“The Fewer, the Merrier”: Compulsory Schooling Laws, Human Capital, and Fertility in the United States*

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Abstract

I investigate the effect of the introduction of compulsory schooling laws on education and fertility in the United States, 1850–1920. I find that compulsory schooling was associated with a 7 percent increase in enrollment and with a 15 percent decline in the fertility of women of reproductive age. My identification strategy is based on a difference-in-differences (DID) methodology involving individuals living in the vicinity of the state border where legislation changed. The results are robust to the inclusion of a number of socio-demographic and geographic controls. The effects on education are particularly strong for black children, whereas the effects on fertility are concentrated among young women. The results suggest that compulsory schooling laws may be a crucial policy for hastening both the demographic transition, and the transition to modern growth.

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1 Introduction

Economic development involves dramatic social transformations. In the process of becoming a modern economy, most countries experience both a rapid decline in fertility and the rise of mass schooling. In the United States, much of the increase in educational attainment was accompanied by social legislation. Laws compelled parents to send their children to school instead of allowing them to toil in the fields, shops, or factories. In the few decades between 1850 and 1920, the United States became one of the world leaders in mass schooling; child labor had been successfully eradicated.

In parallel with the rise in school attendance, modernizing economies often witness a massive reduction in fertility. Around 1850, the average American woman could expect to give birth to about six children during her lifetime. Three generations later, this figure had fallen to a mere three children. Associated with the rise of the nuclear family, the modern concept of childhood first appears. By the end of the process, children have become economically "worthless", but emotionally "priceless" (Zelizer (1985)).

In this paper I provide a direct test of the effects of government intervention on education and fertility. I test the effects of compulsory schooling laws (CSLs) on school enrollment and marital fertility using a difference-in-differences strategy. In order to avoid the potential pitfalls of unobserved heterogeneity, I restrict my attention to border regions. Borders are particularly useful because they suggest abrupt, discontinuous changes. People living on either side of a border region are more likely to be similar in terms of observables and unobservables. However, since they live in different jurisdictions, they are exposed to different regulations. After controlling for any remaining demographic and economic variables, I argue that the differences observed between the outcome variables of individuals living in states that passed the laws and those that did not must be related to the enactment of compulsory schooling.

My analysis of the evolution of education in border regions about the time CSLs were introduced reveals that, contrary to the weight of earlier evidence (Landes & Solmon (1972), Margo & Finegan (1996)), legislation increased the school enrollment of children by about 7 percent. This finding is robust to the inclusion of a variety of socio-demographic and geographic controls. Separate regressions show that the effect of CSLs on education is stronger for black children. Furthermore, I find that the laws increased the enrollment only of those children who were affected by it. I confirm that the increase in enrollment is a consequence of the law by examining the effect of a placebo law.

Next, I turn to the analysis of fertility outcomes. Fertility measures available in the historical census data are quite poor. Using a methodology similar to what the United Nations recommends for countries with poor vital registration, I construct a measure of fertility based on the ages of children living with their mothers at census time (cf. La Ferrara,Eliana *et al.* (2008); U.N. (1983)). With the time-series data of fertility I am able to test changes that occur simultaneously with the law's introduction. Along the borders, I compare the number of births after the CSL to the number beforehand. Considering a time series of 15 years of births, I find that women reduced their fertility about 15 percent as a consequence of the introduction of CSL.

This result is robust to the inclusion of controls, and it holds even when restricted to within-mother variation. The effect seems to be stronger on women who are young at the moment the change in policy occurred. Again, this is consistent with the notion that the effect of the laws should be greater on women who have not yet made most of their fertility decisions. The effects are also robust to the correction for autocorrelation in the treatment (Bertrand *et al.* (2004)).

This paper broadly relates to research in different fields. First, it is connected with the macroeconomic literature on "unified growth", summarized in Galor (2004). A number of unified growth models have specifically considered the effect of state interventions in order to reduce child labor and increase schooling (Doepke (2004); Doepke & Zilibotti (2005); Galor & Moav (2006)). In addition, this paper is related to a number of empirical studies that attempt to measure the "quantity–quality" trade-offs (Rosenzweig & Wolpin (1980b); Angrist *et al.* (2006)). It should be noted, however, that this literature stresses finding good instruments for fertility in order to pinpoint its effect on education (and on other labor outcomes). In contrast, this paper examines how exogenous changes in education affect the optimal fertility decisions of households.

Finally, the paper relates to a strand of literature that focuses on the effects of social legislation in the United States— in particular, compulsory schooling laws (Landes & Solmon (1972); Margo & Finegan (1996); Goldin (1999); Moehling (1999); Lleras-Muney (2002)).

2 From Mass Schooling to Lower Fertility

The relationship between family size and education has been on the research agenda of social sciences for a long time. Theoretical efforts in both economics and demography have posed reasons why education and fertility should be negatively related. Perhaps the most famous of such theories is the quantity–quality tradeoff, first proposed by Becker & Lewis (1973). The main notion is that the cost of "quantity" increases with the level of "quality" given to each child. In a world where the opportunity costs of quality are increasing, the observed effect should be a decline in total fertility.

The main intuition of Becker and Lewis for the negative relationship between the "quantity" and "quality" holds true in economic growth models where altruistic parents decide on the size of the household (Becker & Barro (1988)). In particular, the quantity–quality trade-off has become a central feature in recent unified–growth models. In these models the goal is to explain, within a unique framework, the periods of Malthusian stagnation and modern growth. During the transition from stagnation to growth, fertility is typically affected by two opposing effects. First, since children are a normal good, income tends to have a positive effect on fertility. Second, as the transition to development continues, the skill premium tends to rise, which increases the opportunity cost of children. At some point in the road to development, the second effect prevails and makes the relationship between income and fertility negative.

Given the crucial role of the fertility transition, there have been some attempts to quantify the impact of social legislation (i.e., compulsory schooling laws and child labor laws) on education, fertility, and ultimately growth. Several papers argue that these policies should lead to an eventual reduction in fertility (Bardhan & Udry (1999); Moav (2005)). In particular, Hazan & Berdugo (2002) suggest that the enactment of compulsory schooling laws entails an immediate escape from the poverty trap. The authors further argue that, if compulsory schooling is combined with redistributive taxation, then it is possible to achieve an allocation that Pareto dominates the competitive equilibrium with child labor.[1]

Doepke (2004) tries to pinpoint the effects of child labor laws by conducting a calibration exercise with a standard unified growth model using data from a Korea, Brazil, and the United Kingdom. While Korea rapidly enacted both a child labor and compulsory schooling law, Brazil lagged behind. Calibrating his model to these cases, Doepke argues that at the introduction of the reform, there is a discrete drop in fertility that leads to replacement fertility in only two generations. Similarly, growth rates peak immediately upon the introduction of the law.

All the papers discussed so far have emphasized the economic channel between the opportunity cost of children and fertility. Theoretical and empirical efforts from the demography literature have also focused on noneconomic aspects of the fertility choice. Studying the case of developing economies and the past experience of industrial countries, Caldwell (1982) proposes several channels through which education may affect fertility. He conjectures that mass education hastens the transition from a family-based high-fertility

[1] In most of the models discussed here a compulsory schooling law and a child labor law have similar consequences.

regime to a capitalistic low-fertility regime. The process occurs not only through reduction of the market value of child labor but also through the increased school-related expenditures families must make and, more generally, from the introduction of modern values that run counter the previous "family morality". All these factors belong to the so-called demand channel between education and fertility. Easterlin & Crimmins (1985) identify other channels that work in contradicting ways. Mass education can increase the natural supply of children as it improves hygienic conditions while possibly devaluing cultural practices that limit natural fertility (e.g., intercourse taboo, prolonged breastfeeding). All of these would increase the potential supply of children. On the other hand, mass education will also increase awareness of contraception, thereby reducing fertility.

The demographic literature has also tried to assess empirically the causes and timing of this fertility transition. The European Fertility Project (EFP) investigated the evolution of marital fertility in Europe during the nineteenth century. The project discovered that, regardless of socio-economic conditions, all regions of Europe began the fertility transition at about the same time. Furthermore, the spread of this transition occurred on linguistic and religious bases. Both claims taken together are usually interpreted as a "diffusion" view of the fertility transition that minimizes the role of economics. More recently, however, Brown & Guinnane (2003) have disputed this conclusion by pointing out some statistical flaws in the methodology used by the EFP papers.[2]

[2]The two flaws concern the aggregation level of the data used and the use of only a cross-section of the data.

Assessing the relationship between education and fertility has also been on the agenda of empirical economists. The main difficulty in this literature has been that, since fertility and education are the outcome of a joint choice within the household, there could be unobserved variables driving both choices. Hence that any ordinary least squares (OLS) regression would yield biased estimates.

A number of papers corrected for this problem by using an instrumental variable (IV) approach (Rosenzweig & Wolpin (1980b); Lee (2004); Angrist *et al.* (2006)). A different approach involved estimating the unobservable "fecundity" of couples based on a parametric reproduction technology (Rosenzweig & Paul Schultz (1987)). All these studies find that fertility causally reduces the educational attainment of children.[3] In contrast to the attention received by the effects of fertility on education received, few papers have focused on the converse relationship.

A notable exception is Leon (2006), who investigates the effects of educational attainment on fertility in the United States between 1950 and 1990. He uses compulsory schooling laws as an instrument for estimating the returns to education (cf. Angrist & Krueger (1991); Acemoglu & Angrist (1999)). Using a similar IV estimation, Leon finds that female education has a sizable effect on fertility. According to his IV estimation, having three more years of education reduces completed fertility by one child. Given that the average completed fertility in the sample is about 2.5 children, this effect is substantial.

[3] All of these papers are closely related to a parallel strand of this literature that explores the effects of fertility on labor market outcomes (see e.g., Rosenzweig & Wolpin (1980a); Angrist & Evans (1998)). A complete summary of the literature is given by Schultz (2005).

Although our papers aim at similar questions, there are a number of dimensions in which they differ. I use a different fertility measure, one that is based on the birth histories of the mothers. In addition, I examine the contemporaneous effect on the fertility of women who were of reproductive age at the moment the policy was introduced, and I study a period during which this demographic transition was taking place. (By 1920, total fertility rates in the United States were already close to modern standards.) Finally, our papers also differ in methodological aspects. Leon (2006) uses an IV strategy that focuses on women who were forced to remain in school as a consequence of compulsory schooling laws.[4] My empirical strategy, however, is based on women living around the borders of jurisdictions where the CSLs changed and comparing fertility before and after that change (cf. Card & Krueger (1994)).

3 Compulsory Schooling in the United States

The notion that the education of children is desirable for a society is relatively old. In the United States, the first schooling laws trace back to the pilgrims of New England. However, it was not until after independence that states attempted to increase education by passing school laws. During the 1830s and 1840s a "free school" movement successfully eliminated tuition fees (technically, "rate bills"[5]) in most northern and eastern states. After

[4]Note that the validity of his IV strategy relies on the assumption that education is the only channel through which compulsory schooling affects fertility. But this may not be the case if, as suggested by the demographic literature, mass schooling affects other outcomes related to fertility

[5]A recent paper by Go (2008) analyzes the political economy of the elimination of 'rate bills' (sort of tuition fees). The abolition of 'rate bills' implied that richer areas in the

the achievement of free schools, reformers focused on a more ambitious goal: compulsory school attendance for all children.

Abolishing rate bills was difficult, but compulsory schooling proved to be a major challenge. Reformers and most parents recognized that mass schooling was beneficial for society because it contributed to good citizenship. It was the compulsory aspect that was disturbing. In the long tradition of liberty in the United States, some objected to the state's intervening to tell parents how to raise their children. Probably more important was that the opportunity costs of schooling—in terms of forgone earnings of child labor—were substantial (Fishlow (1966); Parsons & Goldin (1989);Moehling (2005)).

In response to the strength of persuasive reformers like Horace Mann, Massachusetts in 1852 became the first state to pass a compulsory schooling law. Although slow at first, the diffusion of legislation was steady from north to south (see Figure 2). By 1920, the law had spread to all the states in the Union, turning the country into a world leader in mass schooling.[6].

Exactly what part compulsory schooling played in this picture is not clear. A naive look at the enrollment rates reveals that overall enrollment increased substantially between 1850 and 1920, exactly at the same time compulsory schooling was becoming widespread (Figure 3). Of course that this coincidence is just a necessary condition for compulsory schooling to have had an effect on fertility.

Another necessary condition is that CLSs did, in fact, increase enrollment. Surprising as it may seem, there is a strand in the literature that holds

state had to cross subsidize poorer regions.

[6]Richardson (1980) explores the difference in timing of enactment of the laws using a number of state controls

compulsory schooling laws did not play a major role increasing enrollment (See Ensign (1921); Goldin (1999)). According to this view, such laws were enacted as a *consequence* of high enrollment (Landes & Solmon (1972)) and had, only a marginal effect on school enrollment when combined with child labor legislation (Margo & Finegan (1996)).

Before proceeding to study the effect of CSL on fertility, I need to find whether compulsory schooling laws did actually increase school attendance. I will do this after first describing the identification strategy in detail.

4 Identification Strategy

The federal structure of the United States provides us with a natural way of testing the effect of policy changes. Since states enforce different laws, the evaluation of a policy may seem easy: simply observe the evolution of the outcome variable in the state that passed the law and compare it with a state that did not. The problem with this naive strategy is that the economic, social, and demographic characteristics of the states also differ. It is thus difficult to identify which is the source of variation that explains changes in the outcome variable.

In the language of experiments, the problem here is that the treatment and control groups are not truly comparable. In other words, it is not sensible to assume that, in absence of CSLs, Massachusetts would have looked a lot like Mississippi. By 1850 Massachusetts already had much higher school enrollment, limiting the potential for further improvement. In contrast, southern states—and Mississippi in particular—were lagging behind. Even without the advent of compulsory schooling, it is clear that enrollment in Mississippi would have grown faster than enrollment in Massachusetts during that decade.

The identification strategy pursued in this paper is to define an appropriate control group. In order to do this, I exploit the discontinuity that borders offer by limiting my attention to regions that are close to the borders where the change in legislation occurred. Rather than comparing the fertility and school attendance of people living in Massachusetts and Mississippi, I compare the outcomes between people living in Berkshire county (MA) with people living just across the border in Columbia county (NY). When possible, I will use even township level data (see Appendix A).[7] A complete county map for these borders between 1860 and 1910 and for the townships between 1860 and 1870 is presented in Figures 5 and 4. The construction of the county and township sample is described in Appendix A.[8]

Once the treatment and control groups are correctly specified, I will apply a standard difference-in-differences (DID) strategy. In a nutshell, the idea is to compare the changes in the average outcome variable in both the treatment group and a control group. If y denotes the outcome of interest, then the DID estimator simply captures the change of the outcome variable in the treated group with respect to the similar change in the control group. More

[7] A number of papers have used proximity to the border in order to assess the impact of legislation. See Card & Krueger (1994); Holmes (1998).

[8] To illustrate the potential pitfalls of using aggregate data and to reproduce earlier findings, I will sometimes use two samples other than the ones described in the text. I will also conduct my empirical analysis on the "border state" sample (i.e. the sample of states that were on a legislative border in a given year) and the full sample.

formally,

$$DID = (y_{Treat,After} - y_{Treat,Before}) - (y_{Control,After} - y_{Control,Before}) \tag{1}$$

where $After$ and $Before$ refer to the time the CSL was enacted. This is easily implemented with a regression that includes dummies for the treatment, the period, and an interaction.

5 Did Compulsory Schooling Increase School Attendance?

In order to answer to the question of whether CSLs had any effect on education and fertility, I will compare several education outcomes of children living near the border; before and after the law was implemented. The main specification that I will regress is

$$y_{i,s,t,g} = X_{i,s,t,g}\beta + \psi CSL_{s,t} + \tau_t + \lambda_s + \mu_g + \epsilon_{i,s,t,g}, \tag{2}$$

where $t = 1850, ..., 1910$. The sample chosen is that of children aged 8 to 14 with both parents living in the household. For each border, I will use only the census data corresponding to the years exactly before and after the legislation affected those residents. For instance, for the border between Massachusetts and New York, I will use the 1850 and the 1860 censuses, because the legislation was passed in 1852. Restricting my attention to the period immediately before and after the treatment is not only intuitive but also allows me to avoid the problems that multiple periods cause for difference-in-differences approaches (Bertrand *et al.* (2004)).

In regression (2), $y_{i,s,t,g}$ refers to school attendance. Individual controls $X_{i,s,t,g}$ include a set of demographic variables (race, age, and gender) and their interaction with the year; labor force participation of the mother; and a set of dummy variables for father's occupation. I also control for whether the household is foreign by indicating the nationality of the father. Finally, μ_g stands for a series of geographical controls. These controls make sure that the we will comparing neighboring individuals also in a east–west sense. For that, I will use a control for the "segment" of the border in which the individual resides with reference to bordering states, counties, or townships (see Appendix A). Again, these geographical controls have ensure that I will only be comparing people who reside on neighboring regions of the border.

5.1 Data

This paper combines micro-data from the U.S. federal censuses with information about compulsory schooling legislation in the United States. The latest version of IPUMS includes county and minor civil division data from the U.S. Federal censuses (Steven Ruggles (2004)). The definition of the schooling laws is obtained from Fishback (2008).[9]

For the education regressions I use the sample of children in treated ages, 8 to 14, from the censuses of 1850 through 1910. Because many of the controls in the regression refer to the household head or to the child's mother, I will

[9] An alternative dataset on CSLs is provided by Goldin & Katz (2008). The two coincide in all cases but Louisiana. See the appendices for a detailed discussion of the construction of the variables.

restrict my attention to those children whose parents are both present in the household at census date.

Before analyzing the effect of compulsory schooling on fertility, I need to establish that CSLs had a causal effect on increasing school attendance. Tables 1 and 2 contain the summary statistics for the estimating set of variables used in school enrollment equation at (respectively) county and township level. All the variables are reported for children affected by the legislation (ages 8–14) whose parents are present in the household.[10] The school attendance variable originally recorded whether the child had attended school in the year previous to the census. I will refer to it as school attendance or school enrollment interchangeably.

The first thing to notice about school attendance is that it was already quite high at the moment the laws were enacted (Landes & Solmon (1972)). Despite this, it is also evident that changes in school attendance differ depending on whether the child is on the side of the border that enacted compulsory schooling. Figure 6 shows that school attendance increased faster on the CSL side of the border. Furthermore, the average school enrollment for different age groups also varies consistently with the assumption that the laws increased enrollment. That is, the increase in school attendance in the CSL states occurs exactly for the ages (8–14) covered by the law. In contrast, the change in enrollment of slightly younger or slightly older children is essentially the same on both sides of the border. These facts all support the hypothesis that CSLs effectively increased school attendance.

[10]The provisions of the legislation depended on the state. Early laws were usually modeled after Massachusetts where it was mandatory for children between the ages of 8 and 14 to attend school. Later laws, often extended the mandatory ages.

In general, Table 2 shows that people living on either side of the border look very much alike. There are, however, some differences in the occupations of the children's fathers. The side of the border without the law seems to be slightly more rural: fathers are more likely to be farmers than operatives or clerks. Enrollment differs substantially between some groups. Whereas white children have an average enrollment of 83 percent, this figure reaches only 50 percent for black children.

5.2 Main Results

Regression analysis of the effects of compulsory schooling on education is presented in Tables 3 and 4 for (respectively) counties and townships in the border. Both tables demonstrate that school attendance increased substantially after the introduction of compulsory schooling. At the township level, the main specification reveals a causal effect of compulsory schooling laws on school enrollment of around 7 percent. It is interesting that the estimate of the effect of the law is quite stable irrespective of the other controls added. Starting from a simple specification with no additional controls, adding state and time fixed effects and then a full set of controls barely affects the point estimate or its statistical significance.

In the tables, I also make a distinction between "early" and "late" borders. Although my regressions control for time effects through the dummies for year and border segment, there are reasons to believe that New England may have behaved differently than the rest of the states. After all, compulsory schooling laws were enacted much earlier in New England than elsewhere in the United States. Although in both cases (county and township) the effects are significant, the early borders seem to have increased school attendance by more than the late borders.

I also report, when possible, an individual regression for black children. Insofar as compulsory schooling laws should affect more poor children, we expect the effect of the legislation to be large for them. The point estimate is indeed substantially larger, indicating an increase in the probability of attending school of about 30 percent as a consequence of the law. These results are consistent with black children catching up, in terms of school attendance, with their white counterparts.[11]

5.3 Robustness Checks

A major assumption of my identification strategy is that the control group is correctly specified. In other words, were it not for the treatment, the treated group should look exactly like the control group. This means that, in a period were the treatment did not occur, we should not be able to find any effect. A natural way of testing this in the context of my model is to run regression (2) but for the period immediately *before* the legislation was passed–that is, to assume that the treated group received the treatment at a moment of time in which it did not. Towards this end, I construct a set of "placebo" compulsory schooling dates for the decade immediately prior to

[11]The fact that black children would reap most of the benefits of compulsory schooling probably deterred the passage CSLs in the south. When considering a CSL for Georgia in 1909, one member of the Georgia legislature stated that "such a law would mean increased usefulness of the Negro in the states, and the law would affect Negro children as well as white, and the results would be more beneficial to the Negro population and more to the detriment of whites". *The Washington Bee*, Sept. 4, 1909. The law was finally defeated.

the decade in which the treatment actually took place.[12]

The results of this placebo estimation are presented in Table 5. For the same categories as in the main specification, the effect of the placebo compulsory schooling is small, negative and statistically insignificant. This holds for both early and late CSL borders, and whites and blacks alike. From these regressions it can be concluded that the increase in enrollment on the CSL side of the border occurs exactly during the decade when the legislation was passed. Thus the difference-in-differences estimator is *not* reflecting the effect of different trends for the treatment and control groups.

Another possibility is that our estimator simply reflects a generalized increase in enrollment that is unrelated to compulsory schooling legislation. If that were the case, then we would observe school attendance to increase not only for children of ages covered by the CSL, but also for the rest of the school-age population. In order to test for that possibility, I ran a model similar to the baseline model but including all individuals aged between 5 to 20 and allowing the legislation to have a different effect for different ages. The individual coefficients and estimated standard errors are plotted in Figure 7, which shows a discrete jump in the effect of legislation exactly at ages 8 and 14. Between these two ages, the effect seems to be constant and approximately equal to 10 percent. When I perform a similar exercise using placebo CSL, the effect becomes equal to zero for all ages. This too, is consistent with the legislation affecting the differential school attendance of children aged 8 to 14. Moreover, the effect is not observed when I look at a

[12]For instance, Vermont passed its compulsory schooling law in 1867. My main specification uses the border between Vermont and New York/New Hampshire for the 1860 and 1870 censuses; my placebo CSL compares the same border between 1850 and 1860.

different period of time.

Finally, I investigate the reasons why my paper finds a result that had previously not been observed in the literature. In order to do so, I run regression (2) but using all the available observations—not just those that correspond to the border. Comparing Table 8 with Table 3 reveals a story that is consistent with previous studies on compulsory schooling (Landes & Solmon (1972)). Using the full sample or the state sample and ignoring state fixed effects, compulsory schooling is correlated with a strong positive coefficient of about 20 percent. However, as soon as a full set of state dummies is added, we find a *negative* impact of about 5 percent. The reason for this is simple. The original identification was obtained from pooling a cross-section of states with very different school enrollments. When I restrict my attention to the changes in school attendance that occur *within* each state, the effect actually becomes negative. This is so because states that passed compulsory schooling laws already had very high enrollment rates of 80 percent, compared to an average of 60 percent for states without the law. Even if legislation increase schooling in CSL states, this effect will be overshadowed by the convergence in school enrollment experienced by the other states. Yet we can observe that, as one moves to narrower geographical definitions (i.e. “border states”, “border counties”) the negative effect of compulsory schooling vanishes.

The previous research of Landes and Solmon did not have the benefit of the detailed geographical data used in my regressions and had to rely instead on aggregate data. In these aggregate regressions, the authors found that compulsory schooling was associated with a higher enrollment rate both after *and* before the legislation was passed. This observation led them to conclude that CSLs were ineffective and only came about only after they were no longer needed. If I were to take a similar approach with the census micro-data, I would arrive to the same conclusion. However, the border assumption, allows me to isolate the causal effect of CSLs. I find that, even though enrollment was high already at the moment compulsory schooling was adopted, these laws had the effect of increasing school attendance by about 7 percent. This effect is not small; it represents about one fourth of the gap from full school attendance.

6 Did Compulsory Schooling Reduce Fertility?

Having established that laws effectively increased education, let us now turn to the study of fertility outcomes. There are two types of fertility measures used throughout this paper, "stock" and "flow" fertilities [13].

Stock fertility refers to a measure of the number of children at a specific moment in time. These are common outcomes when working with census data. At census time, a mother is asked about the number of children (or the number of surviving children) she has ever had. The problem with these measures is that they put equal weight on all fertility decisions, regardless of when they were made. Furthermore, owing to the stock nature of these variables, they move sluggishly over time. Past fertility behavior would thus

[13] The term flow fertility is borrowed from La Ferrara,Eliana *et al.* (2008), who define a similar measure.

attenuate any sharp response in fertility due to, say, a new social policy.

In order to isolate the timing of fertility changes, I construct a so-called flow fertility measure based on the ages of own children living in the household.[14] The procedure is straightforward. For each household, I identify the children living with their mothers; Then subtracting their ages from the census date, I infer their birth years. Based on this, I construct for each mother a variable (*Births*) that is equal to 1 in the years in which their children were born and 0 otherwise. When constructing the fertility histories of mothers I consider as many as 14 years before the census date.

Both stock and flow measures of fertility may exhibit some biases that stem from the way they are constructed. Both measures refer to the number of children living in the household at census date.[15] If children are not present in the household for some reason (child mortality, child fostering), then the estimates of fertility will be biased downward. With regard to the flow fertility measure, the farther away one moves from a census date, the more likely it is that the child had already left home. This is precisely the rationale for cutting off 14 years before the census date.[16]

In principle, there is no reason why the downward bias in fertility should affect one side of the border more than the other. In other words, if the

[14]This strategy is widely used by demographers when constructing fertility estimates for countries with poor vital registration (U.N. (1983)). Recent research in economics has also used a similar measure (La Ferrara,Eliana *et al.* (2008))

[15]The U.S. federal census did not ask questions about fertility except in 1900 and 1910. This is why even the "stock" measures of fertility refer only to the number of children living in the household at census date as inferred from the census schedules.

[16]We can roughly check the validity of this assumption by looking at the surviving children variable in the 1900 census. In this census, mothers were directly asked to report the number of "surviving children". For white mothers with children of 14 years or less the number of children reported by the mothers coincides about 90 percent of the times with the number actually living in the household.

identification strategy is appropriate, then the downward bias on the fertility measure should increase only the standard errors without affecting the estimator's consistency.

If all the measures have the same problems, flow fertility stands out because it has a clear benefit: it allows us to pinpoint changes in fertility occurring exactly at the time of the policy innovation. For doing that, I regress the number of births of mother i at time t on a number of covariates and on a $CSL_{s,t}$ dummy. In this context, t stands for the period before the census date. ($t = 0, 1, 2, ..., 14$):

$$Births_{i,s,t,g} = X_{i,s,g}\beta + \psi CSL_{s,t} + \tau_t + \lambda_s + \mu_g + \epsilon_{i,s,t,g}. \quad (3)$$

The covariates in $X_{i,s,g}$ include age, nativity, race, urban status, and a number of dummy variables for the occupation of the husband. Note that because I observe mothers only at one specific census date, individual characteristics do not vary over time. The time, state, and geographical dummies are defined exactly as in the education regressions. As set up in equation (3), the regressions do not take advantage of the fact that the data comes from a panel of mothers. One could do so by running a panel regression with mother fixed effects, using CSL and time dummies as covariates.

Although appealing, the approach just described has some drawbacks. The most important refers to autocorrelation which may bias the standard errors downward (Bertrand *et al.* (2004)).[17] A simple solution for this prob-

[17]In a nutshell: the idea is that, by the very nature of the DID estimators, the laws change sluggishly over time and, once enacted, a law tends to remain in force over time. This generates autocorrelation in the treatment variable. When combined with relatively large number of periods and a highly autocorrelated dependent variable, the bias in the

lem is to collapse the data into a pre-treatment and a post-treatment period. By reducing the number of periods to two, the autocorrelation problem is mitigated.

I implemented a solution to the autocorrelation problem by constructing the accumulated fertility of each mother p periods before and after the shock ($Fert(p)_{i,s,t,g}$). Then I ran a regression of the effect of compulsory schooling on fertility for mother i living in state s before and after the shock occurred.

$$Fert(p)_{i,s,t,g} = X_{i,s,g}\beta + \psi After_t * CSL_s + \delta After_t + \lambda_s + \mu_g + \epsilon_{i,s,t,g}. \quad (4)$$

where $p = 1, 3, 5$ and $t = Before, After$. Here $After_t$ is a dummy variable that takes value 1 only if the observation comes from after the treatment.

Aside from the specifications already outlined for flow fertility, I also report some estimations using the stock measures of fertility. For that I use a regression similar to (2). In this case, $y_{i,s,t,g}$ refers to stock fertility measures– that is, the number of own children and the number of own children aged 5 or less, living in the household at census date. This applies to the set of mothers aged 15 to 49 whose husbands are present in the household. In this case, $X_{i,s,t,g}$ includes a set of demographic variables (race, age, gender), foreign status and labor force participation, and a set of dummy variables for husband's occupation.

standard errors could be substantial.

6.1 Data

The data for the fertility regressions is constructed from all the available federal census micro-data since 1850 until 1920. Because the focus is on marital fertility, I restrict my attention to the sample of married women whose husbands are present in the household at census date. The sample is further restricted to women aged between 15 and 49 when the new legislation was enacted; Thus, I considered only women of reproductive age who may have been affected by the legislation.

The summary statistics of the data are presented in Tables 9 and 10 (respectively) for stock and flow fertility. Most of the fertility measures tend to be lower in states that adopted compulsory schooling. The township sample is particularly revealing. Examining the probability of births for the 15 years before census date, it makes clear that at least for the 10 years following the new law's introduction, the probability of giving birth is lower in the sample of women living on the CSL side of the border (Figure 8). Compulsory schooling laws are correlated with lower fertility in the full sample, but is there a difference between old and young women?

It is expected that younger women would react more to the CSL treatment, since their fertility decisions have not yet been made. In contrast, older women have already chosen their fertility and are stuck at that level. In order to illustrate this point, I considered the change in fertility, from 5 years before the treatment to 5 years after for young and old women both. Figure 9 reflects this change for young and old women. As expected, the change is positive for women at the start of their fertility spell and it is negative for older women, who are close its end. Young women in CSL states increased their fertility much less than young women in non-CSL states. However, the effect on older women is imperceptible. These results are consistent with the hypothesis that women reduced their fertility as a consequence of compulsory schooling laws. Regression analysis would allow us to see this more clearly.

6.2 Main Results

As with the education variables, I present the results of regression analysis both for counties and townships (respectively) at the border in Tables 11 and 12. In each table, I present two sets of regressions. First, the regressions are performed using each individual year in the fertility history of the mother– that is, using the number of births in each of the years before the census took place. I also report the regressions for fertility one, three, and five periods from CSL in an attempt to attenuate the problem of autocorrelation in the treatment dummy.

For townships, the effect of compulsory schooling on fertility in the full model is negative and significant both using year observations and collapsing the data. If we take the change in accumulated fertility 5 years from CSL as a benchmark, legislation reduced fertility by a factor of about 0.1 as a consequence of compulsory schooling laws. Compared to an average of 0.6, this effect is high (15%) but does not seem implausible. Table 11 presents a number of checks in order to make sure that the results are not driven by a specific age or ethnic group. Specification (8) in the table implements a panel data regression with mother fixed effects.

The intuition that young women should be more affected by the law is also confirmed by the data. Columns (4) and (5) report the full specification separately for young and old women. The effect of CSL on younger women is about 50 percent larger than for the pooled sample. For older women, the effect is about half the average effect and is mostly insignificant.

Finally, I explore the incidence of compulsory schooling for foreign and white women in order to see whether the results were driven by a combination of groups with potentially different response to the treatment. If we exclude black women from the sample, the point estimates are essentially unaffected. A separate regression for foreign women reveals that this group reacted more to the effect of compulsory schooling.

6.3 Robustness Checks

I also ran the fertility regressions (using the stock measures of fertility) both at the township and county level; The results are presented in Table 13. Although the stock fertility measures are quite noisy, the picture obtained from these regressions is similar to my main specification. Compulsory schooling has a negative effect on the number of children living in the household, and the effect persists even when controlling for individual and household characteristics. The coefficients are similar but insignificant when I run the same regressions on the border townships. The other stock variable–number of children aged less than 5–has the wrong sign and is insignificant both in county and township border areas.

Given that these variables are not reported in the census, they are by construction similar to summing up the births of the mothers using the children's ages. The main difference with my "flow" fertility measure is that I compare the fertilities just before and just after the CSL treatment occurred. These measures compare either the changes in number of total children in the family or the births in the 5 years before census, independently of when the treatment took place. If we were to mix births that occurred before treatment with births that occurred after, then these measures would reveal only weakly the effect of compulsory schooling laws.

I also check for trends in the data using a placebo law (as in education regressions); The results are presented in Table 14. As with education, the effect of compulsory schooling is seen only at the time when the actual laws were enforced. A similar regression on data 10 years before the CSLs fails to find any significant effect.

7 Conclusion

Every country with high per capita income today broke free from Malthusian shackles at some point after 1750. Growth no longer translated into larger populations; incomes per head grew. The fertility transition was crucial for this change. Yet for all its importance in models of unified growth, the causes of the rapid fertility decline in most Western countries after 1830 have remained unclear. Although the Princeton Fertility Project argued that cultural and linguistic factors were key, there are important challenges to the aggregative method employed (See Brown & Guinnane (2003)). The quantity–quality trade-off argument, prominent in the literature since the contributions of Becker & Lewis (1973) and Becker & Barro (1988), is of doubtful value given the history of changes in wage premia (Galor & Moav (2006); Mokyr & Voth (2006)).

This paper provides empirical evidence that state intervention in the form of compulsory schooling laws was an important factor behind the fertility decline. I use micro-data from the U.S. census to identify cleanly the effects of schooling, using the border effect as my source of identification (in the spirit of Card & Krueger (1994)). If compulsory schooling mattered then part of the effect should come from increasing enrollment, and this is what my evidence suggests. Based on the geographic identification strategy, this study finds that compulsory schooling laws effectively increased enrollment by 7 percent on the side of the border that received the treatment. The finding is robust to the inclusion of individual, household, and geographical controls. It holds true for the states that passed the law early as well as for those that did so late. Furthermore, the effects of the law concentrate on children from age 8 to 14 years (i.e., those ages covered by the legislation). The evidence also reveals that the effect of education on school enrollment occurs exactly at the moment when compulsory schooling was enacted and is not due to differences in previous trends.

In terms of fertility analysis, I find that mothers decreased their fertility by 0.1 (15% of average fertility) as a consequence of the CSLs. These results are also robust to the inclusion of a number of individual, household, and geographical controls and, some alternative fertility measures.

This paper contributes to two important strands in the literature. First, it contributes to the discussion of the effect of nineteenth-century social legislation. Education historians have often argued against the effectiveness of CSLs based on both high initial enrollment rates and anecdotal evidence of poor enforcement. Although these two effects may have been present, this paper is the first to find a discernible effect of compulsory schooling laws increasing enrollment. Furthermore, my estimates suggest that the effect was sizable, especially for some disadvantaged groups. How much credit must be given to CSLs for the future success of the American mass education system remains an open question.

Second, the paper also contributes to the development literature on fertility transition and growth. A growing theoretical literature studies how compulsory schooling and child labor laws may hasten the transition to lower fertility and higher growth. This paper is also the first to quantify the short-term effects of a social policy on fertility. The effects found here are only a lower bound on the overall effect of education. I have not explicitly considered such effects for the generation that was affected by CSLs. Given that one of the American education system's "virtues" was the high level of gender equality (Goldin (1999)), one can well suppose that the long-term effects of compulsory schooling are much greater.

Much of contemporary policy advice focuses on the importance of educating women and on the effect this has on fertility. My findings suggest that compulsory schooling laws can help reduce the total number of children borne by another channel—by shifting the balance of costs and rewards for parents. In this sense, compulsory schooling laws may affect fertility more rapidly and more comprehensively than implied by policy recommendations that focus on the educational level of the parent generation.

A Geographical Samples

My identification strategy relies on the definition of the relevant sample according to a geographical criterion. Throughout the paper I report results for 'All States', 'Border States', 'Border Counties' and 'Border Townships'. When I refer to the 'All States' sample, I consider all the continental U.S. states including the District of Columbia. By 'Border States' I imply all the states that are in the border between the states that had the legislation and those that did not. For example, in 1860, only Massachusetts had a CSL (passed in 1852) and thus, the 'Border State' sample for 1860 includes Massachusetts plus the 5 states that have a border with Massachusetts (New Hampshire, Vermont, New York, Connecticut and Rhode Island). The variable is similarly defined for other years. Similarly, the 'Border County' sample includes only the counties that are in the border between states with the law and those with not. As the border expands west (1880, 1900 and 1910), there are some cases in which counties were created later, disappeared or merged. In these few cases, I refer to the county structure as it existed at the time of study. This is usually only a problem in the west and mountain regions, which are marginally used in the paper.

Finally, the narrowest geographical border is the 'Township border'. This restricts the attention to observations pertaining to the townships, or more generally, 'Minor Civil Divisions'(MCDs) at the border. The matching of the MCDs was done following the U.S. Census Bureau maps U.S. Census Bureau (2008). A major problem with this definition is that states have very heterogenous subcounty divisions. In most of the north and midwest, counties are divided into townships, which are easily matched across censuses. For northern states, there was an almost perfect correlation between the MCD's reported in the data and those published by the U.S. Census bureau in 2000. Problems arise with southern states, which have a number of overlapping territorial divisions which, furthermore, change between censuses. The most extreme example is Tennessee, where county are subdivided into numbered 'civil districts', that are not necessarily constant over time. For these reason, I will restrict my township border definition to the states in which matching can be done reliably, that is, MA, VT, NH, NY, CT, RI, PA, OH, IN, MI, WI, MN, IA, IL, SD, MO and NE.

The border between CSL and No CSL states may sometimes include a major metropolitan area. I decide to exclude them from the county and border definitions in order to keep the sample balanced. Specifically, I exclude Philadelphia-Trenton(PA/NJ) and Cincinnati(OH) in 1880; Baltimore(MD) and St. Louis(MO) in 1900 and Memphis(TN) in 1910.

I also define a variable that indicates from which part of the border/year the observation comes from. These are: MA-RI, 1860; MA-CT, 1860; MA-NY, 1860; MA-VT, 1860, MA-NH, 1860; VT-NY, 1870; VT-NH, 1870; DC-VA/MD, 1870; CT-RI, 1880; NJ-PA, 1880; NY-PA, 1880; OH-PA, 1880; OH-WV, 1880; OH-KY, 1880; OH-IN, 1880; MI-IN, 1880; IL-WI, 1880; IA-WI, 1880; MN-WI, 1880; KS-NE, 1880; KS-MO, 1880; KS-CO/OK, 1880; CA/NV-AZ/UT/ID/OR, 1880; PA-DE/MD, 1900; WV-MD/VA, 1900; KY-VA/TN, 1900; NE-IA/MO, 1900; CO/NM-OK/TX, 1900; NC-SC, 1910; TN/GA-MS-AL, 1910; AR/LA-MS, 1910 and OK-TX, 1910.

B Variable Definition

B.1 Fertility variables

The stock measures of fertility (*nchild* and *nchlt5*) are directly provided by IPUMS and no further transformations are needed. However, as in the regressions I will be using a set of controls from the household, I will need to restrict my attention to women whose husband is present at the house. In order to do so, I discard all the cases in which the matching between the husband and the wife is 'doubtful', as reported by IPUMS (*sprule* different than 1 or 2). For the flow fertility measure I further need to obtain the ages of own children. I do so using the *momloc* variable. Finally, I limit my analysis to the mothers who live in the border and who were between 15 and 49 years at the time of the treatment.

- **Number of Children(*nchild*)**: Number of children living with their mothers at census day (IPUMS constructed).
- **Number of Children under 5 (*nchlt5*)**: Number of children under 5 years living with their mothers at census day (IPUMS constructed) children ever born to each ever-married woman and who are still alive, regardless of whether they are living in the household or not. This variable is in the census schedule and it is only available only for the 1900 and 1910 children ever
- **Births at year "X" (*Births(X)*)**: Number of children born in year "X". I construct this variable based on the ages of the own children living in the household. Subtracting their ages from the census year I construct the fertility of married women for the 15 years before census date.
- **Accumulated births in the "X" years from the treatment treatment occurred (*Fert(X)*)**: Number of children born during the "X"

years following the introduction of the CSL. I just sum *fert* over the the "X" years that follow the introduction of the legislation. If the mother is a resident of Agawam, MA (in the border with CT), the treatment time is 1852. Therefore, *Fert(X)* will be the number of births to this mother between 1853 and 1857. Similarly, *Fert(-X)* will be the number of children born in the 5 years prior to the passing of the law. For the mother considered above, that would be the number of births between 1847 and 1851. In the text, I will use 3 dates, 1, 3 and 5 years from the census date. The reason for doing this is that the longer the period, the fewer the number of mothers that I can observe. p=5 requires that I observe the fertility of a mother for at least 11 years.

B.2 Other Controls

Most of the variables used in the paper are self-explanatory (age, race and foreign status). The cases in which the construction is not straghtforward are explained below. the child is attending school (*school* variable in IPUMS) or declares "at school" as his occupation. I define this variable for children ages 8 to 14.

- **School Attendance**: Dummy variable equal to 1 if the child is attending school. Defined as School Attendance but including the cases in which *school* is equal to zero and occupation (*occ*) is set to 'at school'.
- **Months Sch. Attendance**: Number of months of school attended in the school year ending on June 1st, 1900. Only available for 1900.
- **Some Literacy 10-14**: Dummy variable equal to one if the child knows how to read, to write, or both. The universe is the group of children between 10 and 14.
- **Child Labor 10-14**: Dummy variable equal to one if the child is employed in any occupation. For 1900, includes all cases in which *occ* responses are between 1 and 300. For 1910 and 1920, it includes all the *occ* responses between 1 and 997. This is due to coding changes in the IPUMS *occ* variable for 1910 and 1920. The universe of this variable is the group of children aged 10 to 14.
- **Urban Status**: Dummy variable equal to 1 if the location in which the person resides is a incorporated town.

Figure 1: TFR and School Enrollment, 1880-1920

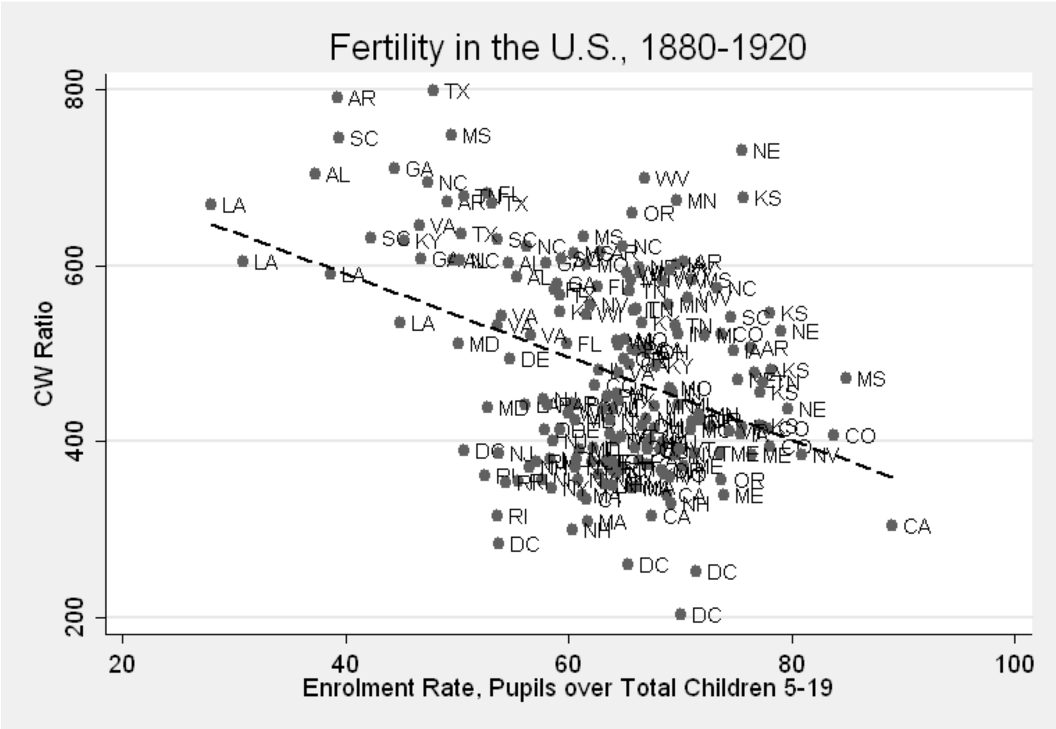

Figure 2: Compulsory Schooling Laws

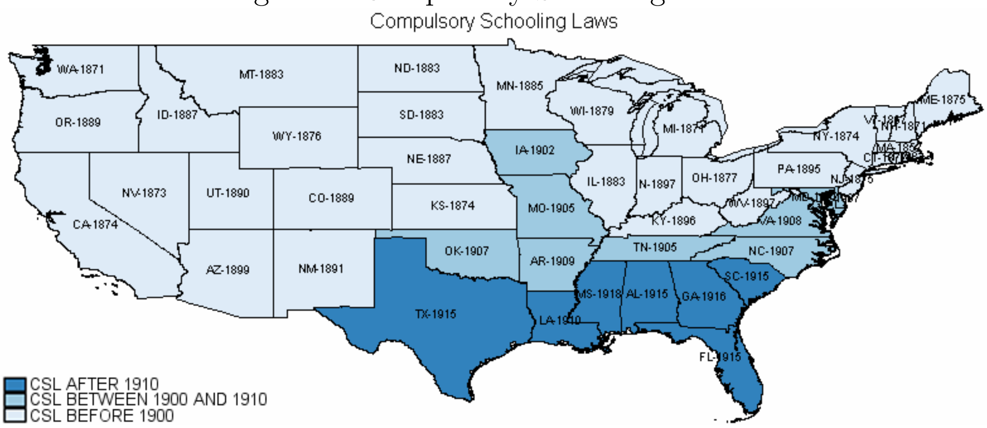

Figure 3: Compulsory Schooling and Fertility, 1850-1920

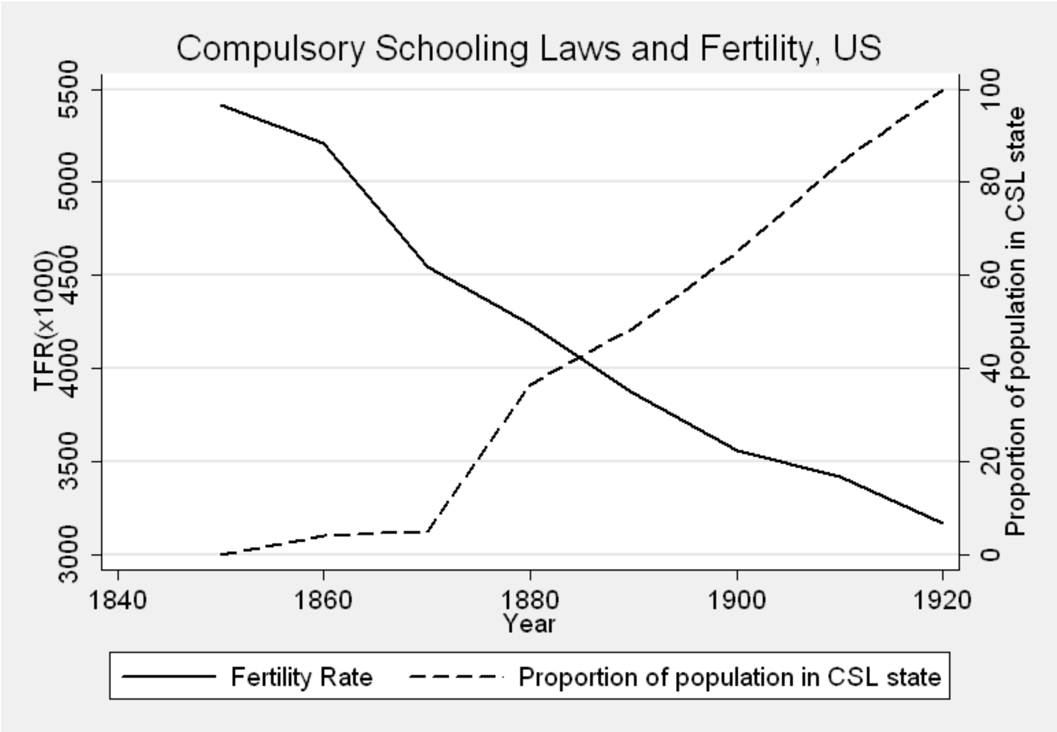

Figure 4: Evolution of the CSL Border - 1860-1870 - By Township

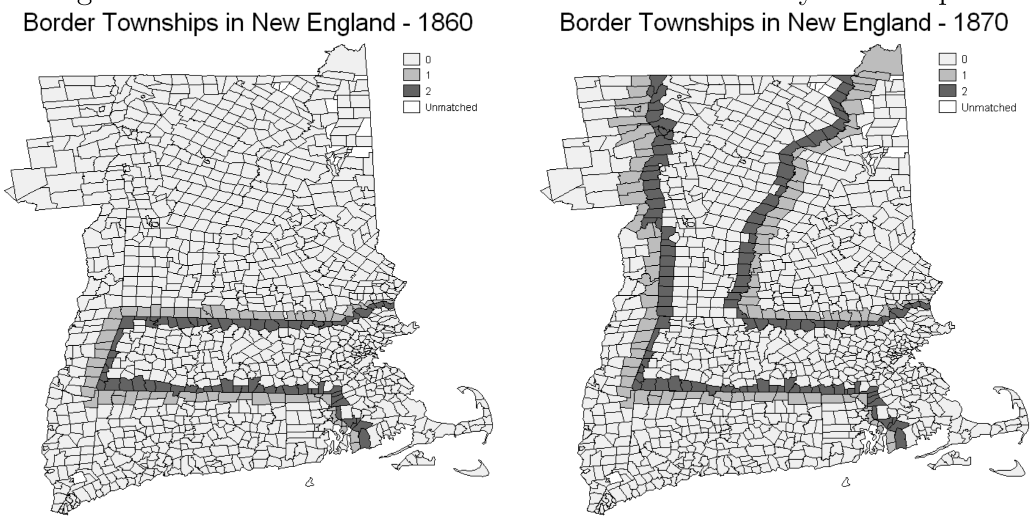

Figure 5: Evolution of the CSL Border - 1860-1910 - By County

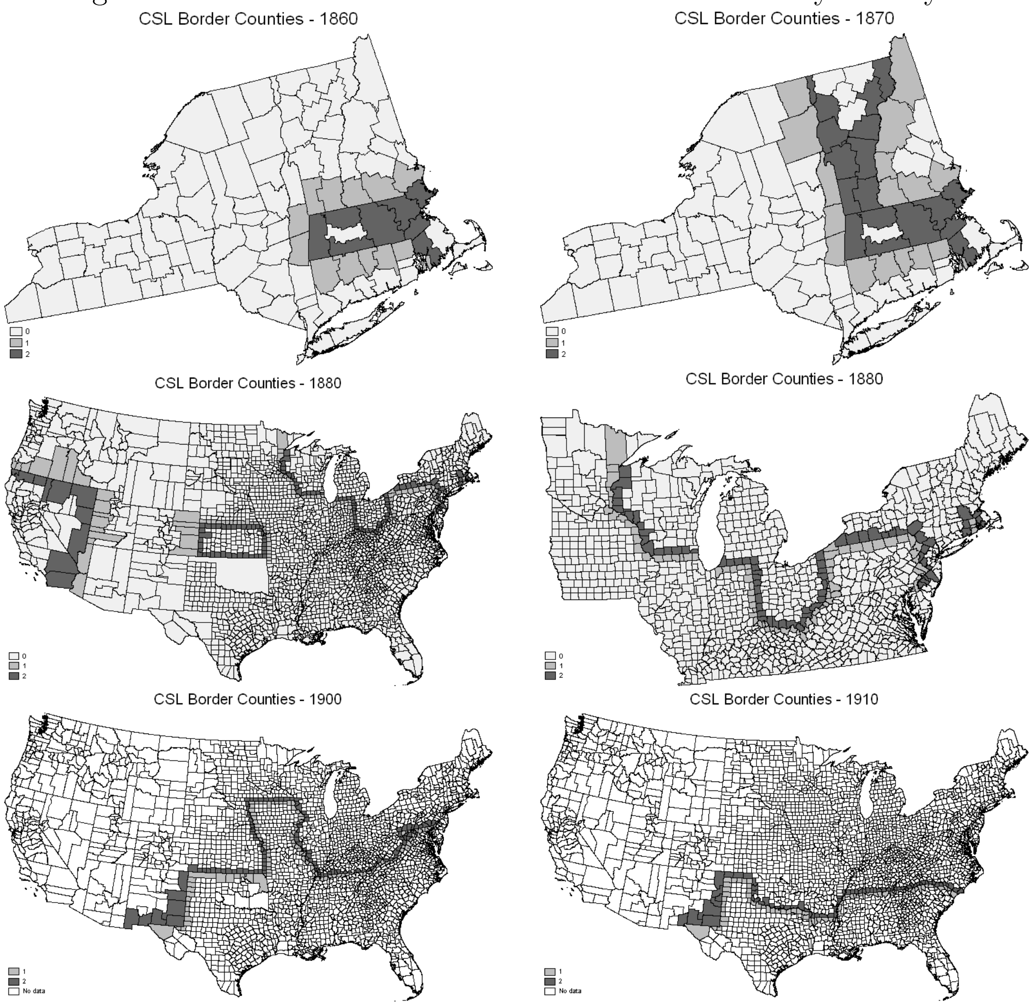

Figure 6: Change in School Attendance by Age: by CSL

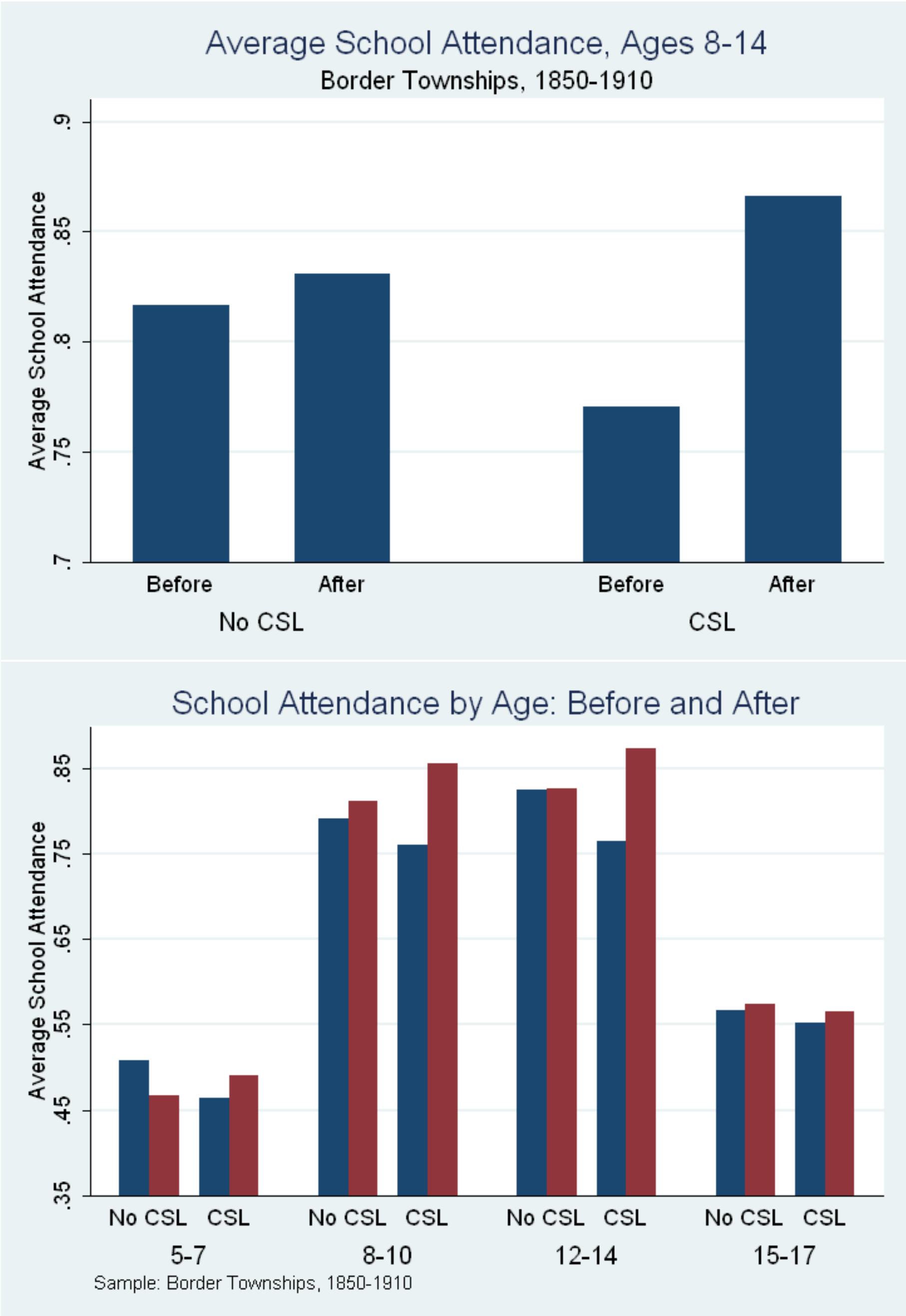

Figure 7: CSL and School Attendance: by Ages

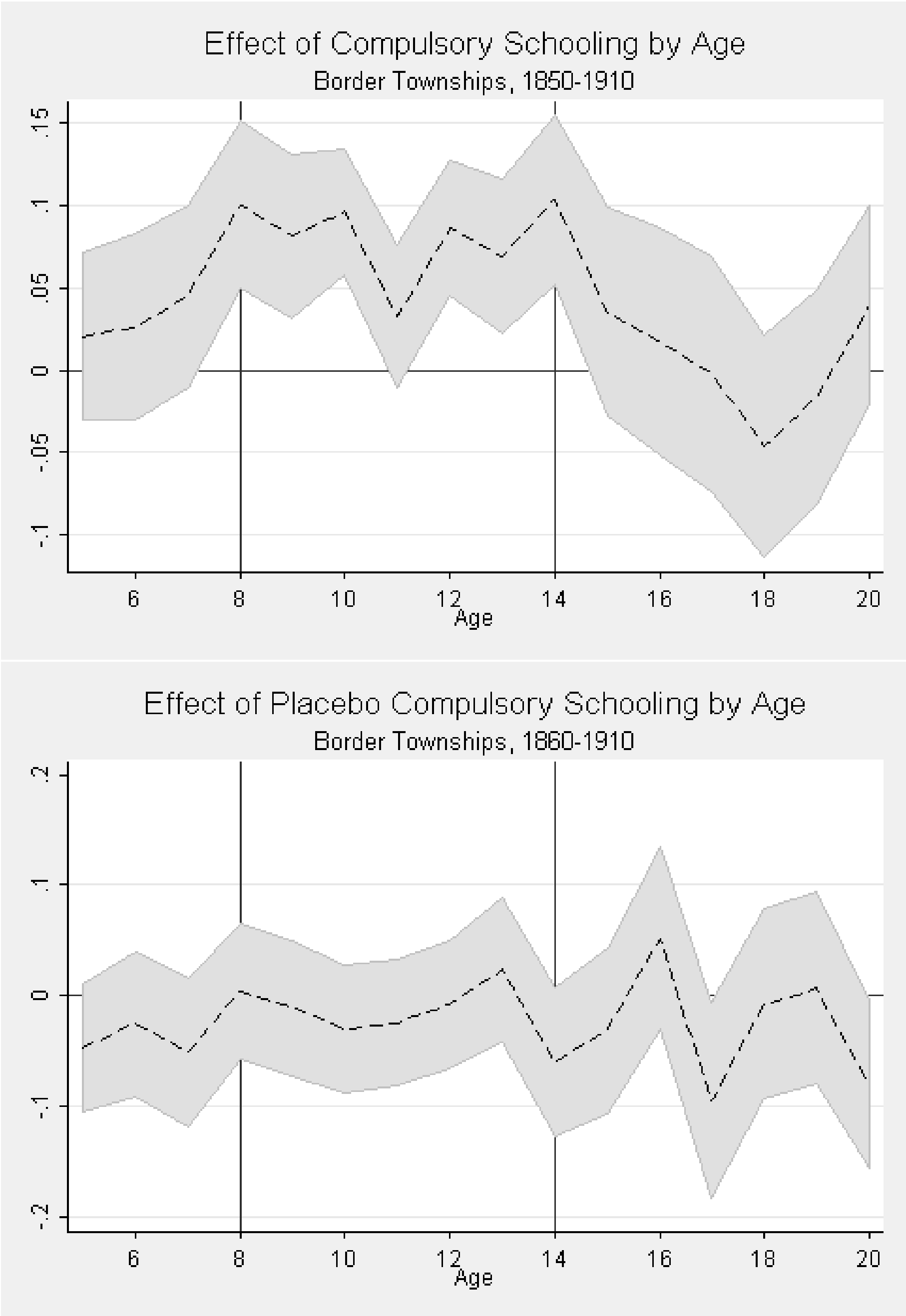

Figure 8: Births From CSL Date

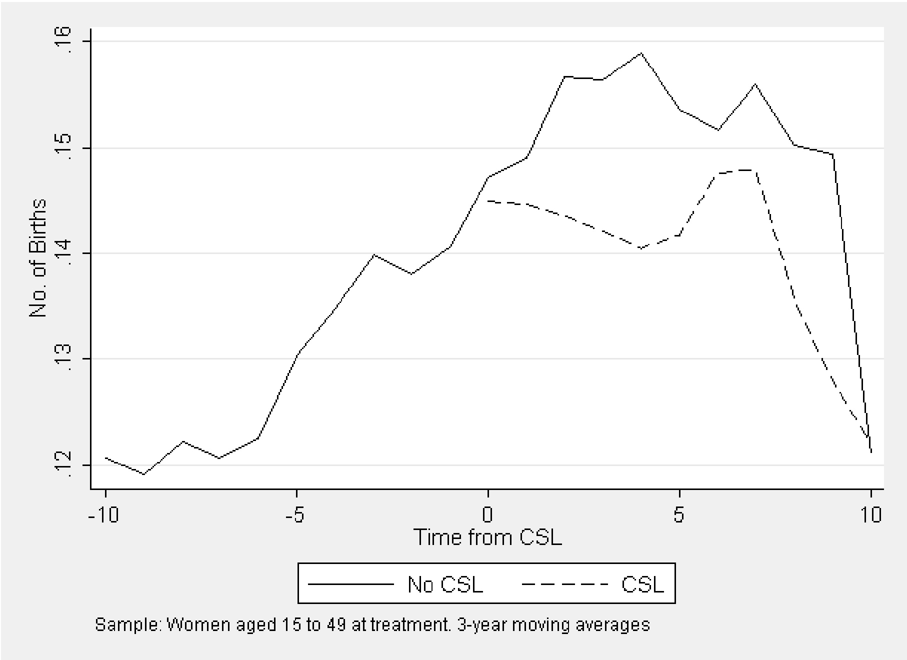

Figure 9: Fertility Change: By CSL

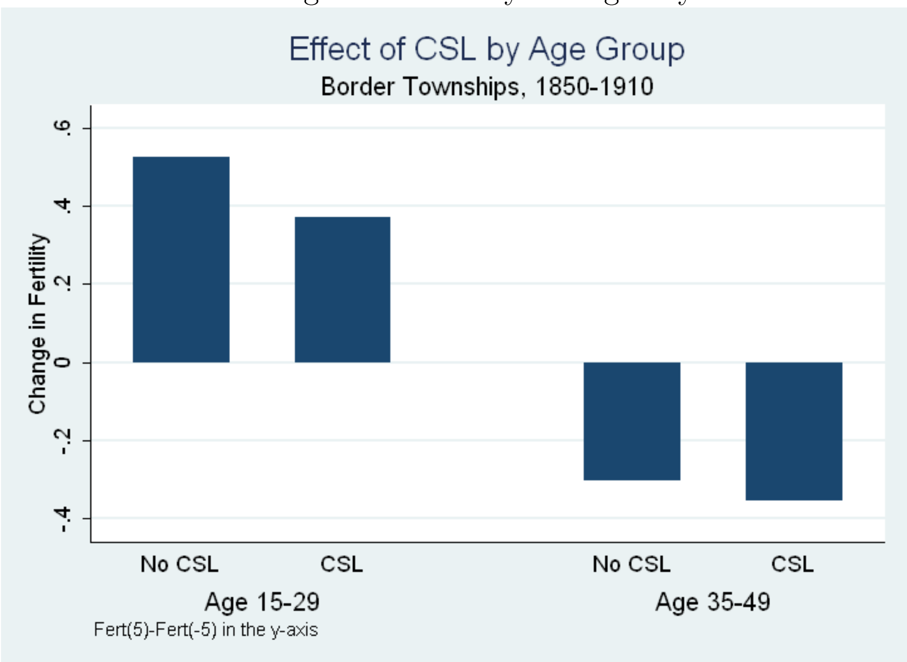

Table 1: **Summary Statistics: Border Counties**

		No CSL			**CSL**			**CSL - No CSL**		
		Obs	Mean	St. Dev.	Obs	Mean	St. Dev.	Diff.	t-test	p-val
Child	School Attendance	15,806	0.764	0.425	16,331	0.818	0.386	0.054	12.000	0.000
	Before	7,094	0.724	0.447	7,314	0.781	0.414	0.058	8.010	0.000
	After	8,712	0.797	0.402	9,017	0.848	0.359	0.052	9.020	0.000
	Literacy	9,807	0.902	0.298	9,872	0.936	0.246	0.036	7.510	0.000
	Before	4,290	0.883	0.321	4,290	0.915	0.279	0.031	4.810	0.000
	After	5,517	0.916	0.277	5,582	0.952	0.215	0.036	7.550	0.000
	Age	15,806	10.873	1.996	16,331	10.845	1.989	-0.028	-1.270	0.204
	Male	15,806	0.509	0.500	16,331	0.517	0.500	0.008	1.490	0.136
Mother	Mother Employed	15,806	0.057	0.231	16,331	0.031	0.173	-0.026	-11.290	0.000
Father	Foreign	15,806	0.568	0.495	16,331	0.524	0.499	-0.044	-7.890	0.000
	Literacy	15,806	0.863	0.344	16,331	0.877	0.328	0.014	3.790	0.000
Occupation	Professional	15,806	0.020	0.141	16,331	0.020	0.142	0.000	0.170	0.864
	Farmer	15,806	0.560	0.496	16,331	0.505	0.500	-0.055	-9.930	0.000
	Manager	15,806	0.057	0.232	16,331	0.058	0.233	0.001	0.210	0.831
	Clerk	15,806	0.006	0.078	16,331	0.008	0.092	0.002	2.510	0.012
	Salaried	15,806	0.013	0.112	16,331	0.014	0.118	0.002	1.210	0.226
	Craftman	15,806	0.118	0.323	16,331	0.136	0.342	0.018	4.750	0.000
	Operative/App.	15,806	0.063	0.242	16,331	0.084	0.277	0.021	7.250	0.000
	Laborer/Farm Lab.	15,806	0.135	0.342	16,331	0.150	0.358	0.016	3.990	0.000

t-tests reported do not assume equal variance among groups.

Table 2: **Summary Statistics: Border Townships**

		No CSL			**CSL**			**CSL - No CSL**		
		Obs	Mean	St. Dev.	Obs	Mean	St. Dev.	Diff.	t-test	p-val
Child	School Attendance	2968	0.825	0.380	3062	0.823	0.381	0.00	-0.15	0.880
	Before	1293	0.817	0.387	1371	0.770	0.421	-0.05	-2.97	0.003
	After	1675	0.831	0.375	1691	0.866	0.340	0.04	2.86	0.004
	Literacy	1831	0.903	0.296	1813	0.907	0.290	0.01	-0.47	-0.639
	Before	737	0.879	0.326	720	0.875	0.331	0.01	0.88	0.379
	After	1094	0.919	0.274	1093	0.929	0.258	0.00	-0.25	0.805
	Age	2968	10.917	1.982	3062	10.852	1.985	-0.07	-1.28	0.201
	Male	2968	0.503	0.500	3062	0.512	0.500	0.01	0.70	0.483
Mother	Mother Employed	2968	0.041	0.199	3062	0.035	0.184	-0.01	-1.25	0.211
Father	Foreign	2968	0.510	0.500	3062	0.511	0.500	0.00	0.05	0.959
	Literacy	2968	0.896	0.305	3062	0.885	0.319	-0.01	-1.43	0.152
Occupation	Professional	2968	0.021	0.143	3062	0.021	0.143	0.00	0.00	0.997
	Farmer	2968	0.462	0.499	3062	0.405	0.491	-0.06	-4.52	0.000
	Manager	2968	0.078	0.268	3062	0.073	0.260	-0.01	-0.78	0.433
	Clerk	2968	0.008	0.091	3062	0.016	0.126	0.01	2.69	0.007
	Salaried	2968	0.014	0.120	3062	0.022	0.145	0.01	2.07	0.039
	Craftman	2968	0.160	0.367	3062	0.159	0.366	0.00	-0.07	0.944
	Operative/App.	2968	0.076	0.265	3062	0.090	0.286	0.01	1.97	0.049
	Laborer/Farm Lab.	2968	0.149	0.356	3062	0.179	0.383	0.03	3.16	0.002

t-tests reported do not assume equal variance among groups.

Table 3: **Effect of CSL on School Enrollment**

	Border Counties						
Dep. Var: *School Attendance*	(1) No Controls	(2) Year	(3) Year/State	(4) Full	(5) Full/White	(6) Full/Black	N. Obs. White/Black
	0.0791***	0.0943***	-0.00356	0.00320	0.00853	-0.0430	
All Borders	(0.00468)	(0.00502)	(0.00843)	(0.00883)	(0.00904)	(0.0366)	W: 32,137
	[0.0228]	[0.0236]	[0.0138]	[0.0119]	[0.0129]	[0.0324]	
R^2	0.008	0.023	0.071	0.100	0.072	0.112	B: 3,089
	0.0687***	0.109***	0.0632***	0.0682***	0.0632***	0.264*	
Early Borders	(0.0105)	(0.0131)	(0.0193)	(0.0203)	(0.0204)	(0.145)	W: 4173
	[0.0265]	[0.0313]	[0.0247]	[0.0235]	[0.0219]	[0.0709]	
R^2	0.008	0.023	0.069	0.127	0.094	0.428	B: 110
	0.0800***	0.0970***	-0.00428	-0.00758	-0.00129	-0.0478	
Later Borders	(0.00515)	(0.00554)	(0.00942)	(0.00973)	(0.0100)	(0.0370)	W: 24,875
	[0.0243]	[0.0274]	[0.0139]	[0.0129]	[0.0143]	[0.0328]	
R^2	0.007	0.018	0.069	0.095	0.068	0.100	B: 2,979

OLS regression of equation (2) with robust standard errors in parentheses. Robust standard errors clustered by state/year reported in square brackets.***, ** and * denote 1%, 5% and 10% significance according to the robust standard errors.

Table 4: **Effect of CSL on School Enrollment**

	Border Townships						
Dep. Var:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
School Attendance	No Controls	Year	Year/State	Full	Full/Twn	Full/White	Full/Black
All Borders	0.0588***	0.0666***	0.0849***	0.0674***	0.0763***	0.0681***	0.377**
	(0.0102)	(0.0109)	(0.0185)	(0.0192)	(0.0243)	(0.0248)	(0.169)
	[0.0243]	[0.0296]	[0.0229]	[0.0176]	[0.0214]	[0.0217]	[0.0534]
Obs	6030	6030	6030	6030	6030	5676	354
R^2	0.005	0.007	0.048	0.091	0.215	0.215	0.281
Early Borders	0.0193	0.0927***	0.112***	0.123***	0.132***	0.120**	
	(0.0234)	(0.0277)	(0.0408)	(0.0434)	(0.0504)	(0.0505)	N/A
	[0.0596]	[0.0468]	[0.0584]	[0.0329]	[0.0358]	[0.0363]	
Obs	1199	1199	1199	1199	1199	1146	
R^2	-0.000	0.018	0.049	0.178	0.224	0.225	
Later Borders	0.0700***	0.0905***	0.0766***	0.0564***	0.0705**	0.0660**	0.388**
	(0.0113)	(0.0126)	(0.0210)	(0.0216)	(0.0276)	(0.0284)	(0.172)
	[0.0264]	[0.0341]	[0.0244]	[0.0201]	[0.0249]	[0.0256]	[0.0583]
Obs	4831	4831	4831	4831	4831	4530	301
R^2	0.006	0.011	0.052	0.078	0.220	0.219	0.323
Geog. FE	None	None	State	Segment	Township	Township	Township
Controls	None	Year	Year	Full	Full	Full	Full
Sample	All	All	All	All	All	White	Black

OLS regression of equation (2) with robust standard errors in parentheses. Robust standard errors clustered by state/year reported in square brackets.***, ** and * denote 1%, 5% and 10% significance according to the robust standard errors.

Table 5: **Effect of Placebo CSL on School Enrollment**

	No Controls	Year	Year/State	Full	Full/White	Full/Black
1860-1910	-0.0247*	0.00547	-0.0459*	-0.0236	-0.0209	-0.0612
	(0.0140)	(0.0144)	(0.0239)	(0.0237)	(0.0240)	(0.148)
	[0.0446]	[0.0333]	[0.0241]	[0.0201]	[0.0212]	[0.0785]
Observations	4,447	4,447	4,447	4,447	4,212	235
R^2	0.000	0.041	0.095	0.142	0.111	0.166
1860-1880	-0.0128	-0.0254	-0.0235	-0.0158	-0.0158	
	(0.0156)	(0.0174)	(0.0284)	(0.0281)	(0.0281)	
	[0.0304]	[0.0363]	[0.0279]	[0.0269]	[0.0257]	N/A
Observations	2,808	2,808	2,808	2,808	2,765	
R^2	-0.000	0.001	0.042	0.099	0.081	
1900-1910	-0.0708***	0.0316	-0.0393	-0.0192	-0.00630	-0.108
	(0.0270)	(0.0306)	(0.0444)	(0.0436)	(0.0458)	(0.147)
	[0.0770]	[0.0831]	[0.0119]	[0.0184]	[0.0253]	[0.0637]
Observations	1,639	1,639	1,639	1,639	1,447	192
R^2	0.004	0.037	0.116	0.160	0.131	0.162

OLS regression of equation (2) with robust standard errors in parentheses. Placebo CSL defined as 10 years before the actual CSL. Robust standard errors clustered by state/year reported in square brackets.***, ** and * denote 1%, 5% and 10% significance according to the robust standard errors.

Table 6: **Summary Statistics: All Observations**

		No CSL			**CSL**			**CSL - No CSL**		
		Obs	Mean	St. Dev.	Obs	Mean	St. Dev.	Diff.	t-test	p-val
Child	School Attendance	154,702	0.653	0.476	226,652	0.874	0.332	0.221	158.2	0.000
	Before	135,433	0.629	0.483	139,023	0.824	0.381	0.194	116.7	0.000
	After	128,027	0.647	0.478	225,529	0.874	0.332	0.227	150.7	0.000
	Literacy	64,644	0.774	0.418	155,132	0.968	0.175	0.261	137.2	0.000
	Before	51,272	0.760	0.427	94,070	0.964	0.187	0.204	102.8	0.000
	After	64,644	0.774	0.418	155,132	0.968	0.175	0.195	114.1	0.000
	Age	154,702	10.819	2.003	226,652	10.872	1.999	0.053	7.97	0.000
	Male	154,702	0.509	0.500	226,652	0.506	0.500	-0.003	-2.02	0.043
Mother	Mother Employed	154,702	0.075	0.264	226,652	0.033	0.177	-0.043	-55.9	0.000
Father	Foreign	154,702	0.361	0.480	226,652	0.834	0.372	0.473	326.3	0.000
	Literacy	154,702	0.788	0.409	226,652	0.908	0.288	0.120	99.8	0.000
Occupation	Professional	154,702	0.024	0.153	226,652	0.026	0.160	0.002	4.6	0.000
	Farmer	154,702	0.593	0.491	226,652	0.371	0.483	-0.222	-137.7	0.000
	Manager	154,702	0.048	0.213	226,652	0.085	0.279	0.037	46.4	0.000
	Clerk	154,702	0.005	0.069	226,652	0.018	0.132	0.013	39.9	0.000
	Salaried	154,702	0.009	0.096	226,652	0.028	0.165	0.019	43.8	0.000
	Craftman	154,702	0.105	0.307	226,652	0.167	0.373	0.062	56.0	0.000
	Operative/App.	154,702	0.046	0.209	226,652	0.116	0.320	0.070	81.6	0.000
	Laborer/Farm Lab.	154,702	0.144	0.351	226,652	0.150	0.357	0.006	5.2	0.000

t-tests reported do not assume equal variance among groups.

Table 7: **Summary Statistics: Border States**

		No CSL			**CSL**			**CSL - No CSL**		
		Obs	Mean	St. Dev.	Obs	Mean	St. Dev.	Diff.	t-test	p-val
Child	School Attendance	101,925	0.714	0.452	35,055	0.809	0.393	0.095	37.610	0.000
	Before	45,213	0.664	0.473	15,490	0.763	0.425	0.100	24.510	0.000
	After	56,712	0.754	0.431	19,565	0.845	0.362	0.091	28.930	0.000
	Literacy	61,502	0.854	0.353	22,527	0.929	0.257	0.096	35.280	0.000
	Before	26,510	0.823	0.381	9,853	0.903	0.297	0.079	20.850	0.000
	After	34,992	0.878	0.328	12,674	0.950	0.218	0.072	27.610	0.000
	Age	101,925	10.839	1.999	35,055	10.836	1.990	-0.003	-0.240	0.810
	Male	101,925	0.507	0.500	35,055	0.511	0.500	0.004	1.220	0.223
Mother	Mother Employed	101,925	0.069	0.254	35,055	0.040	0.196	-0.029	-22.230	0.000
Father	Foreign	101,925	0.524	0.499	35,055	0.580	0.494	0.056	18.150	0.000
	Literacy	101,925	0.805	0.396	35,055	0.872	0.334	0.067	30.650	0.000
Occupation	Professional	101,925	0.023	0.150	35,055	0.023	0.151	0.000	0.090	0.927
	Farmer	101,925	0.556	0.497	35,055	0.512	0.500	-0.045	-14.530	0.000
	Manager	101,925	0.055	0.229	35,055	0.065	0.247	0.010	6.520	0.000
	Clerk	101,925	0.007	0.082	35,055	0.009	0.096	0.002	4.140	0.000
	Salaried	101,925	0.013	0.113	35,055	0.016	0.125	0.003	3.890	0.000
	Craftman	101,925	0.113	0.317	35,055	0.129	0.335	0.015	7.500	0.000
	Operative/App.	101,925	0.063	0.242	35,055	0.080	0.272	0.018	10.870	0.000
	Laborer/Farm Lab.	101,925	0.144	0.351	35,055	0.141	0.348	-0.003	-1.480	0.138

t-tests reported do not assume equal variance among groups.

Table 8: **Effect of CSL on School Enrollment**

			All States				
Dep. Var: *School Attendance*	(1) No Controls	(2) Year	(3) Year/State	(4) Full	(5) Full/White	(6) Full/Black	N. Obs. White/Black
All Years	0.216***	0.177***	-0.0451***	-0.0364***	-0.0411***	0.000337	
	(0.00124)	(0.00169)	(0.00241)	(0.00241)	(0.00248)	(0.00943)	W:343,711
	[0.0201]	[0.0211]	[0.0186]	[0.0149]	[0.0151]	[0.0245]	
R^2	0.068	0.085	0.157	0.189	0.125	0.248	B: 37,643
			Border States				
All Borders	0.124***	0.106***	-0.0281***	-0.0218***	-0.0208***	-0.0237	
	(0.00289)	(0.00315)	(0.00512)	(0.00508)	(0.00525)	(0.0186)	W: 115,564
	[0.0210]	[0.0267]	[0.0144]	[0.0136]	[0.0145]	[0.0285]	
R^2	0.010	0.030	0.088	0.128	0.077	0.153	B: 21,416
1850-1870 Borders	0.173***	0.183***	0.0577***	0.0591***	0.0523***	0.239*	
	(0.00809)	(0.00909)	(0.0138)	(0.0135)	(0.0134)	(0.131)	W: 16,683
	[0.0675]	[0.0277]	[0.0249]	[0.0213]	[0.0339]		
R^2	0.012	0.146	0.200	0.256	0.153	0.430	B: 1,051
1880-1910 Borders	0.123***	0.102***	-0.0218***	-0.0187***	-0.0175***	-0.0176	
	(0.00306)	(0.00335)	(0.00555)	(0.00549)	(0.00571)	(0.0190)	W: 98,881
	[0.0213]	[0.0288]	[0.0112]	[0.0117]	[0.0127]	[0.0292]	
R^2	0.010	0.029	0.074	0.112	0.068	0.125	B: 20,365

OLS regression of equation (2) with robust standard errors in parentheses. Robust standard errors clustered by state/year reported in square brackets. ***, ** and * denote 1%, 5% and 10% significance according to the robust standard errors.

Table 9: **Summary Statistics: Stock Fertility**

		Counties				**Townships**			
		No CSL		**CSL**		**No CSL**		**CSL**	
		Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
Stock	Own Children	2.642	2.142	2.681	2.167	2.623	2.120	2.571	2.115
Fertility	Children<5	0.815	0.909	0.901	0.921	0.878	0.923	0.736	0.874
Women	Age	34.103	9.532	32.350	8.856	33.163	8.988	35.399	9.822
Controls	Literacy	0.912	0.283	0.889	0.314	0.909	0.287	0.916	0.277
	Foreigner	0.152	0.359	0.148	0.355	0.222	0.416	0.243	0.429
	Labor Force Part.	0.044	0.206	0.084	0.278	0.048	0.213	0.044	0.205
	Urban	0.125	0.331	0.087	0.281	0.114	0.318	0.271	0.445
	White	0.913	0.282	0.858	0.349	0.940	0.238	0.936	0.246
Household	Professional	0.028	0.165	0.027	0.162	0.028	0.166	0.031	0.174
Head	Farmer	0.417	0.493	0.485	0.500	0.382	0.486	0.317	0.465
Occupation	Manager	0.067	0.250	0.060	0.237	0.074	0.261	0.090	0.286
	Clerk	0.015	0.123	0.009	0.094	0.014	0.120	0.027	0.161
	Salaried	0.020	0.140	0.017	0.129	0.019	0.138	0.031	0.174
	Craftman	0.139	0.346	0.124	0.330	0.169	0.375	0.168	0.374
	Operative/App.	0.097	0.296	0.077	0.267	0.104	0.305	0.119	0.324
	Laborer/Farm Lab.	0.182	0.386	0.166	0.372	0.170	0.376	0.178	0.383
Observations		17899		12155		2484		3540	

t-tests reported do not assume equal variance among groups.

Table 10: **Summary Statistics: Flow Fertility**

		No CSL			**CSL**			**CSL - No CSL**		
		Obs	Mean	St. Dev.	Obs	Mean	St. Dev.	Diff.	t-test	p-val
Accumulated	Fert(t+1)	3,528	0.150	0.360	4,002	0.139	0.348	-0.011	-1.340	0.179
Fertility	Fert(t+3)	2,448	0.432	0.623	2,866	0.406	0.605	-0.026	-1.540	0.124
	Fert(t+5)	1,502	0.644	0.833	2,046	0.628	0.827	-0.016	-0.580	0.565
Women Controls	Age	3,528	36.32	9.692	4,002	36.31	9.614	-0.008	-0.040	0.972
	Literacy	3,528	0.902	0.297	4,002	0.915	0.280	0.012	1.810	0.071
	Foreigner	3,528	0.194	0.396	4,002	0.199	0.400	0.005	0.540	0.589
	Labor Force Part.	3,528	0.066	0.248	4,002	0.066	0.249	0.001	0.120	0.902
	Urban	3,528	0.333	0.471	4,002	0.424	0.494	0.091	8.170	0.000
	White	3,528	0.923	0.266	4,002	0.916	0.277	-0.007	-1.190	0.236
Household Head	Professional	3,528	0.026	0.159	4,002	0.032	0.176	0.006	1.530	0.126
Occupation	Farmer	3,528	0.388	0.487	4,002	0.333	0.471	-0.054	-4.910	0.000
	Manager	3,528	0.078	0.268	4,002	0.080	0.272	0.003	0.450	0.654
	Clerk	3,528	0.015	0.123	4,002	0.028	0.165	0.013	3.810	0.000
	Salaried	3,528	0.026	0.159	4,002	0.029	0.168	0.003	0.770	0.441
	Craftman	3,528	0.160	0.367	4,002	0.165	0.372	0.006	0.650	0.515
	Operative/App.	3,528	0.105	0.307	4,002	0.134	0.341	0.029	3.880	0.000
	Laborer/Farm Lab.	3,528	0.162	0.369	4,002	0.165	0.371	0.003	0.330	0.744
Resident	At CSL(t)	3,528	0.905	0.293	4,002	0.904	0.295	-0.002	-0.260	0.793

t-tests reported do not assume equal variance among groups.

Table 11: **Effect of CSL: Flow Fertility - Townships**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Dep. Var: Births in year t					
CSL	0.000631	-0.0110**	-0.0111**	-0.0150**	-0.0123*	-0.0104*	-0.0173**	-0.0120**
	(0.00402)	(0.00527)	(0.00542)	(0.00742)	(0.00686)	(0.00561)	(0.00697)	(0.00543)
Observations	56475	56475	56475	30645	19665	51930	11130	56475
R^2	-0.000	0.005	0.022	0.037	0.060	0.022	0.015	0.001
			Dep. Var: Fertility 1 year from CSL					
CSL	-0.0137	-0.00392	-0.00391	-0.0229	-0.00159	-0.00715	0.00850	-0.00531
	(0.0118)	(0.0152)	(0.0149)	(0.0216)	(0.0213)	(0.0154)	(0.0360)	(0.0148)
Observations	7530	7530	7530	4086	2622	6924	1484	7530
R^2	0.000	0.006	0.045	0.038	0.044	0.048	0.063	0.001
			Dep. Var: Fertility 3 years from CSL					
CSL	-0.0479**	-0.0495	-0.0542*	-0.0949**	-0.0360	-0.0562*	-0.169**	-0.0438
	(0.0239)	(0.0327)	(0.0314)	(0.0450)	(0.0442)	(0.0321)	(0.0809)	(0.0291)
Observations	5314	5314	5314	2824	1896	4872	934	5314
R^2	0.001	0.019	0.102	0.088	0.115	0.106	0.098	0.003
			Dep. Var: Fertility 5 years from CSL					
CSL	-0.0708*	-0.108*	-0.117**	-0.167**	-0.0542	-0.119**	-0.259*	-0.109**
	(0.0411)	(0.0553)	(0.0526)	(0.0728)	(0.0744)	(0.0536)	(0.135)	(0.0509)
Observations	3548	3548	3548	1928	1236	3370	682	3548
R^2	0.005	0.012	0.108	0.142	0.146	0.114	0.107	0.013
Ages	15-49	15-49	15-49	15-29	34-49	15-49	15-49	15-49
Sample	All	All	All	All	All	White	Foreign	All
Controls	No	State/Year	Full	Full	Full	Full	Full	Mother FE

OLS estimates with robust standard errors in parentheses standard errors reported. In the first row, equation (3) is estimated and standard errors are clustered by mother. In the other rows, the estimated equation is (3). All regressions but (1) use all the controls described in the text. *** p<0.01, ** p<0.05, * p<0.1

Table 12: **Effect of CSL: Flow Fertility - Counties**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Dep. Var: Births in year t					
CSL	0.00143	-0.0109***	-0.00629***	-0.00693**	-0.00816***	-0.00604**	0.0102	-0.00798***
	(0.00171)	(0.00213)	(0.00226)	(0.00306)	(0.00302)	(0.00242)	(0.00716)	(0.00226)
Observations	329580	329580	329580	181305	109290	285450	38970	329580
R^2	0.000	0.004	0.019	0.036	0.054	0.020	0.017	0.002
			Dep. Var: Fertility 1 year from CSL					
CSL	-0.00746	-0.00123	0.000656	0.000686	-0.00530	0.00150	-0.000315	0.00114
	(0.00497)	(0.00655)	(0.00644)	(0.00922)	(0.00933)	(0.00693)	(0.00781)	(0.00625)
Observations	43,944	43,944	43,944	24,174	14,572	38,060	31,114	43,944
R^2	0.000	0.005	0.038	0.033	0.041	0.041	0.039	0.000
			Dep. Var: Fertility 3 years from CSL					
CSL	-0.0159	-0.00209	0.00321	-0.00283	0.0137	-0.00316	0.00939	0.00894
	(0.00993)	(0.0133)	(0.0127)	(0.0182)	(0.0184)	(0.0134)	(0.0153)	(0.0119)
Observations	32,874	32,874	32,874	17,860	11,046	29,080	23,592	32,874
R^2	0.001	0.016	0.099	0.092	0.121	0.105	0.105	0.002
			Dep. Var: Fertility 5 years from CSL					
CSL	-0.0480**	-0.0225	-0.0181	-0.00517	-0.00683	-0.0218	-0.00117	-0.0135
	(0.0189)	(0.0252)	(0.0240)	(0.0334)	(0.0348)	(0.0246)	(0.0292)	(0.0230)
Observations	17,852	17,852	17,852	9,746	5,938	16,794	12,294	17,852
R^2	0.005	0.015	0.106	0.139	0.156	0.110	0.114	0.011
Ages	15-49	15-49	15-49	15-29	34-49	15-49	15-49	15-49
Sample	All	All	All	All	All	White	Foreign	All
Controls	No	State/Year	Full	Full	Full	Full	Full	Mother FE

OLS regression with robust standard errors in parentheses standard errors. When the dep. var. is *Births*, errors are clustered by mother id. All regressions but (1) use all the controls described in the text. *** p<0.01, ** p<0.05, * p<0.1

Table 13: **Effect of CSL: Stock Fertility**

	(1)	(2)	(3)	(4)	(5)	(6)
	Dep. Var: Number of Own Children					
CSL	-0.0977**	-0.102**	-0.259**	-0.0900	-0.0869	0.0217
	(0.0456)	(0.0478)	(0.129)	(0.0994)	(0.101)	[0.220]
Observations	30038	26756	4523	6014	5637	1408
R^2	0.158	0.161	0.128	0.145	0.146	0.127
	Dep. Var: Number of Children Younger than 5					
CSL	0.00696	0.00905	-0.0421	0.00198	0.0110	-0.000137
	(0.0195)	(0.0203)	(0.0523)	(0.0423)	(0.0429)	(0.0884)
Observations	30038	26756	4523	6014	5637	1408
R^2	0.156	0.166	0.186	0.169	0.175	0.193
Age at Treatment	15-49	15-49	15-49	15-49	15-49	15-49
Sample	All	White	Foreign	All	White	Foreign
Border	County	County	County	Township	Township	Township
Controls	Full	Full	Full	Full	Full	Full

OLS regression with robust standard errors in parentheses standard errors clustered by state/year in brackets. All regressions but (1) use all the controls described in the text. *** p<0.01, ** p<0.05, * p<0.1

Table 14: **Effect of Placebo CSL: Flow Fertility - Townships**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dep. Var: Births in year t							
Placebo CSL	0.00284	-0.00833	-0.00578	0.0112	-0.0235***	-0.00494	-0.00694	-0.00474
	(0.00412)	(0.00526)	(0.00549)	(0.00731)	(0.00704)	(0.00561)	(0.0119)	(0.00559)
Observations	57990	57990	57990	30465	20850	54810	14745	57990
R^2	-0.000	0.005	0.020	0.045	0.051	0.020	0.019	0.002
	Dep. Var: Fertility 1 year from CSL							
Placebo CSL	-0.0282**	-0.0186	-0.0180	-0.0196	0.00273	-0.0234	-0.0402	-0.00835
	(0.0119)	(0.0160)	(0.0157)	(0.0222)	(0.0231)	(0.0160)	(0.0343)	(0.0151)
Observations	7732	7732	7732	4062	2780	7308	1966	7732
R^2	0.000	0.005	0.043	0.039	0.045	0.044	0.049	-0.000
	Dep. Var: Fertility 3 years from CSL							
Placebo CSL	-0.0633***	-0.00723	-0.00773	0.0162	-0.0508	-0.00373	0.0541	-0.00160
	(0.0244)	(0.0332)	(0.0315)	(0.0446)	(0.0470)	(0.0319)	(0.0688)	(0.0293)
Observations	5672	5672	5672	2976	2010	5406	1420	5672
R^2	0.002	0.010	0.106	0.124	0.128	0.110	0.124	0.004
	Dep. Var: Fertility 5 years from CSL							
Placebo CSL	-0.0626	0.0695	0.0677	0.0719	-0.0178	0.0687	0.222*	0.0505
	(0.0438)	(0.0592)	(0.0560)	(0.0744)	(0.0856)	(0.0565)	(0.131)	(0.0541)
Observations	3358	3358	3358	1824	1152	3256	786	3358
Adjusted R^2	0.005	0.013	0.119	0.178	0.177	0.122	0.135	0.014
Ages	15-49	15-49	15-49	15-29	34-49	15-49	15-49	15-49
Sample	All	All	All	All	All	White	Foreign	All
Controls	No	State/Year	Full	Full	Full	Full	Full	Mother FE

OLS regression with robust standard errors in parentheses standard errors. When the dep. var. is *Births*, errors are clustered by mother id. All regressions but (1) use all the controls described in the text. *** p<0.01, ** p<0.05, * p<0.1

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