	0.392	6.388	0.283
Age: 50–54 years old	5.982	0.422	6.894	0.412	7.074	0.291
Age: 55–59 years old	5.429	0.452	6.537	0.416	6.168	0.536
High school province: NFL	1.116	0.242	0.301	0.209	0.351	0.249
High school province: PEI	0.119	0.325	−0.344	0.259	−0.170	0.315
High school province: NS	0.520	0.228	−0.515	0.209	0.252	0.198
High school province: NB	0.270	0.255	0.282	0.168	0.281	0.210
High school province: QUE	0.331	0.176	0.012	0.124	−0.026	0.150
High school province: MAN	0.284	0.230	−0.229	0.179	−0.017	0.199
High school province: SAS	0.478	0.192	−0.769	0.182	−0.450	0.199
High school province: ATA	0.236	0.210	−0.480	0.170	−0.248	0.187
High school province: BC	0.119	0.199	−0.760	0.166	−0.852	0.206
Siblings: 1 sister	−0.260	0.143	−0.071	0.121	−0.064	0.141
Siblings: 2 sisters	−0.453	0.161	−0.202	0.132	−0.147	0.151
Siblings: 3 or more sisters	−0.366	0.158	−0.092	0.133	−0.207	0.156
Siblings: 1 brother	−0.055	0.143	−0.015	0.117	−0.026	0.131
Siblings: 2 brothers	−0.029	0.160	−0.014	0.130	−0.025	0.148
Siblings: 3 or more brothers	0.108	0.154	0.129	0.132	0.050	0.151
Constant	−6.329	0.549	−6.097	0.474	−5.586	0.364
Hazard = $\rho\sigma$	−0.304	0.099	−0.296	0.067	−0.119	0.043
WAGE EQUATION (2nd STAGE)						
Marital status: married	0.059	0.037	0.056	0.033	0.078	0.034
Marital status: div./wd./sep.	0.052	0.048	0.103	0.042	0.061	0.047
Province: NFL	−0.574	0.100	−0.358	0.073	−0.218	0.076
Province: PEI	−0.213	0.091	−0.137	0.082	−0.166	0.082
Province: NS	−0.340	0.071	−0.205	0.068	−0.259	0.054
Province: NB	−0.226	0.073	−0.284	0.056	−0.214	0.060
Province: QUE	−0.144	0.054	−0.086	0.043	−0.052	0.043
Province: MAN	−0.181	0.069	−0.133	0.058	−0.148	0.057
Province: SAS	−0.289	0.064	−0.129	0.066	−0.199	0.059
Province: ATA	−0.092	0.055	−0.066	0.051	−0.072	0.047
Province: BC	−0.007	0.054	0.144	0.054	0.039	0.048
Immigrant	0.068	0.067	0.001	0.055	−0.023	0.057
Experience	0.026	0.005	0.031	0.004	0.033	0.004
Experience squared	0.000	0.000	−0.001	0.000	−0.001	0.000
Children at home: 1	−0.012	0.038	−0.031	0.035	0.001	0.038
Children at home: 2	−0.012	0.043	−0.003	0.037	−0.009	0.041
Children at home: 3 or more	0.092	0.063	0.016	0.061	0.070	0.062
Union/Collective agreement	0.277	0.030	0.252	0.027	0.235	0.029
Education dummy	0.536	0.166	0.665	0.106	0.582	0.066
Constant	2.002	0.055	1.874	0.062	1.982	0.048

NOTES: The reference group is high-school graduates for each estimation. Sample as defined in table 1.

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