Essays on the Economic History of the Family

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A mis queridos viejos

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In a sense I started writing this thesis many years ago when, as a child, I felt a fascination

for my forefathers and for their journey to the Americas. As a descendant of those Italians and Spaniards that went to *fare l'America*, I have been always curious about their motivations and feelings. I have always wondered how hard it must have been to step on a boat without looking back, leaving a whole family and a motherland behind. Although this thesis is not directly about migrations, it involves immigrants who, like them, dreamt of a better life. It is for these reasons that I wish to dedicate my work especially to them.

E tanti sun li Zenoexi
e per lo mondo sì distexi,
che und'eli van o stan
un'atra Zenoa ge fan.[1]

[1] Anonimo Genovese, Poesie, No. 138. Quoted from Epstein, Steven A. (1996) "Genoa and the Genoese 958-1528", Chapel Hill: UNC Press, p. 166

Abstract

This thesis studies the economic effects of child labor and compulsory schooling laws (CLLs and CSLs). In the first two chapters I study the consequences of the enactment of CSLs on education and fertility. I use a combination of a difference-in-difference (DID) methodology with an identification strategy based on legislative borders to find that the laws increased enrollment by 7 % and educational attainment by about 0.3 years of education over the long run. As for fertility, I find that CSLs imply a contemporaneous reduction in fertility of about 15 %. In the long run, women that received compulsory education were expected to have approximately 0.15 to 0.3 fewer children. In the third chapter of this dissertation I look at the effect of CLLs on industrial performance. I find that industries that initially relied extensively on child labor suffered a significant reduction in growth as a consequence of the social legislation. I conjecture that the potentially sizable but narrowly concentrated effects of CLLs could explain why child labor is still common in the developing world today.

Resumen

Esta tesis estudia los efectos económicos de las leyes de trabajo infantil (CLL) y educación obligatoria (CSL). En los primeros dos capítulos, se exploran las consecuencias de la implementación de una CSL en los niveles de educación y fecundidad. Utilizando una metodología que combina diferencia-en-diferencias (DID) con una estrategia de identificación basada en las fronteras legislativas, se encuentra que estas leyes incrementaron la escolarización en un 7 % y, en el largo plazo, el número de años de educación en 0.3. En cuanto a fecundidad, se halla que una CSL implica una reducción contemporánea de la misma en el orden del 15 %. En el largo plazo, las mujeres que recibieron educación tienen aproximadamente 0.15 a 0.3 hijos menos. En el tercer capítulo de esta tesis se estudian los efectos de una CLL en el desempeño de la industria. Se encuentra que las industrias que al principio dependían ampliamente del trabajo infantil sufren una reducción significativa en sus tasas de crecimiento como consecuencia de la legislación social. Se conjetura que el hecho que estos efectos sean potencialmente grandes, aunque concentrados en unos pocos agentes, podría ser la razón por la cual el trabajo infantil es aún hoy tan común en el mundo en desarrollo.

Foreword

The perennial question remains: why are some countries profusely rich while others linger in agonizing poverty? The names of the countries may change, but the asymmetry of modern economies in terms of average wealth, education, health, and fertility always startles. For instance, in 2008 the per capita GDP of the U.S. reached 46,350 dollars. The average level of education was slightly above 12 years and American women on average gave birth to about two children over their lifetimes. In contrast, the per capita GDP of Niger was just 364 dollars–less than one percent of the American figure. Nigeriens could expect to receive about one year of schooling, and, if they were women, they were likely to bear more than seven children during their lifetimes.[2]

How did Americans break free from the Malthusian poverty trap of low growth and high fertility? Is there anything the Nigerien government can do to hasten the transition to self-sustained growth? As Nobel Laureate Robert E. Lucas, Jr. put it, "the consequences for human welfare involved in questions like these are simply staggering: once one starts to think about them, it is hard to think about anything else"Lucas (1988).

To find explanations for the stark differences between developed and developing countries, economists have resorted to induction. First, they identify the common features of countries in each group. Then, they construct mathematical models to assess the extent to which these features can account for the differences in development between groups.

The early literature stressed differences in capital levels between rich and poor countries. Quite intuitively, Nigerien farmers cannot possibly be as productive as Americans if they lack tractors and plows to produce wheat, silos to store it, and roads to take it to the market (e.g., Rebelo (1991), Solow (1956)). The Nigerien farmer would like to have a tractor, but nobody will give him a loan. Thus, Rajan and Zingales (1998) argued that underdeveloped financial systems may be one of the reasons why Niger is poor.

Perhaps our Nigerien farmer is too sick to work on his plot today. Given the poor conditions of the medical facilities and the lack of doctors, he is lucky to have survived the yellow fever outbreak earlier this year. Several papers recognize this direct relationship between health and productivity (e.g. Strauss and Thomas (1988), Weil (2007)). Healthy workers not only produce more but also tend to invest more in human capital as their life expectancy is significantly longer. The acquired education then feeds back on productivity, establishing a virtuous circle of development. Finally, another strand of the literature suggests a more subtle connection between healthy environments and development: better institutions.

[2]The data on per capita GDP and total fertility come from the World Bank, World Development Indicators 2008. The data on average schooling come from the Barro-Lee dataset for 1999.

Acemoglu, Johnson, and Robinson (2001) propose a theory suggesting that high morbidity may have an effect on output, but only through the degradation of institutions. In their view, colonizing powers chose not to settle in insalubrious places. Instead, they found colonies with "extractive" institutions characterized by a high risk of expropriation. Persistent colonial institutions did the rest. Niger's environment could not be better evidence for supporting this theory: 82% of the European crew that ventured up the Niger river in 1841 fell sick. A third of them died. One witness of that suffering later commented: "[t]he scenes at night were most agonizing. Nothing but muttering delirium, or suppressed groans were heard on every side on board the vessels, affording a sad contrast to the placid character of the river and its surrounding scenery." While Niger seems to fit the theory nicely, recent research suggests that the empirical relationship between settler mortality and institutions is not robust (Albouy (2008)).[3]

Could there be anything intrinsic about weather that causes underdevelopment? Abstracting from institutions, Sachs (2001) argues that tropical areas are doomed to low agricultural productivity; they simply cannot compete with the ecological conditions in temperate areas. Such a theory seems very attractive to the empirical researcher; after all, with the exception of Hong Kong and Singapore, there are no developed countries in tropical areas. Other geographical features have been suggested to explain underdevelopment, such as the orientation of the continents (East-West versus North-South) or whether countries are landlocked. Niger nicely fits all of these geographical theories– it is located in the tropics, is landlocked and is in a continent with a North-South orientation.[4]

So the theories go on, stressing many relevant factors, such as the role of human capital, ethnic fragmentation, or even the availability of foreign aid. Most of these theories provide interesting insights into the differences between developed and developing economies.[5]

What these theories cannot explain, however, is the process that leads to development.

[3]Technically speaking, the Niger expedition did not reach the present day borders of Niger, spending most of its time in the lower course of the river in Nigeria. The quotation comes from Allen and Thompson (1848, chap. XIV). The figures cited come from Curtis (1998, p.21). In a recent paper, Albouy (2008) finds that the results are driven by the settler mortality figures which turn out to be improper for 36 out 64 countries in the sample. When more reliable estimates are used, the IV estimates become insignificant.

[4]Another author who stresses the geographical theory of underdevelopment is Diamond (1997). In his book, the East-West orientation of Eurasia contributed to a series of ecological advantages of Europe relative to the other continents.

[5]This introduction is not a comprehensive survey of the theories of economic development, for which an entire volume would be needed. It is rather intended to highlight the inductive manner in which research has evolved. There are many fascinating books, like Easterly (2002), that broadly summarize the literature. I do not intend to discredit these theories, which, as I suggest, have provided many interesting insights on the differences in levels of development. I do intend to criticize the ways in which these theories have been tested: namely, through aggregate cross-sectional/panel regressions. On this, see also Easterly (2002, p.55 *ff.*)

Take again our examples of Niger and the United States. A cross-sectional theory may recognize that low risk of expropriation is associated with developed countries like the U.S., while poor countries like Niger suffer from constant risk of expropriation. From a single observation of both countries at a moment in time, such a theory would yield the following thought experiment: what if we replaced the U.S. level of expropriation with that of Niger? What would happen? This experiment implicitly assumes the existence of a development process that takes countries from one point to the other, yet it cannot tell us much about how or why this process occurred in the first place.

Only by following countries over time can we understand their developmental processes. How did the U.S. move away from a pre-industrial economy characterized by high fertility, low wages and child labor during the nineteenth century? What were the processes that triggered this transformation? Unless we can understand the dramatic transformations that took place in today's rich countries during their development, it is unlikely that we can help poor economies.

Recent theories of economic development have realized the importance of understanding long run development processes. In particular, the "Unified Growth Theory" (UGT; Galor and Weil (2000)) has proposed a "time-series" approach to economic development. Recognizing that development is a process, these theories have tried to find a *unified* model that can explain the three observed stages of economic development within a single framework. Initially, economies are trapped in what is called a "Malthusian" equilibrium. High fertility rates, subsistence wages and slow technological progress all characterize this regime.

In the intermediate stage, industrialization increases the demand for qualified workers and incomes increase. Fertility faces two opposing forces: on the one hand, income is higher, and because children are a normal good, families increase the consumption of them. On the other hand, there is a "quality-quantity" tradeoff in place (as in Becker (1991) and Becker and Lewis (1973)). The higher the number of children chosen, the lower the level of education each of them receives. As the demand for qualified workers increases, so does the skill premium, and families find it optimal to substitute quality for quantity. As income continues to rise, income effects become less and less important. Fertility starts to decline, reinforcing the virtuous circle of education and income. The economy is now in the self-sustaining stage.

One of the most attractive features of the UGT is that it is an integrated framework that can explain long run growth. While theoretically elegant, these models do not assume different production functions for rich and poor countries. Although the factor that triggers the transition to self-sustained growth differs among the different models, they all tend to focus on increasing returns to human capital. In the canonical UGT model–Galor and Weil (2000)–the transition is triggered by interactions between population size and technological progress. A population that grows slowly during the Malthusian period makes human capital investments more profitable. There is nothing unique about this factor; many other factors have been hypothesized as triggers, such as capital-skills complementarities, prolonged life expectancy and even genetic evolution. [6]

More important in the context of UGTs are the processes that the trigger initiates. In particular, these models all predict that a "quantity-quality" tradeoff will unleash a demographic transition.[7] As anticipated by the theory, every country that is now developed experienced a dramatic reduction in fertility and mortality shortly after takeoff. Furthermore, the sequential reductions in mortality first and fertility slightly later generate population dynamics that are consistent with the three typical stages of UGTs. Fertility rates are high but stagnant in the Malthusian period, grow in the transition period, and decline in the self-sustained stage, when substitution effects dominate. This pattern is similar to what demographers have found in historical demographic transitions and, especially, the European experience.

The European Fertility Project (EFP) was a research project of the Princeton Office of Population Research designed to understand the reasons behind the fertility transition in Western Europe. The interest in the fertility transition in Europe was motivated not only by its stark magnitude but also by the synchronization with which it occurred. The vast and disparate lands that lie west of the imaginary line that unites St. Petersburg to Trieste saw their fertilities plummet in the second half of the nineteenth century with an astonishing level of coordination; the decline started in the northwest and swiftly spread to the south and east. The EFP concluded that fertility decline occurred independently of economic conditions. They argued that patterns of decline could be observed only along ethno-linguistic lines: areas that spoke similar languages had fertility transitions at roughly the same time. Overall, the "Princeton view" is that the demographic transition in Europe was driven by *diffusion* rather than *economics*.[8]

The UGT predicts the fertility transition that the EFP finds. However, the conclusion of the EFP is at odds with the economic explanation of the UGT. If economics had little to do with the fertility transition and parents did not substitute quality for quantity, then UGT models are unable to explain the transition to development. Furthermore, it could be the case that economic development is not the consequence of demographic transition, but rather its cause.

This thesis revisits the "quantity-quality" tradeoff during the transition to self-sustained growth. I argue that the only way to disentangle causality in this context is by looking at the micro-evidence of how families reacted to sudden changes in the return to education. Did the families reduce their fertilities in response to increasing returns to human

[6] For a comprehensive summary of the UGT and the different "triggers" of self-sustained growth, see Galor (2004).

[7] A calibration of the basic Galor-Weil model to fit the historical data can be found in Lagerlöf (2006).

[8] The main conclusions of the EFP can be found in Coale and Watkins (1986). A good summary of the project and its conclusions as well as a technical criticism of its methods can be found in Brown and Guinnane (2003).

capital? What was the role of social policy and, in particular, Compulsory Schooling Laws (CSLs) and Child Labor Laws (CLLs) in the process? Did CSLs and CLLs induce changes in family decision-making or technologies for production? Did they reflect passive adaptation to a changing environment?

In order to answer these questions, I look at the historical record of the United States between 1850 and 1920, when CSLs and CLLs were first enacted. My identification strategy takes advantage of the different timings of the legislations among the states in the Union. In order to maintain comparability between the "treated" and "control" groups, I use different identification strategies based on geography (Chapters One and Two) or technological dependence (Chapter Three).

The first chapter of this dissertation studies the effects of compulsory schooling laws on school enrollment and long run educational attainment. The previous literature on compulsory schooling and school enrollment in the United States has been particularly skeptical about the effectiveness of state compulsory schooling regulations, at least for the nineteenth century. In this chapter, I show that the negative results obtained by previous researchers are due to the fact that the treatment and control groups in the analysis are not really comparable. I argue that, given the limited number of variables available for which the researcher can control, a reasonable option is to narrowly define similar treatment and control groups. I do so by restricting my attention to the evolution of enrollment in the vicinity of the state border where legislation changed. I find that there is a discernible effect both in short run school enrollment and in educational achievement several decades after treatment. The effect occurs exactly for the ages covered by the law and precisely at the time when the legislation was passed. I interpret all of these findings as evidence that compulsory schooling increased enrollment in nineteenth-century America.

Chapter Two–a joint work with Joachim Voth–focuses directly on the quantity-quality tradeoff. Economic theory argues that the effect of compulsory schooling on fertility depends on two opposing effects. On the one hand, there is a child labor effect. Insofar as compulsory schooling reduces the market value of children, we should expect these laws to promote a quantity-quality tradeoff. The opposing force would come from the implicit income transfer that compulsory–and free–education would mean for households. We argue that the first effect should dominate, and we study the change in the fertility behavior of households affected by the legislation. We find that a significant 15% decline in fertility over the period can be attributed to compulsory schooling legislation through the direct channel of family incentives. In the second part of the chapter, we study the indirect channel through the change in fertility choices of women who received education. We find that as a consequence of the compulsory schooling legislation, educated women bear between 0.15 and 0.3 fewer children.

In the third chapter I explore the effects of the introduction of child labor laws in the United States between 1899 and 1919. Using a newly collected data set of industrial statistics from the manufacturers' census of the United States, I consider industrial growth rates before and after the introduction of the legislation. My identification strategy takes advantage of the fact that some industries were initially more dependent than others on child labor. I conjecture that these "child labor-dependent" industries should suffer a greater reduction in their growth rates upon the introduction of the child labor law. I test this hypothesis directly using different measures of child labor dependence and child labor law intensity, all with similar results. Despite being concentrated within a small group of highly dependent industries, child labor laws have a discernible, deleterious effect on industrial growth.

The three chapters of this thesis highlight the importance of social legislation in hastening development. They suggest that there are substantial returns to enforcing CSLs and CLLs across the developing world. However, they also imply that the short run implementation of these policies may present complications. Despite the positive returns of these policies in the long run, the short run costs of implementing the laws may fall disproportionably on a few agents. Based on the historical case of a developing economy during its transition to self-sustained growth, my thesis supports the expansion of development programs that promote school enrollment and the eradication of child labor. It suggests that programs such as Brazil's *PETI* or Mexico's *Oportunidades* could hasten Nigerien development.

Another contribution of this thesis regards its novel approach to economic development. Throughout the three chapters, I stress the enormous possibilities of a "time-series" approach. The history of empirical development is one of cross-sectional regressions, in which all of the countries of the world are pooled at one moment in time. Needless to say, the differences between the United States and Uganda, Paraguay or Vietnam are multidimensional, making it hardly convincing that the researcher would be able to control for all of them in his regressions–even if he controls for fixed effects. Instead, the time-series approach focuses on following a historical developing economy–like that of the United States in the 1900s–throughout the developmental process. The advantage of this approach is that it overcomes the intrinsic problems of comparing disparate countries at the same point in time by concentrating on a single economy through time.

This approach is hardly a panacea and presents its own drawbacks: the world today is not the same as it was a century ago. A century ago, extended families with intricate intra-household bargaining strategies were common. Today, nuclear families live in suburban homes outside the metropolitan areas of Washington and Paris. For those of us who enjoy the comforts and amenities of a modern metropolis, the case for the time-series approach may seem a bit weak. However, from the perspective of developing economies, it is more reasonable. Such countries have limited access to basic commodities and services we associate with the modern world. Furthermore, in developing societies such as Niger's, "complex reciprocal family relationships provide solidarity between urban and rural areas, between households, between generations, and between groups with different levels of welfare. They help cope with crises, diffuse wealth and poverty[...]"(World Bank (1996)). Seen in this light, Niger might not be so different from nineteenth-century America after all. Perhaps there are lessons to be learned from history. Perhaps there is hope for Niger.

Washington D.C., February, 2011

Contents

1. Compulsory Schooling Laws and the Rise of Mass Schooling in the United States, 1850-1920

1.1. 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 a rise in 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 fields, shops or factories. In the decades between 1850 and 1920, the United States became a world leader in mass schooling, and child labor was successfully eradicated.

In parallel with a 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, romanticized concept of childhood appeared for the first time. As a result, children became economically "worthless" but emotionally "priceless" (Zelizer (1985)).

In the next two chapters, I focus on the relationships between compulsory schooling laws (CSLs), school attendance, and fertility in the United States. I focus first on the relationship between education and CSLs, deferring the study of the fertility effects to the next chapter of this dissertation.

In this chapter, I provide a direct test of the effects of government policy on educational outcomes. I test the effects of compulsory schooling laws on school enrollment 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 should be associated with similar observable and unobservable characteristics. However, because 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 living in states that did not must be related to the enactment of compulsory schooling.

The existing literature has been mostly skeptical about the effects of social legislation. In the United States, studies have found few effects stemming from early CSLs or CLLs either in inducing school enrollment or reducing the incidence of child labor (Landes and Solmon (1972); Margo and Finegan (1996);Moehling (1999)), while the effects of later legislation are firmly established (Lleras-Muney (2002)). In a way, this is not surprising. The effects are often difficult to measure, especially due to the multiplicity of confounding effects at the state level (e.g., Landes and Solmon (1972)). Even in the case of the studies that use micro-level data, the effects are difficult to identify in instances in which census data about labor force participation and age are of poor quality.

I study the evolution of school enrollment around the time when CSLs were first introduced. The novelty of my approach is that–similar to Card and Krueger (1994)–I use data only from border regions across which the legislative change happened at different times. Contrary to the majority of earlier evidence (Landes and Solmon (1972), Margo and Finegan (1996)), I find that legislation increased school enrollment by about 7%. 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 school enrollment of only those children who were covered by them. The finding that a "placebo" CSL has no effect on education also supports my main hypothesis: CSLs effectively increased enrollment.

Next, I turn my attention to the long-term consequences of CSLs. In particular, I study whether the short-term school enrollment effect of CSLs translated into higher educational attainment later in life. To do so, I set up a difference-in-differences estimator where I compare the attainment of cohorts that were slightly above and below the age of treatment. As in the analysis of the contemporaneous effect of education, I compare these cohorts just in the areas around the border. I find that education attainment increased by 0.3 years as a consequence of the legislation.

This chapter 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 effects of state interventions to reduce child labor and increase schooling (Doepke (2004); Doepke and Zilibotti (2005); Galor and Moav (2006)). In addition, this chapter is related to a number of classical papers in labor economics that focus on the relationship between compulsory schooling, educational attainment and wages (Angrist and Krueger (1991); Acemoglu and Angrist (2000); Lleras-Muney (2002); for a complete survey Card (1999)). This literature looks at a later period in U.S. history and has a different focus than this chapter as they merely use compulsory schooling as an instrument when studying returns to education.

1.2. A Brief History of Compulsory Schooling in the United States

During most of the nineteenth and twentieth centuries, the United States was a leader in terms of education. By the mid 1800s, primary enrollment rates were higher in the U.S. than in any other country. Furthermore, as early as the 1910s, a "high-school movement" started promoting secondary enrollment, which would then become common. According to Goldin and Katz (2003), the relative success of the United States was due to some "virtues" of the American system and, most notably, its egalitarian nature combined with the public provision by small fiscally independent districts. By promoting equal opportunities, these features are seen as the key to understanding America's education leadership in the nineteenth and twentieth centuries.[1]

The road to a system of free public schools was initially quite slow. At first, schools were financed with a mixed system of local taxes, parental contributions or "rate bills," and the proceeds of public land sales. Between the 1830s and 1860s, groups of reformers led by Horace Mann organized a crusade for "free schools" which ultimately succeeded in eliminating tuition fees. After this achievement, reformers focused on more ambitious goals: in particular, compulsory school attendance for all children.[2]

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 and Goldin (1989); Moehling (2005)).

Inspired by the determination of persuasive reformers, Massachusetts became the first state to pass a compulsory schooling law in 1852. Although slow at first, the diffusion of CSL legislation was steady from North to South (see Figure A.1). By 1920, the law had spread to all states in the Union, transforming the country into a world leader in mass schooling.[3]

Exactly what part compulsory schooling played in this picture is not clear. A naïve look at the enrollment rates reveals that overall enrollment increased substantially between

[1] A complete survey on the history of education in the U.S. is included in Goldin (1999). Much of this section is based on this and on Goldin and Katz (2003). The U.S. leadership in education should not be interpreted as limited progress in other countries, *cf.* Mitch (1983).

[2] A recent paper by Go (2008) analyzes the political economy of the elimination of rate bills; the political economy situation emerged from the fact that richer areas in the state had to cross-subsidize poorer regions. See Mitch (1986) for a comparative analysis for England.

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

1850 and 1920, exactly at the same time compulsory schooling was becoming widespread (Figure B.1). Which part of the increase in enrollment is due to the enactment of the laws is still debated.

There is a strand in the literature that claims compulsory schooling laws did not play a major role increasing enrollment (See Ensign (1969); Goldin (1999)). According to this view, such laws were enacted as a *consequence* of high enrollment (Landes and Solmon (1972)) and had only a marginal effect on school enrollment when combined with child labor legislation (Margo and Finegan (1996)).

In what follows I argue that the negative results found in the literature can be partly explained by the identification strategies employed. For instance, Landes and Solmon (1972) combine aggregate state data for the U.S. in the decades before and after the passing of the legislation. I suggest that given the importance of state unobservables, this strategy could be improved by using microdata from a narrowly defined area around the state borders. I explain the details of the identification strategy in the next section.

1.3. 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 naïve 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 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 had already as many as 87% of its children at school, limiting the potential for further improvement. In contrast, southern states like Alabama, Virginia or Mississippi had enrollment rates around 40%.[4] Even without the advent of compulsory schooling, it is clear that enrollment in the South would have grown faster than enrollment in Massachusetts.

[4]Specifically, Alabama, Virginia and Mississippi had 41%, 41% and 43% of the children in ages 6-14 attending school. Note that the differences in initial enrollment are not due to the ethnic composition of the different states as the 1850 Census reports data only about free persons. The percentage of whites among free persons is invariably above 90%. For the four states used as a comparison in the text, the percentage of white among free people was over 99%, with the exception of Virginia for which it was slightly lower at 94%. All these percentages are calculated from the weighted Integrated Public Use Microdata Series (IPUMS) sample for 1850, and based on children in age 6-14.

The identification strategy pursued in this chapter is to define an appropriate control group. In order to do this, I exploit the discontinuity of state borders by limiting my attention to regions that are close to those borders where the change in legislation occurred. Rather than comparing the school attendance of people living in Massachusetts and Mississippi, I compare the outcomes between people living–for example–in Berkshire county (MA) with people living just across the border in Columbia county (NY) [5]. When possible, I use township level data (see Data Appendix).[6] A complete county map for these borders between 1860 and 1910 and for the townships between 1860 and 1870 is presented in Figures A.2 and A.3. The construction of the county and township sample is described in the Data Appendix.[7]

Once the treatment and control groups are correctly specified, I apply a standard difference-in-differences (DID) strategy. The idea is to compare the changes in the average outcome variable in both the treatment and control groups. 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 formally,

$$DID = (y_{Treat,After} - y_{Treat,Before}) - (y_{Control,After} - y_{Control,Before}) \tag{1.1}$$

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.

1.4. Did Compulsory Schooling Increase School Attendance?

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

$$y_{i,s,t,g} = X_{i,s,t,g}\beta + \psi CSL_{s,t} + \tau_t + \lambda_s + \mu_g + \epsilon_{i,s,t,g}, \tag{1.2}$$

where $t = 1850, ..., 1910$.

[5] In 1850, the proportion of children in age 6-14 at school was 70% and 74% for Columbia county and Berkshire county respectively.

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

[7] To illustrate the potential pitfalls of using aggregate data and to reproduce earlier findings, I sometimes use samples other than the ones described in the text. I 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.

The sample chosen focuses on children of school age (8 to 14) that lived with both of their parents at the time of the census. For each border, I 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 use the 1850 and the 1860 censuses, because the relevant 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 estimation (Bertrand, Duflo, and Mullainathan (2004)).

In regression (1.2), $y_{i,s,t,g}$ 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 and nationality. I also control for the influence of parental culture, by adding controls for the nationality of the father.[8] Finally, μ_g stands for a series of geographical controls. These controls ensure that I compare neighboring individuals also in a "East–West" sense (see border maps). For that, I use a control for the "segment" of the border in which the individual resides with reference to bordering states, counties, or townships (see Data Appendix). Again, these geographical controls ensure that I only compare people who reside in neighboring regions of the border.

a. Data

This chapter combines microdata 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.[10] Because many of the controls in the regression refer to the household head or to the child's mother, I restrict my attention to those children whose parents are both present in the household at the date of the census.

Tables A.4 and A.1 contain the summary statistics for all the variables used in the school enrollment equation at the county and township levels. The school attendance variable originally recorded whether the child had attended school in the year previous to the census. I refer to it as school attendance or school enrollment interchangeably.

In general, Table A.4 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

[8] See Fernández and Fogli (2009)

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

[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.

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 occupational and ethnic groups. Whereas white children have an average enrollment of 83%, this figure reaches only 50% for black children.

b. Main Results

The first thing to notice about school attendance is that it was already quite high at the moment the laws were enacted (Cf. Landes and Solmon (1972)). Despite this, it is also evident that changes in school attendance depended on whether the child was on the side of the border that enacted compulsory schooling. Figure A.4 shows that school attendance increased faster on the CSL side of the border. Initial enrollment is slightly higher on the No CSL side of the border. Given this initial difference in levels, I will check the identification strategy by checking that the CSL and No CSL states follow similar trends.

The average school enrollment by age group also varies in such a way as to suggest that schooling laws increased enrollment–the increase in school attendance in the CSL states occurs exactly for the ages covered by the law (8–14). In contrast, the change in enrollment of slightly younger or slightly older children is essentially the same on both sides of the border. This strongly supports the hypothesis that CSLs directly increased school attendance.

Regression analysis of the effects of compulsory schooling on educational attainment is presented in Table A.2 for townships at the state border where legislation changed. The table shows that school attendance increased by about 7% after the introduction of compulsory schooling. 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. This is consistent with the finding that the samples of the CSL and no-CSL states are very similar and comparable. Despite the slightly different initial *levels* in school enrollment, the introduction of additional control variables seems to have little effect on the point estimate for school enrollment, marginally improving the fit of the model.

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. The results in the table suggest that the effects for early borders might have been higher (12%) than those in the late border (6%). This finding is unsurprising considering that that enrollment showed a secular increase between 1850 and 1900, reducing the scope for further improvement.

Next, I illustrate the importance of forming appropriate control groups. I first replicate the well-known finding at the state-level that CSLs have no–or the "wrong"–effect. Next, I show how the real effect becomes clearer as we focus on more and more comparable geographical units. Tables A.7 and A.8 repeat the same regression as before, but for different geographical border definitions. From larger to smaller jurisdiction, the same baseline regression is run for the country as a whole, the border states (A.8 and the border counties A.7. Using the full sample or the state borders, it is clear that there are severe biases. Although the differences between CSL and No-CSL states are significantly estimated around 20%, they disappear the moment I introduce state and time-fixed effects. That is, when focusing at *changes* rather than *levels* of enrollment within states, it turns out that these become negative and significant. If we were to interpret these results as causal, we would have to conclude that CSLs *decreased* school enrollment by 4%. Of course, the problem here is that states with CSLs have initially higher enrollment that states that did not pass the law. As argued by Landes and Solmon (1972), the states that begin with low enrollment have more scope for catching up with other states. The problem is that the treatment and control group are simply too different. Note also that the coefficient remains negative, although somewhat smaller, even when all the controls available are added in the census. That is, for treatment and control groups that are too different, there is no number of control variables that can eliminate the bias.

In the second part of Table A.8 I look at border states. This reduces the sample to about a third and, quite intuitively, reduces the bias of the point estimate. Even if it is still negative, it has been cut by half just by reducing the sample to border states. When I further reduce the sample to border counties (A.7), I obtain results that begin to resemble the main–township–specification. The reduction of the treatment and control groups from geographically broad areas to very circumscribed border towns seems to *monotonically* reduce the size of the bias. I interpret this smooth reduction in bias as an indication of the appropriateness of the geographical border strategy.

Finally, I also report in my main specification individual regressions for black children. Insofar as compulsory schooling laws should affect poor children more, we expect the effect of the legislation to be larger for them as well. The point estimate is indeed substantially bigger, indicating an increase in the probability of attending school of about 30 percentage points as a consequence of the law. These results are consistent with black children "catching up, " in terms of school attendance, with their white counterparts. It is also consistent with later literature on the effects of child labor laws (Lleras-Muney (2002)). However, given the small sample sizes, these results should be interpreted with caution.[11]

[11]The fact that black children would reap most of the benefits of compulsory schooling was clearly understood by policy-makers and, quite likely, played a role in deterring the passage of CSLs in the South. When considering a CSL for Georgia in 1909, one member of the Georgia legislature stated

c. 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 where the treatment did not occur, I should not be able to find any effect. A natural way of testing this in the context of my model is to run regression (1.2) but for the period immediately *before* the legislation was passed—that is, to assume that the treated group received the treatment at a moment in time in which it did not. Towards this end, I construct a set of "placebo" compulsory schooling dates for the decade immediately prior to the decade in which the treatment actually took place.[12]

The results of this placebo estimation are presented in Table A.3. For the same categories as in the main specification, the effect of the placebo compulsory schooling is small 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 my estimator simply reflects a generalized increase in enrollment that is unrelated to compulsory schooling legislation. If that were the case, then I 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 run 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 A.5, 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%. When I perform a similar exercise using placebo CSLs, 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 different period of time.

Finally, I investigate the reasons why my chapter finds a result that had previously not been observed in the literature. In order to do so, I run regression (1.2) but

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.

[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.

using all the available observations—not just those that correspond to the border.[13] Comparing Table A.8 with Table A.7 reveals a story that is consistent with previous studies on compulsory schooling (Landes and 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%.[14] However, as soon as a full set of state dummies is added, I find a *negative* impact of about 5%. The reason for this is simple. The original identification was obtained by 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 high enrollment rates on the order of 80%, compared to an average of 60% for states without the law. Even if legislation increased schooling in CSL states, this effect will be overshadowed by the convergence in school enrollment experienced by the other states. Yet I observe that as one moves to narrower geographical definitions (i.e. "border states," "border counties") the negative effect of compulsory schooling vanishes.

It should be noted that the previous research of Landes and Solmon (1972) did not have the benefit of the detailed geographical data I use 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. According to their view, CSLs came about when they were no longer needed. In a word, this analysis is consistent with previous findings in the sense that had I used their methods, I would have arrived at the same conclusion.

Finally, let me focus briefly on the size of the effect. At first sight, a 7-10% increase in school attendance may seem meager. However, if we take into account that this effect occurs in regions with relatively high enrollment rates (80%), my research finds that a CSL is responsible for closing between one-third and one-half of the gap to perfect enrollment.

1.5. Long-Run Analysis

In the first part of this chapter, I considered the short-run effects of CSLs on school attendance. I established that the enactment of the legislation increased school enrollment in the short run, at least in the border areas of the state. A further question is whether this increase in short run school enrollment translated into permanent higher

[13]The summary statistics are contained in Tables A.6 and A.5.

[14]This coefficient is simply picking up the advantage of the CSL states. Go and Lindert (2008) attribute the differences in the levels of schooling to the fact that the North had cheaper and autonomous schools, and greater diffusion of voting power.

human capital accumulation. In other words, did people in CSL states actually get more years of education as a consequence of the legislation? If so, how much more education?

a. Data

The first U.S. census that contains information about the number of years of schooling is the 1940 U.S. Federal Census. Unfortunately, the IPUMS samples after 1930 are much poorer in terms of geographic information. In particular, there is no information about county or township of origin. The only geographical variable available is the "State Economic Area" (SEA). Consequently, in the analysis of this section of the chapter I do not benefit from the fine grid of the townships or counties. Instead, I am forced to run my regressions on a larger geographical area. The problem of the larger size of SEAs as compared to townships and counties is mitigated by two features of SEAs. First, they pool contiguous counties in such a way that a homogeneous economic area is defined. That is, the definition itself of the economic area within the state is done so as to ensure comparability in terms of socio-economic characteristics. Also, SEAs are strictly defined at a state-level, allowing me to apply a similar "border" strategy.

b. Identification Strategy

I focus on people living on the 1910 CSL border who are active in the labor market as of 1940.[15] To do so, I have chosen to focus on the sample of individuals that at the time of treatment were between -15—i.e. were born 15 years *after* the passing of the CSL– and 29 years. I have assigned them into 9 five-year-interval age groups. In order to define the treatment status of an individual, I look at his age, the year in which the legislation in his state was passed, and his state of residence. Clearly, only children that were below 5—and lived in a CSL state– received the treatment fully. The control group are children that were above the age for compulsory schooling. This can be interpreted like a difference-in-difference estimator, where the difference in performance between "older" children and "younger" children is compared between states that had the CSL and those that did not. In regression terms, the DID estimator is easily obtained with the following basic OLS specification,

$$Education_{i,s,b} = \alpha + \beta CSL_s + \gamma Age\ Group_{i,s,b} + \psi CSL_s \times Age\ Group_{i,s,b} \qquad (1.3)$$

[15] I had to discard earlier CSL borders because the people treated in those would simply be too old to be active in the labor market. Furthermore, the probability of education misreporting, a problem already high in the 1940 census Goldin (1999), could be potentiated if I use a sample of relatively old people.

where $Age\ Group_{i,s,b}$ refer to people with ages $i = \{[-15,-11],[-10,-6],\ldots,[25,29]\}$ at the time of treatment living in state s and border segment b. The border segment just groups SEAs that are exactly opposed to each other in the border. This is to prevent the model from being identified by differences in treated and control individuals located in SEAs that are far away from each other.

In the main specification of this chapter, I just test the effect between the "young," treated samples (aged -15 to 5 at treatment) and the "old," control sample (aged 14 to 29 at treatment). The main variable of interest in my regressions is the interaction between living in a CSL state and whether it is in age to be treated by the law (less than 5 years old). As before, this has a simple difference-in-difference interpretation given by:

$$DID = (Educ_{Young,CSL} - Educ_{Old,CSL}) - (Educ_{Young,NoCSL} - Educ_{Old,NoCSL}) \quad (1.4)$$

If the young children in CSL state perform significantly better than the old children in the CSL state relative to the no-CSL state, then that is interpreted as evidence that the CSLs were actually effective. In the specifications, I include a number of individual controls like foreign status, whether nor not a state native, occupations, farm status, gender, race, and the interaction between these two. Also, I add fixed effects at three different geographical levels: SEA, border segment, and state.[16]

Finally, I always refer to the "Main Specification" as that which has full individual controls, state FE and border segment FE computed over people who are native from the state in which they reside and who have been residing in the state for the last 5 years. By choosing a sample of "non-mobile" population I tackle the obvious selection problem: what if CSL states are simply more developed and manage to attract an educated workforce? If that were the case, the observed difference would not be due to the school system in the CSL state but rather to the self-selection of immigrants.

c. Summary Statistics

In table A.9 I present a set of summary statistics for the main variables used throughout the analysis by CSL status. The average age of the individual used in the sample is roughly 36 years and the amount of education is about 7 years. In order to see the time profile of educational attainment, I present graphs of the average outcome variables by age. Figure A.6 presents the average attainment by age at treatment. I expect only

[16]Since border segments and states are formed from grouping SEAs, it is impossible to include controls for all three at the same time. In order to convey the estimators a "matching" interpretation, it is more appealing to use state and border segments fixed effects. However, I use full SEA controls, which are in principle more general, in one specification.

people who were less than 6 years of age at the time of the passing of the law to have received the full impact of the legislation. Two things are immediately apparent. First, there is a marked increasing trend in educational attainment over the period. In the 30 years between 1890 and 1920, children increased their average school attendance by about 1.5 years, or about 20%. Strikingly, the average number of years of schooling is already quite high at the beginning of the period–roughly, 7 years of education. This is consistent with the available evidence in the sense that when CSLs are introduced, school enrollment is already quite high. As in the case of contemporaneous school enrollment, it is the people in no-CSL states that have initially slightly higher education levels. Children who are older than 6 at the moment of the enactment of the legislation seem to have about 0.1 *less* years of education in CSL states. However, the data reveal that children in CSL regions who were below 6 at the moment of the enactment of the law have an average 0.2 years more of education than similar children in no-CSL areas. Even more notably, the dramatic change in educational attainment occurs exactly at the age of 5.

d. Regression Analysis

The results from this empirical exercise are presented in Table A.10. Panel A presents the estimates of increasingly complex models. The first regression computes a simple DID without any controls, which yield a point estimate of about 0.45 years of education as a consequence of the legislation. Considering that the average schooling for the "old" children is about 6.8 years, this coefficient would represent a 7% increase in education. As I add individual controls, the coefficient is reduced to 0.28 and it remains surprisingly stable, just settling around 0.24 when I add state, border and SEA fixed effects. This suggests that regional differences are not driving the effect of compulsory schooling.

One might wonder how much the estimation relies on the specific choice of age groups. For instance, it could be the case that the estimate is driven by the anomalous behavior in some extreme age-group, distant from the treatment time. In order to test for this, I included a parsimonious specification in which I compare exclusively the groups of people just above (15-20) and below (0-5) the treatment ages. When I run the full specification, the results are remarkably similar. The point estimate of 0.23 is indistinguishable from the previous estimation. The only difference is that since observations are cut by three-fourths, the standard error increases slightly.

In the final specification of panel A I check for potential trends. One could imagine the CSL states to be "catching up" with no-CSL states as they, on average, had lower education numbers initially. If that were the case, maybe part of the effect I am capturing in my DID estimator simply reflects this fact. In order to test for this, I look for older cohorts that were not treated on either side of the border. If the CSL states were "catching up" to the no-CSL state, I should be able to get a positive and significant estimator for the interaction coefficient. When I compare people aged 15-20 and 20-25, the result is not different than zero. Interestingly enough, this result is not driven by a large standard error but rather by a point estimate close to zero. This evidence seems to support the idea that the main result is not a product of trends in the data.[17]

One could also imagine that the effect is driven largely by groups that were not affected by the law. Clearly, people who were born outside the state are likely not to have been affected by the legislation. Even if they were born in the state, the fact that they were residing in other state before raises questions as to whether they were actually treated by the CSL. Although in all the main specifications I add dummy variables to acknowledge these situations, the safest approach would be to exclude observations involving people born in other states or that were residing in other states in the last 5 years. In panel B, I do this and find that the exclusion of these observations actually increases the point estimator slightly to 0.3 while keeping it highly significant.

Finally, when I run separate regressions by gender and races I find that there are no major differences in the effect across groups; all of them have individual effects around 0.3. Non-white males and females, however, deliver more noisy estimators. The fact that these groups have very few observations makes it difficult to find statistically significant results.

A related question to answer is whether CSLs affected the probability of completing a certain education level: more precisely, elementary school. If the effect of the CSL just affects the upper tail of the distribution of years of education, one could be skeptical as to how much of it came from legislation and how much from other reasons. If CSLs were enforced, the change in the probability of having completed the full elementary education (8 years) should be higher in the CSL states than in the other states. The intuition is again confirmed by the first specification in Panel C. There is about a 4% higher chance of getting a degree as a consequence of the legislation, and this is highly significant. When the same increase is tested for high-school education, I observe that while the effect is still positive and about 2%, the significance level is much lower. This last finding is not necessarily bad news, it just highlights the conclusion that most of the effect of compulsory schooling translated into getting people through elementary education. Again, this is consistent with the fact that the rise in secondary education occurred after the 1910s (Goldin and Katz (2003)).

A final robustness check concerns the effects by age-groups. Instead of grouping the cohorts in the categories of "old" and "young," I run regression (1.3) allowing the CSL to have a different impact for each of the categories.[18] As argued above, it is possible

[17] One can interpret the results of this exercise as testing for a "placebo" law much in the same fashion as I did in the first section.

[18] The base category in this regression is the group that had partial treatment (ages 6 to 14). All the coefficients are to be interpreted as differences relative to this group.

that the effect of education in the main specification is driven by one specific group. In order to control for that, I run the main specification allowing for the education effect to vary according to age group. The coefficients of this regression are presented in Fig. A.7. Confirming the initial intuition from Fig. A.6, there is a distinctive jump in the levels of education for all age groups below 5 years of age. People slightly older who have received the treatment are indistinguishable across the border relative to people that received a mixed treatment. Instead, people slightly younger (age 0-5) at the time of treatment are significantly more educated on the CSL side of the border. This constitutes additional support for the basic finding.

The empirical exercise conducted in this section seems to support the idea that the effects of compulsory schooling were not only contemporaneous. Using a similar identification strategy but focusing on the same state border 30 years after the enactment of a CSL, I am able to show that people treated by the legislation were likely to have about 0.3 more years of education. The coefficient is stable and robust to different specifications and controls. In particular, it does not seem to be driven by a specific age-group or by different time trends.

1.6. Conclusion

The United States was the first country to achieve modern mass education. In the decades between 1850 and 1930, Americans achieved universal elementary school enrollment and were far ahead in the race for high school education (Goldin and Katz (2003)). Historians have attributed this effect to a number of causes, ranging from autonomous school provision to the egalitarian nature of the system (Goldin (1999)).

Interestingly, it has usually been argued that legislation played no role in the increase in schooling (Landes and Solmon (1972)). In the light of the initially high enrollment rates, CSLs were interpreted as a consequence of the increase in schooling, rather than as the cause. The skeptical view of early social policy is not limited to compulsory schooling. More generally, the effectiveness of social legislation during the nineteenth century has also been questioned (Brandeis (1966)).

This chapter argues that, contrary to previous evidence, CSLs increased enrollment in the states that passed such legislation. In contrast with previous research that relied on aggregate data, I use microdata from the U.S. census to identify the effects of schooling. In the spirit of Card and Krueger (1994), I use the border effect as my source of identification. 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% on the side of the border that received the treatment. My findings are consistent with the previous finding that education levels were high to start with. However, I argue that even with initially high levels of enrollment, the legislation is responsible for closing about one half of the gap to full attendance.

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. All of these additional checks are consistent with the main hypothesis: CSLs effectively increased education.

In addition to identifying the contemporaneous effect, I also examine the long-term consequences of CSLs. I find that 30 years after the treatment, we can still observe a difference in educational achievement between individuals that were treated by the law and those that were not. This difference is persistently estimated at about 0.3 years of education, representing about a 7% increase in educational achievement.

Aside from the purely historical interest in identifying the effect of legislation, my research highlights the potential role of education in the transition to development. A number of papers have argued that the key to the transition to self-sustained growth is the "quality-quantity" tradeoff (Becker and Lewis (1973) and Becker and Barro (1988)). Some recent papers have suggested that compulsory schooling and child labor laws could serve as a means of accelerating the transition to development (Galor and Weil (2000), Doepke (2004)). My chapter identifies an exogenous variation in "quality" that should, according to the theory, explain the subsequent change in "quantity," thereby triggering the fertility transition. I will examine these effects in the next chapter of my dissertation.

2. Compulsory Schooling Laws and the Decline in Fertility in the United States, 1850-1920

Joint with Hans-Joachim Voth

"Fulmina il Signor Iddio maleditioni e scomuniche contro quell'i quali mandano ò permettano sÿno manadati li loro figlioli, e figliole si legittimi come naturali inquesto Hospedale Della Pietà havendo il modo e faculta di poterli allevare esseendo obligati al resarcimento diogni danno e spesa fatta per quelli, Ne possono esser assolti se non sodisfano, come chiaramente appare nella bolla di nostro Signor Papa Paolo Terzo data adi 12 Novembre L'anno 1548." Ospedale della Pietà, Venice.[1]

2.1. Introduction

In trying to determine the reasons for America's wealth, Adam Smith highlighted the role of rapid population growth. In Europe, the continent where Smith was writing from, raising a child was so costly that the Pope himself had to threaten parents with eternal damnation if they unnecessarily sent their children to orphanages. In contrast, in America labor was "so well rewarded that a numerous family of children, instead of being a burthen, is a source of opulence and prosperity to the parents." Noting that childbearing is the source of wealth for the family, he concluded that "The value of children is the greatest of all encouragements to marriage." (Smith (1776))

At almost the same time as Adam Smith was writing, fertility rates began to fall in the United States. Total fertility rates dropped from 7 children per woman in 1800 to slightly over 4 in 1880. By 1930, American women had on average 2.4 children over their lifetimes–a figure not very different from modern fertility rates.[2] More importantly, this decline seemed to be idiosyncratic to the United States.[3] European countries underwent their own fertility transitions only later in the nineteenth and early twentieth centuries.

[1]This is the sign at the entrance of the Ospedale della Pietà in Venice. It loosely reads "Let God cast curses and excommunications to those who send–or allow to be sent–their sons and daughters, both legitimate and natural, to this Ospedale della Pietà having the means and ability to raise them. They are obliged to the compensation of every loss and expenditure made for them; they cannot be absolved unless they comply with this as it is clearly stated in the bull of our Lord Pope Paul III given on the twelfth of November of the year 1548".

[2]Fertility rates correspond to the white population in the United States. Data from Haines (2008a).

[3]The only exception is France. The French demographic transition started at about the same time as the demographic transition in the U.S. Its causes are often related to the effects of the French revolution. For a discussion, see Binion (2001).

In the European case, the fertility rates dropped simultaneously and with remarkable coordination.[4] Why would American fertility behave so differently?

If there is one way in which America was special, it was in its educational system. During the same period in which fertility rates declined, the United States consolidated what has been described as an "egalitarian" education system, one that ensured equal opportunities for all.[5] As a consequence, the U.S. became a world-leader in public education. Within a few decades, free elementary schools reached all corners of the country, while legislation drove children out of the sweatshops and into the classrooms. Childhood in America changed forever.

In this chapter, we examine the effects of government intervention on fertility. In Chapter 1, it was established that the enactment of compulsory schooling laws (CSLs) actually increased school enrollment in the short run and educational attainment in the long run. In this chapter, we test the effects of CSLs on marital fertility. In standard economic models like Becker (1991), CLLs and CSLs increase the relative cost of children, leading to a reduction in fertility. This direct effect occurs contemporaneously with the introduction of legislation. In this chapter we use an identification strategy similar to the one in Chapter 1. In essence, we focus on the difference between fertility outcomes before and after the enactment of the legislation and on either side of the state border where the legislative change occurred.[6]

This chapter proposes a new fertility measure to overcome the deficiencies of the available historical census data. Using a methodology similar to what the United Nations recommends for countries with poor vital registration, we construct a measure of fertility based on the ages of children living with their mothers at census time (cf. La Ferrara, Eliana, Chong, Alberto, and Duryea, Suzanne (2008); United Nations (1983)). With the time-series data on fertility, we are able to test changes that occur simultaneously with the law's introduction. Along the border of states changing their legislation, we compare the number of births that occurred after the CSLs were in place to the number beforehand. Considering a time series of 15 years of births, we find that women reduced their fertility by about 15% as a consequence of the introduction of the CSLs. 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 were young at the moment the change in policy occurred. This is consistent with the notion that the effect of the laws should be greater for women who have not yet made most of their

[4] An early investigation of the causes of the fertility transition in Europe was conducted by Princeton's Office of Population Research. In a nutshell, the "European Fertility Project" argued that the European fertility transition was not explainable by economic variables but rather by diffusion and innovation. Coale and Watkins (1986). Some recent research questions the validity of the main findings of this study Brown and Guinnane (2003).

[5] See Goldin and Katz (2003). Note that "all" refers to the white population in this period. In fact, African Americans would be segregated from American schools until the 1954 Supreme Court ruling *Brown vs. Board of Education of Topeka*.

[6] See Chapter 1 for a complete discussion of the identification strategy.

fertility decisions. The effects are also robust to the correction for autocorrelation in the treatment (Bertrand, Duflo, and Mullainathan (2004)).

After establishing the direct, "Beckerian", channel between compulsory schooling and fertility, we turn to the indirect effects. The recent development literature has stressed the connection between female education and lower fertility (Breierova and Duflo (2004)). In order to assess the channel, we use the 1880 U.S. census microdata to investigate the completed fertility periods of women who were treated by CSLs in their childhood. We find that the short run effect associated to the CSLs was up to 0.15 children, or a decline of 15% with respect to average fertility. Furthermore, we use the natural experiment of a border change in Massachusetts to investigate whether the results are robust. The effects of the legislation change in Fall River, Massachusetts, suggest an even stronger long run effect (0.3), which is particularly concentrated among migrant women.[7]

This chapter 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 effects of state interventions designed to trigger the fertility transition (Doepke (2004); Doepke and Zilibotti (2005); Galor and Moav (2006)). In addition, this chapter is related to a number of empirical studies that attempt to measure the "quantity-quality" trade-off (Rosenzweig and Wolpin (1980b); Angrist, Lavy, and Schlosser (2006)). It should be noted, however, that this literature stresses the need to find good instruments for fertility to pinpoint its effects on education and on other labor outcomes. In contrast, this chapter examines how exogenous changes in education affect the optimal fertility decisions of households.

2.2. From Mass Schooling to Lower Fertility

The relationship between family size and education has been on the research agenda of the social sciences for a long time. Theoretical efforts in both economics and demography have suggested 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 (1991), Becker and Lewis (1973). The primary 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 for the negative relationship between the "quantity" and "quality" holds true in economic growth models where altruistic parents decide on the

[7] One should be cautious with the comparison of short run and long run fertilities as these variables are not defined in the same way. In particular, the decline of 0.15 refers to the comparison of the number of births in the 5 years after the law with that in the 5 years before. Differently, long-run fertility refers to the stock of children.

size of the household (Becker and 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 transition from Malthusian stagnation to modern growth. During this period, fertility is typically influenced by two opposing effects. First, since children are a normal good, income growth tends to have a positive effect on fertility. Second, as the transition to development continues, the skill premium rises, which increases the opportunity cost of children. At some point on the road to development, the second effect prevails and the relationship between income and fertility becomes negative.

Given the crucial role of the fertility transition, there have been some attempts to quantify the impact of social legislation (e.g., 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 and Udry (1999); Moav (2005)). In particular, Hazan and 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.[8] 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 Korea, Brazil, and the United Kingdom. While Korea rapidly enacted both a child labor and a 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 regime to a capitalistic low-fertility regime. The process occurs not only through the reduction of the market value of child labor but also through the increased school-related expenditures of families and, more generally, from the introduction of modern values that oppose the prevailing "family morality." All these factors belong to the so-called demand channel between education and fertility. Easterlin and Crimmins (1985) identify other channels that work in contradicting ways. Mass education can increase the natural supply of children as education improves hygienic conditions while possibly devaluing cultural practices that limit natural fertility (e.g., intercourse taboo,

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

prolonged breastfeeding). All of these would increase the potential supply of children. On the other hand, mass education also increases awareness of contraception, thereby reducing fertility.

The demographic literature has also attempted to empirically assess 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 and Guinnane (2003) have disputed this conclusion by pointing out statistical flaws in the methodology used by the EFP papers.[9]

In contrast with the European experience, evaluating the evolution of fertility in the U.S. has been difficult. The main complication is related to data availability. While before the twentieth century the church functioned as a *de facto* demographic registry in Europe, the same did not happen in the U.S.: birth registration is an exclusive right of the states. It was not until the 1930s that an agreement was reached and a nationwide birth registration department was formed.

The historical fertility data for the U.S. is therefore quite incomplete. Nonetheless, American demographers have proposed a number of hypotheses in order to explain the decreasing fertility rate. Yasuba (1962) and Easterlin (1976b) stressed the role of land availability either in delaying marriage or increasing the costs of bequests. A second hypothesis by Sundstrom and David (1988) focuses on the role of children as a form of old age insurance. In their view, the development of a market economy increased the outside options for children, eliminating their value as insurance: children could now simply "default" on their obligations. Finally, Carter and Sutch (1996) propose a variation of the old age insurance argument that focuses on the development of credit markets in the U.S. In empirical work, all of these hypotheses have received some support, making it difficult for the researcher to choose between them.[10]

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 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 and Wolpin (1980b); Lee

[9]The two flaws concern the aggregation level of the data used and the use of only the cross-sectional side of the data.

[10]A complete survey of fertility in the U.S. is found in Haines (1994). For a more recent survey see Jones and Tertilt (2006). Other classic references in the demographic literature on U.S. fertility are Craig (1993), Forster and Tucker (1972), Lindert (1978) and Easterlin (1976a).

(2004); Angrist, Lavy, and Schlosser (2006)). A different approach involved estimating the unobservable "fecundity" of couples based on a parametric reproduction technology (Rosenzweig and Paul Schultz (1987)). All these studies find that fertility causally reduces the educational attainment of children.[11]

In contrast to the attention received by the effects of fertility on education, 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 and Krueger (1991); Acemoglu and 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.

Although our paper aims at a similar question, there are a number of dimensions in which it differs from Leon (2006). We use a different fertility measure, one that is based on the birth histories of the mothers. In addition, we examine the contemporaneous effect on the fertility of women who were of reproductive age at the moment the policy was introduced, and we study a period during which this demographic transition was taking place. 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.[12] Our empirical strategy, however, is based on women living around the borders of jurisdictions whose children were affected by newly-introduced CSLs, comparing fertility before and after that change (cf.Card and Krueger (1994)).

2.3. Identification Strategy

The identification strategy was discussed at length in Chapter 1. In this section we focus on the specific issues of the fertility data. As discussed earlier, our identification strategy is based on two principles. First, the idea that areas around the state border where regulation changed are comparable. Second, that a proper way to compare the evolution of fertility on either side of the border is to use a difference-in-differences (DID) approach.

One of the attractions of the difference-in-differences (DID) approach is that it is quite

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

[12] 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.

easy to interpret. Assuming that the treatment and control groups share the same trend, the causal effect of the treatment is simply the difference between the change in the outcome variable in the treatment group, and the control group. In mathematical terms,

$$DID = (y_{\ treat,after} - y_{\ treat,before}) - (y_{\ control,after} - y_{\ control,before}) \tag{2.1}$$

The outcome variables for this chapter refer to the fertility of married women. We use two basic measures of fertility: "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 attenuate any sharp response in fertility due to, say, a new social policy. In order to isolate the timing of fertility changes, we construct a flow fertility measure based on the ages of own children living in the household.[14] The procedure is straightforward. For each household, we identify the children living with their mothers; then, by subtracting their ages from the census date, we infer their birth years. Based on this, we construct for each mother a variable (*Births*) that is equal to 1 in the years in which her children were born and 0 otherwise. When constructing the fertility histories of mothers we focus on the 14 years before the census date.

Both stock and flow measures of fertility may exhibit some biases that stem from the fact that they are constructed from Census data. 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—e.g., child mortality, child fostering– the estimates of fertility would be biased downward. In the case of flow fertility, this measure becomes less reliable as we move away from the census date because older children are more likely to have left home already. [16]

[13] The term flow fertility is borrowed from La Ferrara, Eliana, Chong, Alberto, and Duryea, Suzanne (2008), who define a similar measure.

[14] This strategy is widely used by demographers when constructing fertility estimates for countries with poor vital registration (United Nations (1983)). Recent research in economics has also used a similar measure (La Ferrara, Eliana, Chong, Alberto, and Duryea, Suzanne (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 the census date, as inferred from the census schedules.

[16] This is precisely the rationale for cutting off 14 years before the census date. One 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.

While these downward biases may affect the *level* of fertility, there is no reason why we should expect them to affect one side of the border more than the other. In other words, if the identification strategy is appropriate, then the downward bias on the fertility measure should increase only the standard errors without affecting the estimator's consistency. In the data appendix, we use the 1900 census to prove that this is the case and that there is no systematic difference in the gap between the number of children *present* and the number of children ever born in CSL and no-CSL states.

As argued above, the main advantage of flow fertility is that it allows us to pinpoint changes in fertility occurring exactly at the time of the policy innovation. In order to do so, we 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 number of years before census date; that is $(t = 0, 1, 2, ..., 14)$:

$$Births_{i,s,t,g} = X_{i,s,g}\beta + \psi CSL_{s,t} + \tau_t + \lambda_s + \mu_g + \epsilon_{i,s,t,g}. \tag{2.2}$$

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 we 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 regressions of chapter 1. As set up in equation (2.2), 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 has been recently described in the literature and refers to autocorrelation which may bias the standard errors downward (Bertrand, Duflo, and Mullainathan (2004)).[17] A simple solution for this problem 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. This is done by constructing the accumulated fertility of each mother p periods before and after the shock ($Fert(p)_{i,s,t,g}$). Then we run a regression of the effect of compulsory schooling on fertility for mother i living in state s before and after the shock occurred.

$$Fert(p)_{i,s,t,g} = X_{i,s,g}\beta + \psi After_t * CSL_s + \delta After_t + \lambda_s + \mu_g + \epsilon_{i,s,t,g}. \tag{2.3}$$

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.

[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 long number of periods and a highly autocorrelated dependent variable, the bias in the standard errors could be substantial.

Aside from the specifications already outlined for flow fertility, we also report some estimations using the stock measures of fertility. For this we use a regression similar to the ones that were run in Chapter 1, equation (1.2), but replacing the outcome variable from school enrollment to fertility. 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 the 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.

Summarizing, the main empirical estimations of this chapter look at the evolution of fertility 1, 3 and 5 years from the census (2.3) while also running a panel data estimation using the entire birth histories of the mothers as in (2.2).

a. Data

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

The summary statistics of the data are presented in Tables B.2 and B.3 for stock and flow fertility measures. Women in border counties and townships seem to have on average 2.6 children living with them in the household. As for the fertility changes since the law, the probabilities of having had a child during the first 1, 3, and 5 years after the law are roughly 0.15, 0.4 and 0.65. Furthermore, the CSL and no-CSL sides of the border look remarkably similar in terms of controls. Regarding the differences between the county and township sample, the latter seems to have a higher incidence of immigrant women than the county sample (0.2 versus 0.15). Otherwise, these two samples look quite similar. Finally, note that maternal labor force participation is marginal, roughly about 4% during the period both in the county and township sample.

We also consider the differential reaction to the policy change among 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 made most of their fertility choices already, and have little scope for adjusting them. In order to illustrate this point, we consider the change in fertility, five years before and after the treatment. In Figure B.2 two features are worth noticing. First, older women have *negative* changes in fertility. This is just reflecting that women between 35 and 49 are close to the end of their fertility periods. More importantly, older women do not seem to behave differently in CSL and no-CSL regions. Young women, instead, have positive changes in fertility both in CSL and No-CSL regions. Again, this is consistent with the fact that they are in their prime childbearing ages. Note, however, that the increment in fertility is much more reduced in the CSL side as compared with the no-CSL side. Furthermore, the difference is about 0.15 children, or about one-quarter of the average fertility for the 5-year period. Of course this is just a simple comparison of means across groups; for a full causal interpretation we need to resort to regression analysis.

b. Main Results

The main results of the regression analysis are presented in tables B.4 and B.5 for townships and counties respectively. In each table, we 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. We also report the regressions for fertility 1, 3, and 5 periods from the enactment of CSLs in an attempt to attenuate the problem of autocorrelation discussed above. 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 B.4 also includes 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 CSLs 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, we 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 responses 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.

c. Robustness Checks

We also ran the fertility regressions using the stock measures of fertility at both the township and county level. The results are presented in Table B.7. Although the stock fertility measures are quite noisy, the picture obtained from these regressions is similar to our 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 we 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.

We argue that the weak results obtained with aggregate data is due to the nature in which they are constructed, which is incapable of capturing the timing of the policy innovation. The main difference with our "flow" fertility measure is that we compare the fertilities just before and just after the CSL treatment occurred. These measures, instead, 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. When the births that occurred before treatment are mixed with births that occurred after, these measures become noisy and, consequently, they can only weakly reflect the effect of compulsory schooling laws.

We also check for trends in the data using a placebo law as in the previous chapter. Similarly, the idea is to generate a "placebo" compulsory schooling law ten years before the actual date. If we are able to find the effects of CSL legislation even before they were enacted, this would be evidence that the CSL and no-CSL states are following different trends. The results are presented in Table B.7, where the effect of compulsory schooling of education is generally insignificant in almost all the specifications. Furthermore, in the main specification of the chapter with full data and controls, the coefficients are all highly insignificant. As in the case of education outcomes, trends do not seem to be driving the results.

2.4. Long-Run Effects

In the previous section, we have focused on the short-term consequences of compulsory schooling laws on fertility decline. There is yet another channel that has received a considerable amount of attention in the literature, namely, the indirect effects of receiving more education on the target fertility in later adult life (Breierova and Duflo (2004), Caldwell (1982)). In this section we show that the cohorts that were affected by compulsory schooling legislation were more likely to have lower fertility on the CSL side of the border. Compared with women slightly too old to have been treated, we find that the effect occurs exactly on the CSL side of the border at the time the legislation was enacted.

In order to explore this hypothesis, we focus on the same border areas where legislation changed but in later censuses. Clearly, there is an implicit assumption that the people that are, say, still living thirty years later in the legislative border region were actually treated by the legislation. Given that most of the border includes people in relatively non-mobile, rural areas, we believe this assumption is not too strong. In addition to that, we run robustness checks in the sub-sample of people that have been particularly immobile. Finally, similar assumptions are not strange in the literature that use CSLs as instruments for education.[18]

One immediate concern is whether changes in migration can affect the results. After all, it is possible that high-fertility women "vote with their feet" when the CSL gets introduced. If that were the case, the observed differences between CSL and no CSL states would just be a reflection of the change in the pool of women; CSL did not reduce fertility but rather draws high fertility women out of the state. It turns out that the data does not support this hypothesis. Simple tabulations show that the fraction of women in no CSL states who come from the cross-border CSL state remains roughly constant after the introduction of the law. If anything, there seems to be evidence of an increase in the proportion of women that move from the no CSL state into the CSL state. Furthermore, the fertilities of women do not seem to depend on whether they were born in the same state, a border state or another state of the Union.

In order to do the analysis, we have distinguished the women living at the border according to three groups. Older women—aged 15 to 29 at the time of treatment– received no treatment in terms of education. An intermediate group of women—aged 0 to 14 at the time of treatment– received the effects of the legislation change if they lived on the CSL side of the border. Finally, a third group—aged -1 to -15 at the time of treatment– might have received part of the effect. However, since the neighboring states started passing CSLs in 1867, it is a group in which treatment is mixed. In terms of equations, for person i living in state j, we run

$$\begin{aligned} Fertility_{i,j} = \alpha + \beta_0 Age[-15,-1]_{i,j} + \beta_1 Age[0,14]_{i,j} + \\ \gamma CSL_j + \psi_0 CSL_j \times Age[-15,-1]_{i,j} \\ + \psi_1 CSL_j \times Age[0,14]_{i,j} + \mathbf{X_{i,j}} + \epsilon_{i,j}. \end{aligned} \tag{2.4}$$

where the omitted category are the older women and the controls are given by a number of socio-demographic and economic variables (race, national origin, husband occupation, labor force participation). We also include fixed effects at the township level. Note that when township fixed effects are included, these are perfectly collinear with the CSL variable, which is then dropped.

a. Data

We test the hypothesis of long-run effects of fertility using the 1880 U.S. federal census. This census offers a unique opportunity to test our hypothesis with microdata. For

[18] See, for instance, Lleras-Muney (2002).

that year, the IPUMS project has the universe (100%) of people residing in the United States, allowing us to focus our attention to a small portion of the border. Indeed, we can do so while still having enough observations to fit regression models with microdata.

We also focus on the Massachusetts 1850 border for the long run analysis of fertility. These borders are more consistently identified throughout the analysis, as all the townships in the area were incorporated well before 1880. Furthermore, the 1880 census is about 28 years after treatment.[19] This allows us to observe mothers that are near the end of their fertility periods. Looking at the fertilities towards the end the reproductive life is necessary in this analysis to make sure that the effects refer to changes in the overall fertility patterns rather than in timing. For the same reason, in this part we look at the available "stock" measure of fertility. Finally, by looking at women at the end of their fertility periods we abstract from short-term effects of CSLs that were identified in the previous section. [20]

b. Summary Statistics

The summary statistics for the long run regressions are presented in Table B.8 where the main variables used in the regression analysis are included. Among the married women with a spouse present the average number of children at home is 2.26, irrespective of the side of the border they live on. The age-group distribution seems also to be very similar between the treated and control groups. Only some categories of husband's occupation differ between the two samples, while most of the categories have similar means according to a t-test. Of particular importance is the significant difference in the proportion of immigrant women between the treated and control groups. Immigrant women are slightly more common on the CSL side (33% against 39%, t-stat: 16.1). This raises two concerns. First, it is not clear that immigrant women were actually treated by the CSL. According to the U.S. 1900 Census that explicitly asks about the year of migration, about one-third of the immigrants migrated before age 10, and two-thirds did it before age 20. This means that there is a reasonable chance that they might have been treated. A second concern has to do with the difference in fertility behavior of immigrants. Indeed, immigrants have higher fertilities (1.75 against 3.16). It is for these reasons that in the main specification of the regression analysis below we focus on native women who were born in the same state where they are residing.

In Figure B.3 we present the comparison between the fertilities of married women on both sides of the border.[21] As usual, we limit our attention to those households residing

[19]Recall that the Massachusetts CSL was passed in 1852. The second state to pass a CSL was Vermont in 1867, only 13 years before the 1880 census, making it too soon to assess long term fertility effects.

[20]This is because all the states had their CSLs by the time we observe them in 1880 with a marginal exception: Rhode Island.

[21]Technically, we use the number of children living in households composed by a married, state-native

in townships within the 1860 CSL border between Massachusetts and its neighbors. As is typical in these charts, it is possible to see an inverted u-shaped curve of fertility that reflects life-cycle fertility behavior. However, when we focus on differences, there seems to be almost no difference in fertility for women older than 15 at the time of the change in the law. In sharp contrast, women who were between 0 and 14 at the moment of treatment have significantly lower fertilities on the CSL side. On average, these women have about 0.15 fewer children than women just across the border. Interestingly, as we move on to consider younger cohorts, the effect vanishes. This is consistent with the fact that all the neighboring states eventually passed a CSL some 15 or 20 years after Massachusetts.

c. Regression Results

The regression results are presented in Table B.9. In the first column, we present the results of a standard difference-in-differences regression where all women in ages -15 to 14 are considered treated and the older ones are considered controls. Specification (2) limits the attention to the sample of married women who are native to the state in which they reside in 1880. In both cases, there is a statistically significant, negative effect of the CSL on fertility of around 0.1 children, implying a decline in long-run fertility of about 5% with respect to the average fertility of women.

The third and fourth column distinguish between a group of women that are exactly treated by the law—aged 0 to 14– and a second group that received a mixed treatment—aged -15 to -1–. Because some neighboring states were already passing their CSLs when these girls were of school age, it is not clear that the control group was actually untreated. As before, the exercise is done for all women (column 3) and native women (column 4) using a full set of township fixed effects. It turns out that the effect of the law increases to 0.12 for the unambiguously treated cohort. As for the younger cohort that potentially got treated on both sides of the border, the results differ depending on whether all women or only natives are taken. If all the women are considered, then the effect is significant and equal to 0.12. If only native women are considered, the effect vanishes. The most parsimonious treatment, i.e., the one with native mothers in the 0 to 14 age group, confirms the hypothesis that long-run fertility declined as a consequence of the CSL.

In columns 5 and 6, an additional full set of socio-demographic controls is introduced. Of particular interest, we introduce a dummy variable indicating whether the woman is employed. The variable is very significant and very large (about 0.6 children) relative to the mean. All the other variables have the expected sign and, in particular, all the occupations included have a significantly higher fertility associated than the omitted white-collar occupations. Regardless of the model, the estimates for the fertility effect

couple in which the male household head was present at the time of the census of CSL seem to be consistently around 0.1 children, which represents a 5 percent decline in baseline fertility. More interestingly, these results seem to be roughly comparable in size with the ones that we found for the short run effect of fertility in the previous section of this chapter. However, given the different definitions in the dependent variables here and in the short run analysis, one should be cautious with these comparisons.

Finally, in the last column of the estimation we repeat the exercise for natives but ignore 2 townships in the border where there was a change in the boundary. These are Fall River (MA) and Pawtucket (RI). The estimation confirms that the long run effects of fertility are not biased by the inclusion of these two towns, assuming a wrong statehood. We will pursue the border change as an exogenous source of variation in the last section of this chapter.

2.5. Fall River

In the previous sections, we showed that fertility is significantly reduced at the border both contemporaneously to the introduction of the CSL, and in the long run through its effect on the education of young girls. So far we have implicitly assumed the approval of the laws can be considered random. Although plausible, many theoretical and empirical contributions (see discussion in chapter 1) have pointed out that legislation is hardly imposed exogenously on a territory. Instead, new legislation is introduced as the result of the political process, which depends on the socio-economic characteristics of the voters. If those characteristics are correlated with both the approval of the new legislation and the fertility outcomes, then the estimates could be biased. For instance, if more modern societies–such as Massachusetts–are more likely to both have lower fertility and to approve compulsory schooling laws, the estimates calculated above would be biased upwards. As was argued above, the identification strategy kept comparability to the maximum by choosing neighboring states which are presumably more similar in terms of socio-demographic characteristics. Nevertheless, in this section we examine the appropriateness of our exogeneity assumption by exploring the effects of an unanticipated border change.

We explore the consequences of a change in state borders that was the result of a Supreme Court decision and, consequently, is unlikely to be related to the reasons why people favored adopting CSLs in the first place. In particular, we consider the case of Fall River, an industrial town on the border between Rhode Island and Massachusetts. The southern portion of this city was granted to Massachusetts in 1861 by a Supreme Court ruling. We explore the fertility consequences of the exogenous change in jurisdiction in what follows.

Fall River is an industrial city on the border between Massachusetts and Rhode Island. The city became one of the first industrial cities in America, attracting a multitude of industries. The eight falls of the Quequetchan river that flows through the city were an ideal source of power for a growing industry. In 1811, Col. Joseph Durfee opened "The Globe Manufactory," a pioneering spinning mill in the region. This mill was soon followed by many other industrial enterprises, mostly textile mills. Less known than its industrial prosperity is the fact that Falls River had been the subject of an old border dispute between Massachusetts and Rhode Island. As prosperity grew, both states quarreled over control of the city. The dispute ended up at the Supreme Court which understood that densely populated areas should be under the jurisdiction of a single state. In 1861 the state border was moved from Columbia St. to State St., thereby putting the whole city under the jurisdiction of Massachusetts (see Figure B.4). In exchange, the Supreme Court altered other sections of the border, awarding the city of Pawtucket to Rhode Island.

We take advantage of the 100% sample of the U.S. 1880 census for Massachusetts and consider the sample of all married women living with their husbands in the city of Fall River. The fertility variable we consider is the number of children living in the household as discussed above. In the analysis we distinguish three separate cohorts of women. First we consider the women that were young enough to receive the treatment in 1861 (below age 10). These women are not expected to differ in their fertility behavior, irrespective of whether they were born in North or South Fall River. Second, we consider women that were between 11 and 20 years of age at the moment the change of borders occurred. These women were only partly affected by the extension of Massachusetts' CSL if they were born on the Rhode Island side of the city, but they were certainly affected if they were on the Massachusetts side of the border. Finally, we consider women who were between 21 and 30 years of age at the moment the border changed. These women were born between 1831 and 1840 and were too old to have benefited from either the Massachusetts CSL of 1852 or the border change. Therefore, we do not expect any difference in their fertility behavior.[22]

Figure B.5 shows the average number of children for women in the three groups mentioned above living in both sides of Fall River. From the graph we can see a dramatic decline in fertility from older cohorts (none treated) to the younger cohorts (both treated). This effect cannot be attributed, at least not entirely, to the fertility transition: for older cohorts we observe completed fertility, while for younger cohorts this is not the case. Working in the opposite direction, there is a downward bias in the fertility of older cohorts due to the fact that we only observe children who are present in the household (see a discussion of this in the Data Appendix).

Although the fertility patterns for women in North and South Fall River looks similar for the cases in which the sides are either both untreated or both treated, the northern side

[22]Note, however, that in this last group there is a potentially confounding effect. These women were already old enough to be mothers at the time of the border change of 1861. In this case, we cannot disentangle the short and long run effects.

of the town seems to have initiated the decline of fertility earlier than the southern part. The timing of this decline exactly matches the period in which the northern section was under the Massachusetts CSL while the southern section was still not. Furthermore, the size of the effect, roughly 0.3 children less, is big relative to the observed fertility for the period (2.6 children). Of course, these are just crude means; differences between the socio-demographic characteristics of the population could be explaining the result.

The regression we estimate is a very simple specification in which we allow the treatment to interact with the ages of the mothers. As argued above, both old and young women should display similar fertilities irrespective of whether they live in the northern or southern part of Fall River. This is because they are either both treated or none of them is treated. Women of intermediate age (11 to 20 years at the time of treatment), however, are treated if they live in the Massachusetts side of the border but are not treated if they live in the (formerly) Rhode Island side of the border. The estimated equation is then,

$$\begin{aligned} Fertility_{i,j} = \alpha + \beta_0 Young_{i,j} + \beta_1 Intermediate_{i,j} + \psi_0 MA_i \times Young_{i,j} \quad + \\ \psi_1 MA_i \times Intermediate_{i,j} + \mathbf{X_{i,j}} + \epsilon_{i,j}. \end{aligned} \tag{2.5}$$

where the omitted category is old women (aged 20-30 at the time of the border change). $\mathbf{X_{i,j}}$ is a set of controls that include race, husband's occupation and labor force participation. We expect both β_0 and β_1 to be negative in order to capture both the fertility transition and, also, some life-cycle effects. More importantly, we expect ψ_0 to be equal to zero and ψ_1 to be negative.

The summary statistics and the results from these regressions are presented in Table B.10 and B.11. We try a number of different specifications. In the first column we run a simple regression without controls. We add fixed effects at the enumeration district level to capture similar "neighborhood" patterns. In column (3) we include controls for husband's occupation, race, and labor force participation. Given the limited number of observations available, it is noticeable that the interaction term between intermediate age and living in the Massachusetts side is persistently negative and significant. The magnitude of the coefficient is roughly 0.3 children, a sizable difference. More interestingly, the difference in fertility between the sides of the city closes when we compare young women. Although they have fewer children than old women (about 2 children less), there is not a distinguishable pattern between North and South Fall River.

In the last column we run the regression considering only foreign women. These represent a large fraction of the sample, which is logical as migrants tend to be relatively over-represented in the sample of young married couples. The effect for migrant women seems to be stronger than for the pooled sample. Again, the effect is concentrated among migrant women that were between 11 and 20 years of age at the moment of the treatment, but only for those that were on the Rhode Island side. Interestingly, there is no effect of living in the southern part for older or younger women. The fact that the coefficient is much larger is easily explained by the differences in the number of children. The effect on foreign women is simply larger because they had higher fertilities to begin with (cf. Fernández and Fogli (2009)).

One may wonder how much of the effect comes from the legislation. In other words, if we are referring to foreign women, what is the likelihood that we will be considering fertility behavior from women who were not even living in the U.S. at the time of treatment. This question is quite difficult to assess with 1880 census data, as the year of migration was not asked. However, we can look up in the distributions of age at migrations from the 1900 census when immigrants were asked about the exact year migration took place. A simple tabulation of ages at migration for females in 1900 reveals that 26%, 60% and 85% of them immigrated at ages lower than 10, 20 and 30 years respectively. Assuming that the 1900 pattern is similar to that of 1880, there is reasonable evidence that a substantial fraction of the foreign women were exposed to the treatment.[23]

Yet the data allows for some basic checks as we can observe the place of birth of children living within the household. Of course, it is necessary to make assumptions about the residence of the mother between births, which we make in the form of two specifications. In the first, we exclude the observations of women who had their first births abroad. In the second, we tried considering the observations of women who were not living abroad in 1861. The results for these specifications is presented in columns 5 and 6 from Table B.11.[24] It seems that there is reasonable evidence to argue that part of the decline in the fertility of foreign women was due to the fact that they received compulsory education in the United States, especially considering that the fertility decline only occurs for those foreign women that live on the northern side of Fall River.

2.6. Conclusion

Every country that currently has a high per capita income experienced a marked decline in fertility at some point in its history.[25] As a result of the fertility transition, growth no longer translated into larger populations, and income per head grew. Yet for all its

[23]For an analysis of the persistence of cultural factors on fertility decisions see Fernández and Fogli (2009).

[24]In order to construct the residence variables, one needs to make assumptions about the residence in between two births. We assumed that the mother lived in the location of the first birth until the second birth occurred. In doing so, our conservative approach potentially excludes mothers that were subject to the treatment.

[25]In fact, there were only three countries with per capita income above 15,000 US$ and more than three births per mother: the oil producing countries of Equatorial Guinea, Oman and Saudi Arabia. See World Bank, World Development Indicators, 2008.

importance in models of unified growth, the causes of the rapid fertility decline in most Western countries after 1830 remain unclear. While the Princeton Fertility Project argued that cultural and linguistic factors are key, there are important challenges to the aggregative method it employed (see Brown and Guinnane (2003)). The quantity-quality trade-off argument prominent in the literature since the contributions of Becker and Lewis (1973) and Becker and Barro (1988) is of doubtful value given the history of changes in wage premia (Galor and Moav (2006); Mokyr and Voth (2010)).

This chapter provides empirical evidence that demonstrates intervention in the form of compulsory schooling laws was an important factor driving the fertility decline. We use microdata from the U.S. census to identify the effects of compulsory schooling. In the same fashion as Card and Krueger (1994), we use the state borders as our source of identification. In the previous chapter, this methodology was used to establish that compulsory schooling laws mattered and that they translated into higher contemporaneous and long-term human capital accumulation.

Based on a similar geographic identification strategy, this chapter finds that compulsory schooling laws effectively decreased fertility by about 15%. The results differ slightly depending on the exact definition of fertility adopted but are in every case significant, with the exception of some of the stock measures in the contemporaneous analysis. These results are also robust to the inclusion of a number of individual, household, and geographical controls. Furthermore, the data provides no support for the hypothesis that the CSL and no-CSL states exhibited different trends. We then turn to consider the long run effects of CSL. In particular, we explore whether it is true that women exposed to CSL during childhood grew up to have fewer children as a consequence. Here, again, our answer is yes. By looking at the fertility of cohorts just above and below the age to be treated, we show that women in CSL states had smaller families. The long run effects of the CSL seem a bit lower, about 0.1 children less or a 5% decline in fertility. While comparing these results with the direct effects is difficult, there seems to be some evidence that the effects of enforcing a CSL should be seen sooner rather than later and that the main channel operates through the decline in contemporaneous child labor. Furthermore, we further test the "exogeneity" of a border by examining the fertilities of women in a city that unexpectedly changed legislation as a consequence of a change of border. Here again, women living on the Massachusetts side of the border were expected to have about 0.3 fewer children as a consequence of the legislation.[26]

One problem that could arise in the analysis is that legislation is not entirely exogenous. In order to consider this possibility, we turn our attention to an exogenous political change that altered the legislation for people living in Fall River, Rhode Island. In 1861, the border change between Massachusetts and Rhode Island meant *ipso facto* the introduction of a CSL in the southern part of Fall River. We show that women who

[26] This analysis is difficult as it involves comparing different periods and a specific geographical region along the Massachusetts border.

lived on the Rhode Island side of the border at the moment the legislation changed had higher fertilities than did similar women on the Massachusetts side. Nevertheless, women who were too old or too young for CSL status to differ between the two sides of the border when they were at the treatment age had comparable fertilities. All of these findings support our original hypothesis.

This chapter contributes to the economic development literature that focuses on fertility transition and growth. A number of recent theoretical contributions study the potential of compulsory schooling and child labor laws to hasten the transition to low fertility (Doepke (2004), Hazan and Berdugo (2002), *inter alia*). This chapter is the first to empirically quantify the short-term effects of a compulsory schooling law on fertility.

The results found here only provide a lower bound on the overall effect of social legislation. The analysis of a sample of women who received education as a consequence of the CSL suggests that the indirect channel–i.e. the education of little girls–was equally strong. Given that one of the American education system's historical "virtues" was its high level of gender equality (Goldin (1999)), one can conjecture that the fertility effect of CSLs was maximized in America.

Finally, much contemporary policy advice focuses on the importance of educating women and on the effect this has on the fertility of the daughters that were educated. Our findings suggest that compulsory schooling laws can help reduce the total number of children in another way 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 policies that focus on the education of young girls alone.

3. What saved the children? Child labor laws in the United States, 1899-1919

If there was one policy issue that could have brought Karl Marx and the Pope together, it was their dislike for child labor. When describing the moral degradation brought about by capitalism, Marx complained that "Previously, the workman sold his own labour-power, which he disposed of nominally as a free agent. Now he sells wife and child. He has become a slave-dealer."(Marx (1867)[Ch. 15, Section 3-A]). From a radically different point of view, Pope Leo XIII gave an emotional warning about the dangers of child labor by noting that "just as very rough weather destroys the buds of spring, so does too early an experience of life's hard toil blight the young promise of a child's faculties, and render any true education impossible." (Leo XIII (1891)). Their concerns were justified. At the time Marx and Pope Leo were writing, the Industrial Revolution was associated with precarious living conditions, low living standards and pervasive child labor.

In the United States, industrialization relied from the outset on child labor. At the turn of the twentieth century, children in the U.S. toiled on farms and in factories in large numbers, often from an early age. In 1900, more than 20% of all children aged 10-15 worked in some form, contributing some 6% to the nation's workforce. From that point, however, the situation changed drastically. Within a few decades, under the reform impetus of the "progressive era" (1900-1920), child labor was successfully eradicated. By 1930, only 5% of children worked, and most of these performed agricultural jobs.

Today, poor economies still experience high rates of child labor. Indeed, child labor today is almost exclusively a problem of economies with per capita incomes below $5,000 (see Figure C.1). Child labor is negligible in Germany, England and the United States, the countries that once motivated Marx's emotional outburst. What allowed these once child-labor-dependent economies to operate without participation by children? What was the role of legislation? What were the costs, and who paid them?

Considerable attention has been given to the reasons for the rapid decline of child labor in the developed world. Interpretations range from the triumph of enlightened reformers to a decline in the demand for child labor as a result of technological change (Moehling (1999), Parsons and Goldin (1989), Nardinelli (1980) *inter alia*). While some degree of controversy persists, the dominant view today assigns only a limited role to child labor laws in reducing work frequency for the young. Instead, such laws are viewed as passively reflecting the reality of the workplace.

In this chapter, I argue that there is strong indirect evidence that child labor laws did not merely adapt to existing production techniques but instead imposed real costs on employers. I use data on value added by industry and the state collected from the 1900, 1910 and 1920 U.S. Manufacturing Censuses. I define a new measure of child dependence at an industry level and show that industries that used child workers extensively in 1900 slowed markedly in states that passed child labor laws.

In addition, I use quantitative and anecdotal evidence to document the technological dependence on child labor in some industries. For the leading employer of children, the glass industry, I develop a case study in which I not only establish technological dependence but also quantify the effects of substituting out of child labor. Furthermore, in order to minimize abstraction from state idiosyncrasies, I illustrate the logic of my argument via the glass industry of the Ohio Valley counties. Otherwise very similar, these counties are divided between three states that had radically different child labor policies. I observe that the industrial growth rates were significantly lower on the side of the border that had a child labor law.

This study is the first to use manufacturing–instead of population–census data. While population censuses often asked questions about labor force participation, the extent to which this was done for children under 14 years of age is not clear. Industrial-level data allows for indirect assessments of both the effects of child labor legislation and its economic cost. To assess the former, I observe that it is *mainly* the industries that depended heavily on child labor within a state that were affected when legislation was enacted. In contrast, other industries suffered no discernible effects. This methodology further allows us to investigate the economic costs of child labor legislation in the United States. I find that these can be large–up to 2.5% in terms of yearly growth rates–but that they are heavily concentrated in a few industries that are highly dependent on child labor. The high concentration of the effect might be useful for understanding the political economy of child labor legislation.

My findings relate to an extensive literature that has examined the incidence of child labor from a historical perspective. There is agreement among historians that the Industrial Revolution implied increased child labor participation (Horrell and Humphries (1995)). While it is true that pre-industrial child labor might have already been quite high (Hindman (2002), Cunningham (1990)), it was the new nature of the child labor brought about by the Industrial Revolution that concerned social reformers. Unlike pre-industrial child labor, industrial child labor was associated with unhealthy environments, a lack of education and the exploitation of young children.

One view on why children were exploited argues that families were simply too poor to do without their children's income (Horrell and Humphries (1995)). This is in line with modern theories of child labor (Basu and Van (1998)). A more dismal view is that of Parsons and Goldin (1989), who emphasize the role of family bargaining. They argue that parents were typically non-altruistic toward children. These authors conclude that, as revealed by their migration and labor decisions, "working-class families apparently sold the schooling and potential future earnings of their offspring very cheaply" (p.655). However, the conclusion that children were exploited by selfish parents has recently been disputed by Bhaskar and Gupta (2006). Using the same data as Parsons and

Goldin (1989), these authors find, more in accordance with the modern development literature, that child labor decreases with income. In addition, they argue that the observed negative correlation between parental and child wages could be the result of that inability of parents to arbitrage across different locations rather than of the effect of a parental lack of altruism.

Historical evidence for the effectiveness of restrictive legislation in the context of the British industrial revolution is scant and disputed. Nardinelli (1980, 1990) views the end of child labor in Britain as resulting from a combination of technological change and rising real income. In his view, the Factory Acts of 1833 did not cause the process, although they might have accelerated it. This belief has been contested by Humphries (1999), who argues that the lack of reliable employment data before 1851 makes it difficult to attribute the demise of child labor to any specific cause. In particular, using an alternative source of information—i.e., working-class families' budgets–, Horrell and Humphries (1995) find that, for the families of factory workers, income and child labor were positively correlated. Furthermore, in a different chapter, they use the family budgets to caution against overemphasizing the role of income increases during the Industrial Revolution (Horrell and Humphries (1992)).[1]

In the U.S., one of the earliest attempts to test the effectiveness of CLLs was Sanderson (1974), who compared the labor market participation of children in states that had CLLs between 1880 and 1900 and those that did not. He concluded that "[i]n all the cases, the legislation appears to be significant in redistributing children away from covered occupations" (p. 298). In a difference-in-differences framework, Moehling (1999) examines the employment status of children one year above and below the age of 14—the age beyond which child labor laws no longer restricted employment. Comparing states with and without child labor laws, she finds no difference in the employment rates of 13-year-olds relative to 14-year-olds. She concludes that the dramatic decline in child labor "was not driven by the legislative success of the child labor movement." Finally, Margo and Finegan (1996) use precise age data from the 1900 U.S. Census to apply an identification strategy similar to that employed in the seminal paper of Angrist and Krueger (1991). Their results are mixed in the sense that enrollment increased only in states that had *both* child labor and compulsory schooling legislation.[2]

My findings also relate to the Unified Growth Theory, which aims at explaining the transition from a low-growth, high-fertility regime (the Malthusian regime) to a modern regime characterized by high growth and low fertility. In that literature, child labor and compulsory schooling laws can be seen as drivers of change in the relative prices of

[1] The question of whether the Industrial Revolution brought about higher living standards is perennial in the economic history literature. Some important references on the topic are Lindert and Williamson (1983), Mokyr (1988), Feinstein (1998) and Voth (1998).

[2] For a more recent period, 1915-1939, Lleras-Muney (2002) establishes not only that compulsory schooling laws were effective, but that they mostly benefited the least educated. More importantly for this study, she argues that the laws were exogenous.

quantity and quality. These changes may trigger the fertility transition and, thus, serve as a catalyst for development.[3] Alternatively, some have viewed child labor legislation as a consequence, rather than a cause, of economic change (Doepke and Zilibotti (2005), Galor and Moav (2006)).[4] The problem with this view is that it emphasizes the role of human capital in triggering the transition, which is at odds with the historical evidence of the Industrial Revolution (Mokyr and Voth (2010)).[5]

Finally, from an empirical point of view, it is crucial to understand whether child labor legislation has an independent effect or merely reflects the ongoing changes in the economic environment. Strikingly, the interpretation of the historical record appears to be at odds with the assumptions underlying recent empirical work in labor economics. Child labor laws and compulsory schooling laws have featured prominently as instruments in various widely recognized labor economic papers (Angrist and Krueger (1991), Acemoglu and Angrist (1999), Manacorda (2006)).[6] However, to be used as instruments, child labor laws must have a strong effect on labor market outcomes. In what follows, I explore if this is the case.

3.1. Child labor in America

a. The first child labor problem and early laws

It is impossible to start a discussion about child labor without noticing that throughout time societies have always used child labor, at least to some extent. The United States is no exception. Child labor during the colonial and early republican period was pervasive. Records of children working are a part of early American history and, in fact, it was child "idleness" rather than child labor what worried the policy-makers of the time (Hindman (2002, p.13 ff.), Trattner (1970, p.25)). Contributing to the agricultural effort of the family or to other tasks within the farm was typically regarded as beneficial and even

[3]For instance, Doepke (2004) conducts a calibration exercise and concludes that child labor regulations were a prime determinant of fertility because they raised the relative cost of having children.

[4]For the purposes of the economic analysis, child labor and compulsory schooling laws are seen as reducing child employment and increasing school attendance. I will often use these words interchangeably.

[5]Doepke and Zilibotti (2005) assume that skill-biased technological change causes a quantity-quality tradeoff. Because children and adults compete in the labor market, as families choose lower fertilities and the number of children is reduced, the cost of a CLL—i.e., the loss of child income– is outweighed by the wage gain of the adults. The constituency for a CLL then grows, and the law is endogenously passed. Galor and Moav (2006) argue that the CSL of England came as a result of the interest of capitalists through complementarities between physical and human capital. Both of these theories predict rising skill premia on the eve of CLLs, which is at odds with the historical evidence.

[6] Angrist and Krueger (1991) use government intervention to assess the effects of education on income, while Acemoglu and Angrist (1999) focus on social returns to education. Manacorda (2006) has sought to identify the labor supply of parents in response to an exogenous change in child labor force participation.

educational. It was considered "work that educates." With the industrial revolution, the nature of child labor changed and so did its perception by society. Parents moved to cities in order to undertake their industrial jobs and eventually brought their children with them to the factories (Parsons and Goldin (1989)). Soon enough, the sweatshops of new (and "old") England were stuffed with children and the child labor problem, as we think of it today, arose.

Early child labor regulation in the United States originated, quite naturally, in New England. During the 1830s and 1840s, the New England states, concerned with the high levels of illiteracy among child workers, implemented a series of educational requirements in order to allow children to work. Thus, the first approach to child labor regulation involved compulsory schooling. It was only later that direct child labor legislation would be passed.

A first national wave of child labor regulation occurred during the ephemeral heyday of the knights of labor (1885-1889). Probably the most outstanding victory of this labor organization was the passage of a child labor law in Alabama in 1887, which then became the first southern state to pass such a law. This victory was also short-lived; in 1895, the Alabama CLL was repealed.

b. The National Child Labor Committee

Despite these early attempts to limit the extent of child labor, the 1900 census statistics revealed a sad reality. According to the enumeration, 1.75 million children were employed in some gainful occupation throughout the country. This meant that children constituted about 6% of the nation's labor force. Furthermore, one in every four had a job. It was not only that the level was high, but both the absolute number and the percentage of working children had been growing since the 1880s.

Given the overwhelming extent of child labor in 1900, it was only a matter of time until the child labor issue became the top priority in the "progressive era" agenda. In 1901, Rev. Edgar Gardner Murphy founded the Child Labor Committee of Alabama, starting what has suggestively been referred to as a "crusade for children" (Trattner (1970)). Three years later in 1904 the National Child Labor Committee (NCLC) was organized. From the very beginning the NCLC carried out systematic investigations in order to find out the extent and characteristics of child labor in the different industries and states. At the same time, they studied the existing laws and statutes in order to develop a model and "uniform" child labor law. Needless to say, none of the existing statutes achieved the standards of regulation and enforcement the NCLC considered minimum. It was then that the NCLC activities turned into a fight for more and better state legislation. Between 1900 and 1910, the states passed 207 bills that regulated child labor (Ogburn (1968)), almost as many as they had passed between 1840 and 1900. Child labor regulation was finally on the move.

c. The Federal Child Labor Laws

Although by 1910 the NCLC had managed to impose “some” child labor regulation in almost every state, their “uniformity” across America was still a pending issue. It was then when the NCLC became convinced that it would only be through federal legislation that minimum child labor standards could be achieved nationwide.

If regulation at the state level had not been easy, a Federal child labor statute would prove to be a utopian enterprise. The southern states, specially the Carolinas, fiercely resisted any federal intervention in their labor laws. In 1906, the Beveridge Child Labor Bill died in utero when it did not receive support from the NCLC. In 1916 a similar bill—the Keating-Owen bill– was passed in the Congress and signed by president Wilson on September 1st., 1916. It came into force exactly one year later and implied the interstate-commerce ban of goods produced using child labor. On June 3rd., 1918, only 10 months after its implementation, the Supreme Court found it unconstitutional (*Hammer vs. Dagenhart*). The decision was made on the grounds that Congress cannot regulate labor relations using the constitutional clause that allows it to regulate interstate commerce. A new child labor law invoking the taxing powers of Congress was passed on February 24th., 1919 (to take effect in April, 1919). It imposed a 10% excise tax on goods made by industries that employed child labor. On May 15th., 1922, it faced the same fate as its predecessor when the Supreme Court found it too to be unconstitutional (*Bailey v. Drexel Furniture Co.*).

After the second unconstitutionality ruling, the NCLC realized that the Supreme Court would systematically prevent Congress from legislating on labor issues. It seemed that the quest for federal child labor legislation had met a new dead end. Unless Congress were to be granted the right to regulate child labor, any attempt to regulate it indirectly would probably be met with the opposition of the Supreme Court. It was then that the NCLC focused on the passage of a child labor amendment.

The slow amendment ratification process began in 1924, and only five states had approved it by 1927. With the onset of the depression, times had changed; many states realized that they could release pressure from their labor markets by sending children back to school. As a result, the child labor amendment was swiftly passed by twenty-three other states between 1931 and 1937. The process was halted with the adoption of the NRA codes first, and once these were ruled unconstitutional, by the Fair Labor Standards Act (1937). These laws contained the essential principles for which the NCLC had been fighting since its foundation. When the Fair Labor Standards Act was upheld by a Supreme Court decision (*United States v. Darby Lumber Co.*, 1941), a federal child labor law restricting the employment of children under 14 was finally in force nationwide. The NCLC had finally succeeded.[7]

[7] Since the Fair Labor Standards Act contained all the characteristics of a CLL, the process of the CLL amendment was never resumed. To this day, the CLL amendment is still pending.

The main lesson we can obtain from the history of American child labor legislation is that passing child labor laws often turned into a complicated and lengthy process. If child labor laws had truly been endogenous, one would have expected laws to be passed swiftly and with little fanfare. However, historical evidence reveals that the fight over child labor regulation was, more often than not, quite fierce.

3.2. Case Study: The Glass Industry

The glass industry employed children more extensively than any other in the United States. In what follows, I discuss the technological reasons that made this industry particularly dependent on children. Furthermore, I evaluate what would have been the consequence of substituting adults for children. Could the glass industry have survived? How important were children for production? I first look at these questions at an aggregate level, and in the last part of this section I take a close look at an area where glass production was crucial: the Ohio valley.

The glass industry in America is almost as old as America itself.[8] The first American glasshouse was established by the English settlers of Jamestown (VA) in 1609. Thereafter, the history of glass-making would be one of constant expansion. For most of the colonial times, glass was produced in workshops located almost exclusively in Massachusetts, New Jersey and eastern Pennsylvania. With time, however, the industry experienced a series of waves of westward migration. The first one took glass-making west of the Appalachian mountains to the rich coal fields of western Pennsylvania and West Virginia. Later expansions led the way to the gas-rich states west of the Ohio river. At the turn of the twentieth century, a "glass belt," encompassing the area between Illinois and New Jersey, produced virtually all the glass manufactured in the United States. Within this glass belt, production was concentrated in a small area around Pittsburgh. Indeed, a few neighboring counties, both in West Virginia and Pennsylvania, were responsible for about 40% of the total American glass production (Lamoreaux and Sokoloff (2000)).

From its foundation in 1904, the NCLC paid special attention to the use of child labor in the glass industry. The reasons were clear: the glass industry was, together with the textile industry, one of the most important employers of children. In fact, the total number of children employed in the industry peaked in 1900 with over 7,000

[8] There are a number of very good papers about the American glass industry. Although the main goal of Lamoreaux and Sokoloff (2000) is to study the relationship between production and invention, they use the glass industry as a case study. The historical sketch draws extensively on this paper. Other sources include the senatorial commission report on the same (U.S. Senate (1911)) and the 1900, 1910 and 1920 census reports on the glass industry. A good summary stressing the role of child labor in the industry is contained in Hindman (2002). Finally, Scofield (1944) and its sequel paper study the early history of the industry until 1880.

child workers, which represented about 13% of the total workforce engaged in glass manufacturing. However, it was not only the widespread use of children that concerned the NCLC but also the extreme conditions under which they worked (Trattner (1970, p.77-78)). In the glass industry children were exposed to the high temperatures of the environment, working long hours both day and night.

Given the extent and severity of child labor in the glassworks, it was not long before the NCLC started campaigning for more stringent CLLs in glass states. Although the glass industry was often successful in softening child labor laws or making their enforcement impossible, a growing body of legislation made the employment of children difficult. Eventually, firms had to reorganize their production in order to accommodate for the new legislation.

In the typical bottle-producing firm, production was organized around a bottle blower who was aided by a team of unskilled laborers and, in particular, boys. Children's duties within the team consisted of a number of simple, mechanical tasks performed under the direction of a bottle-blower.[9] These included opening and closing the molds, eliminating the excess glass (blow over) and carrying output to the reheating or annealing ovens. The whole team would work the same hours, usually alternating day and night shifts. Production essentially depended on the pace imposed by the glass-blower. It is precisely because of this organization scheme that the glass industry was affected not only by the minimum age restrictions to child labor but, more importantly, by the maximum hours and no-night work provisions. Indeed, by 1910, most of the states already had some form of CLL. Glass manufacturers essentially fought in order to prevent maximum hours and night work provisions in particular.

The history of night work legislation for children clearly illustrates the degree of confrontation between the NCLC and the glass manufacturers. By 1910, only Illinois and Ohio had a ban on night employment for children. The rest of the glass-producing states had no such legislation. However, this was not the result of the Committee's lack of interest or effort. Indeed, five NCLC-sponsored night-work restrictions were turned down by the New Jersey legislature before such a legislation was finally approved in 1910. After similar struggles, Indiana, West Virginia and Pennsylvania finally banned night employment. However, this did not occur until 1911, 1919 and 1915 respectively (Trattner (1970, p.79)).

The night work ban was by no means the only dimension in which child labor legislation differed across states. Some glass-producing states like Illinois or Ohio had very advanced child labor legislation with strict age limits, night employment bans, and a limitation on weekly hours children could work. Furthermore, in these states, and particularly in Illinois, the laws were considered to be seriously enforced (Chute

[9] Among the different branches of the glass industry, the production of blown and pressed glass was the one that used children most extensively (see, U.S. Census Office (1902)[p.971]). In contrast, children were used in much lower proportion in the "building glass" production.

(1911)).[10] In fact, the 1903 Illinois CLL was regarded to be one of the best child labor laws not only in glass producing states, but in the whole country. On the other hand Pennsylvania and especially West Virginia had very backward legislations that barely gave legal protection to children Chute (1911, p.124). For instance, the West Virginia CLL of 1909 simply prohibited the employment of children under 12 or 14 years of age, when schools were in session. Not only this, but the law also failed to require any proof of age for children working in occupations other than mining. The CLL of West Virginia placed no restriction on the number of hours children could work nor did it forbid glass manufacturers from employing children at night. In essence, the 1909 child labor legislation of West Virginia can be considered little more than a statement of good intentions. The effective ban to child labor in West Virginia would have to wait until 1919.

a. Was the glass industry really dependent on child labor?

In principle, the heterogeneity of child labor legislation across states could be due to the fact that legislation had little impact on labor costs. After all, if children were not really needed, the progressiveness of the legislation would be immaterial to the glass industry. A first step in arguing that legislation had an effect on the glass industry consisted of establishing that the industry was quite dependent on child labor. To that effect I consider two sources. First, there is a substantial amount of anecdotal evidence consistent with the hypothesis that the glass industry was child-labor dependent. Second, the comparison of the glass industry across the world and in different time periods reveals a striking similarity in the amount of child labor they employed. Both are indicative of technological child-labor dependence.

Anecdotal Evidence

Glass manufacturers periodically expressed their concern about the possible enactment of child labor laws. A typical example of this behavior is the resolution initiative proposed by a member of the Glass Bottle and Vial Manufacturer's Association in a 1908 conference and cited in the *Report on Women and Child Wage Earners.*

> "Whereas the successful operation of a glass factory is dependent on a sufficient supply of minor labor; and
> Whereas glass manufacturers have spent much time and money in the different States to prevent the enactment of laws that will unnecessarily restrict the employment of minors;
> Therefore we request that the different glass workers' associations use their

[10]It is also probably related to the fact that a well-known child labor reformist like Florence Kelley had been the factory inspector of Illinois since 1893.

> influence [...] to create a sentiment on the subject that will be fair alike to all the interests involved." U.S. Senate (1911)[p. 145]

Aside from the purely rhetorical concern, there is also anecdotal evidence about the pernicious effects of the law on the industry. For example, after one year of the enactment of the new child labor law, the Bureau of Statistics of the state of New Jersey presented a special report on labor conditions on the glass industry. When referring to child labor and the changes introduced by the new legislation of 1904, they note the importance of children in this trade and the consequence of the legislation.

> "These boys, or at least the work done by them, is indispensable in the operation of glass factories; without their help blowers could not work and factories would be compelled to close up. Great difficulty is experienced in securing a sufficient supply of this kind of help, especially since the age limit for employment in factories has been raised to fourteen years. The unsuccessful efforts of manufacturers to meet this annoying situation by providing machinery for doing the work performed by these boys, has already been noted." Bureau of Statistics of Labor and Industries of New Jersey (1905)[p. 209]

In an another part of the same report, the statistics bureau inquires about the temporary or permanent suspensions; lack of children is explicitly recorded as the cause for suspension of some glassworks.

> "GLOUCESTER COUNTY. One of the glass factories of Moore Brothers at Clayton was idle for want of boy help. Fires were up and 25 blowers ready to begin the season's work, but were unable to do so for the reasons given above. Similar conditions prevail in other towns." Bureau of Statistics of Labor and Industries of New Jersey (1905)[p. 313]

In addition to publicly declaring that child labor laws negatively affected the industry, the actions of the manufacturers were also consistent with the supposed child labor dependence. Both the unconstitutionality ruling on the 1905 Pennsylvania CLL and the modification of the 1904 New Jersey statute were clearly motivated by the glass industry. In both cases, they successfully attacked the part of the child labor law that most affected them: the night work bans. The case of New Jersey is particularly surprising if one takes into account the amount of political support the new child labor law had gathered. The governor himself put a lot of effort into improving the enforcement of the existing law and passing a new one.[11]

[11] The Sun, April 23rd 1902, Fox (1905)

Finally, the comparative study of the evolution of the glass industry and the whole industrial sector of New Jersey is quite revealing. Upon the introduction of the new CLL of 1904, the value added of the glass industry plummeted. In a single year, the value added of the glass industry was reduced by almost 40%. The under-performance of the glass industry was substantial and persisted thereafter (See Figure C.2). The apparent growth of 1908 masks a major slump in the economy: in a rapidly shrinking industrial sector, the glass industry is simply shrinking more slowly. Interestingly, wages in the glass industry of New Jersey have a 10% premium with respect to the average industrial wage in the state. Moreover, this premium increased at the same time the industry was rapidly shrinking. This is consistent with the introduction of a child labor law: the reduction in labor supply should, *ceteris paribus*, increase wages. In other words, a negative labor supply shock can explain why wages increase at a time when the glass industry in New Jersey is declining rapidly.

At the same time, the evidence coming from reformers was mixed. On the one hand, in some publications they stated that it was difficult to replace children in the kind of jobs they performed, which required speed and agility adult workers did not have (Van der Vaart (1907, p.1)). In other publications, like the NCLC investigation on the glass industry (Chute (1911)), they noted that children were not more dexterous than adults, but simply faster. The senators that investigated the conditions of women and child wage earners (U.S. Senate (1911)) came to similar conclusions (p.201).

Child labor in the Glass Industry around the World

If there is some underlying technological reason for which children are used in the glass industry, the frequency of its employment should be similar for all the countries that do not have restrictions on it. Data from different countries and sources is presented in Table C.3. In the countries where there was not any restriction to the employment of children, the share of children to total workers achieved a striking 25%. This is true for countries as disparate as Argentina, the United States (1880), Sweden or Russia. France shows a slightly lower figure, but this is probably due to the aggregation employed. It may look surprising that the share of children in the glass industry in the U.S. declines sharply between 1880 and 1900. This change is a consequence of the "affidavit" legislation of the 1880s and 1890s. While there is common agreement among historians that these early CLLs were not enforced (Brandeis (1966)), they required children to provide an affidavit stating that they were of legal age to work.

Evidence coming from the factory inspector reports confirm that false affidavits were uncommon until the improvement of the CLLs in the early 1900s. In order to test this statement, I report an independent figure coming from the factory inspections in a glass district (Chief State Inspector of Workshops and Factories (1887)). It also confirms that about a quarter of the glass workers in an unregulated environment would be children. As I will show next, even using the implausibly smaller figures from the U.S. 1900 census, the economic impact of the elimination of child labor in the glass industry would be substantial.

Counterfactual: Replacing Unskilled Adults for Children

Regardless of whether children had actually a specific feature that made them more productive, it is a fact that children worked for lower wages. Under these circumstances, it is reasonable to argue that child labor legislation would have an effect on the industry's growth. However, it remains to be shown that the cost increase derived from the enforcement of child labor laws has a significant impact on industrial value added growth. This, of course, depends on the specific cost characteristics of the glass industry.

In panel C of Table C.4, I present a summary of the glass industry in the U.S. for 1900, when child labor was at its height. It is readily seen that leading among the expenses of the glass industry were labor costs (27 million), which accounted for over 50% of total costs. In fact, the glass industry ranked third among the U.S. industries in 1900 in terms of the proportion of expenses devoted to paying wage-earners (U.S. Census Office (1902)). In addition to the total labor costs, the 1900 census distinguishes between total wages paid to men, women and children. From this data, it is clear that child labor was substantially cheaper than adult-male labor. Children's earnings amounted to just 30% of the average for an adult male worker.[12]

A natural counterfactual in studying the potential effect of child labor legislation on the cost of the glass industry would be to consider the case in which children are replaced by adults. There are at least two problems with this. First, it is not clear that children and adults were equally productive, or worked the same number of hours. Second, the average wage of adult men includes the wages of a variety of skilled and unskilled occupations. Regarding the first issue, as I pointed out before, the senatorial commission concluded that children were about as productive as adults in their occupations. The fact that the industry worked in production teams also ensures that differences in working hours could not have been very large.

The second issue is more complicated. Skills and wages differed dramatically among adult male workers. It is only the wages of unskilled workers that are of interest to the counterfactual, as it is they who would replace children when CLLs become enforced. Therefore, it becomes necessary to find an estimate of the earnings in each of the different occupations of the glass industry. Using weekly-rate data from the U.S. Census Office (1903)[p.lxxxi], I construct a measure of earnings for the different skilled and unskilled occupations of the glass industry. The data are presented in Table C.4, panel B.

It is quite striking how the average wages obtained using the reported median hourly wages closely resemble the actual average wages paid by the industry in 1900. Only, the average imputed wage for children is slightly above the one that results from the

[12] For all the industries in the U.S., children earned about 31% of the adult-male wage.

census data (208 versus 188 dollars). The average wage for unskilled workers reaches 390 dollars per year, almost twice the amount earned by the average child. A set of counterfactuals based on the cost increase of replacing children with unskilled men is presented in panel C. Regardless of whether I use the imputed earning for both children and unskilled workers or only for unskilled workers, the effect of a child labor law is substantial. Assuming unskilled workers would be as productive as children, a perfectly enforced child labor law would increase the cost of the glass industry by 1.4 million dollars. The difference is slightly smaller if I construct the initial wage bill based on the weekly earnings reported by the U.S. Census Office (1903). In either case, the impact of a child labor law is in the order of 5% of total cost.

It turns out that the effect of a child labor law is equivalent to an increase of around 40% in the total fuel costs. The glass industry was notoriously sensitive to the price of fuel (Lamoreaux and Sokoloff (2000)). As a comparison, let us consider an extreme counterfactual: let us assume that all U.S. glass production is carried out using the same relationship between fuel costs and output value as that of Indiana, the state with the cheapest fuel in 1900. Under these circumstances, the aggregate cost of fuel for the glass industry would be reduced by 1.8 million dollars, or about 55% of the total fuel budget. Even under the most extreme fuel "cost pull," the impact on the costs of the glass industry would be comparable to those of a well-enforced child labor law.

b. The impact of child labor laws on growth

So far I have argued that child labor laws differed systematically across states and that they could have increased the industry's costs dramatically. It remains to be seen whether the growth of its glass industry is reduced when a state introduces a child labor law,.

A summary of the glass industry performance in the major glass belt states is presented in Table C.5. The five states considered represent over 70% of the total U.S. glass production throughout the period. Pennsylvania alone produces about 40% of the industry's value added initially, although its importance declines steadily from 1900. One very interesting case is that of Illinois. The glass industry in Illinois systematically outperformed the national average, except in the period after the model child labor law is introduced (1903). In fact, between 1904 and 1909, value added in the glass industry of Illinois declined over 25% while value added at a national level was essentially constant.

Contemporaneously, there is also a reduction in the number of glass firms established in the state, which suggests that industries were migrating to states with lower cost environments. The case of Ohio is quite similar. After outperforming the average growth of the industry in 1904-1909, Ohio's glass industry grew below the average in the following period. The cases of Pennsylvania and Indiana are difficult to interpret as growth rates systematically under-perform the national average. This is probably due to the fact that these two states originally had a significant share of the industry.

Although the aggregate level data seems to be consistent with the hypothesis that child labor laws had a detrimental effect on the glass industry, it does not allow us to rule out rival hypotheses. The problem is that states were heterogenous in a number of dimensions other than the legislation. It is just as possible that most backward states just happened to have, say, the cheaper fuel and the lowest child labor standards. In that case, the observed correlation between child labor laws and low growth rates would be entirely spurious.

A way to disentangle fuel costs from legislation is to restrict my attention to a geographically circumscribed area. The logic is that within a very short range of the state border the cost of materials is approximately equal. On the contrary, labor costs depend on state legislation and, therefore, vary discontinuously at the border. Consequently, if the difference in value added growth among the states is attributable to legislation differences, this should be noticeable at the state border. In order to consider this possibility, I examined the growth rates of manufacturing industries in several counties along the Ohio River Valley. This is a border area between a state with a good child labor legislation, i.e. Ohio, and states with very low child labor standards like Pennsylvania and West Virginia. Furthermore, important glass producing centers like Pittsburgh, Fairmont, Wheeling and Bellaire comprise the area. In fact, a substantial proportion of American glass production was done in these few counties. Since the glass industry was particularly dependent on child labor, the effect of legislation can be expected to be noticeable in these counties.

Table C.6 summarizes the evolution of manufacturing industries in the Ohio River Valley. Both in terms of employment and value added growth, manufacturing industries were generally performing much better on the West Virginia side of the river. Between 1900 and 1920, employment almost doubled and value added increased by two thirds in the West Virginia counties. In contrast, both employment and value added expanded only about 30% on the Ohio side. In addition to the clear discontinuity at the border, the data shows interesting and drastic regional patterns. Industrial employment and value added is growing faster in northern counties than in the southern ones. In fact, there is a particularly depressed area in the southern counties of Ohio and West Virginia. Despite these important intra-state differences, the fact remains that the economic performance of the Ohio counties is poorer than that of their immediate neighbors across the river. The county-level manufacturing data is consistent with the hypothesis that child labor legislation was detrimental for the glass industry. However, the regional differences in value added growth between counties of the same state suggest that economic conditions were not uniform in the whole area under consideration. In principle, it is still possible that the correlation between growth and poor legislation is driven by this heterogeneity.

In order to isolate the effects of child labor legislation, it is necessary to restrict my attention to an even more confined geographical area, where the regional economic conditions are more or less constant. There are two major cities along the border for which manufacturing censuses reported individual information: Wheeling in West Virginia and, a few miles south from it on the Ohio side of the river, Bellaire (See Figure C.3). In addition to being the largest cities on the border between Ohio and West Virginia, the industrial composition of both was quite similar. Coal mining, rolling mills and, in particular, glassworks were abundant on both sides of the river. Fuel was widely available and equally cheap in both Wheeling and Bellaire. Of particular interest to the glass industry, natural gas had become available in the area around 1886 and it was the same firm that provided the service on both sides of the river. Finally, ever since the opening of the Wheeling suspension river in 1849, the area had become increasingly integrated. In a word, it could be reasonably argued that the cost of fuel and inputs in Wheeling and Bellaire was the same. The main difference between these two cities is that they are subject to different legislation as they belonged to different states.

Table C.7 presents aggregate manufacturing data on Wheeling (WV) and Bellaire (OH) for the period 1899-1919. In both cities, employment and value added are growing at the beginning of the period. However, during the period 1904-1909, the evolution of employment starts to diverge between them. The number of wage earners in Bellaire stabilizes around 1914 and starts decreasing afterwards. By 1919, the number of wage earners in Bellaire was just about the same as in 1904. In the same period, employment had doubled in both Ohio and West Virginia, and even the neighboring city of Wheeling had managed to increase its manufacturing employment by over 20%.

The fact that manufacturing employment stagnated does not necessarily mean the decline of Bellaire's industry. In principle, the decline in manufacturing employment could be the result of a labor-saving technological change in the local industries. After all, several of the most important innovations of the glass industry had been invented around the time industrial employment began to collapse in Bellaire. However, this hypothesis does not seem plausible. On the one hand, mechanization was quite slow in the glass industry. Even though the major inventions occurred around 1903, about three-quarters of all firms still used hand methods as late as 1920 (Lamoreaux and Sokoloff (2000, p.715)).[13] On the other hand, starting in 1904, real value added plummeted in Bellaire. Both the decline in value added and manufacturing employment occured in a city quite dependent on child labor, and contemporaneously to the introduction of a new child labor law. Furthermore, the scant evidence available suggests that the legislation was effectively enforced in Bellaire. In particular, in the follow-up NCLC investigation on the glass industry (Chute (1911)), it is mentioned that school enrollment increased sub-

[13]One of the most important inventions in the glass industry was Owen's automatic bottle blowing machine. Its inventor, Michael J. Owens, had left school in order to work in a glass factory in Wheeling when he was only ten years old. Paradoxically, his invention contributed enormously to the eventual elimination of child labor in the glass industry.

stantially in Bellaire as a consequence of child labor legislation. In the meantime, child labor in Wheeling was as high as ever: according to the 1919 census of manufacturers, over 10% of its glass wage-earners were children, which was about the same percentage as for the whole industry in 1900.

All the evidence presented in these pages is consistent with the hypothesis that when child labor legislation is enforced, the growth of a child-labor-dependent industry is considerably reduced. This occurs because when such legislation is introduced, labor costs increase. Furthermore, as the example of the glass industry suggests, the increase in labor costs may be sizable for child-labor-dependent industries. The data shows that manufacturing output growth varies substantially between neighboring areas with comparable cost structures, but subject to different legislation. In addition to this, the divergence occurs in an area that was arguably quite dependent on child labor. Finally, the timing of this divergence is quite revealing: the counties affected by CLLs start under-performing exactly at the time when the legislation is introduced.

All of these independent pieces of evidence are revealing as they point in the same direction. That is, the glass industry seemed to have been technologically quite dependent on child labor. In addition, the stylized counterfactual exercise shows that the enforcement of a child labor law would have meant an increase in cost comparable to a fuel "cost push."

3.3. Methodology

a. Identification Strategy

In the spirit of the seminal work by Rajan and Zingales (1998), my empirical strategy exploits technological differences across industries in order to identify the effect of child labor laws on industrial growth. Although in the period before the passing of the child-labor ban, child labor was equally available to all industries, only some of them relied extensively on the employment of children due to their technological idiosyncracies. As the case study on the glass industry illustrates, children were not equally useful in all the jobs. Consequently, the likelihood that they would be employed varied according to industry-specific characteristics. Institutions such as the regime of apprenticeship, tradition or even prejudice could be responsible for the differential rates of child labor across industries.[14] For this reason I assume that the fact that children were employed extensively in industries such as "Glass-Making" or "Canning and Preserving" denotes, ultimately, a technological characteristic. It is a crucial assumption of this chapter that child labor dependence is technological.

[14] An interesting case of prejudice affecting child labor is that of the southern cotton mills. Although severe labor shortages periodically affected the industry, firms would not hire African Americans. Instead, they systematically preferred to hire children.

The Exogeneity Issue

My identification strategy also requires that child labor laws are mostly external to the industries located in a given state. In a word, I need the laws to be exogenous to the states. There are several reasons why I believe this is the case. First, because the laws were sponsored and even drafted by a philanthropic organization: the National Child Labor Committee (NCLC). Most of the political economy arguments on child labor anticipate that sponsors of the legislation would include interest groups like unions (Krueger and Donohue (2005)), unskilled parents (Doepke and Zilibotti (2005)) or even greedy industrialists that stood to benefit from a better educated labor force (Moav (2005)). Instead, the members of the NCLC came from a very heterogeneous range of backgrounds that included philanthropists, religious ministers, scholars, presidents and even bankers and businessmen (Trattner (1970, ch.2)).

Second, the whole campaign against child labor took place in a relatively short period of time. The NCLC was very active between 1904 and 1919, during which they achieved their goal of child labor legislation in all the U.S. states. This reduces not only the likelihood of deeper endogenous processes occurring, but also increases the reliability of my identification. Indeed, in a short period of time, it becomes easier to argue that the environment is stable in every aspect but in the legislation.

Third, if laws were endogenous I would expect states initially more dependent on child labor to have worse labor legislation. If anything, there seems to be no correlation between the extent of protection against child labor and the initial dependence. The difference in legislation seems to be explained mostly by geographical location; regardless of the amount of child labor dependence, neighbor states tend to have very similar child labor legislations.[15] A simple scatter plot between the state child labor dependence in 1900 and the degree of child labor protection in 1909 clearly illustrates the point: the correlation between the two variables is not significantly different from zero (See Figure C.4).[16]

Finally, the intense legislative and judicial struggle that followed the passing of the legislation is inconsistent with the notion of endogenous laws. Both state and federal laws were tried–often successfully–for constitutionality over and over again. Leading examples of these are the cases of the PA 1905 CLL or the Federal CLLs of 1914 and 1919. The path toward child labor regulation seems, at least until the onset of the Great Depression, to be full of struggles and setbacks.

Another important assumption is that the dependence on child labor is strong enough so that limiting the access to it would have an observable effect. While this is not true in general, the case study of the glass industry clearly shows that for some industries this is the case. For the glass industry in particular, I found that this effect was comparable

[15] This is consistent with the political innovation theories proposed by Walker (1969), Stokes Berry (1994) etc.

[16] See data appendix on the construction of the CLL index.

to the cost-of-fuel effect, which is recognized in the literature as a major location of "cost push" (Lamoreaux and Sokoloff (2000)).

All of these factors combine to produce the central variable in this chapter: $(Child\ Labor\ Dependence)_j \cdot (Child\ Labor\ Law\ Dummy)_s$.

Child Labor Dependence is an industry-level variable indicating the degree to which industry j used child labor when there were not any restrictions to child labor employment. The *Child Labor Law Dummy* indicates whether state s has a child labor law enacted at the period. This interaction represents the extent to which the combination of *Child Labor Dependence* and *Child Labor Law* depress growth.

The outcome variable I consider is the real value added growth, measured as the compound annualized rate of growth. The basic model I estimate in this chapter is then

$$Y_{j,s} = \alpha + \beta_0 X_{j,s} + \beta_1 U_i + \beta_2 V_s + \beta_3 CLL_s x CL\ Dependence_j + \varepsilon_{j,s} \tag{3.1}$$

where $Y_{j,s}$ is the chosen outcome variable. $X_{j,s}$ includes the variables other than the interaction that vary at both state and industry level, like industry size in the state, or the proportion of total value added produced by industry in the state. U_j contains the initial dependence on child labor to be defined later. Finally V_s includes a whole set of controls for state specific characteristics such as illiteracy rate, state investment, initial income per capita and a whole set of demographic controls. Among the state specific variables, I also include whether it has a child labor law or not.

b. Data

This chapter relates industry-state data from the manufacturing censuses with a database of labor laws and child labor laws in particular. The industry data comes from the U.S. manufacturing censuses carried out in 1900, 1910 and 1920. Comparable data are collected for 154 industries each operating on a median of nine states. The analysis was restricted to all forty-eight continental states plus the District of Columbia. Data on capital, output value, cost of materials, horse-power usage and employment of women and children, was collected for all the industries and states available. Compatibility issues between the different definitions of industries were addressed using the instructions and industry description for the manufacturing censuses.

c. Definition of the Control Variables

Child Labor Dependence One of the central variables in my analysis is the amount of "Child Labor Dependence." This variable is constructed from the 1900 census data. I define CL Dependence as the proportion of children with respect to the labor force for the median states in the 1900 distribution. The rationale for defining the variable in such a way is that in 1900 there were not any restrictions to the employment of children. Consequently, industries were employing the optimal amount of child laborers, which ranged from 0 to 13%. Consequently, the higher child dependence, the stronger the output effect of child labor laws. In the construction of the variable, I prefer the median to the mean because it is robust to outliers. For example, outliers could be the result of some states more seriously enforcing their pre-1900 labor regulation. However, as I have argued above, it is the standard view in American labor history (Brandeis (1966)) that labor regulation was merely a statement of good intentions before 1900.

Child Labor Laws The next important piece of data is the one constructed from child labor laws. Defining child labor laws is quite difficult. Laws were heterogenous and so was enforcement. Effective child labor legislation seems to have involved more than an explicit age-limit for working children. Complementary measures such as mandatory schooling laws and a proper factory inspection act were crucial.

Mandatory schooling is the "other side" of the child labor coin: if children are at school, then they are not working. In addition to this obvious effect, mandatory schooling complemented CLLs and increased their enforcement. This became so as school authorities were increasingly put in charge of issuing the working permits. Before this, the minimum-age requirements, when they existed, compelled children to have a parental sworn statement or *affidavit*. As long as parents were willing to commit perjury, underage children had no difficulty getting working papers. By transferring the competence to issue permits from parents to school authorities, mandatory schooling laws constituted a big improvement in CLL enforcement.

The other key factor regarding law enforcement was the passage of factory inspection laws. Clearly, a law is of little use unless an authority to enforce it is created. In this sense, factory inspection laws paved the way for CLLs to be effectively implemented.

In my main specification I consider "effective" child labor laws. That is, I define a state having a child labor law in 1909 only if it has a minimum age-limit, a compulsory schooling law and a factory inspection law. However, in the robustness checks, I explore the sensitivity of my results to alternative definitions of CLLs.

State-Level Controls

Finally, a set of socio-demographic state controls including race, nativity and literacy are also used in the analysis. These were obtained from ICPSR Study 2896 (Haines (2008b)) and refer to state averages. Additionally, as a control for personal income in the state, I use the estimates provided by Easterlin (1971). As a control for the degree of innovation in the state, I used the number of patents granted by a given state in the five years prior to the census. The data used to construct the patent variable comes from Johnson (2002), who collected it from the U.S. Patent Office.

d. Summary Statistics

The data presented in Table C.8 show that industries with high dependence seem to grow faster in states that do not have child labor laws. This intuition is further confirmed by looking at the median values of value added growth. Median growth in a low child dependent industry was quite similar between states that did, and did not, have a child labor law (20% vs. 19%). However, high child-labor dependent industries seem to be performing much better in states without the law (24% against 17%).

The average of all the socioeconomic controls for the group of high and low child labor dependence looks very similar. In fact, a t-test of the mean cannot reject the hypothesis of equality in any case. This can be interpreted as signaling that high and low child labor industries are not distributed in a particularly territorial way. For instance, if all the high child labor industries were located in the south, the proportion of workers who were? black, illiterate, etc., should be larger for the high child labor industries. Since the endogeneity of the law is often discussed on the grounds of prior socio-economic differences, it is important to note that neither in 1910 nor in 1900 (not reported) the mean of the socio-economic variables differ between high and low child dependence industries.

Finally, some variables do exhibit differences according to the level of child labor dependence. High dependence industries seem to be larger and to employ less capital. This is consistent with the idea of child labor being present in manufacturing industries rather than in smaller workshops, as it had been in the past. The image of child labor in this period is of children toiling in the cotton mills of South Carolina or the glass factories of Pittsburgh. The descriptive statistics of the data are consistent with this idea.[17]

3.4. Main Results

In this section I present the results of my estimation of equation (3.1) above. I try four alternative specifications including a number of state and industry controls. In the final specification I concentrate on the interaction term alone and allow for a full set of industry and state controls. The results are presented in Table C.9.

The coefficient of interest is the interaction between child labor dependence and CLLs. This interaction is similar to a second derivative: the differential effect of a child labor law in states that do and do not have a CLL in place.[18] The point estimates for the interaction of child dependence and child labor laws range between -0.2 and -0.3 and

[17] This could very well be due to an artifact of the data, since it is easier to imagine misreporting of children in smaller workshops than in large factories.

[18] It also has a difference-in-differences interpretation, where the dimensions of differentiation are child labor dependence and CLL status.

they are significant at conventional levels. Their effect on growth rates depend on the percentage of children initially employed. For the average firm, employing about 2% of children, this effect could imply a decline in growth of the order of 0.4 to 0.6 percentage points per year.

The other variables I include in the first regression have interesting and plausible signs. Among the industry controls, the proportion of black population and, marginally, urban population are significant. These both represent the accelerated convergence of the southern states to northern growth rates. During the 1910s, it was the less urban South that was starting to catch up with the North. Indeed, this also explains why the state income variable is insignificant. In a way, the proportion of urban and black populations and state income are all proxying for the initial level of wealth.

Finally, two controls similar to those included by Rajan and Zingales (1998) have the expected signs and significance. On the one hand, the larger the industry relative to the state, the lower the growth rate, as in a standard convergence argument. On the other hand, higher state investment in the previous decade is correlated with higher growth today.

The following specification introduces first state, and later industry fixed effects. Since some of the controls only vary at the state or industry level, when including a full set of controls I have to exclude them from the regression. The baseline specification replaces all the industry and state controls for a full set of dummies. As a consequence of this, some of the remaining variables become insignificant. Especially noticeable is the case of initial capital, which had a positive effect until industry fixed effects are included.

In general econometric terms, specifications (1)-(4) look good. The interaction coefficient is surprisingly stable across specifications. This is interesting as the number of controls used in each of the regressions varies dramatically. In the baseline case, a full set of 153 industry dummies and 48 state dummies are included. This contrasts with the mere 13 controls included in specification (1). In spite of these differences, the effect on the coefficient is mild, not affecting its statistical significance. The explanatory power of the regression (R^2) rises significantly after the introduction of a full set of industry dummies. This is a standard result and simply reflects the fact that most of the variation in annual growth rates of industries have to do with industry-specific characteristics.[19] More importantly for my argument, the negative and significant effect subsist even when I control for industry specific growth.

[19] Interestingly, in Rajan and Zingales (1998) the significant rise in R^2 occurred after the inclusion of *country dummies* while *industry dummies* did not do much to increase explanatory power. This is probably explained by two key differences between the databases I use. First, their data refer to a set of different countries, both developed and developing. Hence, the degree of heterogeneity among the countries is certainly higher than the heterogeneity among U.S. states. Second, the definition of industry I use is based on narrowly defined categories whereas Rajan and Zingales (1998) work with more aggregated data. Consequently, inter-industry differences in my data are less attenuated by aggregation.

3.5. Robustness

So far I have established that passing a CSL affects the growth rate of industries that actually depended initially on child labor. In this section I perform a number of checks in order to test the sensitivity of the results to the definition of the data and the techniques used. In particular, for this chapter I defined the concept of child labor dependence and child labor law. A first test involves using alternative definitions for these two variables. Also, it is important to test the robustness of my results to the estimation technique, given that the distribution of child labor dependence is very skewed.

a. Alternative definitions of child labor dependence

In defining a CL dependence variable, it is crucial to find a benchmark economy where industries are employing children without any restrictions. Above, I argued that due to the lack of enforcement of early child labor laws, taking 1900 as a baseline year is quite reasonable. In addition, I focused on the median proportion (across states) of children employed in a given industry. This ensured that the child labor measure was robust even in the event of some isolated state enforcing a child labor law in 1900. In this section, I extend my results focusing on previous censuses. In particular, I consider the proportion of children employed in each industry in 1880 and 1890. In addition to this, I not only report the traditional average regression (OLS) but a median regression, which is less sensitive to outliers. The results are presented in Table C.11.

The results of the regression employing 1890 data are strikingly similar to the baseline estimation. The point estimations are around 0.15 and 0.2, less than one standard deviation away from the baseline coefficients. Furthermore, the fit of the regression is quite good and very similar to that of the baseline model using 1900 data. About 45% of the variance is accounted for in the final specification. However, as I have noted before, most of it comes as the result of the inclusion of industry and state dummies.

Finally, the regression for 1880 yields a lower effect for the interaction term. Yet, the effect is significant (at least for the median regression) and similar in magnitude to others. Again, the distance between the baseline coefficient and the 1880 coefficient is less than 1.5 standard deviations, which again suggests that they are plausibly close to each other.

The reasons for the dimmer effect of 1880 could also be historical. First, the time between 1880 and 1900 was one of deep transformation for the American economy, which was experiencing the zenith of the second industrial revolution. Consequently, the structural changes that occurred over the period could make comparability with later periods difficult.

b. Alternative definitions of child labor laws

An obvious additional robustness check regards the definition of child labor laws. In the benchmark specification, I used the combination of a minimum age requirement with a factory inspection law and a compulsory schooling law. Clearly, defining the laws in a different way leads to different estimations of the effect. The goal in this section is to prove that the effects all point in the same direction. In particular, I want to prove that it is not the case that the negative coefficient of the basic specification comes from the spurious combinations of the three laws. In order to do so, I try three alternative definitions for the child labor law. First, I define a dummy variable that takes value 1 if the state had a minimum age provision in 1909, and zero otherwise. Second, I consider the age provision together with whether the state had documentary proof of age as a condition to grant the work permit. Finally, I consider the effect of using the compulsory schooling law alone.[20]

The results from Table C.10 are consistent with my previous findings. Most importantly, the variations of the coefficient are not further than one standard deviation below the benchmark case. The coefficients range between the plausible -0.13 and -0.19 and are, except in the first case, significant at standard levels. Interestingly, in the case of minimum age provisions alone, the effect of the child labor law is the weakest. This is consistent with evidence coming from other sources (Moehling (1999),Margo and Finegan (1996)). These last authors found that school attendance increased as a result of the joint application of CLL and CSL but they fail to find individual effects for either legislation.

c. Other robustness checks

Neither the definition of the dependence measures nor the child labor laws seem to be crucial for obtaining the result. What about the particular way in which the sample was chosen? After all, almost 40% of the industries in the sample did not use children at all. In the first specification in Table C.12, I run the regression again using only the industries which used at least some children in 1900. The results are surprisingly similar to the baseline estimation, the point estimate being even slightly more negative and significant.

Another check I perform is related to the relative size of an industry in a state. It is reasonable to assume that the larger the proportion of the state manufacturing an industry is responsible for, the more intense the lobbying against child labor. Consequently, including the big industries of a state increases the likelihood that lobbying

[20]Other specifications not reported were also tried. Most notably, I tried defining CLL as the continuous CLL index (based on Ogburn (1968)). In this case the point estimate is also negative and, for the sub-sample of industries that employed children initially, highly significant.

could have played a role in the passing of the CLL. That is, big industries let the law pass only when it has no economic effects for them. It is important to note that even if large industries were lobbying against CLL, small industries cannot act strategically with respect to the law. If they happen to live in a state where there is an important child labor employer, they simply benefit from the law not being passed. Small industries are essentially passive to this. Consequently, the potential issue of law endogeneity could be tackled by simply excluding big industries.

I do this in specification (2) of Table C.12 where I exclude from the sample all the industries representing more than 1% of the total manufacturing output of the state. I find that the absolute value of the interaction coefficient (-0.3) is actually *big* compared to the baseline model. Although it may seem puzzling at the beginning, it makes sense. If the big firms allowed a law to pass in the beginning it is because they anticipated a small impact. The inclusion of these observations biases the estimates downwards. Consequently, when I exclude them from the calculations, the anticipated impact increases.

In specifications (3) and (4) of Table C.12 I address concerns about influential observations driving the results. In order to do so, I report the median and robust regression estimates. Since median regression minimizes the least absolute deviation (LAD) instead of the square errors, it is less affected by the presence of aberrant observations. In the same spirit, robust regression techniques are based on iterative re-weighting of OLS residuals. The procedure continues until a stable set of weights for the sample is found. Both of these methods are supposed to provide us with a more stable estimate for the coefficients.[21]

In the results I present in Table C.12, both methods yield a significant and negative coefficient for the interaction. Although smaller than the baseline estimation (0.23), the coefficient for the interaction between CLL and dependence is still reasonably close to it (less than 1 standard deviation).

3.6. How big are the effects of the legislation?

In the previous sections I focused first on the sign and size of the effect, and later on the sensitivity of the results to the definitions and econometric specifications. Let us now turn to the economic meaning of the coefficients. Is the magnitude of the coefficients found earlier consistent with what we would expect? In other words, are the coefficients "economically" significant?

In order to consider the question, I compute the growth impact of a child labor law for alternative assumptions on the coefficients. In this exercise, the coefficient can take

[21] For a good introduction to median and, in general, quantile regression refer to Koenker and Hallock (2001). An intuitive explanation of robust regression is found in Hamilton (1992).

a low value (-0.1), a medium value (-0.15) and a high value (-0.2), which collectively represent more or less the most common values I found in the regressions throughout the chapter.

I present the results in Table C.13. As a point of comparison I also report the average growth rates for industries in different percentiles of the child-labor dependence variable. The impact of the legislation is over 1% of annual growth rates only for industries above the 90th percentile. These industries account for over 25% of the total growth rate, depending on the industry and the reference period chosen. Although these effects seem high, they are plausible and consistent with the systematic concern these industries showed with respect to the banning of child labor. The extreme case is the glass industry, with a growth impact ranging between 1.2 to 2.5% per year. Admittedly, this last figure seems a bit high, but it must be kept in mind that the glass industry was by far the biggest industrial employer of children. In addition, the glass industry repeatedly insisted on the pernicious consequences of child labor laws.

More representative of a typical industry in terms of child labor employment is the "Brass and Bronze Products" industry. This industry, exactly the median industry in terms of child labor dependence, could expect to suffer a slowdown of 0.14% per year as a consequence of the child labor regulation; an almost imperceptible effect.

The economic effects I found in the data are consistent with the historical evidence. Only a few industries very dependent on child labor have high predicted effects from child labor laws. For the rest of the industries, CLLs had mild effects.

3.7. Conclusion

At the turn of the nineteenth century, in a typical American glassworks children would have accounted for one quarter of the labor force. Within a few decades, the enactment of child labor, compulsory schooling and factory inspection laws significantly reduced the employment of young children while generally improving working conditions. Much of this occurred during a brief period, the "progressive era," and was fueled by the passion of reformers such as Florence Kelley, Edgar Gardner and Lewis Hine.

This chapter investigated the consequences of "progressive era" child labor legislation for the manufacturing sector of the United States. I find that CLLs have a deleterious and potentially large effect on industrial growth. The size of this effect ranged from negligible to over 2% per year for industries employing children extensively. These results are both plausible and consistent with the behavior of industries: rather than enjoying the ride after the CLLs were passed, as modern political-economy models would suggest, industries fought back fiercely.

I also found that the economic effects of CLLs were unevenly distributed. While the median industry would have seen only a minor effect of the legislation, CLLs would have taken about 2.5 growth points in the glass industry, the most child-labor dependent industry. The fact that the effects were substantial, but concentrated in a few industries, could be used to explain why modern developing economies have difficulties eradicating child labor.

Besides shedding light on the issue of child labor and the effect of legislation, this chapter contributes to the literature in at least two ways. First, the data sources I use to study the question of child labor are different from those used in previous studies. In contrast to Moehling (1999) or Margo and Finegan (1996), who use population census data on labor force participation, I focus on manufacturing census data. The surprisingly large amount of U.S. manufacturing data is a very rich but unexploited source of information for economic research.

Based on the very well-known work of Rajan and Zingales (1998), I developed a methodology that exploits the technological differences in child labor dependence between industries. Using this source of variation, I circumvent reverse causality issues at a state level, because it is possible for us to compare industries that received the shock and those that did not *within* the same state. In a word, this methodology allows us to conclude that it is not the growth rate that explains the law, but rather the law that explains the growth rate.

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A. Appendix to Chapter 1

Figure A.1: Compulsory Schooling Laws

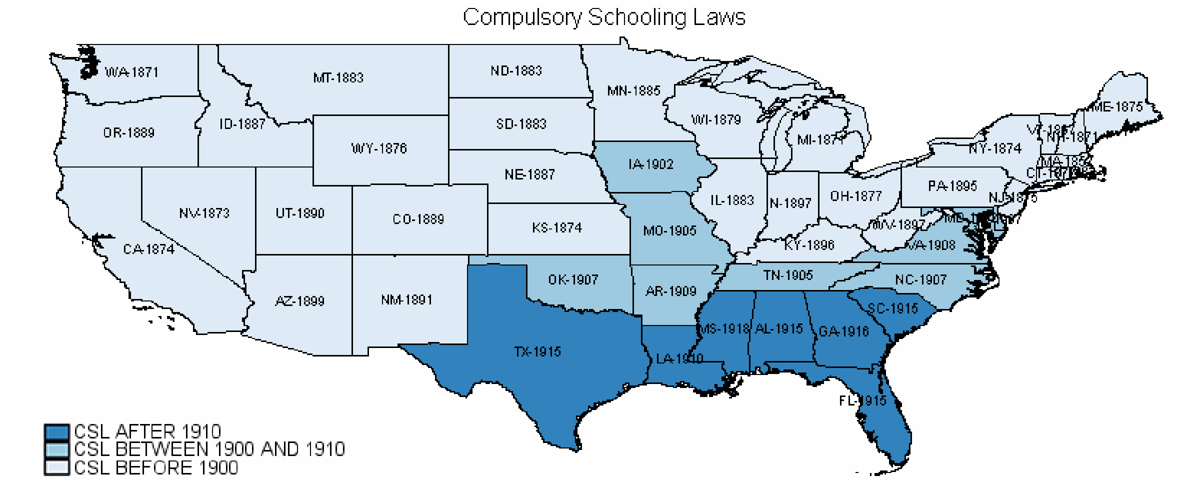


Figure A.2: Evolution of the CSL Border - 1860-1870 - By Township

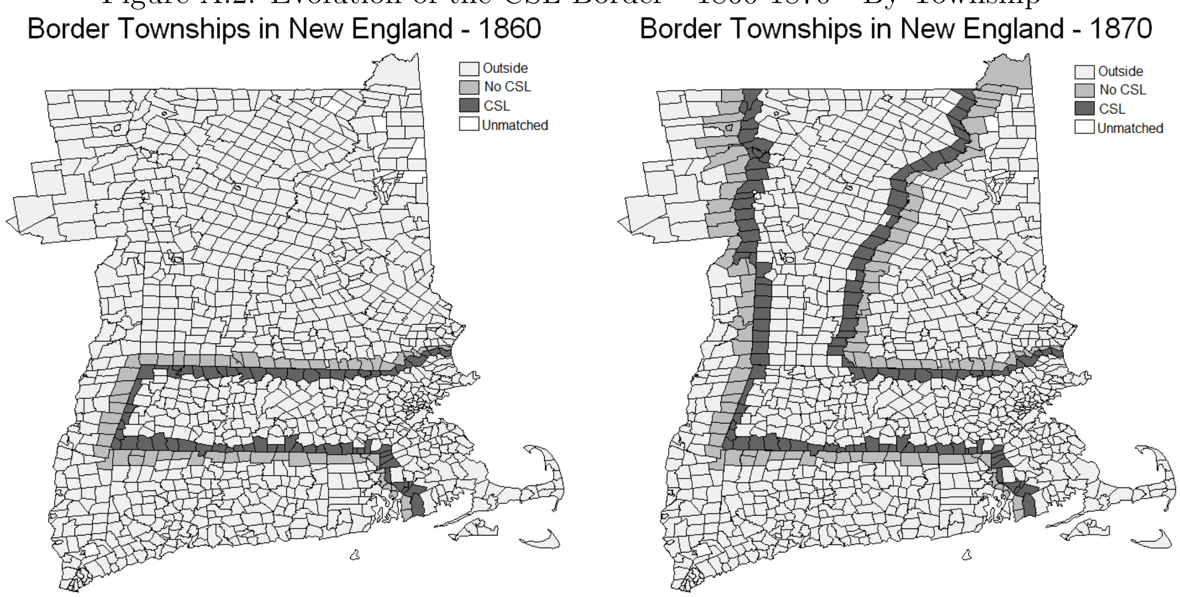


Figure A.3: Evolution of the CSL Border - 1860-1910 - By County

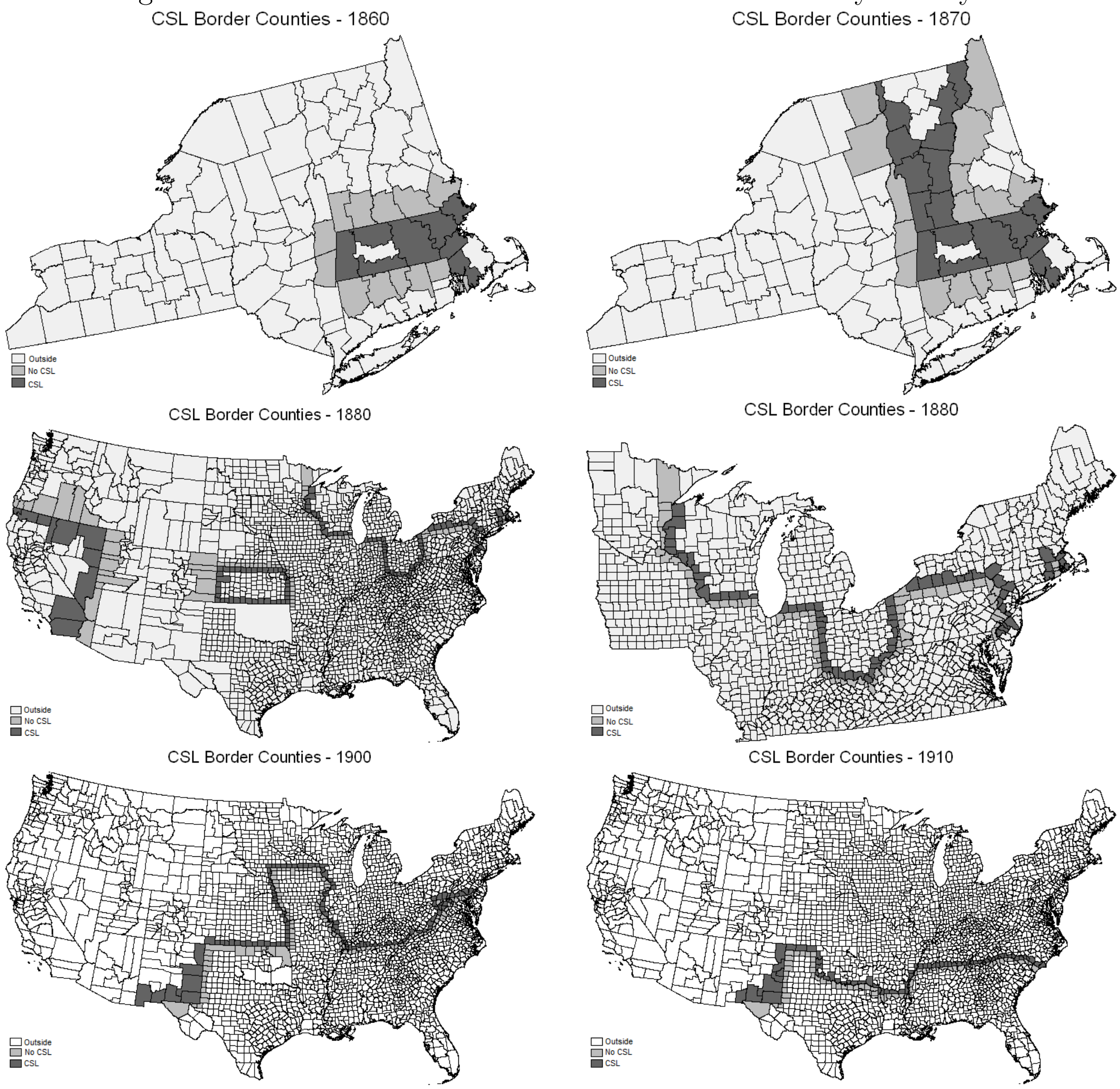


Figure A.4: Change in School Attendance by Age: by CSL

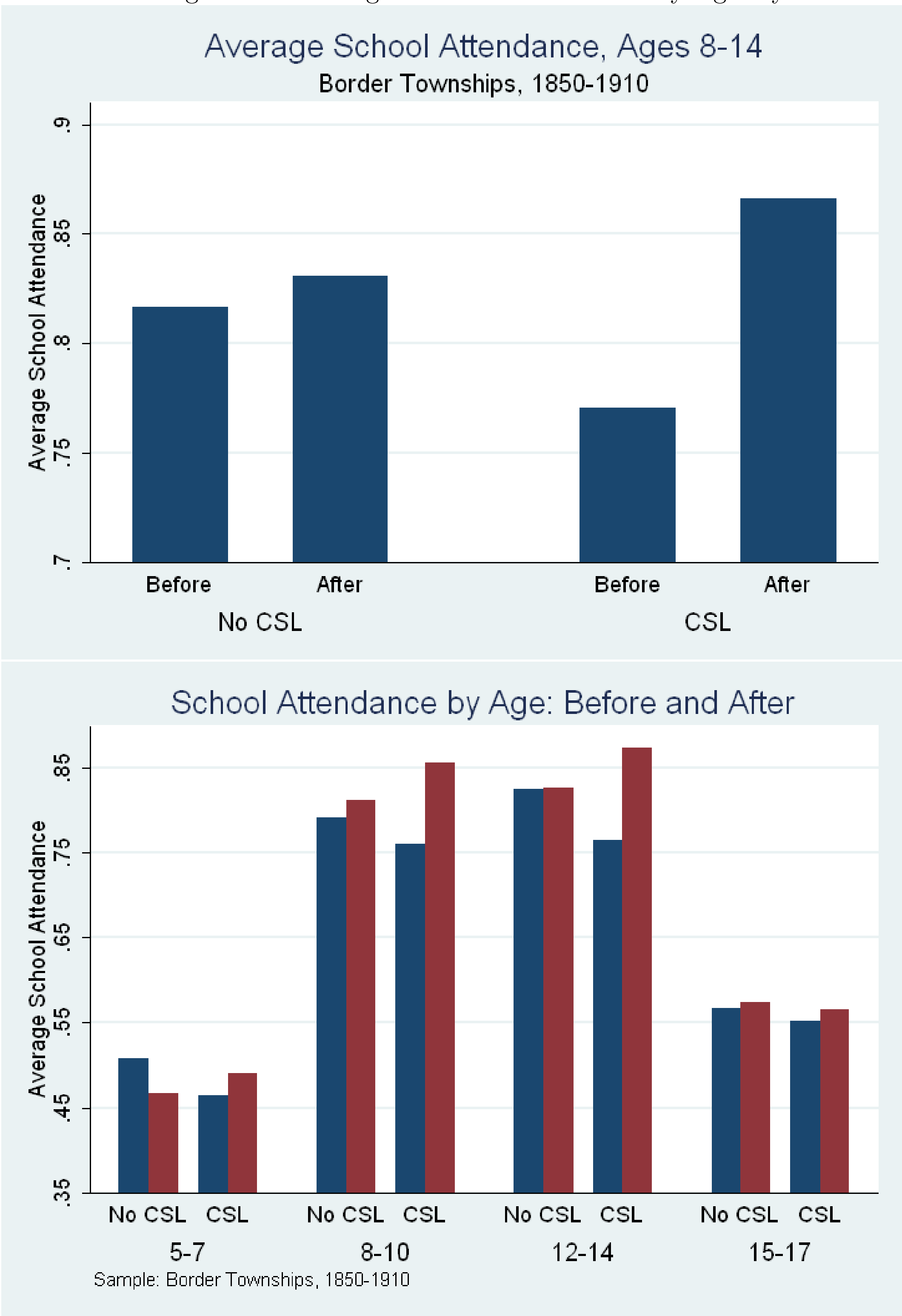


Figure A.5: CSL and School Attendance: Fitted Betas

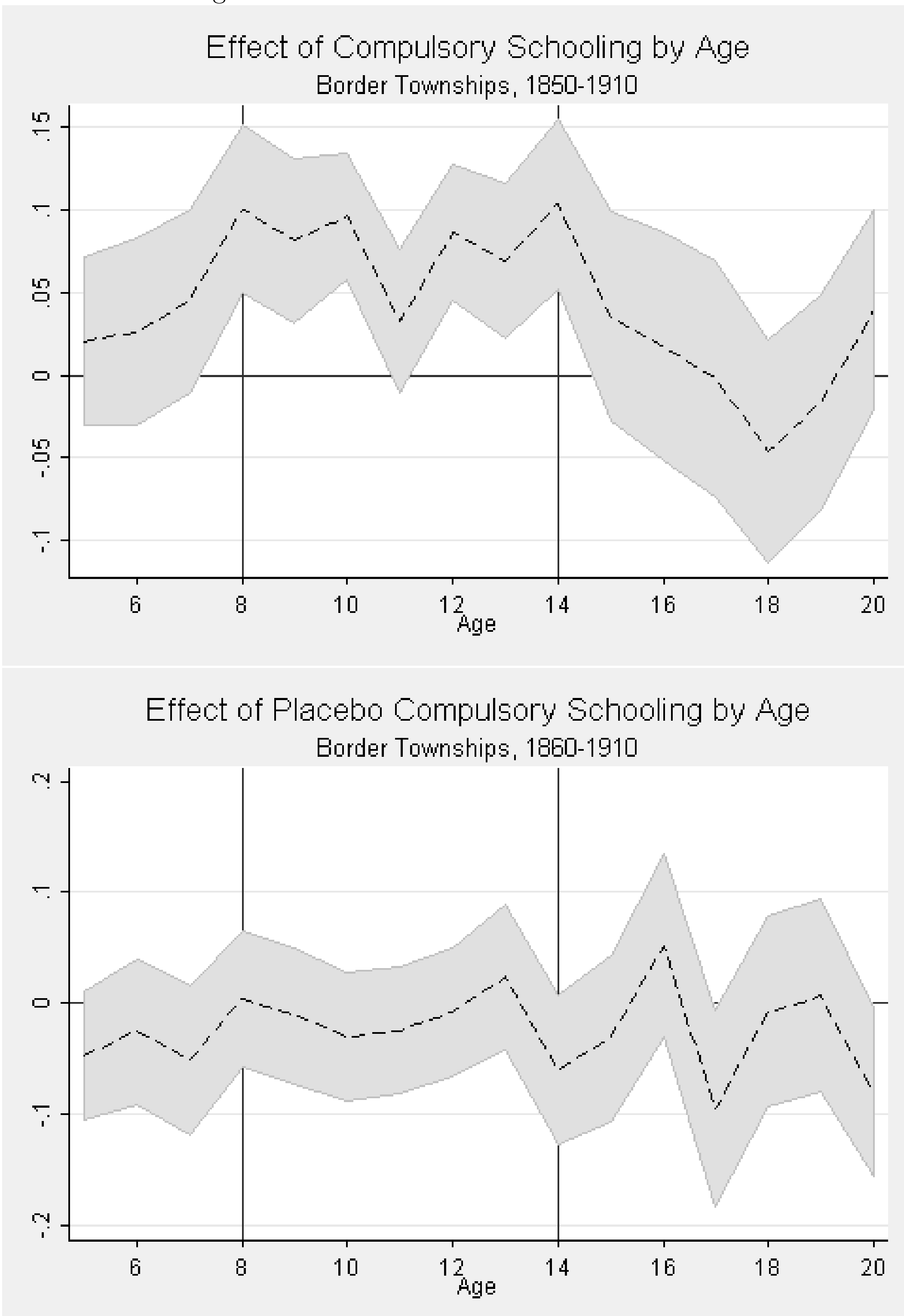


Figure A.6: Years of education and CSL

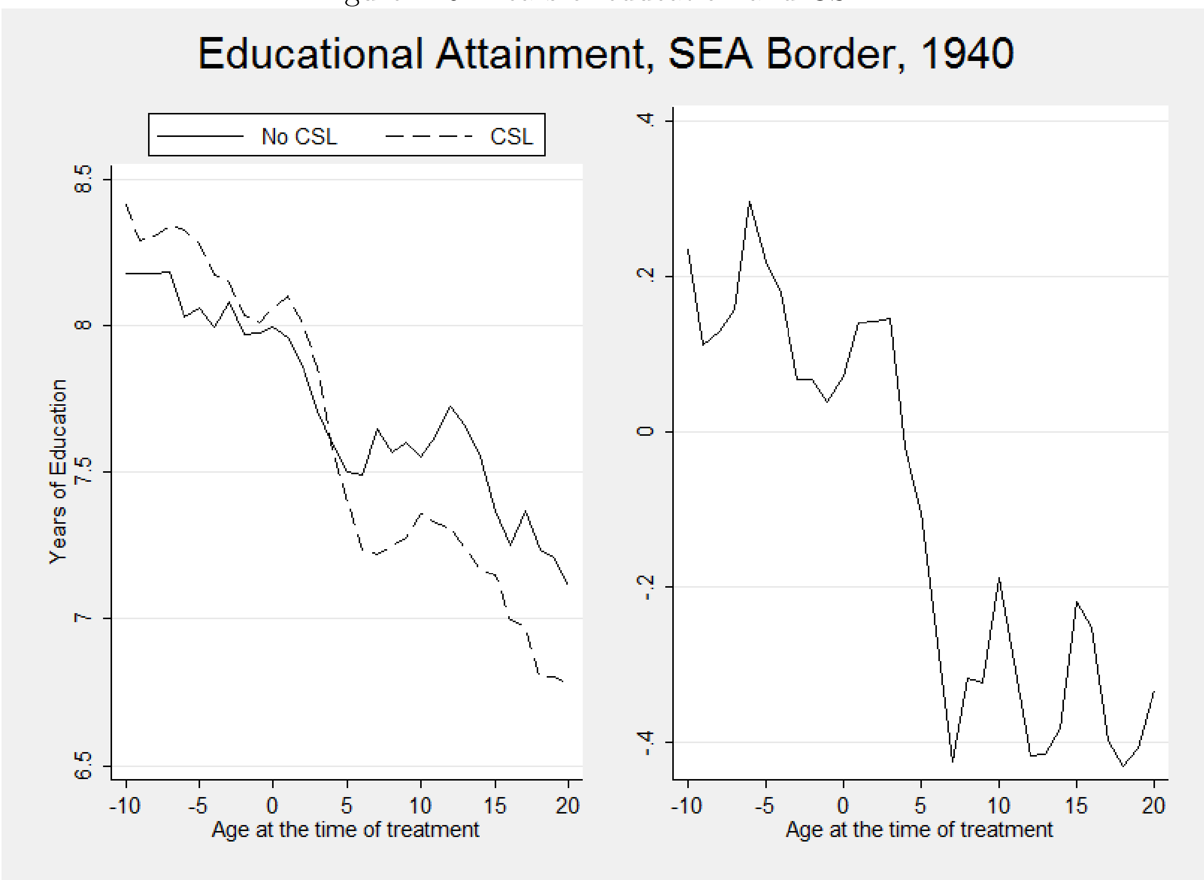
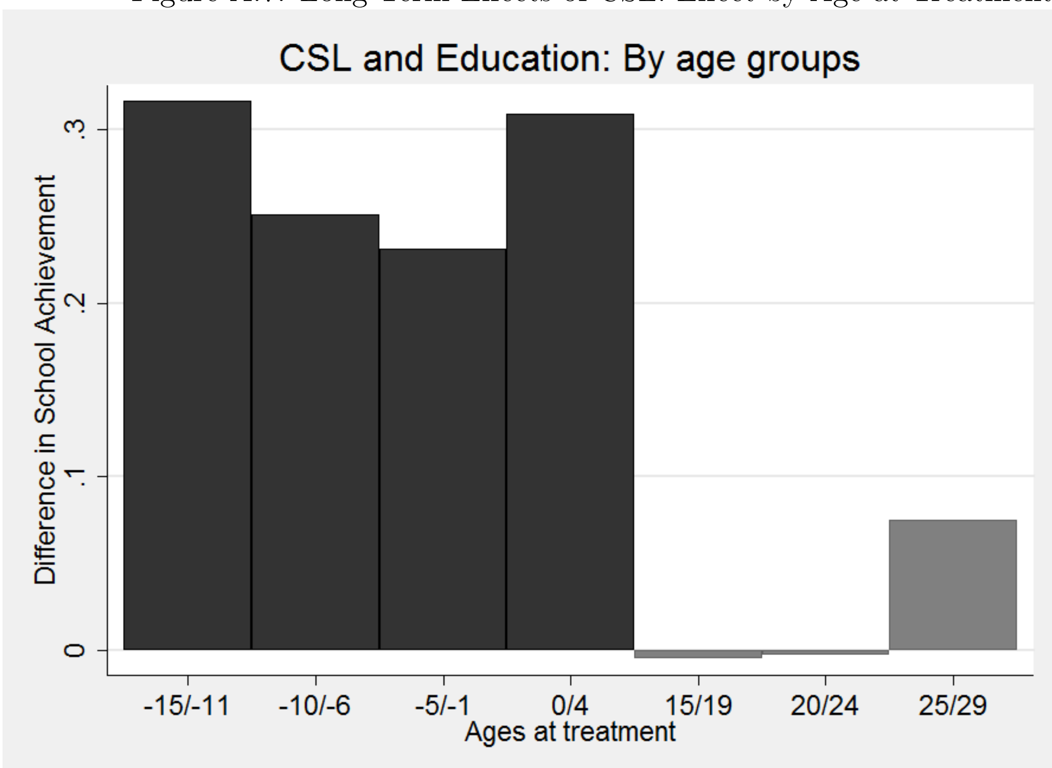


Figure A.7: Long-Term Effects of CSL: Effect by Age at Treatment



The bars represent the OLS coefficients of the main specification, allowing for effects to differ by age-group. The base category in the regression are the people who were between 6 and 14 at the time of treatment. Black bars denote that the coefficient is statistically different than 0 at 95% confidence.

Table A.1: **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

Note: Sample of children aged 8 to 14 living in border counties corresponding to the CSL borders of 1860-1910. t-tests reported do not assume equal variance among groups. For the details on the construction of the sample refer to the Data Appendix.

Table A.2: **Effect of CSL on School Enrollment**

Border Townships							
Dep. Var: *School Attendance*	(1) No Controls	(2) Year	(3) Year/State	(4) Full	(5) Full/Twn	(6) Full/White	(7) 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	6,030	6,030	6,030	6,030	6,030	5,676	3,54
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	1,199	1,199	1,199	1,199	1,199	1,146	
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	4,831	4,831	4,831	4,831	4,831	4,530	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

Note: OLS regression of equation (1.2) 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 A.3: **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

Note: OLS regression of equation (1.2) 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 A.4: **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

Note: Sample of children aged 8 to 14 living in border counties corresponding to the CSL borders of 1860-1910. t-tests reported do not assume equal variance among groups. For the details on the construction of the sample refer to the Data Appendix.

Table A.5: **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

Note: Sample of children aged 8 to 14 living in all the counties of the United States for the U.S. censuses of 1850-1920. t-tests reported do not assume equal variance among groups. For the details on the construction of the sample refer to the Data Appendix.

Table A.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

Note: Sample of children aged 8 to 14 living in all the counties of the United States for the U.S. censuses of 1850-1920. t-tests reported do not assume equal variance among groups. For the details on the construction of the sample refer to the Data Appendix.

Table A.7: **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
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

Note: OLS regression of equation (1.2) 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 A.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
Early 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
Late 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

Note: OLS regression of equation (1.2) 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 A.9: **Long-Run Effects of CSL: Summary Statistics**

	No CSL			CSL			Total		
Variable	**Mean**	**St. Dev.**	**N**	**Mean**	**St. Dev.**	**N**	**Mean**	**St. Dev.**	**N**
Years of Education	7.493	3.831	27,083	7.524	3.619	21,505	7.507	3.739	48,588
Elementary School and above	0.464	0.499	27,083	0.499	0.500	21,505	0.480	0.500	48,588
High School and above	0.191	0.393	27,083	0.178	0.382	21,505	0.185	0.389	48,588
Age at treatment less 5	0.631	0.483	27,083	0.622	0.485	21,505	0.627	0.484	48,588
Male	0.495	0.500	27,083	0.499	0.500	21,505	0.497	0.500	48,588
White	0.726	0.446	27,083	0.793	0.405	21,505	0.756	0.430	48,588
Foreigner	0.020	0.139	27,049	0.012	0.109	21,500	0.016	0.127	48,588
Farm Status	0.507	0.500	27,083	0.543	0.498	21,505	0.523	0.499	48,588
State-Native	0.814	0.389	27,083	0.743	0.437	21,505	0.783	0.412	48,588
Age	36.59	12.80	27,083	36.82	12.85	21,505	36.69	12.82	48,588
Employer	0.016	0.124	27,083	0.012	0.107	21,505	0.014	0.117	48,588
Self-Employed	0.185	0.388	27,083	0.191	0.393	21,505	0.188	0.390	48,588
Occ. Professional	0.026	0.160	27,083	0.028	0.166	21,505	0.027	0.163	48,588
Occ. Farmer	0.155	0.362	27,083	0.160	0.366	21,505	0.157	0.364	48,588
Occ. Manager	0.033	0.178	27,083	0.031	0.173	21,505	0.032	0.176	48,588
Occ. Clerical	0.021	0.145	27,083	0.022	0.147	21,505	0.022	0.145	48,588
Occ. Salaried	0.020	0.141	27,083	0.019	0.135	21,505	0.020	0.139	48,588
Occ. Craftman	0.043	0.203	27,083	0.043	0.203	21,505	0.043	0.203	48,588
Occ. Operative	0.083	0.276	27,083	0.072	0.258	21,505	0.078	0.268	48,588
Occ. Laborer	0.155	0.362	27,083	0.140	0.347	21,505	0.149	0.356	48,588

Note: Sample of individuals aged between -15 to 29 at the time of treatment (1910 CSL Border) living in the border SEAs. Data from the U.S. Federal Census of 1940. See text and Data Appendix.

Table A.10: **Long-Run Effects of CSL**

A. Education Outcomes: Basic Specifications

CSL	0.435 (0.126)***	0.288 (0.109)***	0.24 (0.111)**	0.241 (0.109)**	0.228 (0.132)*	
CSL Placebo						-.036 (0.129)
Ages	All	All	All	All	0-5 15-20	25-30 35-40
Fixed Effects	No	No	SEA	Segm-State	Segm-State	Segm-State
Controls	No	Yes	Yes	Yes	Yes	Yes
N	48,588	48,549	48,549	48,549	11,499	4,639

B. Education Outcomes: Main Specification by Gender and Race

CSL Effect	0.306 (0.124)**	0.292 (0.144)**	0.321 (0.169)*	0.289 (0.221)	0.285 (0.226)
Fixed Effects	Segm-State	Segm-State	Segm-State	Segm-State	Segm-State
Race	All	White	White	Non-White	Non-White
Gender	Both	Males	Females	Males	Females
N	37,249	13,516	13,515	4,915	5,303

C. School Completion

CSL Effect	0.04 (0.019)**	0.022 (0.014)
Dep. Var	Elementary	High-School
Fixed Effects	Segm-State	Segm-State
N	37,249	37,249

Notes: The dependent variable is the number of years of education in Panels A and B. Robust standard errors clustered by SEA (N=43) reported in square brackets. ***, ** and * denote 1%, 5% and 10% significance. Main Specification is the one that includes only state-natives that were resident in the state 5 years prior to the census, and includes full controls as explained in the text (Panels B and C). The reported CSL effects are to be interpreted like DID, that is the difference in the treatment group minus differences in the control group as defined in the text.

B. Appendix to Chapter 2

Figure B.1: Compulsory Schooling and Fertility, 1850-1920

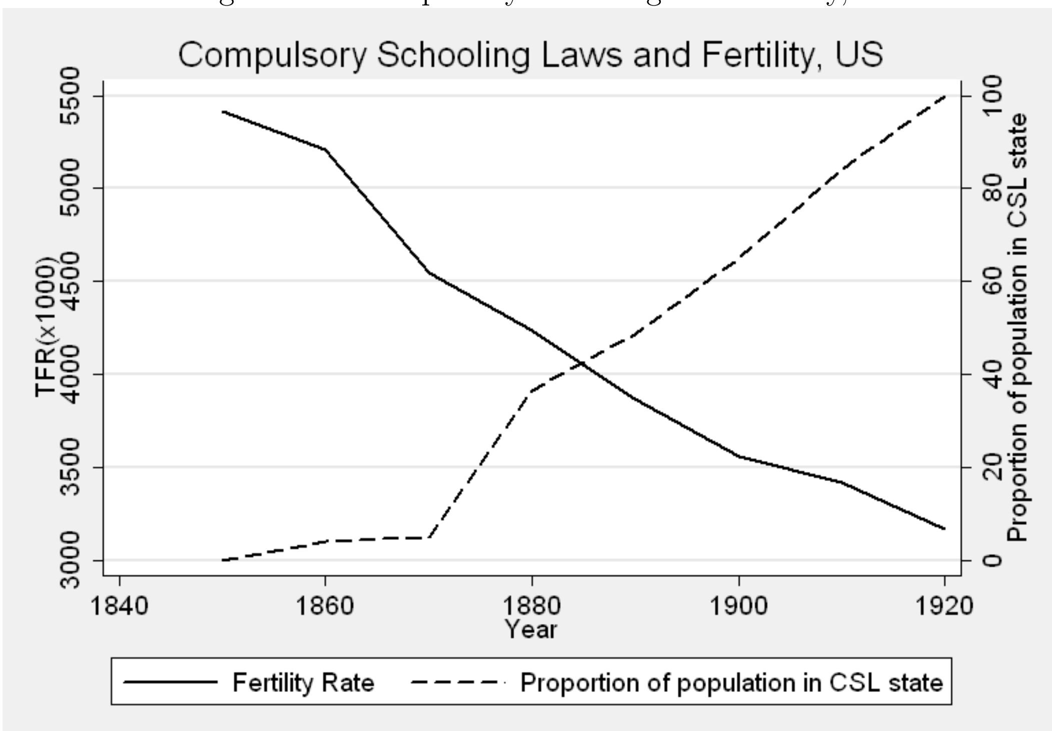
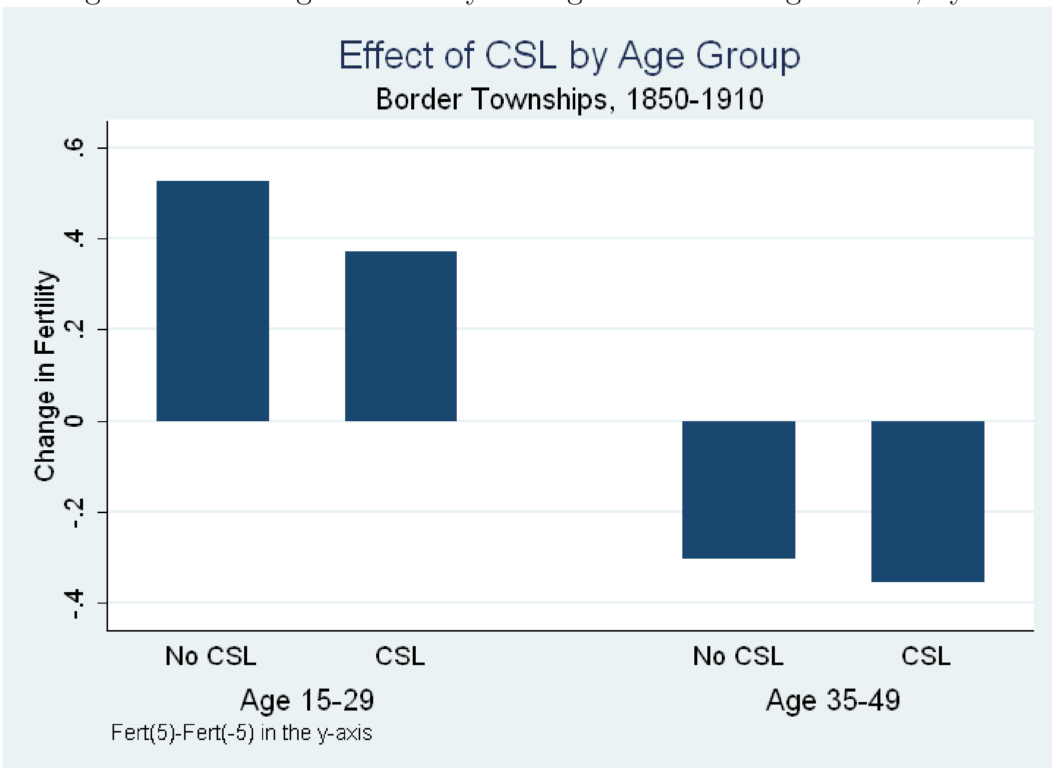


Figure B.2: Change in Fertility among Old and Young Women, by CSL status



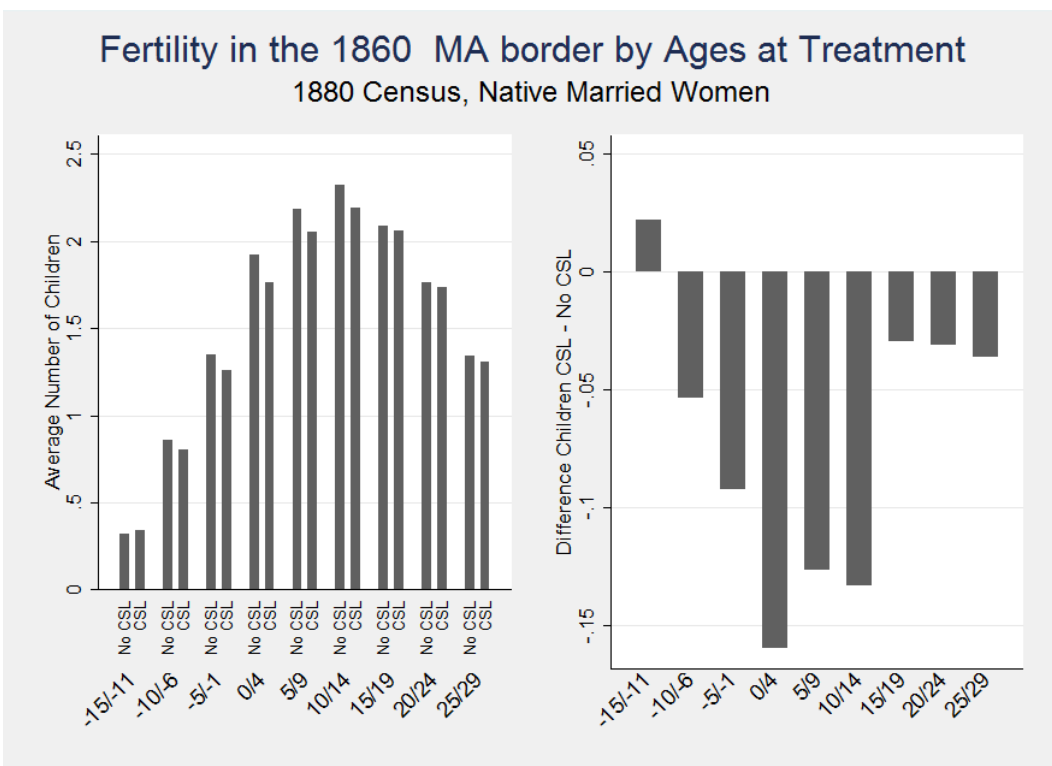
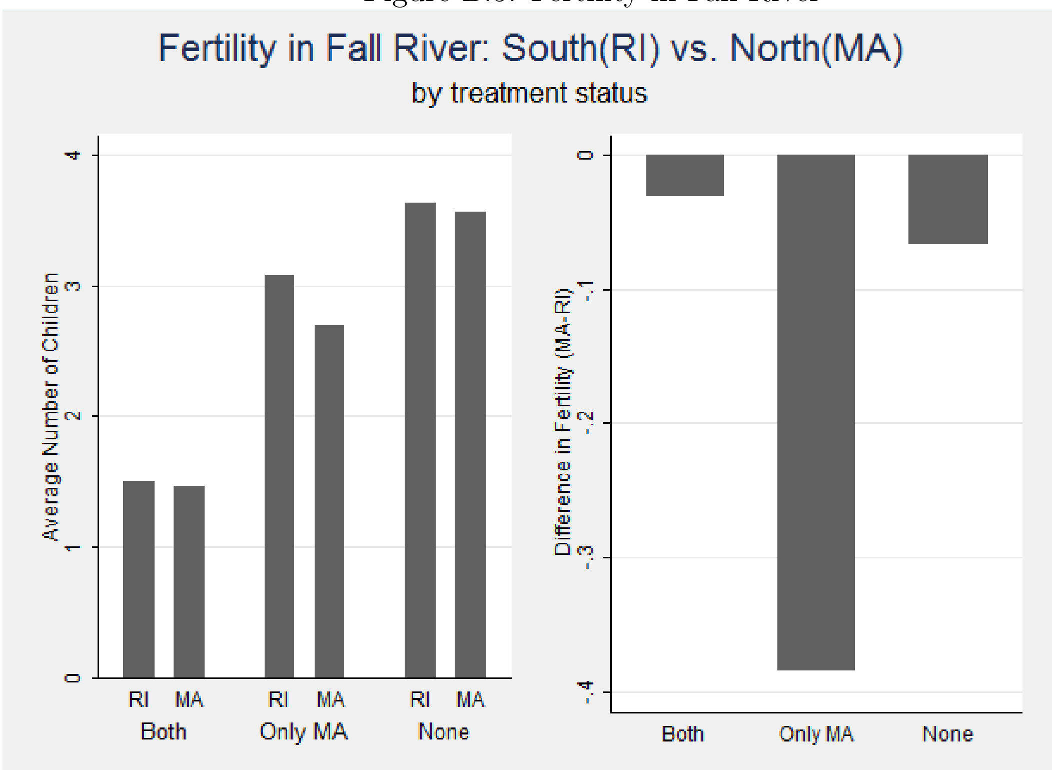


Figure B.3: Fertility differentials of women treated by the 1852 MA law in the 1880 U.S. Census. In the horizontal axis, Ages at Treatment

Figure B.4: Border change in Fall River. Image from Google Maps, borders are approximate and based on historical maps

Figure B.5: Fertility in Fall River



Note: “Both” refer to the cohort aged 0-10 at 1861. Women in this cohort received the treatment irrespective of the state they lived in; “None” refers to the cohort aged 20-30, which did not receive the treatment irrespective of the side of the border they lived in. Finally, “Only MA” refers to women aged 11-20 at the time the border changed in 1861. These women mostly received the education treatment if they lived in Massachusetts but not if they lived in Rhode Island.

Table B.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

Note: Sample of women aged 15 to 49 living in border counties corresponding to the CSL borders of 1860-1910. t-tests reported do not assume equal variance among groups. For the details on the construction of the sample refer to the Data Appendix.

Table B.2: **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 Fertility	Own Children	2.642	2.142	2.681	2.167	2.623	2.120	2.571	2.115
	Children<5	0.815	0.909	0.901	0.921	0.878	0.923	0.736	0.874
Women Controls	Age	34.103	9.532	32.350	8.856	33.163	8.988	35.399	9.822
	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 Head Occupation	Professional	0.028	0.165	0.027	0.162	0.028	0.166	0.031	0.174
	Farmer	0.417	0.493	0.485	0.500	0.382	0.486	0.317	0.465
	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	

Note: Sample of women aged 15 to 49 living in counties and townships corresponding to the CSL borders of 1860-1910. t-tests reported do not assume equal variance among groups. For the details on the construction of the sample refer to the Data Appendix.

Table B.3: **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

Note: Sample of women aged 15 to 49 living in townships corresponding to the CSL borders of 1860-1910 for whom a fertility history can be constructed as indicated in the text. t-tests reported do not assume equal variance among groups. For the details on the construction of the sample refer to the Data Appendix.

Table B.4: **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

Note: OLS estimates with robust standard errors in parentheses standard errors reported. In the first row, equation (2.2) is estimated and standard errors are clustered by mother. In the other rows, the estimated equation is (2.2). All regressions but (1) use all the controls described in the text.
*** p<0.01, ** p<0.05, * p<0.1

Table B.5: **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

Note: 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 B.6: **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

Note: 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 B.7: **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

Note: 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 B.8: **Long-Run Effects of CSL on Fertility: Summary Statistics**

	No CSL			**CSL**			**Total**		
Variable	**Mean**	**St. Dev.**	**N**	**Mean**	**St. Dev.**	**N**	**Mean**	**St. Dev.**	**N**
No. of Children	2.27	2.04	25,811	2.26	2.03	28,801	2.26	2.04	54,612
Age at treatment -15/-11	0.00	0.06	25,811	0.00	0.05	28,801	0.00	0.06	54,612
Age at treatment -10/-6	0.07	0.25	25,811	0.07	0.25	28,801	0.07	0.25	54,612
Age at treatment -5/-1	0.14	0.35	25,811	0.15	0.35	28,801	0.15	0.35	54,612
Age at treatment 0/4	0.16	0.37	25,811	0.17	0.37	28,801	0.16	0.37	54,612
Age at treatment 5/9	0.16	0.36	25,811	0.16	0.37	28,801	0.16	0.36	54,612
Age at treatment 15/19	0.15	0.36	25,811	0.15	0.36	28,801	0.15	0.36	54,612
Age at treatment 20/24	0.13	0.33	25,811	0.13	0.33	28,801	0.13	0.33	54,612
Age at treatment 25/29	0.11	0.31	25,811	0.11	0.31	28,801	0.11	0.31	54,612
Foreign	0.33	0.47	25,811	0.39	0.49	28,801	0.36	0.48	54,612
Irish	0.15	0.36	25,811	0.18	0.38	28,801	0.16	0.37	54,612
British	0.20	0.40	25,811	0.27	0.44	28,801	0.24	0.42	54,612
Canadian	0.10	0.30	25,811	0.10	0.30	28,801	0.10	0.30	54,612
Occ. Professional	0.02	0.15	25,811	0.02	0.14	28,801	0.02	0.15	54,612
Occ. Farmer	0.22	0.42	25,811	0.16	0.37	28,801	0.19	0.39	54,612
Occ. Manager	0.07	0.26	25,811	0.08	0.28	28,801	0.08	0.27	54,612
Occ. Clerical	0.01	0.11	25,811	0.01	0.10	28,801	0.01	0.1	54,612
Occ. Salaried	0.02	0.15	25,811	0.02	0.15	28,801	0.02	0.15	54,612
Occ. Craftman	0.16	0.37	25,811	0.14	0.35	28,801	0.15	0.36	54,612
Occ. Operative	0.28	0.45	25,811	0.37	0.48	28,801	0.33	0.47	54,612
Occ. Laborer	0.18	0.38	25,811	0.15	0.36	28,801	0.16	0.37	54,612
Works	0.05	0.21	25,811	0.06	0.24	28,801	0.05	0.23	54,612
White	0.99	0.07	25,811	0.99	0.08	28,801	0.99	0.08	54,612

Note: Married women born between 29 years before to 15 years after the CSL residing in 1880 in any of the CSL border townships.

Table B.9: **Long Run Fertility Regressions: MA Border - 1880**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CSL	0.065	-.024	0.347	0.268	0.226	0.269	-.126
	(0.033)*	(0.041)	(0.05)***	(0.062)***	(0.045)***	(0.062)***	(0.104)
CSL×-15/14	-.115	-.098					
	(0.039)***	(0.049)**					
Age -15/14	-.185	0.033					
	(0.028)***	(0.034)					
CSL×-15/-1			-.125	-.014	-.091	-.022	-.001
			(0.04)***	(0.052)	(0.038)**	(0.051)	(0.054)
CSL×0/14			-.123	-.119	-.105	-.128	-.132
			(0.042)***	(0.054)**	(0.039)***	(0.054)**	(0.056)**
Age -15/-1			-1.142	-.612	-1.013	-.586	-.582
			(0.029)***	(0.036)***	(0.028)***	(0.037)***	(0.038)***
Age 0/14			0.223	0.374	0.251	0.4	0.418
			(0.03)***	(0.038)***	(0.028)***	(0.038)***	(0.039)***
Foreign					0.737		
					(0.05)***		
Husb. Farmer					0.124	0.344	0.402
					(0.048)***	(0.06)***	(0.065)***
Husb. Craftman					0.063	0.213	0.296
					(0.047)	(0.06)***	(0.066)***
Husb. Operative					0.117	0.229	0.28
					(0.045)***	(0.059)***	(0.064)***
Husb. Laborer					0.322	0.338	0.371
					(0.049)***	(0.065)***	(0.07)***
Works					-1.085	-.596	-.596
					(0.032)***	(0.051)***	(0.057)***
Obs	54,612	21,242	54,612	21,242	54,612	21,242	18,890
R^2	0.003	0.001	0.094	0.072	0.213	0.082	0.085
Township FE	No	No	Yes	Yes	Yes	Yes	Yes
Sample	All	Native	All	Native	All	Native	Native(†)

Note: OLS regression with robust standard errors. The dependent variable is the number of own children. The sample includes all the married women with spouse present at the household who were born in the state in which they reside whose ages are between -15 and 29 at the time of treatment. In the table the coefficients for Irish, British, Canadian, White and some occupations (professional, manager, salaried, clerk) are omitted. The omitted category is the women with ages 15-29 at treatment. (†) In the last column, the sample includes all the native married women that but the ones residing in Falls River (MA) and Pawtucket (RI). Fixed-effects regressions include a dummy for each of the 109 townships in the regression.

Table B.10: **Long-Run Effects of CSL on Fertility, Fall River: Summary Statistics**

	No CSL			**CSL**			**Total**		
Variable	**Mean**	**St. Dev.**	**N**	**Mean**	**St. Dev.**	**N**	**Mean**	**St. Dev.**	**N**
No. of Children	2.66	2.17	2,760	2.58	2.21	2,654	2.62	2.19	5,414
Aged less 10 in 1861	0.34	0.47	2,760	0.31	0.46	2,654	0.33	0.47	5,414
Aged 11-20 in 1861	0.39	0.49	2,760	0.38	0.49	2,654	0.38	0.49	5,414
Foreign	0.73	0.44	2,760	0.59	0.49	2,654	0.66	0.47	5,414
Works	0.16	0.37	2,760	0.11	0.32	2,654	0.14	0.34	5,414
Husband Craftman	0.16	0.36	2,760	0.16	0.37	2,654	0.16	0.37	5,414
Husband Operative	0.48	0.5	2,760	0.42	0.49	2,654	0.45	0.5	5,414
White	1	0.03	2,760	1	0.07	2,654	1	0.05	5,414

Notes: See data appendix.

Table B.11: **Fertility Regressions: Fall River, 1880**

	(1)	(2)	(3)	(4)	(5)	(6)
Aged less 10 in 1861	-2.093	-2.090	-1.672	-1.919	-1.192	-1.327
	(0.13)***	(0.133)***	(0.122)***	(0.146)***	(0.154)***	(0.107)***
Aged 11-20 in 1861	-.586	-.587	-.407	-.420	-.082	0.059
	(0.144)***	(0.15)***	(0.143)***	(0.175)**	(0.143)	(0.139)
North Fall River	-.035					
	(0.234)					
North $\times$ Age less 10	-.001	-.042	-.164	-.333	-.111	-.118
	(0.198)	(0.189)	(0.159)	(0.193)	(0.203)	(0.134)
North $\times$ Age 11/20	-.286	-.280	-.311	-.528	-.486	-.358
	(0.164)*	(0.17)*	(0.162)*	(0.214)**	(0.224)**	(0.187)*
Foreign			1.025			
			(0.081)***			
Works			-1.308	-1.353	-1.149	-1.206
			(0.074)***	(0.083)***	(0.068)***	(0.08)***
Husband Craftman			-.240	-.255	-.254	-.125
			(0.057)***	(0.108)**	(0.125)**	(0.136)
Husband Operative			-.205	-.294	-.222	-.146
			(0.05)***	(0.075)***	(0.093)**	(0.09)
White			-.774			
			(0.476)			
Obs.	5414	5414	5414	3597	2364	3066
Enum.Dist. FE	No	Yes	Yes	Yes	Yes	Yes
Sample	All	All	All	Foreign	Foreign	Foreign
Residence	All	All	All	All	US in 1861	Parity US

Number of Observations by Age at Treatment (1861) and Residence

	Less 10	11-20	21-30
North Fall River	822	1,014	818
South Fall River	938	1,065	757

Note: OLS regression with robust standard errors clustered by enumeration districts in parentheses. The dependent variable is the number of own children. Treatment is defined according to the enumeration districts in areas corresponding to RI and MA before 1861. I include dummies for the two most common occupations of husbands (operative and craftman) which cover about 75 percent of the observations. The sample includes all the married women with spouse present at the household who were between -4 and 30 at the time of treatment residing in Fall River.

C. Appendix to Chapter 3

Figure C.1: Child Labor and GDP: Today and in History

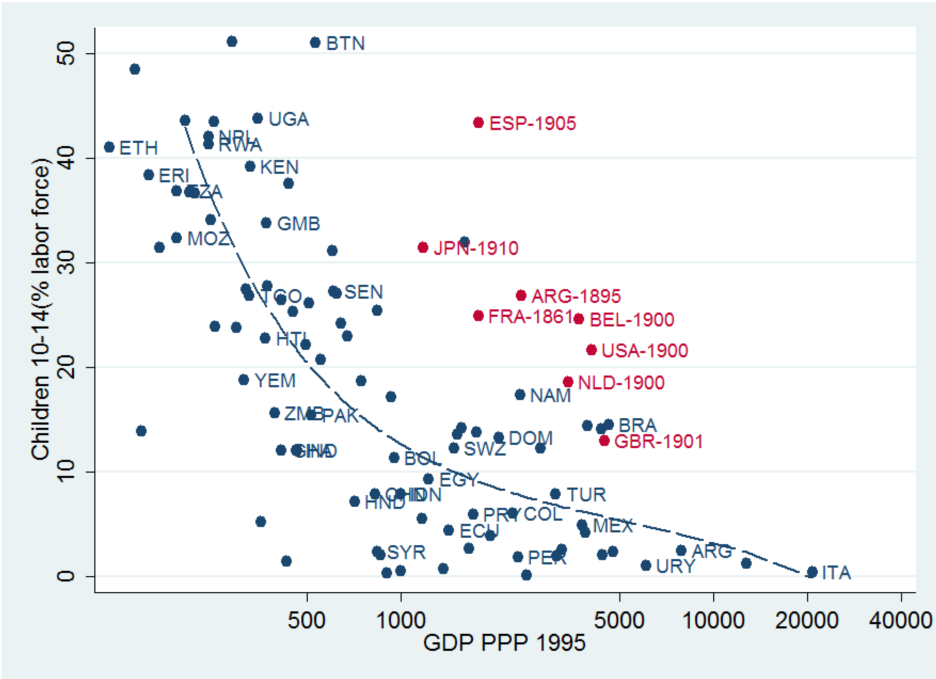


Figure C.2: Glass Industry in New Jersey

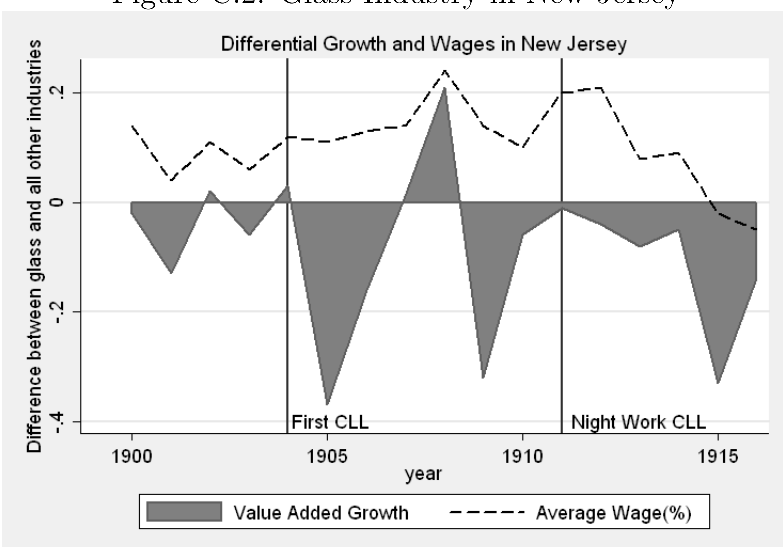


Figure C.3: Industry Growth in the Ohio Valley

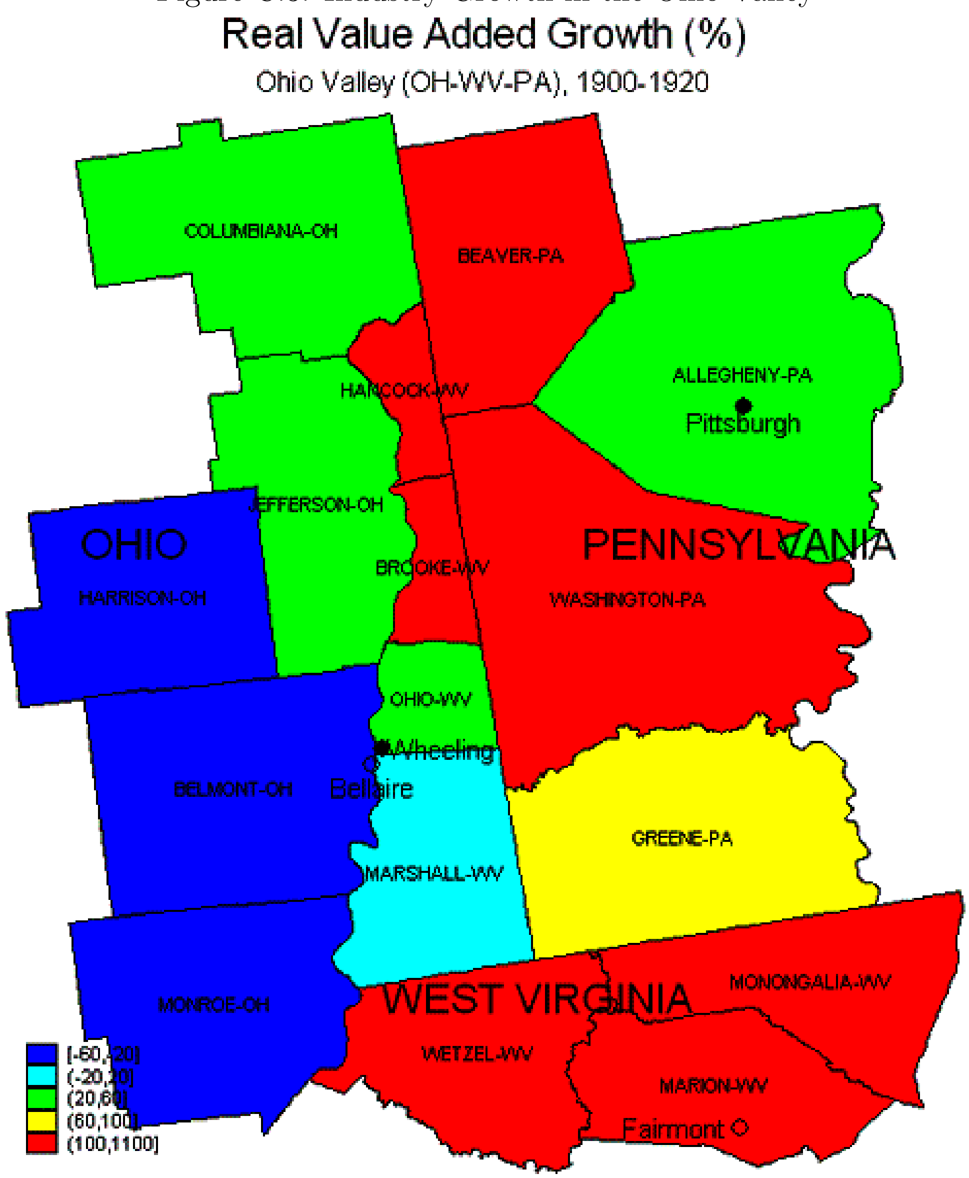


Figure C.4: Child Labor Dependence and CLL Strength in 1909

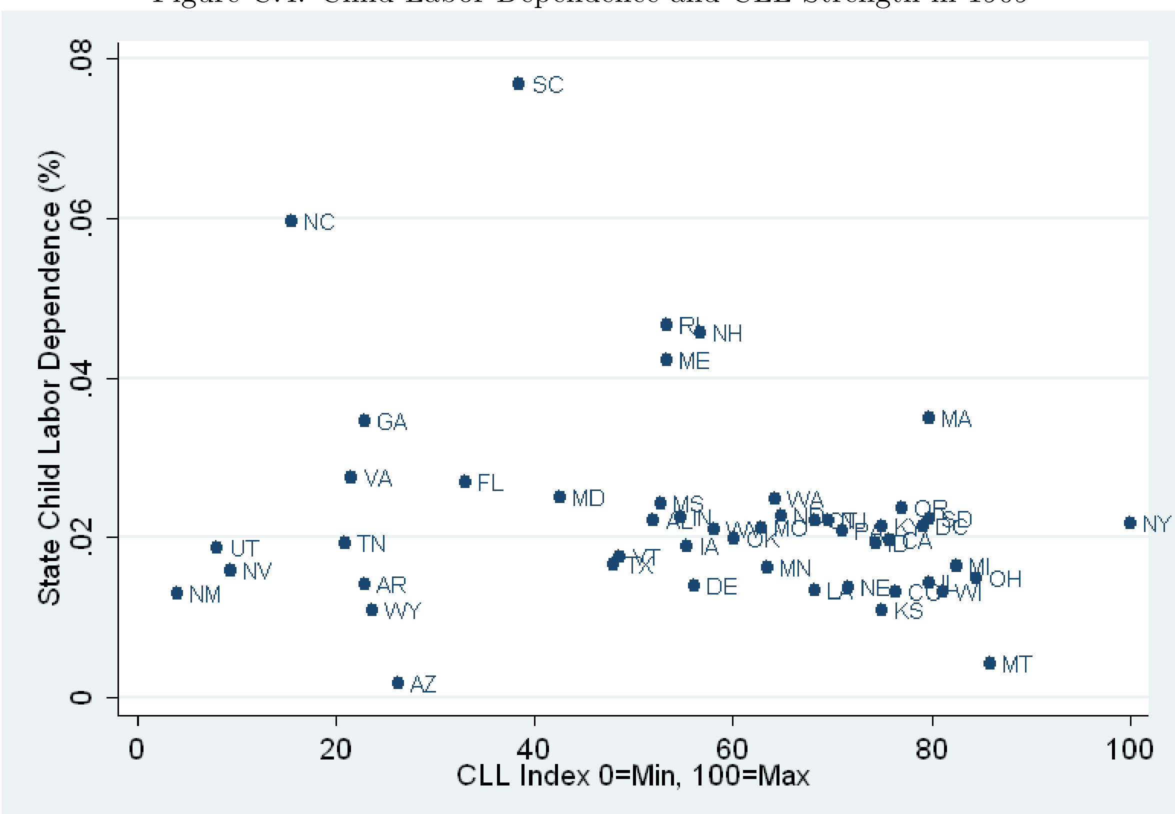


Table C.1: Labor Force Participation of Children in the U.S. 1870-1930

Year	Workers 10-15	Population 10-14	Total Workers	Children (% Labor Force)	Children (Activity Rates)
1870	765	4,786	12,925	5.92%	15.98%
1880	1,118	5,715	17,392	6.43%	19.56%
1890	1,504	7,034	23,318	6.45%	21.38%
1900	1,750	8,080	29,073	6.02%	21.66%
1910	1,622	9,107	37,371	4.34%	17.81%
1920	1,417	10,641	42,434	3.34%	13.32%
1930	667	12005	48830	1.37%	5.56%

Source: Historical Statistics of the U.S. Colonial Times to 1970. Series A119-134, D75-84. First three columns are in thousands.

Table C.2: Child Labor By Industry Groups. United States, 1900

Industry	*Total Workers*	*Child Workers*	*Child Labor**
Food and Kindred Products	313,809	11,229	3.58%
Textiles	1,029,910	77,023	7.48%
Iron and Steel	712,195	7,996	1.12%
Lumber and it remanufactures	546,953	12,098	2.21%
Leather and its finished products	238,202	6,306	2.65%
Paper and Printing	297,551	12,069	4.06%
Liquors and Beverages	63,072	1,369	2.17%
Chemicals	101,522	828	0.82%
Clay, Glass and Stone	244,987	10,644	4.34%
Metals and Metal Products	190,757	4,798	2.52%
Tobacco	142,277	7,913	5.56%
Vehicles	316,214	1,324	0.42%
Shipbuilding	46,781	1,003	2.14%
Miscellaneous Industries	483,273	9,590	1.98%
Hand Trades	559,130	4,402	0.79%

Note:(*) Percentage of total workers. See data appendix for sources.

Table C.3: Children as a percentage of total workers, Glass Industry

Location	**Year**	**Children(*)**
Argentina (Buenos Aires)	1904	26
United States	1880	23
United States	1890	15
United States	1900	13
United States, Belmont Co.,(Ohio)	1886	26
England and Wales	1851	18
India (Firozabad)	2000	25
Japan	1900	24
Russia	1911	32
France(**)	1861-1865	13
Sweden	1850-1862	26
Finland	1850-1862	36
Norway	1870-1874	16
Denmark	1872	19

Notes: (*) Percentage of total workers. (**) includes glass and ceramics. See data appendix for sources

Table C.4: Cost Structure of the Glass Industry - 1900

A. ACTUAL DATA				
		Number	*Total Wages*	*Avg. Wages*
Male		*42,173*	*$ 24,901,233*	*$ 590*
Female		*3,529*	*$ 840,001*	*$ 238*
Children		*7,116*	*$ 1,343,476*	*$ 189*
TOTAL		**52,818**	**27,084,710**	**$ 513**
B. IMPUTED DATA				
	Male Workers	*Number*	*Total Wages*	*Avg. Wages*
Skilled		17,664	$ 15,035,658	$ 851
	Blowers	4,096	$ 5,111,605	$ 1,248
	Cutters	395	$ 267,331	$ 676
	Finishers	819	$ 617,652	$ 754
	Foremen	772	$ 662,452	$ 858
	Gatherers	4,680	$ 3,163,418	$ 676
	Mixers	621	$ 355,462	$ 572
	Packers	1,544	$ 883,270	$ 572
	Pressers	1,704	$ 2,082,587	$ 1,222
	General Occupations	3,032	$ 1,891,881	$ 624
Unskilled		24,509	$ 9,558,554	$ 390
MEN		*42,173*	*$ 24,594,213*	*$ 583*
WOMEN		*3,529*	*$ 825,786*	*$ 234*
CHILDREN		*7,116*	*$ 1,480,128*	*$ 208*
TOTAL		**52,818**	**$ 26,900,127**	**$ 509**

C. COUNTERFACTUAL: Unskilled labor for children

	Actual Data	*Counterf. Cost*	*ΔCost*	*ΔCost/Fuel*
Total Cost of Materials	$ 16,731			
Fuel costs	$ 3,203			
Salaries	$ 2,792			
Wages	$ 27,085	$ 28,516		
Miscellaneous	$ 3,589			
Total Cost (Actual)	$ 50,197	$ 51,629	$ 1,432	44.70%
Total Cost (Imputed Wages)	$ 50,013	$ 51,308	$ 1,295	40.43%
Value of Output	$ 56,540			

Sources: Actual data comes from the report on the glass industry (U.S. Manufacturing census, 1900). Imputed data based on the numbers and median earnings of workers in the glass industry (Special Report on Employees and Wages, Washington, GPO, 1903, p. lxxxi ff.). Counterfactual exercise replaces all the children with unskilled workers. Unskilled workers comprised laborers and general workers whose median weekly earnings were the lowest among adult males ($7.5 weekly).

Table C.5: The glass industry in selected states

District	*Year*	*Children (%)*	*Firms*	*Real V.A. (x1000)*	*Real V.A. Growth*	*Fuel Cost (x1000)*	*Fuel cost (% Real V.A.)*	*Fuel Costs (% Mate-rials)*	*% U.S. Value Added*
Illinois	1899	16.62%	6	3,195	-1.29%	155	4.86%	23.06%	5.43%
1903	1904	8.03%	13	7,069	121.2%	341	4.83%	24.39%	7.89%
	1909	4.72%	11	5,186	-26.6%	468	9.03%	30.41%	5.85%
	1914	0.25%	10	7,154	37.94%	957	13.38%	34.09%	6.32%
	1919	0.86%	14	9,153	27.94%	2,108	23.03%	37.92%	7.41%
Ohio	1899	13.99%	28	4,873	-37.1%	249	5.12%	19.90%	8.27%
1908	1904	9.38%	37	10,367	112.7%	618	5.97%	21.80%	11.58%
	1909	4.26%	45	14,305	37.99%	1,091	7.63%	23.28%	16.12%
	1914	0.50%	39	17,834	24.67%	1,676	9.40%	23.79%	15.76%
	1919	0.40%	44	16,297	-8.62%	3,668	22.51%	28.99%	13.20%
Indiana	1899	11.30%	110	15,053	268.9%	355	2.36%	7.75%	25.56%
1911	1904	7.25%	96	15,323	1.79%	1,068	6.97%	19.22%	17.11%
	1909	5.11%	44	10,155	-33.7%	998	9.83%	21.11%	11.45%
	1914	1.70%	41	11,712	15.33%	1,331	11.37%	19.29%	10.35%
	1919	4.74%	35	12,231	4.43%	2,641	21.59%	20.08%	9.91%
Penn-	1899	14.10%	119	23,041	1.21%	1,421	6.17%	22.09%	39.13%
sylvania	1904	12.57%	122	30,732	33.38%	2,360	7.68%	25.31%	34.32%
1915	1909	6.47%	112	29,858	-2.84%	2,898	9.71%	22.94%	33.65%
	1914	5.36%	103	35,618	19.29%	3,760	10.56%	24.19%	31.47%
	1919	2.19%	102	38,110	7.00%	7,761	20.37%	28.06%	30.87%
West	1899	8.31%	16	1,892	47.80%	88,905	4.70%	14.99%	3.21%
Virginia	1904	12.28%	39	5,595	195.7%	229	4.11%	18.25%	6.25%
1919	1909	5.19%	51	8,111	44.98%	400	4.93%	17.42%	9.14%
	1914	0.94%	63	14,674	80.92%	720	4.91%	15.53%	12.97%
	1919	1.47%	77	20,872	42.24%	2,790	13.37%	20.22%	16.91%
United	1899	13.47%	355	58,889	6.33%	3,203	5.44%	19.15%	
States	1904	10.06%	399	89,551	52.07%	6,243	6.97%	23.88%	
	1909	5.17%	363	88,722	-0.93%	7,523	8.48%	23.43%	
	1914	2.67%	348	113,169	27.55%	10,934	9.66%	23.76%	
	1919	1.82%	371	123,452	9.09%	24,357	19.73%	26.83%	

Sources: Glass reports from the U.S. Manufacturing census (various years). The value added series was converted into real terms with the U.S. wholesale price index (Carter, Susan et al.(2008)[Vol 3, 3-175])

Table C.6: Manufacturing Employment and Value Added Growth in selected counties (1900-1920)

		Manufacturing Employment				*Value Added*
County	*Pop.*	*1900*	*1920*	Δ	Δ%	Δ%
Hancock	6,693	697	6,420	5,723	821.09%	1003.18%
Brooke	7,219	762	2,979	2,217	290.94%	387.74%
Ohio	48,024	7,348	9,032	1,684	22.92%	35.26%
Marshall	26,444	3,714	5,336	1,622	43.67%	-10.53%
Total	88,380	12,521	23,767	11,246	89.82%	67.37%
WV State Total	958,800	33,272	83,036	49,764	149.57%	137.86%
% of State Total	9.22%	37.63%	28.62%	22.60%		
Columbiana	68,590	8,809	12,663	3,854	43.75%	49.97%
Jefferson	44,357	4,302	7,054	2,752	63.97%	56.86%
Belmont	60,875	5,472	5,643	171	3.13%	-24.02%
Monroe	27,031	188	137	-51	-27.13%	-58.99%
OH State Total	200,853	18,771	25,497	6,726	35.83%	27.42%
Ohio	4,157,545	345,869	730,733	384,864	111.27%	114.31%
% of State Total	4.83%	5.43%	3.49%	1.75%		

Sources: Own calculations based on Manufacturing Census of the U.S. 1919, 1899. Ohio and West Virginia State Reports.

Table C.7: Industrial statistics - Bellaire and Wheeling

	Year	*Firms*	*No. Wage Earners*	*Value Added*	*Δ% Employment*	*Δ% Avg. State Empl.*	*Δ% Real Value Added*	*Δ% State Real Value Added*
Bellaire	1919	45	2277	3300	-12.5%	43.16%	-22.3%	41.10%
OH	1914	40	2603	4247	0.23%	14.21%	-2.1%	23.25%
	1909	36	2597	4339	18.96%	22.68%	-23.7%	25.13%
	1904	37	2183	5689	13.23%	18.24%	4.16%	11.61%
	1899	30	1928	5462				
Wheeling	1919	243	8622	18998	8.86%	16.82%	8.67%	18.32%
WV	1914	201	7920	17482	1.42%	11.25%	6.92%	19.97%
	1909	176	7809	16350	9.57%	46.01%	4.86%	36.70%
	1904	195	7127	15592	15.14%	32.28%	22.06%	31.02%
	1899	178	6190	12774				

Sources: Manufacturing census state reports for various census dates. The value added series is deflated with the wholesale price index (Carter, Susan et al.(2008)[Vol 3, 3-175])

Table C.8: **Summary Statistics**

		CL Dependence		
A. VA Growth (1910-1920)		Low Dep	High Dep	Total
No CLL 1910	Mean	0.015	0.022	0.019
	Median	0.019	0.024	0.022
	St. Dev	0.074	0.07	0.072
	Observations	579	717	1296
CLL 1910	Mean	0.02	0.018	0.019
	Median	0.02	0.017	0.018
	St. Dev	0.074	0.065	0.069
	Observations	425	474	899
B. Other Variables	Whole Sample	Low Dep	High Dep	Source
VA Growth (1900-1910)	.0384	.0401	.0369	Man. Census
	(.083)	(.088)	(.079)	
Child Labor Dependence	0.019	0.002	0.033	Man. Census
(1900)*	(0.025)	(0.002)	(0.026)	
Ln. Prop. Manufacturing	-5.461	-5.502	-5.427	Man. Census
	(1.869)	(1.994)	(1.757)	
Real Capital*	14.233	14.412	14.082	Man. Census
	(2.001)	(2.074)	(1.924)	
State Investment	0.439	0.442	0.437	Man. Census
(1900-1910)	(0.319)	(0.314)	(0.323)	
Ln Wage Earners*	6.174	6.027	6.297	Man. Census
	(1.851)	(1.864)	(1.831)	
Population Black	0.083	0.08	0.085	ICPSR 2896
	(0.130)	(0.128)	(0.132)	
Population Illiterate	0.067	0.066	0.069	ICPSR 2896
	(0.058)	(0.056)	(0.058)	
Population Foreign	0.169	0.17	0.168	ICPSR 2896
	(0.102)	(0.1)	(0.103)	
Population Urban	0.501	0.503	0.5	ICPSR 2896
	(0.222)	(0.217)	(0.226)	
Ln State Income	5.334	5.345	5.324	Easterlin(1957)
	(0.401)	(0.397)	(0.405)	
Avg. Sei Index	25.328	25.423	25.247	IPUMS
	(3.277)	(3.238)	(3.308)	
Ln Patents	-6.364	-6.347	-6.379	Johnson(2002)
	(0.664)	(0.653)	(0.673)	
CLL&Dep	0.007	0.001	0.012	Man. Census
	(0.017)	(0.001)	(0.021)	
Sample Size	2195	1004	1191	

Standards errors in parenthesis.* denotes that the means are different from zero according a t-test with unequal variances. Sample size for Ln. Wage Earners is 1004 and 1190 respectively.

Table C.9: **Basic Specification**

	(1)	(2)	(3)	(4)
Ln. Wage Earners	0.004	0.01	0.003	0.016
	(0.002)	(0.003)***	(0.004)	(0.006)***
Ln. Real Capital	0.005	0.012	0.002	0.007
	(0.002)**	(0.003)***	(0.004)	(0.004)
Prop. Manufacturing	-.012	-.026	-.013	-.030
	(0.002)***	(0.005)***	(0.002)***	(0.006)***
CL Dependence	0.327	0.303		
	(0.067)***	(0.068)***		
CLL	-.0003		0.004	
	(0.005)		(0.004)	
State Investment 1900-10	0.02		0.019	
	(0.007)***		(0.005)***	
Black (%)	0.119		0.143	
	(0.041)***		(0.035)***	
Urban (%)	-.038		-.034	
	(0.02)*		(0.017)**	
Illiteracy (%)	-.082		-.131	
	(0.092)		(0.077)*	
Foreign (%)	0.005		0.007	
	(0.036)		(0.029)	
Ln. Patents	0.003		0.005	
	(0.009)		(0.008)	
Ln. State Income 1900	0.019		0.016	
	(0.012)		(0.011)	
CLL&Dependence	-.301	-.304	-.248	-.232
	(0.106)***	(0.104)***	(0.096)***	(0.094)**
N	2154	2194	2154	2194
R^2	0.043	0.074	0.427	0.458
Industry Dummies	NO	NO	YES	YES
State Dummies	NO	YES	NO	YES

Note: OLS coefficients from regression 1 in the text. Dependent variable: Annual Compounded growth rate of value added for the period 1910-1920. Robust standard errors in parenthesis. *,**,*** denote 10,5 and 1 percent significance respectively. Unless otherwise specified, all the variables correspond to 1910.

Table C.10: **Other CLL Laws**

	(1)	(2)	(3)	(4)
Ln. Wage Earners	0.016	0.016	0.016	0.016
	(0.006)***	(0.006)***	(0.006)***	(0.005)***
Ln. Real Capital	0.007	0.007	0.007	0.007
	(0.004)	(0.004)	(0.004)	(0.004)
Prop. Manufacturing	-.031	-.031	-.031	-.032
	(0.006)***	(0.006)***	(0.006)***	(0.006)***
CLL Alone	-.130			
	(0.088)			
CLL&Papers		-.188		
		(0.093)**		
CSL Alone			-.157	
			(0.089)*	
CLL				-.247
				(0.095)***
N	2194	2194	2194	2142
R^2	0.457	0.457	0.457	0.463
Sample	ALL	ALL	ALL	But NC & SC

Note: OLS coefficients from regression 1 in the text. Dependent variable: Annual Compounded growth rate of value added for the period 1910-1920. Robust standard errors in parenthesis. All regressions include a full set of year and industry dummies. For the exact definition of each of the laws see the text. *,**,*** denote 10,5 and 1 percent significance respectively. Unless otherwise specified, all the variables correspond to 1910.

Table C.11: **Other CL Dependence Measures**

	(1)	(2)	(3)	(4)
Ln. Wage Earners	0.017	0.013	0.016	0.011
	(0.006)***	(0.006)**	(0.006)***	(0.006)*
Ln. Real Capital	0.007	0.005	0.007	0.006
	(0.005)	(0.005)	(0.004)	(0.005)
Prop. Manufacturing	-.032	-.022	-.031	-.022
	(0.007)***	(0.006)***	(0.006)***	(0.007)***
Children 1880&CLL	-.084	-.106		
	(0.057)	(0.065)*		
Children 1890&CLL			-.156	-.210
			(0.092)*	(0.11)*
N	2057	2057	2194	2194
R^2	0.459		0.457	

Note: Columns 1 and 3 estimated by OLS with robust standard errors. Median regression coefficients with bootstrapped standard errors (200 reps.) reported in columns 2 and 4. Dependent variable: Annual Compounded growth rate of value added for the period 1910-1920. All regressions include a full set of year and industry dummies. *,**,*** denote 10,5 and 1 percent significance respectively. Unless otherwise specified, all the variables correspond to 1910.

Table C.12: **Other Robustness Checks**

	(1)	(2)	(3)	(4)
Ln. Wage Earners	0.016	0.022	0.012	0.012
	(0.006)**	(0.007)***	(0.005)**	(0.004)***
Ln. Real Capital	0.002	0.005	0.006	0.006
	(0.005)	(0.006)	(0.005)	(0.003)**
Prop. Manufacturing	-.023	-.044	-.022	-.023
	(0.007)***	(0.008)***	(0.008)***	(0.004)***
CL Dependence&CLL	-.270	-.314	-.157	-.160
	(0.102)***	(0.138)**	(0.091)*	(0.093)*
N	1609	1434	2194	2194
R^2	0.457	0.474		0.495
Est. Method	OLS	OLS	LAD	Robust
Sample	$CL\ Dep > 0$	$Prop.\ Manuf < 1\%$	$Whole$	$Whole$

Note: Robust standard errors for specifications (1) and (2). Bootstrapped standard errors (20 reps.) in specifications (3). Specification (4) is a regression where observation with large residuals are reweighted in order to attenuate their impact (robust regression, Huber(1964)). Bi-weights set to 8 times the median absolute deviation from the median residual. Dependent Variable: Annual Compounded growth rate of value added for the period 1910-1920. All regressions include a full set of year and industry dummies. *,**,*** denote 10,5 and 1 percent significance respectively. Unless otherwise specified, all the variables correspond to 1910.

Table C.13: **Growth Impact of CLL**

		Annual Growth		**CL Dep.**	**Growth Impact**		
Perc.	Industry	1900-10	1910-20		Low	Med.	High
50	Brass and Bronze	4.30%	6.71%	0.68%	0.07%	0.10%	0.14%
60	Wood, turned and carved	1.91%	-2.29%	1.21%	0.12%	0.18%	0.24%
70	Jewelry	5.63%	0.18%	1.74%	0.17%	0.26%	0.35%
75	Corsets	6.98%	0.55%	2.19%	0.22%	0.33%	0.44%
80	Artificial Flowers	6.25%	1.65%	3.16%	0.32%	0.47%	0.63%
85	Cork Cutting	7.59%	4.34%	4.04%	0.40%	0.61%	0.81%
90	Hat and Cap Materials	8.18%	5.70%	5.05%	0.51%	0.76%	1.01%
95	Printing and Publishing	5.29%	5.06%	6.70%	0.67%	1.01%	1.34%
100	Glass	3.30%	4.08%	12.65%	1.26%	1.90%	2.53%

Note: Growth rates computed as the average of all the growth rates of the industry across the states for the corresponding year. Low, Medium and High correspond to the assumptions about the coefficient of the interaction, that is, -0.1, -0.15 and -0.2 respectively.

D. Data Appendix

D.1. Data Sources for Chapter 1

a. International data on child labor in the glass industry

These data comes from the following sources: Argentina(Dirección General de Estadística Municipal de Buenos Aires (1906)[pp. 176-187]), Japan(Cunningham and Viazzo (1996))[pp. 73-91], United States, 1880(U.S. Census Office (1883)), United States, 1890(U.S. Census Office (1895)), United States, 1900(U.S. Census Office (1902)[pp. 469-483]), France(Republique Française (1873)[pp. xv-xxiv]), Barcelona, Spain(Llop (1999)), Belgium (Cunningham and Viazzo (1996))[pp. 31]), United Kingdom(Census of England and Wales (1903)), Netherlands(Centraal Bureau voor de Statistiek (1902)), Finland(Schybergson (1974)), Norway(Departementet for der Indre (1874)), Sweden (Schybergson (1974)), Russia(Ministère du Commerce et de l'Industrie de l'Empire de Russie (1912)), India(Kitchlu (1996)), Belmont, Co.(Chief State Inspector of Workshops and Factories (1887)), Denmark(Christensen (2002)). For more references on child labor in Europe, refer to the comprehensive study of Marjatta Rahikainen (Rahikainen (2004)). The historical GDP data cames from Angus Maddison. The modern data was retrieved from the World Bank's World Development Indicators (WDI).

b. U.S. Manufacturing Data

The data on industries comes from the Manufacturing censuses carried out in the United States in 1899, 1909 and 1919. The data comes from the individual industry tables presented under the section "General Tables" in each census. I collect all the available data for all individual states and industries in each census date.[1] I discard any industry that is not present in a given state in all three census dates. This typically involves industrial classifications that changed during the period or industries that are too marginal in a given state at the beginning of the period. Attrition is generally not an issue, because the tendency is for the report to include more states, rather than less, over time. I also discard industries for which the classification was altered during the period or suffer shocks clearly unrelated with child labor legislation (e.g. "carriages and wagons" and "automobiles"). Finally, I exclude industries that are present in only 1 or 2 states. The reason for this is that my identification strategy exploits comparisons of treated and untreated industries across states. Naturally, that comparison only makes sense if the industry is present in at least few states. In addition, observations corresponding to industries that are presents in few states have extremely high weight in the OLS estimator

[1] I ignored from my analysis the residual category, "All other states" as it is impossible to track it to individual states.

(leverage). In such circumstances, it is often advised to exclude the observations.

This leaves me with 2,195 industry-state pairs for each census date. The amount of observations in the restricted seems to give slightly higher weight to the very industrial U.S. north-east and less weight to western and southern states. However, a chi-square test of the state frequencies of the sample included and excluded is not significant at 5 percent level. In the main specification all the variables correspond to 1910 except for the growth rate of real value added and state investment. The former corresponds to the period 1910 and 1920 while the latter refers to the period 1900-1910.

In what follows I provide a short description of the variables used in the analysis that required some specific transformation.

- Industrial Real Value Added Growth: Constructed as the log difference of the real value added. Nominal value added deflacted with the wholesale price index (1926=100) from the U.S. Millenial Statistics (Carter, Sigmund Gartner, and Haines (2005)).

- Child Labor dependence: Constructed as the median value of the proportion of children employed in a given industry in 1900. This proportion is calculated as the number of child workers over total wage earners. In 1900 corresponds to the year average number of men, women and children.

- Child Labor Laws: As explained in the text and due to difficulties with the definition of child labor legislation, I have chosen a composite definition of child labor law. A state is considered to have a child labor law in 1910 if it has a CLL (Fishback (2008)), a CSL (Fishback (2008)) and a Factory Inspection Law (Fishback, Holmes, and Allen (2008)).

- Real Capital Growth: Constructed as the log difference of the real capital. Nominal value added deflacted with the wholesale price index (1926=100) from the U.S. Millenial Statistics

- Log. of the Proportion of industry's participation in state Value Added$_{j,s}$: Computed as the log of value added of industry j divided by the total state value added of state s.

- State investment 1900-1910: Computed as the log difference of the real capital all the industries operating in state s during the period 1900 and 1910.

- Child labor data for 1880, 1890, 1900: U.S. Census Office (1902)[Table 1].

- Child labor law index: Constructed as an average of the intensity of child labor legislation in different dimensions like, for example, occupations covered, age limits, minimum age exemptions, maximum hours, exemptions to maximum hours, educational requirements, proof of age and working papers. All of these dimensions are taken from Ogburn (1968).

- State Income: Comes from Easterlin (1957). They do not report income data for the District of Columbia
- Patents: Comes from Johnson (2002). Patent data is unavailable before 1912 (statehood year) for Arizona and New Mexico.

c. Glass Industry Data

Glass industry data comes from the reports on the glass industry from the U.S. Census of Manufactures. I also used the data on glass companies from Lamoreaux and Sokoloff (2000). Finally, the series for value added of the glass industry in New Jersey was comes from the annual reports of the Bureau of Labor and Industries Bureau of Statistics of Labor and Industries of New Jersey (1905) from 1899 to 1919.

D.2. Data Sources for Chapter 2 and 3

Unless otherwise specified, the remarks on the construction of the samples applies to both the education and the fertility papers, which were developed together. Unless I explicitly indicate it, all the data sources refer to both papers.

a. Geographical Samples

My identification strategy in both papers relies on the definition of the relevant sample according to a geographical criterion. Throughout the chapter 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 chapter.

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. Bureau of the Census (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.

Geographical Samples - SEA

In the long-run analsis of education I focus on people living on the 1910 CSL border using the 1940 census. Since the 1940 census does not contain county or township information I had to rely on State Economic Areas. I used the 43 SEAs that correspond to the 1910 border. I had to exclude the SEAs between the border of Arkansas and Louisiana. The reason for this exclusion is that Arkansas just passed the CSL in 1909, just one year before Louisiana. It is impossible then to identify the effect of the legislation in this part of the border. For the rest of the border, the average difference is about 11 years between the adoption of the CSL on one side of the border and the opposite. The final sample includes 43 SEAs. The complete list of included SEAs is SEA-001, SEA-002, SEA-027, SEA-029, SEA-071, SEA-073, SEA-222, SEA-223, SEA-226, SEA-275, SEA-276, SEA-294, SEA-302, SEA-303, SEA-308, SEA-310, SEA-340, SEA-344, SEA-348, SEA-349, SEA-384, SEA-385, SEA-386, SEA-388, SEA-

390, SEA-391, SEA-402, SEA-403, SEA-404, SEA-405, SEA-407, SEA-409, SEA-410, SEA-411,SEA-412, SEA-415, SEA-416, SEA-420, SEA-421, SEA-422, SEA-425, SEA-426, SEA-429. For the exact definition of which counties each of these includes, refer to IPUMS (http://usa.ipums.org/usa/volii/seacodes.shtml). Similar to what I did in the main section of the chapter, I have excluded the metropolitan areas on the border. In this case, I excluded Memphis(TN), Chattanooga (TN) and Charlotte (NC).

b. Variable Definition

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): Number of children under 5 years living with their mothers at census day (IPUMS constructed)

- Surviving Children: Number of 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 censuses.

- Children Ever Born: Number of children ever born to ever-married women (1910) or women in general (1900). This variable is in the census schedule and it is only available for women born in 1900 and 1910.

- 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, say, 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.

Other Controls

Most of the variables used in the chapter are self-explanatory (age, race and foreign status). The cases in which the construction is not straightforward are explained below.

- **School Attendance**: Dummy variable equal to 1 if the child is attending school.
- **Educational Attainment**: Number of years of education achieved, IPUMS, 1940 Census only.
- **Urban Status**: Dummy variable equal to 1 if the location in which the person resides is a incorporated town.

c. Fall River Sample

This section briefly explain the procedure to determine whether a person lived in the treated or the control part of Fall River. I just used the street names provided in the census microdata and matched them with a contemporaneous map of Fall River. Since enumeration districts cover contiguous areas, I attributed enumeration district to Massachusetts or Rhode Island depending on whether the streets were north or south from Columbia Street. In four cases (out of 22), there were streets that run through both states within a single enumeration district. In those cases I attributed the enumeration district to the state with most streets within the enumeration district. In any case, I also constructed a parsimonious treatment variable in which I ignored the enumeration districts that could not be exclusively attributed to a state. Finally, in my data enumeration districts 83-88, 91, 97 and 98 are unambiguously assigned to South Fall River (RI). Enumeration districts 89-90 and 92-93 are potentially mixed and dropped from the baseline estimation. The rest of the enumeration districts, i.e 94-96, 100-105, are unambiguously assigned to North Fall River (MA).

d. Methodological Issues with the Fertility Measure

In this section I discuss a series of methodological issues related the construction of fertility variables. In various specifications, I have constructed a time-series of births for each mother taking into account the last 15 years of their birth histories. That is, if I observed a woman in the 1870 census living with three children aged 2, 4 and 6, I assume I could construct a series of births by year that would have zeroes in every year from 1856 until 1870, with the exception of 1864, 1866 and 1868. I argued that there are 2 sorts of bias one could imagine with this data. First, child mortality will make us underestimate actual fertility. Also, children above age 14 are most likely to live outside their parents' home. That was the rationale for cutting the birth histories about 14 years before census date. I further argued that since we are comparing the CSL and no-CSL borders, so long as we could argue that these biases were not different in either side of the border, they did not invalidate our identification strategy. In this section I provide direct evidence on the extent of the difference in number of births and children present at the house. Furthermore, I am able to verify that this difference behaves similarly in CSL and no-CSL states. The way I do that is using the observations from the 1900 U.S. Federal 5% sample from IPUMS. The 1900 Census is the only early census that asked women explicitly about the number of births. Like in any other IPUMS sample, the number of own-children living in the household is available.[2]

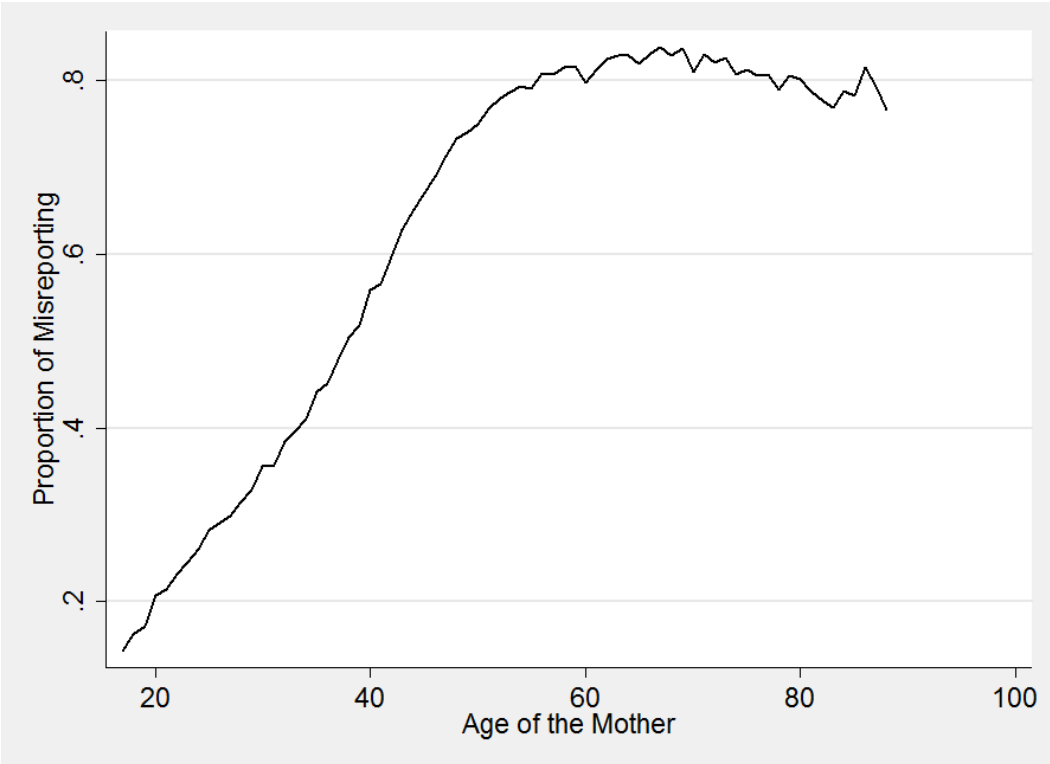
I define a dummy variable that assigns 1 if the number of children living in the household is different that the number of children the mother declared to have given birth to. I then compute an average by age of the mother at the moment of the census. In figure D.1, I plot the proportion of misreporting as a function of mothers age. I have excluded some extreme ages for which there are few data points and the mean is noisily estimated.

In about 20% of the cases, the reported number differs from the counted number for young mothers. This is mostly attributable to child mortality or problems in the imputation procedure of *nchild*. As expected this figures increases almost at a constant rate of about 1.5% per year with mothers age, at least until age 60. For mothers aged 60 and more, only 1 out of 5 has living at home all the children she has given birth to. In most of my analysis, I have to compare the fertility of groups of women of different age. However, this does not introduce a bias because the difference-in-difference nature of the estimation nets out group means. This approach is valid so long as we keep the discussion on *differences* rather than *absolute values*. If one intends to make an argument about the absolute values of fertility, the *nchild* variable should need to be adjusted to take into consideration that mismatching is a function of mother's age.

Next, I turn to the question of whether the measurement error was different in CSL and no-CSL areas. Assume that Non CSL areas were more backwards, then they would

[2]The rules on how children are assigned to an adult are based on ages of the parents and age differences with the mother. For the details see, http://usa.ipums.org/usa/chapter5/chapter5.shtml.

Figure D.1: Difference Number of Children and Children Ever Born



have higher child mortality that CSL areas, biasing fertility downwards. As usual, so long as these differences in child mortality are not correlated with the border side, the identification strategy is reliable. I investigate any differences in the extent of error according to CSL status. In figure D.2 I present the extent of the difference in the number of children and the number of children ever born at each side of the border. It is apparent that the picture is similar to the aggregate one presented before. Note, however, that I had to restrict the extreme ages even more due to lack of observations. The difference between the extent of measurement error does not seem to depend on whether a person lives in the CSL or the No CSL side of the border. Furthermore, I find that you it cannot be rejected the hypothesis that the difference is white noise (Q-test= 26.2, p-value: 0.56). All of these leads me to conclude that the pattern of mismatch between the observed number of children and the actual number of births is not systematically different in either side of the border. Therefore, using *nchild* seems not to introduce bias in the difference-in-differences estimation.

Figure D.2: Measurement Error: CSL and No CSL