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Does Temporary Interruption in Postsecondary Education Induce a Wage Penalty? Evidence from Canada

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Abstract

Almost 40% of Canadian youth who left postsecondary education in 1999 had returned two years later. This paper investigates the extent to which schooling discontinuities affect post-graduation starting wages and whether the latter are influenced by the reasons behind these discontinuities. We use data from the 2007 National Graduate Survey. We apply Lewbel's (2012) generated instruments approach. The source of identification is a heteroscedastic covariance restriction of the error terms that is a feature of many models of endogeneity. We also perform two-stage quantile regressions. We find a positive effect on wages of temporary interruption for men who held a full-time job during their out-of-school spell(s). Both men and women witness a wage decrease if their interruption depends on health issues. Women bear a wage penalty if their interruption is due to a part-time job, to lack of money, or is caused by reasons other than health, work, and money.

Keywords : Schooling Interruption, Wages, Temporary Attrition, Postsecondary Education, Lewbel IV, Two-Stage Quantile Regression, Box-Cox.

JEL Classification : C21, C26, C31, I21, I23, I26.

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1 Introduction

The standard human capital theory (Becker, 1964; Mincer, 1974) predicts that dropping out from school leads to a wage penalty. This result can be explained by the Mincer (1958) schooling model of earnings. His approach is based on an elementary formulation of the *compensating differences theory* which assumes that individuals only differ by the number of schooling years. Those who choose to work in occupations that call for more schooling require a compensating wage differential, given the additional (direct and indirect) costs associated with higher education (*supply side*). Moreover, competition between employers leads them to bid up wages to attrack more educated workers, given their higher marginal productivity (*demand side*)[1].

Another Mincerian (1974) model explaining the earnings-schooling-experience relationship is the so-called *accounting identity model* (see Heckman *et al.*, 2006). This framework focuses on the life cycle dynamics of earnings and on the relationship between observed earnings, potential earnings, and human capital investment. Although these two theoretical models are motivated differently, they algebraically yield a similar specification of individual earnings (in log) as linearly increasing with schooling, the latter approach also including a quadratic expression in work experience.

However, most of the studies that address the issue of the economic consequences of schooling interruption, examine dropping out as a *permanent* decision. Little attention has so far been given to the effect of *temporary* dropout on future wages despite the substantial number of dropouts who at some point decide to re-enroll and complete their education. In Canada, one of the reasons why this phenomenon received little interest was the lack of data. However, since the emergence of new data sets [*e.g.*, the Youth in Transition Survey (YITS)[2], the National Graduate

[1]One potential limitation of the standard human capital theory is that in this approach a diploma is just a *piece of paper* which, *per se*, does not affect productivity (see Clark and Martorell 2014). Therefore, *for a fixed number of completed schooling years*, the model predicts the same impact on future earnings of dropping out from school before finishing or not dropping out while obtaining a diploma. On the contrary, the signaling theory (Spence 1974) suggests that dropping out from school may *per se* induce a wage penalty as long as holding a diploma is the source of a signaling value. However, Clark and Martorell found little evidence of diploma signaling effects at least at the high school level.

[2]The YITS was undertaken by Statistics Canada and has tracked the behavior of a representative sample of post-

Survey (NGS)[3]], the issue of persistence in postsecondary education in Canada has been much more examined. Thus, using the YITS, Finnie and Qiu (2008) report a dropout rate of 26% for university students and 32% for college students. However, these rates do not take into account those who re-enroll at a later date. The same study states that about 40% of university attrition and 54% of college attrition are temporary. Lambert *et al.* (2004) use information from the YITS provided in both 2000 and 2002 about the students postsecondary experiences, and find that 38% of those who left postsecondary education (PSE) between the ages of 18 and 20 had returned within two years. Temporary schooling interruption thus characterizes a quite significant part of postsecondary students. Given the observed commonness of such a behavior[4], an in-depth analysis of its impact on future wages seems to us most timely and relevant.

The current study investigates the extent to which temporary schooling interruption affect post-graduation starting real wages and whether the latter are differently influenced by the reasons behind this interruption. To perform our econometric analysis, we use the retrospective 2007 NGS data targeting Canadian men and women who graduated from a postsecondary public institution in 2005. We focus on individuals who were under the age of 35 in 2007, who did not enroll in another PSE program between their graduation in 2005 and 2007, and who worked full time at least once between 2005 and 2007[5]. It is important to mention that we do not analyze the decisions to drop out *and* to return to school as our model applies to the subpopulation of young postsecondary graduates who happen to be working at some point within two years after graduation and who did not pursue further education since their graduation[6].

secondary students over time at two-years intervals since 1999.

[3]The NGS was undertaken by Statistics Canada. It targets graduates from Canadian public postsecondary education institutions (universities, colleges, trade schools), and aims to obtain information on their occupational achievements, focusing on employment, occupations, and the relationship between jobs and education. The NGS interviews graduates two and five years after graduation. To date, seven graduating classes have been surveyed: 1982, 1986, 1990, 1995, 2000, 2005 and 2009/2010. Note that the 2013 NGS (Class of 2009/2010) was conducted three years after graduation, whereas previous NGS were conducted two years after graduation. Note also that there is no follow-up survey for the 2007 NGS (class of 2005).

[4]See the literature review section for more references relating to Canada and the U.S.

[5]Bayard and Greenlee (2009) also use data from the 2007 NGS and provide a descriptive analysis of the educational experiences, labor market outcomes and financing of higher education of the 2005 graduates. Ferguson and Wang (2014) perform a similar analysis using data from the 2013 NGS.

[6]However, we do take into account the endogeneity of temporary dropout and the level of schooling years corre-

Figure 1 displays this paper's basic issue. Consider two individuals, i and j, both graduate from the same and unique postsecondary program, and start working at the same time just after their graduation[7]. However, individual i whose educational and occupational paths are represented by the star symbols, completes her education without interruption, while agent j interrupts her education for a period of time before resuming her schooling until graduation (the horizontal discontinuous lines). The two do not necessarily start their program at the same time. We assume that the wage path of the interrupter can follow three scenarios, which are plotted on Figure 1: (i) if the temporary interruption increases subsequent real wage rates (at a constant level, for simplicity), the wage profile of the interrupter is represented by the solid line, (ii) if the interruption does not affect wages, the interrupter, and the non-interrupter would earn the same amount of income, which is shown by the star and circle symbols. The third scenario (iii) according to which temporary interruption reduces wages (at a constant level), is represented by the dotted line. In this paper, we investigate which post-graduation starting real wage would have an interrupter as compared with a non-interrupter, conditional on the levels of schooling and (full-year) work experience, and given the reason of her schooling interruption.

According to an extended version of the human capital theory, temporary interruption of schooling is expected to reduce real wage rates since it is likely to lead to human capital depreciation and obsolescence[8]. The literature that tackles skill deterioration usually relates it to employment interruptions, particularly in the form of unemployment spells or family leaves (Mincer and Ofek, 1982; Möller, 1990; Pissarides, 1992; Laroche *et al.*, 1999; Albrecht *et al.*, 1999; Gregory and Jukes, 2001; Baum, 2002). The argument behind this is that human capital deterio-

sponding to complete postsecondary education.

[7]For the moment, we assume that they do not work before graduating. This assumption will be removed later. We also suppose that tuition fees are free.

[8]From the perspective of the signaling theory (Spence, 1974), the impact of temporary schooling interruption (followed by a completed diploma) on future wage rates is ambiguous. On the one hand, interrupters may convey a signal of lack of commitment, perseverance, and motivation that may be responsible for lower returns. At the same time, a student who takes a break from formal education to travel around the world and gain a broader horizon on life or make a contribution to society may reveal character traits that are appreciated by employers. Since the signaling theory does not provide a non-ambiguous prediction on the impact of temporary schooling interruption on subsequent wage rates, we rather focus on the human capital approach.

rates when it is idle. Similar logic extends to temporary schooling interruption since out-of-school spells may influence the efficiency of acquisition and maintenance of cognitive skills, and thus impair the abilities that individuals have acquired throughout their program. In particular, skills acquired before the interruption may become less worthwhile as knowledge becomes obsolete and students forget their past learning. This form of depreciation is known as human capital atrophy and was introduced by Mincer and Polachek (1974).

Of course, the reason why a student lives a period of schooling interruption[9] is likely to play a crucial role on skill depreciation and therefore on subsequent real wages. The NGS provides information on five main reasons why a temporary interruption occurred. These include the following: lack of money, because of health issues, the respondent had a part-time job, a full-time job, or other reasons[10]. Now if a respondent interrupted studies due to lack of money or health issues, this suggests that she acquired her pre-graduation full-time work experience (if any), and her years of schooling before interruption, earlier than that of a non-interrupter with similar characteristics. Moreover, bad health in the interruption period may also reduce the quality of pre-interruption schooling and experience. Given the atrophy phenomenon, one should thus expect interruption for lack of money or health issues to have a negative effect on subsequent real wage rates.

Now let us focus on respondents who spent some time working during their pre-graduation period and assume that schooling interruption (if any) is whether due to full-time or part-time jobs. Here, the discontinuity effect on post-graduation wages is ambiguous. On the one hand, an interrupter who enters the labor market will acquire some pre-graduation work experience *after* her counterfactual non-interrupter. This will have the effect of increasing the former's post-graduation wages, at a given level of schooling and experience. This is due to the more depreciated experi-

[9]In this study, schooling discontinuity is defined as any break or leave of absence taken during a degree, and causing the delay of its completion. Our definition excludes out-of-school spells taken between two degrees, and degree-related gap years required by certain educational programs.

[10]The latter include pregnancy/carrying for own child, family obligation, because they only needed a few courses, or because their program was not offered full time. The NGS questionnaire also allows respondents to report whether the interruption had occurred because of another non-specified reason. This category could, for example, include lack of interest, academic difficulties, lack of career planning, or taking time off to travel.

ence acquired in the past by the non-interrupter. The positive effect on the wage rate may also be explained by the fact that the job landed by the interrupter during an out-of-school spell had higher schooling requirements compared to the one landed by her counterfactual non-interrupter. This is more likely the case for full-time work than for part-time work. On the other hand, the quality of the interrupter's pre-interruption schooling is reduced as compared to the one of the non-interrupter, due to the atrophy phenomenon. This will decrease the interrupter's post-graduation wage rate as compared to the one of the non-interrupter.

The potential endogeneity of schooling and covariates reflecting the reasons of schooling interruption should not be ignored when examining their impact on wages. Indeed, they may be correlated with unobserved variables such as ability and learning motivation that in turn influence wages. Measurement error could also arise. Since we do not have a sufficient number of valid external instruments to circumvent the endogeneity problem (we need at least six instruments), we resort to the Lewbel (2012) two-stage heteroskedasticity-based instrument approach. To identify the model, this method exploits the conditional second moments of the data, under heteroskedasticity of the error terms of the endogenous regressors. As shown by Lewbel, these assumptions are satisfied by (but not limited to) models in which error covariances across equations arise due to an unobserved common factor. As an example, learning motivation may influence both schooling interruption and subsequent real wage rates.

The Lewbel approach provides generated instruments from the sample data that can be constructed from the error terms of the endogenous regressors, multiplied by at least a subset of the included exogenous variables. A number of researchers recently applied Lewbel's technique (*e.g.*, Sabia, 2007; Emran and Hou, 2008; Kelly and Markowitz, 2009; Millimet and Roy, 2016), and conclude that Lewbel's instruments perform well by yielding strong first stage F-statistics and satisfying the overidentification test. We also provide such tests in our paper and we conclude that our instruments are generally relevant and exogenous. Moreover, we present a description of the Lewbel approach.

Note that Klein and Vella (2010) also propose a strategy to circumvent the need of valid exclusion restrictions. Like Lewbel, they exploit conditional second moments to obtain identification. However, contrary to Lewbel, their approach assumes a multiplicative form of heteroskedasticity which imposes additional restrictions on how the third and higher moments of the error terms depend on regressors[11]. Millimet and Roy (2016) provide a detailed analytical and empirical comparison between these two procedures. Applying them on the pollution haven hypothesis, both approaches lead to similar qualitative results. Therefore, we will focus on the Lewbel approach.

Besides Lewbel's generated instruments, we have also constructed three external instruments: the change in real average annual tuitions fees by province and program of study, the change in annual average unemployment rates by province, and the mother's education level. These instruments are also used in some specifications to improve the efficiency of our estimates.

In short, our paper provides three contributions. First, to the best of our knowledge, our study is the first one to address the issue of the impact of temporary schooling interruption on subsequent wage rates using Canadian data. The availability of new data from the 2007 NGS where graduates from Canadian public postsecondary education institutions were interviewed two years (2007) after graduation in 2005, provided us with the information needed to perform our analysis. Furthermore, while most of earlier efforts on this topic focus on the effect of interrupted schooling on white American men's outcomes, the current study uses more recent data and extends the analysis to include both men and women, while controlling for racial identity.

Second, our modeling strategy does not only test the extent to which schooling discontinuities affect post-graduation starting real wage rates, but also investigates whether labor market outcomes are affected by the reasons behind these discontinuities. To the best of our knowledge, earlier efforts do not test the latter hypothesis and implicitly assume that the effect of an interruption is the same regardless of the reason that has caused it. We test and reject the equality of coefficients across the reasons-related covariates, which justifies the importance to control for

[11] Klein and Vella (2009) apply their approach to estimate the return to schooling when education is treated as endogenous. They obtain an estimate of 10% in contrast to the OLS estimate of 6%.

them. In particular, we find that the different reasons for interruption seem to balance each other out in their effects on men's post-graduation starting wage rate.

Third, our estimation techniques based on Lewbel's generated instruments depart from our precursors. This procedure allows to account for the endogeneity of schooling and reasons for schooling interruption. Moreover, we perform robustness checks to see how our estimates behave when the functional form of the wage equation is modified. We first look at whether the natural logarithm is the appropriate transformation of wage rates. Then, we re-estimate our equations after adding a quadratic term in schooling, and including higher order polynomials in work experience. We also estimate a wage equation in which log real wage rate is not an additively separable function of schooling and experience. Finally, we perform two-stage quantile regression with endogenous covariates (see Chernozhukov and Hansen, 2005), using Lewbel's and external instruments. The use of quantile regressions is appealing in our context since standard linear regression techniques provide only a partial picture of the relationship between our variables of interest. Indeed, it might be interesting to obtain a more comprehensive analysis of the relationship at different points in the conditional distribution of our dependent variable and thus to allow for the presence of heterogeneity in the effects of schooling interruption on subsequent wages. In addition, quantile regression is more robust to outliers than standard regressions. These specification checks suggest that our estimates of the causal effects of schooling interruption reasons on subsequent wage rates are plausible and robust.

Based on our specification combining generated and external instruments, our results find that, conditional on the levels of education and work experience, temporary schooling interruption leads to a 21% increase in starting wages for men who had worked full-time during their out-of-school spell(s). Both men and women witness a wage decrease of 21% and 14% respectively if their interruption is associated with health issues. Women also bear a wage penalty of 28% if their interruption is due to a part-time job, of 35% if it is money-related, and of 13% if it is caused by other reasons.

This paper proceeds as follows: Section 2 provides a survey of the literature on the issue of interrupted schooling. Section 3 describes the data and provides some descriptive statistics. Section 4 specifies the wage equations. Section 5 provides a brief description of Lewbel's procedure. Section 6 presents our findings. Section 7 contains robustness checks. Section 8 concludes.

2 Literature Review

Until recently, research on the subject of discontinuous patterns of school attendance has been more extensive in American studies. However, the emergence of new data sets (*e.g*, YITS, NGS) has enabled researchers to examine this issue more carefully for Canada. Doray *et al.* (2012) use data from the YITS and show that the re-enrollment in postsecondary education is more common during the first three quarters after the decision to interrupt. Finnie and Qiu (2008) also analyze data from the YITS and find that "*by one year after first having left school, 22.3% of college leavers and 35.6% of university leavers have returned. By three years later ... the returns stand at 40.3% and 54%, respectively, for college and university leavers*". In the same vein, Shaienks *et al.* (2008) employ data from the first four cycles of the YITS and report that 35% of those who had dropped out relatively early in their studies returned within two years, and 46% within four years. Lambert *et al.* (2004) use information provided in both 2000 and 2002 about the students postsecondary experiences, and find that 38% of those who left postsecondary education between the ages of 18 and 20 had returned within two years.

American studies show similar results. Stratton *et al.* (2008) analyse a multinomial logit model of college *stopout* and *stayout* behaviors[12]. They use data from the 1990/94 Beginning Post-secondary Survey and find that 40% of all first-year attrition is temporary. Horn (1998) tracks the path of undergraduates who left college in their first year to examine their educational experiences. She reports that almost 30% of students enrolled do interrupt during their first year. She states that

[12]The term *Stopout* refers to temporary dropouts who return to school. Permanent dropouts who do not re-enroll are called *stayouts*.

64% of the students who left the 4-year sector, and 50% of those who left the public 2-year sector, returned within 5 years.

2.1 Who Interrupts and Why?

Discontinuous schooling can be explained by different causes. In our empirical analysis, we rely on reasons provided by the NGS data where students reported their own (retrospective) explanations. The design of the NGS questionnaire allows interrupters to specify if the interruption was caused by lack of money, health issues, part-time work, full-time work, pregnancy/carrying for own child, family obligation, only needing a few courses, or the program was not offered full-time. Now if the interruption was driven by a reason that is different from the above-mentioned ones[13], the questionnaire allows respondents to indicate so. However, no further information about the specifics of that reason is provided.

Besides, Finnie and Qiu (2008) exploit data from the YITS and derive that "*students leave school because the schooling is judged not to be the right thing for them or they want to do other things such as work, make a change or take a break*". Furthermore, previous Canadian research reveals that "lack of interest" comes out among the most reported reasons for discontinuing studies, implying that motivation plays an important role with respect to the continuity of postsecondary education. Other reasons include lack of career planning or academic difficulties (Berger et al., 2007). In line with these findings, Shaienks and Gluszynki (2007) analyze data from the first four cycles of the YITS and investigate the reasons for dropping out from postsecondary education. They examine two separate groups of dropouts : those who borrowed to finance their postsecondary education and those who did not. Among those who did borrow money to finance their studies, the most often cited reasons for leaving school were the fact that they did not like their program, or not having enough money (both at 18%), and wishing to work (17%). The most frequently mentioned reasons for dropping out among those who did not borrow money were: not

[13] As mentioned earlier, such reasons could, for example, include lack of interest, academic difficulties, lack of career planning, or taking time off to travel.

liking the program (29%), wanting to work (15%) and lack of money (13%). Moreover, Lambert (2004) examines data from the two first cycles of the YITS. According to his analysis, the main reported reason for dropping out is related to lack of program fit. More precisely, about one-third of dropouts reported that they did not like their program, or their program was not for them. The same study states that 9% of dropouts cited that they were going to change program or schools, and 11% left because they did not have enough money.

In addition to the self-reported explanations, the literature provides different portraits of the attributes and factors associated with interrupted schooling. Although coverage of these explanations is beyond the scope of this study, it seems vital to understand the determinants of this behavior. In addition, we use this literature to select external instruments in order to take into account the endogeneity problem of the interruption-related variables.

Finnie and Qiu (2008) use a logit model in which the dependent variable is whether a student who quit PSE returned in the subsequent years. They conclude that being a woman increases the probability of returning. They also find that younger college students are more likely to return than older ones, and that students with more educated parents are more likely to return to PSE if they do leave than students with less educated parents[14]. Beverly Duncan (Duncan *et al.* 1972) states that *"elements of the family's structure and status which are conducive to high educational attainment are also conducive to continuity in schooling"*. Her conclusions are supported by the findings of many other studies, according to which the decision of young adults to resume schooling varies according to socio-demographic characteristics, some traits of transition to adulthood as well as current living conditions (Doray *et al.*, 2012; Seftor and Turner, 2002; Thomas, 2001; Smart and Pascarella, 1987). Among the socio-demographic information that our dataset provides, we select the mother's education level as one of the external instruments.

Moreover, the motivation to re-enter higher education is defined by other factors such as the characteristics of the previous academic experience (Smart and Pascarella, 1987; Doray*et*

[14] Among university students, there are no significant differences by age, and the effects of parental education are ambiguous.

al., 2012; Seftor and Turner, 2002). Studies show that the chances of graduating for an interrupter increase with the length of her prior attendance (Eckland, 1964), and that the prior academic success reduces the duration to re-enrolling (Thomas, 2001). The level of the previous program also acts as a predictor of returning to school. It is also established that the decision to re-enter higher education is influenced by changes in tuition levels, the first year financial aid type (Stratton *et al.*, 2008) and other financial considerations (Horn, 1998). Based on these findings, we construct an instrument that measures the changes in tuition fee levels since they reflect the cost of schooling when the individual was making schooling-related decisions.

Previous studies have also established that the decision to go back to school is closely associated with labor supply decisions as well as earnings opportunities (Altonji, 1993). Weiss (1971) argues that individuals who plan to switch jobs in response to changes in relative wages, may return to school in order to acquire new skills required by their new occupations. Marcus (1986) also estimates a discrete time model of the probability of re-enrollment in school for individuals who are currently out-of-school. Marcus finds that a significant predictor of returning to school is earnings below expected gain in the National Longitudinal Survey (NLS) Young Men sample. Light (1996) extends the analysis of schooling demand to a dynamic framework by estimating a continuous time hazard model for out-of-school spells. She finds that local unemployment rates and wage rates are significant determinants of student re-enrollment. We therefore use changes in unemployment rates as another external instrument to control for the potential endogeneity of the interruption-related variables.

2.2 Survey of Empirical Findings

Empirical efforts that tackle the issue of the economic consequences of temporary interruption in postsecondary education are rather scarce and have yet to reach a consensus. Note that, contrary to our approach, no study takes reasons for schooling interruption into account in their analysis. In this subsection, we separate the existing literature into two sets of studies according to their findings.

The first set of contributions finds a negative effect of temporary schooling interruption on educational and occupational achievements. These studies include the work of Duncan *et al.* (1972) who point out that diminished occupational status attainment could be related to schooling discontinuities.

Griliches and Mason (1972) use a sample of post-World War II veterans whose schooling interruptions were caused by military enlistment. The sample was drawn from the 1964 Current Population Survey. They find that the duration of military service significantly reduced wages. Our analysis does not take the duration into account since our survey does not provide information on this variable[15].

Featherman and Carter (1976) use a 1939-40 birth cohort of Michigan men to point out that men who either had postponed enrollment into college after leaving high school or interrupted their college matriculation, achieved less education than those who experienced continuous enrollment. They also argue that, for men who completed equivalent levels of education, the college matriculants secured a more prestigious first full-time job than did the non-regular school graduates. Featherman and Carter (1976) conclude that discontinuities in schooling impede socio-economic achievement for a number of reasons, including the fact that *"societies normally process age-specific cohorts, failure to retain membership in a cohort as it is processed into the labor market handicaps men vis-à-vis their former associates"*. However, their analysis does not investigate whether socio-economic achievements are also affected by the reasons of interrupted schooling.

In the same vein, Robertshaw and Wolfle (1983) use a national sample of U.S. white men and confirm Featherman and Carter's results according to which educational discontinuities impede educational attainment. They state that when people delay entry into higher education, or interrupt their schooling once enrolled, it costs them about a half year of education.

Light (1995) also examines the effects of interrupted schooling on wages. She uses data

[15] In another study, Griliches (1980) also controls for the length of the schooling interruption. The estimated coefficient is not statistically significant.

from the National Longitudinal Survey of Youth and finds that young men who interrupt their schooling receive wage boosts that are smaller than those received by their continuously enrolled counterparts. However, the wage gap disappears after certain years of experience. The panel nature of Light's data allows her to observe changes in schooling status and therefore capture enrollment spells of any length held at any time during the survey. However, Light's population of interest is limited to white men and her study ignores the reasons of interruption..

Monks (1997) shows a disparity of earnings between younger graduates and those who complete university study at a later age. The negative correlation between age at graduation and entry level wages holds also once he controls for work experience, job tenure, hours of work, measures of ability and individual fixed effects. However, in Monk's framework, delayed graduation is not necessarily caused by schooling interruption.

The second set of studies finds no significant effect of discontinuous schooling on labor market outcomes. These studies include the work of Griliches (1980) who uses 1966-70 data from the Young Men cohort of the National Longitudinal Survey and finds that wage rates were not significantly altered by schooling interruption. Griliches' population of interest only includes men. Similarly, Marcus (1984) employs the same data that Griliches (1980) uses and shows that there is no substantial difference in the rate of return to education between interrupters and non-interrupters at the same level of schooling. Contrary to our analysis that controls for different reasons of discontinuous schooling, Marcus's study only focuses on those who interrupted their schooling with years of work.

In light of the above, the most recent study that investigates this issue uses American data from 1979 to 1989. Our paper aims to shed light on this topic using more recent Canadian data and several estimation techniques. It also considers various contributions. The most important involves the estimation of the impact of several reasons of interrupted schooling on wages as it is often hypothesized in the literature that the effect is the same across the different reasons. Furthermore, while previous work mostly focuses on white men's outcomes, we extend the analysis to include both genders.

3 Data and Descriptive Statistics

Our study uses data from the 2007 NGS where graduates from Canadian public postsecondary education institutions (universities, colleges, trade schools) were interviewed two years (2007) after graduation in 2005. We opt for the NGS because its target population is the best fit for our study question. In particular, we are interested in Canadian postsecondary graduates [16].

The 2007 NGS is a sample survey with a cross-sectional design. However, a number of variables were collected retrospectively as the survey's respondents were asked about their employment history between graduation and the time of the interview, their pre-graduation work experience, their leaves of absences from studies and the reasons for the latter, and other levels of education completed before enrolling in their 2005 program. The survey also supply information on the major activities that respondents had been doing during the 12 months before enrolling in their 2005 program (school, work, family responsibilities, other, *etc*.). Stratified simple random sampling is applied within each stratum. Stratification is based on three variables which are geographical location of the institution (ten provinces and the three northern territories), level of certification (five) and field of study (12). In total, combining these variables yields 780 possible strata. The final number of strata created was 506 after excluding all strata in which there are no respondents[17].

Following the literature, our analysis is run using a sample of graduates under the age

[16] We chose to use the NGS rather than the YITS mainly because respondents in the latter survey are not necessarily postsecondary graduates at the same reference periods. Therefore, if the YITS were to be used, stricter selection criteria would have to be applied in order to investigate the effect of temporary schooling stoppage on post-graduation wages.

[17] In previous reference periods, the NGS involves a longitudinal design with graduates being interviewed at two different times: at two years and five years after graduating (follow-up) from postsecondary institutions in Canada. However, there is no follow-up survey for the class of 2005 since respondents were only interviewed once, two years after graduation. We choose to work with the class of 2005 since it was the most recent available reference period of the NGS when we started developing our study. Moreover, it corresponds to a period before the Canadian 2008-2009 Great Recession.

of 35[18], who happen to be full time employed as paid workers[19] at least once since their graduation. We also require that the respondents did not seek further education since their graduation in 2005. These selection criteria are imposed because we want to capture the immediate impact of schooling discontinuities on subsequent wages. Following Klein and Vella (2009), we consider our wage determination process as conditional on our selection criteria, and do not address any sample selectivity issues induced by the latter in our econometric analysis.

Since temporary schooling interruption is our interest, we define it as any leave of absence that a graduate took from her studies, and that delays the completion of her program. Respondents were asked about the reasons behind their discontinuous schooling (if any). Since this information was collected using post-school observations, we use the answers they provide to describe the activities they undertook during their out-of-school spells. We create dummy variables for the main reasons why graduates interrupted their education: lack of money $(Money)$, health issues $(Health)$, full-time work $(FullWork)$, part-time work $(PartWork)$. We also create a dummy that takes a value of 1 if the interruption is caused by reasons other than the four aforementioned ones $(Other)$. The latter category includes graduates who interrupted their schooling because of family obligations (15%), because they only needed a few courses, or because their program was not offered full time (5%). It also includes students who interrupted for reasons that are different from the above-mentioned ones (80%). Such reasons could, for example, include lack of interest, lack of satisfaction with the program, wanting to travel, or academic abilities. Hence, we end up with five dummies of reasons for interrupted schooling to integrate into our regressions. We exclude from our sample respondents who interrupted for pregnancy-related reasons since we only have few of them (73 observations). We also remove 49 observations that reported more than

[18]Griliches and Mason (1972) base their analysis on 1,454 full-time employed men who were between the ages of 21 and 34 and not enrolled in school. Marcus (1984) uses data from the NLS of Young Men and tracks 5,225 males between 1966 and 1973 and who were aged 21-31 years in 1973. The average male represented in his sample was 25 years old. Light (1995) focuses on 2,489 white men who range in age from 16 to 32 during the observation period. In a second study (1996), Light uses data from the NLSY and use a sample of 3,209 men who range in age from 16 to 33 between 1987 and 1990. She follows them for up to a maximum of 10 years.

[19]We exclude the self-employed as they may provide no reliable data on their earnings. Besides, our results are robust to removing the restriction that respondents happened to be full time employed.

one reason for their interrupted schooling in order to distinguish the effect of each reason on wages. After imposing the sample selection criteria described above, we are left with a sample of 9,759 graduates, divided by gender in almost equal shares (4,920 men and 4,839 women)[20].

We use the natural logarithm of the hourly starting wage rate for the first job held after graduation as the dependent variable of our baseline Mincerian wage equations. For those whose job began before they graduated and continued after graduation, we use the wage rate they earned once they completed the requirements of their program, controlling for the accumulated experience in this job. To construct this variable, information from different questions are used. In the questionnaire, respondents were requested to specify the easiest way for them to report their wage or salary (whether it is yearly, monthly, weekly, hourly or on some other basis). For those who did not provide their hourly wage rate, we use information on how many months in a year, how many weeks a month, and how many hours a week they usually worked at that job in order to compute it. Nominal hourly wages are deflated by the CPI (by province and year) in year 2000 Canadian dollars. We choose to use the hourly wage rate rather than annual earnings as a pay concept in our study since it is not influenced by the differences in the quantity of labor provided and thus allows for removal of the impact of work hours.

In order to measure labor market experience, many studies have relied on potential experience[21] (Mincer, 1974). However, using potential experience as a proxy for time spent in the labor market restrictively assumes continuous participation. In other words, it assigns the same amount of work experience to individuals with the same age and schooling but not necessarily the same labor market history. A measure of potential experience may thus suffer from measurement error and lead to biased coefficient estimates. In our estimation, we avoid this limitation by including *actual* years of full-time work experience as measured by the sum of pre-graduation full-time

[20] The initial sample counts 23,801 observations which provided complete information about the variables we use in our analysis. After imposing the age criterion, the sample is reduced to 17,466 observations. We did not loose any observations because of the employment criterion. We deleted 721 observations because they were not a paid worker, 3,131 because they were not employed full time, and 3,732 because they did enroll in another PSE program after their graduation in 2005. We also lost an observation because the respondent indicated an interruption but did not specify the reason.

[21] Potential experience is usually measured as: *age - years of schooling - 5*.

employment years.

The heterogeneity in the duration of non-work period that respondents accumulate just after their graduation could bias the estimated effects of schooling and interruption. In an attempt to remedy this problem, we control for the elapsed time between graduation and the first job landed. We define our education variable as the number of years of schooling completed. We compute this variable by generating the average curricular number of years for the highest degree attained by 2005[22].

Table 1 provides descriptive statistics on the variables used in our model for the full sample, the interrupters, and the non-interrupters in the sample. It also presents the same information broken down by gender[23]. The first column of Table 1 indicates that, on average, respondents are aged 26 with 16 years of schooling and 3 years of work experience. The average duration of the elapsed time between graduation and the first job (*Spell*) is two months. Looking at the interrupters' characteristics (see column 2), we find that 30% of interrupters are married and 15% of them belong to a visible minority. Besides, interrupters are on average a little older (aged 28) than non-interrupters (aged 26). In the Canadian Prairies, students were more likely to interrupt their PSE than anywhere else in the country. It might be because they have more opportunities to work and find an acceptable salary. In addition, columns 5 and 8 indicate that female interrupters earn slightly less than male interrupters (2.83 *vs.* 2.84). The gender wage gap is due to a variety of causes that go beyond the scope of this paper. Notice also that on average interrupters tend to accumulate more schooling than non-interrupters (17 *vs.* 16). One might speculate that those who interrupt tend to pursue programs that are longer to complete compared to non-interrupters.

Column 1 also indicates that nearly 7% of the graduates in the sample have interrupted

[22] We set years of schooling to 14 years for those with a trade/vocational diploma or college/CEGEP certificate, 15 years for university diploma or certificate below bachelor's level, 17 years for a bachelor's degree, 18 years for a university diploma or certificate above bachelor's level but below master's level, 19 years for a master's degree, and 22 years for respondents with an earned doctorate. However, some suggested that education levels should replace schooling years in the wage equation. Therefore, we replace years of schooling with different levels of education (several dummies). Little difference has been found. Moreover, Lewbel's instruments are weaker when using this approach in some specifications.

[23] In all our specifications, we rejected coefficients of equality between men and women.

their schooling (*Leave*). Slightly more than half are men. Almost 30% of interrupters report work as the reason for their discontinuous schooling. About 11% interrupted because of health issues, and around 7% of interruptions have occurred because of money-related reasons. Notice the gender difference associated with health-related interruptions. Most cases of health-related interruptions are reported by women (15% for women *vs.* 7% for men). A number of studies state that the poorer self-rated health of women is an accurate reflection of their actual health status, and that women are actually more often sick than men (*e.g.*, Malmusi *et al.*, 2012). Research also finds that women are more likely than men to visit the doctor or to have annual exams, even after excluding pregnancy-related visits[24]. Moreover, most cases of full-time work related interruptions are reported by men (22% for women *vs.* 30% for men) while part-time work-related interruptions seem to be slightly more common among female interrupters[25]. These figures mesh with the existing empirical explanation for gender differences in time allocation. Studies show that parenthood does not have the same effect on the employment patterns of both parents (Gobbi *et al.*, 2015; Barnett *et al.*, 1994). Women are more likely to alter their work paths and choose non-standard work arrangements, such as part-time employment, because such work is easily compatible with family responsibilities. Some also argue that this choice may not always be voluntary, since women face reduced full-time employment opportunities compared to men[26]. Finally, 54% of interrupters report other reasons for their discontinuous schooling, this figure being almost the same for men and women.

Table 1 also reveals that interrupters earn more than non-interrupters. One way to explain this observation is that a substantial number of interrupters leave school to work full time, which, as discussed earlier, may positively affect their future wages. In our empirical analysis, we investigate whether this hypothesis still holds once we control for observable individual heterogeneity and take the potential endogeneity of our variables of interest into account.

[24] *Vital and Health Statistics*. Series 13, Number 149. U.S Department of Health and Human Services. National Center for Health Statistics.

[25] Statistics on $PartWork$ cannot be shown due to Statistics Canada confidentiality restrictions.

[26] "Gender Equality in the Labour Market: Lessons Learned: Final Report". Evaluation and Data Development, Strategic Policy, Human Resources Development Canada (2002).

4 Specification of the Baseline Wage Equation

The current study compares wages of interrupters and non-interrupters, given their levels of schooling and work experience. To run our analysis, we start with a Mincerian wage equation, augmented by several terms to take into account the impact of the reasons for interruption during their more recent pre-graduation schooling. Since we did not reject the null hypothesis that the rate of return on schooling is the same for interrupters and non-interrupters for all specifications, we did not introduce interaction variables between schooling and schooling interruption. Therefore, we allow schooling interruption to have a level effect but not a slope effect. We use the starting hourly real wage rate (in log) for the first job held after graduation as the dependent variable of our baseline Mincerian wage equation. Such specification allows us to identify the immediate effect of this behavior on wages. As mentioned earlier, we control for five reasons of discontinuous schooling, which we integrate into the wage equation in the form of dummy variables.

Our set of explanatory variables also includes the square of the actual work experience, age, a dummy indicating whether graduates belong to a visible minority (*Visib*), a dummy that takes a value of 1 if the respondent is married (*Married*), area of work (five dummies indicating whether the respondent works in the Atlantic provinces, Ontario, the Prairie provinces, the West Coast, or Northern Canada), and the duration in months of the elapsed time between graduation and first job, (*Spell*). Our baseline wage equation (1) is hence defined as follows:

$$\begin{aligned}\log w_i = \alpha_0 + \alpha_1 Educ_i + \alpha_2 Money_i + \alpha_3 Health_i + \alpha_4 FullWork_i + \alpha_5 PartWork_i + \alpha_6 Other_i \\ + \alpha_7 Exp_i + \alpha_8 Exp_i^2 + \alpha_9 Age_i + \alpha_{10} Visib_i + \alpha_{11} Married_i + \alpha_{12} Atlantic_i + \alpha_{13} Prairies_i \\ + \alpha_{14} West_i + \alpha_{15} North_i + \alpha_{16} Ontario_i + \alpha_{17} Spell_i + \varepsilon_i,\end{aligned} \tag{1}$$

where ε_i is the error term.

In our model, the schooling variable and the interruption-related covariates except the health dummy variable are endogenous[27]. Since we do not have a sufficient number of valid external instruments to account for the potential endogeneity of these variables, we resort to the Lewbel (2012) two-stage heteroskedasticity-based instrument approach. The Lewbel method is useful when external instruments are weak or too limited in number (not enough exclusion restrictions). It generates valid internal instruments from the sample data under certain assumptions. In particular, Lewbel shows that the model is identified: 1) when the model is triangular[28], 2) the error terms of the endogenous regressors of the structural (*e.g.*, log wage) equation are heteroskedastic, 3) at least a subset of the exogenous variables of the structural equation are correlated with the variances of these errors, and 4) but they are not correlated with the covariances between these errors and the second-stage error. The next section provides a more detailed presentation of Lewbel's approach.

When estimating a Mincerian equation, one standard approach to account for endogeneity due to correlation between schooling decisions and ability or to measurement errors, is to use institutional features on the supply side of the education system as instruments. We hence create an additional external instrument that measures the change in real average annual tuitions fees by province and program of study between the year the respondent starts her studies and the year of her graduation, which is 2005 for the whole sample. Furthermore, we follow several authors (e.g., Hausman and Taylor, 1981; Corman, 1983) in constructing a second external instrument that measures the change in annual average unemployment rates by province, also between the year the respondent starts her studies and 2005. We also use the mother's education as a third external instrument. To control for the endogeneity of our covariates, we first rely on Lewbel's generated instruments alone, and then we use them along with the three aforementioned external instruments. Introducing additional instruments may help improve the estimates' efficiency. However, it is not clear that this is the case since supplementing these instruments with Lewbel's, our sample size is reduced due to observations with missing values in the external instruments (4920 *vs.* 3710 for men, and 4839 *vs.* 3451 for women).

[27]The health variable is assumed exogenous.

[28]The system being triangular is a sufficient but not necessary condition.

Aside from the continuity of the school attendance, educational patterns of graduates may vary according to several other factors. Hence, we make two assumptions in order to isolate the impact of temporary interruption on wages from other components. In particular, we assume that the intensity of enrollment (full/part-time) has no effect on wages, neither do interruptions that took place during or between previous educational programs. Furthermore, we reject equality of coefficients between men and women[29]. Consequently, we conduct separate analyses for men and women to see how the effect of discontinuous schooling on post-diploma starting real wage rates differs across gender.

5 Lewbel's Approach

This section presents a simplified version of the Lewbel (2012) approach we use to provide generated instruments from the sample data. To illustrate this approach, consider the following linear triangular model with one endogenous regressor:

$$y_1 = \mathbf{x}'\boldsymbol{\beta_1} + y_2\gamma + \varepsilon_1 \tag{2}$$

$$y_2 = \mathbf{x}'\boldsymbol{\beta_2} + \varepsilon_2 \tag{3}$$

where y_1 and y_2 are scalars of observed endogenous variables, $\mathbf{x}$ is a vector of observed exogenous regressors, and $\boldsymbol{\varepsilon} = (\varepsilon_1, \varepsilon_2)$ is a (2×1) vector of unobserved errors.

Generated instruments can be constructed from the auxiliary equation' error, ε_2, multiplied by each of the included exogenous variables in mean-centered form:

$$\mathbf{z} = (\mathbf{x} - \bar{\mathbf{x}})\varepsilon_2$$

[29] When using generated instruments only, the p-value=0.0183. If all available instruments are used, p-value =0.0121.

Traditionally, identification of this model would be reached by imposing equality constraints on some coefficients, such as assuming that some elements of $\boldsymbol{\beta_1}$ are zero since it means availability of excluded instruments (exclusion restrictions). Parameters would also be identified if the errors ε_1 and ε_2 are uncorrelated. Lewbel provides identification conditions that do not require restrictions on $\boldsymbol{\beta_1}$ or uncorrelated errors. His method achieves identification by restricting correlation of $\boldsymbol{\varepsilon\varepsilon'}$ with $\mathbf{x}$, and assuming heteroskedasticity of ε_2. In other words, identification is achieved through the presence of covariates correlated with the conditional variance of ε_2, but not with the conditional covariance between ε_1 and ε_2. More formally, all that is required for identification and estimation are the moments:

$$E(\mathbf{x}\varepsilon_1) = 0,\ E(\mathbf{x}\varepsilon_2) = 0,\ cov(\mathbf{z}, \varepsilon_1\varepsilon_2) = 0,\ \text{and}\ cov(, \mathbf{z}, {\varepsilon_2}^2) \neq 0,$$

where some or all of the elements of $\mathbf{z}$ can also be elements of $\mathbf{x}$.

As shown by Lewbel (2012), these assumptions are satisfied by (but not limited to) models in which error covariances across equations arise due to an unobserved common factor. In our context, measurement error in the schooling-related variables or an omitted index of crucial unobserved variables impacting both schooling interruption variables and wages such as learning motivation are plausible examples of such a common factor.

Lewbel shows that the structural parameters $\boldsymbol{\beta_1}$ and γ can be estimated using a two-stage least squares regression (or GMM) of y_1 on $\mathbf{x}$ and y_2 using $\mathbf{x}$ and $\mathbf{z} = (\mathbf{x} - \bar{\mathbf{x}})\varepsilon_2$ as instruments. The assumption that $\mathbf{z}$ is uncorrelated with $\varepsilon_1\varepsilon_2$ means that $\mathbf{z} = (\mathbf{x} - \bar{\mathbf{x}})\varepsilon_2$ is a valid instrument for y_2 in equation (2) since it is uncorrelated with ε_1, with the strength of the instrument corresponding to the degree of heteroskedasticity of ε_2 with respect to $\mathbf{z}$ (the correlation of the instrument with y_2 is proportional to the covariance of $(\mathbf{x} - \bar{\mathbf{x}})\varepsilon_2$ with ε_2. To construct the instruments, the residuals of the first stage OLS estimation of equation (3) are used. Note finally that tests of heteroskedasticity, overidentification and weak instruments can be performed to check the quality of the generated instruments.

Similar logic extends to the case of multiple endogenous regressors.

6 Empirical Results

Let us first focus on the various tests we performed concerning the quality of our generated instruments for men and for women. Table 2 shows the results of the underidentification, weak identification, and overidentification tests. It reveals that Lewbel's instruments, whether used separately or supplemented with external ones, are appropriate since they satisfy the relevance conditions (they are not jointly weak) and they are valid. More precisely, the underidentification test, for which the null is *no correlation* between the tested instruments and the endogenous regressors, reports the p-value of the Kleibergen-Paap LM statistic. Results reject the null and therefore underidentification. The test for weak identification is based on the Kleibergen-Paap Wald F statistic, the null being *weak correlation* between the tested instruments and the endogenous regressors in the sense that it is subject to a bias that the investigator finds unacceptably large[30]. The test also strongly rejects the null, which suggests that the tested instruments are not weak. The overidentification test reports the p-value of the Hansen J statistic. This test does not reject the instruments' validity, which means that one cannot jointly reject that they are uncorrelated with the error term of the wage equation, and that they are excluded from the latter.

Also, performing the Breusch-Pagan test, we reject that the error terms of the endogenous regressors of our model are homoskedastic. More precisely, results provided in Table 3 indicate that we reject this hypothesis at the 0.01% level for all the error terms of these regressors. This is consistent with the presence of heteroskedasticity, which is a necessary condition for Lewbel's instruments to be relevant.

Now let us focus on our baseline wage equations with dummies for reasons of schooling

[30] We apply the "rule of thumb" of Staiger and Stock (1997), which states that the F statistic should be at least 10 for the weak identification not to be considered a problem.

interruption. The results are presented in Table 4. Our analysis is carried out separately for men and women. Estimates from Lewbel's IV approach are provided using a two-Step GMM estimator. OLS estimates are provided for comparison. The left and right sections of Table 4 report estimates for men and women respectively. The first column of each section exhibits OLS coefficients. Columns 2 and 5 show estimates from Lewbel's approach using generated instruments only (GenInst-GMM). Finally, columns 3 and 6 provide estimates when our three external instruments are used along with Lewbel's generated ones (GenExtInst-GMM)[31]. Note that the natural approach, when using an instrumental variable method, is to use all available instruments, because theoretically this leads to the most asymptotically efficient estimator. Therefore, our analysis focuses on estimates from columns 3 and 6, namely the ones providing results using both generated and external instruments[32]. In addition, we briefly discuss coefficients obtained from only using generated instruments since the two latter specifications produce relatively similar results in terms of estimates and standard errors. In practice, using too many instruments may lead to larger finite-sample bias because the magnitude of finite-sample biases of IV estimators increases with the number of instruments (Hahn and Hausman, 2003)[33]. However, given the size of our sample, this limitation is likely to be minor in our study.

By looking at column 1 of Table 4, we see that OLS estimates report a significant decrease in wages by 22% ($p < 0.01$) for men who interrupt their schooling because of lack of money, and by 23% ($p < 0.1$) when the interruption is related to health issues. Moreover, we find that interruption related to reasons other than money, health or work reduces men's wages by a significant 11% ($p < 0.01$).

However, estimated parameters from OLS are not likely to be consistent, given the endogeneity problem. Lewbel's method, which circumvents this drawback under certain assumptions, rather suggests that male interrupters who had held a full-time job during their out-of-school

[31] As mentioned, when combining external and generated instruments, the sample is reduced to only include observations that have available information on the external instruments.

[32] In an additional specification, we estimate our baseline model using a reduced set of generated instruments. Overall, our results are robust to this specification.

[33] We face this limitation with one specification. Further details are given later in the paper.

spell(s) witness a significant 21% increase in their wages ($p < 0.01$, column 3). A similar result is obtained when only Lewbel's generated instruments are used (see column 2). One way to explain this positive effect is that the human capital deteriorates over time. In other words, knowledge, skills, and abilities become obsolete when new and improved technology becomes available. Since the interrupter has presumably acquired a part (or all) of his pre-graduation work experience at a more current time compared to his continuously enrolled counterpart, his more recent experience is expected to be more valuable than the pre-graduation experience acquired by a non-interrupter, even after controlling for the level of work experience. Another reason for this positive sign is that human capital obtained from schooling and from work experience are complementary (Becker, 1964). Therefore, employers are likely to offer higher paying jobs to individuals with more recent pre-graduation schooling. In other words, it is possible that the job that an interrupter held during his interruption required more schooling, while the one held by a non-interrupter did not require as much qualifications, given the fact that the latter landed his job before enrolling in his 2005 program.

Also, Lewbel's specification in column 3 suggests that interruption related to health issues reduces men's wages by 21% ($p < 0.05$), conditional on the levels of schooling and work experience. No significant effect is assessed when only generated instruments are employed (see column 2). In order to isolate and identify wage losses caused by health-related interruption from those associated with current bad health, we include a dummy to control for current health status[34]. By doing so, Lewbel's specification reveals that the negative effect of the health-related interruption decreases (-16%, $p < 0.1$). At the same time, having current bad health significantly reduces their real wage rates by 18%. The presence of chronic illness could explain this result. Indeed, workers who are affected by chronic conditions would be expected to be less productive and thus earn less than their healthy counterparts, *ceteris paribus*.

As for women, OLS estimates suggest that schooling interruption only affects female individuals who had held a part-time job during their out-of-school spell(s). These people witness

[34]Estimates from this specification are available at the online Appendix at this link.

a significant 32% decrease ($p < 0.05$) in their wages. A similar effect is obtained from Lewbel's specification when using all available instruments (28% decrease, $p < 0.01$). This result may be explained by the fact that the possible positive effects of working during an interruption are dominated by the negative effect induced by the atrophy phenomenon.

This specification also suggests that female interrupters witness a significant 35% wage decrease ($p < 0.01$) when their discontinuous schooling is driven by lack of money, and a 14% decrease ($p < 0.05$) when it is caused by health issues. When controlling for current health status[35], the negative impact of health-related interruption on wages becomes less relevant (-10%, $p < 0.1$), while having current bad health significantly reduces their real wages by 13% ($p < 0.01$). Similarly, women with chronic disease bear a wage penalty that might be caused by health-related productivity loss. For reasons other than money, health, and work, temporary schooling interruption induces a wage penalty that amounts to a significant 13% ($p < 0.05$). A possible explanation of these findings is that skills acquired by interrupters in previous periods may depreciate. Notice that both Lewbel's specifications produce similar results. However, results obtained when generated instruments are supplemented with external ones tend to be more precise.

In addition, our results suggest that the estimated return to education obtained through Lewbel's procedure amounts to 4% for men and 12% for women ($p < 0.01$). While the OLS and GMM rate of return are quite close for men (0.05 and 0.04), they are much higher for women (0.09 and 0.12). The latter result is consistent with results obtained in the literature as studies[36] show that men have significantly lower returns to schooling than women (Montenegro and Patrinos, 2014; Pekkarinen, 2012; Dougherty, 2005; Trostel, 2002).

According to Lewbel's specifications, the coefficient on *age* significantly amounts to 2% ($p < 0.01$) for males, however, this variable does not have an effect on the post-graduation starting wages of females. In addition, Lewbel's specification that only uses generated instruments finds

[35] Estimates from this specification are available at the online Appendix at this link.

[36] A higher return to female schooling appears to be a consistent pattern in the literature. Dougherty (2005) provides a survey of previous studies reporting wage equations, most of which confirm this result. He also discusses possible causes of this effect.

significant effects of an additional year of work experience that reach 2% and 6% for men and women respectively, as shown in columns 2 and 5. No significant effect is found for experience when using all available instruments (columns 3 and 6). We reject the null hypothesis that the coefficients on *experience* and *experience2* are simultaneously zero in the former specification, but we did not reject it in the latter. Our findings also reveal that an additional month of elapsed time between graduation and first job reduces wages by 1% ($p < 0.01$) for both men and women.

One important contribution of our paper is to control for different reasons of temporary schooling interruption. In this context, we use the Wald method applied to the Lewbel's specifications to test the equality of coefficients of the interruption-related covariates. For both men and women, we strongly reject the null hypothesis according to which the coefficients of the interruption variables are equal ($p < 0.000$)[37]. We conclude that the interruption reasons should be included in the model.

Despite this result, it can be relevant, for comparison purposes with other studies, to provide results of another specification where we assess the impact of temporary schooling attrition regardless of what caused it. In other words, we impose equality of coefficients across the reasons-related covariates. Our baseline wage equation (3) is hence rewritten as follows:

$$\begin{aligned}\log w_i = \beta_0 + \beta_1 Educ_i + \beta_2 Leave_i + \beta_3 Exp_i + \beta_4 (Exp_i)^2 + \beta_5 Age_i + \beta_6 Visib_i + \beta_7 Married_i \\ + \beta_8 Atlantic_i + \beta_9 Prairies_i + \beta_{10} West_i + \beta_{11} North_i + \beta_{12} Ontario_i + \beta_{13} Spell_i + \varepsilon_i,\end{aligned} \tag{4}$$

where ε_i is the error term.

In this portion of the analysis, we consider two regressors as potentially endogenous; the schooling variable and *Leave*, a dummy indicating whether the respondent had discontinuous schooling. As it is shown in Table 5, Lewbel's instruments, whether used separately or supplemented with external ones are appropriate for men as they are related to the schooling and inter-

[37] Using generated instruments only, the Wald test reports a F statistic of 36.80 for men and of 54.47 for women. When generated instruments are supplemented with the external ones, the Wald test reports a F statistic of 27.08 for men and of 37.11 for women.

ruption variables, and unlikely to be related to wages. However, these instruments seem to be weak for women. We attempt to remedy this problem by reducing the number of used instruments. Following Millemet and Roy (2015), we use the Koenker (1981) version of the Breusch-Pagan test for heteroskedasticity to identify variables significantly related to the first-stage error variances. Based on the results of this test, we keep the two instruments that are generated from the *age* variable, by which we supplement our three external instruments. The reduced set of instruments satisfies the weak identification as it is shown in Table 5.

Table 6 summarizes the estimation results of this second model, with no dummies for reasons of schooling interruption. Four specifications are estimated. Columns 1 and 5 of Table 6 report OLS estimates for men and women respectively. Columns 2 and 6 provide coefficients from Lewbel's approach using generated instruments only. Columns 3 and 7 show estimates when our three external instruments are used along with Lewbel's generated ones. Finally, columns 4 and 8 present estimates obtained from the reduced set of instruments. In what follows, we focus our analysis on results obtained by the use of all available instruments since this specification yields more efficient estimates.

OLS estimates, as shown in columns 1 and 5 of Table 6, suggest that temporary interruption has no significant effect on subsequent real wages at given level of education and experience for both men and women. Moreover, inspection of column 3 in Table 6 reveals that interrupted schooling (the *Leave* coefficient) has no significant effect on men's subsequent wages. This result is consistent with the conclusion reached by Griliches (1980). Recall that this specification is the most comparable to existing research. A possible explanation of this effect is that the different reasons for interruption seem to balance each other out in their effects on subsequent wages (see Table 4, column 3: *Health*, 21%; *FullWork*, -21%), thus the importance to control for them. As for women, temporary interruption decreases their wages by 24% at the 0.1 level. However, the test of weak identification suggests weak correlation between the used instruments and the endogenous regressors. The use of the reduced set of instruments seems to remedy this problem and yields a significant and stronger negative effect of interrupted schooling on women's wages (-52%). These findings come to justify the importance of controlling for different reasons of interrupted schooling. Also, including reasons dummies in our model seems to be useful in helping to provide a more complete picture of the impact of this behavior on post-graduation wages.

7 Robustness Checks

In this section, we present a series of robustness checks that address several important concerns about the empirical performance of our baseline Mincerian equation extended to account for schooling interruption variables.

In spite of its wide adoption within empirical economics, and its numerous applications in areas of labor, the Mincerian wage equation had been recently criticized for not being able to provide a good fit of the real data at least in the U.S. The main aspects that stir up the controversy about the Mincer framework are the linearity of log wages equations in schooling, the quadratic function in experience, the additive separability in education and experience, and heterogeneity in the causal effect of covariates. Several articles have tested the specification of the Mincer wage equation (Card, 1999; Heckman, Lochner and Todd, 2006; Lemieux, 2006; Belzil, 2008).

The structure of this section is based on the work of Lemieux (2006). In his paper, Lemieux states that the Mincerian framework remains an accurate benchmark for estimating wage equations in the U.S. when adjusted by i) replacing the quadratic function of work experience with a quartic one; ii) allowing for a quadratic term in schooling; and iii) adding cohort effects.

In this section, we first look at whether the natural logarithm is the appropriate transformation of wages. Then, we re-estimate our equations after adding a quadratic term in schooling, and including a quartic function of experience instead of a quadratic one. We also estimate a wage equation in which log of wage rates is not an additively separable function of schooling and experience. Finally, we perform two-stage quantile regressions. The specificity of the latter technique with respect to standard linear methods is to give a more accurate quality assessment as it provides an estimate of conditional quantiles of the dependent variable instead of its conditional mean. This approach allows to reveal heterogeneity in the causal effect of covariates on the dependent variable.

The Box-Cox Test

Here, we test for the appropriate functional form of the dependent variable by comparing the goodness of fit of models in which the wage rate variable is in log or in level. To do so, we perform the Box-Cox test. In other words, we transform the data so as to make the residual sum of squares (RSS) comparable between the two models[38]. The adjusted model with the lowest RSS is the one with the better fit. We conclude that the linear and logarithmic models are significantly different in terms of goodness of fit, and that the logarithmic specification for real wage rates is preferred[39].

Quadratic Education and Quartic Work Experience Function

As mentioned earlier, some argue that the average impact of years of schooling on wage rates is likely to be nonlinear in schooling. Following the suggestion of Lemieux (2006), we re-estimate our wage equation after allowing for a quadratic term in schooling to better capture the convexity of the experience-wages profiles, and including a quartic function of experience instead of a quadratic one[40]. Overall, little difference has been found when comparing the R^2, estimates, and standard errors between this specification and the standard functional form we initially use in the paper. In particular, we notice a small quantitative, but not qualitative difference with regard to some interruption-related coefficients for men when some specifications are used. Moreover, the coefficients of the added variables are not statistically significant, except for the coefficient of the education squared variable. However, it is not economically significant as its magnitude is very small (around 0.62% for men and 2% for women).

Additive Separability in Education and Experience

In order to account for the non-separability between education and work experience, we add an interaction variable of schooling and experience in our wage equation (Edex)[41]. Overall, little dif-

[38] Just comparing R^2 of the two models is not valid as the total sum of squares in w is not the same as the total sum of squares in $log\ w$.

[39] See Table 7 in the Appendix for the test value.

[40] Estimation results are available at the online Appendix at this link.

[41] Estimation results are available at the online Appendix at this link.

ference has been found in terms of R^2, coefficients, and standard errors between this functional form of the wage equation and our initial specification. Particularly, very negligible difference is found in the interruption-related coefficients for both men and women. Furthermore, the interaction term is statistically but not economically significant as its coefficient is around 0.5 - 0.6%.

Two-Stage Quantile Regression with instrumental variables

In what follows, we compare the estimates of our GenExtInst-GMM linear model with those from the two-stage quantile regression with endogenous covariates (see Chernozhukov and Hansen, 2005), using Lewbel's generated instruments along with our three external ones. Quantile regression allows us to consider the impact of our covariates on the entire distribution of the wage variable, not merely on its conditional mean. We choose to present the results via a graphical display of coefficients of interest and their respective confidence intervals, as shown in Figures 2 and 3 for men and women, respectively[42].

The point estimates and confidence intervals from our linear Lewbel's model are shown by dashed and dotted horizontal lines respectively. They do not vary with the quantiles. The instrumented quantile regression coefficients are plotted as lines varying across the quantiles with confidence intervals around them. If the quantile coefficient is outside the linear confidence interval, then we have significant differences between the instrumented quantiles and linear GMM coefficients. If they are statistically significant, the quantile estimates reveal heterogeneous covariates effects. Recall that we use Lewbel's generated instruments combined with our three external ones to produce our linear and quantiles coefficients.

The upper plots of Figure 2 shows that for men, the quantile coefficients for *Money*, and *Health* are not significantly different from the Lewbel's GMM coefficient. However, the middle-left plot states that the quantile coefficients for *FullWork* tend to differ significantly from our linear

[42] With the help of Ali Yedan, we developed an algorithm that is inspired from the Stata command IVQREG. IVQREG estimates a quantile regression model with endogenous variables. While IVQREG allows up to two endogenous variables to be specified, our algorithm is able to perform two-stage quantile regressions with multiple (more than 2) endogenous regressors. Our estimator is consistent but not efficient. In an attempt to remedy the efficiency problem, we apply a bootstrap to our model. However, given the large number of the endogenous regressors and instruments, the process is computationally demanding and the model fails to converge.

coefficient in the lowest quantiles. Furthermore, the point estimates in the middle-right plot of Figure 2 suggest that over the 80^{th} quantile, schooling discontinuity caused by part-time work is significantly different from the linear Lewbel's GMM coefficient. In addition, Figure 2 reveals some heterogeneity in the effect of full-time work interruption on wages as the quantile point estimates associated with such interruption state a significant increase in wages that ranges from 18 to 31% ($p < 0.05$) above the 50^{th} quantile. The middle-right plot of Figure 2 also shows some heterogeneity in the effect of part-time work interruption on wages in the highest quantiles. Moreover, the lower plot of Figure 2 displays a significant 15 to 17% decrease in wages for those who interrupted their schooling because of other reasons and who are at lower quantiles.

As for women, inspection of Figure 3 reveals that the quantile coefficients for *Money*, *Health*, and *PartWork* are significantly different from the Lewbel's GMM coefficient at some ranges of quantiles. Also, the point estimates in the middle-right plot of Figure 3 suggest the presence of some heterogeneity in the effect of part-time work interruption on wages as the quantile point estimates associated with such interruption state a significant decrease in wages that ranges from 31 to 64% ($p < 0.05$) at lower quantiles. Heterogeneity is also found in the effect of money-related interruption on wages at the lowest quantiles.

Overall, the instrumented quantile regression estimates do not significantly differ from our linear GMM coefficients for most of the interruption-related variables. Also, Figures 2 and 3 report the presence of heterogeneity in the effect of some reasons of interruption on subsequent wages at some ranges of quantiles for both men and women.

8 Concluding Remarks

By applying an empirical model based on the human capital model of wage determination to a sample of Canadian postsecondary graduates (from the 2007 National Graduate Survey), we explore the causal effect of temporary dropping out on post-graduation starting wages, given the levels of schooling and work experience. Our analysis distinguishes the impact of various reasons at the source of the pre-graduation schooling interruption. The reasons provided in our survey include lack of money, health issues, part-time work, full-time work, and other reasons. At the econometric level, our identification strategy is based on Lewbel's (2012) approach. The latter imposes some reasonable restrictions on the conditional second moments of the data, under heteroscedasticity of the error terms of the endogenous covariates. Under these constraints, the Lewbel framework provides generated instruments that we use with additional external instruments, to estimate our model.

In our preferred specifications, we find that the causal effect of temporary schooling interruption on post-graduation starting wages differs significantly across gender. For males, interruption caused by full-time work has a positive effect on their post-graduation wage while health-related interruption induces a wage decrease. As for females, the effect is negative for all the reasons (but not significant when interruption is caused by full-time work). We also find that the hypothesis of the equality of coefficients across the reasons-related covariates is rejected and gives a highly incomplete picture of how temporary interruption of schooling might affect future real wage rates. In particular, the different reasons for interruption seem to balance each other out in their effects on subsequent wages for men. We conclude that to obtain a more complete outlook of the impact of this behavior on wages, it is necessary to carry out an analysis that takes into account different reasons of interrupted schooling.

Furthermore, we perform robustness checks to see how these coefficient estimates behave when the regression specification is modified. We first look at whether the natural logarithm is the appropriate transformation of wages. Then, we re-estimate our equations after adding a quadratic term in schooling, and including a quartic function of experience instead of a quadratic one. We also estimate a wage equation in which the log of real wage rates is not an additively separable function of schooling and experience. Furthermore, we perform two-stage quantile regressions using Lewbel's generated and external instruments. Overall, these specification checks allow us to conclude that our estimates are robust to a number of alternative specifications.

One important result of our study is that the impact of out-of-school periods on post-graduation wages depends on the activity undertaken during the interruption. Moreover, one explanation of the negative effects of temporary schooling interruption on post-graduation wages for most of these activities (an important exception being full-time work in the case of men where the effect is positive), is that they may be the source of depreciation and obsolescence of human capital acquired by interrupters before their out-of-school spell. Finally, our results suggest that policies that discourage not only permanent but also temporary schooling interruption may help postsecondary students to increase their subsequent wage.

In this paper we propose a model for the subpopulation of young postsecondary education graduates who happen to be employed within two years after graduation and who did not enroll in another postsecondary education program since their graduation. Therefore we do not model the decisions to drop out *and* to return (or not) to school. We acknowledge that a more complete model would also analyze these decisions using the relevant population. Such a framework is beyond the scope of this paper and remains a topic for further research.

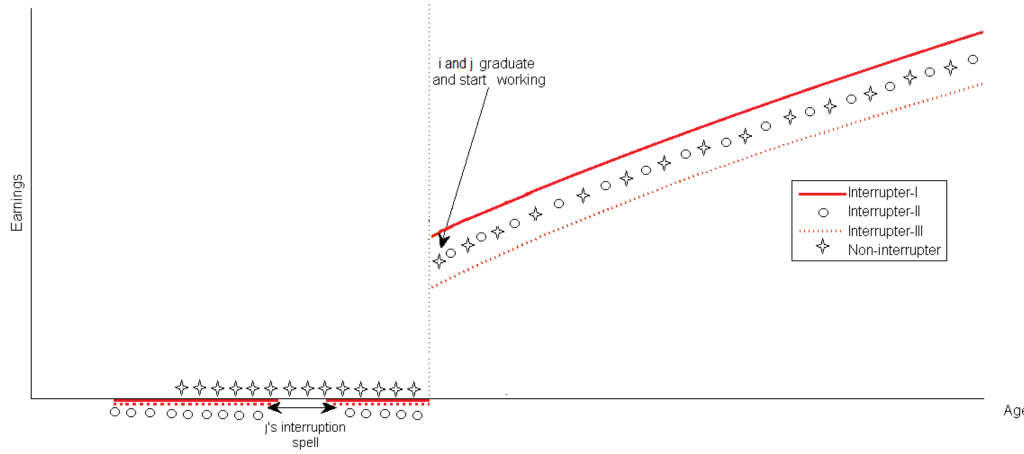

Figure 1: Potential Wage Profiles for Interrupters and Non-Interrupters

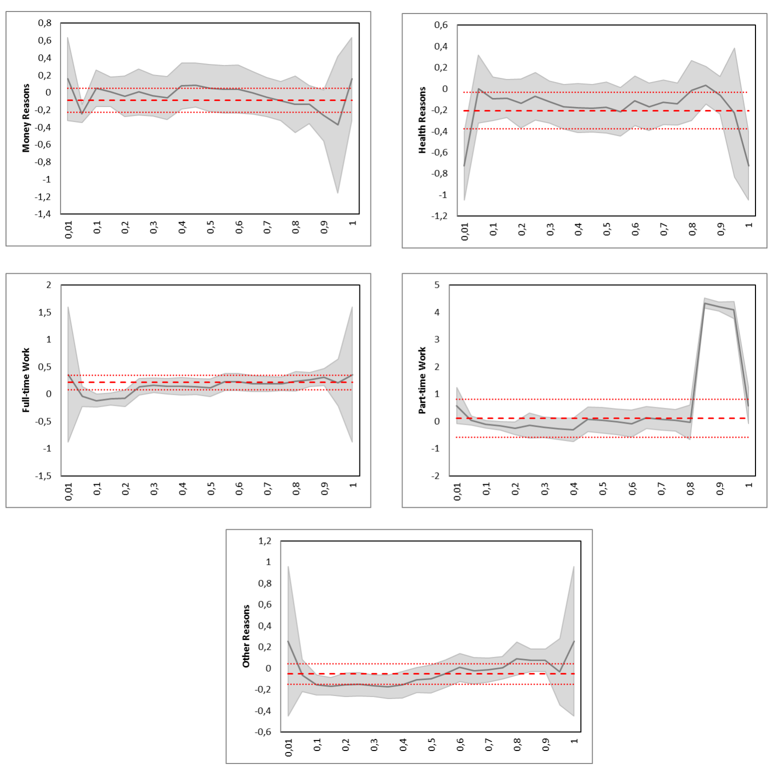

Figure 2: Two-Stage Quantile Regression and GMM Linear Lewbel's Coefficients, and Confidence Intervals for the Interruption-Related Variables - Men

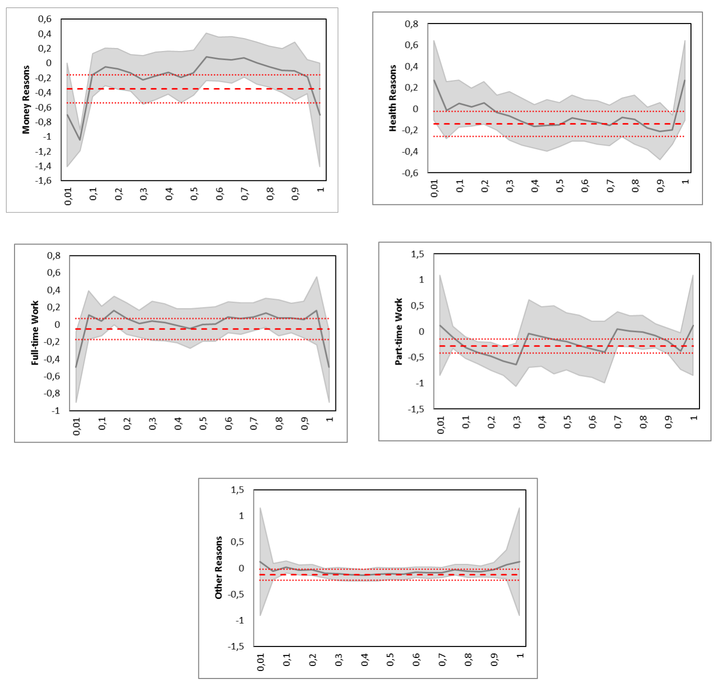

Figure 3: Two-Stage Quantile Regression and Linear Lewbel's Coefficients and Confidence Intervals for the Interruption-Related Variables - Women

Table 1: Descriptive Statistics[a]

Variables	Definition	Full sample (both genders)			Men			Women		
		Total (1)	Interrupters (2)	Non-interrupters (3)	Total (4)	Interrupters (5)	Non-interrupters (6)	Total (7)	Interrupters (8)	Non-interrupters (9)
Dependent variable										
log w	log of CPI-deflated starting hourly wage after graduation[b]	2.80	2.83	2.80	2.81	2.84	2.81	2.80	2.83	2.80
		(1.07)	(0.91)	(1.09)	(1.05)	(0.72)	(1.08)	(1.10)	(1.09)	(1.10)
Explanatory variables										
Educ	Years of schooling completed by 2005	16.09	16.74	16.04	15.93	16.74	15.87	16.25	16.74	16.22
		(2.18)	(1.86)	(2.19)	(2.21)	(1.89)	(2.22)	(2.13)	(1.84)	(2.15)
Exp	Years of pre-graduation full-time work experience	3.03	3.27	3.01	3.08	3.19	3.07	2.97	3.37	2.95
		(2.93)	(2.88)	(2.93)	(3.02)	(2.82)	(3.03)	(2.84)	(2.94)	(2.83)
Exp2	Pre-graduation work experience squared	17.67	19.00	17.76	18.61	18.15	18.64	16.91	20.01	16.70
		(40.96)	(36.10)	(41.30)	(46.87)	(33.52)	(47.78)	(33.90)	(38.99)	(33.52)
Leave	=1 if the respondent took a leave of absence from her 2005 program	0.07	1.00	-	0.07	1.00	-	0.06	1.00	-
		(0.25)	(0.00)		(0.26)	(0.00)		(0.24)	(0.00)	
Money	=1 if the interruption is caused by lack of money	0.00	0.07	-	0.00	0.07	-	0.00	0.06	-
		(0.07)	(0.25)		(0.07)	(0.25)		(0.06)	(0.25)	
Health	=1 if the interruption is caused by health issues	0.01	0.11	-	0.00	0.07	-	0.01	0.15	-
		(0.09)	(0.31)		(0.07)	(0.26)		(0.10)	(0.36)	
FullWork	=1 if the interruption is caused by full-time work	0.02	0.27	-	0.02	0.30	-	0.01	0.22	-
		(0.13)	(0.44)		(0.15)	(0.46)		(0.12)	(0.42)	
PartWork[c]	=1 if the interruption is caused by part-time work	.	.	-	.	.	-	.	.	-
		(.)	(.)		(.)	(.)		(.)	(.)	
Other[d]	=1 if the interruption is caused by any other reason	0.04	0.54	-	0.04	0.54	-	0.03	0.54	-
		(0.19)	(0.50)		(0.20)	(0.50)		(0.18)	(0.50)	
Age	Age at interview	26.55	27.79	26.46	26.39	27.93	26.26	26.72	27.62	26.66
		(3.56)	(3.24)	(3.57)	(3.63)	(3.21)	(3.64)	(3.48)	(3.28)	(3.49)
Spell	Elapsed time between graduation and first job	2.15	2.14	2.15	2.26	2.35	2.25	2.04	1.90	2.05
		(3.74)	(3.88)	(3.73)	(3.79)	(3.97)	(3.78)	(3.68)	(3.77)	(3.68)
Married	=1 if married	0.24	0.30	0.23	0.22	0.25	0.21	0.26	0.35	0.26
		(0.43)	(0.46)	(0.42)	(0.41)	(0.44)	(0.41)	(0.44)	(0.48)	(0.44)
Visib	=1 if belonging to a visible minority	0.13	0.15	0.13	0.15	0.18	0.14	0.12	0.11	0.12
		(0.34)	(0.35)	(0.34)	(0.35)	(0.38)	(0.35)	(0.33)	(0.31)	(0.33)
Atlantic	=1 if the respondent works in the Atlantic Canada	0.20	0.17	0.21	0.19	0.14	0.19	0.22	0.20	0.22
		(0.40)	(0.37)	(0.40)	(0.39)	(0.35)	(0.39)	(0.42)	(0.41)	(0.42)
Prairies	=1 if the respondent works in a Prairie Province	0.31	0.35	0.30	0.31	0.36	0.30	0.31	0.34	0.30
		(0.46)	(0.48)	(0.46)	(0.46)	(0.48)	(0.46)	(0.46)	(0.47)	(0.46)
West	=1 if the respondent works in the West Coast	0.11	0.13	0.11	0.12	0.14	0.12	0.10	0.12	0.10
		(0.31)	(0.34)	(0.31)	(0.33)	(0.35)	(0.32)	(0.30)	(0.32)	(0.30)
North	=1 if the respondent works in Northern Canada	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01
		(0.09)	(0.11)	(0.09)	(0.10)	(0.10)	(0.09)	(0.09)	(0.11)	(0.09)
Ontario	=1 if the respondent works in Ontario	0.16	0.15	0.16	0.16	0.15	0.16	0.16	0.15	0.16
		(0.37)	(0.36)	(0.37)	(0.37)	(0.36)	(0.37)	(0.37)	(0.36)	(0.37)
N		9,759	669	9.090	4,920	364	4,556	4,839	305	4,534

[a] Standard deviation in parentheses. Figures may not add up due to rounding. [b]The CPI by province and year were used to deflate nominal hourly wage rates. The base year is 2002, in which purchasing power across provinces is assumed to be equal. [c] Data on $PartWork$ are suppressed due to Statistics Canada confidentiality restrictions. [d]For reasons other than money, health, and work such as family obligations, because they only needed a few courses, because their program was not offered full time, lack of interest, program not meeting expectations, lack of career planning, academic difficulties, or taking time off to travel.

Table 2: Underidentification, Weak Identification, and Overidentification Tests for the Baseline Specification

	Men		Women	
	Generated Inst.	Generated + External Inst.	Generated Inst.	Generated + External Inst.
Underidentification test (Kleibergen-Paap rk LM statistic)	383.96	406.86	237.83	327.26
p =	0.00	0.00	0.00	0.00
Weak identification test (Kleibergen-Paap rk Wald F statistic)	35.24	33.11	19.09	23.27
Hansen J statistic (overidentification test of all instruments)	65.44	55.62	58.33	73.39
p =	0.16	0.56	0.35	0.08
N	4,920	3,710	4,839	3,451

Table 3: Breusch-Pagan Test for Homoskedasticity

Dependent Variables	Men		Women	
	chi2(1)	Prob >chi2	chi2(1)	Prob >chi2
Baseline Model				
Educ	519.14	0.00	263.03	0.00
Money	1815.89	0.00	4006.84	0.00
FullWork	1555.47	0.00	1531.57	0.00
PartWork	4646.87	0.00	2393.83	0.00
Other	606.87	0.00	250.29	0.00
Second Model				
Educ	518.77	0.00	262.77	0.00
Leave	564.07	0.00	215.53	0.00

Table 4: OLS and Lewbel's Estimates of the Wage Equations for Men and Women (With Dummies for Reasons of Interruption)[a]. Dependent Variable: log of Starting Real Wage After Graduation

	Men			Women		
	(1) OLS	(2) GenInst-GMM	(3) GenExtInst-GMM	(4) OLS	(5) GenInst-GMM	(6) GenExtInst-GMM
Educ	0.05***	0.06***	0.04***	0.09***	0.15***	0.12***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Money	-0.22***	-0.06	-0.09	-0.19	-0.25**	-0.35***
	(0.07)	(0.07)	(0.07)	(0.13)	(0.11)	(0.10)
Health	-0.23*	-0.19	-0.21**	-0.13	-0.13*	-0.14**
	(0.13)	(0.11)	(0.09)	(0.09)	(0.07)	(0.06)
FullWork	0.08	0.28***	0.21***	-0.09	0.01	-0.05
	(0.08)	(0.06)	(0.07)	(0.06)	(0.07)	(0.06)
PartWork	0.57	0.06	0.12	-0.32**	-0.31***	-0.28***
	(0.77)	(0.25)	(0.35)	(0.14)	(0.05)	(0.07)
Other[b]	-0.11***	-0.08*	-0.05	0.02	-0.16***	-0.13**
	(0.04)	(0.04)	(0.05)	(0.11)	(0.05)	(0.05)
Age	0.02***	0.02***	0.02***	0.02***	-0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Experience	0.01	0.02**	0.01	0.02	0.06***	0.02
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)
Experience2	0.00	-0.00	0.00	-0.00	-0.00***	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Married	0.06	0.04	0.04	0.04	0.03	0.08***
	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Visib	0.03	0.01	-0.00	0.01	0.06*	-0.02
	(0.05)	(0.03)	(0.03)	(0.05)	(0.03)	(0.03)
Atlantic	-0.10*	-0.12***	-0.05	-0.01	-0.01	-0.04
	(0.05)	(0.04)	(0.05)	(0.05)	(0.04)	(0.04)
Prairies	0.06	0.02	0.06	0.09**	0.09**	0.09**
	(0.05)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
West	-0.02	-0.03	0.02	0.10*	0.07*	0.10***
	(0.04)	(0.03)	(0.04)	(0.05)	(0.04)	(0.03)
North	0.25*	0.27***	0.62***	0.77**	0.71***	0.42***
	(0.15)	(0.06)	(0.10)	(0.39)	(0.23)	(0.09)
Ontario	0.08*	0.07*	0.12***	0.10*	0.09**	0.12***
	(0.05)	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)
Spell	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
_cons	1.43***	1.30***	1.55***	0.77***	0.38***	0.57***
	(0.13)	(0.10)	(0.11)	(0.15)	(0.14)	(0.15)
N	4920	4920	3710	4839	4839	3451

[a]Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. [b]For reasons other than money, health, and work such as family obligations, because they only needed a few courses, because their program was not offered full time, lack of interest, program not meeting expectations, lack of career planning, academic difficulties, or taking time off to travel.

Table 5: Underidentification, Weak Identification, and Overidentification Tests for the Second Specification

	Men			Women		
	Generated Inst.	Generated + External Inst.	GenExtRed[a]	Generated Inst.	Generated + External Inst.	GenExtRed[a]
Underidentification test (Kleibergen-Paap rk LM statistic)	222.84	131.18	91.09	48.96	62.15	43.38
p =	0.00	0.00	0.00	0.00	0.00	0.00
Weak identification test (Kleibergen-Paap rk Wald F statistic)	28.95	16.00	47.85	3.34	4.72	15.59
Hansen J statistic (overidentification test of all instruments)	21.58	19.80	2.27	25.16	23.11	4.34
p =	0.36	0.65	0.52	0.19	0.45	0.23
N	4,920	3,710	3,710	4,839	3,451	3,451

[a]The reduced set of instruments is composed of our three external instrument + the two instruments that are generated from the age variable. The latter instruments are chosen on the basis of results from the Koenker (1981) version of the Breusch-Pagan test.

Table 6: OLS and Lewbel's Estimates of the Wage Equations for Men and Women (With No Dummies for Reasons of Interruption)[a]. Dependent Variable: log of Starting Real Wage After Graduation

	Men				Women			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	GenInst-GMM	GenExtInst-GMM	GenExt-RED[b]	OLS	GenInst-GMM	GenExtInst-GMM	GenExt-RED[b]
Educ	0.05***	0.07***	0.05***	0.05***	0.09***	0.15***	0.13***	0.12***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.03)
Leave [c]	-0.06	0.21**	0.13	0.09	-0.05	-0.00	-0.24*	-0.52**
	(0.04)	(0.08)	(0.08)	(0.15)	(0.06)	(0.11)	(0.13)	(0.22)
Age	0.02***	0.01	0.02**	0.02**	0.02***	-0.01	-0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Experience	0.02	0.03***	0.01	0.02	0.02	0.05***	0.04**	0.04*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Experience2	0.00	-0.00	-0.00	0.00	-0.00	-0.00**	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Married	0.05	0.04	0.04	0.03	0.04	0.03	0.08**	0.06*
	(0.04)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.04)
Visib	0.02	0.01	0.01	0.03	0.01	0.01	-0.03	-0.02
	(0.05)	(0.03)	(0.04)	(0.05)	(0.05)	(0.04)	(0.04)	(0.05)
Atlantic	-0.10*	-0.13***	-0.06	-0.04	-0.01	-0.01	-0.05	-0.05
	(0.05)	(0.04)	(0.05)	(0.06)	(0.05)	(0.04)	(0.05)	(0.05)
Prairies	0.06	0.02	0.04	0.07	0.09**	0.08*	0.08*	0.10*
	(0.05)	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)	(0.05)
West	-0.02	-0.04	0.00	0.00	0.10*	0.07*	0.10**	0.14*
	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)	(0.04)	(0.04)	(0.06)
North	0.25*	0.14	0.75***	0.72**	0.77**	0.57**	0.50***	0.51***
	(0.15)	(0.10)	(0.18)	(0.33)	(0.39)	(0.24)	(0.12)	(0.14)
Ontario	0.09*	0.06	0.08*	0.09	0.10**	0.08*	0.07	0.08
	(0.05)	(0.04)	(0.05)	(0.06)	(0.05)	(0.05)	(0.05)	(0.06)
Spell	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***	-0.01*	-0.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
_cons	1.41***	1.33***	1.46***	1.43***	0.77***	0.46***	0.59***	0.54**
	(0.13)	(0.11)	(0.13)	(0.15)	(0.15)	(0.17)	(0.18)	(0.22)
N	4920	4920	3710	3710	4839	4839	3451	3451

[a]Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. [b]The reduced set of instruments is composed of our three external instrument + the two instruments that are generated from the age variable. The latter instruments are chosen on the basis of results from the Koenker (1981) version of the Breusch-Pagan test. [c]Assuming equality of coefficients between the different reasons of interrupted schooling.

Appendix

Box-Cox Test

As it shown in Table 7, the estimated value of the test

$$BoxCox = (N/2) * log(RSS_{largest}/RSS_{smallest}) \sim \chi^2_{(df)}$$

exceeds the critical value (at 5% level with 17 degrees of freedom). Consequently, the linear and logarithmic models are significantly different in terms of goodness of fit[43].

Table 7: Statistics for the Box-Cox Test

	Men	Women
N	4920	4839
RSS (log model)	5251	5588
RSS (linear model)	4.85e12	4.90e12
Box-Cox estimated value	50784	48923
df	17	17
Critical value (at 5% level)	27.587	27.587

[43]The null hypothesis is that the models are the same.

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