

**The Compelling Effects of Compulsory Schooling:  
Evidence from Canada**

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**Abstract:** Compulsory school laws have existed for more than a hundred years, and policies to mandate further education continue to be discussed. The implications of these laws, however, are not well understood. Historical changes to compulsory schooling in Canada permit an examination of their effects on would-be-dropouts under very different circumstances than those during changes in other countries. Mandating education substantially increased adult income and substantially decreased the likelihood of being below the poverty line, unemployed, and in a manual occupation. These findings suggest significant gains from this legislation, which seem unlikely offset by the costs incurred while having to remain in school.

Key Words: compulsory school laws, returns to education

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## **I. Introduction**

Compulsory school laws mandate the minimum length of time children and youths must spend in school before having the option to leave. The motivation for introducing these laws, or for updating them, often relates to assumptions that children wishing to leave school early are better off from staying on, or that society benefits collectively from raising a country's overall education attainment because doing so promotes good citizenship and economic development. Archibald Macallum, an Ontario teacher, summarized these arguments vigorously in a report favoring the introduction of compulsory schooling in Canada:

“Society has suffered so cruelly from ignorance, that its riddance is a matter of necessity, and by the universal diffusion of knowledge alone can ignorance and crime be banished from our midst; in no other way can the best interests of society be conserved and improved than by this one remedy – the compulsory enforcement of this great boon – the right of every Canadian child to receive that education that will make him a good, loyal subject, prepared to serve his country in the various social functions which he may be called on to fill during his life; and prepare him, through grace, for the life to come” [Annual Report of the Ontario Teachers' Association, 1875, as cited in Prentice and Houston, 1975, pp. 175-176].

Similar reasons are given for continuing to enforce compulsory schooling, or for updating these laws. Many states, provinces, and countries initially mandated a school

leaving age of 12, but most school jurisdictions presently enforce a school leaving age of 16, and several governments have implemented or are discussing raising the school leaving age to 18. In North Carolina, for example, the State Superintendent wants to raise the legal school attendance age to 18 because this age, “better reflects the maturity required to make such an important and life-changing decision”. Thomas Upson, a Connecticut Senator expressed the argument this way: “Too many people regret the reckless decisions they made when they were 16 and decided to drop out. Our goal is to try to prevent that”.

Surprisingly little research discusses the welfare implications of compulsory schooling, even though these policies have existed for more than a hundred years. Most arguments in favour of compulsory schooling, including those cited above, seem to rely more on intuition than on theoretical or empirical evidence. The most common model of school choice in social science, however, suggests compulsory schooling should lower lifetime welfare [e.g. Becker, 1965, Chiswick, 1967]. The human capital model of school choice views education as an investment. Students invest time, forgo earnings, and endure possible psychological costs from attending school, but only if the anticipated benefits from doing so are large enough to offset these costs. If not, a student’s optimal decision should be to leave. Compulsory schooling prevents that option. If students already choose optimally when to leave, then imposing a longer stay in school makes them worse off, on average, than before. Only in the presence of positive externalities, or sub-optimal school attainment can compulsory attendance legislation improve lifetime welfare.<sup>1</sup>

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<sup>1</sup> See Eckstein and Zilcha (1994) and Oreopoulos (2003).

Several previous studies use historical changes to compulsory school laws as natural experiments to identify causal estimates of the returns to education. Although these studies generally do not discuss the welfare and policy implications of their results, all point to significant individual and social gains from compulsion. Angrist and Krueger (1991) use differences in the timing of individual births to identify youths that entered school earlier because they were born just prior to the entry-date cut-off as opposed to just after. They find youths that left school with one extra year of school because of these entry laws experienced an increase in annual earnings of 10.1 percent, on average for U.S. males. The gain is likely incurred directly, since this experiment compares relative earnings differences for two cohorts that differ by birth date only. Externalities from increasing the education group of one group would likely affect the other. Acemoglu and Angrist (2002) find more direct evidence that school compulsion raises earnings to would-be-dropouts directly by instrumenting education attainment with school entry laws that affect would-be-dropouts directly and state compulsory laws that may have direct and indirect effects from externalities. They conclude individual returns to compulsory schooling are 8 percent or more, while additional social returns are negligible.

Other papers find evidence of social returns, but for non-pecuniary outcomes. Lochner and Moretti (2001), for example, find school compulsion lowers crime. Lleras-Muney (2002) finds school compulsion improves health. Dee (2003) and Milligan et al. (2003) estimate that tighter restrictions to school-leaving laws in the U.S. and UK correspond to increases in civic activities (like voting and discussing politics). And Oreopoulos et al. (2003) find that children of mothers and fathers compelled to take

additional schooling are less likely to fail a grade and less likely to drop out of high school.

One concern with this empirical literature is that the effects of school compulsion on overall grade attainment are small, and so the instrumental variable estimates on the returns to compulsory schooling are weak and potentially biased. Bound, Jaeger, and Baker (1995) show that if education attainment and compulsory school laws are only weakly correlated, estimates can be biased in the same direction OLS results are biased. Many of the issues raised by these authors have been addressed by Staiger and Stock (1997) and Cruz and Moreira (2002), who conclude that accounting for the possibility that these instruments are weak does not generally change the conclusion that the effects of compulsory schooling on earnings are substantial and significant. A second concern involves the potential that the compulsory school laws are still correlated with other factors related to earnings and other outcome variables. Angrist and Krueger (1991) try to address the possibility that school entry laws are related to seasonal birth patterns that are independently correlated with adult earnings. Lleras-Muney (2002) suggests changes to compulsory school laws were more for political reasons than driven by region-specific economic or social trends.

My aim in this paper is to provide further evidence of the effects of compulsory schooling, and to present this evidence in a framework for discussing the potential policy implications of these laws rather than in the framework of the returns to schooling literature. Historical changes in Canada provide an opportunity to examine whether school leaving and entry laws affect would-be-dropouts under different circumstances than those in the U.S. studies. I find students compelled to take an extra grade of school

experienced an average increase of 12.3 percent in annual income. I also find students with additional schooling are more likely to speak two languages, work, and less likely to be below the Low Income Cut-off, unemployed, and in a manual occupation. In reduced form, the laws affect only birth cohorts directly, and only those individuals without post-secondary education. This suggests the laws had primarily a direct impact on economic outcomes and little impact on other cohorts. I arrive at similar estimates whether looking over different periods between 1920 and 1980, and whether looking over different regions. The results reinforce the earlier studies and suggest compulsory schooling served as an effective policy tool for improving many long-run social-economic outcomes among early school leavers, at least on average.

Next section provides a brief history of compulsory schooling in Canada. Section III describes the data sources used in the paper. Section IV shows the methodology for identifying the effects of compulsory schooling. The estimates for the effects of compulsion on grade attainment, adult earnings, and other labor market outcomes are displayed in Section V. Section VI carries out a simple cost-benefit exercise from the perspective of a social planner, to evaluate whether these policies had overall beneficial effects. The last section concludes.

## **II. A Brief History of Compulsory Schooling in Canada**

The prime motivation for compulsory schooling followed soon after the introduction of the public education system in Canada, at the end of the 19th Century. Even as schooling became more available, school attendance was often sporadic and

infrequent. At the end of the nineteenth century, 70 percent of the Canadian population still lived in rural areas. The needs of the farm often dictated the frequency and timing of school attendance. Teachers and administrators expressed concern over the irregular turnout. Some families sent one child to school for a few weeks and then another [Phillips, 1957]. In the winter, when children were least needed at home, attendance remained low because leisure was often preferred over sitting still in cramped and cold schoolhouses. The average daily attendance rate (among those enrolled) for the whole of Canada was 61 percent in 1900. The number of years typically spent in school were also few. Both boys and girls often left by ages nine or ten to begin work in factories or at home.

Teachers, inspectors, and politicians were among the first to argue for school compulsion. One argument was that weak attendance disrupted the class and prevented any meaningful learning from occurring. For example, a local superintendent in Ontario in 1971 claimed, “irregularity of attendance is the bane and curse of the public schools; it is a log and chain upon the progress of instruction for it blasts and withers the noblest purposes of the best of teachers’ [Axelrod, 1997, p 51]. Another argument was that students were being kept home for trifling reasons, or that poor families resisted sending their children to school, when doing so would help them avoid poverty as adults. Authorities felt the reasons for introducing public education were being compromised by poor attendance.

Figure 3 summarizes the compulsory school laws in Canada over the last hundred years. The figure highlights the minimum school leaving ages of each province, and the ages which children must begin school. The underlying details of these laws are

complex, and exceptions were made. Ontario, for example, took the first step of introducing compulsory school laws in 1871. Parents were obliged by threat of fine to have children attend school for at least four months a year between the ages of seven and twelve. Legislation in 1891 raised the limits to eight and fourteen and required a child to remain at school, even after reaching fourteen, until the end of term. The new law was more definite in stipulating penalties for parents who refused to comply and for employers who hired children who should have been at school. But, as with many of the new compulsory school laws first introduced by the other provinces, many exceptions were allowed, and authorities failed to enforce, especially in rural areas. Aside from sickness, the main exception involved living more than 2 miles away from school for a child under age 10, and 3 miles away if over age 10 unless transportation was provided. The Adolescent School Attendance Act increased the age of compulsory attendance in 1921 to sixteen in urban areas, but any adolescent was exempted with a home permit or an employment certificate signed by a parent. Employed fourteen and fifteen year-olds were required to attend part-time evening classes, but only in municipalities that provided such instruction. In 1954, the Schools Administration Act imposed a minimum school leaving age of 16 for all children in Ontario, but allowed adolescents over fourteen to work on a family farm, at home, or elsewhere if doing so was necessary for subsistence. By 1970, these exemptions were removed.<sup>2</sup>

In addition to compulsory schooling, many provinces also introduced child labour legislation near the beginning of the twentieth century. Employment certificates, for example, could exempt children from the minimum school leaving age law. Some certificates were obtained by passing a grade seven or eight equivalence test. Others

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<sup>2</sup> Details of the other provincial laws are provided by Oreopoulos (2002).



required only evidence of reading and writing skills. For certain occupations, employment certificates were required for children over the minimum school leaving age, mainly mining. Another type of labour law allowed children to work below the minimum school leaving age if doing so was necessary for subsistence. By 1933, all provinces had removed these exemptions.

In certain provinces employers were also not allowed to hire children during school hours. This condition was usually incorporated into compulsory school legislation. For virtually every case, the minimum working age restriction (during school hours) corresponded with the minimum school-leaving age restriction. The purpose of restricting employment below the minimum school leaving age was to place liability on employers, in addition to the liability already imposed on parents and guardians through compulsory schooling. Most provinces implemented these laws the same time as other amendments, designed to strengthen compulsory school laws, were being made.

In short, child labour laws in Canada either reinforced existing compulsory school legislation or provided opportunities to avoid it. They were designed to work together. Children were allowed to work earlier than the minimum school leaving age if they had already obtained a satisfactory minimum level of education, or if they were from poor households. But many school authorities believed these exemptions provided unnecessary loopholes used to avoid the school leaving age law without reasonable cause. By 1942, all provinces except Saskatchewan removed these exemptions. Saskatchewan abandoned its Grade 8 exemption in 1974.

### **III. Data Description and Sources**

Data on compulsory schooling and child labour laws were compiled directly from provincial Statutes and Revised Statutes containing all original Acts of legislation and amendments since inception. Additional sources helped verify the compilation. In particular, the Dominion Bureau of Statistics (later renamed Statistics Canada) published a near annual survey of education beginning in 1921 detailing revisions to Education Acts and summarizing compulsory school legislation (and occasionally child labour laws) for each province. The Appendix lists the Acts, the education surveys, and all additional sources used to create the variables in this paper.

I created four variables to summarize compulsory school attendance rules, coded by year of legislation for each province from 1900 to 2000:

1. Maximum age of compulsory schooling in towns and cities (town dropout age)
2. Maximum age of compulsory schooling in rural areas (rural dropout age)
3. Minimum age of compulsory schooling (entry age)
4. Indicator for whether employment certificates provided exemptions to the dropout age

The town and rural dropout age rarely differed, and so I used the town dropout age only. In addition, I constructed a variable for the number of years in school a child would have to attend if she entered school at the age that compulsory attendance laws required and left immediately after reaching the minimum school leaving age (minimum

number of years in school). Acemoglu and Angrist (2002) and Lleras-Muney (2001) use a similar variable for their United States analyses.

To explore the effects of compulsory schooling on education attainment, and on subsequent social-economic outcomes, the schooling law variables were matched to census data, from the 33 percent sample of the 1971 Census, and the 20 percent samples from the 1981, 1986, 1991, 1996, and 2001 Census files.<sup>3</sup> The Censuses provided information on gender, province-of-residence, province-of-birth, age, labour market status, income and wages, and education attainment. I kept all individuals born in a Canadian province that were 14 years of age between 1920 and 1970. The sample aged 14 before 1920 are older than 65 in the 1971 Census, and compulsory schooling laws were sometimes weakly enforced during that period compared to afterwards. School-leaving age changes after 1980 may have affected high school dropouts differently compared to changes that occurred during the 1920 to 1970 period, when education attainment and enrolment rose substantially. For these reasons, I limited my analysis of the effects of compulsory schooling over this fifty-year period. During this period, all provinces experienced rapid increases in average education attainment. The remarkable increases by province and year aged 14 are shown in Figure 2. Note that, together, the data displayed in Figures 1 and 2 can generate similar estimates of the effects from the compulsory school on average grade attainment, even though discontinuities at the years when the laws change do not appear visible. We should not expect large discontinuities, because the education attainment variable used here (grade attainment) does not

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<sup>3</sup> These files are available internally at Statistics Canada. I found no difference in the results whether working with these files or the Public Use Data Files. The STATA program codes for working with these files are available on request. The 1976 public use census sample excludes earnings information and was omitted.

correspond precisely with the law changes (age requirements). Raising the school leaving age may increase the length of time spent in school, but may not raise education attainment for those retained a grade.

The Census recorded highest grade attained, highest degree completed, and an indicator for high school certificate attainment. I focused on the grade attainment variable, since the school laws before 1970 did not constrain children to finish high school. A child that entered Grade 1 at age 6 and advanced one grade per year reaches Grade 11 at age 16. I examined whether compulsory schooling affected high school completion or education levels beyond high school with the other variables.<sup>4</sup>

Different provincial grade requirements for high school graduation complicated the use of grade attainment variables to examine the effects of compulsory schooling on earnings. By 1960, for example, Nova Scotia, Quebec, and Newfoundland allowed graduation after Grade 11. Newfoundland did not offer schooling beyond this grade until 1984.<sup>5</sup> The other provinces allowed graduation after Grade 12. Ontario and British Columbia offered a senior matriculation year with Grade 13 that generally reduced by one year the time required for a university degree. Almost 50 percent of 14 year olds in 1960 attended Grade 13. Compelling children to attain Grade 11 likely had different implications depending on the province they were in.

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<sup>4</sup> Children compelled to take additional years of high school may also be more likely to graduate since the number of years until completion after gaining the option to drop out declines. If the perceived costs of completing the remaining years after attaining an extra grade are considered smaller, students that would have dropped out under less restrictive compulsory school laws may choose to continue beyond the maximum age of compulsion. Compulsory legislation might also affect children wanting to leave after the school leaving age. Lang and Kropp (1986) suggest that individuals intending to complete one or two additional years of schooling after the minimum leaving age requirement may acquire additional education after legislation raises the limit to avoid signalling to employers they are in the same category as those who leave school at soon as possible.

<sup>5</sup> See Sweetman (2001) for a discussion on the effects from increasing the high school curriculum in Newfoundland by one year before graduation.

The explore whether differences in high school graduation dates, or other unrelated factors affecting upper levels of education, affect the results, I also estimated the regressions for only those attaining grade 11 or less. If compulsory schooling had a minimal impact on those attaining grades beyond 11, this sample omits a large population of students unaffected by the laws, but whose outcomes may by have differed by province for other reasons. We should not expect the estimated effects of compulsory schooling on adult labour market outcomes to differ if the laws impact only high school dropouts or early school leavers. Section VI shows this hypothesis generally holds.

I matched individuals to the compulsory school laws that were in place in their province of birth when they 14 years old, except the school entry laws which were matched according to when individuals were 6 years old.<sup>6</sup> Acemoglu and Angrist (2002), Lleras-Muney (2001), Schmidt (1996), and Goldin and Katz (2003) followed the same procedure for the United States. Schmidt (1996) examined this assumption and found the effect compulsory school laws in the United States were largest when matching to individuals at this age.

Finally, I also matched individuals with characteristics of their province-of-birth when they were 14 years old: number of schools per student, number of teachers per student, total per capita education expenditure, fraction in province in a rural area, and fraction of workers in province working in the manufacturing sector. The appendix describes in more detail how these variables were compiled.

The combined dataset contained 9,393,327 Canadian born individuals aged 20 to 64 who were 14 years old between 1920 and 1970.

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<sup>6</sup> Individuals that moved provinces before reaching high school were mismatched. Children did not likely move provinces because of a change in compulsory school legislation, so such laws changes are unlikely correlated with error.

#### IV. Methodology for estimating the effects of compulsory school on education and adult outcomes

I first generate cell means for all variables by census year, birth cohort, and province. This improves the manageability of the dataset, and avoids heteroskedasticity at the individual level. I estimate the following equation:

$$(1) \quad EDUC_{pcy} = \gamma CL_{pc} + \beta X_{pc} + e_p + e_c + e_y + e_{pcy},$$

where  $EDUC_{pcy}$  is the average education attainment level for the group born in province  $p$ , from birth cohort  $c$ , from census year  $y$ ,  $CL_{pc}$  is a vector of provincial compulsory schooling laws and child labour laws,  $X_{pc}$  is a vector of provincial controls,  $e_p$ ,  $e_c$ , and  $e_y$  are fixed effects for province, birth cohort, and census year respectively, and  $e_{pcy}$  is the error term. The errors are clustered by province and birth cohort.

Identifying the effects from the laws comes from difference in the timing of the changes in these laws across provinces. The analysis is therefore similar to difference-in-difference estimation, but with more than one intervention and more than one ‘treatment group’. The methodology does not identify the true impact of these laws on enrolment if other provincial factors that influence enrolment change coincidentally at the same time as the laws themselves. Several specification checks are carried out to examine this possibility.

I estimate the returns to compulsory schooling on earnings and other social economic outcomes by substituting actual education attainment with predicted education attainment from compulsory schooling differences across province and time. The baseline instrumental variables equation is:

$$(3) \quad Y_{pcgy} = \overline{\overline{\delta EDUC}}_{pcy} + \beta X_{pc} + u_p + u_c + u_y + u_{pcy},$$

where  $Y_{pcy}$  is the average outcome for the group born in province  $p$ , from birth cohort  $c$ , from census year  $y$ , and  $\overline{\overline{EDUC}}_{pcy}$  is predicted education for the group after estimating equation (1).

## V. Results

### A. The Impact of Compulsory Schooling on School Enrolment and Education Attainment

Table 1 presents the predicted effects of the compulsory school variables on grade attainment from estimating equation (1), weighted by cell mean sample size. The estimates use the full sample of Canadian born individuals who were 14 years old between 1920 and 1970, and who were between 20 and 65 years old when they completed their survey. Column 1 shows the coefficients for the effects from facing different school-leaving ages. The omitted dropout age is age 12 or less (or no dropout age). Average grade attainment is .24 points higher with a school-leaving age of 14 compared to a lower or no school-leaving age, after controlling for average provincial,

birth cohort, and census year fixed effects. Raising the minimum school leaving age to 14, compared to any lower limit is associated with an percentage point increase to a province's school enrolment rate. Raising the dropout age to 15 versus 14 exhibits an even larger increase in average grade attainment (.63 points). However, the effect from increasing the limit again to age 16 is negative, compared to provinces with a school leaving age of 15. The lower estimate occurs consistently with other specifications. As shown in the next tables, this finding may, in part, be due to enrolment patterns in Quebec after 1950.

The table shows a negative association between grade attainment and the exemptions indicator. Being exempted from the school leaving age by means of work-permit or proof of necessity lowers average grade attainment by .27 points. This estimate stays about the same when including other laws or provincial controls in the regression. Lower entry ages are also associated with higher grade attainment. Average grade attainment for adults that faced a school entry age of 7, versus no entry age or an entry age of 8, are .32 grades higher. However, students from provinces with the entry age to 6 experience a .13 reduction in average grade attainment.

Taken overall, the relative effects of the compulsory schooling variables are the same when restricting the sample to only those with less than Grade 12 attainment. Adding the control variables does not alter the coefficient estimates on the effects of the laws by much. Provincial school expenditures matched to students when aged 14 are strongly associated with grade attainment (while including province and birth cohort fixed effects). Fraction in province in rural areas is negatively correlated with grade



attainment and fraction of workers in province in the manufacturing sector is positively correlated.

The summary variable for compulsory schooling identifies a strong link between grade attainment and the minimum number of years required to remain in school before being allowed the option to leave. A one year increase in the number of mandatory school years is associated with .19 increase in grade attainment, on average. This estimate falls to .13 after adding the 5 provincial school control variables.

This estimate is similar to that found by Lleras-Muney (2002) for the United States. She found a one year increase in mandatory number of years in school, from the school entry age to the earliest age allowed to obtain a work permit, increased the number of years of schooling by 0.051 years. This estimate is notably smaller than the effect from a one year increase to mandatory years of schooling in Canada. The measures, however, span different years, and the U.S. estimate includes an additional control for whether a student must attend continuation school after working.<sup>7</sup>

Average grade attainment increased from 8.1 in 1920 to 11.3 in 1970. The mean years of mandatory schooling rose by 3.8 years, from 5.8 to 9.6. Table 1 implies that the changes in provincial compulsory school laws can explain an increase in grade attainment of .50 years. Thus, about 15.6 percent ( $.5 * 3.8 / 3.2$ ) of the increase in grade attainment over the fifty year period from 1920 to 1970 can be accounted for by changes in compulsory school legislation. Goldin and Katz (2003) find somewhat lower impact of compulsory school changes on grade attainment in the United States from 1910 to 1939.

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<sup>7</sup> The U.S. regressions also include a number of state-year control variables, such as per capital expenditures and demographic status. The forthcoming revision will add province-year controls.

They estimate compulsory schooling and child labor laws explain about 5 percent of the year increase in U.S. average schooling from the 1896 to 1925 birth cohorts.

Table 2 compares the estimates of the effects from the compulsory school law variables on different levels of education using the full sample. One specification check for whether other factors underlie the relationship between these laws and the outcome variables is to examine if changes in the laws are associated with changes in higher levels of education attainment. We should not expect the laws to substantially affect education attainment beyond high school. We should also not expect early dropout ages to impact on later grades. Table 2 shows the predicted effects of compulsory school law variables on different levels of education attainment generally hold. Raising the dropout age to 14 does not significantly affect higher levels of education. The point estimates are precisely estimates at zero for Grade 12 attainment or higher, but the standard errors for the estimates of the effect on lower levels of education are high. Allowing work permits to exempt students from staying in school lowers education attainment at all levels of high school, but does not affect schooling beyond high school. Finally, school entry age differences generally impact Grade 7, Grade 8, and Grade 9 attainment. The lower row summarizes the effects of compulsory schooling on different levels of education attainment with the mandatory number of school years variable. The coefficient on this variable is largest for Grade 7 and 8 attainment, and then declines. The variables has no association with high school completion or with post-secondary education. The results strongly corroborate the predicted zero effect for compulsory schooling on education attainment beyond high school.

Since the compulsory school laws do not appear to affect higher levels of education attainment, we may prefer to carry out the analysis on the sample of early high school leavers. Table 3 shows the first stage results for both the full Census sample, and the sample of individuals that attained Grade 11 or less. The Census asks both an individual's highest grade attainment and highest degree, so while most of the second sample did not complete high school, that does not preclude individuals in it from having obtained community college or bachelor degrees. The differences in coefficients for the estimated effects of the school laws are small. The effect of facing a dropout age of 14 versus something less is smaller for the dropout sample, and the effect for facing a school entry age of 6 is higher.

Since these estimates are weighted by cell mean sample sizes, they place far more emphasis on what happened in Quebec and Ontario, where more than 50 percent of Canada's population resides, than for what happened in the other provinces. The cell mean sample sizes are large enough for all provinces to consider placing equal weight on all provinces (and all birth cohorts). The second set of columns in Table 3 excludes the cell weights, thus treating law changes that happened in smaller provinces equal with changes that happened in larger ones. Removing emphasis on Quebec and Ontario leads to a larger coefficient on the school leaving age variables, a smaller coefficient on the exemptions variable, and a larger coefficient on the school entry age variables. But generally, whether looking at the dropout sample, or the non-weighted sample, the results indicate changes to compulsory school laws in Canada had a significant impact on average grade attainment.<sup>8</sup>

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<sup>8</sup> The estimates are very similar when separating males and females (results not shown, but available on request).

## B. The Returns to Compulsory Schooling on Earnings and other Outcomes

The subsection above examines how changes to compulsory schooling legislation, from 1920 to 1970, impacted average education attainment. Here we investigate what happened to those who were affected by these changes, in terms of their earnings, income, and other outcomes measured in the Census. Table 4 shows returns to schooling estimates from instrumenting grade attainment with predicted grade attainment. The Table compares these results with least-squares estimates that use the full sample of adults aged 20 to 64 who were aged 14 between 1920 and 1970. From Column 3, children compelled to take another grade of school end up with about 12.3 percent higher annual income, on average, than children not restricted to stay in school longer. This estimate is lower compared to the one in Column 4 that restricts the sample to only high school dropouts. The estimated returns to compulsory schooling are about the same when placing equal weights across the provinces in the last two columns. The effect of the school compulsion on earnings is lower than that for total income. One extra grade raises annual earnings by about 7.1 percent. The results also indicate significant increases in earnings and income for males, and for combined household income. Placing equal weight on the compulsory school law effects for all provinces, for example, compelling boys to take one additional grade raises their average annual earnings by 7.0 percent.

The instrumental variables estimates are about the same, or higher than the least-squares estimates shown in Columns 1 and 2. This finding is not uncommon, compared

to previous studies, despite the likelihood that OLS estimates for the returns to schooling are downward biased. Several recent papers attempt to explain why the IV approach may arrive at higher returns to schooling estimates than OLS. One explanation is that IV estimates, if specified correctly, are without omitted variables bias, and without measurement error bias. Mis-measured independent variables may attenuate estimates and bias results downwards. A second explanation is that the IV estimates measure the returns to schooling for only those affected by compulsory school law changes. Clearly, characteristics underlying the types of individuals influenced by these laws differ from the average characteristics of those in the entire sample.

Table 5 shows the IV estimates with the full sample and dropouts sample using different time periods. The table shows the returns to compulsory schooling estimates using smaller, thirty-year, periods. All regressions find positive and strongly significant estimates, similar to those using the full sample ones. For example, the returns to compulsory schooling estimates range from about 8.7 percent to 13.5 percent for any thirty-year period beginning in 1920 until 1980, with the sample of early high school leavers. The table indicates the estimated effects are not driven by any particular law change, or any particular time period.

We do not have to restrict the analysis of outcome variables to just earnings and income. Looking at other social-economic outcomes provides more evidence (for or against) the effect of compulsory schooling, and whether individuals may benefit in non-pecuniary ways. Table 6 displays the returns to compulsory schooling on other outcome variables. The estimates imply school compulsion lowers the likelihood of reporting being not working while looking for work, and raises the likelihood of reporting working

fulltime. The IV results also predict additional schooling lowers the probability of working in a manual occupation, and raises the likelihood of working in clerical service. Those compelled to take an extra grade are 7.9 percentage points less likely to fall below Statistics Canada's Low Income Cut-off, and more likely to report being fluent in both English and French. Finding that occupational composition changes and self-reported language skills improve from one more grade of compulsory schooling seems to indicate real skills are acquired during the extra years of school.

#### **IV. A Social Planner's Perspective on the Effects of Compulsory Schooling**

There are many potential costs and benefits associated with raising mandatory school requirements, some of which cannot easily be measured. There is only one benefit we can extract from the above results, which is the financial gain for cohorts directly affected by changes in compulsory schooling. To get some estimate of the financial lifetime gain, Figure 3 shows predicted income profiles for males that left school at age 15 or age 16 from regressing equation (2) with an added quartic in age. The specification that the return from compulsory schooling is constant, estimated at 11.8 percent, is clearly an assumption. Allowing the return to differ by age does not alter this discussion.<sup>9</sup> The profiles take the usual shape, increasing at early ages and peak at age 47. The amounts are measured in 2001 Canadian dollars.

Table 7 converts the annual differences between these profiles into present value amounts using alternative discount rates. I wish to consider the earnings gains from the perspective of a social planner. The appropriate discount rate is the risk-free rate,

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<sup>9</sup> For an example, see Oreopoulos (2003).

although I also show the present value conversion using higher rates. Table 7 also considers lower annual returns to compulsory schooling using rates of 10 and 8 percent. With a 3 percent discount rate, the estimated present value increase in income from compelling a would-be-dropout to continue an extra grade is about \$44,450. That amount is more than 6 times the first year earnings of the student that leaves at age 15, and more than 2 times her maximum annual salary. Using a 5 percent discount rate and an 8 percent return, the predicted present value difference in earnings is still greater than a dropout's peak annual earnings.

The costs from attaining an extra grade may extend beyond forgone earnings and school expenditure costs. Students may incur psychological costs, or additional liquidity constraint costs from being obliged to attend more school. These costs cannot easily be measured. A social planner would have to assess whether the costs from school compulsion offset the present value subsequent gains. While it is impossible to tell from this data, I provide, in another paper, some evidence that liquidity constraint costs and psychological costs from attending school do not appear significantly large [Oreopoulos, 2003]. Furthermore, any non-pecuniary gains from compulsory schooling, such as utility gains from being less likely to be unemployed, or more likely to be in a more enjoyable job, would also have to be included when comparing direct costs and benefits.

Another consideration to a social planner is whether compulsory schooling induces positive or negative externalities for others not directly affected. If the net gains for dropouts affected by school compulsion is positive, we need only determine whether the net gains for others is non-negative to favour a compulsory school law policy. Negative externalities might arise, for example, if increasing the supply of high school

graduates with compulsion, relative to high school dropouts, leads to lower wages for graduates. In an attempt to test this, Table 8 shows the results from regressing log income on the mandatory number of school years faced when aged 14, and the number of mandatory school years faced by other birth cohorts. The regression includes birth cohort and province fixed effects, as well as the provincial controls. This reduced form equation shows the overall (income) effect of raising school attendance on the cohort directly influenced, and the effect from raising school attendance on earlier and later cohorts. If school compulsion raises wages for those directly affected at the expense of other cohorts, we should expect to see the coefficient on mandatory number of school years faced to be positive, while that for other cohorts to be negative. On the other hand, the presence of positive externalities (on adult income) may lead to the coefficient for the laws faced by other cohorts to be positive.

Table 8 indicates the school laws faced by individuals when young impact income directly, but the laws faced by other cohorts have no significant effect. Increasing a cohort's school leaving age or reducing their school entry age year raises adult income, on average, by about 1.4 percent. But raising the school leaving age for cohorts born a few years earlier or later does not affect average income. This result occurs whether analyzing the full sample, or just the sample that never attained Grades 12 or 13. Whether looking at cohorts born a few years later or earlier, the coefficient on their mandatory number of school years is consistently around zero. These results suggest compulsory schooling has no additional negative or positive affect on income for cohorts not directly affected.



## VII. Conclusion

Education levels rose dramatically in Canada between 1920 and 1970. The national grade attainment average increased from 8.1 to 11.3. Provinces also implemented or tightened many compulsory school limits. I estimate changes in these limits had a significant impact on the rise in grade attainment, accounting for about 13 percent of the rise. Those affected experienced about a 14 percent increase in their annual income. Compelling would-be-dropouts to take additional education also lowered the chances for unemployment, decreased the chances of working in a manual occupation, and raised the fraction reporting speaking both English and French. I find no evidence that the law changes affected income of other cohorts indirectly.

The results presented here reinforce remarkably similar estimates of the benefits from compulsory schooling found in the United States and United Kingdom. Taken together, they suggest compelling early school leavers to stay on longer generated real gains. Whether these gains were offset by the direct costs of attending school another year is subject to debate. The costs would have to exceed at least 2 times a dropout's maximum annual income for a social planner to have preferred not imposing the law changes.

Compulsory attendance laws have existed for more than a hundred years, and policies to mandate further education continue to be discussed. The implications of raising the minimum school leaving age further, however, are not well understood. Evidence presented here suggests these types of policies, designed to directly benefit would-be-dropouts, were historically effective.

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## **Data Appendix**

### A. Constructing the Compulsory School Law Variables

The three compulsory school law variables used in this paper summarize a complex set of policies. For most of the provinces, identifying the minimum school leaving age since 1900 was straightforward from accessing historical provincial legislation and the Dominion Bureau of Statistics' annual survey of education. On occasion in the first half of the century, some provinces had two school leaving ages: one for towns and one for rural areas. There were not enough of these cases to identify accurately separate effects. In these cases, the town school leaving age was used.

Some provinces exempted students from attending school. The exemption requirements varied widely from province to province and year to year. The exemptions indicator variable equals one if: home permits or work certificates were allowed in any form; parents could claim their children had to work for reasons of subsistence; or mandatory attendance was for 6 months or less in a year. The variable equals zero otherwise. In 1920, 36 percent of 14 year olds faced some type of exemption. By 1970, 14 percent of 14 year olds faced exemptions.

The mandatory number of school years variables was created as the difference between the school leaving age an individual faced when aged 14 and the school entry age faced when aged 6. If no dropout age existed, a dropout age of 10 was used, and if no school entry age existed, an entry age of 8 was used. If youths had to remain until the

end of the school year, even after attaining the school leaving age, before being allowed to leave, the mandatory number of school years was increased by .5.

## B. Constructing the Provincial Controls

The regressions in the paper include 5 controls for provincial economic and social trends: log education expenditure per student, number of schools per student, number of teachers per student, fraction in province in a rural area, and fraction of workers in province working in the manufacturing sector. Aggregates of the first three variables come from the Dominion Bureau of Statistics' Annual Surveys of Education. Values exist for most years between 1920 and 1970. The Consumer Price Index was used to convert total education expenditure by province and year into real terms (in 1991 dollars). All variables were smoothed using STATA's `ksm` function, with a bandwidth of .1. I imputed values for missing years by linear interpolation and divided the aggregate values by the population of 5 to 16 year olds by province and year. The population data comes from Statistics Canada's social-economic database, CANSIM.

The rural and manufacturing variables were created using tables published from the 1921, 1926, 1931, 1936, 1941, 1946, 1951, 1956, 1961, 1966, and 1971 Censuses. I divided the number of individuals in a rural area (for each province) by the total provincial population, and the total number of workers by the number of workers in the manufacturing sector. Missing values were imputed by linear interpolation.

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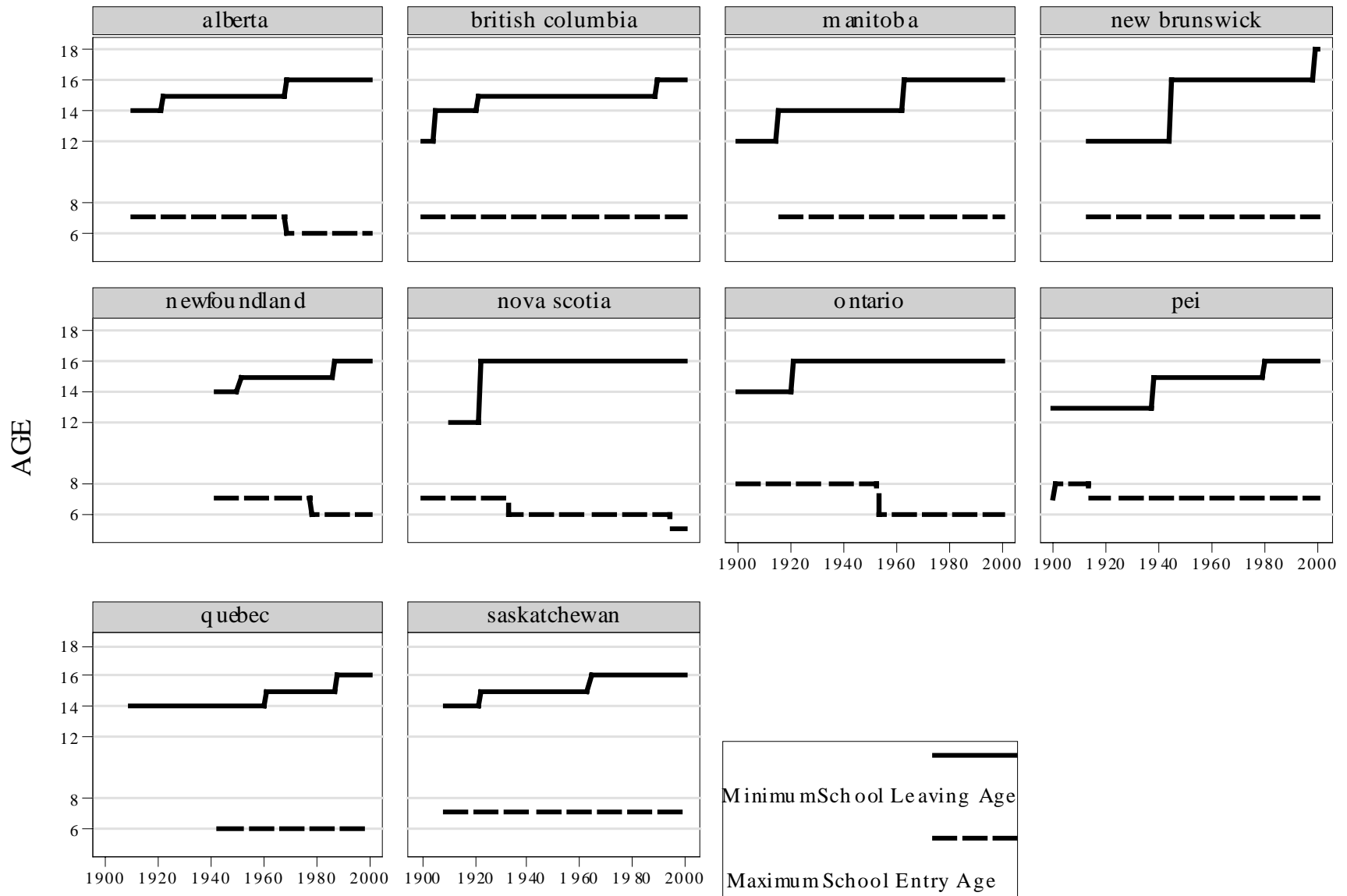
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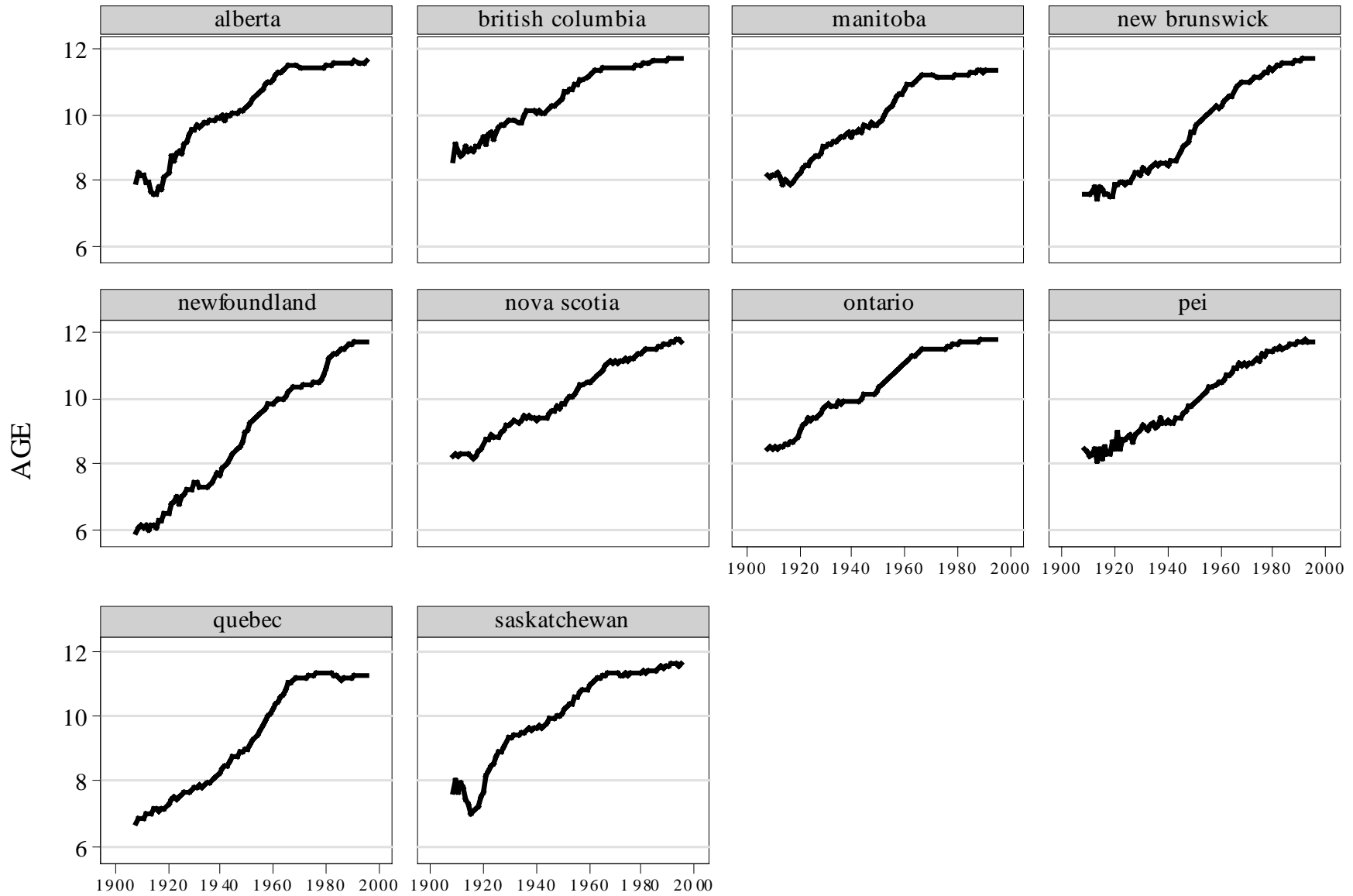
**Figure 1: Minimum School Leaving Ages and Maximum School Entry Ages by Province, 1900 – 2000**



Graphs by province

Notes:

**Figure 2: Average Grade Attainment by Year Turned 14 Years-Old and Province, 1900 – 2000**



Graphs by provname

**Table 1**  
**Effects of Compulsory Schooling and Child Labor Laws on Education Attainment**  
**Dependent Variable = Grade Attainment**

Full Sample: (Grade Attainment Mean = 10.27)								
School Leaving Age = 14	0.228 [0.0608]***	0.1926 [0.0644]***	0.1179 [0.0601]*	0.1001 [0.0603]*	0.1108 [0.0640]*	0.2047 [0.0645]***		
School Leaving Age = 15	0.8555 [0.0576]***	0.702 [0.0683]***	0.6142 [0.0644]***	0.4939 [0.0646]***	0.5181 [0.0699]***	0.6111 [0.0862]***		
School Leaving Age = 16	0.482 [0.0524]***	0.3033 [0.0664]***	0.3243 [0.0584]***	0.2105 [0.0682]***	0.2207 [0.0734]***	0.2878 [0.0787]***		
Exemptions to Leaving Age Allowed		-0.2668 [0.0295]***	-0.2518 [0.0274]***	-0.2717 [0.0280]***	-0.2616 [0.0291]***	-0.2583 [0.0328]***		
School Entry Age = 7			0.3191 [0.0512]***	0.3544 [0.0509]***	0.3272 [0.0548]***	0.3207 [0.0563]***		
School Entry Age = 6			0.1835 [0.0280]***	0.1117 [0.0286]***	0.1002 [0.0272]***	0.095 [0.0255]***		
Number of Mandatory School Years							0.1851 [0.0121]***	0.1324 [0.0127]***
Log Provincial School Expenditure				0.1823 [0.0318]***	0.1531 [0.0347]***	0.1774 [0.0331]***		0.2832 [0.0487]***
Fraction in Province in Rural Area					-0.0832 [0.1939]	-0.164 [0.1843]		-1.1994 [0.3302]***
Fraction in Province in Manufacturing					4.0468 [1.3602]***	4.0591 [1.3442]***		4.9888 [2.3131]**
Number of Schools in Province per Student						3.3149 [2.4471]		4.2732 [2.4987]*
Number of Teachers in Province per Student						-21.8088 [6.6833]***		-15.3073 [9.7826]
Number of Individuals								
Grouped Observations	2010	2010	2010	2010	2010	2010	2010	2010
R-squared	0.96	0.97	0.97	0.97	0.97	0.97	0.94	0.95

Notes: Regressions are on cell means from Census data, grouped by birth cohort, province, and census year. All regressions are weighted by cell sample size and include fixed effects for province, birth cohort, and census year. Huber-White standard errors are shown, clustered by province and birth cohort. One, two, and three asterix indicate coefficient is significantly different from zero at a 10 percent, 5 percent, and 1 percent confidence level. See text for details.

**Table 2**  
**Effects of Compulsory Schooling and Child Labor Laws on Various Education Attainment Levels**

	<u>Gr. 7 Attained</u>	<u>Gr. 8 Attained</u>	<u>Gr. 9 Attained</u>	<u>Gr. 10 Attained</u>	<u>Gr. 11 Attained</u>	<u>High School Cert.</u>	<u>College</u>	<u>College or More</u>	<u>University Degree</u>
<b>Detailed Specification</b>									
School Dropout Age =14	0.0493 [0.0116]***	0.0148 [0.0141]	0.0067 [0.0092]	-0.0061 [0.0095]	-0.0134 [0.0084]	-0.0024 [0.0060]	0.002 [0.0058]	0.0013 [0.0061]	-0.0004 [0.0035]
School Dropout Age =15	0.0974 [0.0148]***	0.1169 [0.0200]***	0.0725 [0.0120]***	0.0682 [0.0127]***	0.0311 [0.0101]***	-0.004 [0.0071]	0.0028 [0.0059]	0.0019 [0.0066]	0.0015 [0.0039]
School Dropout Age =16	0.0559 [0.0135]***	0.0455 [0.0180]**	0.0275 [0.0109]**	0.0152 [0.0116]	-0.014 [0.0086]	-0.0037 [0.0057]	0.0112 [0.0046]**	-0.0063 [0.0056]	-0.0088 [0.0034]**
Exemptions to Leaving Age Allowed	-0.0463 [0.0054]***	-0.0664 [0.0079]***	-0.0327 [0.0045]***	-0.028 [0.0051]***	-0.0223 [0.0038]***	0.0052 [0.0023]**	-0.0018 [0.0017]	-0.0025 [0.0021]	-0.001 [0.0013]
School Entry Age = 7	0.0592 [0.0086]***	0.0458 [0.0119]***	0.0512 [0.0094]***	0.0089 [0.0103]	-0.001 [0.0096]	0.0019 [0.0075]	-0.0011 [0.0054]	-0.0006 [0.0054]	0.0018 [0.0028]
School Entry Age = 6	0.0244 [0.0041]***	0.0225 [0.0056]***	0.0098 [0.0043]**	-0.004 [0.0052]	-0.0058 [0.0041]	0.003 [0.0032]	-0.0013 [0.0023]	-0.0126 [0.0024]***	-0.0051 [0.0015]***
<b>Summary Specification</b>									
Mandatory Number of School Years (separate regression)	0.0255 [0.0014]***	0.027 [0.0016]***	0.019 [0.0014]***	0.0118 [0.0012]***	0.0037 [0.0010]***	0.0007 [0.0009]	0.0004 [0.0008]	-0.0007 [0.0009]	-0.0006 [0.0006]

Notes: Regressions are on cell means from Census data, grouped by birth cohort, province, and census year. All regressions are weighted by cell sample size. The regressions include controls for log provincial public school expenditure, fraction by province in rural area, fraction by province in manufacturing sector, number of schools in province, number of teachers in province, and fixed effects for province, birth cohort, and census year. Huber-White standard errors are shown, clustered by province and birth cohort. One, two, and three asterix indicate coefficient is significantly different from zero at a 10 percent, 5 percent, and 1 percent confidence level. See text for details.

**Table 3**  
**Effects of Compulsory Schooling on Education Attainment**  
**Alternative Samples and Weighting**

	Population Weights		No Weights	
	<u>Full Samp.</u>	<u>Dropout Samp.</u>	<u>Full Samp.</u>	<u>Dropout Samp.</u>
School Dropout Age =14	0.2047 [0.0645]***	0.0813 [0.0750]	0.4031 [0.0488]***	0.2955 [0.0627]***
School Dropout Age =15	0.6111 [0.0862]***	0.6114 [0.0927]***	0.6737 [0.0621]***	0.6481 [0.0808]***
School Dropout Age =16	0.2878 [0.0787]***	0.0394 [0.0770]	0.3926 [0.0533]***	0.2776 [0.0662]***
Exemptions to Leaving Age Allowed	-0.2583 [0.0328]***	-0.2079 [0.0295]***	-0.1083 [0.0341]***	-0.0335 [0.0448]
School Entry Age = 7	0.3207 [0.0563]***	0.4416 [0.0699]***	0.3975 [0.0491]***	0.5655 [0.0639]***
School Entry Age = 6	0.095 [0.0255]***	0.2605 [0.0384]***	0.3006 [0.0403]***	0.4922 [0.0577]***
Grouped Observations	2010	2010	2010	2010

Notes: Regressions are on cell means from Census data, grouped by birth cohort, province, and census year. The first two columns are weighted by cell sample size. The last two columns give equal weight to provinces and birth cohorts. The regressions include controls for log provincial public school expenditure, fraction by province in rural area, fraction by province in manufacturing sector, number of schools in province, number of teachers in province, and fixed effects for province, birth cohort, and census year. Huber-White standard errors are shown, clustered by province and birth cohort. One, two, and three asterix indicate coefficient is significantly different from zero at a 10 percent, 5 percent, and 1 percent confidence level. See text for details.

**Table 4**  
**Effects of Compulsory Schooling on Education Attainment**  
**for Males and Females**

	Grade Attained: Full Sample		Grade Attained: Dropout Sample	
	Males	Females	Males	Females
School Dropout Age =14	0.3073 [0.0685]***	-0.0048 [0.0618]	0.1972 [0.0803]**	-0.1297 [0.0747]*
School Dropout Age =15	0.7207 [0.0906]***	0.3905 [0.0820]***	0.7473 [0.0999]***	0.393 [0.0883]***
School Dropout Age =16	0.3675 [0.0841]***	0.1215 [0.0723]*	0.1582 [0.0852]*	-0.1669 [0.0704]**
Exemptions to Leaving Age Allowed	-0.2494 [0.0332]***	-0.2673 [0.0318]***	-0.2136 [0.0315]***	-0.1964 [0.0286]***
School Entry Age = 7	0.3403 [0.0582]***	0.2563 [0.0603]***	0.4917 [0.0715]***	0.333 [0.0748]***
School Entry Age = 6	0.0968 [0.0287]***	0.0959 [0.0236]***	0.2912 [0.0408]***	0.2101 [0.0376]***
Grouped Observations	2010	2010	2010	2010

Notes: Regressions are on cell means from Census data, grouped by birth cohort, province, and census year. All regressions are weighted by cell sample size. The regressions include controls for log provincial public school expenditure, fraction by province in rural area, fraction by province in manufacturing sector, number of schools in province, number of teachers in province, and fixed effects for province, birth cohort, and census year. Huber-White standard errors are shown, clustered by province and birth cohort. One, two, and three asterix indicate coefficient is significantly different from zero at a 10 percent, 5 percent, and 1 percent confidence level. See text for details.

**Table 5**  
**OLS and IV Estimates for the Effect of Grade Attainment on Earnings and Income**

	OLS		IV: Population Weights		IV: No Weights	
	<u>Full Sample</u>	<u>Dropout Sample</u>	<u>Full Sample</u>	<u>Dropout Sample</u>	<u>Full Sample</u>	<u>Dropout Sample</u>
<b>Log Income</b>	0.112 [0.002]***	0.097 [0.005]***	0.1229 [0.0138]***	0.165 [0.0148]***	0.1421 [0.0249]***	0.1854 [0.0258]***
<b>Log Earnings</b>	0.1 [0.003]***	0.085 [0.006]***	0.0709 [0.0085]***	0.117 [0.0140]***	0.0783 [0.0180]***	0.1248 [0.0208]***
<b>Log Income Males</b>	0.11 [0.004]***	0.102 [0.004]***	0.0787 [0.0157]***	0.1102 [0.0194]***	0.1161 [0.0264]***	0.153 [0.0311]***
<b>Log Earnings Males</b>	0.088 [0.004]***	0.076 [0.004]***	0.0439 [0.0106]***	0.0823 [0.0156]***	0.07 [0.0213]***	0.1125 [0.0280]***
<b>Log Household Income</b>	0.115 [0.001]***	0.102 [0.004]***	0.051 [0.0190]**	0.1044 [0.0172]***	0.1022 [0.0238]***	0.145 [0.0264]***

Notes: Instrumental variables regressions instrument grade attainment on school leaving ages, an employment certificate exemption indicator, and school entry laws (first stage shown in previous tables). Regressions are on cell means from Census data, grouped by birth cohort, province, and census year. All regressions are weighted by cell mean sample sizes, except the last two columns, which are show unweighted results. The regressions include controls for log provincial public school expenditure, fraction by province in rural area, fraction by province in manufacturing sector, number of schools in province, number of teachers in province, and fixed effects for province, birth cohort, and census year. Huber-White standard errors are shown, clustered by province and birth cohort. One, two, and three asterix indicate coefficient is significantly different from zero at a 10 percent, 5 percent, and 1 percent confidence level. See text for details.

**Table 6**  
**OLS and IV Estimates for the Effect of Grade Attainment on Earnings and Income**  
**for Different Time Periods**

	IV Full Sample					IV Dropout Sample				
	All years	1920-50	1930-60	1940-70	1950-80	All years	1920-50	1930-60	1940-70	1950-80
Grade Attained Population Weights	0.1229 [0.0138]***	0.0942 [0.0145]***	0.0888 [0.0114]***	0.1311 [0.0126]***	0.1607 [0.0134]***	0.165 [0.0148]***	0.1035 [0.0193]***	0.1439 [0.0152]***	0.1787 [0.0116]***	0.1857 [0.0167]***
Grade Attained No Weights	0.1421 [0.0249]***	0.0925 [0.0293]**	0.088 [0.0117]***	0.1312 [0.0306]***	0.1205 [0.0196]***	0.1854 [0.0258]***	0.1209 [0.0399]**	0.1494 [0.0157]***	0.1815 [0.0282]***	0.1611 [0.0273]***
<b>Years Included</b>	<b>All years</b>	<b>1920-50</b>	<b>1930-60</b>	<b>1940-70</b>	<b>1950-80</b>	<b>All years</b>	<b>1920-50</b>	<b>1930-60</b>	<b>1940-70</b>	<b>1950-80</b>

Notes: Instrumental variables regressions instrument grade attainment on school leaving ages, an employment certificate exemption indicator, and school entry laws (first stage shown in previous tables). Regressions are on cell means from Census data, grouped by birth cohort, province, and census year. The top row is weighted by cell mean sample size, while the bottom row excludes weights. The regressions include controls for log provincial public school expenditure, fraction by province in rural area, fraction by province in manufacturing sector, number of schools in province, number of teachers in province, and fixed effects for province, birth cohort, and census year. Huber-White standard errors are shown, clustered by province and birth cohort. One, two, and three asterix indicate coefficient is significantly different from zero at a 10 percent, 5 percent, and 1 percent confidence level. See text for details.



**Table 7**  
**OLS and IV Estimates for the Effect of Grade Attainment on Earnings and Income**  
**for Different Regions**

	IV Full Sample					IV Dropout Sample				
	Maritimes	Quebec	Ontario	Quebec & Ontario	Western Provinces	Maritimes	Quebec	Ontario	Quebec & Ontario	Western Provinces
Grade Attained Population Weight	0.1277 [0.0052]***	0.1695 [0.0522]**	0.122 [0.0355]***	0.1635 [0.0290]***	0.1047 [0.0177]***	0.1807 [0.0200]***	0.1911 [0.0440]***	0.1666 [0.0290]***	0.1913 [0.0239]***	0.1443 [0.0321]***
Grade Attained Equal Weight	0.1134 [0.0090]***	0.1441 [0.0308]***	0.1465 [0.0219]***	0.1413 [0.0257]***	0.1086 [0.0279]**	0.168 [0.0163]***	0.1839 [0.0357]***	0.1854 [0.0233]***	0.1898 [0.0269]***	0.1564 [0.0329]***
<b>Provinces Excluded</b>	<b>Maritimes</b>	<b>Quebec</b>	<b>Ontario</b>	<b>Quebec &amp; Ontario</b>	<b>Western Provinces</b>	<b>Maritimes</b>	<b>Quebec</b>	<b>Ontario</b>	<b>Quebec &amp; Ontario</b>	<b>Western Provinces</b>

Notes: Instrumental variables regressions instrument grade attainment on school leaving ages, an employment certificate exemption indicator, and school entry laws (first stage shown in previous tables). Regressions are on cell means from Census data, grouped by birth cohort, province, and census year. The top row is weighted by cell mean sample size, while the bottom row excludes weights. The regressions include controls for log provincial public school expenditure, fraction by province in rural area, fraction by province in manufacturing sector, number of schools in province, number of teachers in province, and fixed effects for province, birth cohort, and census year. Huber-White standard errors are shown, clustered by province and birth cohort. One, two, and three asterix indicate coefficient is significantly different from zero at a 10 percent, 5 percent, and 1 percent confidence level. See text for details.

**Table 8**  
**OLS and IV Estimates for the Effect of Grade Attainment on Various Outcomes**

	Mean	OLS		IV: Population Weight		IV: Equal Weight	
		Full Sample	Dropouts	Full Sample	Dropouts	Full Sample	Dropouts
Unemployed; looking for work	0.044	-0.008 [0.001]***	-0.004 [0.001]***	0.005 [0.002]*	-0.01 [0.003]***	0.001 [0.001]	-0.008 [0.002]***
Working	0.478	0.039 [0.003]***	0.035 [0.002]***	0.013 [0.010]	0.057 [0.017]***	0.0393 [0.0068]***	0.059 [0.0074]***
Manual Occupation	0.237	-0.047 [0.003]***	-0.027 [0.003]***	-0.008 [0.006]	-0.05 [0.004]***	-0.0189 [0.0074]**	-0.0636 [0.0036]***
Clerical Service Occupation	0.410	0.013 [0.001]***	0.016 [0.001]***	0.054 [0.013]***	0.056 [0.012]***	0.0335 [0.0061]***	0.0486 [0.0053]***
prof manager	0.363	0.042 [0.002]***	0.018 [0.003]***	-0.026 [0.010]**	-0.001 [0.007]	-0.01 [0.0063]	0.0238 [0.0097]**
Skill or Trade Occupation	0.002	0 [0.000]***	0 [0.000]***	0.001 [0.000]**	0.002 [0.000]***	0.0002 [0.0001]**	0.0003 [0.0001]***
Bilingual	0.201	0.025 [0.017]	0.022 [0.017]	0.026 [0.007]***	0.013 [0.007]***	0.0431 [0.0118]***	0.0135 [0.0094]
Below low income level	0.131	-0.041 [0.002]***	-0.039 [0.001]***	-0.051 [0.008]***	-0.079 [0.010]***	-0.0399 [0.0027]***	-0.0521 [0.0049]***
Unemployment Insurance Benefits	0.118	-0.009 [0.001]***	-0.002 [0.002]	0.009 [0.006]	0.005 [0.011]	0.0012 [0.0057]	-0.0027 [0.0068]

Notes: Instrumental variables regressions instrument grade attainment on school leaving ages, an employment certificate exemption indicator, and school entry laws (first stage shown in previous tables). Regressions are on cell means from Census data, grouped by birth cohort, province, and census year. All regressions are weighted by cell mean sample sizes, except the last two columns, which are show unweighted results. The regressions include controls for log provincial public school expenditure, fraction by province in rural area, fraction by province in manufacturing sector, number of schools in province, number of teachers in province, and fixed effects for province, birth cohort, and census year. Huber-White standard errors are shown, clustered by province and birth cohort. One, two, and three asterix indicate coefficient is significantly different from zero at a 10 percent, 5 percent, and 1 percent confidence level. See text for details.