The Relationship between Education and Mortality: Evidence from Canada

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Abstract

A positive relationship between health and education is well-established [Grossman, 1972]. However, the empirical estimation of the effect of education on health is plaque with endogeneity problems. Specifically, the direction of the causal relationship is unclear and the estimates suffer from omitted variable bias. This study investigates causal relationship between education and mortality rate in Canada using Canadian Censuses of 1971, 1981, 1986, 1991, 1996 and 2001. Following Lleras-Muney (2005), I construct synthetic cohorts and use Canadian compulsory schooling laws from 1920 to 1970 as instruments for education. I find that education has a significant and negative impact on mortality rates in Canada -- increasing educational attainment lowers the average mortality rate. Moreover, I find that cohorts with higher proportions of individuals who graduated from high school or who obtained university degrees have lower mortality rates than those who do not. These findings suggest that policies that focus on increasing educational attainment, with a special emphasis on secondary education, might be a powerful tool to improve the health of Canadians in the long term.

I. Introduction

Health and education are two important components of human capital that contribute to economic growth in the long run through capital accumulation. It is thus clearly of importance for policy makers to find effective policy levers in order to improve population health in the long term. However, Canadian governments are incurring a serious financial burden while trying to achieve such a goal. On the one hand, Canada's health care expenditures accounted for as much as 10.9 percent of GDP in 2012 (World Bank 2015), and this percentage is very likely to rise in the future. On the other hand, the Canadian population is aging. According to Employment and Social Development Canada (2015), "In 2011, the median age in Canada was 39.9 years… In 1971, the median age was 26.2 years… By 2051, about one in four Canadians is expected to be 65 or over." The aging of the population will likely raise the demand for publicly funded health care and lead to higher health expenditures. Seeking other cost-effective policy levers is thus of interest to governments that wish to improve population health in the long run and efficiently allocate public expenditures.

In recent economic studies, researchers have investigated if there is a causal relationship between education and health. There is nothing surprising about such interest, because if there is a causal relationship, higher education leads to better health, and subsidizing the education sector might become a cost-effective policy for improving health (Cutler and Lleras-Muney 2006). Within the framework of neoclassical economics, both education and health capital are two significant components of human capital through which a country realizes its long-run economic growth. So, if education is indeed an effective means of improving population health, then allocating more public resources to the education sector will kill two birds with one stone. However, the empirical estimation of the effect of education on health is plagued with endogeneity problems. Specifically, the direction of the causal relationship is unclear and the estimates suffer from omitted variable bias.

This study investigates the causal relationship between education and mortality rates in Canada using data from the Canadian censuses of 1971, 1981, 1986, 1991, 1996 and 2001. Following Lleras-Muney (2005), I construct synthetic cohorts and use Canadian compulsory schooling laws from 1920 to 1970 as instruments for education. I find that education has a significant and negative impact on mortality rates in Canada -- increasing educational attainment lowers the average mortality rate. Moreover, I find that cohorts with higher proportions of individuals who graduated from high school or who obtained university degrees have lower mortality rates than those who do not. These findings suggest that policies that focus on increasing educational attainment, with a special emphasis on secondary education, might be a powerful tool to improve the health of Canadians in the long term.

The rest of this paper is organized as follows. The next section provides the literature review. Section III briefly introduces the evolution of Canada's public health insurance (1920-1970). Section IV describes the econometric model and methods. Section V presents the data. I report the results in Section VI. The last section provides the conclusion.

II. Literature Review

An important goal for policy makers is to improve the health of a population. In recent years, researchers have shown that the return to increasing health care spending is uncertain (Weinstein and Skinner 2010). Given the importance of health capital and a demographic trend towards an aging population and its resulting demand for higher health care expenditures, policy makers are urged to choose actions to improve population health and reduce the pressure on health care funding.

In comparison to the uncertain returns from increasing health expenditures, research has shown a positive relationship between education levels and health status (e.g., Grossman 1972, Wagstaff 1986, and Lleras-Muney 2005). Using a nationally representative 1963 United States survey (which was conducted by the Center for Health Administration Studies and National Opinion Research Center, University of Chicago), Grossman (1972) finds a positive effect of schooling on self-rated health and a negative effect of schooling on work days lost due to illness and injury. In subsequent work, Grossman (1975) restricts the sample to white men who reported positive full-time salaries in 1955 and finds a positive schooling effect on self-reported health. Using the 1976 Danish Welfare Survey, Wagstaff (1986) reports a positive effect of schooling on a measure of good health and a negative effect of schooling on physician visits. Erbsland, Ried and Ulrich (1995) use 1986 West German Socio-economic Panel data and a structural equation technique that combines maximum likelihood estimation and principal components analysis to estimate the reduced form demand for health services. They find that schooling has a positive effect on the demand for health services and a negative effect on the demand for visits to general physicians. Employing the 1987 US National Medical Expenditure Survey, Gilleskie and Harrison (1998) find that schooling has a positive effect on self-reported health for both men and women. They control for the past stock of health, as measured by the number of chronic conditions and body mass index.

However, the above studies do not pay attention to the endogeneity problems related to schooling, i.e., that the estimates of the coefficients of education may be biased. Studies of the time preference hypothesis, such as Fuchs (1982), Farrell and Fuchs (1982) and Leigh (1985), argue that the positive relationship between education and health can be explained by a third variable, time preference, which causes both higher educational attainment and health behaviours. Because it is difficult to measure time preference and identify the way in which time preference affects schooling and health behaviour, more and more researchers do not attempt to estimate time preference. Instead, education is assumed to be endogenous and an instrumental variable (IV) strategy is employed in order to address endogeneity. In earlier IV studies, such as Berger and Leigh (1989), Sander (1995a, b), and Leigh and Dhir (1997), the instruments used for education include average real per capita income, IQ, parents' schooling, parents' income, number of siblings and so on.

Berger and Leigh (1989) use two American data sets (National Health and Nutrition Examination Survey (NHANES I) and the National Longitudinal Survey of Young Men (NLS)) representing different age groups and four measures of health to investigate the relationship between education and health. The instruments for education include per capita average real income, per capita average real expenditures on education, parents' schooling, IQ, and Knowledge of Work test scores. They report that education has a positive impact on health because schooling helps people acquire better knowledge of health. Sander (1995a) investigates the schooling effect on the probability of quitting smoking. He uses data generated from the General Social Survey conducted by the American National Opinion Research Center. Sander (1995a) uses parents' schooling, region of residence when people were 16 and number of siblings as instruments for education. The author finds schooling has a significant positive effect on the probability of quitting smoking. In Sander (1995b), the author uses the same data set and instruments to investigate the effect of education on the probability of smoking. The result indicates that schooling has a negative effect on smoking behaviour. Leigh and Dhir (1997) study education and seniors' health. They exploit data resulting from the American Panel Study of Income Dynamics (PSID) and employ parents' schooling, parents' income and state of residence in childhood as instruments for education. They argue that "… education itself, rather than simply self-efficacy or time or risk preference, acts as preventive medicine" (Leigh and Dhir 1997, 45).

Several recent studies (Llreas-Muney 2005, Mazumdar 2008 and Clark and Royer 2013) employ instruments resulting from quasi-natural experiments in order to address the endogeneity due to the possible reverse causality between education and health or from omitted variable bias (i.e., education and health are jointly determined by the same governing process). Lleras-Muney (2005) constructs synthetic cohorts using the U.S. censuses of 1960, 1970 and 180 and employs two compulsory schooling law variables as instruments for education. More specifically, she defines compulsory schooling years as the difference between the age at which children had to go to school and that at which children were allowed to work and uses a dummy variable to indicate whether the state required a child to attend school as a part-time student even though he or she held a work permit. She finds a large casual effect of education on mortality. Specifically, one additional year of education lowers the ten-year death rate by approximately 1.3 percentage points using GLS estimation, and by at least 3.6 percentage points using IV estimation. If the IV estimation result is translated into life expectancy gains, this means that an additional year of education (in 1960) increases life expectancy for a person aged 35 by as much as 1.7 years (Lleras-Muney 2008).

However, Mazumder (2008) states that the assertion of a positive causal effect of education on health is questionable after robustness checks. Following Llreas-Muney, Mazumder (2008) expands the number of U.S. censuses and employs robustness checks such as the addition of state-specific time trends, for instance. The results show that after including state-specific time trends, the impact of education on mortality is not robust, which casts doubt on the causal relationship between education and mortality. Mazumder's findings are reliable for two reasons. First, he improves the measure of compulsory schooling laws by using data on the maximum school entry age when the influenced individuals are around the age that they start school, instead of when they are 14. Second, he also examines different specifications of the econometric models by adding a cubic fuction of age and interactions between region and cohort.

Like Mazumdar, several other authors using data for countries other than the U.S. have found little or no evidence of a positive relationship between education and health. Albouy and Lequien (2009) employ a French longitudinal dataset and a regression discontinuity approach, where the Zay and Berthoin reforms are exploited.[1] They use survival rates as their health measure and do not find any casual effect of education on survival rates. Kemptner, Jürges and Reinhold (2010) examine the impact of education on health in West Germany. German compulsory schooling law reforms that occurred between 1940 and 1969 are employed as instruments for education. They find that education has a significant negative effect on male work disabilities and long-term illness, and a weak impact on weight problems. None of the evidence shows a causal effect of education on smoking behaviour.

Based on data constructed from the British Labour Force Survey and the Health Survey for England, Braakmann (2011) uses the likelihood that individuals born in February get any degrees or diplomas as an instrument for education. The author stresses that this instrument passes all the weak instrument tests and avoids some of the potential endogeneity problems

[1] The Zay and Berthoin reforms increased the minimum school leaving age to 14 and 16 years, respectively (Albouy and Lequien 2009).

inherent in quarter of birth instruments.[2] The researcher does not find any casual effect of education on health related measures (e.g., blood circulation problem, chest or breathing problem and diabetes) nor on health related behaviours (e.g., smoking and drinking). In another British study that takes advantage of the two compulsory schooling changes in Britain in 1947 and 1972, Clark and Royer (2013) find that the effect of education on mortality induced by compulsory schooling laws is very small. Among the strengths of this paper is the fact that the two reforms have a much larger influence on the relevant population - 50 percent for the first reform and 25 percent for the second reform. However, the biggest improvement in this paper is that month and year of birth are used instead of just year of birth to match individuals to changes in compulsory schooling laws, so that their effect on education levels is measured more precisely.

Fischer, Karlsson and Nilsson (2013) provide evidence from Sweden that one extra compulsory school year increases the 50-year survival rate by 3.2 percentage points. Their instrument for education is the one extra school year induced by the 1936 national reform in Sweden. Zhong (2015) investigates the effect of a college degree on health in China with various health measures being tested. The researcher exploits the dramatic college expansion in China after 1999 using the regression discontinuity method. It is shown that education does not reduce smoking or drinking behaviour, nor reduce the likelihood of being ill in the past three months. However, education does decrease the possibility of having a hypertension problem.

To sum up, research investigating the relationship between education and mortality can

[2] Buckles and Hungerman (2013) found evidence that women with different characteristics choose to give birth during different times over a year, which may result in different parental background for children born in different quarters. Braakmann argues that using February birth as an instrument lowers the probability of potential endogeneity problems when the quarter of birth is employed as instrument, because such an instrument is more likely to be correlated with the unobserved variable, parental backgrounds.

be divided into three phases. In the first phase, a positive effect of education and morality are reported in spite of potential endogeneity issue. In the second phase, researchers take endogeneity into consideration and employ IV strategy to address this issue, but the instruments used in some studies are questionable. In the third phase, more robust instruments resulting from quasi-experiments are exploited to address the endogeneity. There is no single answer as to whether there is a causal relationship between education and health: the answer varies with different measures of health and data used for different countries. However, little work seems to have been done on this topic using Canadian data.

III. The Evolution of Public Health Insurance in Canada, 1920-1970

Universal health insurance is an important policy in Canada. On the one hand, health insurance lowers the cost of access to health services for citizens, which should potentially improve health and thus lower the mortality rate. Before the introduction of provincial hospital insurance, Canadians had to either pay the cost themselves or purchase commercial insurance; however, after Newfoundland introduced its cottage hospital plan in 1936, followed by hospital insurance plans established in other provinces, Canadians started to benefit from free health care services. During the period of 1920 to 1970, hospital insurance and medical insurance underwent a huge evolution in Canada at both the provincial and federal levels. Therefore it is important to investigate the impact of universal health insurance on mortality. In the remaining part of this section, I briefly introduce the evolution of Canada's public health insurance (1920-1970), relying heavily on Health Canada (2012), Canadian Museum of History (2010) and Marchildon (2012).

In this paper, the impact of provincial hospital insurance is of particular interest due to the fact that hospital plans were one of the earliest types of health plans that were established in most provinces from 1920 to 1968. The population of interest in this paper are Canadians who turned 14 from 1920 to 1970, during which time most of the provinces were establishing their government-run hospital insurance. It is thus important to investigate the effect of hospital insurance, among other forms of health insurance, on mortality. From 1929 to 1939, on the one hand, Canada experienced significant growth in population, from 8.7 million to 10.3 million (Canadian Museum of History 2010). On the other hand, Canada also experienced the Great Depression and its long-term influence on the economy, with professional incomes declining by 36 per cent between 1928 and 1933 (Canadian Museum of History 2010). The increasing demand and the decreasing ability to pay for health care urged Canadians to put pressure on both provincial and federal governments to cooperate and establish an advanced health care system. In response, the Canadian Medical Association (CMA) started to study the concept of health insurance, and provinces such as Newfoundland, British Colombia and Alberta started the first attempts to create provincial hospital insurance plans. In 1934, Newfoundland and Labrador introduced a cottage hospital system to provide hospital and medical services to citizens in the outports. British Columbia and Alberta passed health insurance legislation in 1936, but because of considerable objections from both doctors and insurance companies, neither province's legislation was implemented.

During the Second World War (1939 to 1945), all Canadians' civil liberties and freedoms were compromised to the strong willingness to defeat the Axis Powers. While recruiting new soldiers, the Canadian government encountered a problem in that 56 percent of the volunteers failed to pass the initial physical exams (Canadian Museum of History 2010). This issue led the

Canadian government to pay more attention to improving Canadians' health, even during wartime, and to prepare post-war plans with the long-term policy goal of doing so. As a result, in 1942 the Federal Interdepartmental Advisory Committee on Health Insurance was created and expected to build up a national health insurance plan in response to the increasing demand for public health. After the Second World War, while the Canadian economy was returning to full employment induced by the war industries, both the federal and provincial governments started to improve hospital services. In 1947, Saskatchewan introduced the first provincial hospital insurance plan in Canada, followed by British Columbia and Alberta in 1949 and 1950 respectively. More specifically, the Saskatchewan Hospital Services Insurance Plan provided universal and comprehensive coverage; in contrast, the British Columbia Hospital Insurance Service plan implemented "pay direct" premiums or employer-based deductions from 1949 to 1952 and then introduced co-insurance charges, in order to cover hospital expenses. In Alberta, Ernest Manning's Social Credit government only subsidized the indigent and provided a mix of public and private plans and program.

Meanwhile, through the National Health Grants Program in 1948, the federal government started to provide grants to provinces and territories in order to support various health-related initiatives, such as supporting provincial health surveys, which helped provincial governments to conduct comprehensive reviews and assessments regarding local hospital services, professional training and facility supplies. In 1957, the *Hospital Insurance and Diagnostic Services Act* was proclaimed and came into force on July first, 1958. In this Act, the federal government provided 50/50 cost sharing for provincial and territorial hospital insurance plans. Under this Act, in 1958 Manitoba, Newfoundland, Alberta and British Columbia created hospital insurance plans with federal cost sharing, and the Saskatchewan hospital insurance plan was also brought in under federal cost sharing. Ontario, New Brunswick, Nova Scotia and Prince Edward Island created hospital insurance plans with federal cost sharing in 1959. Québec established its hospital insurance plan with federal cost sharing in 1961.

When it came to comprehensive medical insurance, Canadians began a ten-year debate about what the role of the federal government would be. In each province, citizens were split into two parties and argued about the pros and cons of a medical service insurance plan involving provincial and federal cooperation. In 1962, Saskatchewan took the initiative of creating the *Saskatchewan Medical Care Insurance Act*. Under this plan, doctors were to be paid by fee-for-service and the patients would symbolically pay a small portion of the cost. Doctors in the province responded to this act with a 23-day strike, because such an insurance plan “infringed on doctors’ rights to treat their patients and it would lower the quality of service by forcing doctors who believed in free market principles to leave the province”(Canadian Museum of History (2010). However, this act was implemented after negotiations and compromises by both the doctors and the Saskatchewan government, in that private insurance plans were still provided and doctors were allowed to opt out of the plan. In 1963, both Alberta and Ontario introduced alternatives to Saskatchewan’s medical care insurance. The Alberta government adopted an approach which subsidized private insurance plans, while in Ontario, voluntary and multi-payer approaches were introduced. Three years later, in 1965, British Columbia established a provincial medical plan, “Bennettcare,” under which a multi-payer approach was established and was carried by non-profit insurance carriers.

The *Medical Care Act* was introduced by the federal government in 1966 and put in force in 1968. In this act, the federal government provided 50/50 cost sharing for provincial and territorial medical insurance plans. In 1968, Saskatchewan and British Columbia were able to qualify immediately for federal cost sharing based on their existing medical plans. Newfoundland, Nova Scotia, Manitoba, Alberta and Ontario established medical insurance plans with federal cost sharing in 1969, followed by Québec and PEI in 1970 and New Brunswick in 1971.

Table 1 shows the year of introduction of provincial hospital insurance and medical insurance in the Canadian provinces. Later, this table will be used as the basis of dummy variables for health insurance that will be included in the econometric model.

IV. Econometric model

In terms of econometric model and estimation method, I follow Lleras-Muney (2005). I use pseudo panels (synthetic cohorts) instead of panel data, because there is no Canadian panel data following the same individuals over a long period of time. For instance, consider the Canadian Community Health Survey (CCHS), which is a cross-sectional survey that collects information related to health status, health care utilization and health determinants for the Canadian population. It has been conducted annually since 2007 and covers approximately 98% of the Canadian population aged 12 and older.[3] Various health indicators, such as self-perceived health status, diabetes, asthma, dental visits and work stress conditions, are provided in the CCHS. However, this survey doesn't follow the same sample of individuals every year and thus I am not able to follow the same individuals and investigate the effect of education on their health. Another example showing the necessity of constructing pseudo panels is a recent Statistics Canada health study (Tjepkema, Russell and Long 2013) on cause-specific mortality by education in Canada, where the researchers construct data from the 1991 to 2006 Canadian Census Mortality Follow-up Study that includes a sample of 2.7 million Canadian adults aged 25

[3] Statistics Canada website: Canadian Community Health Survey - Annual Component (CCHS)

or older; however, these panels are very short (it is a16-year follow-up mortality study). In comparison, repeated cross-sectional population surveys that are conducted at consecutive points in time, five or ten years apart for instance, provide sufficient population information to construct pseudo panel and cohort characteristics, such as age, year of birth, province of birth, gender, mother tongue, and education status. Because such surveys generate large enough successive random samples of individuals, I am able to construct pseudo panels which allow one to "infer behavioural relationships for the cohort as a whole just as if the panel data were available" (Deaton 1985, 110).

There are two rationales for choosing the mortality rate as the indicator of health. Firstly, despite the measurement errors in cohort mortality rates that are inevitably generated when applying pseudo panels (Deaton 1985), the mortality rate is a traditional health indicator that is widely used for describing the health status of a population. Secondly, mortality rates can easily be generated for synthetic cohorts constructed using census data. Compared with aggregate provincial and territorial age-standardized mortality, the cohort mortality rate is more representative in that it conveys more information about the health status of the members of more specific and precise groups.

The model to be estimated is based on that of Lleras-Muney (2005), and equation (1) is used in the first stage and equation (2) is used in the second stage of two-stage least squares estimation. The two equations are

$$E_{pgyt} = \partial^E + \mu' \ CL_{py} + \pi' \ X_{pgyt} + ß' \ W_{py} + f_p + f_y + f_t + \varepsilon^E{}_{pgyt} \quad (1)$$

$$D_{pgyt} = \partial^D + \kappa' \ E_{pgyt} + \omega' \ X_{pgyt} + \nu' \ W_{py} + f_p + f_y + f_t + \varepsilon^D{}_{pgyt} \ , \quad (2)$$

where E_{pgyt} is the average educational attainment level observed in census year *t*, of a specific group of individuals who have the same gender g, year of birth *y*, and province of birth p. CL_{py} is a vector of provincial compulsory schooling variables. X_{pgyt} is a vector containing the demographic characteristics of a cohort group retrieved from census year *t*. W_{py} represents a vector of provincial characteristics when individuals were 14 years old. f_p is a vector of province-of-birth fixed effects. f_y is a vector of year-of-birth fixed effects, and f_t is a vector of census year fixed effects. D_{pgyt} is the death rate between census years *t* and *t-1* for a given province of birth, gender and year-of birth cohort.

In estimating both equations, I take into account the possibility of clustered errors; i.e. that within a specific group of individuals defined by year of birth *y* and province of birth *p*, the errors are correlated in an unobservable way. For instance, although census data are randomly selected from the population in each census year, it is reasonable to argue that say, individuals born in 1915 in Nova Scotia may be influenced by the same policy. In such a case, the iid (independently and identically distributed) assumption about the error term will be violated and the OLS estimates of the standard errors will be less accurate. I thus correct the standard errors for clustering based on province and birth year in both stages.

Equation (1) captures the effect of provincial compulsory schooling laws on educational attainment. Weighted least squares (WLS) estimation (with standard errors corrected for clustering) is used to estimate equation (1), where weights are cell sizes. I use WLS instead of Ordinary Least Squares (OLS) for two reasons. The first reason is to correct for heteroskedasticity due to variability in cell sizes. For example, the number of females in the sample born in one of the Atlantic Provinces in 1932 is only 49; by comparison, the number of females born in Quebec in 1932 is 319. In such cases a heteroskadasticity problem exists and the

OLS estimates will be inefficient. The second reason is that the effect of changes in compulsory schooling laws in large provinces are likely to be greater than in smaller provinces (Oreopoulos 2005). It is thus reasonable to place more weight on what happened in population-intensive provinces, such as Quebec and Ontario.

In addition to testing the relevance of the instruments by estimating equation (1), I carry out various diagnostic tests for potential problems: endogeneity of the education variables, the validity of overidentifying restrictions and weak instruments. First, an endogeneity test is carried out to determine whether the education variables are correlated with the error term in equation (2).[4] This is necessary since if there is no correlation, the WLS estimator is more efficient than the IV estimator. Next, the Hansen J statistic is calculated in order to test overidentifying restrictions.[5] Finally, I carry out an F-test of the null hypothesis that the coefficients of the instruments are jointly zero in the first stage equation, to see if the instruments are weak. Weak instruments will cause the IV estimator to be worse than the WLS estimator.

In cases where the education variables are found to be endogenous, Instrumental Variables (IV) estimation is used to estimate equation (2) in order to address the endogeneity problems pertaining to schooling that may cause the coefficient estimates to be biased. Following Lleras-Muney (2005) and Oreopoulos (2005), I employ the changes in compulsory schooling laws as instrumental variables for education, so that endogeneity problems can be addressed and the effect of education on population health can be more accurately captured.

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[4] The endogeneity test statistic is robust and is the difference between two Sargan-Hansen statistics.

[5] The J statistic is also robust to clustering.

V. Data definitions and sources

The data used in this empirical paper are obtained from a variety of different sources. First, from the Canadian census public use microdata files (PUMF) for the years 1971, 1981, 1986, 1991, 1996 and 2001, information on birth year, province of birth, mother tongue, sex and education were retrieved. For each PUMF, the sample includes all Canadian-born individuals who were 14 years old from 1920 to 1970 inclusive. I dropped individuals born in Prince Edward Island (PEI) and those who were born in the territories and outside Canada. The rationale for dropping PEI is partly that PEI is not identified by the census place of birth variables beginning with the 1991 census. The other reason for dropping individuals born in PEI and the territories is that the populations of PEI and the territories are very small compared to that of the other provinces. For example, in 1924, the sample includes 31 males in PEI and one in the Yukon and Northwest Territories, as compared to 279 born in Quebec. Individuals who were born outside Canada are also excluded mainly because of the large measurement error in calculating their cohort mortality rates in that there is no information about their place of birth. Another rationale for excluding immigrants is that they are less likely to get education in Canada and thus less likely to be affected by compulsory schooling laws, although there is a chance that such individuals may immigrate to Canada at the time they start schooling.

Following Lleras-Muney (2005), I construct synthetic cohorts based on province of birth, gender and year of birth from the census data. Then I estimate both 5-year and 10-year mortality rates between censuses (descriptive statistics are in Table 2 and Table 3). In other words, cell death rates are estimated, where each cell includes all individuals who have the same province of birth, gender and year of birth. The reason for which I calculate both 5-year and 10-year mortality rates is because the effect of education on health may vary over time. Comparing

5-year and 10-year mortality rates will allow me to investigate such a potential difference.

To calculate the mortality rate, the number of individuals in each cell is computed for each census year, and then the proportion that have died between censuses is calculated, using the formula.

$$M_{pgy,t} = \frac{W_{t-1}N_{pgy,t-1} - W_t N_{pgy,t}}{W_{t-1}N_{pgy,t-1}}$$

where as before *p* is province of birth, *g* is gender, *y* represents year of birth and *t* is the census year. $N_{pgy,t}$ represents the number of people in cell *pgy* in census year *t*, where *t* = 1986, 1991, 1996 and 2001 for 5-year mortality rates and *t* = 1981, 1991 and 2001 for 10-year mortality rates. W_t is the weighting factor for each census year *t*, which indicates how many people one observation in a sample represents. W_t equals 100, 50, 50, 33.33, 35.71 and 37.037 for the census years 1971, 1981, 1986, 1991, 1996 and 2001 respectively.[6]

Because the census age variables are top-coded beginning in 1981, such that age 85 in fact includes all individuals who are 85 or older, I dropped those who are aged 85 or older beginning with census 1991. Otherwise, the 10-year and 5-year mortality rates for older cohorts calculated between census years 1981 and 1991 as well as 1986 and 1991 will be incorrectly estimated. As can be seen in Tables 2 and 3, the standard error of the10-year mortality rate decreases significantly from 0.27 to 0.13 after I drop those who were born before 1917 and those who were born in the Atlantic Provinces. One explanation for this change is that I cannot follow people who were born before 1917 through all censuses. Because of the top-coding problem beginning in the 1981 census PUMF, the 10-year mortality rates of cohorts who were born between 1906 and 1917 cannot be estimated after1991. After I drop cohorts who were born

[6] For the weights of each census, please see the descriptions of the census PUMFs.

before 1917, all the cohorts remaining in the sample can be tracked over all subsequent censuses and thus the set of cell mortality rates is more complete.

Dropping cohorts born in the Atlantic Provinces as well as those born before 1917 provides another robustness check. As the numbers in Table 4 indicate, dropping the Atlantic Provinces results in the loss of no more than 13% of the total observations in each census. Although it is not desirable to drop observations, a potential problem when constructing pseudo panels is measurement error. More specifically, pseudo panel construction involves using cohort means to represent population means, and the size of the error in measuring the population mean depends on the cohort size. In my dataset, the cohort sizes vary greatly across provinces. Although I have intentionally used cohort weights to put more weight on the effect of changes in compulsory schooling laws in population-intensive provinces such as Ontario and Quebec, the measurement errors remain and are likely to be particularly serious in the Atlantic Provinces where cohort sizes are small. Dropping cohorts who were born in the Atlantic Provinces will reduce the variety of cohort sizes and thus reduce the measurement error. This is reflected in the smaller standard deviation of the mortality rate after the Atlantic Provinces are dropped.

Special attention should be paid to negative mortality rates which are caused by measurement error when the cell mortality rate is calculated rather than using the true mortality obtained from following the same individuals over decades. More specifically, negative mortality rates are to be expected due to the random sampling in the census data, which means we face a 50 percent chance of either overestimating or underestimating the true mortality rate. Nonetheless the estimates are still consistent estimates of the true mortality rates. In the 5-year full sample, 890 negative mortality rates are observed, in comparison to 2,456 non-negative mortality rates. Another factor that contributes to negative mortality rates is emigration. Since there is no data source which records emigrants' mortality, statistically emigrants are considered to be dead.

The vector X_{pgyt} of demographic characteristics includes two variables, *male* and *french*. I include *french* in both the first-stage and second-stage equations in order to capture the impact of culture on educational attainment and mortality. More specifically, *french* represents the proportion of individuals in a cell whose mother tongue is French. A dummy variable representing males, *male,* is also included in both the first-stage and second-stage equations in order to allow for the potential effect on both educational attainment and mortality of gender differences. Information on how I constructed the vector X_{pgyt} from Canadian census public use microdata files is provided in the Data Appendix.

Three educational variables were constructed: *yrs_educ* (the average years of education in cell *pgy* for census year *t*), *hsdiploma* (the proportion of individuals who have a high school diploma in cell *pgy in* census year *t*) and *unideg* (the proportion of individuals who have a university degree in cell *pgy in* census year *t*). All three variables are constructed from Canadian census public use microdata files (PUMF) for the years 1971, 1981, 1986, 1991, 1996 and 2001. I aim to investigate the impact of different levels of education on mortality rate and thus in equation (2) I use these three educational variables individually. Detailed information on how these three variables were constructed is presented in the Data Appendix.

Data on Canadian compulsory schooling laws was kindly provided by Philip Oreopolous. He used these data in his 2005 paper on the relationship between education and adult income, where cohorts were matched to the compulsory dropout ages that were in place in their province of birth when they were 14 years old, and to the maximum school entry age laws that were in place when cohorts were six years old. More specifically, I construct three entry-age indicators (*entryage6, entryage7, entryage8)*, and four dropout-age indicators (*dropoutage*12, *dropoutage14, dropoutage15* and *dropoutage16)*. For instance, *entryage6* equals one if a province required its residents to start school at age six, and *dropoutage12* equals one when a province permitted students to leave school at age 12.

Canada's health care system embodies the Canadian perspective that medically necessary health care services should be provided on the basis of need, rather than the ability to pay. This feature of the Canadian health care system contrasts strongly with that of the United States, and cannot be ignored when investigating the impact of education on Canadian mortality. Therefore, three dummy variables, *hosp_ins*, *comp_ins*, *med_or_hosp*, are constructed in accordance with Table 1, which summarizes the years in which each province introduced hospital insurance and comprehensive medical insurance. These variables are constructed so as to reflect the state of health insurance in province *p* during the year that individuals in cell *pgyt* turned 14. The dummy variable *hosp_ins* captures the effect of provincial hospital insurance only; more specifically, *hosp_ins* equals 1 if a province had implemented a public hospital insurance plan for its residents, and equals 0 if there is no public hospital insurance available in a province. The indicator variable *comp_ins* captures the effect of both provincial hospital and medical insurance; i.e, if *comp_ins* equals 1, it means that the province provides comprehensive insurance to residents. Finally, the dummy variable *med_or_hosp* is a combination of *hosp_ins* and *comp_ins*, which equals 1 if either public hospital or medical insurance is available in a province, and 0 if neither type of publicly-funded health insurance exists. It is obvious that *hosp_ins*, *comp_ins*, and *med_or_hosp* are linearly related, and thus I use no more than two of them to represent the effect of the Canadian health care system on Canadian mortality in equation (2).

I also construct a set of two dummy variables, *birth_hosp_ins* and *birth_comp_ins*, corresponding to the year in which the cohort was born, which allow the potential effect of provincial health insurance when individuals were young. The rational for constructing this set of dummy variables is the possibility that the presence of health insurance mattered more at an earlier age than when the individual was 14. On the one hand, comparing the potentially different impacts of health insurance on population mortality rates allows me to further investigate whether individuals' early health plays significant role in the long term. On the other hand, this comparison will cast light upon reasonable resource distribution. For instance, if publicly funded health insurance improves population health more significantly in individuals' childhoods, it is reasonable for governments to focus on reducing health disparities starting in childhood.

Finally, the vector W_{py} of provincial control variables includes *teacher_ps* (number of teachers per student), *operexpen_ps* (the log of real operating expenditure per student), and *rural* (fraction of the population that lives in rural areas), where the first two variables are obtained from the *Historical Statistics of Canada* (Leacy 1983) and the variable *rural* is obtained from Statistics Canada (2011). All three variables are matched with the year when cohorts turned 14 years old and the province of birth. Detailed information on how these three variables are constructed is provided in the Data Appendix. Table 5 describes the variable names and their definitions. In the remainder of the paper, variables names will be used.

VI. Results

The results are presented in this section. In section A, I investigate whether compulsory schooling laws are good instruments for education. In section B, I examine the effect of education on mortality. Both the 5-year and 10-year models with three different samples are estimated. The first sample is the full sample that includes Canadian-born individuals who were

14 years old between 1920 and 1970. In order to test model sensitivity, I then drop cohorts who were born before 1917 (Subsample 2), and finally also drop cohorts born in the Atlantic Provinces (Subsample 2).

VI. A. The Effect of Compulsory Schooling Laws on Education

Although heteroskedasticity is to be expected when working with pseudo-panel data with considerable variability in cohort sizes, I begin by testing for heteroskedasticity in equation (1). The Breusch-Pagan / Cook-Weisberg test detects heteroskedasticity when equation (1) is estimated using OLS, with years of education, the proportion of those who have graduated from high school or those who have a university degree as the dependent variables. After doing Weighted Least Squares (WLS) estimation, where the weights are the cell sizes, heteroskedasticity still exists in both regressions, so I employ robust standard errors that correct for clustering as well in order to generate more reliable test statistics. As shown in Table 6, heteroskedasticity is detected when *yrs_educ* and *hsdiploma* are the dependent variables (the p-values all equal zero). In the weighted 5-year full sample, I can reject the null that the variance is constant at the 5 percent significance level when *unideg* is the dependent variable. However, I cannot reject the null in the 5-year full sample when WLS is used and in the10-year full sample for both the OLS and WLS estimates at 5 the percent significance level.

Table 7 presents the results showing the effect of compulsory schooling laws on years of education (*yrs_educ*) in equation (1). The first three columns present the results for the 5-year sample, and the last three columns present the results for the 10-year sample. In each case, the omitted dropout age is *dropoutage12* and the omitted entry age is no or a lower school-entry age than *entryage6*.

As shown in Table 7, the compulsory schooling entry ages play a significant role in increasing educational attainment. We would expect that earlier mandatory school-entry ages raise years of education. The results show that in the 5-year full sample, the average years of education rise by 0.53 years (in column 1 of Table 7) with a school-entry age of 6 compared to no or a lower mandatory school-entry age, after controlling for provincial, birth cohort, and census year fixed effects. This coefficient is statistically significant in all 5-year and 10-year samples. For example, in the 10-year sample that excludes cohorts born before 1917, the average years of education are 0.50 years higher with a school-entry age of 6 than with to no or a lower mandatory school-entry age (column 4 of Table 7). In the 5-year full sample, increasing the entry age to 7 instead of 6 has a slightly lower positive effect on the average years of education (0.49 years versus 0.53 years). The effect of increasing the mandatory school-entry age to age 8 is again smaller than that for a school entry age of 7 (0.36 years verses 0.49 years in the 5-year full sample). This lower estimate is consistent with other specifications, as well as with the expectation that earlier mandatory school-entry ages raise years of educational attainment more. The effect should decline when the mandatory school entry age is increased.

Overall, higher dropout ages also contribute to more years of education in both the 5-year and 10-year samples. For instance, in column 4 of Table 7 for the 10-year full sample, average years of education are 0.43 years higher when the compulsory dropout age increases to 15. This means people will increase their years of education by 0.43 years on average when the compulsory schooling dropout age is increased to 15 as compared to no or lower mandatory school-dropout age. Increasing the dropout age to 16 instead of 15 has a slightly lower effect on average years of education (0.38 years instead of 0.43 years).

The effects of compulsory schooling law variables on the proportion of individuals who graduated from high school (*hsdiploma*) and university (*unideg*) are presented in Tables 8 and 9 respectively. Since the Canadian compulsory schooling laws (1900-2000) are associated only with educational attainment below high school, "we should not expect the laws to substantially affect education attainment beyond high school" (Oreopoulos 2005, 16). As shown in Table 8, only mandatory school-entry ages have a small positive effect on the proportion who have graduated from high school. For instance, in column 1 of Table 8 for the 5-year full sample, the proportion of high school graduates will increase 5.12 percentage points with a school-entry age of 6 as compared to no or a lower mandatory school-entry age. Raising the entry age to 7 versus 6 has slightly larger positive effect on high school graduation (the proportion of high school graduates is 0.23 percentage points higher). The mandatory-school dropout ages do not appear to play any significant positive role when it comes to the effect on the proportion of those graduated from high school or university.

As far as the provincial controls are concerned, operating expenditures per student are strongly correlated with the years of education obtained. For instance, column one of Table 7 for the 5-year full sample indicates that increasing the operating expenditures by 10% will contribute 0.068 more years of education obtained.[7] Similar significant positive effects on educational attainment can be observed in other specifications in both the 5-year and 10-year samples. However, both the number of teachers per student and the fraction of the population living in rural areas do not seem to have a significant relationship with schooling.

[7] If the mean of real operating expenditure per student for 5-year full sample is E, a 10% increase in this figure would amount to E (1+10%) or 1.1E.The resulting change in the log of real operating expenditures per student would be Ln(1.1E)-Ln(E)=Ln(1.1), or 0.0953. Average years of education would then increase by 0.0953*0.717, or 0.068 years.

The aforementioned estimation results have similarities and differences with those of Oreopoulos (2005), whose sample also includes those Canadian-born individuals who were 14 years old between 1920 and 1970.[8] Both of us find that compulsory school-entry ages significantly increase average educational attainment at the secondary level, but do not affect educational attainment beyond high school. For the compulsory school-leaving ages, my coefficients for dropoutage14 are different from those of Oreopoulos (2005), where the coefficient is positive and statistically significant; however, I obtain a significant negative coefficient in Subsample 2 of both the 5-year and 10-year samples. At the same time, there are differences in the magnitudes of certain coefficients of the compulsory schooling and provincial control variables. For instance, in my 5-year full sample, average years of education rise by 0.53 years with a school-entry age of 6 compared to no or a lower mandatory school-entry age; Oreopoulos (2005) obtains a coefficient of 0.095 years. Similarly, in my 5-year full sample, the coefficient of operating expenditures is 0.717 as compared to 0.177 in Oreopoulos (2005).

There are a couple of possible explanations for these differences. Firstly, our samples are different in that my samples are constructed from PUMFs that constitute a one percent sample for 1971, a 2 percent sample for 1981 and 1986, a 3 percent sample for 1991, a 2.8 percent for 1996 and a 2.7 percent for 2001; Oreopoulos (2005) uses a 33 percent sample from the 1971 Census, and 20 percent samples from the 1981, 1986, 1991, 1996, and 2001 Census Master files. Moreover, I group the censuses into 5-year and 10-year samples, rather than pooling all the censuses as does Oreopoulos. Thirdly, when I check model sensitivity, I also drop individuals who were born before 1917 or born in the Atlantic Provinces (Newfoundland, Nova Scotia, and

[8] In Oreopoulos (2005), the dependent variable is grade attainment (mean=10.27), which is quite similar to my variable *yrs_educ*, which has a mean of 10.24 in my 10-year full sample.

New Brunswick). Fourthly, we use slightly different provincial control variables in that I do not include variables such as the fraction in the province employed in manufacturing and an exemption indicator.

In a nutshell, the above results indicate that compulsory school laws in Canada have a significant impact on average years of education obtained. In addition to the effect of compulsory schooling laws on educational attainment, I also find that people whose mother tongue is French have less years of education than those whose mother tongue is not French. For instance, in the 10-year full sample, the fraction of people whose mother tongue is French is negatively correlated with years of education.

In the remainder of this section, I present the results of diagnostic tests to determine whether or not the compulsory schooling law variables are legitimate instruments for education, namely tests for weak instruments and overidentification. All the test statistics are reported in Table 10. Because the standard errors are clustered by province of birth and year of birth, the critical values for formal tests for weak instruments developed by Stock and Yogo (2005) are not directly applicable. Therefore I simply test the joint significance of the compulsory schooling law variables and find most of the F statistics are larger than 10. The only exceptions are those when *unideg* is instrumented in Subsample 2 for both 5-year and 10-year mortality rates and the 10-year full sample. These results suggest that compulsory schooling law variables are strongly correlated with *yrs_educ* and *hsdiploma*, but less strongly correlated with *unideg.*

However, the overidentification tests for whether or not the extra instruments are exogenous cast doubt on the reliability of compulsory schooling laws as instruments for education. In both the full sample and Subsample 1, which excludes cohorts who were born before 1917, the overidentification tests on the instruments indicate that at least one compulsory schooling law variable is endogenous. When I also exclude cohorts born in the Atlantic Provinces, I cannot reject the null that the additional instruments are exogenous when *yrs_educ* is instrumented in Subsample 2 for both 5-year and 10-year mortality rates. When *hsdiploma* and *unideg* are instrumented, I cannot reject the null at the 1% significance level for both 5-year and 10-year mortality rates. In summary, the instruments are appropriate in Subsample 2.

In previous studies of health in which compulsory schooling laws are employed as instruments for education, only Lleras-Muney (2005) and Clark and Royer (2013) briefly mention their diagnostic tests for the credibility of the instruments. In both papers, the compulsory schooling laws are exogenous instruments (the exception is in Clark and Royer (2013), where endogeneity of the instruments exists when smoking behavior is considered as a health indicator). Oreopoulos (2005) and Mazumdar (2008) do not report the results of any overidentification tests.

VI.B The Effect of Education on Mortality

In this section I examine the effect of education, as measured by *yrs_educ* (years of education), *hsdiploma* (proportion with a high school diploma) and *unideg* (proportion with a university degree) respectively on the mortality rate.[9] Firstly I report the results of endogeneity tests for the three education variables (*yrs_educ*, *hsdiploma*, and *unideg*). As shown in Table 10, all these education variables appear to be endogenous in the full sample and Subsample 1. However, in Subsample 2 I cannot reject the null that the education variable is exogenous at the 10 % level of significance, except in the case of 5-year mortality rates for *hsdiploma* and *unideg*. In these two cases I can reject the null hypotheses at the 5% level of significance. Given that

[9] The ivreg210 command written by Baum, Schaffer and Stillman (2015) was used to carry out IV estimation and compute related test statistics.

WLS estimation is more efficient than weighted IV estimation when these is no endogeneity, I also present those unbiased WLS estimates for Subsample 2, where I exclude cohorts born before 1917 and also those born in the Atlantic Provinces.

Table 11 presents the effect of years of education on the mortality rate. Since the diagnostic tests of Table 10 imply that the instruments are not good for the full sample and Subsample 1, Table 11 contains only results for Subsample 2. Although the overidentification tests show that the instruments are exogenous in Subsample 2, at the same time the endogeneity tests imply that WLS estimation is appropriate, which means if we consider efficiency the WLS estimates are more reliable than the IV estimates. Nonetheless, both IV and WLS estimates are presented for purpose of comparison.

In column 1 of Table 11 for the 5-year Subsample 2, the IV estimate of the coefficient of *yrs_educ* is -0.0266 and is significant at the 1% level which suggests that increasing average years of education by one additional year lowers the 5-year mortality rate by at least 2.6 percentage points. The corresponding WLS estimate implies a smaller decrease in the 5-year mortality rate – 1.9 percentage points. In column 4 of Table 11 for the 10-year Subsample 2, the IV estimate implies that increasing average years of education by one year lowers the 10-year mortality rate by 3.46 percentage points, which is slightly smaller than the estimate of Lleras-Muney (2005), who found that one additional year of education lowers the 10-year mortality rate by 3.6 percentage points. My WLS estimate is 2.3 percentage points (column 4 of Table 11).

The significant negative effect of years of education on mortality rates remains when I exclude the provincial control variables. For instance, in column 3 of Table 11 for 5-year mortality rates, the WLS estimate is -0.0189, which is very close to that in column 2 (-0.0199). Similarly, in column 6 of Table 11, increasing educational attainment by one year lowers the 10-

year mortality rate by 1.99 percentage points, compared to 2.34 percentage points when I include provincial control variables. Overall, the significant negative effect of *yrs_educ* on health does not vary much whether I include provincial control variables or not.

Table 12 presents the results when education is measured as the proportion of the cell population who have graduated from high school (*hsdiploma)*. Again, results are provided only for Subsample 2, because the diagnostic tests implied that the instruments were not legitimate for the other samples. IV estimates are provided for 5-year mortality rates only, because *hsdiploma* appeared to be endogenous only in that case. Those results indicate that the effect of graduating from high school is in accordance with that of years of education. For example, in column 1 of Table 12 for the 5-year mortality rate, the IV estimate is -0.237, which means that if we increase the proportion of graduates from high school by 5%, the 5-year mortality rate will decrease by 0.64 percentage points.[10] In column 2 of Table 12 for the 10-year mortality rate, the WLS estimate is -0.187 (the significance level is 1%), which means that a 5% increase in the proportion of those who graduate from high school lowers the 10-year mortality by at least 0.49 percentage points.[11]

Finally, Table 13 presents the results when education is measured as the proportion of the cohort population that has a university degree. Again results are provided only for Subsample 2 for the same reason mentioned above. The proportion with a university degree only has a significantly negative effect on the 5-year mortality rate (the IV estimate is -0.599 and significant

[10] The mean of *hsdiploma* in in the 5-year Subsample 2 is 0.53, which means the proportion of the cohort population who have graduated from high school is 53% on average. A 5% increase in this percentage would amount to a 2.7 percentage point increase in the proportion of the cohort population that has graduated from high school. The resulting decrease in the mortality rate will be 0.237*2.7, or 0.64 percentage points.

[11] The mean of *hsdiploma* in the 10-year Subsample 2 is 0.53. A 5% increase in this figure is a 2.7 percentage point increase in the proportion of the cohort population who have graduated from high school. This means the mortality rate will decrease by 0.49 percentage points (0.187*2.7).

at the 1% level), which means that a 5% increase in the proportion of people who have a university degree lowers the 5-year mortality by at least 0.3 percentage points.[12]

It is worth mentioning that the effect of provincial public insurance is very weak. In column 2 of Table 13, *comp_ins* (medical and hospital insurance) has a statistically significant positive effect (the WLS estimate is 0.0215) on the10-year mortality rate; *hosp_ins* (hospital insurance) also has a positive effect on the 10-year mortality rate (the WLS estimate is 0.0130). For other regressions, neither *comp_ins* nor *hosp_ins* has a significant effect on mortality rates. The insignificance of the coefficients does not depend on whether the publicly-funded health insurance variables are matched with the year of birth or with the year at which individuals were 14.[13]

There are several potential explanations for these unexpected results with respect to public health insurance. Firstly, I may not have constructed the model appropriately, resulting in functional form misspecification. For instance, my model excludes the interaction term *hosp_ins*rural*, that would allow the effect of *hosp_ins* on mortality rates to differ with the proportion of people who live in rural areas. Another possibility is that the effect of *hosp_ins* alters with different levels of education, in which case I should have included the interaction term *hosp_ins*yrs_educ*. Secondly, there is a great chance that publicly-funded health insurance does improve population health, but not necessarily by lowering the average mortality rate. For example, publicly-funded medical insurance may have a negative effect on the incidence of asthma and diabetes, which are not included as measures of health in this paper. Weinstein and

[12] The mean of *unideg* in the 5-year Subsample 2 is 0.11. A 5% increase in this figure is a 0.55 percentage point increase in the proportion of the cohort population who has a university degree. This means the mortality rate will decrease by 0.33 percentage points (0.599*0.55).

[13] The only exception is in 5-year Subsample 2 when *hsdiploma* is instrumented. The coefficient of *birth_hosp_ins* is 0.0106 and significant at 10%. However, when *birth_hosp_ins* is used, the coefficients for *hsdiploma* and *unideg* in 5-year mortality rate are not statistically significant.

Skinner (2010) argue that no association or a negative association between health expenditures and health outcomes can be explained by the fact it is difficult to measure the value of some health expenditures, “such as testing of prostate-specific antigen levels in older men with limited life expectancy, arthroscopic surgery for osteoarthritis of the knee, and ventricular reconstruction surgery” (Weinstein and Skinner 2010, 4).

Other features of the results that are worth pointing out are the following. Firstly, the estimates for 5-year mortality rates are consistent with those for 10-year mortality rates. For instance, the 5-year and 10-year WLS estimates of the effect of *yrs_educ* on mortality rates are -0.0199 and -0.0234 respectively (columns 2 and 4 of Table 11). This result suggests that educational attainment has a positive effect on health both in the short and long term. Secondly, I find that the higher the proportion of people whose mother tongue is French, the lower the mortality rate. This result is statistically significant in both the 5-year and 10-year samples and does not alter when the education variable changes or when the provincial variables are excluded. For example, in column 2 of Table 11 for the 5-year sample, the WLS estimate is -0.0311, which means if the proportion of people whose mother tongue is French increases by 5 percentage points, the average mortality rate will fall by 0.16 percentage points. Thirdly, I find that males on average have higher mortality rates than females. This result is statistically significant in both the 5-year and 10-year samples and does not vary when the educational variable changes or if the provincial control variables are excluded. For example, in column 6 of Table 11 for the 10-year subsample 2 (where I exclude the provincial variables), the WLS estimate is 0.0282, which means the mortality rates of males are on average 2.82 percentage points higher than those of females.

VII. Conclusion

In this paper, I expand upon the previous literature that attempts to identify whether there is a causal effect of education on health. I closely examine the effects of education induced by Canadian compulsory schooling laws (1920-1970) on health, as measured by both the 5-year and 10-year mortality rates. Following Lleras-Muney's (2005) approach, I construct synthetic cohorts (pseudo panels) using Canadian census data and address the potential endogeneity of education.

I find that the compulsory school laws in Canada have a significant impact on average years of education obtained, which is in accordance with Oreopoulos' (2005) findings, despite some differences in certain coefficients of the instruments. I also report the results of diagnostic tests regarding the reliability of the compulsory schooling law instruments, which are not conducted in Oreopoulos (2005). The results suggest that the instruments are exogenous and strongly correlated with the educational variables in a subsample from which I exclude people who were born before 1917 and also those born in the Atlantic Provinces.

I also find that the significant negative effect of *yrs_educ* (years of education obtained) on health does not alter much whether or not I include provincial control variables. This significant result applies to both 5-year and 10-year mortality rates, which suggests that the positive effect of education on health is robust over time. It is worth mentioning that the WLS estimates are more efficient than the IV estimates when *yrs_educ* is used as the education measure, in that I cannot reject the null of the endogeneity test that WLS estimates are appropriate in Subsample 2 for both 5-year and 10-year mortality rates. My WLS estimate for the 10-year mortality rate is 2.3 percentage points, which is smaller than that in Lleras-Muney (2005), who found that increasing years of education by one additional year lowers the 10-year mortality rate by 3.6 percentage points. Although we should not expect compulsory schooling laws to have a large effect on educational attainment beyond high school, I sill find significant negative effects of *hsdiploma* (the proportion of people who graduate from high school) and *unideg* (the proportion of people who have a university degree) on mortality rates in Subsample 2.

I also find little measureable effect of provincial public insurance, perhaps due to misspecification of my model or the possibility that there are non-measureable results other than a lower mortality rate due to implementing publicly-funded health care insurance. Males on average have higher mortality rates than females.

There are certain limitations of my paper. Firstly, I am forced to exclude cohorts who were born before 1917 (because of the top coding problem). In addition, because of the unavailability of certain variables in the PUMF for 1976, my samples do not include any observations from the 1976 census. I am thus unable to capture any potential educational effect on health induced by the compulsory schooling laws for certain cohorts. Secondly, for this paper I was unable to collect data on more provincial control variables such as the proportion of people who work in the manufacturing sector, the number of physicians per capita, and the average salary or wage per capita in different provinces. All these provincial control variables are probably related to health and education. Thirdly, the evaluation of the effects of provincial health insurance is limited by the measures of health used. A set of health indicators should be examined rather than just using the mortality rate.

I suggest that future research on the casual relationship between education and health pursue the following directions. Firstly, census Master Files should be used in further studies so that some of the observations that I exclude because of the top-coding problem in the PUMFs can be included. Secondly, one could attempt to include more measures of health in order to investigate the mechanism through which educational attainment may affect health, and the relationship between the two components of human capital. Thirdly, one could attempt to resolve the functional misspecification problems of my model.

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Table 1. Year of introduction of hospital and medical insurance

		Hospital Insurance	Medical Insurance
1	Newfoundland	1936[1]	1969
2	PEI	1959	1970
3	Nova Scotia	1959	1969
4	New Brunswick	1959	1971
5	Quebec	1961	1970
6	Ontario	1959	1969[2]
7	Manitoba	1958	1969
8	Saskatchewan	1947	1962
9	Alberta	1950[3]	1963
10	British Columbia	1948[4]	1965

SOURCE: Marchildon (2012); Health Canada (2012); Canadian Museum of History (2010)

NOTES:

1. In 1934, Newfoundland and Labrador introduced a cottage hospital plan that provided hospital and medical services for citizens in the outports. In 1936, there were 23 government-run cottage hospitals established. “By 1954, 120,000 inhabitants were paying annual premiums for doctors, nurses and hospitals along the province’s coasts.”
2. In 1963, Ontario government introduced Ontario Medical Services Insurance Plan (OMSIP) that subsided the citizens who could not afford the private insurance.
3. In 1950, Alberta introduced a limited hospital insurance that is financed and administered municipally.
4. In 1948, British Columbia established government-supported hospital insurance, British Columbia Hospital Insurance Services, and provided its citizens with limited hospital insurance.

Table 2. 5-year sample descriptive statistics

	Full sample1		Subsample 1		Subsample 2	
	mean	sd	mean	sd	mean	sd
5-year mrate	0.130	0.244	0.105	0.231	0.052	0.117
male	0.500	0.500	0.500	0.500	0.500	0.500
birthyear	1,933.7	13.692	1,937	11.442	1,936.5	11.546
french	0.169	0.273	0.169	0.273	0.177	0.308
mothereng	0.742	0.257	0.749	0.258	0.713	0.279
hsdiploma	0.495	0.166	0.524	0.153	0.531	0.155
yrs_educ	10.490	1.245	10.710	1.110	10.794	1.131
unideg	0.098	0.061	0.105	0.059	0.108	0.055
comp_ins	0.073	0.260	0.084	0.277	0.104	0.306
hosp_ins	0.312	0.463	0.358	0.480	0.325	0.468
rural	40.676	12.510	39.433	12.348	33.283	8.312
operexpen_ps	6.730	0.660	6.828	0.636	6.993	0.553
teacher_ps	0.037	0.005	0.037	0.005	0.038	0.004
N	3,184		2,768		1,920	

NOTES:
1.Full sample includes all the Canadian-born individuals who were 14 years old between 1920 to 1970 (those who were born in PEI are excluded because of the aforementioned reasons)
2. Subsample 1 excludes cohorts who were born before 1917
3. Subsample 2 excludes cohorts born in Atlantic Provinces as well as those born before 1917

Table 3. 10-year sample descriptive statistics

	Full sample		Subsample1		Subsample2	
	mean	sd	mean	sd	mean	sd
10-year mrate	0.176	0.268	0.149	0.256	0.083	0.142
male	0.500	0.500	0.500	0.500	0.500	0.500
birthyear	1,933.4	13.919	1,937	11.443	1,936.5	11.547
french	0.172	0.276	0.171	0.275	0.179	0.309
mothereng	0.744	0.258	0.751	0.258	0.714	0.279
hsdiploma	0.488	0.164	0.519	0.150	0.528	0.152
yrs_educ	10.412	1.273	10.663	1.113	10.763	1.123
unideg	0.094	0.061	0.103	0.059	0.106	0.055
comp_ins	0.072	0.259	0.084	0.277	0.104	0.306
hosp_ins	0.308	0.462	0.358	0.480	0.325	0.469
rural	40.802	12.537	39.433	12.349	33.283	8.312
operexpen_ps	6.721	0.664	6.828	0.636	6.993	0.553
teacher_ps	0.036	0.005	0.037	0.005	0.038	0.004
N	2,412		2,076		1,440	

NOTES:

1.Full sample includes all the Canadian-born individuals who were 14 years old between 1920 to 1970 (those who were born in PEI are excluded because of the aforementioned reasons)

2. Subsample 1 excludes cohorts who were born before 1917

3. Subsample 2 excludes cohorts born in Atlantic Provinces as well as those born before 1917

Table 4. Number of observations per province

Province	1971	1981	1986	1991	1996	2001
Newfoundland	3,532	6,842	7,120	2,473	1,944	1,571
Nova Scotia	5,546	10,616	9,882	3,762	3,237	2,766
New Brunswick	4,543	8,847	8,332	3,452	2,898	2,466
Quebec	35,616	66,464	62,318	88,944	75,031	66,804
Ontario	32,034	59,243	56,487	80,067	67,261	59,175
Manitoba	6,697	12,389	11,837	16,515	13,840	11,904
Saskatchewan	8,550	16,258	15,293	21,857	18,236	15,492
Alberta	7,482	14,033	13,464	18,941	16,477	14,718
British Columbia	6,195	11,364	11,196	16,249	14,067	12,887
Total	110,195	20,6056	195,929	252,260	212,991	187,783

SOURCE: Author’s calculations based on census PUMFs.

Table 5. Variable definitions

Variable name	Variable definition
yrs_educ	average years of education of cohort
hsdiploma	proportion of individuals in cohort who graduated from high school
unideg	proportion of individuals in cohort who have a university degree
male	dummy variable equal to 1 if male cohort, 0 otherwise
french	proportion of individuals in cohort whose mother tongue is French
hosp_ins	dummy variable equal to 1 if a province provides a public hospital insurance plan for its residents when they were 14, 0 otherwise
comp_ins	dummy variable equal to 1 if a province provides comprehensive insurance to residents when they were 14, 0 otherwise
med_or_hosp	dummy variable equal to 1 if either public hospital or medical insurance is available in a province, 0 otherwise
birth_hosp_ins	dummy variable equal to 1 if a province provides a public hospital insurance plan for its residents when they were born, 0 otherwise
birth_comp_ins	dummy variable equal to 1 if a province provides comprehensive insurance to residents when they were born, 0 otherwise
teacher_ps	number of teachers per student
operexpen_ps	log of real elementary and secondary school operating expenditures per student
rural	fraction of the individuals in cohort that live in rural areas
entryage6	dummy variable equal to 1 if mandatory school entry age is 6, 0 otherwise
entryage7	dummy variable equal to 1 if mandatory school entry age is 7, 0 otherwise
entryage8	dummy variable equal to 1 if mandatory school entry age is 8, 0 otherwise
dropoutage14	dummy variable equal to 1 if mandatory school dropout age is 14, 0 otherwise
dropoutage15	dummy variable equal to 1 if mandatory school dropout age is 15, 0 otherwise
dropoutage16	dummy variable equal to 1 if mandatory school dropout age is 16, 0 otherwise
5-year mortality rate	5-year mortality rate of cohort
10-year mortality rate	10-year mortality rate of cohort

Table 6. Breusch-Pagan / Cook-Weisberg test for heteroskedasticity

Deppendent Variable	5-year full sample		10-year full sample	
	unweighted	weighted	unweighted	weighted
yrs_educ	118.32 (0.0000)	74.41 (0.0000)	79.01 (0.0000)	51.17 (0.0000)
hsdiploma	77.93 (0.0000)	62.20 (0.0000)	54.26 (0.0000)	40.18 (0.0000)
unideg	0.65 (0.4185)	3.87 (0.0490)	0.83 (0.3635)	3.57 (0.0587)

NOTES: Values in parentheses are p-values. All equations include province of birth, year of birth, and census fixed effects.

Table 7. The effect of compulsory schooling laws on years of education

yrs_educ	5-year mortality			10-year mortality		
	Full sample	Sub-sample 1	Sub-Sample 2	Full sample	Sub-sample 1	Sub-Sample 2
entryage6	0.532***	0.493***	0.478***	0.496***	0.489***	0.468***
	(8.32)	(7.36)	(7.18)	(8.10)	(7.28)	(7.05)
entryage7	0.485***	0.279	0.454	0.505***	0.331*	0.782*
	(3.32)	(1.38)	(1.34)	(4.05)	(1.77)	(1.95)
entryage8	0.360***	0.302***	0.268***	0.340***	0.327***	0.288***
	(4.26)	(3.49)	(2.97)	(4.02)	(3.59)	(3.11)
dropoutage14	-0.0544	-0.0512	-0.255***	0.123	0.0532	-0.199***
	(-0.66)	(-0.58)	(-5.23)	(1.35)	(0.55)	(-4.07)
dropoutage15	0.254**	0.250**	-0.0641	0.433***	0.344***	-0.0352
	(2.23)	(2.11)	(-1.45)	(3.43)	(2.63)	(-0.79)
dropoutage16	0.244***	0.228***		0.378***	0.288***	
	(3.35)	(2.84)		(4.58)	(3.18)	
operexpen_ps	0.717***	0.758***	1.194***	0.646***	0.694***	1.185***
	(3.14)	(3.30)	(11.19)	(2.61)	(2.69)	(11.41)
teacher_ps	0.603	-1.271	0.427	0.153	2.078	4.800
	(0.11)	(-0.21)	(0.06)	(0.03)	(0.33)	(0.69)
rural	0.00336	0.00296	-0.00286	0.00540	0.00258	-0.00315
	(0.56)	(0.48)	(-0.48)	(0.89)	(0.41)	(-0.53)
french	-1.201***	-1.101***	-0.618*	-1.176***	-1.020***	-0.292
	(-4.45)	(-3.83)	(-1.68)	(-3.61)	(-2.92)	(-0.65)
male	-0.0693***	-0.0542***	-0.0414***	-0.0665***	-0.0457***	-0.0316**
	(-5.19)	(-4.09)	(-3.02)	(-4.60)	(-3.20)	(-2.13)
constant	3.721***	3.674***	1.820**	3.341***	3.723***	1.362*
	(3.71)	(3.17)	(2.54)	(3.21)	(2.93)	(1.92)
N	3,184	2,768	1,920	2,412	2,076	1,440
R^2	0.964	0.964	0.976	0.964	0.963	0.975
F	710.7	697.9	922.3	733.2	703.8	810.9

NOTES: Values in parentheses are t-statistics. *denotes significance at 10%,** denotes significance at 5%, and ***denotes significance at 1%. The dependent variable for all equations is *yrs_educ*. All equations were estimated using WLS, with standard errors corrected for clustering based on province and year of birth. All equations also include fixed effects for province of birth, year of birth, and census year.

Table 8. The effect of compulsory schooling laws on proportion with a high school diploma

	5-year mortality			10-year mortality		
hsdiploma	Full sample	Sub-sample 1	Sub-sample 2	Full sample	Sub-sample 1	Sub-sample 2
entryage6	0.0512***	0.0503***	0.0522***	0.0555***	0.0562***	0.0569***
	(8.15)	(7.32)	(7.43)	(9.03)	(8.09)	(8.00)
entryage7	0.0535***	0.0502***	-0.107**	0.0509***	0.0448**	-0.0415
	(3.88)	(2.61)	(-2.00)	(4.19)	(2.44)	(-0.62)
entryage8	0.0263***	0.0249***	0.0294***	0.0301***	0.0325***	0.0365***
	(3.15)	(2.79)	(3.06)	(3.34)	(3.28)	(3.39)
dropoutage14	-0.0333***	-0.0394***	-0.000489	0.000873	-0.0133	0.0125*
	(-3.08)	(-3.23)	(-0.08)	(0.09)	(-1.25)	(1.94)
dropoutage15	-0.0231*	-0.0282**	0.00147	0.0103	-0.00537	0.0106*
	(-1.82)	(-2.04)	(0.24)	(0.88)	(-0.45)	(1.94)
dropoutage16	-0.0278***	-0.0339***		-0.00424	-0.0191**	
	(-2.85)	(-3.08)		(-0.48)	(-2.08)	
operexpen_ps	0.0392**	0.0356**	0.0634***	0.0372**	0.0351*	0.0636***
	(2.31)	(2.12)	(5.13)	(2.07)	(1.94)	(5.25)
teacher_ps	-0.439	-0.298	0.336	-0.185	0.0756	0.731
	(-0.72)	(-0.43)	(0.45)	(-0.30)	(0.10)	(0.91)
rural	0.00179***	0.00167***	0.00124*	0.00209***	0.00157**	0.00129*
	(3.04)	(2.70)	(1.96)	(3.49)	(2.46)	(1.96)
french	-0.201***	-0.218***	-0.217***	-0.159***	-0.186***	-0.138*
	(-5.25)	(-5.02)	(-3.62)	(-3.36)	(-3.37)	(-1.81)
male	0.00752***	0.00823***	0.00842***	0.0124***	0.0136***	0.0135***
	(3.51)	(3.64)	(3.48)	(4.85)	(5.03)	(4.66)
constant	-0.154**	-0.120	-0.00954	-0.247***	-0.138	-0.0923
	(-2.03)	(-1.39)	(-0.11)	(-3.09)	(-1.50)	(-0.93)
N	3,184	2,768	1,920	2,412	2,076	1,440
R^2	0.957	0.955	0.966	0.950	0.945	0.956
F	1,253.3	1,154.4	1,195.5	13,27.9	1,139.3	1,708.6

NOTES: Values in parentheses are t-statistics. *denotes significance at 10%,** denotes significance at 5%, and ***denotes significance at 1%. The dependent variable for all equations is *hsdiploma*. All equations were estimated using WLS, with standard errors corrected for clustering based on province and year of birth. All equations also include fixed effects for province of birth, year of birth, and census year.

Table 9. The effect of compulsory schooling laws on proportion with a university degree

unideg	5-year mortality			10-year mortality		
	Full sample	Sub-sample 1	Sub-sample 2	Full sample	Sub-sample 1	Sub-sample 2
entryage6	0.00432	0.00495*	0.00729**	0.00426	0.00635*	0.00635*
	(1.58)	(1.66)	(2.23)	(1.47)	(1.91)	(1.74)
entryage7	-0.000716	-0.00154	-0.0429	0.00448	0.00177	0.0370
	(-0.12)	(-0.18)	(-1.40)	(0.96)	(0.28)	(1.02)
entryage8	-0.0142***	-0.0139***	-0.0102*	-0.0139***	-0.0116**	-0.0121*
	(-3.09)	(-2.86)	(-1.76)	(-2.83)	(-2.16)	(-1.86)
dropoutage14	-0.0269***	-0.0293***	-0.0101**	-0.0159**	-0.0268***	-0.00888*
	(-4.11)	(-4.00)	(-2.12)	(-2.40)	(-3.94)	(-1.96)
dropoutage15	-0.0219***	-0.0242***	-0.00781*	-0.0108	-0.0222***	-0.00720*
	(-3.12)	(-3.08)	(-1.88)	(-1.55)	(-3.04)	(-1.71)
dropoutage16	-0.0168***	-0.0190***		-0.00776	-0.0187***	
	(-3.05)	(-2.98)		(-1.46)	(-3.45)	
operexpen_ps	0.00700	0.00732	0.0156**	0.000495	0.00126	0.0132
	(1.05)	(1.04)	(2.03)	(0.07)	(0.16)	(1.57)
teacher_ps	1.281***	1.425***	1.870***	1.541***	1.885***	2.136***
	(3.62)	(3.66)	(4.48)	(3.96)	(4.33)	(4.41)
rural	-0.000200	-0.000196	-0.000564	-0.000184	-0.000356	-0.000656
	(-0.54)	(-0.49)	(-1.34)	(-0.49)	(-0.86)	(-1.50)
french	-0.0615***	-0.0623**	-0.0834**	-0.0341	-0.0260	0.00424
	(-2.86)	(-2.54)	(-2.34)	(-1.39)	(-0.91)	(0.10)
male	0.0420***	0.0424***	0.0429***	0.0443***	0.0452***	0.0462***
	(25.02)	(24.03)	(22.65)	(26.06)	(24.97)	(23.71)
constant	-0.0497	-0.0787**	-0.0565	-0.0488	-0.0538	-0.130**
	(-1.41)	(-2.00)	(-1.01)	(-1.32)	(-1.24)	(-2.12)
N	3,184	2,768	1,920	2,412	2,076	1,440
R^2	0.865	0.860	0.888	0.861	0.854	0.879
F	415.6	395.7	420.9	331.2	340.1	354.6

NOTES: Values in parentheses are t-statistics. *denotes significance at 10%,** denotes significance at 5%, and ***denotes significance at 1%. The dependent variable for all equations is *unideg*. All equations were estimated using WLS, with standard errors corrected for clustering based on province and year of birth. All equations also include fixed effects for province of birth, year of birth, and census year.

Table 10. Diagnostic tests for IV estimates

Education variable: yrs_educ	5-year mortality			10-year mortality		
	Full sample	Sub-sample 1	Sub-sample 2	Full sample	Sub-sample 1	Sub-sample 2
F statistic (Weak identification test)	24.25 (0.0000)	20.74 (0.0000)	28.94 (0.0000)	23.27 (0.0000)	18.81 (0.0000)	26.66 (0.0000)
Hansen J statistic (overidentification test)	23.31 (0.0003)	15.111 (0.00099)	2.027 (0.5669)	26.298 (0.0001)	12.087 (0.0336)	0.264 (0.9666)
Endogeneity test of endogenous regressors	12.446 (0.0004)	9.071 (0.0026)	0.233 (0.6295)	17.271 (0.0000)	13.902 (0.0002)	0.504 (0.4777)

Education variable: hsdiploma	5-year mortality			10-year mortality		
	Full sample	Sub-sample 1	Sub-sample 2	Full sample	Sub-sample 1	Sub-sample 2
F statistic (Weak identification test)	30.10 (0.0000)	26.95 (0.0000)	25.16 (0.0000)	37.72 (0.0000)	33.73 (0.0000)	37.81 (0.0000)
Hansen J statistic (overidentification tests)	15.248 (0.0094)	10.728 (0.0571)	1.926 (0.5879)	23.846 (0.0002)	10.835 (0.0548)	1.008 (0.7992)
Endogeneity test of endogenous regressors	19.048 (0.0000)	11.517 (0.0007)	3.972 (0.0463)	11.265 (0.0008)	6.975 (0.0083)	0.250 (0.6172)

Education variable: unideg	5-year mortality			10-year mortality		
	Full sample	Sub-sample 1	Sub-sample 2	Full sample	Sub-sample 1	Sub-sample 2
F statistic (Weak identification test)	10.69 (0.0000)	10.61 (0.0000)	9.04 (0.0000)	7.46 (0.0000)	10.06 (0.0000)	8.17 (0.0000)
Hansen J statistic (overidentification tests)	13.500 (0.0191)	11.370 (0.0445)	2.309 (0.5108)	24.495 (0.0002)	10.619 (0.0595)	2.148 (0.5424)
Endogeneity test of endogenous regressors	17.914 (0.0000)	8.255 (0.0041)	5.539 (0.0186)	10.849 (0.0010)	6.966 (0.0083)	2.202 (0.1378)

NOTES: values in parentheses are p-values. The dependent variable in all equations is the mortality rate. All equations were estimated using cell sizes as weights with standard errors corrected for clustering based on province of birth and year of birth. All equations also include fixed effects for province of birth, year of birth, and census year.

Table 11. IV and WLS estimates of mortality rate models including *yrs_educ*

	5-year mortality			10-year mortality		
	IV	WLS	WLS	IV	WLS	WLS
yrs_educ	-0.0266***	-0.0199***	-0.0189***	-0.0346**	-0.0234**	-0.0199***
	(-2.96)	(-3.31)	(-5.58)	(-2.38)	(-2.53)	(-3.87)
french	-0.0339***	-0.0311***	-0.0274***	-0.0349***	-0.0301***	-0.0296***
	(-5.86)	(-5.84)	(-5.61)	(-3.75)	(-4.15)	(-4.38)
male	0.0179***	0.0182***	0.0182***	0.0277***	0.0281***	0.0282***
	(8.77)	(8.53)	(8.62)	-9.65	(9.44)	(9.57)
comp_ins	-0.00657	-0.00383		0.00882	0.0132	
	(-0.96)	(-0.58)		-0.74	(1.18)	
hosp_ins	-0.00171	-0.000315		0.00623	0.00862	
	(-0.36)	(-0.07)		-0.81	(1.18)	
rural	-0.0000204	-0.0000351		0.000118	0.000108	
	(-0.04)	(-0.06)		-0.13	(0.12)	
Log provincial school expenditure_ps	0.0190	0.00744		0.0397	0.0205	
	(1.10)	(0.60)		-1.52	(1.04)	
teacher_ps	0.528	0.670		0.519	0.712	
	(0.97)	(1.24)		-0.75	(1.07)	
constant	0.155**	0.157**	0.230***	0.07	0.0760	0.247***
	(2.14)	(2.11)	(5.31)	-0.68	(0.71)	(3.92)
N	1,920	1,920	1,920	1,440	1,440	1,440
R^2	0.288	0.288	0.288	0.35	0.350	0.349
F	130.5	139.7	137.7	110.9	97.11	93.71

NOTES: All estimates are for Subsample 2. Values in parentheses are t-statistics. *denotes significance at 10%,** denotes significance at 5%, and ***denotes significance at 1%. All equations were estimated using cell sizes as weights, with standard errors corrected for clustering based on province and year of birth. All equations also include fixed effects for province of birth, year of birth, and census year.

Table 12. IV and WLS estimates of mortality rate models including *hsdiploma*

	5-year mortality IV	10-year mortality WLS
hsdiploma	-0.237***	-0.187***
	(-2.75)	(-2.87)
french	-0.0241***	-0.0186***
	(-5.26)	(-3.05)
male	0.0212***	0.0314***
	(8.34)	(9.59)
comp_ins	-0.00362	0.0154
	(-0.52)	(1.39)
hosp_ins	-0.000858	0.00936
	(-0.17)	(1.30)
rural	0.000258	0.000317
	(0.45)	(0.34)
Log provincial school expenditure_ps	-0.00380	-0.000115
	(-0.35)	(-0.01)
teacher_ps	0.836	0.921
	(1.58)	(1.39)
constant	0.159**	0.0746
	(2.08)	(0.69)
N	1,920	1,440
R^2	0.281	0.352
F	178.0	90.13

NOTES: All estimates are for Subsample 2. Values in parentheses are t-statistics. *denotes significance at 10%,** denotes significance at 5%, and ***denotes significance at 1%. All equations were estimated using cell sizes as weights, with standard errors corrected for clustering based on province and year of birth. All equations also include fixed effects for province of birth, year of birth, and census year.

Table 13. IV and WLS estimates of mortality rate models including *unideg*

	5-year mortality IV	10-year mortality WLS
unideg	-0.599***	-0.0884
	(-3.03)	(-0.85)
french	-0.0505***	-0.0246***
	(-4.68)	(-3.02)
male	0.0448***	0.0329***
	(4.96)	(5.75)
comp_ins	0.000000854	0.0215**
	(0.00)	(2.04)
hosp_ins	0.000907	0.0130*
	(0.20)	(1.87)
rural	-0.000541	0.00000247
	(-0.95)	(0.00)
Log provincial school expenditure_ps	-0.0200***	-0.0186
	(-2.60)	(-1.59)
teacher_ps	2.507***	1.342*
	(3.43)	(1.89)
constant	0.145**	0.0861
	(1.99)	(0.78)
N	1,920	1,440
R^2	0.270	0.349
F	115.4	71.77

NOTES: All estimates are for Subsample 2. Values in parentheses are t-statistics. *denotes significance at 10%,** denotes significance at 5%, and ***denotes significance at 1%. All equations were estimated using cell sizes as weights, with standard errors corrected for clustering based on province and year of birth. All equations also include fixed effects for province of birth, year of birth, and census year.

Data Appendix

A. Demographic Variables

A.1 Education Variables

Table A.1 presents the sources from which the three education variables *yrs_educ*, *hsdiploma*, and *unideg* are constructed. Special attention should be paid to the variable *yrs_educ*, to which I assign values according to the education levels in different censuses. For instance, for the 1971 census, I assign *yrs_educ* a value of 2.5 years if EDUCAT (level of schooling) equals 1. Then I assign *yrs_educ* a value of 6.5 if EDUCAT equals 2, and so on. For each cell, the proportions of individuals who graduate from high school and who have university degrees are calculated. High school graduates include all individuals with a high school diploma or higher level of educational attainment. Those with a university degree include all individuals with at least a Bachelor's degree.

Table A.1 Education Variables Retrieved from Census PUMFs

Census Year	*yrs_educ*	*hsdiploma*	*unideg*
1971	EDUCAT	EDUCAT	EDUCAT
1981	HGRAD; PSUV	DGREE	HLOS
1986	HGRAD; PSUV	DGREE	HLOS
1991	HGRADP; PSUVP	DGREE	HLOSP
1996	HGRADP; PSUVP	DGREE	HLOSP
2001	HGRADP; PSUVP	DGREE	HLOSP

Note:
EDUCAT: Level of schooling; PSUV, PSUVP: years of university; HGRAD, HGRADP: highest level of elementary/secondary school; HLOS, HLOSP: highest level of schooling; DGREE, DGREEP: highest degree/certificate/diploma

A.2 Other Demographic Variables

Table A.2 presents the sources from which the variables *birthyear*, *french*, *male* and *prov* are constructed.

Table A.2 Other Demographic Variables Retrieved from Census PUMFs

Census Year	*birthyear*	*french*	*sex*	*prov*
1971	AGE	USMOTHTG	SEX	PLCBIRTH
1981	BRTHYR	MOTHERTG	SEX	BIRTHPLA
1986	AGEP	MOTHERTG	SEXP	BIRTHPLA
1991	AGEP	MTNP	SEXP	POBP
1996	AGEP	MTNP	SEXP	POBP
2001	AGEP	MTNP	SEXP	POBP
Note: AGE, AGEP: age; USMOTHTG, MOTHERTG, MTNP: mother tongue; SEX, SEXP: male; PLCBIRTH, BIRTHPLA, POBP: place of birth				

Each cell includes individuals who share the same year of birth, province of birth and gender. The cell size is determined by counting the number of nonzero values of the AGE or AGEP variable in each cell. Since there are no missing values for these variables, readers should be assured that the cell sizes are computed correctly.

For each cell the proportion of individual whose mother tongues is French is calculated to generate the variable *french.* The variables *birthyear, sex,* and *prov* are used to identify the cells.

B. Provincial Control Variables

The variable *rural* is constructed using tables published with the 2001 Census, in (*Statistics Canada* 2011). Note that "the rural population for 1981 to 2011 refers to persons living outside centres with a population of 1,000 AND outside areas with 400 persons per square kilometre. Previous to 1981, the definitions differed slightly but consistently referred to populations outside centres of 1,000 population." (*Statistics Canada* 2011) The data were collected every ten years between 1921 and 1951 and every five years between 1951 and 1971. The missing values were imputed by linear interpolation.

For the variables *operexpen_ps* and *teacher_ps,* the aggregates come from the *Historical Statistics of Canada (Section W: Education).* The per-student values are obtained by dividing the aggregates by the number of students in public schools. The values of operating expenditures of public school boards on elementary and secondary education come from table W275-300a. The data on full-time teachers in public elementary and secondary schools come from table W192-247. The number of students in public schools is obtained from table W67-93. The Consumer Price Index (CPI), obtained from CANSIM Table 326-0021, was used to convert total operating expenditures by province and year into constant dollars (2002 dollars). All values exist for most years (the data were collected every five years from 1920 to 1955) and are imputed for missing years by linear interpolation.