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Does Education Reduce Teen Fertility? Evidence from Compulsory Schooling Laws

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Abstract

While much descriptive work implies less-educated women are more likely to give birth as teenagers, there is much less evidence the relationship is causal. We investigate this possibility using variation in compulsory schooling laws (CSLs) in an instrumental variables framework to identify the impact of formal education on teen fertility for a large sample of women drawn from multiple waves of the Canadian Census. We find that greater CSL-induced schooling reduces the probability of giving birth as a teenager by roughly four to eight percentage points. We also explore possible mechanisms underlying this relationship by examining the timing of our estimates. We find evidence that education affects the timing of births in a way that strongly implies an "incarceration" effect of education. In particular, we find large negative impacts of education on births to young women aged seventeen and eighteen, but little evidence of an effect after these ages, consistent with the idea that being enrolled in school deters fertility in a contemporaneous manner, but not in the longer-run. Our findings are robust to the inclusion of several province-level characteristics including multiple dimensions of school quality as well as expenditures on public programs.

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1. Introduction

Prior research suggests that teen fertility entails significant private and social costs. Private costs associated with teen childbearing include lower levels of educational attainment by teen mothers as well as diminished labor market outcomes like lower labor force participation and lower wages, while plausibly external costs include greater dependence on welfare programs and an increased need for remedial school services. Beyond maternal costs, children born to teen mothers are more likely to experience reduced life chances due to poor school performance, greater substance abuse and an increased propensity to engage in criminal activity. While it is not clear that such associations are causal in nature, it seems likely that at least some of the negative consequences associated with teen fertility are indeed causal. To the extent that they are, policies that reduce teen fertility can improve the well-being of women who are predominantly considered disadvantaged relative to the general population.

Despite this possibility, there exists relatively little research on the possible causal effect of schooling on teen childbearing. Indeed, while there is much descriptive evidence that less-educated women are more likely to give birth as teens, there is much less credible evidence that this relationship is causal in nature. We investigate the causal relationship between educational attainment and teen childbearing for a large sample of women drawn from multiple waves of the Canadian Census. More precisely, we exploit variation in compulsory schooling laws (CSLs) in an instrumental variables framework to identify the impact of schooling on fertility as a teenager. While prior studies have been hampered by weak instruments, our estimates are unique in the literature because they imply a strong first stage relationship. In turn, we find that CSL-induced schooling reduces the probability of giving birth as a teenager by between four and eight percentage points. We also explore possible mechanisms underlying this relationship by examining the *timing* of our estimates as it relates to age of first birth. We find evidence that education affects the timing of births in a way that suggests a strong "incarceration" effect. In particular, we find large negative impacts of education on births to young women aged seventeen and eighteen, but little evidence of an effect after these ages, consistent with the idea that being enrolled in school deters fertility in a contemporaneous manner, but not in the longer-run. To address a recent critique of the broader CSL literature by Stephens and Yang (2014), we also include controls for school quality in our models. Our main estimates are quite robust to their inclusion; if anything both first stage and structural estimates become slightly larger. To the extent that avoiding a teen birth leads to improved life outcomes, our findings suggest that policies which attempt to increase educational attainment at the lower end of the distribution may have substantial non-pecuniary returns, at least in this dimension.

In the following section, we provide background information on Canadian compulsory school laws, discuss mechanisms by which schooling may affect teen fertility decisions and review related literature. Section 3 describes our data, focusing on key definitions such as how we label teen mothers, and also the relevant history of minimum school leaving ages which provide the variation which we use to identify the impact of schooling on teen fertility. Section 4 presents our empirical strategy which relies on instrumental variables estimation. We also discuss issues surrounding appropriate variance estimation when there are few clusters or sources of independent variation. Since CSLs are a matter of provincial policy and since there are only ten Canadian provinces, we cluster at the province level, but also implement the Wild cluster bootstrap procedure outlined in Cameron and Miller (2015). Overall, the precision of our estimates is robust to this procedure. Section 5 presents our findings, while Section 6 discusses them and concludes the paper.

2. Background

2.1. A Brief History of Canadian CSLs

Despite relatively recent attention by economists, compulsory schooling laws have existed in North America for well over a century. Perhaps not surprisingly, their pattern of development in Canada and the United States is quite similar, mirroring other key similarities including education being a function of state/provincial governments delivered by local governments and the use of similar, most often local, funding mechanisms in the relevant time periods (Katz, 1976). In what follows, we briefly describe the history of CSLs in Canada drawing heavily on existing research (Phillips, 1957; Axelrod, 1997; Oreopoulos, 2005). We describe the law changes we use for identification purposes in greater detail in the following section.

As in the United States, compulsory schooling laws in Canada were first enacted in the latter part of the 19th Century (Katz, 1976). Early versions of these laws were subject to many exemptions, most often based on the age of children, their necessity in supporting their families and distance lived from school. Generally speaking, however, these laws became more binding over time. Though an early adopter, the province of Ontario provides a good example of the typical evolution of CSLs in Canada. In 1871, Ontario became the first province in Canada to enact a compulsory schooling law, requiring children aged seven to twelve to attend school for at least four months per year.[1] Two decades later these ages were raised to between eight and fourteen, and legislation introduced penalties for non-compliance as well as for hiring school aged children, though many exemptions remained. For example, children under ten were

[1] CSLs appeared in British Columbia shortly afterward in 1873 with most Canadian provinces enacting them by the end of the first decade of 20th Century. Quebec is an interesting exception. Despite high levels of school attendance, Quebec did not enact formal CSL legislation until 1943, though it had relatively strict child labor laws which restricted children from working until age sixteen unless they could read and write.

exempted if they lived more than 2 miles from school while children ten and over were similarly exempted if they lived more than 3 miles away. Moreover, there was lax enforcement of the law, particularly in rural areas.[2] By the mid-1950s, the Schools Administration Act raised the age of school attendance to sixteen for all students in Ontario, though farm children over the age of fourteen were exempted as were children who were deemed to be essential to their family's subsistence. Similar to other Canadian provinces, even these exemptions were lifted in the early 1970s, which is also consistent with many U.S. states (Katz, 1976). Moreover, some Canadian provinces have further increased the age of compulsory attendance. For example, New Brunswick raised it to eighteen in 2000, as did Ontario in 2007 and Manitoba in 2011. Again, note that this broad overview does not explicitly discuss the law changes we use in our analysis; instead we do this in Section 3.

2.2. Why might compulsory schooling affect teen fertility?

Prior work outlines two broad mechanisms by which compulsory education might affect fertility. The first, consistent with economic models based on human capital theory, suggests that education lowers female fertility through an increase in the opportunity cost of time.[3] In short, if there are positive returns to additional schooling, women's labor market opportunities, including their wage rates, improve. Since children are highly time-intensive, such increases

[2] The deference shown to rural areas, mostly based on their agrarian nature and extensive use of child labor, is also apparent in the Adolescent School Attendance Act of 1921 whereby Ontario increased the compulsory age of attendance from fourteen to sixteen years old, but only for young adults living in urban areas. Perhaps not surprisingly, newly required fourteen and fifteen year olds were exempted from the law if they were employed at home or for wages and if they possessed a parent-endorsed "certificate of employment", which exempted youth from minimum school leaving laws, were often obtained by passing equivalency tests, typically at the level of grade 7 or 8, but sometimes merely tested basic skills like reading or writing. Interestingly, these young adults were still required to attend part-time instruction in the evenings, where such classes existed.

[3] Following Black, Devereux and Salvanes (2008), we collectively label these explanations "human capital" mechanisms in the discussion that follows.

might alter female fertility decisions, with the primary prediction that they reduce total fertility.[4] Indeed, theoretical models that consider lifecycle fertility highlight the wage rate as a key parameter (Willis, 1973; Wolpin, 1984; Barro and Becker, 1988; Hotz and Miller, 1988). While the literature understandably focuses on lifetime fertility, it is conceivable that teen fertility is also altered. If, for instance, additional compulsory schooling sufficiently improves the labor market opportunities of high school aged girls and if those impacted recognize this, it might change their near-term behavior. In other words, even if the bulk of returns to additional education are realized beyond the teen years, they may affect teen fertility if girls are even somewhat forward-looking and did not previously realize the economic opportunities afforded by additional education. In addition, it is possible that additional education improves either the amount of health information possessed or the efficiency with which individuals use it (Grossman, 1972; Rosenzweig and Schultz, 1982; Kenkel, 1991). For example, if education improves information regarding alternative forms of birth control and/or their appropriate use, fertility might be impacted. Indeed, prior work has shown that women with greater levels of formal education are more likely to use birth control and to use it properly (c.f., Balakrishnan et al., 1985; Tanfer and Horn, 1985). Again, while it seems likely that these human capital related mechanisms affect longer-run fertility, it is at least possible that they affect nearer-term, and hence teen, fertility.

The second broad mechanism by which education might affect fertility is more mechanical in nature and involves the allocation of time. In particular, when individuals are in school there is less time to engage in behaviors that occur outside of school, including sexual

[4] This explanation only references the substitution effect; there may be income effects which, if children are normal goods, the corresponding increase in income would increase investment in children; however, the dominant thinking is that such income effects are small relative to substitution effects.

activity.[5] Naturally, this implies that minimum school leaving ages have their desired effect; that is, they are indeed binding on the amount of formal schooling one receives which implies greater time devoted to schooling. More generally, they must also bind on a non-trivial fraction of students. In principle, it could be that greater seat time in school leads to reduced fertility or it might be due to programs or services offered in the school setting. Lovenheim et al. (2014), for instance, find that school-based family health centers reduce teen fertility. While their estimates are specific to the U.S. and cover a later time period than ours, they highlight a possible school-based mechanism. Peer effects are another possible avenue through which incarceration may deter fertility. For example, individuals induced by CSLs to remain in school (i.e., compliers) may spend less time outside of school with peers who continue to drop out (i.e., non-compliers) than prior to the CSL. In turn, this could mean less time with peers who might engage in risky behaviors that might influence sexual behavior and hence teen fertility.

Conceptually, these two broad mechanisms have different implications regarding the *timing* of fertility. In particular, if the incarceration effect operates to the exclusion of the human capital effect, as we describe it, one would expect less fertility around the relevant compulsory schooling ages. More precisely, one would expect to observe effects only in that relatively narrow period and not beyond it. Nevertheless, as noted above, the human capital mechanisms described might also affect near-term fertility if girls induced to complete additional education contemporaneously realize the potential returns to their now higher education level. Therefore, finding only a near-term effect of education on fertility, while most ostensibly consistent with incarceration, could also be due to what we label human capital mechanisms. Of course, it is plausible that higher minimum school leaving age laws have both incarceration and human capital effects; in other words, it is entirely possible for additional mandated schooling to impact

[5] In what follows, we label this mechanism an “incarceration effect” as in Black, Devereux and Salvanes (2008).

fertility in the short and longer runs for different reasons. Though imperfect, we explore each of these broad mechanisms by examining the impact of formal schooling on fertility at various maternal ages. In general, our findings point to a pure incarceration effect since education reduces fertility in the late teen years, but not beyond these ages.

2.3 Relevant literature

The relationship between education and labor market outcomes like earnings and employment has long interested economists. More recently, economists have become interested in broader notions of returns to education, including its possible impact on non-pecuniary outcomes like health, crime, and civic-oriented behavior. It is well-recognized that estimating the causal effect of education on any of these outcomes is challenging because educational choices reflect factors other than schooling, factors often not easily observed by the researcher. As a result, and in the absence of random assignment to different levels of education, economists have increasingly explored quasi-random variation in schooling induced by natural events or government policy. In what follows, we first briefly describe the broader literature and then focus on two studies which are most relevant to our work. We also review a recent study which challenges much of the broader literature and describe how we deal with issues raised therein.

One government policy which has received much attention is compulsory schooling laws (CSLs). Several studies use different parameterizations of these laws to instrument for educational attainment or estimate reduced form relationships. The earliest of these studies focused on the amount of schooling required, in essence subtracting compulsory school starting ages from corresponding school leaving ages, and studied educational attainment as well as labor market outcomes such as wage rates and earnings (c.f., Lang and Kropp, 1986; Acemoglu and

Angrist, 2000, Goldin and Katz, 2003, Black, Devereux and Salvanes, 2005; Oreopoulous, 2006). Later studies focused on broader outcomes and tended to find positive social impacts of increased formal schooling (c.f., Lleras-Muney, 2002, Lochner and Moretti, 2004, Lleras-Muney, 2005).[6] The findings of even more recent, and in large part, non-U.S. based studies, are decidedly more mixed both with respect to labor market outcomes (c.f., Meghir and Palme, 2005; Pischke and von Wachter, 2008; Grenet, 2013) and also broader social outcomes (c.f., Clark and Royer, 2013).

Two studies are of greatest relevance to our work, with the second being most relevant. First, McCrary and Royer (2011) examine the relationship between maternal education and fertility, as part of a larger study focusing on child health. Using data from California and Texas, these authors implement a regression discontinuity strategy based on school starting ages. They find little evidence of a relationship between increases in CSL-induced education and fertility.[7] While their data are very appropriate for examining the relationship between maternal education and child health, they are more limited in examining fertility since they use natality data which, by its nature, is comprised solely of births (i.e., those females who have given birth). The second paper, Black, Devereux and Salvanes (2008) is more directly relevant to our work. These authors examine the impact of educational attainment on teen fertility using compulsory schooling reforms in the United States and Norway. Of the two corresponding sets of analysis, the U.S. component is most similar to our work since it focuses on minimum school-leaving ages as well as the similarity of U.S. and Canadian educational systems. In particular, Black, Devereux and Salvanes (2008) use U.S. Census data from 1940 to 1980 and include women ages

[6] For an excellent review of work related to the social or non-pecuniary benefits of compulsory law-induced schooling see Oreopoulos and Salvanes (2011).

[7] McCrary and Royer (2011) also find no relationship between such education and child health, which again is the focus of their study.

20 to 30 in a given census year. As in our analysis, they infer whether a woman's first birth occurred in her teen years from her current household composition; in particular, the age difference between the mother and her eldest co-resident child. Restricting the sample to women under thirty years old thus reduces issues surrounding older teen children leaving mother's home and should reduce assignment error. Unlike most other literature, the authors do not use these laws to instrument educational attainment since they do not find a systematic first-stage relationship.[8] Instead, they estimate reduced form models that include state and cohort fixed effects as well as models that include state trends and variables that proxy labor market conditions. Their main estimates imply that an extra year of schooling is associated with between a five and nine percent decrease in teen fertility, depending on whether the CSL in question requires school attendance until age 16 or age 17, respectively. Finally, Black, Devereux and Salvanes (2008) test the notion of whether "incarceration" versus "human capital" mechanisms drive their estimates and find evidence for both explanations, though relatively weaker evidence for incarceration. We discuss these issues in detail later in the paper.

Despite reasonably consistent findings of a positive relationship between compulsory schooling laws and outcomes like earnings, health, civic behavior and others, forthcoming work by Stephens and Yang (2014) poses what may be a serious challenge to this literature. In particular, these authors revisit many of the CSL-induced education findings cited earlier, but systematically discover that they are not robust to the inclusion of a region-specific trend variable; the implication being that there exist factors other than CSLs which have changed over the period in question and that these factors may have impacted the outcomes studied. More

[8] Black, Devereux and Salvanes (2008) note that the lack of statistical significance is due to earlier studies clustering on state-year cells, rather than just state cells. More recent work suggests that clustering at the state level is preferred. That said, there is concern when the number of clusters is "too small". Since there are only ten Canadian provinces, we cluster at the province level and also implement the Wild cluster bootstrap procedure outlined in Cameron and Miller (2015) as discussed in detail in Section 4.

precisely, Stephens and Yang (2014) find that inclusion of a region-specific trend variable does not greatly affect the relevant first stage estimates (i.e., the regression of educational attainment on CSLs), but substantially alters corresponding structural estimates (i.e., the estimate of instrumented education on the ultimate outcome). These authors also explore one possible factor that changed along with CSLs, namely "school quality" as measured by Card and Krueger (1992).[9] When included, they find similar impacts on the estimates of studies they replicate; I.e., first stage estimates that remain relatively strong, but substantially different structural estimates. While this does not necessarily imply that school quality is "the" missing factor, the overall pattern of their findings casts at least some doubt on the findings of earlier work since they suggest pathways other than CSL-induced increases in formal schooling.

We address this critique in two ways—one conceptual and one empirical. First, we note that our main finding, as previewed in the introduction, is strongly consistent with an incarceration effect, rather than a human capital effect. In essence, our finding reflects the mechanical nature of schooling's impact on time use. As such, it is plausibly independent of factors like school quality to the extent that quality does not influence the efficacy of CSLs in increasing schooling. That said, we include a measure similar to the Card and Krueger (1992) measures of school quality used by Stephens and Yang (2014), but specific to Canadian provinces, in some of our models. As will be seen, our estimates—both first stage and structural—continue to obtain with their inclusion and even become slightly stronger. While we realize that this does not rule out other factors, we believe that it rules out a potentially important one.

[9] These school quality measures, which vary by state and year, include pupil to teacher ratio, length of the school year, and relative teacher salaries.

3. Data

The data used in this study are assembled from the 1971, 1976, 1981, 1986, and 1991 confidential extracts of the Canadian Census.[10] This sample was comprised of women between the ages of 16 and 40, and for these women, it was determined whether or not they had given birth to a child by a certain age. This could not be ascertained from a direct survey question posed to the women in the Census, since the Census does not directly ask women about the age at which they first gave birth. But since the Census is a household-level survey, it contains variables that identify individual households as well as the families within those households. Specifically, it identifies "Census Families"[11] living within these households (since some households contain multiple families), as well as characteristics of family members. These questions could be exploited to determine the age at which women in the sample had their first child. This was accomplished by calculating the age differential between the female head of the household (or female partner of the head of the household) and their eldest offspring present in the household. The analysis will use this age gap to consider women who did or did not give birth to children by a particular age (in particular, during their teenaged years). For the sake of reference, Figure one displays a schematic for the method used to identify potential mothers in our sample.

A drawback to this approach is that it's prone to some bias; as the age of the female household head increases, it is possible that their eldest child is no longer present in the household. This flaw leads to the possibility of misclassification of women who gave birth by a

[10] The confidential extracts of the Census were essential for this exercise because of a number of factors. Only the confidential Census extracts contain an individual's exact date of birth, as well as the necessary identifiers to link parents and children.

[11] A "Census family" refers to a married couple and the children, if any, of either or both spouses; a couple living common law and the children, if any, of either or both partners; or, a lone parent of any marital status with at least one child living in the same dwelling and that child or those children.

particular age, and so to account for this issue, our analysis will focus on increasingly young women. In the Census, virtually all heads of household are at least 16 years old; thus, by lowering the maximum age of women in the sample to thirty years of age, and by analyzing the proportion of women who have children at an age no younger than sixteen, we can assure that this misclassification bias is effectively eliminated.

To provide a general sense of the characteristics of the sample, Tables 1A through 1E displays means and standard deviations of certain variables for women in each of the five Censuses who either did or did not give birth to their eldest child as a teenager. The variables in the samples are: educational attainment (in years), age (in years), annual earnings (in the year prior to the Census, and reported in terms of nominal dollars from that year), the proportion who are married, and the number of weeks worked in the year prior to the Census. For the sake of simplicity, the tables compare three age ranges of progressively younger samples: women between the ages of 16 and 40, women between the ages of 16 and 35, and women between the ages of 16 and 30. In all three samples, the relative differences in the characteristics persist across all five Censuses: women who have had children in their teens are significantly less-educated, work fewer weeks of the year, and earn less money[12] than women who have not had children in their teens. Although this finding has been documented elsewhere in the literature, it confirms that women who have had children as teenagers also exhibit worse economic outcomes than those who have not had children as teens.

[12] The 1976 Census did not contain information on earnings or weeks worked, so these variables were omitted from Table 1B

4. Regression Results

Although not conclusive, the evidence in Tables 1A through 1E are suggestive of the fact that lower educational attainment may be a key factor in determining the proportion of women who give birth as teens. In order to further investigate the relationship between these two variables, Table 2 displays results from the following OLS regression:

$$(Proportion\ of\ Women\ no\ older\ than\ A\ who\ gave\ birth\ by\ age\ B)_{bpc}$$
$$= \beta(Years\ of\ Education)_{bpc} + \gamma X_{bpc} + \alpha_p + \eta_c + \delta_t + e_{bpc}.$$

In this regression, all of the variables contain individual data points from data consisting of means calculated for birth cohort b, in each province p, and each Census extract c. The dependent variable changes along two different dimensions: first, the maximal age of women in our sample (denoted above as "A") begins at 40, and is decreased by one year at a time until the maximal age is 27. The second dimension on which the dependent variable changes is the age by which women have given birth (denoted above as "B"), which ranges from 16 to 23. The other control variables in the regression (X_{bpc}) include: age as well as its square, cube and quartic; controls for rural status, the percent employed in manufacturing, married status, aboriginal status, and immigrant status; and fixed effects for province of birth, year of birth and the census extract. Furthermore, since we are concerned about potential within-province correlation of the errors, the regression's standard errors were calculated using a Wild cluster bootstrap procedure – since Canada has only ten provinces, Cameron et. al. (2008, 2015) suggest that this is the appropriate clustering approach.

For brevity, Table 2 only presents the coefficient on education from the regressions for all of the dependent variables described above. For visual ease, positive coefficients have cells with a red background, and negative coefficients have cells with a blue background. Statistically insignificant coefficients have pale colours, and statistically significant coefficients have backgrounds that contain darker colours. The coefficients are listed above their p-values, which are displayed in brackets.

Overall, the results in Table 2 are somewhat inconclusive. Although the majority of the coefficients on education are negative, few are statistically significant, except for the samples with the highest maximal age. However, as discussed before, these samples are more prone to misclassification bias, so the most instructive findings are listed for the samples whose maximal age is in the mid- to low-thirties. In these samples, there is no clear impact of education on the proportion of mothers who gave birth at a young age, although there are some coefficients that are negative and significant at the ten percent level of significance.

However, these regressions are descriptive, and can't have a causal interpretation, given the clear endogeneity of the education variable. Instead, it is necessary to find an approach that will purge the model of its endogeneity, and we have opted for a two-stage least squares approach that relies upon compulsory schooling laws as an instrument for educational attainment. To begin, we estimate the first stage of the model:

$$EDUC_{bpc} = \lambda CSL_{bp} + \rho X_{bp} + \theta_p + \mu_b + \psi_c + \nu_{bpc}$$

Once again, p represents province, b represents cohort of birth and c represents Census year. $\mathbf{EDUC}_{bpt}$ again represents the average years of education of women from a particular province, p, born in a particular birth cohort, b, in a particular Census year, c. $\mathbf{CSL}_{bp}$ represents our compulsory schooling law instruments which are based on province-determined minimum school leaving ages and are specific to particular birth cohorts, $\mathbf{X}_{bp}$ represents provincial level controls and $\boldsymbol{\theta}_p$, $\boldsymbol{\mu}_b$ and $\boldsymbol{\psi}_c$ represent province, birth cohort and Census year fixed effects, respectively, while $\boldsymbol{\nu}_{bpc}$ is the error term. Since this specification includes province and time related fixed effects, the coefficients on the CSL variables (λ) are identified by both variation in CSLs across provinces as well as variation within-province over time. We specify the CSL instrument as a dummy variable equal to one if the birth cohort may only drop out of the province's educational system once they are either 15 or 16 years of age, and zero otherwise.[13] Again, due to the relatively small number of clusters in our data, the Wild bootstrap clustering procedure was necessary for the two-stage least-squares estimation approach, and this has been discussed by Davidson and MacKinnon (2010).

Table 3 reports the main findings from the first stage, which show that the instrument has acceptably large F-statistics and low p-values for samples whose maximal age is between 30 and 40. This suggests that these samples do not suffer from a weak instrument problem within this framework.[14] This is a substantive improvement over the existing literature, which was unable to find a workable first stage with American and Norwegian data. Furthermore, our estimates are similar to other papers that have used the Canadian Census to explore the relationship between compulsory schooling laws and educational attainment.[15]

Our second stage results are presented in Table 4, and the results are quite different from the OLS findings. Looking down the columns, education has no significant effect on having had a child by the age of 16, but there is a significant negative effect of having a child by 17 or 18. Furthermore, beyond this point – having a child by 19 or later – the negative impact of education disappears, as most of the coefficients in these columns are statistically insignificant, except for the samples with the oldest maximal ages.

[13] Information on the specific timing for changes in these laws is presented in Appendix Table 1.
[14] As before, the standard errors and related p-values for the first stage were determined using a Wild bootstrap clustering procedure.
[15] Specifically, our estimates are similar to those presented by Oreopoulos (2007, 2008).

This suggests that education does not have a permanent, causal impact on fertility patterns for young women. Although raising the drop-out age does effectively compel students to obtain more education, a significant causal impact of this change is only evident one or two years after the students are permitted to leave school. We interpret these results are being supportive of an incarceration effect of education. If young women were postponing their fertility in order to maximize the return to their increased education, then the IV results would be significantly negative well after they had exited school. Instead, the results suggest that there is – at best – two years during which fertility is lower for compliers, and then this effect expires. This is consistent with an incarceration effect where compliers are unable to have children while in school due, but are seeking to have children almost as soon as they are able to leave school. In the case where the law permits a woman to leave school at the age of 14, given the time necessary to find a mate as well as a gestation period of 9 months, women intent on having children relatively soon after they leave school would report doing so by the age of 16 or 17. By comparison, compliers who are also seeking to begin a family as quickly as possible once they leave school at the age of 16 would be less likely to have children by the age of 17 or 18, but equally likely to have had a child by the age of 19 or 20. This is precisely what the two-stage least squares results show us.

As a further test of this model, we seek to ensure that our estimates are not prone to the same problems identified by Stephens and Yang (2014). The authors found that the IV return to education was not significantly different than zero when controls such as school quality specific to the state and year were included in the IV framework. To that end, we include three variables to account for school quality within each province and birth cohort: the annual per-capita amount of money spent by the provincial government on education, the annual per-capita number of schools in the province, and the annual per-capita number of teachers in the province. With these controls included amongst our X_{bpc} variables, we re-estimate the first and second stages of the model. Table 5 reports the new OLS results, and it has many of the same aspects of the results presented in Table 2, except that the impact of education on fertility at later ages is now stronger. Indeed, many of the coefficients in the more rightward part of the table are now statistically significant as well as negative, and this is more consistent with the general notions conveyed by Tables 1A to 1E: that the impact of education is negative on fertility patterns.

However, when the IV approach is applied to this framework, different findings are evident. Table 6 shows that the first stage still has a strong relationship between changes in compulsory schooling laws and educational attainment, with our F-statistics and related p-values being of sufficient magnitude to suggest that the results are not hindered by weak instruments. Furthermore, Table 7 shows the same overall pattern that was evident in Table 4: that the causal effect of education on fertility is negative only for women giving birth at a young age. There is a statistically significant and negative impact of education on the proportion of women who give birth at the age of 16, 17 or 18, but beyond these ages, education's effect is insignificant.

Lastly, we replicate the analysis again by not only including the three aforementioned measures of school quality, but also a fourth variable to account for overall annual provincial public expenditures per capita. If it is the case that changes in spending on government initiatives such as income support programs are contemporaneous with changes in compulsory schooling laws, then this could contaminate our results. However, the results in Tables 8 through 10 demonstrate that this is not the case, and echo the findings in Tables 5 through 7. Table 8 shows that the OLS impact of education is now more negative and significant after including overall annual provincial public expenditures per capita along with the three school quality controls, but this does not undermine the first stage (reported in Table 9) and it does not alter the main findings in the second stage, reported in Table 10. The IV effect of education is transitory: after the age of 18 in some samples, and after 17 as the maximal age of the sample is lowered.

5. Conclusion

The childbearing tendencies of young women have long been a matter of policy concern, and although the literature has explored different factors that might impact this outcome, there has not yet been a documentation of the *causal* effect of education on the fertility of young women. Broadly, there are two theories which have different predictions for the way in which education could impact childbearing for young women. First, "time-use" theories emphasize the "incarceration effect" of education, and suggest that spending more time in school leaves less time for other activities, and hence decreases the likelihood of childbearing. As emphasized in this paper, this theory has strong predictions about the timing of fertility decisions as compulsory schooling laws change: when more schooling is required, fertility decisions are temporarily postponed, but not permanently decreased. Second, "human capital" theories argue that increased productivity gained through additional schooling makes it more expensive to leave the labor market to bear and raise a child. This substitution effect would have a longer-term impact for decreasing fertility rates.

The results in this paper demonstrate that there is strong evidence increased educational attainment causes fertility to decline around the margin where students may first leave school, but not far beyond this point. Furthermore, this effect is robust to different specifications of our model, which is critical, given the recent critique of Stephens and Yang (2014) on the compulsory schooling law literature. Overall, we argue that the transitory negative effect of education is consistent with an incarceration effect, but is more difficult to reconcile with theories which argue in favour of human capital effects influencing the fertility patterns of young women. Given our results, we can only detect a causal, negative impact of education on fertility for women giving birth at the age of 17 or (possibly) 18, but no effect beyond this point. This is consistent with compliers in our data seeking to bear children almost as soon as they are permitted to leave school.

That said, the magnitude of our findings at these ages represent substantial reductions in teen fertility. Our main estimates imply that an extra year of schooling leads to a four to eight percentage point reduction in fertility. To the extent that they reflect causality, our findings imply that policies which seek to increase educational attainment in the lower tail of the education distribution may reduce teen fertility. In turn, if reduced teen childbearing, results in better life outcomes, our findings point to a role for educational policy in improving the life chances of lower socioeconomic status women.

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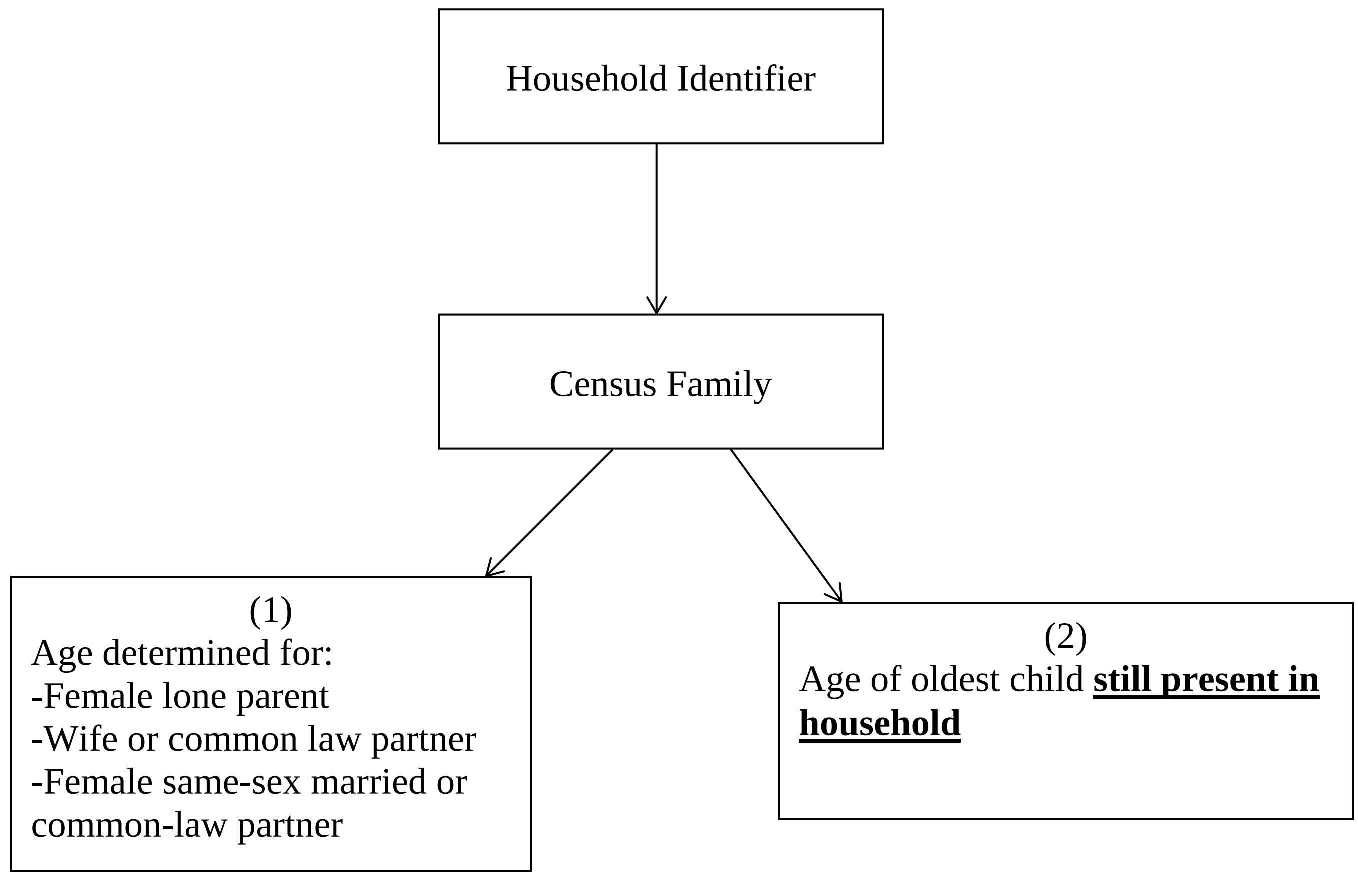
Figure 1: Schematic for Identifying Women Who Were Teenagers When They Gave Birth



The differential between the age of the woman identified in box (1) and the child identified in box (2) determines the age at which the woman in box (1) gave birth to her eldest child.

Table 1A: Characteristics of Mothers Who Did or Did not Have Children as Teenagers from the 1971 Canadian Census

	1971 Census					
	Age 30 or less		Age 35 or less		Age 40 or less	
	Teen Birth	No Teen Birth	Teen Birth	No Teen Birth	Teen Birth	No Teen Birth
Education	9.56	11.43	9.46	11.18	9.38	10.90
	(2.22)	(2.85)	(2.30)	(2.99)	(2.35)	(3.08)
Age	24.22	25.25	26.39	27.59	27.67	30.15
	(3.66)	(3.13)	(4.96)	(4.48)	(5.88)	(5.99)
Earnings	757	2095	835	1895	881	1781
	(1555)	(2508)	(1652)	(2524)	(1714)	(2529)
Married	0.929	0.884	0.927	0.893	0.926	0.897
	(0.257)	(0.320)	(0.260)	(0.309)	(0.261)	(0.304)
Weeks Worked	12.18	23.56	13.32	21.56	13.95	20.70
	(18.69)	(22.42)	(19.54)	(22.44)	(19.96)	(22.46)

Table 1B: Characteristics of Mothers Who Did or Did not Have Children as Teenagers from the 1976 Canadian Census

	1971 Census					
	Age 30 or less		Age 35 or less		Age 40 or less	
	Teen Birth	No Teen Birth	Teen Birth	No Teen Birth	Teen Birth	No Teen Birth
Education	10.14	12.11	10.05	11.94	9.98	11.69
	(2.23)	(2.75)	(2.33)	(2.93)	(2.39)	(3.07)
Age	24.18	25.40	26.47	27.72	27.75	29.97
	(3.73)	(3.21)	(5.06)	(4.45)	(5.94)	(5.84)
Married	0.873	0.964	0.874	0.956	0.877	0.948
	(0.333)	(0.185)	(0.331)	(0.206)	(0.329)	(0.222)

Table 1C: Characteristics of Mothers Who Did or Did not Have Children as Teenagers from the 1981 Canadian Census

	1971 Census					
	Age 30 or less		Age 35 or less		Age 40 or less	
	Teen Birth	No Teen Birth	Teen Birth	No Teen Birth	Teen Birth	No Teen Birth
Education	10.57 (2.22)	12.50 (2.44)	10.50 (2.37)	12.43 (2.68)	10.43 (2.46)	12.25 (2.85)
Age	24.56 (3.57)	25.56 (3.14)	27.01 (4.91)	28.18 (4.43)	28.45 (5.85)	30.46 (5.71)
Earnings	3385 (5057)	6572 (6578)	3988 (5616)	6457 (7009)	4240 (5798)	6438 (7232)
Married	0.826 (0.380)	0.955 (0.209)	0.834 (0.373)	0.944 (0.231)	0.838 (0.369)	0.934 (0.248)
Weeks Worked	18.67 (20.79)	29.30 (21.51)	21.05 (21.59)	28.05 (22.01)	22.09 (21.88)	27.84 (22.21)

Table 1D: Characteristics of Mothers Who Did or Did not Have Children as Teenagers from the 1986 Canadian Census

	1971 Census					
	Age 30 or less		Age 35 or less		Age 40 or less	
	Teen Birth	No Teen Birth	Teen Birth	No Teen Birth	Teen Birth	No Teen Birth
Education	10.76 (2.18)	12.73 (2.47)	10.78 (2.30)	12.78 (2.64)	10.71 (2.40)	12.67 (2.81)
Age	25.12 (3.48)	26.02 (2.93)	27.70 (4.71)	28.69 (4.18)	29.43 (5.74)	31.26 (5.52)
Earnings	4675 (6995)	9427 (9458)	5770 (8059)	9730 (10280)	6388 (8605)	9998 (10856)
Married	0.774 (0.419)	0.939 (0.238)	0.790 (0.407)	0.929 (0.257)	0.800 (0.400)	0.917 (0.276)
Weeks Worked	19.63 (21.07)	31.16 (21.13)	22.44 (21.85)	30.56 (21.62)	23.96 (22.16)	30.65 (21.84)

Table 1E: Characteristics of Mothers Who Did or Did not Have Children as Teenagers from the 1991 Canadian Census

	1971 Census					
	Age 30 or less		Age 35 or less		Age 40 or less	
	Teen Birth	No Teen Birth	Teen Birth	No Teen Birth	Teen Birth	No Teen Birth
Education	11.17	13.20	11.20	13.22	11.20	13.17
	(2.42)	(2.62)	(2.46)	(2.71)	(2.52)	(2.80)
Age	25.03	26.35	27.97	29.19	29.84	31.80
	(3.62)	(2.92)	(4.88)	(4.07)	(5.84)	(5.32)
Earnings	7017	13212	8485	13871	9510	14588
	(9358)	(12326)	(10517)	(13576)	(11407)	(14565)
Married	0.596	0.692	0.657	0.750	0.687	0.777
	(0.491)	(0.462)	(0.475)	(0.433)	(0.464)	(0.417)
Weeks Worked	22.42	33.69	25.53	33.62	27.40	34.16
	(21.47)	(20.31)	(21.92)	(20.70)	(22.02)	(20.76)

Table 2: OLS Impact of Education on Being a Young Mother (Wild Bootstrapped standard errors)

	Child by 16	Child by 17	Child by 18	Child by 19	Child by 20	Child by 21
Age ≤ 40	-0.000 [0.991]	-0.018 [0.106]	-0.026 [0.018]	-0.021 [0.062]	-0.016 [0.044]	-0.015 [0.014]
Age ≤ 39	0.001 [0.965]	-0.017 [0.148]	-0.025 [0.016]	-0.019 [0.068]	-0.013 [0.060]	-0.012 [0.106]
Age ≤ 38	0.002 [0.821]	-0.016 [0.148]	-0.023 [0.020]	-0.017 [0.072]	-0.010 [0.120]	-0.009 [0.260]
Age ≤ 37	0.003 [0.645]	-0.015 [0.184]	-0.022 [0.044]	-0.015 [0.088]	-0.008 [0.292]	-0.007 [0.472]
Age ≤ 36	0.004 [0.581]	-0.014 [0.228]	-0.021 [0.070]	-0.013 [0.122]	-0.006 [0.462]	-0.005 [0.567]
Age ≤ 35	0.005 [0.587]	-0.012 [0.226]	-0.019 [0.092]	-0.011 [0.200]	-0.004 [0.663]	-0.004 [0.721]
Age ≤ 34	0.005 [0.639]	-0.012 [0.318]	-0.017 [0.110]	-0.009 [0.220]	-0.004 [0.747]	-0.004 [0.757]
Age ≤ 33	0.005 [0.639]	-0.011 [0.252]	-0.016 [0.118]	-0.009 [0.312]	-0.003 [0.787]	-0.004 [0.791]
Age ≤ 32	0.006 [0.569]	-0.010 [0.366]	-0.016 [0.170]	-0.008 [0.362]	-0.003 [0.817]	-0.003 [0.791]
Age ≤ 31	0.006 [0.505]	-0.009 [0.374]	-0.015 [0.178]	-0.007 [0.420]	-0.002 [0.883]	-0.003 [0.905]
Age ≤ 30	0.006 [0.609]	-0.009 [0.434]	-0.014 [0.214]	-0.006 [0.478]	-0.002 [0.855]	-0.005 [0.731]
Age ≤ 29	0.006 [0.737]	-0.008 [0.565]	-0.012 [0.336]	-0.005 [0.478]	-0.004 [0.569]	-0.007 [0.354]
Age ≤ 28	0.006 [0.801]	-0.007 [0.683]	-0.011 [0.420]	-0.006 [0.402]	-0.005 [0.418]	-0.010 [0.400]
Age ≤ 27	0.006 [0.751]	-0.005 [0.723]	-0.010 [0.414]	-0.007 [0.300]	-0.008 [0.344]	-0.013 [0.256]

Coefficients are listed in each cell with p-values in brackets underneath. Controls include: (i) dummies for: year at which the person is 14, province of birth, census extract; (ii) age (and its square, cube and quartic), and indicators: rural status, married status, aboriginal status, immigrant status. Standard errors which underlie the p-values were clustered at the province level.

Table 3: First stage of Education on Dropout Age

Maximal Sample Age	Coefficient	F-statistic	p-value from Wild Bootstrap cluster
Age ≤ 40	0.646	9.93	0.0040
Age ≤ 39	0.654	10.64	0.0040
Age ≤ 38	0.665	11.52	0.0000
Age ≤ 37	0.680	12.77	0.0000
Age ≤ 36	0.696	13.68	0.0000
Age ≤ 35	0.696	13.46	0.0000
Age ≤ 34	0.705	12.87	0.0000
Age ≤ 33	0.732	14.15	0.0000
Age ≤ 32	0.775	16.86	0.0000
Age ≤ 31	0.811	16.04	0.0000
Age ≤ 30	0.798	12.10	0.0000
Age ≤ 29	0.771	8.31	0.0100
Age ≤ 28	0.744	7.66	0.0280
Age ≤ 27	0.699	6.58	0.0240

Controls are the same as in Table 2, but also include an indicator equal to one if the drop-out age is 15 or higher, and zero if it is less

Table 4: IV Impact of Education on Being a Young Mother (Wild Bootstrapped standard errors)

	Child by 16	Child by 17	Child by 18	Child by 19	Child by 20	Child by 21
Age ≤ 40	-0.004 [0.326]	-0.025 [0.000]	-0.034 [0.000]	-0.024 [0.014]	-0.013 [0.174]	-0.005 [0.524]
Age ≤ 39	-0.004 [0.296]	-0.024 [0.000]	-0.033 [0.000]	-0.022 [0.042]	-0.009 [0.346]	-0.001 [0.798]
Age ≤ 38	-0.004 [0.366]	-0.024 [0.000]	-0.031 [0.000]	-0.018 [0.100]	-0.004 [0.614]	0.003 [0.702]
Age ≤ 37	-0.003 [0.468]	-0.023 [0.000]	-0.028 [0.006]	-0.014 [0.226]	0.000 [0.999]	0.006 [0.544]
Age ≤ 36	-0.003 [0.430]	-0.021 [0.002]	-0.025 [0.022]	-0.009 [0.422]	0.003 [0.752]	0.008 [0.464]
Age ≤ 35	-0.002 [0.578]	-0.020 [0.004]	-0.022 [0.052]	-0.005 [0.660]	0.007 [0.480]	0.011 [0.352]
Age ≤ 34	-0.001 [0.730]	-0.019 [0.012]	-0.019 [0.090]	-0.001 [0.896]	0.010 [0.356]	0.013 [0.312]
Age ≤ 33	-0.002 [0.698]	-0.020 [0.004]	-0.019 [0.080]	0.000 [0.910]	0.012 [0.306]	0.014 [0.264]
Age ≤ 32	-0.003 [0.504]	-0.020 [0.002]	-0.020 [0.058]	0.000 [0.938]	0.011 [0.248]	0.013 [0.246]
Age ≤ 31	-0.003 [0.304]	-0.021 [0.002]	-0.019 [0.078]	0.003 [0.732]	0.013 [0.158]	0.014 [0.162]
Age ≤ 30	-0.003 [0.328]	-0.021 [0.018]	-0.017 [0.128]	0.005 [0.374]	0.017 [0.084]	0.016 [0.148]
Age ≤ 29	-0.001 [0.540]	-0.020 [0.056]	-0.016 [0.208]	0.007 [0.530]	0.018 [0.104]	0.017 [0.236]
Age ≤ 28	-0.005 [0.308]	-0.021 [0.074]	-0.015 [0.170]	0.007 [0.572]	0.020 [0.144]	0.016 [0.400]
Age ≤ 27	-0.009 [0.096]	-0.020 [0.068]	-0.016 [0.274]	0.006 [0.642]	0.018 [0.212]	0.015 [0.430]

Coefficients are listed in each cell with p-values in brackets underneath. Controls include: (i) dummies for: year at which the person is 14, province of birth, census extract; (ii) age (and its square, cube and quartic), and indicators: rural status, married status, aboriginal status, immigrant status.

Table 5: OLS Impact of Education on Being a Young Mother, including School Quality Measures

	Child by 16	Child by 17	Child by 18	Child by 19	Child by 20	Child by 21
Age ≤ 40	-0.003 [0.863]	-0.021 [0.152]	-0.031 [0.016]	-0.029 [0.002]	-0.028 [0.002]	-0.031 [0.002]
Age ≤ 39	-0.002 [0.949]	-0.019 [0.174]	-0.031 [0.014]	-0.028 [0.002]	-0.027 [0.002]	-0.030 [0.002]
Age ≤ 38	-0.000 [0.977]	-0.019 [0.210]	-0.030 [0.004]	-0.028 [0.002]	-0.027 [0.002]	-0.030 [0.002]
Age ≤ 37	0.001 [0.959]	-0.018 [0.174]	-0.030 [0.002]	-0.029 [0.002]	-0.027 [0.002]	-0.029 [0.002]
Age ≤ 36	0.002 [0.915]	-0.018 [0.206]	-0.030 [0.004]	-0.029 [0.002]	-0.027 [0.006]	-0.030 [0.014]
Age ≤ 35	0.003 [0.891]	-0.016 [0.242]	-0.029 [0.020]	-0.028 [0.002]	-0.026 [0.002]	-0.030 [0.006]
Age ≤ 34	0.003 [0.871]	-0.016 [0.228]	-0.029 [0.038]	-0.028 [0.002]	-0.028 [0.002]	-0.032 [0.008]
Age ≤ 33	0.003 [0.891]	-0.016 [0.316]	-0.029 [0.030]	-0.029 [0.012]	-0.030 [0.006]	-0.033 [0.014]
Age ≤ 32	0.003 [0.895]	-0.015 [0.414]	-0.030 [0.098]	-0.030 [0.024]	-0.031 [0.028]	-0.035 [0.022]
Age ≤ 31	0.004 [0.897]	-0.015 [0.384]	-0.030 [0.106]	-0.030 [0.024]	-0.031 [0.054]	-0.036 [0.014]
Age ≤ 30	0.003 [0.907]	-0.015 [0.484]	-0.030 [0.160]	-0.030 [0.044]	-0.032 [0.036]	-0.039 [0.024]
Age ≤ 29	0.002 [0.977]	-0.015 [0.595]	-0.029 [0.206]	-0.031 [0.062]	-0.037 [0.038]	-0.045 [0.036]
Age ≤ 28	0.001 [0.977]	-0.015 [0.585]	-0.029 [0.184]	-0.034 [0.084]	-0.041 [0.020]	-0.050 [0.034]
Age ≤ 27	-0.000 [0.997]	-0.014 [0.611]	-0.029 [0.192]	-0.036 [0.092]	-0.045 [0.020]	-0.054 [0.012]

Coefficients are listed in each cell with p-values in brackets underneath. Controls include: (i) dummies for: year at which the person is 14, province of birth, census extract; (ii) age (and its square, cube and quartic), and indicators: rural status, married status, aboriginal status, immigrant status; (iii) provincial-level controls about: schools per capita, teachers per capita, and school expenditures per capita.

Table 6: First stage of Education on Dropout Age, Including School Quality Measures

Maximal Sample Age	Coefficient	F-statistic	p-value from Wild Bootstrap cluster
Age ≤ 40	0.345	13.16	0.0000
Age ≤ 39	0.353	13.49	0.0000
Age ≤ 38	0.365	14.23	0.0000
Age ≤ 37	0.383	15.06	0.0000
Age ≤ 36	0.402	14.74	0.0000
Age ≤ 35	0.400	13.14	0.0000
Age ≤ 34	0.409	11.63	0.0000
Age ≤ 33	0.431	13.09	0.0000
Age ≤ 32	0.474	15.43	0.0000
Age ≤ 31	0.519	11.05	0.0000
Age ≤ 30	0.500	8.15	0.0120
Age ≤ 29	0.473	5.43	0.0460
Age ≤ 28	0.441	5.06	0.0739
Age ≤ 27	0.389	3.86	0.0879

Controls are the same as in Table 5, but also include an indicator equal to one if the drop-out age is 15 or higher, and zero if it is less

Table 7: IV Impact of Education on Being a Young Mother, including School Quality

	Child by 16	Child by 17	Child by 18	Child by 19	Child by 20	Child by 21
Age ≤ 40	-0.014 [0.058]	-0.037 [0.000]	-0.047 [0.000]	-0.033 [0.000]	-0.021 [0.000]	-0.018 [0.002]
Age ≤ 39	-0.014 [0.058]	-0.037 [0.000]	-0.048 [0.000]	-0.035 [0.000]	-0.024 [0.000]	-0.021 [0.006]
Age ≤ 38	-0.014 [0.064]	-0.038 [0.000]	-0.049 [0.000]	-0.035 [0.000]	-0.023 [0.014]	-0.020 [0.140]
Age ≤ 37	-0.013 [0.074]	-0.036 [0.000]	-0.046 [0.000]	-0.032 [0.004]	-0.020 [0.118]	-0.019 [0.230]
Age ≤ 36	-0.013 [0.074]	-0.034 [0.000]	-0.042 [0.000]	-0.026 [0.064]	-0.018 [0.150]	-0.017 [0.238]
Age ≤ 35	-0.012 [0.070]	-0.034 [0.000]	-0.039 [0.000]	-0.022 [0.134]	-0.013 [0.316]	-0.015 [0.278]
Age ≤ 34	-0.011 [0.060]	-0.032 [0.000]	-0.036 [0.002]	-0.016 [0.252]	-0.010 [0.478]	-0.012 [0.446]
Age ≤ 33	-0.012 [0.034]	-0.035 [0.000]	-0.038 [0.000]	-0.017 [0.186]	-0.009 [0.518]	-0.011 [0.494]
Age ≤ 32	-0.013 [0.002]	-0.036 [0.000]	-0.040 [0.000]	-0.019 [0.088]	-0.010 [0.466]	-0.011 [0.468]
Age ≤ 31	-0.014 [0.000]	-0.037 [0.000]	-0.038 [0.000]	-0.015 [0.200]	-0.007 [0.556]	-0.008 [0.558]
Age ≤ 30	-0.016 [0.000]	-0.038 [0.000]	-0.038 [0.000]	-0.013 [0.326]	-0.003 [0.848]	-0.006 [0.680]
Age ≤ 29	-0.016 [0.004]	-0.039 [0.002]	-0.037 [0.002]	-0.011 [0.442]	-0.003 [0.852]	-0.007 [0.710]
Age ≤ 28	-0.023 [0.002]	-0.046 [0.000]	-0.043 [0.000]	-0.017 [0.304]	-0.006 [0.742]	-0.013 [0.658]
Age ≤ 27	-0.049 [0.000]	-0.044 [0.004]	-0.042 [0.004]	-0.019 [0.286]	-0.014 [0.486]	-0.019 [0.516]

Coefficients are listed in each cell with p-values in brackets underneath. Controls include: (i) dummies for: year at which the person is 14, province of birth, census extract; (ii) age (and its square, cube and quartic), and indicators: rural status, married status, aboriginal status, immigrant status; (iii) provincial-level controls about: schools per capita, teachers per capita, and school expenditures per capita.

Table 8: OLS Impact of Education on Being a Young Mother, Including School Quality Controls and Overall Provincial Program Spending

	Child by 16	Child by 17	Child by 18	Child by 19	Child by 20	Child by 21
Age ≤ 40	-0.006 [0.460]	-0.024 [0.060]	-0.035 [0.002]	-0.031 [0.002]	-0.029 [0.004]	-0.032 [0.002]
Age ≤ 39	-0.005 [0.589]	-0.023 [0.082]	-0.034 [0.002]	-0.031 [0.002]	-0.029 [0.008]	-0.032 [0.002]
Age ≤ 38	-0.004 [0.717]	-0.022 [0.088]	-0.034 [0.004]	-0.031 [0.002]	-0.028 [0.008]	-0.031 [0.004]
Age ≤ 37	-0.002 [0.881]	-0.021 [0.114]	-0.034 [0.002]	-0.031 [0.002]	-0.028 [0.002]	-0.031 [0.008]
Age ≤ 36	-0.001 [0.969]	-0.021 [0.142]	-0.033 [0.004]	-0.031 [0.002]	-0.028 [0.018]	-0.031 [0.016]
Age ≤ 35	-0.001 [0.987]	-0.019 [0.156]	-0.032 [0.012]	-0.030 [0.002]	-0.027 [0.004]	-0.032 [0.010]
Age ≤ 34	-0.001 [0.999]	-0.019 [0.220]	-0.032 [0.020]	-0.030 [0.002]	-0.029 [0.004]	-0.033 [0.012]
Age ≤ 33	-0.001 [0.995]	-0.019 [0.266]	-0.032 [0.038]	-0.032 [0.006]	-0.031 [0.022]	-0.034 [0.010]
Age ≤ 32	-0.001 [0.941]	-0.018 [0.232]	-0.034 [0.034]	-0.033 [0.036]	-0.033 [0.046]	-0.035 [0.036]
Age ≤ 31	0.001 [0.957]	-0.018 [0.318]	-0.034 [0.042]	-0.033 [0.046]	-0.032 [0.064]	-0.036 [0.052]
Age ≤ 30	0.000 [0.967]	-0.017 [0.380]	-0.033 [0.116]	-0.033 [0.060]	-0.032 [0.052]	-0.040 [0.036]
Age ≤ 29	-0.001 [0.955]	-0.018 [0.464]	-0.033 [0.152]	-0.034 [0.088]	-0.038 [0.070]	-0.046 [0.034]
Age ≤ 28	-0.002 [0.991]	-0.017 [0.472]	-0.032 [0.170]	-0.037 [0.088]	-0.043 [0.042]	-0.052 [0.012]
Age ≤ 27	-0.003 [0.979]	-0.016 [0.627]	-0.033 [0.238]	-0.040 [0.072]	-0.047 [0.024]	-0.056 [0.020]

Coefficients are listed in each cell with p-values in brackets underneath. Controls are the same as in Table 5, but also include the annual provincial level of spending on public programs.

Table 9: First stage of Education on Dropout Age, Including School Quality Measures and Provincial Program Spending

	Coefficient	F-statistic	p-value from Wild Bootstrap cluster
Age ≤ 40	0.309	11.47	0.0000
Age ≤ 39	0.318	12.53	0.0000
Age ≤ 38	0.329	13.23	0.0000
Age ≤ 37	0.345	14.00	0.0000
Age ≤ 36	0.361	13.96	0.0000
Age ≤ 35	0.361	12.81	0.0000
Age ≤ 34	0.373	11.76	0.0000
Age ≤ 33	0.394	13.62	0.0000
Age ≤ 32	0.435	17.65	0.0000
Age ≤ 31	0.478	12.34	0.0000
Age ≤ 30	0.462	8.94	0.0120
Age ≤ 29	0.440	5.81	0.0320
Age ≤ 28	0.410	5.53	0.0160
Age ≤ 27	0.375	4.73	0.0540

Controls are the same as in Table 8, but also include an indicator equal to one if the drop-out age is 15 or higher, and zero if it is less.

Table 10: IV Impact of Education on Being a Young Mother (Wild Bootstrapped standard errors)

	Child by 16	Child by 17	Child by 18	Child by 19	Child by 20	Child by 21
Age ≤ 40	-0.019 [0.022]	-0.042 [0.000]	-0.052 [0.000]	-0.036 [0.000]	-0.022 [0.000]	-0.018 [0.000]
Age ≤ 39	-0.019 [0.018]	-0.042 [0.000]	-0.053 [0.000]	-0.038 [0.000]	-0.024 [0.000]	-0.021 [0.000]
Age ≤ 38	-0.018 [0.022]	-0.043 [0.000]	-0.054 [0.000]	-0.038 [0.000]	-0.024 [0.000]	-0.021 [0.054]
Age ≤ 37	-0.018 [0.012]	-0.041 [0.000]	-0.051 [0.000]	-0.034 [0.002]	-0.021 [0.092]	-0.019 [0.166]
Age ≤ 36	-0.018 [0.000]	-0.039 [0.000]	-0.046 [0.000]	-0.029 [0.022]	-0.018 [0.130]	-0.017 [0.200]
Age ≤ 35	-0.017 [0.006]	-0.038 [0.000]	-0.043 [0.000]	-0.023 [0.116]	-0.013 [0.292]	-0.014 [0.272]
Age ≤ 34	-0.015 [0.014]	-0.036 [0.000]	-0.039 [0.001]	-0.017 [0.234]	-0.009 [0.506]	-0.011 [0.460]
Age ≤ 33	-0.015 [0.014]	-0.038 [0.000]	-0.041 [0.000]	-0.018 [0.150]	-0.009 [0.524]	-0.009 [0.514]
Age ≤ 32	-0.017 [0.000]	-0.040 [0.000]	-0.044 [0.000]	-0.020 [0.040]	-0.009 [0.474]	-0.009 [0.490]
Age ≤ 31	-0.017 [0.000]	-0.040 [0.000]	-0.042 [0.000]	-0.016 [0.124]	-0.006 [0.624]	-0.006 [0.640]
Age ≤ 30	-0.018 [0.000]	-0.041 [0.000]	-0.041 [0.000]	-0.014 [0.272]	-0.001 [0.934]	-0.004 [0.770]
Age ≤ 29	-0.020 [0.000]	-0.042 [0.001]	-0.039 [0.004]	-0.010 [0.448]	-0.000 [0.986]	-0.004 [0.832]
Age ≤ 28	-0.021 [0.002]	-0.049 [0.000]	-0.050 [0.000]	-0.021 [0.234]	-0.005 [0.782]	-0.010 [0.746]
Age ≤ 27	-0.024 [0.020]	-0.059 [0.000]	-0.064 [0.000]	-0.032 [0.098]	-0.014 [0.578]	-0.015 [0.682]

Coefficients are listed in each cell with p-values in brackets underneath. Controls include: (i) dummies for: year at which the person is 14, province of birth, census extract; (ii) age (and its square, cube and quartic), and indicators: rural status, married status, aboriginal status, immigrant status; (iii) provincial-level controls about: schools per capita, teachers per capita, and school expenditures per capita.

Appendix Table 1: Variation in the instrument

	Initial Dropout Age	Dropout Age = 14	Dropout Age = 15	Dropout Age = 16
Newfoundland	None Until 1941	1942-1951	1952-1986	1987-2000
PEI	13 Until 1937	...	1938-1979	1980-2000
Nova Scotia	12 Until 1933	…	…	1933-2000
New Brunswick	None until 1912	1913-1945	…	1946-1998 (18 thereafter)
Quebec	None Until 1942	1943-1960	1961-1987	1988-2000
Ontario		1900-1953		1954-2000
Manitoba	None Until 1906 Then 12 until 1913	1914-1962		1963-2000
Saskatchewan	None Until 1908	1909-1921	1922-1963	1964-2000
Alberta	None Until 1909	1910-1922	1922-1968	1969-2000
British Columbia	12 Until 1904	1905-1921	1922-1988	1989-2000