Estimating Average and Local Average Treatment Effects of Education when Compulsory Schooling Laws Really Matter

By Philip Oreopoulos*

The change to the minimum school-leaving age in the United Kingdom from 14 to 15 had a powerful and immediate effect that redirected almost half the population of 14-year-olds in the mid-twentieth century to stay in school for one more year. The magnitude of this impact provides a rare opportunity to (a) estimate local average treatment effects (LATE) of high school that come close to population average treatment effects (ATE); and (b) estimate returns to education using a regression discontinuity design instead of previous estimates that rely on difference-in-differences methodology or relatively weak instruments. Comparing LATE estimates for the United States and Canada, where very few students were affected by compulsory school laws, to the United Kingdom estimates provides a test as to whether instrumental variables (IV) returns to schooling often exceed ordinary least squares (OLS) because gains are high only for small and peculiar groups among the more general population. I find, instead, that the benefits from compulsory schooling are very large whether these laws have an impact on a majority or minority of those exposed. (*JEL* I20, I21, I28)

Researchers routinely use the IV method to evaluate programs and estimate consistent treatment effects. A valid instrumental variable, which helps determine whether an individual is treated, but does not determine other factors that affect the outcomes of interest, can overcome estimation biases that often arise when using the OLS method. Recent work in this field clarifies how to interpret estimated treatment effects using IV. When the treatment being evaluated has the same effect for everyone, *any* valid instrument will identify this unique parameter. But when responses to treatment vary, different instruments measure different effects. Guido W. Imbens and Joshua D. Angrist (1994) point out that, in this more realistic environment, the only effect we can be sure that this method estimates is the average treatment effect among those who alter their treatment status because they react to the instrument; they call this parameter the LATE. In some cases, the LATE also equals the average treatment effect among those exposed to the treatment, but only when persons do not make decisions to react to the instrument based on factors that also determine treatment gains (James J. Heckman, 1997).

In the return to schooling literature, researchers frequently use IV. Some often-cited examples include studies that measure the LATE among persons who attend college because they live close to college (David Card, 1995); persons who attend college because tuition is low (Thomas J. Kane and Cecilia Elena Rouse, 1995) and persons obliged to stay in school longer because they face more restrictive com-

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pulsory school laws (Angrist and Alan B. Krueger, 1991; Daron Acemoglu and Angrist, 2001). These instruments typically affect fewer than 10 percent of the population exposed to the instrument and often generate treatment effects that exceed those generated from OLS. Card (2001) and Lang (1993) suggest that the higher IV results could occur because they approximate average effects among a small and peculiar group, whereas OLS estimates, in the absence of omitted variables and measurement error biases, approximate average effects among everyone. In a model where individuals weigh costs and benefits from attaining additional school, LATE estimates from IV could exceed OLS estimates for a variety of reasons, for example, because individuals affected by the instruments are more credit constrained, have greater immediate need to work, or have greater distaste for school.[1]

Previous LATE estimates have provided little inkling about what the gains to schooling are for a more general population. An alternative parameter that captures this is the ATE, the expected gain from schooling among all individuals (or all individuals with a given set of covariates). Unlike the LATE, it does not depend on a particular instrument or on who gets treated. The ATE offers a theoretically more stable parameter when considering potential gains for anyone receiving an extra year of schooling or college study. A comparison between ATE and previous LATE estimates would help determine whether the earlier LATE results are anomalies, or whether the effects of education are by and large similar across a wider population.

In this paper, I exploit historically high dropout rates in the United Kingdom to estimate the LATE for secondary schooling. The result is an IV estimate that is probably closer to the ATE than any previously reported. I focus on a period when legislative changes had a remarkable effect on overall education attainment. Figure 1 shows this effect on the fraction of British-born adults that left full-time education at ages 14 or less and 15 or less. It is not difficult to realize at a glance of the figure that the minimum school-leaving age in Britain increased from 14 to 15 in 1947. Within two years of this policy change, the portion of 14-year-olds leaving school fell from 57 percent to less than 10 percent. The trend for the fraction of children dropping out at age 15 or less, however, remained intact—it appears virtually everyone who would have left school at age 14 left at age 15 after the change. I found an equally dramatic response to an increase in the minimum legal school-leaving age in Northern Ireland (see Figure 2), where the law change occurred ten years later.

I use these two U.K. experiences to make two contributions to the returns-to-schooling literature. First, I estimate average returns to high school from instrumental variables that affect almost half the population. The magnitude of the response from these policies provides a test to whether previous IV estimates often match or exceed OLS estimates because they identify average returns to schooling for a select group in the population. I demonstrate this test by comparing the results with LATE estimates from compulsory schooling laws in the United States and Canada, where very few students left early, to the U.K. estimates. If high school dropouts have more to gain than high school graduates from staying on an extra year, the local average treatment effect of compulsory education will be higher than the average treatment effect for the total population.[2] As the fraction affected by compulsory education increases, however, the LATE and ATE converge since the ATE includes the population of would-be dropouts. When the fraction affected by compulsory schooling increases, we can use the gradient of these estimates to determine the ATE population. This approach is similar in spirit to one proposed by Heckman and Edward Vytlacil (2001).

My second contribution is to adopt a regression discontinuity (RD) design to estimate average returns to schooling. The RD approach differs from previous studies by comparing education attainment and adult earnings for students just prior to and just after the policy

[1] Alternatively, IV results might exceed OLS if the instruments are invalid. Pedro Carneiro and James J. Heckman (2002) criticize the validity of some earlier studies by demonstrating that some often-used instruments correlate with proxies for innate ability.

[2] The term "dropout" is something of a misnomer in the United Kingdom, since those who failed to advance to secondary school were expected to leave at the earliest opportunity. I shall occasionally refer to U.K. school leavers as dropouts for convenience.

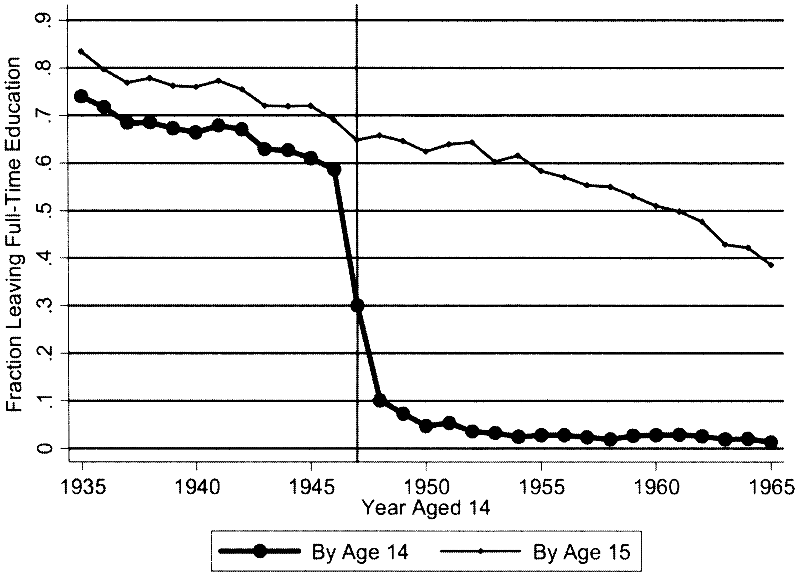

FIGURE 1. FRACTION LEFT FULL-TIME EDUCATION BY YEAR AGED 14 AND 15
(*Great Britain*)

Note: The lower line shows the proportion of British-born adults aged 32 to 64 from the 1983 to 1998 General Household Surveys who report leaving full-time education at or before age 14 from 1935 to 1965. The upper line shows the same, but for age 15. The minimum school-leaving age in Great Britain changed in 1947 from 14 to 15.

change. The break in education attainment is so stark in both Britain and Northern Ireland that we can show graphically the corresponding break in earnings and the source of the LATE identification.

My analysis addresses some previous concerns that have been expressed in the literature about the validity of earlier approaches to estimate returns to compulsory schooling. One approach, introduced by Angrist and Krueger (1991) uses quarter of birth to identify students allowed to drop out with less education than others because they were born after the school-entry cutoff date and waited a full year before entering school, compared to those born prior to the cutoff date. John Bound et al. (1995) show that if education attainment and quarter of birth are only weakly correlated, estimates can be biased in the same direction as OLS results are biased.[3] A second concern is that timing of birth may be related to other factors affecting earnings. Carneiro and Heckman (2002) provide some evidence that day of birth also relates to proxies for early childhood development.

Another approach takes advantage of differences in the timing of compulsory schooling law changes across regions (e.g., Acemoglu and Angrist, 2001; Lance Lochner and Enrico Moretti, 2004; Adriana Lleras-Muney, 2005). This technique uses the same identification strategy as a differences-in-differences research design. To estimate consistently the LATE of compulsory schooling, the timing of the law changes must not be correlated with any other policy changes or regional characteristics that also relate to the outcome variables. These studies assume, in the absence of law changes, that relative trends in the outcome variables are the same across regions.

[3] Douglas Staiger and James H. Stock (1997) and Luiz M. Cruz and Marcelo J. Moreira (2005) address concerns about this source of bias. Both attempt to correct for the weak instrument bias that may result from using quarter of birth to estimate returns to compulsory schooling and arrive at generally similar estimates to Angrist and Kureger's original study.

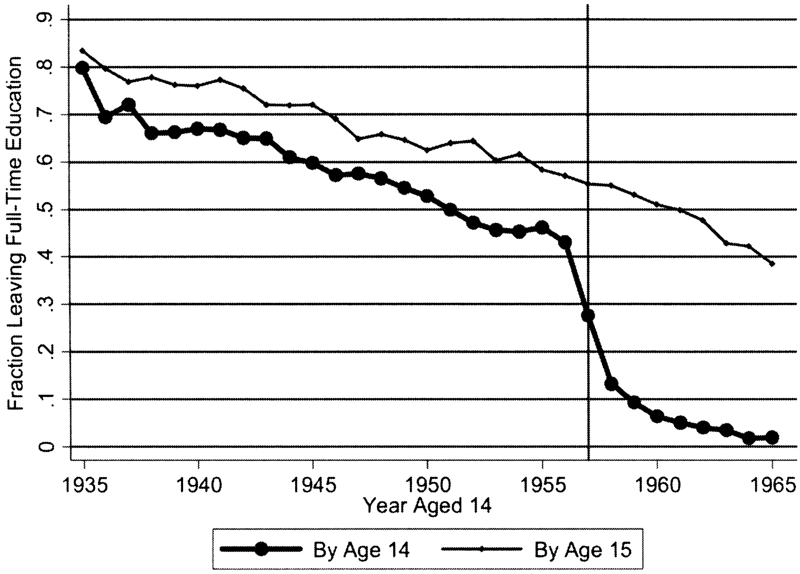

FIGURE 2. FRACTION LEFT FULL-TIME EDUCATION BY YEAR AGED 14 AND 15 (*Northern Ireland*)

Note: The lower line shows the proportion of Northern Irish adults aged 32 to 64 from the 1985 to 1998 General Household Surveys who report leaving full-time education at or before age 14 from 1935 to 1965. The upper line shows the same, but for age 15. The minimum school-leaving age in Northern Ireland changed in 1957 from 14 to 15.

A third approach also uses changes to the school-leaving age in the United Kingdom. Colm Harmon and Ian Walker (1995) were the first to adopt these school-leaving age changes to estimate returns to schooling, but did not include birth cohort effects in their regressions to account for a rising trend in education attainment and earnings. As a result, their estimates do not allow for systematic inter-cohort changes in educational attainment, and do not identify effects from cohorts who attended school just before and just after the law changes.

The regression discontinuity method used in this paper offers more compelling evidence on the average treatment effects of high school. The main conclusion of the paper is that the effects from compulsory schooling, are very large—ranging from an annual gain in earnings between 10 to 14 percent—whether a majority or minority of the population is affected. I also identify significant gains to health, employment, and other labor market outcomes. The results provide evidence that IV LATE estimates may often be similar to ATE, and that the main reasons why OLS results are weaker than IV are not likely due to differences in the population of individuals affected by the instrument.

The next section outlines a theoretical framework for interpreting returns to compulsory schooling and provides a parametric example of how increasing the fraction affected by compulsory schooling affects the LATE estimate relative to ATE. Section II provides background to the school-leaving age increases in Britain and in Northern Ireland. The regression discontinuity and instrumental variables analysis is also carried out in Section II. Section III compares the RD-IV results from Britain and Northern Ireland to the IV results for Canada and the United States. Section IV concludes with a discussion about why compulsory school laws seem to generate large gains for individuals who otherwise would have lefts school earlier.

I. Theoretical Framework

A. *Causal Inference and Compulsory Schooling*

In this section, I discuss the theoretical background to my analysis, beginning with how compulsory-schooling laws facilitate making causal inferences about various effects of education. I illustrate the differences between the LATE and ATE using a parametric example, then discuss the optimal schooling within the context of the setup, if schooling is seen as an investment. I follow the theoretical framework used by Angrist and Krueger (1999) and by Angrist (2004) to link the LATE identified using compulsory schooling as an instrument with the population ATE.

Let S_i be an indicator for whether child i attends school at age 15 ($S_i = 1$) or leaves at age 14 ($S_i = 0$). I define schooling, thus, as a binary variable indicating only a measure of high school attainment because this approach best represents the level of attainment affected by raising the minimum school-leaving age.[4] Let Y_{1i} be child i's circumstances after age 15 if $S_i = 1$, and let Y_{0i} be her circumstances after age 15 if $S_i = 0$. Although only one of these potential outcomes is ever observed, the average treatment effect, $E(Y_{1i} - Y_{0i})$, can be used to make predictive statements about the impact of high school on a randomly chosen person.

Let Z_i be a binary variable with $Z_i = 1$ if the school-leaving age equals 15, and $Z_i = 0$ if it equals 14. Suppose S_i is determined by the latent-index assignment mechanism,

$$S_i = 1(\gamma_0 + \gamma_1 Z_i > \eta_i) \tag{1}$$

where $\gamma_1 > 0$ and η_i is a random variable independent of the instrument. The potential treatment assignments are $S_{0i} = 1(\gamma_0 > \eta_i)$ and $S_{1i} = 1(\gamma_0 + \gamma_1 > \eta_i)$. The instrument has no effect on those already intending to attend school at age 15, but has a nonnegative effect on schooling for those leaving at age 14 without the law, so that $S_{1i} \geq S_{0i}$. Imbens and Angrist (1994) show that this monotonicity assumption, together with independence, implies:

$$\frac{E(Y_i|Z_i = 1) - E(Y_i|Z_i = 0)}{E(S_i|Z_i = 1) - E(S_i|Z_i = 0)} = E(Y_{1i} - Y_{0i}|S_{1i} > S_{0i}). \tag{2}$$

The left-hand side of this expression is the population analog of the Wald Estimator. Since $S_{1i} > S_{0i}$ holds only for individuals who leave at age 14 in the absence of the more restrictive law, the LATE on the right-hand side is the average effect of an additional year of school among those who otherwise would not have taken that extra year.

B. *A Parametric Example*

Following Angrist (2004), I calculate a parametric model to show the relative differences between LATE and ATE using the compulsory-school-law instrument. First, I assume that the distribution of $(Y_{1i} - Y_{0i}, \eta_i)$ is bivariate normal: $(Y_{1i} - Y_{0i}, \eta_i) \sim N_2[\mu_\beta, \mu_\eta, \sigma_\beta^2, \sigma_\eta^2, \rho]$. Thus, the ATE $[E(Y_{1i} - Y_{0i})]$ is μ_β, the mean of η_i is μ_η, the correlation between $Y_{1i} - Y_{0i}$ and η_i is ρ, and the respective variances for $Y_{1i} - Y_{0i}$ and η_i are σ_β^2 and σ_η^2. If ρ is positive, the likelihood of dropping out at age 14 is correlated with higher gains from school (after age 15). Since everyone must attend school at age 15 when the school-leaving age is 15 ($\gamma_1 \rightarrow \infty$), the LATE is equal to the treatment effect on the *nontreated*—the average effect of attending school at age 15 for those who leave at age 14. It can be written as:

$$\begin{aligned} \underset{\gamma_1 \rightarrow \infty}{E} (Y_{1i} - Y_{0i}|S_{1i} > S_{0i}) &= E(Y_{1i} - Y_{0i}|S_{0i} = 0) \\ &= E(Y_{1i} - Y_{0i}|\eta_i > \gamma_0) \\ &= E(Y_{1i} - Y_{0i}) \\ &\quad + \rho\sigma_\beta\lambda(x_\eta) \\ &= \mu_\beta + \rho\sigma_\beta\lambda(x_\eta) \end{aligned} \tag{3}$$

[4] The focus here is on high school. The average or local average treatment effects at the college level may differ from those examined here.

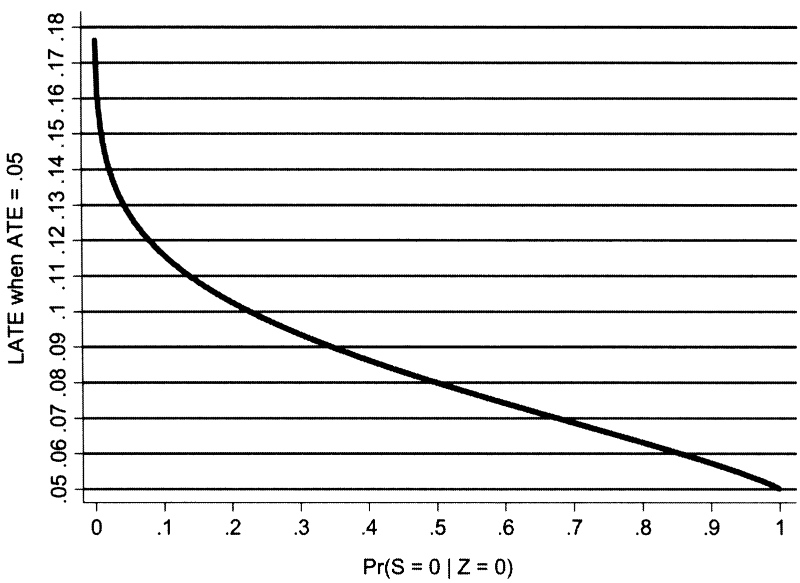

FIGURE 3. LATE AND ATE BY ALTERNATIVE FRACTIONS AFFECTED BY BINARY COMPULSORY SCHOOLING INSTRUMENT AND THE DEGREE OF SELECTION ON GAINS

Note: $\Pr(S = 0|Z = 0)$ is the fraction of the population affected by the binary compulsory schooling instrument Z. The figure assumes ATE = 0.05, $\sigma_\beta = .25$, and $\rho = 0.15$. See text for details.

where $\lambda(x_\eta)$ is the inverse Mill's ratio, $\varphi(x_\eta)/[1 - \Phi(x_\eta)]$; $\varphi(x_\eta)$ and $\Phi(x_\eta)$ are, respectively, the Normal density and distribution functions; and $x_\eta = (\gamma_0 - \mu_\eta)/\sigma_\eta$. This expression is the special case to Imbens and Angrist's (1994) definition of LATE when the instrument affects everyone with $S_{0i} = 0$. The discrepancy between the LATE and ATE decreases with $\Pr(S = 0|Z = 0) \equiv 1 - \Phi(x_\eta)$, the fraction of the population affected by the compulsory-school law, and increases with ρ, the correlation between treatment and gains. The interaction between these parameters matters as well. The LATE when the fraction affected by the law is large (for example, 0.5) is closer to the ATE when ρ is small.

Even with ρ and σ_β unknown, the ATE can be determined from two LATE values that differ only by the fraction of the population affected by the compulsory-school law. Suppose, for example, we have one LATE with $\Pr(S = 0|Z = 0) = p^L$, corresponding to $\lambda(x_\eta) = \lambda^L$, and the another with $\Pr(S = 0|Z = 0) = p^H$, corresponding to $\lambda(x_\eta) = \lambda^H$. Assume the other parameters are the same and $p^L < p^H$ ($\lambda^L > \lambda^H$). The ratio of the difference between the LATE and ATE is:

$$(4) \quad \frac{E(Y_{1i} - Y_{0i}|S_{0i} = 0, p^L) - \mu_\beta}{E(Y_{1i} - Y_{0i}|S_{0i} = 0, p^H) - \mu_\beta} = \frac{\lambda^L}{\lambda^H}.$$

Equation (4) indicates we can back out the value for μ_β from two LATE values that differ by $\lambda(x_\eta)$. The comparison provides much information on the value of ATE, and also on the direction of ρ. Equal LATE values imply $\rho = 0$. If the ratio of the two LATE values, $E(Y_{1i} - Y_{0i}|S_{0i} = 0, p^L)/E(Y_{1i} - Y_{0i}|S_{0i} = 0, p^H)$, exceeds one, $\rho > 0$ and vice versa.

To illustrate, Figure 3 plots the ATE and LATE against the fraction of would-be-dropouts, $\Pr(S = 0|Z = 0)$, when $\mu_\beta = 0.05$, $\sigma_\beta = 0.25$, and $\rho = 0.15$. The figure shows the difference between ATE and LATE declining as the fraction affected by compulsory schooling increases.[5]

[5] This is generally not the case if the instrument does not fully constrain all individuals (see Angrist, 2004).

When ρ is positive, a smaller fraction affected by the instrument always leads to a larger difference between the LATE and ATE. The LATE is 0.15, 0.13, and 0.08 when the instrument affects, respectively, 1, 5, and 50 percent of the population. The absolute difference between the LATE and ATE when the instrument affects 5 percent of the population is 2.6 times the difference when the instrument affects 50 percent of the population.

The main point for the purposes of this paper is that, with about half the students in mid-century United Kingdom leaving school as soon as possible, the LATE from raising the school-leaving age should come close to the ATE. In addition, comparing compulsory school law effects across countries helps verify how close this estimate is to the ATE. A substantial difference between LATE estimates using North American compulsory-school laws (that affect few) and those from the United Kingdom (that affect many) would suggest a high value for ρ. On the other hand, a small difference would suggest that the correlation between dropping out and above-average gains is small, and that a large correlation is an unlikely explanation for why OLS and IV returns to schooling estimates sometimes differ.

C. *ATE, LATE, and Optimal School Attainment*

It is interesting to note the effect of compulsory schooling when education is viewed as an investment. Suppose the total cost of child i's schooling is c_i, which may include effort, psychological costs, forgone earnings, and lowered immediate consumption caused by not being able to borrow. An investment model of education assumes an individual leaves school at age 14 ($S_i = 0$) if costs exceed gains:

$$c_i > Y_{1i} - Y_{0i}. \tag{5}$$

In this example, the only individuals affected by the instrument are those for which costs from additional schooling exceed circumstantial gains. In such a situation, compulsory schooling restricts choice and lowers welfare among individuals wanting to leave sooner, even among those who are credit constrained.[6]

Expressions (1) and (5) imply

$$\eta_i - \gamma_0 = c_i - (Y_{1i} - Y_{0i}). \tag{6}$$

If individuals choose education attainment by weighing costs and benefits as in expression (6), and ρ_{10} is positive, then costs must be proportionately higher for those who drop out at age 14 than for those who continue on to age 15. Similar reasoning has been used to explain why IV estimates of the returns to compulsory schooling are often higher than corresponding OLS estimates (see Card, 2001). If OLS estimates of the ATE are upward biased because students with better cognitive and noncognitive skills tend to obtain more schooling, IV estimates that attempt to correct for this bias should be lower.

II. Minimum Schooling Laws in Great Britain and Ireland

A. *U.K. Data*

The data used for the U.K. analysis are derived from combining 15 U.K. General Household Surveys (GHS) from 1983 to 1998 (the 1997 GHS was cancelled) with 14 Northern Ireland Continuous Household Surveys from 1985 to 1998. (For simplicity's sake, I use the term GHS to refer to both kinds of surveys, since the two questionnaires were almost identical.) The major difference was that earnings from the U.K. GHS were coded exactly, while earnings from the Northern Ireland GHS were coded by category. Average earnings in the Northern Irish GHS were assigned for individuals within grouped earnings categories. A fortuitous characteristic of the U.K. data is that education is recorded as the age an individual completed full-time education. This measure corresponds

[6] While not advocating either for or against compulsory schooling, Barry R. Chiswick (1969) notes, "while those compelled to over-invest [in school] experience an increase in their annual post-investment income, they experience a decrease in their marginal and average internal rates of return."

exactly with the school-leaving-age laws. The combined dataset contains 66,185 individuals who were age 14 between 1935 and 1965 and 32 to 64 years of age at the time of the survey. (The data go back only to 1983, so we cannot use respondents younger than 32 for this analysis.) I also examine unemployment outcomes and health status using the full sample of earners and nonearners. The U.K. GHS sample includes only British-born adults, while the Northern Ireland GHS includes all native and foreign born respondents, since the Northern Ireland surveys did not record place of birth. The data were aggregated into cell groups by survey year, gender, birth cohort, and region (Britain or Ireland). The remaining number of cells was 1,492, and half this for males.

B. *A Brief History of the 1947 and 1957 U.K. School-Leaving-Age Reforms*[7]

There were two changes to the school-leaving age in Britain and Northern Ireland between 1935 and 1965, both of which had a remarkable influence on the education attainment of British young people. Legislation from Great Britain's 1944 Education Act raised the school-leaving age in England, Scotland, and Wales in 1947 from 14 to 15 years. Figure 1, previously mentioned in the introduction, shows the effect of this legislative change: before 1947, a very high fraction of children in Britain left full-time school at age 14 (or before). Over just three years, however (between 1945 and 1948), the portion of 14-year-olds leaving schools fell from about 57 percent to less than 10 percent.[8] This massive rise in enrollment was made possible through a concerted national operation that expanded the supply of teachers, buildings, and furniture.[9]

The government's motivation for increasing the school-leaving age was to "improve the future efficiency of the labour force, increase physical and mental adaptability, and prevent the mental and physical cramping caused by exposing children to monotonous occupations at an especially impressionable age" (Halsey et al., 1980, p. 126). Public support for raising the school-leaving age was widespread for many years before the legislation was enacted. The Education Act of 1918, which raised the school-leaving age from 12 to 14, also called for a further increase to age 15 "as soon as possible," but for some time this proposal did not garner much political support out of fear of the costs involved. Further attempts to raise the amount of compulsory schooling were made in 1926, 1929, 1933, 1934, and 1936, with no success, again mostly because of budgetary concerns. But some officials opposed to additional age restrictions had also argued that the majority of the population did not perceive an advantage from extended education (Bernbaum, 1967). In the years leading up to the 1944 legislation, however, public support for raising the school-leaving age grew widespread.

Prior to 1947, those wanting to advance in school beyond age 14 usually moved from elementary to secondary school at age 12. Transfers were possible afterward, but not common. Those planning to leave school and seek work as soon as the law permitted generally remained in elementary school, which usually offered education up to age 14. Pupils transferred to secondary school at no cost on the basis of competitive examinations. The proportion of mandated free places began in 1907 at 25 percent of total attendance and rose to more than 50 percent by 1931. Students at the secondary level who did not win free places paid fees that were subsidized by more than two-thirds by the state. The 1944 Education Act removed these fees and made the first year of secondary school compulsory. The observations based on Figure 1, that the removal of fees in 1944 had little effect on education attainment and that the raising of the school-leaving age had little effect on education attainment beyond age 15, suggest that fees were not the chief factor preventing early school-leavers from staying on.

Northern Ireland's 1947 Education Act was closely modeled on Britain's, similarly raising

[7] For a more detailed discussion of the history of British and Irish education over the period of analysis, good sources include Albert H. Halsey et al. (1980), Gerald Bernbaum (1967), Howard C. Barnard (1961), Harold C. Dent (1954, 1957, 1970, 1971), P. H. J. H. Gosden (1969), and Thomas J. Durcan (1972).

[8] The finding that some adults reported finishing school at age 14, even after the school-leaving age had changed, may reflect measurement error, noncompliance, or delayed enforcement.

[9] The government dubbed these operations HORSA and SFORSA: Hutting, Seating, and Furniture Operations for the Raising of the School-leaving Age.

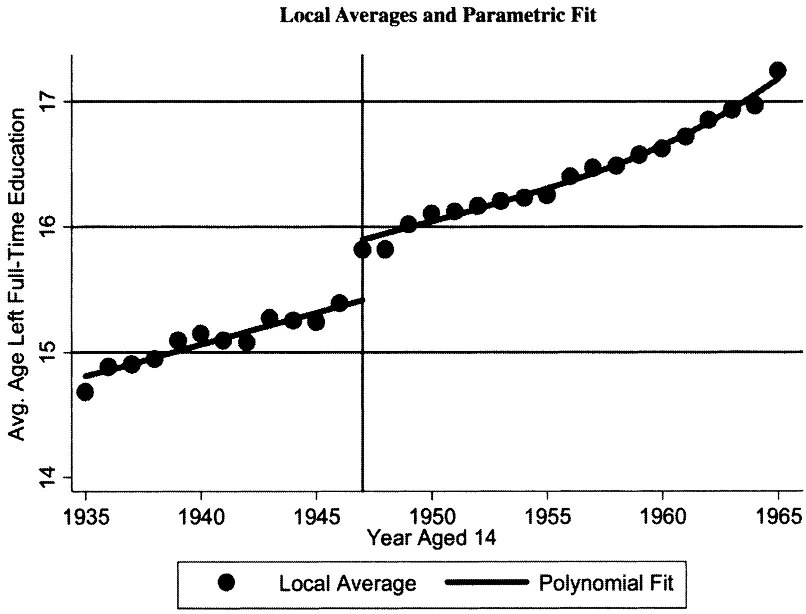

FIGURE 4. AVERAGE AGE LEFT FULL-TIME EDUCATION BY YEAR AGED 14
(Great Britain)

Note: Local averages are plotted for British-born adults aged 32 to 64 from the 1983 to 1998 General Household Surveys. The curved line shows the predicted fit from regressing average age left full-time education on a birth cohort quartic polynomial and an indicator for the school-leaving age faced at age 14. The school-leaving age increased from 14 to 15 in 1947, indicated by the vertical line.

the school-leaving age from 14 to 15. In Northern Ireland, however, the change was not implemented until 1957 due to political stonewalling. Figure 2 charts the proportion of Northern Irish youths dropping out at age 14 and the total proportion dropping out at age 15 or younger. A clear break can be seen for portion of early school-leavers in 1957. By this time, the fraction of 14-year-old school-leavers had already gone down by 11.1 percentage points from its 1946 level, but this was still a striking drop in this variable, around 39.8 percentage points, in just two years.[10]

[10] Bernbaum (1967) discusses the degree of disruption caused by World War II, especially to those evacuated from some urban areas in Britain. The regression discontinuity design of this study avoids a general comparison between students in school before and after the war. Furthermore, I find similar results using an older group of cohorts from the Northern Ireland data.

C. *A Regression Discontinuity and Instrumental Variables (RD-IV) Analysis for the Returns to Compulsory Schooling in Britain and Northern Ireland*

In order to estimate more familiar and comparable returns to years of schooling, I also measured education changes by the age at which respondents left full-time school; the discontinuities observed in Figures 1 and 2 held true. The jump in education attainment turned out to be, not surprisingly, similar, since raising the school-leaving age to 15 had little effect on students who stayed on beyond that age.

Figure 4 plots the mean age cohorts left full-time school by the year they were age 14, using native-born 32- to 64-year-olds in the 1983 to 1998 British GHS. The vertical line indicates the year after which cohorts faced a school-leaving age of 15. While the average education attainment rises for progressively later birth co-

TABLE 1—ESTIMATED EFFECT OF MINIMUM SCHOOL-LEAVING AGE ON AGE FINISHED FULL-TIME EDUCATION AND LOG ANNUAL EARNINGS
(Great Britain and Northern Ireland, ages 25–64, 1935–1965)

Sample population	(1) (First stage) dependent variable: Age finished full-time school	(2)	(3)	(4) (Reduced form) dependent variable: log annual earnings	(5)	(6)	(7) Initial sample size
Great Britain	0.440	0.436	0.453	0.065	0.064	0.042	57264
	[0.065]***	[0.071]***	[0.076]***	[0.025]**	[0.026]**	[0.043]	
Northern Ireland	0.397	0.391	0.353	0.054	0.074	0.074	8921
	[0.074]***	[0.073]***	[0.100]***	[0.27]*	[0.025]***	[0.045]	
G. Britain and N. Ireland with	0.418	0.397	0.401	0.073	0.058	0.059	66185
N. Ireland Fixed Effect	[0.040]***	[0.043]***	[0.045]***	[0.016]***	[0.016]***	[0.018]***	
Birth Cohort Polynomial Controls	Quartic	Quartic	Quartic	Quartic	Quartic	Quartic	
Age Polynomial Controls	None	Quartic	None	None	Quartic	None	
Age Dummies	No	No	Yes	No	No	Yes	

Notes: The dependent variables are age left full-time education and log annual earnings. Each coefficient is from a separate regression. Each regression includes controls for a birth cohort quartic polynomial and indicator whether a cohort faced a school leaving age of 15 at age 14. Columns 2, 3, 5, and 6 also include age controls: a quartic polynomial and fixed effects where indicated. Each regression includes the sample of 25- to 64-year-olds from the 1983 through 1998 General Household Surveys, who were aged 14 between 1935 and 1965. Data are first aggregated into cell means and weighted by cell size. Regressions are clustered by birth cohort and region (Britain or N. Ireland).

horts, there is a clear spike in mean attainment after 1947. Average schooling increases by exactly half a year between the cohorts that were age 14 in 1946 and in 1948. The figure also plots the fitted values from regressing the means on a quartic polynomial for year of birth and an indicator term for whether or not a cohort faced a minimum school-leaving age of 15 (all 14-year-olds in 1947 and after). The fit predicts an increase in education attainment between 1946 and 1947 of 0.44 years ($R^2 = 0.995$).[11]

This result, and the accompanying standard error (0.065), can also be seen in column 1 of Table 1, which was produced from the same GHS data as the figures. The data were first aggregated into cell means by birth cohort, region, and age. All regressions are weighted by cell size and clustered by cohort and region (Britain or Northern Ireland) using Huber-White standard errors.[12] Column 1 shows the predicted break in education attainment of 0.44, corresponding to Figure 2A. The t-statistic for rejecting the discontinuity is 6.5.

Figure 5 shows the analogous graph for Northern Ireland, where the school-leaving age increased from 14 to 15 in 1957. The plotted averages are somewhat less smooth than for Britain because of the smaller sample size ($R^2 = 0.989$). The quartic polynomial fit, also presented in Table 1, predicts an increase in average education attainment of 0.397 years in 1957, indicated by the vertical line.

Figures 6 and 7 plot the corresponding raw mean log earnings for the British and Northern Irish samples for 32- to 64-year-olds who were 14 years old between 1935 and 1965. Earnings are measured in 1998 U.K. pounds using the Retail Price Index.[13] Earnings from the com-

[11] I also tried fitting the grouped means in several other ways: with a quadratic, with a quadratic allowed to differ before and after 1947, and by omitting 14-year-olds in 1947, since not all faced a higher school-leaving age that year. These alternative specifications generate similar results.

[12] Card and David S. Lee (2004) underscore the importance of avoiding using conventional standard errors in a discrete RD design, such as this one, which depends on a specific functional form to compute the cohort trend.

[13] Note that the cross-section panel of the 1983 to 1998 panel includes fewer older cohorts at younger ages. For example, the GHS observes all those older than 51 who were 14-year-olds in 1945, but only those 14-year-olds of 1945 who are older than 61. This discrepancy does not affect testing for a trend break in mean earnings following the introduction of the more restrictive school-leaving age, but it does lead to an upward trend in earnings for younger cohorts. To account for the sensitivity of the results to the different age composition by birth cohorts, I also estimate the discontinuity with age polynomial controls and age fixed effects. Age fixed effects are possible with cohort effects

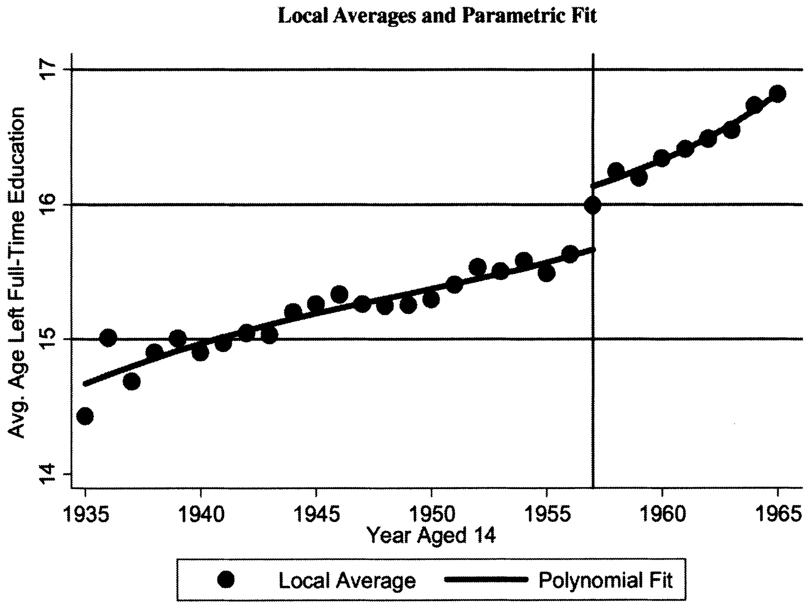

FIGURE 5. AVERAGE AGE LEFT FULL-TIME EDUCATION BY YEAR AGED 14 (*Northern Ireland*)

Note: Local averages are plotted for Northern Irish adults aged 32 to 64 from the 1985 to 1998 General Household Surveys. The curved line shows the predicted fit from regressing average age left full-time education on a birth cohort quartic polynomial and an indicator for the school-leaving age faced at age 14. The school-leaving age increased from 14 to 15 in 1957, indicated by the vertical line.

bined British GHS were 10.9 percent higher for cohorts aged 14 in 1948 than for those aged 14 in 1946. The ratio of reduced form coefficients for these two groups is 0.279 (0.109/0.390). The polynomial fit in Figure 6, which includes a quartic cohort trend, predicts that average earnings increased by about 6.5 percent for the cohorts that come after the rise in the school-leaving age. Column 4 in Table 1 shows this break is statistically significant, but the 95-percent confidence region around this value is considerably wide 0.016 to 0.114). The fitted increase in log earnings for Northern Ireland in 1957, however, is similar: a quartic fit with no age controls predicts an increase in log earnings of 0.054 points from raising the school-leaving age to 15. These point estimates are robust to the inclusion of quartic age controls, shown in column 5 of Table 1. The confidence region widens somewhat with the inclusion of age fixed effects (shown in column 6), but generally the results remain consistent with the trends shown in the previous figures.

Table 2 calculates RD-IV estimates for both Britain and Northern Ireland by regressing mean log earnings on a fourth-order polynomial control for birth cohort, the average age a cohort left full-time school, with this education measure instrumented by the minimum school-leaving age a cohort faced at age 14. All regressions use weighted cell means clustered by cohort. These results are shown in column 4: the estimated average increase in earnings in Britain from raising the school-leaving age to 15 is 14.7 percent. This figure corresponds to the Wald estimate from dividing the estimated average earnings discontinuity from Figure 6 by the estimated education attainment discontinuity from Figure 4 (0.065/0.442). The compara-

because the GHS contains multiple years of cross-section data. The results are also robust to the inclusion of survey year controls.

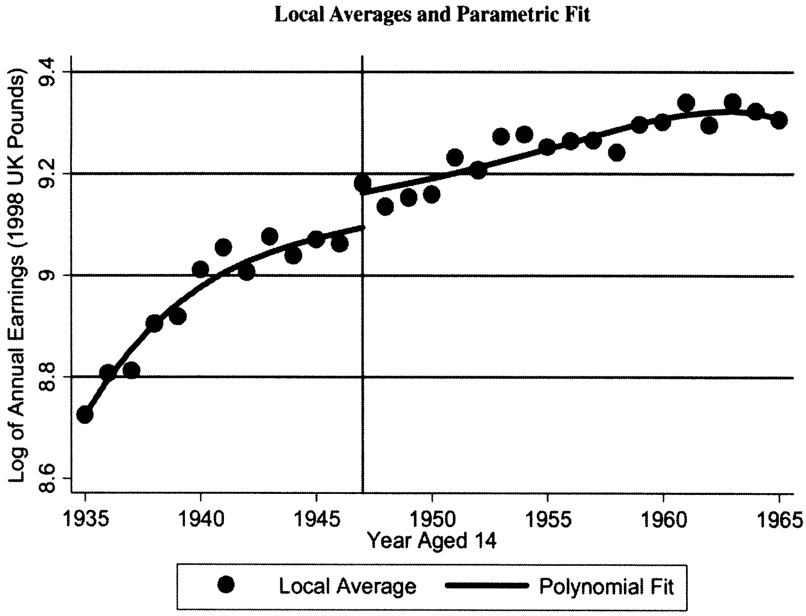

FIGURE 6. AVERAGE ANNUAL LOG EARNINGS BY YEAR AGED 14 (*Great Britain*)

Note: Local averages are plotted for British-born adults aged 32 to 64 from the 1983 to 1998 General Household Surveys. The curved line shows the predicted fit from regressing average log annual earnings on a birth cohort quartic polynomial and an indicator for the school-leaving age faced at age 14. The school leaving age increased from 14 to 15 in 1947, indicated by the vertical line. Earnings are measured in 1998 U.K. pounds using the U.K. retail price index.

ble estimate for Northern Ireland is 13.5 percent, and about 20 percent with the inclusion of age controls (see columns 5 and 6). These estimated effects are similar to previous returns to compulsory schooling estimates, in particular those presented in Harmon and Walker's (1996) U.K. analysis.

The regression discontinuity approach leads to some imprecision, given that earnings are tapering off for successively older birth cohorts at the time the discontinuity occurs. The analysis is strengthened, however, by moving to a difference-in-differences and instrumental-variables analysis by combining the two sets of U.K. data. Doing so lowers the average fraction affected by compulsory schooling a little, compared to using Britain alone, but the difference is not great. The third row in Table 1 shows the estimated effects of raising the school-leaving age on the average age respondents left full-time school for the combined British and Northern Irish samples. Again I regressed average education attainment on a quartic birth cohort control, an indicator for the minimum school-leaving age a cohort faced, and now an indicator for Northern Ireland. The estimated increase in years of schooling from the higher school-leaving age with the combined data is 0.42 years, compared to 0.44 using only the British sample. The standard error, however, falls considerably, to 0.04 (*t*-statistic = 13.6), and the results are robust to including age controls (shown in columns 2 and 3).

Figure 8, which shows the corresponding combined plots of British and Northern Irish education attainment by cohort, clearly illustrates the differences in attainment before and after the change in the school laws. Both regions follow almost identical upward trends in schooling until 1947, when Britain's attainment spikes. The average difference over the next ten years remains constant at about 0.5 years, and

Local Averages and Parametric Fit

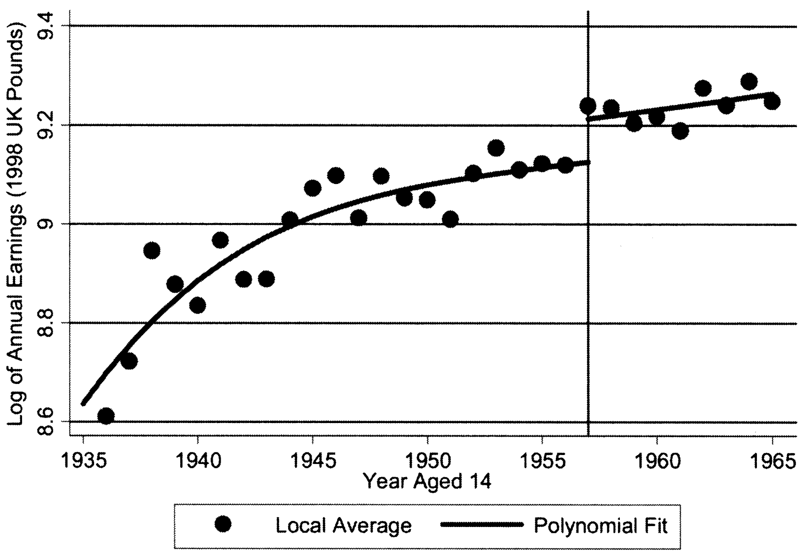

FIGURE 7. AVERAGE ANNUAL LOG EARNINGS BY YEAR AGED 14
(Northern Ireland)

Note: Local averages are plotted for Northern Irish adults aged 32 to 64 from the 1985 to 1998 General Household Surveys. The curved line shows the predicted fit from regressing average log annual earnings on a birth cohort quartic polynomial and an indicator for the school-leaving age faced at age 14. The school-leaving age increased from 14 to 15 in 1957, indicated by the vertical line. U.K. pounds using the U.K. retail price index.

TABLE 2—OLS AND IV RETURNS TO (COMPULSORY) SCHOOLING ESTIMATES FOR LOG ANNUAL EARNINGS
(Great Britain and Northern Ireland, ages 25–64, 1935–1965)

	(1)	(2)	(3)	(4)	(5)	(6)	(7) Initial sample size
	Returns to schooling: OLS			Returns to compulsory schooling: IV			
Great Britain	0.078	0.079	0.079	0.147	0.145	0.149	57264
	[0.002]***	[0.002]***	[0.002]***	[0.061]**	[0.063]**	[0.064]**	
Northern Ireland	0.111	0.113	0.113	0.135	0.187	0.21	8921
	[0.004]***	[0.004]***	[0.004]***	[0.071]*	[0.070]**	[0.135]	
G. Britain and N. Ireland with	0.082	0.082	0.083	0.174	0.149	0.148	66185
N. Ireland fixed effect	[0.001]***	[0.001]***	[0.001]***	[0.042]***	[0.044]***	[0.046]***	
Birth cohort polynomial controls	Quartic	Quartic	Quartic	Quartic	Quartic	Quartic	
Age polynomial controls	None	Quartic	None	None	Quartic	None	
Age dummies	No	No	Yes	No	No	Yes	

Notes: The dependent variable is log annual earnings. Each regressions includes controls for a birth cohort quartic polynomial and age left full-time education (instrumented by an indicator whether a cohort faced a school leaving age of 15 at age 14 in columns 4 through 6). Columns 2, 3, 5, and 6 also include age controls: a quartic polynomial and fixed effects where indicated. Each regression includes the sample of 25- to 64-year-olds from the 1983 through 1998 General Household Surveys who were aged 14 between 1935 and 1965. Data are first aggregated into cell means and weighted by cell size. Regressions are clustered by birth cohort and region (Britain or N. Ireland).

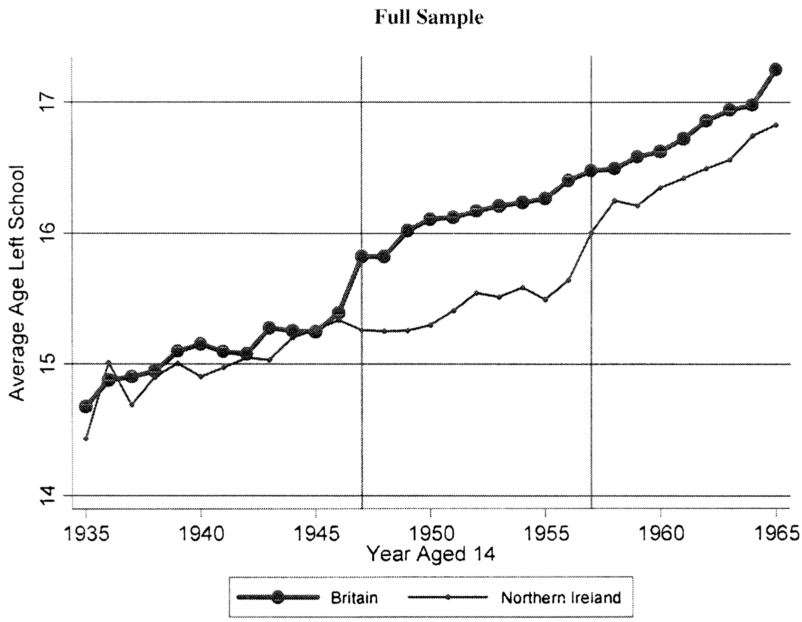

FIGURE 8. AVERAGE AGE LEFT FULL-TIME EDUCATION BY YEAR AGED 14
(*Great Britain and Northern Ireland*)

Note: The upper dark line shows the average age left full-time education by year aged 14 for British-born adults aged 32 to 64 from the 1983 to 1998 General Household Surveys. The lower light line shows the same, but for adults in Northern Ireland.

the gap quickly narrows again after 1957, the year when Northern Ireland increased its school-leaving age.

Combining the two sets of U.K. data also helps increase the precision in estimating the reduced-form effect on earnings. Figure 9 presents the mean log earnings plots from Figures 6 and 7 on the same grid. While the trend breaks for Britain in 1947 and for Northern Ireland in 1957 are apparent, comparing the two samples reinforces these changes. The combined U.K. reduced form estimates for the average increase in earnings using quartic cohort and age controls are displayed in the third row of Table 1, columns 4 and 5. Again, estimates based on the combined data are very similar to the separate RD results: that raising the school-leaving age to 15 increased earnings, on average, by about 5.5 or 7 percent, depending on the age controls used. This combined analysis allows for more robust results than the individual calculations, with a considerably lower standard error.

Table 2 shows IV estimates corresponding to the data displayed in Figures 8 and 9. Column 4 of the third row shows that, without age controls, raising the school-leaving age is associated with a 17.4-percent increase in earnings. Adding a quartic age control (column 5) or age fixed effects (column 6) lowers this estimate to 14.9 percent.

The OLS results shown in columns 1 to 3 are lower, consistent with many previous IV and OLS comparisons. It may seem surprising that the OLS results differ from the IV results for Britain, since the law change had an impact on much of the same group used to estimate the OLS results. But these results assume linear returns to all levels of schooling. If we restrict the OLS sample to only those who left school at age 16 or before, the resulting OLS estimates are more similar to the IV. The OLS estimates corresponding to columns 1, 2, and 3 for this restricted sample of early school leavers are 14.5, 14.1, and 14.0, respectively. Thus, while OLS and IV results for the high school dropout

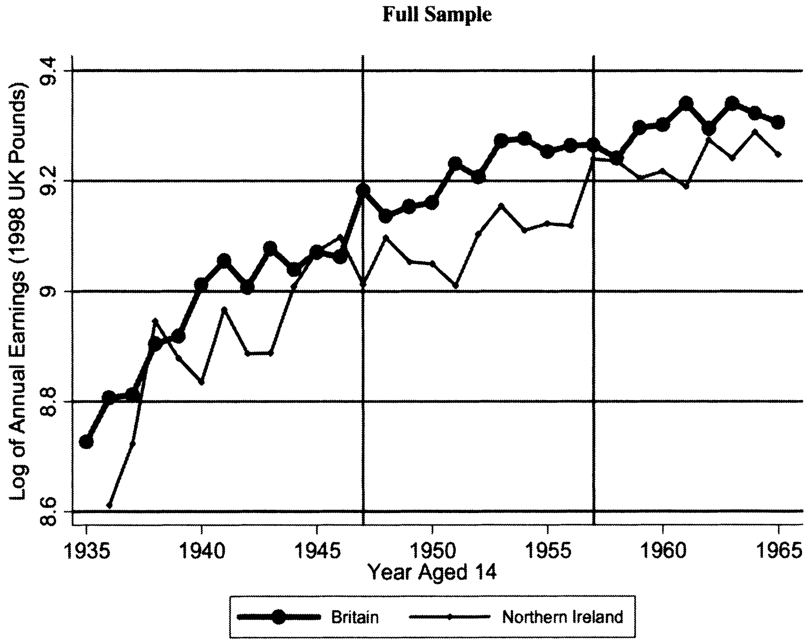

FIGURE 9. AVERAGE LOG ANNUAL EARNINGS BY YEAR AGED 14
(*Great Britain and Northern Ireland*)

Note: The upper dark line shows the average log annual earnings by year aged 14 for British-born adults aged 32 to 64 from the 1983 to 1998 General Household Surveys. The lower light line shows the same, but for adults in Northern Ireland.

sample are similar—which is not surprising given the large fraction in this sample affected by the school-leaving age—the OLS returns for those older than the school-leaving age are still smaller than the IV results.

III. A Cross-Country Comparison

In this section, I move across the Atlantic to consider the returns to compulsory schooling in North America and how they compare with the results for the United Kingdom. After a brief discussion of my Canadian and U.S. data sources, I show that the effects on education attainment from changes in the minimum school-leaving age in the United States and Canada were much smaller than for the United Kingdom. I then look at how the LATE compares across countries. The fact that my findings are reasonably similar suggests the LATE and ATE for high school may also be reasonably similar.

A. *North American Data*

Specific details of the U.S. and Canadian data extracts are provided in the Data Appendix. Wherever possible, I tried to maintain consistency in sample selection, school laws, and variable definitions across countries. The samples include all 25- to 64-year-old males and females who were age 14 in the years that the school-leaving ages were available (1915 to 1970 for the United States, and 1925 to 1970 for Canada). The U.S. analysis uses all native-born individuals age 25 to 64 from the six decennial census microdata samples from 1950 through 2000, and the Canadian data use native-born 25- to 64-year-olds from the 33-percent sample of the 1971 Census, and the five 20-percent samples of the 1981 through 2001 Censuses. For both countries, individuals are matched to the minimum school-leaving age in their state or province of birth the year that they were age 14. I also matched to each individual a number of regional controls. For the United States, these included average age, as well as the fractions of the population in each state that lived in an urban city, lived on a farm, was black, was in the labor force, and worked in the manufacturing industry. For Canada, I matched average per capita school and public expenditures and fractions of the population in each province that lived in urban areas and worked in the manufacturing sector.[14] I collapsed both datasets into cell means by state or province, birth cohort, census year, and gender.

B. *The Effect of School-Leaving Laws on School Attainment*

Table 3 presents the first-stage effects of the school-leaving age changes on education attainment and the corresponding reduced form effects of the school-leaving age on earnings. The first panel shows results for the United States, and the second for Canada; the third shows comparable results from the combined British–Northern Irish sample. All data are aggregated into cell means and weighted by population size. All regressions include fixed effects for birth year, region, survey year, gender, race, and a quartic in age—and additional regional demographic and economic controls according to when cohorts were age 14. The fourth panel repeats the regression discontinuity results for Britain, discussed in Section IIC. Since cohort fixed effects are not identified while trying to estimate the compulsory schooling effect, I use instead a quartic to control for cohort trends and age fixed effects, as before.

Column 1 shows the estimated impact from raising the school-leaving age on the total number of years of completed schooling. Note that the impact is much smaller for the United States and Canada than for the United Kingdom: raising the minimum school-leaving age by one year increased education attainment by only about 0.11 years. Furthermore, both Lochner and Moretti (2004) and I (Oreopoulos, 2003) find additional effects from compulsory schooling on education attainment beyond the minimum requirements for North America. Students compelled to stay in school an extra year who end up staying beyond the new limit push the estimated effect in column 1 higher, so the fraction actually affected by compulsory schooling in these countries may be smaller. (This situation differs from that in the United Kingdom; as discussed in the last section, Figures 1 and 2 show that raising the school-leaving age from 14 to 15 had no noticeable impact on students finishing beyond age 15.) The first-stage effect for all countries is considerably powerful, and we easily reject that the coefficients are zero.

Comparing columns 2 and 3 of Table 3 provides a specification check for whether other region-specific policies or economic conditions improved at the same time that minimum school-leaving ages increased. It is reassuring that law changes have no positive effect on the fraction of individuals who attained at least some post-secondary education or who left school beyond age 17 (column 3).[15] The coefficients for the samples of those with fewer than 12 years of completed schooling and of those who finished school by age 16 are about the same (column 2), as we would expect if those compelled to take additional schooling by changes in these laws still dropped out, only later.[16]

The last three columns show the reduced-form estimates for the effects of these dropout ages on earnings and wages. The main purpose of showing these results is to demonstrate that changes in the dropout age do not affect earnings in the post-secondary sample. Just as we should not expect compulsory schooling laws to affect post-secondary-schooling attainment, we should not expect them to affect outcome variables for the higher educated sample. If we did observe either of these effects, we might be concerned that other factors, ones affecting both dropouts and graduates, underlie the correlations in Table 1. While the laws are strongly

[14] Results were not very sensitive to the inclusion of alternative controls. Below I display the LATE estimates with and without the control variables, and with and without cohort trends.

[15] The fourth panel of Table 3 shows that in the British sample very few students stayed in school beyond age 16, which makes it hard to draw any conclusions about the corresponding coefficient in column 3.

[16] The results also suggest that changes in U.S. school-leaving laws influenced would-be dropouts to graduate. If we restrict the initial sample to those with 12 or fewer years of completed schooling, the findings are very similar to those from using the full sample.

TABLE 3—FIRST-STAGE EFFECTS OF COMPULSORY SCHOOLING ON EDUCATION ATTAINMENT AND EARNINGS FOR THE UNITED STATES, CANADA, AND THE UNITED KINGDOM

	(1)	(2)	(3)	(4)	(5)	(6)
	First-stage effects of dropout ages on schooling			Reduced form coefficients on earnings		
	Full sample	Sample with < high school	Sample with > high school	Full sample	Sample with < high school	Sample with > high school
	United States [1901–1961 birth cohorts aged 25–64 in the 1950–2000 Censuses]					
Dependent variable	Number of years of schooling			Log weekly wage		
Minimum school-leaving age at age 14	0.110	0.100	0.003	0.016	0.010	0.003
	[0.0070]***	[0.0097]***	[0.0027]	[0.0015]***	[0.0024]***	[0.0017]*
Initial sample size	2,814,203	727,789	1,173,880			
F-test: Schl.-leaving age coeff. is zero	243.5					
	Canada [1911–1961 birth cohorts aged 25–64 in the 1971–2001 Censuses]					
Dependent variable	Number of years of schooling			Log annual wage		
Minimum school-leaving age at age 14	0.130	0.130	−0.026	0.012	0.012	−0.003
	[0.0154]***	[0.0129]***	[0.0114]**	[0.0037]***	[0.0047]**	[0.0049]
Initial sample size	854,243	355,299	298,342			
F-test: Schl.-leaving age coeff. is zero	70.5					
	United Kingdom [1921–1951 birth cohorts aged 32–64 in the 1983–1998 GHHS]					
Dependent variable	Age left full-time education			Log annual wage		
Minimum school-leaving age at age 14	0.369	0.487	0.062	0.058	0.052	0.005
	[0.0305]***	[0.0309]***	[0.0785]	[0.0198]***	[0.0242]**	[0.0369]
Initial sample size	66,185	47,584	13,760			
F-test: Schl.-leaving age coeff. is zero	184.9					
	Britain [1921–1951 birth cohorts aged 32–64 in the 1983–1998 GHHS]					
Dependent variable	Age left full-time education			Log annual wage		
Minimum school-leaving age at age 14	0.453	0.483	0.351	0.042	0.045	−0.014
	[0.076]***	[0.0868]***	[0.2412]	[0.043]	[0.0387]	[0.0542]
Initial sample size	57,264	46,835	10,429			
F-test: Schl.-leaving age coeff. is zero	36.0					

Note: Regressions in the top three panels include fixed effects for birth year, region (state, province, Britain/N. Ireland), survey year, sex, and a quartic in age. The U.S. results also include a dummy variable for race, and state controls for fractions living in urban areas, black, in the labor force, in the manufacturing sector, female, and average age based on when a birth cohort was age 14. Provincial controls for Canada include fractions in urban areas, in the manufacturing sector, and controls for per capital public and school expenditures. Data are grouped into means by birth year, nation, sex, race (for the U.S.) and survey year and weighted by cell population size. Huber-White standard errors are shown from clustering by region and birth cohort. Single, double, and triple asterisks indicate significant coefficients at the 10-percent, 5-percent, and 1-percent levels, respectively. The omitted variable indicates ability to drop out at age 13 or lower for the U.S. and Canada, and 14 or less for the U.K. Samples include all adults aged 25 to 64. Dependent variable in column 3 for Canada is 1 = some post-secondary schooling, 0 otherwise. The last panel shows results with only the British sample, using a quartic birth cohort polynomial instead of cohort fixed effects. See text for more data specifics.

related to earnings among dropouts, column 6 shows no noticeable relationship between the minimum schooling a cohort faced when young and its average earnings for the post-secondary-school sample. These results are also suggestive that raising high school attainment had no region-specific externalities on the birth cohorts that attained post-secondary school.[17]

[17] For more specification checks to the robustness of these results, see Oreopoulos (2003).

C. *The LATE of Compulsory Schooling for the United States, Canada, and the United Kingdom*

I estimate that dropouts compelled to take an additional year of high school earn about 10 to 14 percent more than dropouts without the additional year. The returns to compulsory-schooling estimates are similar across all countries, whether restricting the initial sample by gender or by race. The effects generally appear to be the largest for the United Kingdom

TABLE 4—OLS, IV-DD, AND IV-RD ESTIMATES OF THE RETURNS TO (COMPULSORY) SCHOOLING FOR THE UNITED STATES, CANADA, AND THE UNITED KINGDOM

	(1) OLS full sample	(2) IV with regional controls	(3) IV with regional trends	(4) IV with regional trends and regional controls
Dependent variable	United States [1901–1961 birth cohorts aged 25–64 in the 1950–2000 censuses]			
Log weekly earnings (all workers)	0.078	0.142	0.175	0.405
	[0.0005]***	[0.0119]***	[0.0426]***	[0.7380]
Log weekly earnings (males)	0.070	0.127	0.074	0.235
	[0.0004]***	[0.0145]***	[0.0384]*	[0.1730]
Log weekly earnings (black males)	0.074	0.172	0.119	0.264
	[0.0004]***	[0.0137]***	[0.0306]***	[0.1295]**
	Canada [1911–1961 birth cohorts aged 25–64 in the 1971–2001 censuses]			
Log annual earnings (all workers)	0.099	0.096	0.095	0.142
	[0.0007]***	[0.0254]***	[0.1201]	[0.0652]**
Log annual earnings (males)	0.087	0.124	−0.383	0.115
	[0.0008]***	[0.0284]***	[1.1679]	[0.0602]*
	United Kingdom [1921–1951 birth cohorts aged 32–64 in the 1983–1998 GHHS]			
Log annual earnings (all workers)	0.079	0.158	0.195	NA
	[0.0024]***	[0.0491]***	[0.0446]***	
Log annual earnings (males)	0.055	0.094	0.066	NA
	[0.0017]***	[0.0568]	[0.0561]	
	Britain [1921–1951 birth cohorts aged 32–64 in the 1983–1998 GHHS]			
	OLS	RD		
Log annual earnings (all workers)	0.078	0.147	NA	NA
	[0.002]***	[0.061]**		
Log annual earnings (males)	0.055	0.150	NA	NA
	[0.0017]***	[0.130]		

Note: Regressions in the top three panels include fixed effects for birth year, region (state, province, Britain/N. Ireland), survey year, sex, and a quartic in age. The U.S. results also include a dummy variable for race, and state controls for fractions living in urban areas, black, in the labor force, in the manufacturing sector, female, and average age based on when a birth cohort was age 14. Provincial controls for Canada include fraction in urban areas, in the manufacturing sector, and controls for per capita public and school expenditures. Data are grouped into means by birth year, nation, sex, race (for the U.S.) and survey year and weighted by cell population size. Huber-White standard errors are shown from clustering by region and birth cohort. Single, double, and triple asterisks indicate significant coefficients at the 10-percent, 5-percent, and 1-percent levels, respectively. The omitted variable indicates ability to drop out at age 13 or lower for the U.S. and Canada, and 14 or less for the U.K. Samples include all adults aged 25 to 64. The last panel repeats regression discontinuity results from Table 2 using the British sample only and a quartic birth cohort polynomial instead of cohort fixed effects. See text for more data specifics.

sample, though the associated standard errors are high. Overall, there is no evidence that the U.S. or Canadian effects are higher than the British ones, except perhaps for U.S. black males.

The detailed IV estimates for the returns to compulsory schooling are shown in Table 4. Column 2 includes the IV results corresponding to the first and second stages in columns 1 and 2 of Table 3. All regressions in the first three panels include a quartic in age and fixed effects for birth cohort, region, survey year, and gender. The U.S. results also include a dummy variable for race; a number of state controls (fraction of state that lives in urban areas, is black, is in the labor force, works in the manufacturing sector, is female); and a variable for average age based on when the birth cohort was age 14. Province controls were used for Canada, including the fraction of the province that lives in urban areas and works in the manufacturing sector, as well per capita public and school expenditures. Data are grouped into means by birth year, region, gender, race (for the United States), and survey year. Huber-White standard errors are shown from clustering by region and birth cohort. The IV-RD results for Britain are repeated in the fourth panel of Table 4 and include a quartic cohort control and age fixed effects.

The results are robust to including linear region-specific cohort trends (see columns 3 and 4 of Table 4). These were included to control for relative changes in education attainment or earnings over time that differ by state or province, or between Northern Ireland and Britain, and are not due to changes from compulsory school laws. The regressions, however, unintentionally absorb some of the effects of compulsory schooling if schooling or earnings trend over time in a nonlinear way, or if the effects from the minimum school-leaving age take time to become fully enforced. Table 4, column 3, shows coefficient estimates after including linear region-cohort trends, but drops the regional control variables already included in column 2. Including these trends produces point estimates that are less precise—some smaller and some larger than in column 2—but not very different from before. Column 4 includes both regional cohort trends and the previous regional controls. Including both controls leads to multicollinearity, since regional changes in demographics and economic conditions both try to capture overall differences in regions over time. The range of possible true values for returns to schooling shown here is so wide that we cannot draw any meaningful conclusions from the results in this column.

The comparable full sample OLS results are shown in Table 4, column 1. I aggregated the country data also by level of schooling to calculate these results (still weighted by cell sample size). For all countries, OLS point estimates are significantly lower than the IV results. Across countries, however, the IV results are not very different, even though the proportion affected by changes in the school-leaving age in the United Kingdom exceeded that in the United States and Canada by 35 percentage points or more.

The results presented in Table 5 show other effects from compulsory schooling. Health outcomes are strongly associated with minimum school-leaving age changes, corroborating Lleras-Muney's (2002) finding that schooling lowers mortality. The 1990 and 2000 U.S. Censuses ask questions about physical and mental health limitations. Over 9 percent of the individuals in the sample aged 25 to 84 claim a physical or mental health disability that limits their personal care; I estimate that an additional year of compulsory schooling lowers the likelihood of reporting such a disability by 1.7 percentage points, a figure that is similar to the OLS estimate. Another year of compulsory schooling also lowers the likelihood of reporting a disability that limits one's daily activity by 2.5 percentage points. In the United Kingdom, the GHS questionnaire asks respondents to self-report whether they are in good, fair, or poor health. A one-year increase in schooling lowers the probability that a respondent reports being in poor health by 3.2 percentage points, and raises the chances he or she reports being in good health by 6 percentage points.

Schooling also affects many labor market outcomes in addition to earnings. In all three countries, I find that compulsory schooling lowers the likelihood of respondents being in the labor force and looking for work. The magnitude of the effect is similar across countries, and also comparable to corresponding OLS estimates. Further compulsory schooling also lowers the likelihood of receiving welfare and being classified as poor. Dropouts who drop out one year later are 6 percentage points less likely to fall below the U.S. poverty line and 3 percentage points less likely to fall below Canada's low-income cutoff.[18]

IV. Discussion and Conclusion

Because most students in the United Kingdom at mid-century tended to leave school at the earliest legal age, studying the effects of raising this age from 14 to 15 allowed me to estimate local average treatment effects of education that come close to mirroring average treatment effects. The regression discontinuity design of my U.K. analysis avoids having to assume unobservable trends in regional characteristics that could also affect the outcomes. Comparing LATE estimates for North America, where few students were affected by compulsory school laws, to the U.K. estimates provides a test of whether IV returns to schooling often exceed OLS because gains are high only for small and peculiar groups among the more gen-

[18] A household falls below the low-income cutoff if it spends more than 20 percentage points above the average comparative household on food, clothing, and shelter.

TABLE 5—OLS AND IV ESTIMATES FOR EFFECTS OF (COMPULSORY) SCHOOLING ON SOCIALECONOMIC OUTCOMES

	(1) Mean < HS sample	(2) OLS	(3) IV full sample
Country (schooling variable)			
	Health outcomes (ages 25–84)		
United States (total years of schooling)			
Physical or mental health disability that limits personal care	0.092	−0.014	−0.025
		[0.0003]***	[0.0058]***
Disability that limits mobility	0.128	−0.020	−0.043
		[0.0004]***	[0.0070]***
United Kingdom (age left full-time education)			
Self reported poor health	0.150	−0.037	−0.032
		[0.0016]***	[0.0113]***
Self reported good health	0.564	0.065	0.060
		[0.0021]***	[0.0155]***
	Other socialeconomic outcomes (ages 25–64)		
United States (schooling variable: total years of schooling)			
Unemployed	0.064	−0.004	−0.005
		[0.0002]***	[0.0040]
Receiving welfare	0.067	−0.013	−0.011
		[0.0002]***	[0.0024]***
Below poverty line	0.220	−0.023	−0.064
		[0.0002]***	[0.0085]***
Canada (total years of schooling)			
Unemployed: looking for work	0.062	−0.038	−0.010
		[0.0044]***	[0.003]***
Below low-income cutoff	0.227	−0.038	−0.026
		[0.0004]***	[0.0038]***
United Kingdom (age left full-time education)			
In labor force: looking for work	0.110	−0.030	−0.032
		[0.0044]***	[0.0150]**
Receiving income support	0.066	−0.025	−0.059
		[0.0024]***	[0.0259]**

Note: All regressions include fixed effects for birth year, region (state, province, Britain/N. Ireland), survey year, sex, and a quartic in age. The U.S. results also include a dummy variable for race, and state controls for fractions living in urban areas, black, in the labor force, in the manufacturing sector, female, and average age based on when a birth cohort was age 14. Provincial controls for Canada include fraction in urban areas, in the manufacturing sector, and controls for per capital public and school expenditures. Data are grouped into means by birth year, nation, sex, race (for the U.S.) and survey year and weighted by cell population size. Huber-White standard errors are shown from clustering by region and birth cohort. Single, double, and triple asterisks indicate significant coefficients at the 10-percent, 5-percent, and 1-percent levels, respectively. See text for more data specifics.

eral population. I find, instead, that the gains from compulsory schooling are very large—between 10 and 14 percent—whether these laws affect a majority or minority of those exposed.

This finding of high returns to compulsory schooling raises the question of why dropouts drop out in the first place. Why did so many leave school in the United Kingdom if staying on would have led to substantial gains, on average, to labor market and health outcomes?

One possibility, sometimes used to explain why IV returns to schooling estimates exceed OLS, is that individuals dropping out are credit-constrained. Considering the similarity of IV results across countries, this explanation could apply only if students from the United Kingdom tend to face greater financial constraints from staying on than students from the United States and Canada. As I discuss in Section II, however, while about half of secondary students in Britain paid some fees to attend school, the removal of these fees in 1944 did not affect attainment beyond age 15. Furthermore, many early school leavers do not work. Among 15- and 16-year-olds recorded in the 1950 U.S. Census as not in school, fewer than half (41 percent) were in the labor force and 89 percent lived with their parents.[19]

Several alternative explanations for dropout behavior exist. First, dropouts may simply abhor school. Poor classroom performance and condescending attitudes from students and teachers may make students want to leave as soon as possible, even at the expense of forgoing large monetary sums (Valerie E. Lee and David T. Burkam, 2003). Second, the uncertainty of additional earnings from staying on may be too high. If a student is risk-averse, higher expected returns from additional schooling may not be enough to offset higher probabilities of earning particularly low wages (David Levhari and Yoram Weiss, 1974; Stacey H. Chen, 2001). A third alternative is that dropouts may ignore or severely discount future consequences of their decisions (e.g., Ted O'Donoghue and Matthew Rabin, 1999). Cultural or peer pressures might also predominate adolescent decision-making and lead to dropout behavior; cultural norms that devalue schooling, a lack of emotional support, and low acceptance for higher education among peers may exacerbate students' distaste for school beyond the minimum age (e.g., George A. Akerlof and Rachel E. Kranton, 2002; and James C. Coleman, 1961). A final consideration is that students may simply mispredict, making incorrect present-value calculations of future returns or else underestimating the real gains of increased schooling.

We cannot determine with this paper's analysis which of these reasons might matter most, since the effects of compulsory schooling examined here arise only after leaving school, and costs (pecuniary and nonpecuniary) are not examined. But each explanation carries quite different implications about education policy. Exploring these issues more directly through innovative field experiments or by gathering data on high school students' expectations on gains and costs from staying on longer may shed further light on understanding dropout behaviour and, more generally, the overall education attainment decision-making process.

[19] Fifty years later, the pattern has not changed much. Among 17-year-olds not in school, according to the 2000 U.S. Census, for example, 90.4 percent lived with parents, and 45 percent were not in the labor force.

Data Appendix

A. The United States

Most of the U.S. analysis uses an extract of native-born individuals aged 25 to 64 from the six decennial census microdata samples between 1950 and 2000.[20] All censuses contained individual wages, poverty status, and labor force participation, but only the 1990 and 2000 datasets contained disability outcome measures. The initial sample size among those with positive wages was 2,814,203. After collapsing these into cell means, there were 29,804 cells by state, birth cohort, census year, and gender, and 15,003 cells among males. Hawaiian- and Alaskan-born respondents were excluded.

I coded the schooling variable for individuals in the 1950–1980 data as highest grade completed, capped at 17 to impose a uniform topcode across censuses. Following Acemoglu and Angrist (2001), average years of schooling were assigned to categorical values in the 1990 and 2000 Censuses using the imputation for males and females in Jin Huem Park (1999). The earnings variable, log weekly wage, was calculated by dividing annual wage and salary income by weeks worked, then taking logs.

The cell groups were assigned a minimum school-leaving age according to the year in which members of a birth cohort were 14 years old and the state in which they were born.[21] I measured each school-leaving age as the mini-

[20] The specific datasets used were the 1950 General 1/330 sample (limited to those with long-form responses), the 1960 General 1-percent sample, the 1970 Form 2 State 1-percent sample, the 1980 Metro 1-percent sample, the 1990 1-percent unweighted sample, and the 2000 1-percent unweighted random sample.

[21] My analysis assumes that Americans went to school in their state of birth, Canadians went to school in their province of birth, British-born residents went to school in Britain, and Northern Irish residents went to school in Northern Ireland. Some individuals will be mismatched. If mobility across regions is unrelated with law changes, this measurement error will not bias our estimates. Lleras-Muney (2005) concludes this seems to be the case for the United States.

mum between a state's legislated dropout age and the minimum age required to obtain a working permit.[22] During this period, a few states had no laws in place. I grouped the 2.2 percent of the sample that faced school-leaving ages lower than 14 into one category (school-leaving age < 14). All others faced dropout ages of 14, 15, or 16. The laws changed frequently over this period, both across states and within states over time. About one-third of the variation in the school-leaving age is across states and two-thirds is within. Not all changes were positive; in some instances the minimum school-leaving age went down.

I also generated control variables for state economic and demographic conditions in the year the laws were in place. For each census between 1910 and 1980, I calculated the average age of the population in each state, as well as the fraction living in an urban city, living on a farm, black, in the labor force, and working in the manufacturing industry. Values between decades were generated by linear interpolation.

B. Canada

The data extract for Canada comes from the four public-use micro-data censuses from 1981 to 1996. The main extract contained 25- to 64-year-olds born in a Canadian province who were 14 years old between 1925 and 1975. While provincially legislated school-leaving ages were available for earlier years, I chose to begin with 1925 for two reasons: the cohorts aged 14 before 1925 were older than 55 in the 1971 Census, and compulsory school laws were often minimally enforced at the beginning of the century. The initial sample size among those with positive wages was 854,243. After collapsing the data into province, birth cohort, gender, and census groups, the cell sample size was 3,296 among males and females, and 1,648 among males.

[22] Acemoglu and Angrist (2001), Lleras-Muney (2005), and Claudia Goldin and Lawrence Katz (2003) find working permit restrictions were often more binding than school-leaving age restrictions. The results are not sensitive to using just the dropout age as the compulsory school law variable, or the predicted mandatory number of school years, used by Acemoglu and Angrist (2001) and Lochner and Moretti (2004).

The education variable used for the Canadian analysis was total years of schooling, which refers to the total sum of the years (or grades) of schooling at the elementary, secondary, university, and nonuniversity years. I used the log of annual wages and salaries as the earnings variable for the Canadian data. I did not convert this variable to weekly earnings because a considerable number of full-time workers excluded their paid vacations or sick leave when reporting number of weeks worked in the previous year, contrary to census instructions.

The school-leaving laws were compiled directly from provincial statutes and revised statutes containing the acts of legislation and their amendments since inception. In a previous study, I documented the history of these changes and other compulsory school laws extensively (Oreopoulos, forthcoming). A few provinces in the first half of the century legislated different dropout ages for urban and for rural areas. For these cases, I recorded the dropout age as that for rural areas, since for most of that period the majority of residents lived in rural areas. All provinces except British Columbia experienced legislated increases in the school-leaving age during the period under study. Most provinces allowed for working permit exceptions to the age laws, but they were rarely applied. Fewer than 12 percent of the sample faced a school-leaving age of 16. I chose to group individuals facing a school-leaving age of either 15 or 16, since the effect on grade attainment from raising the school-leaving age to 16 from 15 was not significantly different from zero and was imprecisely measured. Including an indicator for facing a school-leaving age equal to 16 did not alter the second-stage estimates.

As with the U.S. extract, I generated control variables for provincial economic and demographic conditions using historical tabulations and linear interpolation. Oreopoulos (forthcoming) describes these control variables in more detail.

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