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THE EFFECT OF EDUCATION ON OVERALL FERTILITY

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ABSTRACT

Fertility rates have long been falling in many developed countries while educational attainment in these countries has risen. We attempt to reconcile these two trends with a novel application of a recent model to generate plausibly causal effects of education on these decreases in fertility. Specifically, we find that education "compresses" the fertility distribution – women are more likely to have at least one child, but less likely to have multiple children. We demonstrate that the mechanism for this effect is through the positive impact of education on earnings and marriage.

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I. Introduction

Both birth rates and female educational attainment have changed dramatically over the past half-century. In many developed countries between 1960 and 2010, total fertility rates have fallen by one-third to over one-half.[1] Over the same period, female educational attainment has significantly increased. In the United States, for example, the fraction of women aged 25-29 with at least a bachelor's degree increased threefold—from roughly 12 percent to about 35 percent. In addition to these trends, there are large differences in total fertility across women of different education levels. Fertility among American women aged 40 to 50 years old in 2012 varied substantially by educational attainment—from about 2.6 for those with less than a HS degree to about 1.8 for women with a bachelor's degree and 1.7 for those with a graduate or professional degree. Moreover, while just less than 12 percent of those with less than a high school degree were childless, nearly one-quarter of women with a graduate or professional degree had no children when surveyed. Such descriptive evidence, coupled with conceptually plausible mechanisms, suggests there may be a causal relationship between female education and lifetime fertility.

Despite these suggestive trends, and a long standing interest by economists in the impact of schooling on fertility, there exist relatively few studies that credibly estimate causal relationships, particularly with data from the developed world. In this paper, we attempt to estimate the causal impact of education on lifetime fertility, which we define as the number of children a woman gives birth to by age forty. After reviewing the most relevant literature, we present a model of fertility, based on a standard child quantity-quality framework, which

[1] In the United States, total fertility decreased from about 3.5 children per woman in 1960 to roughly 2.0 children in 2010, while Canada saw a decline in total fertility from 3.9 children in 1960 to 1.6 in 2010. In England and Wales, the comparable rates are 2.7 in 1960 and 1.9 in 2010; in Ireland, 3.8 in 1960 and 2.1 in 2010; in Finland, 2.7 in 1960 and 1.9 in 2010; in the Netherlands, 3.1 in 1960 and 1.8 in 2010; in Italy, 2.4 in 1960 and 1.5 in 2010; in France, 2.9 in 1960 and 2.0 in 2010; in Germany, 2.4 in 1960 and 1.4 in 2010.

demonstrates that education may affect fertility differentially on extensive and intensive margins. In other words, the model shows that education may have different impacts on the probability a woman has any children (i.e., the extensive margin) and the number of children she has, conditional on having children (i.e., the intensive margin) that work through changes in the price of child quantity and child quality.

We test this model using multiple waves of the Canadian Census. More precisely, we use quasi-experimental variation in compulsory schooling laws (CSLs) to identify the causal impact of education on each fertility margin separately. We find evidence that education "compresses" fertility. That is, we find an extra year of CSL-induced schooling decreases the number of children a given woman has, but increases the probability that a woman has any children. We also find that additional CSL-induced schooling leads to a greater likelihood of marriage as well as higher earnings for women. While the latter are interesting findings independent of their connection to lifetime fertility, we argue they are consistent with the compressive fertility pattern we observe. In particular, increased marriage should reduce the price of child quality which, in turn, should decrease the likelihood of childlessness, while reducing the number of children along the intensive margin.

Our estimates are robust along many dimensions including several different parameterizations of our compulsory schooling instrument, as well as robust to the inclusion of school quality measures and region-specific trends that address a recent critique of the compulsory schooling literature by Stephens and Yang (2014). Our results contribute to a small, but growing literature that uses quasi-experimental variation in education to examine important socioeconomic outcomes like fertility.

In what follows, we provide background information on Canadian compulsory schooling laws, discuss related studies and the present our theoretical model which draws substantially on Galor (2012) and Aaronson, Lange and Mazumder (2015). In Section 3, we describe our data, focusing on key definitions and also the relevant history of minimum school leaving ages which provide the variation which we use to identify the impact of schooling on lifetime fertility. Section 4 presents our empirical strategy which involves instrumental variables estimation. We also discuss important issues regarding appropriate variance estimation when there are few sources of independent variation: since minimum school leaving ages are province-specific and since there are only ten Canadian provinces we implement the Wild Cluster Bootstrap procedure described in Cameron and Miller (2015). Section 5 presents our findings. As noted, we find that additional schooling has a "compressive" effect on lifetime fertility, reducing the likelihood of childlessness while reducing the number of children a woman has conditional on having any. Section 6 discusses our findings and their implications and concludes the paper.

II. Background

A. Canadian Compulsory Schooling Laws

Compulsory schooling laws have existed in North America for well over a century. As detailed below, their historical development in Canada and the United States is quite similar. Other key similarities between the Canadian and U.S. educational systems include education being a function of state/provincial governments that is delivered by local governments as well as 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 Section 3.

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. These laws, however, became more binding over time as the list of exemptions narrowed. 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.[2] 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 some exemptions remained. For example, children under ten were 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.[3] 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). More recently, some Canadian provinces have further increased the age of compulsory attendance: New Brunswick raised it to eighteen in 2000, as did Ontario in 2007 and Manitoba in 2011. Again, note that this

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[2] 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.

[3] 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. These young adults were still required to attend part-time instruction in the evenings, where such classes existed.

broad overview does not explicitly discuss the law changes we use in our analysis; instead we do this in Section 3.

B. Literature review

The literature on the relationship between family size and economic circumstances begins with the work of Becker (1960, 1965), which proposed an analysis of fertility based upon the view of children as durable goods, and documented a negative relationship between household income and the number of children produced within a household. Becker and Lewis (1973), Willis (1973) and Becker and Tomes (1976) further analyzed this phenomenon by proposing a fundamental trade-off between the quantity and quality of children produced within a family. Generally, models in these papers postulate that household utility, $U(n, w, y)$, is a function of three goods: n represents the number of children, w represents the quality of each child, and y is an aggregate of all other goods. The budget constraint within the model is: $p_y y + pnw = I$, where p_y represents the price of the aggregate good, p is the price of nw, and I represents income. The optimal choices in n and w reveal that there exists a trade-off between these two goods: an increase in n raises the shadow price of w, and vice versa.

The quantity-quality model has been empirically tested in a large set of papers, which can be broadly classified into two groups. The first group of papers relate changes in child quantity to parental investment in their children's education, which is deemed to improve child quality. Within this first group of papers, the evidence for a trade-off between quantity and quality has been mixed. One subset of these papers has found a negative relationship between child quality and quantity (c.f., Rosenzweig and Wolpin, 1980; Rosenzweig and Schultz, 1987; Hanushek, 1992; Caceres (2006); Lee (2008); Li, Zhang and Zhu, 2008); Rosenzweig and Wolpin, 2009), while a different subset finds less evidence for such a trade-off (c.f., Kessler, 1991; Black, Devereux and Salvanes, 2005; Angrist, Levy and Schlosser, 2010; Black, Devereux and Salvanes, 2010; Lucas, 2013; Qian, forthcoming).

In the second group of papers, which is more relevant to our work, the central empirical strategy for analyzing the quantity-quality trade-off hinged on factors that change the shadow prices of children (as opposed to the quantity of children). Many of these papers have used historical data, and unlike the first set of papers, exhibit a greater consensus on the presence of a quantity-quality trade-off. Becker et. al. (2010) use nineteenth-century Prussian data and use an IV strategy for educational attainment in order to create variation in the price of education (the price of quality) and fertility, and they demonstrate that there is a negative causal relationship in this regard. Schultz (1985) uses historical Swedish data to examine a plausible change in the relative value of women's time in the labor market due to a change in the price of the goods produced by women and men. His IV strategy shows that the increased valuation of labor market time reduces fertility, which is consistent with the quantity-quality model because an increase in the opportunity cost of women's time will increase the price of the quantity of children produced within the family.[4] Bleakley and Lange (2009) use the eradication of hookworm disease at the beginning of the twentieth century as a plausibly exogenous decrease in the price of quality of children. They argue that the eradication of hookworm raised the returns to education for children who could, absent this disease, obtain higher education more easily and improve the retention of training from education. As the quantity-quality tradeoff would predict, a decrease in the price of quality should induce greater consumption of quality by

[4] Becker et. al. (1990) develop a theoretical model to demonstrate that, in the context of economic growth and development, increased investment in educational attainment may lead to an equilibrium with smaller families – directly in accordance with the quantity-quality model.

parents; the resulting increase in the quality will raise the price of quantity and thus cause a decrease in overall fertility.

A paper that uses more recent data is a study by McCrary and Royer (2011), which examines the relationship between maternal education and fertility, in the context of a more prominent focus on child health. Using data from California and Texas, these authors implement a regression discontinuity strategy based on compulsory schooling laws (CSL). They find little evidence of a relationship between increases in CSL-induced education and fertility.[5] 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 those females who have given birth.

C. Model

The most relevant papers to this study are Galor (2012) and Aaronson, Lange and Mazumder (2014) (hereafter ALM). Galor (2012) provides the theoretical underpinning for ALM to analyze the construction of Rosenwald schools in the U.S. south at the beginning of the twentieth century. These two papers use a quantity-quality framework with a few small changes. Specifically, the household maximizes its preferences based upon the following utility function with three arguments subject to the budget constraint:

$$\max \quad U(c, n, e) \; s.t. \, n(\tau^q + \tau^e e) + c < I$$

In this case, $\boldsymbol{c}$ represents goods and services consumed, $\boldsymbol{n}$ represents the number of children, and $\boldsymbol{e}$ is the investment in the quality of the $\boldsymbol{n}$ children. The household can spend at most $\boldsymbol{I}$ in household income on $\boldsymbol{c}$, and the $\boldsymbol{e}$ amount of quality for all $\boldsymbol{n}$ children. Unlike the earlier models in this literature, there are two prices that need to be paid in this process: τ^q is a fixed cost for

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

raising children (and it is independent of the investments made into the children) and τ^e is the cost of investing in the quality of children.

The interior solution for this problem is denoted by (n^*, e^*), and the shadow prices of quantity and quality are:

$$p_n = \tau^q + \tau^e e^*$$

$$p_e = n^* \tau^e$$

As was the case with the earlier models in this literature, these shadow prices still lead to a quantity-quality tradeoff:

(1) There is a positive relationship between p_n and e^*, which implies that increased investments in the quality of children (e) will raise the shadow price of having more children. As such, an increase in the investment in child quality will tend to reduce the number of children in the household.

(2) There is a positive relationship between p_e and n^*, which implies that additional children will raise the shadow price of investment in child quality. As such, an increase in the number of children in the household will tend to reduce investment in children in the household.

For the purpose of later exposition, it should be noted that this component of the model governs the decision-making process for the *intensive* margin.

The key insight from ALM is that the fertility literature (including Galor) often imposes the Inada condition, which obscures the quantity-quality trade-off, because it requires households to have at least one child. That is: the model considers the intensive margin (the number of children in a family, conditional on having at least one child), but ignores the extensive margin. To better understand the extensive margin, it is necessary to consider the relative utility of having no children in the context of this model, and how it compares to the case where a household has children. Let the value of remaining childless be captured by $V_0(I)$, and let the value of having the optimal non-zero number of children be represented by $V(I, \tau^q, \tau^e)$ – note that this value is negatively related to the two prices τ^q and τ^e. In this case, a woman will choose to have children if $V(I, \tau^q, \tau^e)$ is larger than $V_0(I)$.

Within the context of this discussion, it is instructive to consider how changes in the model's parameters will impact both the intensive and extensive margins for fertility. In this case, there are two effects of interest:

(i) If τ^e decreases, then $V(I, \tau^q, \tau^e)$ will increase, while $V_0(I)$ will remain constant. This will make it more likely that a woman has at least one child – that is, a woman will be less likely to be childless given this change. Thus, a decrease in τ^e has a positive effect on the extensive margin of fertility. However, the fall in this price will induce a substitution effect between $\boldsymbol{e}$ and $\boldsymbol{n}$: $\boldsymbol{e}$ will increase, and $\boldsymbol{n}$ will decrease – this will cause a negative effect on the intensive margin for fertility. Overall, then, a decrease in τ^e will compress fertility decisions for women: they will be more likely to have at least one child, but also more likely to have fewer children overall.

(ii) If τ^q increases, then $V(I, \tau^q, \tau^e)$ will decrease, and this will decrease fertility on both the intensive and extensive margins. An increase in τ^q makes it less likely that $V(I, \tau^q, \tau^e)$ will be larger than $V_0(I)$ (which remains constant as τ^q increases), and the substitution effect will induce a decrease in $\boldsymbol{n}$ in favour of an increase in $\boldsymbol{e}$.

Having established points (i) and (ii), it is now possible to consider how changes in different characteristics of women impact their fertility decisions. Typically, it has been argued that improvements in labour market opportunities for women represent an increase in τ^q, which should decrease fertility on both the intensive and extensive margins. One of the most wellrecognized instances of improved labour market opportunities would come from an increase in educational attainment – or an improvement in the quality of educational training – for women. ALM also argue that an increase in the quality of schooling should reduce τ^e, since it is easier for parents to produce higher-quality children with access to better schools.

With these notions in hand, ALM consider how access to Rosenwald schools altered fertility for two cohorts of African-American women in the southern U.S.: the cohort of children who had varying degrees of access to the Rosenwald schools, and their parents (who were too old to access the schools). ALM argue that the construction of the Rosenwald schools should decrease τ^e for the parents since their children now have access to higher-quality schools. However, since the parents' education remains unaffected by the construction of these schools, τ^q should remain constant. In contrast, the Rosenwald schools should decrease τ^e and increase τ^q for the children who had access to these schools, since their labor market opportunities have improved as a result of these schools (as documented by Aaronson and Mazumder (2011)). By comparing the fertility patterns of the children who either did or did not have access to the Rosenwald schools with the fertility parents of their parents, ALM demonstrate that the schools "compressed" fertility: children educated in the Rosenwald schools were more likely to have at least one child compared to their parents, but (conditional on having at least one child) less likely to have as many children as their parents. The Rosenwald schools generated opposing effects on the extensive and intensive margins of fertility, as predicted by the Galor and ALM models.

This paper contributes to the relevant literature in several ways. First, modern data from a developed country, and not historical data or data from a developing country, will be analyzed in conjunction with a policy change that is similar in spirit to ALM: increased educational attainment for women due to changes in compulsory schooling laws. Although it has already been argued that a change in CSLs would raise τ^q, this paper's second contribution will be the documentation of this fact, while taking into account concerns about this effect that have been raised by Stephens and Yang (2014) (hereafter SY). In their study, SY find that the inclusion of region-specific trends or measures of school quality effectively reduce to zero instrumental-variable estimates of the return to education for U.S. data. These findings cast at least some doubt on the conclusions of earlier work on CSL-induced increases in formal schooling. We address this critique by using Canadian data, while incorporating Canadian variables that correspond to those used by SY. As will be seen, our estimates—both first stage and structural—are robust to their inclusion, thus providing validation for the empirical strategy of using CSL-induced changes in education to study increases in τ^q.

A third contribution of this paper is evidence we will present on the way in which changes in one's own educational attainment may also impact τ^e. We will argue that that increased educational attainment makes it more likely that a woman becomes and remains married. In turn, this will decrease τ^e if, compared to a single parent, a married parent faces a lower price of investment in the quality of her child(ren), due to a couple's increased joint monetary resources and greater flexibility in non-work time, which increases the total parental time which can be allocated to raising children. Indeed, existing research suggests that such differences exist between married and unmarried families. Kendig and Bianchi (2008) find that single mothers tend to spend less time in child care than married mothers, including routine care as well as care deemed more intensive such as playing with or reading to children. Pencavel (1998) finds that as husband's education increases, women's labour supply decreases, implying intra-couple coordination of time devoted to the raising of children. In addition, single-parent households exhibit relatively less involvement from the non-resident parent. Bianchi (2011) shows that offspring in mother-only households receive relatively less paternal child care; this gap has increased dramatically over the past decade, to roughly half of that spent by the father in married households. While non-resident fathers may routinely spend time with children (e.g., on weekends), Cooksey and Fondell (1996) show that resident fathers, particularly resident biological fathers, tend to spend more time in their children than non-resident ones. To the extent that investment in children is time-intensive, it follows that marriage reduces the cost of such activities.

Given this, we assert that an improvement in educational training of women will increase τ^q and decrease τ^e. Although both of these price changes will have an unambiguously negative effect on the intensive margin of fertility, they will have counteracting effects on the extensive margin of fertility. It is possible, if the decrease in τ^e is the dominant effect on the extensive margin, that a change in educational training for women will have a "compressing" effect on overall fertility – women will be more likely to have at least one child, but less likely to have large families. Overall, this analysis of Canadian data will take place in a manner that is entirely new to the literature to demonstrate this compressing effect for education.

III. Data

The primary data for this study consist of women between the age of 40 and 65 from the confidential[6] extracts of the 1981 and 1991 decennial Canadian Censuses, which were necessary for a number of reasons. First, in addition to the standard demographic variables, such as gender, marital status, citizenship status and ethnicity, these extracts also contain information on an

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[6] The confidential data sets were only accessible through secure sites, and contain information not collected for the publicly-available extracts of the decennial censuses. In particular, the confidential files contain specific information about earnings in the prior year, as well as specific amounts of education obtained by the household member and an individual's exact date of birth

individual's province[7] of birth as well as their exact date of birth; both variables are necessary to examine the relationship between changes in compulsory schooling laws and educational attainment. Second, they contain detailed labour market information. The confidential extracts of the Census contain annual earnings from the prior year, as well as labour market participation and weeks worked in the prior year, which will be used to determine the relationship between years of education and labour market performance. Third, the Census asks a retrospective question about fertility for all of its female respondents: what is the total number of children you've had over your lifetime? From this, it is possible to determine a woman's cumulative lifetime fertility. All three sets of variables will permit a proper analysis of our adapted ALM model: it will be possible to determine whether or not education is related to fertility, and whether or not earnings and marital status are key mechanisms within this effect.

To give a sense of the data, Table 1 displays the sample means of a variety of variables for women in our sample with different levels of educational attainment: less than a grade six education, educational attainment of grade seven or eight, some high school education (between grade nine to eleven) or at least a high-school graduate's level of education. These subsamples were chosen to demonstrate the relative fertility, relative earnings and marital patterns of women with different levels of education, especially over the levels of education that were impacted by the instrument we selected.

The first three rows of the table demonstrate well-documented facts about the cross-sectional relationship between education and age, income and weeks worked. In the first row, the monotonic decrease in average age as the educational attainment of the subsample increases shows that younger cohorts of women have obtained relatively more education. Of particular

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[7] A Canadian "province" is analogous to an American state. There are ten provinces in Canada, and three territories; the sample analyzed within this study will include only data drawn from individuals born in one of the ten provinces.

note for this study is the relationship between education and marital status. The second and third rows show that the fraction of women currently married and the fraction who have ever been married are increasing in educational attainment over the first three educational categories, but decreases in the fourth column. As will be demonstrated later, the instrument we employ induces changes in educational attainment between the first three columns, but not in the fourth; as such, over the range of education that is influenced by our instrument, education is positively related to both the propensity of being currently married as well as the propensity to have been married at some time.

Another notable relationship between educational attainment and total fertility is evident in the fourth and fifth rows of the table. The fourth row demonstrates that the total number of children to which a woman has given birth is negatively related to educational attainment. However, the fifth row shows that over the range of education that is influenced by our instrument (the first three columns of the table), there is a positive relationship between educational attainment and the average proportion of women who have ever had a child. Within the first three columns of these cross-tabulations, education has a compressing effect on fertility: more education is related to a decrease in the total number of children to which a woman gives birth, but an increase in the likelihood that she has at least one child.

The last relationship of note is displayed in the last two rows of the table, which document the average income and weeks worked for women across the four educational categories. Since labour market attachment can decrease with age, these averages were calculated for the same cohort in the first five rows, but ten years earlier (during prime working years): the sample used to compute these averages consist of women between the ages of 30 and 55 from the 1971 and 1981 Canada Censuses. The increase in average earnings and weeks worked as education rises, as shown in the second and third rows, echoes a well-documented cross-sectional relationship. These cross-sectional relationships will be explored in greater detail as we move to an instrumental variables framework, but as a preliminary analysis, they establish that the data exhibit relatively standard patterns found in studies that analyze the effect of education on different labour market outcomes.

This study will also incorporate another set of variables from outside of the Census, which include information on school quality, by province over time. In particular, we use three different variables that measure different annual aspects of school quality: average per-capita number of teachers in a province, annual per-capita spending on education by the provincial government, and the annual per-capita number of schools in a province. These data were provided to us by Phil Oreopoulos, and the specifics about their collection and definition are detailed in Oreopoulos (2006b).

In addition to the variables available in the Census extracts and the variables on school quality, it is important for the sake of the analysis to delineate the sample chosen for this work. In order to test the ALM model, it is necessary to assemble a sample of women whose child-bearing is completed at the time of the data collection. Since younger women are relatively less likely to have completed their child-bearing, the analysis focused on women who were at least 40 years of age at the time of the survey. Although this is an arbitrary cut-off, vital statistics indicate that only approximately 0.5% of 40-year-old women from the 1980 and 1990 Censuses gave birth after the age of 40,[8] and although the results are not reported in this paper, the main conclusions presented from the analysis are not substantively different if the cut-off age is

[8] http://www.statcan.gc.ca/pub/91-209-x/2013001/article/11784/c-g/fig04-eng.htm

raised.[9] Finally, we limit the sample to women sixty-five years old and younger to avoid issues of differential mortality that might be attributed causally to increased educational attainment.

IV. Empirical Strategy

Though descriptive, the evidence in Table 1 suggests that lower educational attainment impacts lifetime fertility. To more systematically investigate a relationship between schooling and lifetime fertility, we specify the following model:

$$BIRTH_{bpc} = \beta EDUC_{bpc} + \gamma X_{bpc} + \alpha_p + \eta_c + \delta_t + e_{bpc}.$$

Here, **β** is the coefficient of greatest interest, as it represents the relationship between schooling and fertility. Unless otherwise noted, all variables are aggregated from individual-level Census data by calculating means for each birth cohort *b*, in each province *p*, and for each Census extract *c*. The dependent variable (**BIRTH**) is either the proportion of women who have given birth to at least one child by age forty (i.e., the extensive margin) or the average number of children for those women who have given birth to at least one child by age forty (i.e., the intensive margin). As described in the previous section, these variables as based on a question which asks women in the Census, "what is the total number of children you've had in your lifetime?" Compared to relying on household composition data, this information better addresses measurement issues such as those that might be created by custody issues and child mortality, for example. They key independent variable (**EDUC**) is defined as the average number of years of education attained by sample women in each cohort-province-Census extract cell. Control variables ($\boldsymbol{X_{bpc}}$) 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.

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9 In particular, the analysis was replicated with a cut-off age of 42 and 44, and the results are essentially the same as will be reported.

Further, we seek to ensure that our estimates are not prone to the issues identified in recent work by Stephens and Yang (2014) who found that the IV return to education was not significantly different from zero when controls such as school quality specific to the U.S. state and year were included in the IV framework. To address this, we include three variables to account for school quality within each province and birth cohort: the annual per-capita spending 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. In a separate model, we include region-level trends, again designed to address the critique of Stephens and Yang (2014) who found that U.S. results were sensitive to controlling for a South region indicator variable. Finally, since we are concerned about potential within-province correlation of the errors, we calculate standard errors using a Wild cluster bootstrap procedure – since Canada has only ten provinces, Cameron et al. (2008, 2015) suggest this is the appropriate clustering approach.

Despite the non-random assignment of education, we first estimate equation (1) via Ordinary Least Squares (OLS). For completeness, we will present OLS estimates of β in the following section, but understand fully that they do not have a causal interpretation, given the clear endogeneity of the education variable. To better estimate a valid causal relationship, we employ a two-stage least squares approach that relies upon compulsory schooling laws as an instrument for educational attainment. Our first stage equation takes the following form:

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

Once again, b represents cohort of birth, p represents province, and c represents Census extract year. Again, $\mathbf{EDUC}_{bpc}$ represents the average years of education of sample 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 instrument which, in our main models, is based on province-determined minimum school leaving ages and is specific to particular birth cohorts, $\mathbf{X}_{bpc}$ represents provincial level controls mentioned above including the three province level school quality variables mentioned and $\boldsymbol{\theta}_p$, $\boldsymbol{\mu}_b$ and $\boldsymbol{\psi}_c$ represent province, birth cohort and Census year fixed effects, respectively, while $\mathbf{v}_{bpc}$ 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. In our main models, we specify the CSL instrument as a dummy variable equal to one if a particular 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.[10]

Given the multiple dimensions of CSLs as well as a lack of consensus on how best to represent required schooling, we employ several different parameterizations of the CSL instrument, including using the dropout age, total years of schooling required, entry age, as well as combinations of these variables—as robustness checks on our instrument. We report these estimates, which align closely with estimates from our main models, in the following section. Again, due to the relatively small number of clusters in our data, we implement the Wild bootstrap clustering procedure for the two-stage least-squares estimation approach, as discussed by Davidson and MacKinnon (2010).

V. Estimates

A. Estimated effects of additional schooling on fertility

We report our main estimates in Table 2. As discussed earlier, our model implies the necessity of examining lifetime fertility separately on intensive and extensive margins. The first three columns of Table 2 present estimates from models where the dependent variable is the

[10] Information on the specific timing for changes in these laws is presented in Appendix Table 1.

number of children *greater than one* (i.e., our intensive margin measure), while the last three columns present models where the dependent variable is the percent of sample women with at least one child (i.e., our extensive margin measure). Both three-column sets first present estimates from a base model with controls listed in the table notes, the second column specification adds the education quality variables discussed in the data section, while the third column specification includes region-specific trends. Table rows are consistently defined across the three outcomes where the first row represents the mean of the dependent variable and the second row represents naive OLS estimates of the relationship in question. Rows 3-7 report IV estimates with the estimates in Row 3 representing our preferred instrument set (i.e., "Dropout Dummies"), while rows 4-7 report different parameterizations of the CSLs facing sample women at the relevant ages and in the relevant province. Specifically, Row 4 uses a single variable to represent the total required years of education (defined as the difference between the mandated entry and drop-out ages), Row 5 uses a single variable to capture the mandated age at which an individual may drop-out of school, Row 6 uses three dummy variables corresponding to cases when the age at which children are required to attend school is 6, 7 or 8 years of age in a given province and year, and Row 7 includes the three dropout dummies and three age-at-entry dummies described above.

Before discussing the IV estimates, however, we note that the first-stage relationship between CSLs, parameterized as dropout indicators in our main models and in various other ways and our schooling variable, is consistently strong. As seen in Appendix Table 4, all first-stage F-statistics are greater than 10, with roughly half over 50, and most exhibiting the appropriate p-values to indicate that they are not weak instruments. While there are differences in the magnitudes of these F-statistics, estimates across models that use these different IV parameterizations are very robust and quite similar, as we note below.

We now turn to our main estimates in Table 2. As can be seen in the first three columns, which are based on a sample of women with at least one child, our first set of intensive margin estimates (i.e., Row 3) imply that an additional year of CSL-induced schooling reduces the lifetime number of children by roughly 0.9 of a child. Off a base of almost three-and-a-half children per sample woman, this represents a decrease of roughly twenty-five percent, which is very consistent with the totality of our estimates from the other instrumental variable parametrizations, which collectively imply a percent reduction in the number of children that is between twenty-five to forty percent. Indeed, the relevant estimates in Rows 4-7 are quite similar to our main estimates in Row 3 as well as remaining similar across the three model specifications presented for each dependent variable. We interpret this as strong evidence of the robustness of our main estimates.

While seemingly large, the discrete nature of childbearing may be responsible for large proportional magnitudes. Indeed, our estimates are very much in line with ALM, who find that construction of the Rosenwald Schools in the U.S. South is associated with about a one-third increase in fertility conditional on having at least one child (i.e., our definition of intensive margin fertility) in their preferred specification linked to their Table 4. To explore this issue in somewhat greater detail, we examine birth effects across the distribution of the number of children, before we turn to the extensive fertility margin. In particular, we present estimates of the fraction of women who have at least two, at least four, at least six and at least eight children in Table 3 where column tables correspond to these outcomes. As can be seen, we find no systematic relationship between CSL-induced schooling and intensive margin fertility at the two children threshold. By contrast, we find much stronger evidence at the four, six and especially the eight children thresholds. Taken together, these suggest that intensive margin fertility reductions occur largely among women who would have relatively large numbers of children.

Turning to the extensive fertility margin (i.e., the last three columns of Table 2), we find a very different pattern. Consistent with our naive OLS estimates, we find that an additional year of CSL-induced schooling actually *increases* the percent of women who report having given birth to at least one child, implying an increase in extensive margin of fertility. Again, our IV estimates are remarkably consistent across different parameterizations of the CSL instrument and are also very similar across the three specifications we present. These IV estimates of the relationship between education and extensive margin fertility range from 0.042 to 0.057 which corresponds to a 5 to 7 percent increase in the fraction of sample women who have at least one child by age forty. Overall, we find that additional schooling reduces the number of children a woman has over her lifetime, while also reducing childlessness. In other words, we find that education has a "compressing" effect on lifetime fertility—the intensive margin contracts while the extensive margin expands. In the next section, we reiterate our argument in favour of education having a "compressing" effect on lifetime fertility and present empirical evidence that supports this pattern.

B. Reconciling main estimates with model predictions

The observed pattern—reductions in lifetime fertility along the intensive margin coupled with increases along the extensive margin—can be understood in the context of our model. Recall, that additional education affected fertility through changes in τ^q and τ^e in the model we presented. Consistent with ALM, we asserted that additional schooling raised τ^q, the cost of child quantity, and lowered τ^e, the cost of child quality or child investment. As discussed, both such changes imply a clear negative impact on child quantity. That is, an increase in τ^q and a decrease in τ^e both imply a reduction in the number of children to which a woman is predicted to give birth. However, these changes were shown to have an ambiguous effect on the extensive margin of fertility. In particular, the model implied that τ^q increasing should reduce the probability a woman has at least one child, but that a decrease in τ^e should increase it. That is, the two changes oppose each other along the extensive fertility margin. Our estimates in the last three rows of Table 2 (i.e., those that pertain to extensive-margin fertility) suggest that the impact of the reduction in the cost of child quality (τ^e) outweighs the impact of the increase in the cost of child quantity (τ^q) since we find an increase in the fraction of women who have at least one child. This is also consistent with the work of ALM, who also found that increases in mandated schooling levels made it more likely for women to exhibit positive changes on the extensive margin of fertility.

In what follows, we examine two major channels through which education plausibly affects the costs of fertility, τ^q and τ^e. In particular, we examine: (1) the impact of education on marriage, since it should principally reduce the price of child quality (τ^e) and (2) the impact of education on earnings, which should principally affect the price of child quantity (τ^q). We find that education increases the probability of being married, while reducing the probability of never being married; we believe that these impacts of education reduce the cost of child quality (τ^e) which, in turn, may generate the increase in extensive margin fertility we estimate. We also find that education increases earnings among sample women which represents an increase in the cost of child quantity (τ^q). Together, these findings provide support for our assertion that education increases τ^q while decreasing τ^e, a pattern which may generate the "compressing" effect of education on fertility that we find.

C. Empirical evidence on plausible mechanisms

Table 4 presents estimates of the impact of schooling on marriage. While the rows of the table correspond to Table 2, the dependent variable in the first three columns is current marital status, while in the second three columns it is the fraction of women who report *ever* being married. As in Table 2, the three columns associated with each dependent variable represent three different model specifications. Consistent with corresponding OLS estimates, IV estimates from the first three columns of Table 4 suggest a positive relationship between schooling and current marital status. In particular, these estimates, which are nearly identical across the three specifications, suggest a roughly one percent increase in the proportion of women in a birth year/province cohort who are currently married. Estimates from other IV parameterizations (Rows 4-7) are very similar and also statistically precise. Though slightly smaller in magnitude, they support the idea that CSL-induced schooling increased marriage. Estimates presented in the last three columns paint a similar picture: an increase in education is associated with a roughly 3 to 4 percentage-point increase in the likelihood of being married at some point for a woman. Both sets of results—those pertaining to current and historical marital status—imply that education increased marriage among women who were induced to obtain more schooling by CSLs. This increase in marriage is a primary mechanism though which education may plausibly have reduced the costs of child quality, τ^e. In turn, this reduction is consistent with the increase in extensive margin fertility we presented earlier in this section.

In Table 5, we present estimates for models that focus on two labour market outcomes—earnings and annual weeks worked—which are closely linked to τ^q, the cost of child quantity. Table 5 follows the structure of Table 4, but now the first three columns contain estimates from models where the dependent variable is the log of annual income, while the second three columns present estimates from models where the dependent variable is annual weeks worked, a measure of labour supply. To avoid issues related to early retirement, our samples for these models (income and weeks worked) are based on women aged 30-55 years old instead of aged 40-65 years old as in all other models. As can be seen in Row 3, our main IV estimates suggest a 14-15 percent rate of return to an additional year of schooling, which is consistent with other studies using Canadian data (Oreopoulos, 2006b). Furthermore, unlike the case of Stephens and Yang, the inclusion of school quality and region-specific trends does not impact the significance or magnitude of the returns to education in the second stage of the IV regression. And, as was the case in earlier tables, estimates are quite similar across the three specifications and the different IV parameterizations in Rows 4-7 are very similar and most are precisely estimated at conventional levels of significance. This overall finding supports the idea that increased schooling raises τ^q, the cost of child quantity, via increasing the opportunity cost of having children. In turn, it supports our finding of a negative impact of schooling on intensive margin fertility. While it should also reduce extensive margin fertility, our estimates suggest such an effect is outweighed by a reduction in τ^e, a reduction we link to our estimates above which show additional CSL-induced schooling increased marriage. The estimates in the final three columns of the table indicate that, if anything, education did not have a consistently significant impact on labour supply for women in our sample. In rows three through seven, the IV estimates of this effect show about a one- to two-week decrease in weeks worked in a year, but most of these estimates are quite imprecise and not statistically significant. This indicates that the effect of education on income in the table's first three columns was not driven by an increase in labour supply.

VI. Discussion

Changes in fertility patterns have been dramatic over the past half-century, and the analysis of these patterns has often focused entirely on the intensive margin of fertility, and the way it has been affected by improved labor market opportunities for women. But this has ignored the extensive margin of fertility, and in an earlier analysis, ALM demonstrated with a sample of Black women from the U.S. south that improvements in educational training had a "compressing" effect on the distribution of fertility for women, who were more likely to have at least one child, but less likely to have many children. But the choice of sample in ALM raises the question about the generalizability of their finding. The work of Stephens and Yang showed that education does not have a significant effect on earnings for a sample of all Americans, which undercuts the ability to extend an analysis of changes in education on fertility, which implicitly assumes that more education will increase labor market opportunities for women, and hence change the price of the quantity of children.

This paper provides several contributions to the literatures analyzing the effect of education on both fertility and earnings. First, we use a large, representative sample of Canadian women to demonstrate that the I.V. effect of education is to compress the fertility distribution. Second, we document the fact that – even in the presence of school quality measures and region-specific trends – education has a significant and positive I.V. effect on income for women. This finding is both novel to the literature on the returns to education, which has not seen an equivalent finding since the work of Stephens and Yang, and the crucial to the fertility literature, which requires a mechanism to alter the price of the quantity of children. Third, we investigate a novel mechanism through which education may impact the price of quality for children: marriage propensities. Since it is less costly to achieve a given level of child quality with a married couple (as opposed to a single parent), we explore the I.V. effect of education on the propensity to be currently married or to have ever been married. In both cases, education has a positive and statistically significant I.V. effect, which demonstrates a plausible mechanism through which it may impact the price of child quality. Overall, these results represent a new interpretation for the effect of education on fertility for a representative sample of women from a developed, Western economy.

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Table 1: Descriptive Statistics

	Grade 6 Education or Less	Between Grade 7 and 8 Education	Between Grade 9 and 11 Education	At Least HS Education
Age	54.91 (7.07)	53.56 (7.23)	51.54 (7.40)	49.97 (7.38)
Currently Married	0.661 (0.473)	0.738 (0.440)	0.765 (0.424)	0.756 (0.429)
Ever Married	0.935 (0.247)	0.964 (0.186)	0.968 (0.176)	0.952 (0.215)
Number of Children	4.221 (3.284)	3.682 (2.555)	3.100 (1.999)	2.602 (1.603)
Percent with Any Children	0.890 (0.313)	0.926 (0.262)	0.937 (0.243)	0.921 (0.269)
Income	6961 (11252)	7444 (12281)	10925 (15431)	19885 (23026)
Weeks Worked	9.15 (16.02)	9.74 (16.32)	14.37 (19.58)	21.91 (22.24)

The sample consists of women, born in one of the ten Canadian provinces. The data displayed in the first five rows are assembled from the 1981 and 1991 Canada Censuses for women between the ages 40 to 65. The data displayed in the last two rows are assembled from the 1971 and 1981 Canada Censuses for women between the ages 30 to 55. Standard deviations are listed in parentheses beneath the means within each cell.

Appendix Table 1: The Effect of the Instrument on Educational Attainment and Individual Grade Completion

	Years of Education	Complete Grade 7	Complete Grade 8	Complete Grade 9	Complete Grade 10	Complete Grade 11	Graduate HS?
Drop out at age 14?	0.462	0.087	0.091	0.043	0.024	0.029	-0.014
	[0.024]	[<0.001]	[<0.001]	[0.080]	[0.226]	[0.008]	[0.670]
Drop out at age 15?	0.961	0.131	0.194	0.133	0.116	0.088	-0.045
	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[0.048]
Drop out at age 16?	0.453	0.067	0.055	0.042	0.017	0.020	0.011
	[<0.001]	[<0.001]	[0.130]	[0.174]	[0.527]	[0.164]	[0.605]
F-stat on Dropout Dummies	73.14	62.36	59.15	16.18	9.84	12.45	3.63
	[<0.001]	[<0.001]	[0.002]	[0.048]	[0.146]	[0.044]	[0.212]

The sample consists of women, aged 40 to 65, born in one of the ten Canadian provinces. The data are assembled from the 1981 and 1991 Canada Censuses. The dependent variables in the regressions underlying these results are listed in the table's columns, and the coefficients listed in the rows correspond to the dummy variable equal to one if the law permitted an individual to cease their education at age 14 (in row 1), age 15 (in row 2) or age 16 (in row 3). The fourth row reports the F-statistic on the test of the hypothesis that all three coefficients are equal to zero. The Wild Cluster Bootstrap was used at the province level to compute the p-values for the table, which are listed in brackets beneath the coefficient estimates (or F-statistics in row 4). The other controls in the regressions include: year-of-birth dummies, province-of-birth dummies, a quartic in age, a dummy for the Census extract which produced the data, rural status, the percentage of manufacturing jobs, an aboriginal indicator, an immigrant indicator.

Appendix Table 2: The Effect of the Instrument on Educational Attainment and Individual Grade Completion With Educational Quality Controls

	Years of Education	Complete Grade 7	Complete Grade 8	Complete Grade 9	Complete Grade 10	Complete Grade 11	Graduate HS?
Drop out at age 14?	0.458	0.087	0.090	0.042	0.022	0.028	-0.009
	[<0.001]	[<0.001]	[0.002]	[0.062]	[0.232]	[0.002]	[0.496]
Drop out at age 15?	0.791	0.122	0.169	0.108	0.090	0.069	-0.036
	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[0.224]
Drop out at age 16?	0.294	0.062	0.033	0.015	-0.011	-0.002	0.019
	[0.008]	[0.008]	[0.460]	[0.551]	[0.647]	[0.931]	[0.482]
F-stat on Dropout Dummies	46.13	40.59	24.44	15.99	7.54	8.26	3.27
	[0.001]	[<0.001]	[0.019]	[0.019]	[0.119]	[0.094]	[0.238]

The sample consists of women, aged 40 to 65, born in one of the ten Canadian provinces. The data are assembled from the 1981 and 1991 Canada Censuses. The dependent variables in the regressions underlying these results are listed in the table's columns, and the coefficients listed in the rows correspond to the dummy variable equal to one if the law permitted an individual to cease their education at age 14 (in row 1), age 15 (in row 2) or age 16 (in row 3). The fourth row reports the F-statistic on the test of the hypothesis that all three coefficients are equal to zero. The Wild Cluster Bootstrap was used at the province level to compute the p-values for the table, which are listed in brackets beneath the coefficient estimates (or F-statistics in row 4). The other controls in the regressions include: year-of-birth dummies, province-of-birth dummies, a quartic in age, a dummy for the Census extract which produced the data, rural status, the percentage of manufacturing jobs, an aboriginal indicator, an immigrant indicator, the per capita number of schools in the province, the per capita number of teachers in the province, and the real per capita annual expenditures on schooling.

Appendix Table 3: The Effect of the Instrument on Educational Attainment and Individual Grade Completion With Provincial Trends

	Years of Education	Complete Grade 7	Complete Grade 8	Complete Grade 9	Complete Grade 10	Complete Grade 11	Graduate HS?
Drop out at age 14?	0.447	0.093	0.092	0.037	0.016	0.025	0.003
	[<0.001]	[<0.001]	[<0.001]	[0.026]	[0.010]	[<0.001]	[0.651]
Drop out at age 15?	0.817	0.137	0.181	0.111	0.085	0.063	-0.035
	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[0.492]
Drop out at age 16?	0.291	0.064	0.025	0.012	-0.019	-0.003	0.038
	[0.284]	[0.048]	[0.773]	[0.693]	[0.504]	[0.911]	[0.158]
F-stat on Dropout Dummies	68.51	110.50	62.07	22.58	9.47	30.44	4.64
	[0.002]	[0.002]	[0.001]	[0.033]	[0.112]	[0.025]	[0.105]

The sample consists of women, aged 40 to 65, born in one of the ten Canadian provinces. The data are assembled from the 1981 and 1991 Canada Censuses. The dependent variables in the regressions underlying these results are listed in the table's columns, and the coefficients listed in the rows correspond to the dummy variable equal to one if the law permitted an individual to cease their education at age 14 (in row 1), age 15 (in row 2) or age 16 (in row 3). The fourth row reports the F-statistic on the test of the hypothesis that all three coefficients are equal to zero. The Wild Cluster Bootstrap was used at the province level to compute the p-values for the table, which are listed in brackets beneath the coefficient estimates (or F-statistics in row 4). The other controls in the regressions include: year-of-birth dummies, province-of-birth dummies, a quartic in age, a dummy for the Census extract which produced the data, rural status, the percentage of manufacturing jobs, an aboriginal indicator, an immigrant indicator, and three trend terms to capture trends in three different regions of Canada – the Atlantic provinces (Newfoundland, PEI, Nova Scotia and New Brunswick), the central provinces (Ontario and Quebec), and the "Prairie" provinces (Manitoba, Saskatchewan and Alberta).

Appendix Table 4: The Effect of Various Instruments on Educational Attainment

	(1)	(2)	(3)
F-stat on Dropout Dummies (Income Sample Only)	25.14 [0.001]	32.23 [<0.001]	40.79 [0.003]
F-stat on Dropout Age	101.2 [<0.001]	111.7 [<0.001]	396.8 [<0.001]
F-stat on Total required years	87.05 [<0.001]	111.5 [<0.001]	441 [0.008]
F-stat on Dropout Dummies	73.14 [<0.001]	46.13 [0.001]	68.51 [0.002]
F-stat on Entry Dummies	59.57 [0.050]	50.56 [0.065]	288.41 [<0.001]
F-stat on Dropout & Entry Dummies	115.59 [0.081]	187.48 [0.022]	227.74 [0.003]
School Quality Controls?	No	Yes	No
Trends?	No	No	Yes

The sample consists of women who are at least 40 years old, and born in one of the ten Canadian provinces. The data are assembled from the 1981 and 1991 Canada Censuses. The dependent variable is equal to the average educational attainment for women in each birth-year/province cohort. Each cell reports the F-test on the instrumental variable(s) used in the regression, as well as the p-values for these test statistics, which are listed in brackets beneath the F-statistics, and were computed with the Wild Cluster Bootstrap at the province level. The instruments differ by row: the first row involves the age at which an individual is allowed to drop out of school, the second row involves the total number of required years of education, the third row uses three dummy variables to capture the age at which a student was permitted to cease their schooling, the fourth row uses three dummy variables to capture the age at which a student was required to begin their schooling, and the fifth row involves all six dummies involved in the regressions from the third and fourth rows. The independent variables in the first column's regressions include: year-of-birth dummies, province-of-birth dummies, a quartic in age, a dummy for the Census extract which produced the data, rural status, the percentage of manufacturing jobs, an aboriginal indicator, an immigrant indicator. The second column's regressions are the same as column one, but also include the per capita number of schools in the province, the per capita number of teachers in the province, and the real per capita annual expenditures on schooling. The third column's regressions are the same as column one, but also include three trend terms to capture trends in three different regions of Canada – the Atlantic provinces (Newfoundland, PEI, Nova Scotia and New Brunswick), the central provinces (Ontario and Quebec), and the "Prairie" provinces (Manitoba, Saskatchewan and Alberta).

Table 2: The Effect of Education on the Intensive and Extensive Margins of Fertility

	Number of Children (at least one)			Percent With At least One Child		
Mean Dep. Variable	3.332 (2.049)			0.924 (0.264)		
OLS Coef. Education	-0.620 [0.002]	-0.740 [0.002]	-0.851 [0.002]	0.033 [0.014]	0.038 [<0.001]	0.041 [<0.001]
IV Education (Dropout dummies)	-0.832 [<0.001]	-0.944 [<0.001]	-1.046 [<0.001]	0.046 [<0.001]	0.055 [<0.001]	0.055 [<0.001]
IV Education (Total required years)	-1.174 [<0.001]	-1.223 [<0.001]	-1.386 [<0.001]	0.044 [<0.001]	0.047 [<0.001]	0.046 [<0.001]
IV Education (Dropout Age)	-1.171 [<0.001]	-1.221 [<0.001]	-1.349 [<0.001]	0.042 [<0.001]	0.044 [<0.001]	0.042 [<0.001]
IV Education (Entry Dummies)	-0.971 [0.004]	-0.902 [<0.001]	-0.884 [<0.001]	0.055 [<0.001]	0.057 [<0.001]	0.057 [<0.001]
IV Education (Dropout & Entry Dummies)	-0.855 [0.007]	-0.956 [<0.001]	-1.005 [<0.001]	0.047 [<0.001]	0.055 [<0.001]	0.054 [<0.001]
Quality Controls?	No	Yes	No	No	Yes	No
Trends?	No	No	Yes	No	No	Yes

The sample consists of women, aged 40 to 65, born in one of the ten Canadian provinces. The data are assembled from the 1981 and 1991 Canada Censuses. The first three columns use a dependent variable equal to the average total number of children born to woman (in each birth-year/province cohort), the next three use this same average, but compute it for women with at least one child, and the last three columns use a dependent variable equal to the percent of women (in each birth-year/province cohort) who have given birth to at least one child. The means of the dependent variables are listed in the first row of the table. The second row reports the coefficient on years of education from OLS regressions with controls that vary depending on the column. The first, fourth and seventh column's regressions include: year-of-birth dummies, province-of-birth dummies, a quartic in age, a dummy for the Census extract which produced the data, rural status, the percentage of manufacturing jobs, an aboriginal indicator, an immigrant indicator. The second, fifth and eighth column's regressions are the same as columns one and four, but also include the per capita number of schools in the province, the per capita number of teachers in the province, and the real per capita annual expenditures on schooling. The third and sixth column's regressions are the same as columns one and four, but also include three trend terms to capture trends in three different regions of Canada – the Atlantic provinces (Newfoundland, PEI, Nova Scotia and New Brunswick), the central provinces (Ontario and Quebec), and the "Prairie" provinces (Manitoba, Saskatchewan and Alberta). The third row reports the coefficient on years of education from IV regressions which use three dummy variables to capture the age at which a student was permitted to cease their schooling. The Wild Cluster Bootstrap was used at the province level to compute the p-values for the table, which are listed in brackets beneath the coefficient estimates in rows two and three.

Table 3: The Effect of Education on the Distribution of the Intensive Margin of Fertility

	At Least 2 Children		At Least 4 Children		At Least 6 Children		At Least 8 Children	
Mean Dep. Variable	0.753		0.294		0.099		0.038	
	(0.431)		(0.456)		(0.298)		(0.191)	
OLS Coef.	-0.003	-0.002	-0.053	-0.072	-0.045	-0.072	-0.031	-0.050
Education	[0.529]	[0.531]	[0.022]	[0.002]	[0.056]	[0.002]	[0.028]	[0.002]
IV Education	-0.001	-0.001	-0.073	-0.093	-0.066	-0.088	-0.044	-0.059
(Dropout dummies)	[0.765]	[0.781]	[<0.001]	[<0.001]	[0.046]	[<0.001]	[0.065]	[<0.001]
IV Education (Total	-0.021	-0.023	-0.123	-0.139	-0.089	-0.119	-0.057	-0.077
required years)	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]
IV Education	-0.021	-0.023	-0.124	-0.137	-0.092	-0.116	-0.057	-0.075
(Dropout Age)	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]
IV Education	-0.003	0.009	-0.091	-0.069	-0.077	-0.076	-0.050	-0.053
(Entry Dummies)	[0.615]	[0.020]	[<0.001]	[<0.001]	[0.061]	[<0.001]	[0.122]	[0.026]
IV Education	-0.003	0.001	-0.078	-0.087	-0.066	-0.086	-0.044	-0.058
(Dropout & Entry Dummies)	[0.488]	[0.761]	[<0.001]	[<0.001]	[0.056]	[<0.001]	[0.118]	[<0.001]
Trends?	No	Yes	No	No	Yes	No	No	Yes

The sample consists of women, aged 40 to 65, born in one of the ten Canadian provinces. The data are assembled from the 1981 and 1991 Canada Censuses. The first two columns use a dependent variable equal to the average proportion of woman (in each birth-year/province cohort) who birthed at least two children, the next two use a dependent variable equal to the average proportion of woman who birthed at least four children, the next two use a dependent variable equal to the average proportion of woman who birthed at least six children, and the last two columns use a dependent variable equal to the average proportion of woman who birthed at least eight children. The means of the dependent variables are listed in the first row of the table. The second row reports the coefficient on years of education from OLS regressions with controls that vary depending on the column. The first, third, fifth and and seventh column's regressions include: year-of-birth dummies, province-of-birth dummies, a quartic in age, a dummy for the Census extract which produced the data, rural status, the percentage of manufacturing jobs, an aboriginal indicator, an immigrant indicator. The second, fourth, sixth and eighth column's regressions are the same as the other columns, but also include three trend terms to capture trends in three different regions of Canada – the Atlantic provinces (Newfoundland, PEI, Nova Scotia and New Brunswick), the central provinces (Ontario and Quebec), and the "Prairie" provinces (Manitoba, Saskatchewan and Alberta). The third row reports the coefficient on years of education from IV regressions which use three dummy variables to capture the age at which a student was permitted to cease their schooling. The fourth row reports the coefficient on years of education from IV regressions which use a single variable to capture the difference between the age at which a student was permitted to cease their schooling and the age at which a student must begin her schooling. The fifth row reports the coefficient on years of education from IV regressions which use a single variable to capture the age at which a student was permitted to cease their schooling. The sixth row reports the coefficient on years of education from IV regressions which use three dummy variables to capture the age at which a student must begin her schooling. The seventh row reports the coefficient on years of education from IV regressions which use three dummy variables to capture the age at which a student was permitted to cease their schooling, and three to capture the age at which she must cease her schooling. The Wild Cluster Bootstrap was used at the province level to compute the p-values for the table, which are listed in brackets beneath the coefficient estimates in rows two through seven.

Table 4: The Effect of Education on Marital Status

	Currently Married?			Ever Married?		
	(1)	(2)	(3)	(4)	(5)	(6)
Mean of Dependent Variable		0.748 (0.434)			0.958 (0.202)	
OLS Coefficient on Education	0.007 [0.056]	0.008 [0.046]	0.008 [0.047]	0.021 [0.028]	0.024 [<0.001]	0.025 [<0.001]
IV Coefficient on Education (Dropout dummies)	0.009 [<0.001]	0.011 [<0.001]	0.012 [<0.001]	0.027 [<0.001]	0.034 [<0.001]	0.033 [<0.001]
IV Education (Total required years)	0.006 [<0.001]	0.006 [0.108]	0.008 [<0.001]	0.041 [<0.001]	0.042 [<0.001]	0.039 [<0.001]
IV Education (Dropout Age)	0.005 [0.057]	0.006 [0.112]	0.007 [<0.001]	0.039 [<0.001]	0.040 [<0.001]	0.036 [<0.001]
IV Education (Entry Dummies)	0.010 [0.046]	0.012 [<0.001]	0.012 [<0.001]	0.036 [<0.001]	0.032 [<0.001]	0.027 [<0.001]
IV Education (Dropout & Entry Dummies)	0.009 [0.011]	0.011 [<0.001]	0.011 [<0.001]	0.029 [<0.001]	0.033 [<0.001]	0.030 [<0.001]
Quality Controls?	No	Yes	No	No	Yes	No
Trends?	No	No	Yes	No	No	Yes

The sample consists of women, aged 40 to 65, born in one of the ten Canadian provinces. The data are assembled from the 1981 and 1991 Canada Censuses. The first three columns use a dependent variable equal to the average percentage of married women (in each birth-year/province cohort), and the last three columns use a dependent variable equal to the average percent of women (in each birth-year/province cohort) who have never been married. The means of the dependent variables for columns one to three are columns four to six are listed in the first row of the table. The second row reports the coefficient on years of education from OLS regressions with controls that vary depending on the column. The first and fourth column's regressions include: year-of-birth dummies, province-of-birth dummies, a quartic in age, a dummy for the Census extract which produced the data, rural status, the percentage of manufacturing jobs, an aboriginal indicator, an immigrant indicator. The second and fifth column's regressions are the same as columns one and four, but also include the per capita number of schools in the province, the per capita number of teachers in the province, and the real per capita annual expenditures on schooling. The third and sixth column's regressions are the same as columns one and four, but also include three trend terms to capture trends in three different regions of Canada – the Atlantic provinces (Newfoundland, PEI, Nova Scotia and New Brunswick), the central provinces (Ontario and Quebec), and the "Prairie" provinces (Manitoba, Saskatchewan and Alberta). The third row reports the coefficient on years of education from IV regressions which use three dummy variables to capture the age at which a student was permitted to cease their schooling. The Wild Cluster Bootstrap was used at the province level to compute the p-values for the table, which are listed in brackets beneath the coefficient estimates in rows two and three.

Table 5: The Effect of Education on Income and Weeks Worked

	Income			Weeks Worked		
	(1)	(2)	(3)	(4)	(5)	(6)
Mean of Dependent Variable		9.383			26.24	
		(1.330)			(23.24)	
OLS Coefficient on Education	0.117	0.126	0.126	–2.502	–2.206	–2.803
	[0.012]	[<0.001]	[0.028]	[0.072]	[0.104]	[0.002]
IV Coefficient on Education (Dropout Dummies)	0.153	0.156	0.149	–1.148	–1.392	–2.185
	[<0.001]	[<0.001]	[<0.001]	[0.346]	[0.064]	[<0.001]
IV Education (Total required years)	0.171	0.166	0.138	–0.633	–0.721	–1.849
	[<0.001]	[<0.001]	[<0.001]	[0.318]	[0.140]	[<0.001]
IV Education (Dropout Age)	0.156	0.158	0.129	–0.832	–0.795	–1.820
	[<0.001]	[<0.001]	[<0.001]	[0.202]	[0.174]	[<0.001]
IV Education (Entry Dummies)	0.198	0.162	0.140	–1.102	–1.764	–1.948
	[<0.001]	[<0.001]	[0.033]	[0.400]	[0.132]	[0.128]
IV Education (Dropout & Entry Dummies)	0.165	0.155	0.134	–0.981	–1.474	–1.967
	[<0.001]	[<0.001]	[<0.001]	[0.300]	[0.054]	[0.038]
Quality Controls?	No	Yes	No	No	Yes	No
Trends?	No	No	Yes	No	No	Yes

The sample consists of women, aged 30 to 55, born in one of the ten Canadian provinces. The data are assembled from the 1971 and 1981 Canada Censuses for the first three columns, and only the 1981 Canada Census for the last three. The first three columns use a dependent variable equal to the average log annual income (in each birth-year/province cohort), and the last three columns use a dependent variable equal to the average total number of weeks worked (in each birth-year/province cohort). The means of the dependent variables for columns one to three are columns four to six are listed in the first row of the table. The second row reports the coefficient on years of education from OLS regressions with controls that vary depending on the column. The first and fourth column's regressions include: year-of-birth dummies, province-of-birth dummies, a quartic in age, a dummy for the Census extract which produced the data, rural status, the percentage of manufacturing jobs, an aboriginal indicator, an immigrant indicator. The second and fifth column's regressions are the same as columns one and four, but also include the per capita number of schools in the province, the per capita number of teachers in the province, and the real per capita annual expenditures on schooling. The third and sixth column's regressions are the same as columns one and four, but also include three trend terms to capture trends in three different regions of Canada – the Atlantic provinces (Newfoundland, PEI, Nova Scotia and New Brunswick), the central provinces (Ontario and Quebec), and the "Prairie" provinces (Manitoba, Saskatchewan and Alberta). The third row reports the coefficient on years of education from IV regressions which use three dummy variables to capture the age at which a student was permitted to cease their schooling. The Wild Cluster Bootstrap was used at the province level to compute the p-values for the table, which are listed in brackets beneath the coefficient estimates in rows two and three.