The Effect of Child Labor Restrictions on Fertility: Evidence From the Early 20th Century

Yannay Shanan

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Tel Aviv University

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Abstract

This chapter investigates whether child labor restrictions imposed by compulsory schooling laws and child labor regulation have an impact on fertility. Exploiting variation induced by changes in legislation across time and between states during the early 20th century U.S, I find that parents chose to have less children as a response to the constraints imposed on the labor supply of their prospective children, and that the largest response was among parents whose children were more likely to work. I address possible threats to the validity of the identification strategy, and show that the results are robust to various robustness checks. My results suggest that legislation aimed at increasing children's educational attainment and decreasing the prevalence of child labor has spillover effects on parents' fertility, providing additional empirical support to the notion that financial incentives play a role in determining household fertility decisions.

1 Introduction

During the 19th and early 20th centuries, the United States has experienced a sharp reduction in the prevalence of child labor alongside a large decline in fertility. Child labor was almost completely eradicated by the mid 20th century, while the fertility rate of women reaching childbearing age has dropped from an average of around seven live births during the early 19th century to less than three children by 1930 (Bailey and Hershbein, 2015). The literature concerned with investigating this demographic transition and the decline in child labor lists a range of plausible forces driving these changes. Among them are industrialization, urbanization, rising educational attainment and life expectancy, skill-biased technological change, an increase in the relative wages of women, and a decline in the demand and value of child labor. These forces tended to raise incomes and increase the implicit cost of having and raising children. In addition, this period in US history was characterized by a legislative expansion of compulsory school attendance laws and child labor regulation. Previous research has suggested that the introduction of compulsory school attendance laws and child labor regulation had an impact on the educational attainment and labor supply of children in the U.S, and consequently affected their later life outcomes. Lleras-Muney (2002) as well as Goldin and Katz (2011) show that compulsory school attendance and child labor laws contributed to the large increase in secondary schooling in the United States between 1910 and 1940. In a recent working paper by Clay et al. (2016), U.S state schooling laws introduced after 1880 are found to have increased attendance, enrollment, and educational attainment of children first exposed to compulsory schooling. Others have used the introduction of these laws or the changes in the requirements set by these laws over time as an instrument for schooling in order to estimate the returns to schooling on various outcomes. Prominent examples include the effect of schooling on wages (Acemoglu and Angrist, 2000; Oreopoulos et al., 2006; Clay et al., 2016), crime (Lochner and Moretti, 2004), fertility (Black et al., 2008), and mortality (Lleras-Muney, 2005).

In this chapter, I investigate the effect of the enactment of these laws and the alternating extant of child labor restrictions imposed by them, on household fertility decisions by exploiting the variation in compulsory schooling laws and child labor regulation across states and over time. Others, such as Black et al. (2008), have previously analyzed the impact of these laws on the fertility of women exposed to these laws during their childhood, in order to estimate the causal effect of schooling on teenage fertility. To my knowledge, my study is the first to analyze the impact of these laws on the fertility of young adult women who are already out of school. I find that compulsory attendance and child labor laws had a significant negative effect on the fertility of women in their thirties, consequently contributing to the overall decline in fertility during this period. I argue that because the labor supply of children was restricted by these laws, their "quantity" price increased and induced parents to have fewer children.

This study broadly relates to three strands of the literature. One is the macroeconomic growth theory literature that examines the role of factors such as child labor, fertility and human capital accumulation in the transition from stagnation to growth. Second is the literature which focuses on the role of social legislation in the U.S. Third is the empirical literature concerned with estimating the magnitude of the impact of different economic constraints and financial incentives on fertility. I show that compulsory schooling attendance laws and child labor regulation have contributed to the fertility decline in the United States at the dawn of 20th century, possibly hastening the demographic transition, and provide empirical evidence for the economic theory that posits that child labor restrictions should reduce fertility in an economy in which children are part of the labor force. My findings suggest that parents internalized the implicit costs of raising another child due to the labor market restrictions imposed on their prospective children, and chose to have smaller families.

The remainder of the chapter is organized as follows. Section 2 provides institutional background and discusses the theoretical channels through which restrictions on child labor may affect the price of children and in turn affect fertility. Section 3 describes the data and the samples used throughout this study. Section 4 discusses the identification strategy and presents the econometric model. In section 5 I show how child labor restrictions affected the time allocation of school age children. Section 6 shows and discusses the main results. Section 7 addresses possible threats to the validity of my results. In section 8, I shed some light on the mechanisms underlying my findings, look at heterogeneous effects, and perform additional robustness checks. Section 9 concludes.

2 Background

2.1 Theoretical background

Following Gary Becker's seminal work on the economics of the family, financial considerations are regarded as a key component in explaining parental fertility decisions. Thinking of children as a durable normal good, an increase in the explicit or implicit price of children should lower the optimal number of children one would like to have. Studies which examine the effect of considerable changes in the costs of childbearing on fertility using individual level data indicate that fertility indeed responds to financial incentives. Milligan (2005) finds that the introduction of a pronatalist tax policy had a substantial impact on the fertility of women in Quebec. Cohen et al. (2013) examine the impact of a sharp reduction of Israeli child subsidies and find it negativity affected fertility, including that of the ultra-orthodox, whose norms generally discourage family planning. Ebenstein et al. (2015) demonstrate how the privatization of the Israeli kibbutzim during the mid-1990s, which significantly increased the costs of childbearing among its members, had led to a large fertility decline among kibbutz members. Consistent with the idea that parents view children as a form of investment, Qian (2008) finds that an increase in the relative income of women in rural China, induced by exogenous shocks to the price of tea (women have a comparative advantage in producing tea compared to other cash crops), had a positive effect on the survival of girls, consequently influencing the country's sex imbalance.

The specific role of compulsory schooling, child labor and child labor restrictions in fertility decisions has also been examined by economists. Two key assumptions underlie many of the theoretical models that address child labor: the luxury axiom and the substitution axiom (Basu and Van, 1998). The luxury axiom treats child non-work as a luxury good, suggesting that parents send their children to work only when driven by poverty. This axiom goes hand in hand with an altruistic household head decision problem, where child labor is not simply a result of disregard to children's welfare. In section 8, I provide some empirical evidence which suggests that the probability a child participates in the labor force is significantly lower in wealthier households; strengthening the notion that parents withdraw their children from the labor force when they could afford to do so. The substitution axiom presumes that adult and child labor are substitutes to some extent, thus allowing household income to be affected not only through the earnings from child labor but indirectly through its affect on adult wages. Accordingly, a child labor ban will not necessarily have a negative income effect. A prominent example in which this axiom plays a central role is a model by Doepke and Zilibotti (2005), in which a child labor ban is endogenously determined. In this model, a child labor ban is supported by workers who compete with children in the labor market, unless their own working children provide a large enough fraction of family income.

Theoretical models based on the above assumptions tend to reach the conclusion that absolute or marginal child labor bans (i.e legislation forbidding child labor altogether or partly restricting the labor of children of certain ages, or in certain occupations) should drive child labor downwards and have an impact on the fertility choices of households. In a model that focuses on the role of technological progress in the dynamics of child labor and fertility, Hazan and Berdugo (2002) suggest the possibility of multiple equilibria, where certain populations might find themselves in a child-labor poverty trap. They too assume that parents care about their children's welfare (future earnings) and show that in a dynamic process, an increase in the wage differential between parental and child labor due to technological progress, decreases fertility and child labor and increases children's human capital which in turn further increases the wage differential in the next period. As a result, given that the economy in not trapped at a low steady state equilibrium (i.e. when parental human capital is below a certain threshold), families become smaller and more educated with time. According to the model, child labor regulation should expedite the transition process and generate a pareto dominating outcome. In a closely related paper, Doepke (2004) develops a model which examines to what extent do government policies that affect the opportunity costs of education (such as child labor laws) can account for cross-country differences in the demographic transition. By simulating the calibrated model under different policy regimes, Doepke further argues that child labor restrictions have had an actual impact on the timing of the fertility transition in different countries. Another relevant theoretical model is an extension of the Becker and Lewis (1973) model to incorporate child labor, by Fan (2004). Fan shows that when the role of child labor is taken in to account, the theoretical negative correlation between fertility and income can be obtained independently of the elasticity of substitution between the quantity and quality of children. According to the extended model, fertility declines and children's education increases with parental income when the earnings of children are sufficiently low relative to their cost. Child labor regulation should therefore not only directly affect the supply of child labor, but also influence parents decisions on fertility. Implementation of laws that punish or ban child labor reduces the returns to child labor, making the quantity of children more expensive.

2.2 Institutional background

Massachusetts was the first state to enact a compulsory school attendance law (CSL henceforth) in 1852. Following the example of Massachusetts, states gradually enacted similar laws, and by 1920 all states had a law in place which specified an entry age by which a child is required to attend school and a dropout age at which a child can choose to unconditionally stop attending school. Figure 1 shows when states first introduced compulsory attendance laws. Newly state legislated laws often used ages eight and fourteen as the entry and exit ages, respectively, and these ages were lowered and raised over time. There were two types of exceptions that allowed children to stop attending school before the exit age found in the compulsory attendance law: the completion of a specified number of schooling years, or a requirement of secured employment, and a specified minimum age in addition to the completion of a specified number of schooling years.

Compulsory attendance laws were often complemented with child labor regulation (CLR henceforth) that allowed children to stop attending school before the official dropout age.

These labor laws let employed children stop attending school before the exit age found in the compulsory attendance law, usually after the child had attained a certain level of education and reached a certain age, as in the exemptions found in compulsory school attendance laws. These ages were also lowered and raised over time, yet much like CSL, the overall trend was increasing the required years of schooling and the work permit age over time. Some states have also legislated continuation school laws, which required children at work to continue their education on a part-time basis. Like the CSL and CLR laws, these laws specified attendance until a certain age and allowed exemption for children who completed a certain number of schooling years. [1] The different laws were in many cases not entirely coordinated in the sense that the requirements specified by the compulsory schooling law differed from the ones specified by the child labor laws. For example, in 1920 Illinois, children had to enter school at the age of 7 and could stop attending at 16 according to the compulsory schooling laws. However, they were eligible for a work permit at the age of 14, given they have completed five years of schooling.

Based on the different ages and exemptions specified in the CSL and CLR, I calculate the effective exit age in which a child could stop attending school and work full time in each state for the years 1880-1930 (see more details in section 3). I focus on the exit age as opposed to other aspects of the legislation such as the school entry age or the the total years of compulsory schooling, because I believe the age a child is allowed to leave school and work full time is the most likely candidate to successfully capture the impact of the restrictions imposed on the labor supply of children. Figure 2 shows these exit ages in each state and for each decennial year. It is worth noting that there was also federal legislation targeting child labor during this period. In 1918 and 1922 the U.S congress passed two laws restricting child labor that were soon declared unconstitutional by the supreme court. In 1924, congress proposed a constitutional amendment prohibiting child labor, yet it has not been ratified by the requisite three-fourths of the states. Only in the Fair Labor Standard Act of 1938 child labor was restricted at the national level.

3 Data

The data on compulsory attendance and child labor laws was collected from multiple sources by Clay et al. (2016).[2] These data builds on previous work by Lleras-Muney (2002) and Goldin and Katz (2011) by expanding it to the pre-1910 period, and incorporating the

[1] In my analysis, I ignore the legislation regarding continuation schools both for the sake of simplicity and due to the fact that the focus of this research is the impact of child labor restrictions and not of educational attainment.

[2] Karen Clay, Jeff Lingwall and Melvin Stephens Jr were kind enough to share this data with me.

improved coding of the laws for the post-1910 period by Stephens and Yang (2014) (see Clay et al. (2016) for more derails). It includes yearly information on the ages specified in compulsory school attendance and child labor laws, as well as information on the exemptions requirements mentioned above, for each state from 1880 to 1930.

The data on CSL and CLR is matched to four waves of census data, from the Integrated Public Use Microdata Series (IPUMS) 1900, 1910, 1920 and 1930 US Censuses of Population samples (Ruggles et al., 2015). The census provides information on sex, race, age, number of children in the household and their ages, farm and home ownership status, state of residence, state of birth, and marital status. For my main analysis, I restrict the sample to all white non-hispanic native born married women aged 30-40 at the time of census, residing in the continental United States. I specifically focus on women and not men both for the sake of reliably estimating fertility using the census data, and in order to capture the effect of exposure to child labor restrictions on fertility within a bounded biological reproductive window. To avoid confounding the estimates with the effects of migration, the sample is further restricted to individuals who were born in the same state they reside in at the time of the census, so that only individuals who may have been affected by the legislation are considered.[3] In some specifications I also use the 1940 full count census and the 1880 census of population sample. The main 1900-1930 sample consists of 359,317 observations.

In a similar fashion to Aaronson et al. (2014) who examine the impact of a large U.S construction program during the early 20th century on the fertility of black women, I construct exposure measures aimed at capturing the labor market restrictions women of reproductive age might expect for their children based on the laws in place at their state of residence. These exposure measures are based on whether there was a law of any type (either CSL or CLR) restricting child labor when these women were between the age of twenty and thirty, and on the average effective school exit age during this ten year interval. The effective exit age is defined as the earliest age a child is allowed to exit school and work full time, given the ages specified by the state level legislation at the time. When there are no restrictions, the effective exit age is set to zero. In cases where a legislation of either CSL or CLR included an exemption based on an age limit and a specified number of completed schooling years, I assume children start school at the age required by law, and calculate the exit age as the school entry age plus the years of schooling required to qualify for exit.[4] I then define the minimum between the base exit age, and the entry age plus the schooling requirement for

[3]Ideally, I would have restricted the sample to individuals who resided in their current state during their 20's. However, the census data for these years contains no information on the timing of migration from one state to another. I discuss this issue in more detail in section 8.3.

[4]For a few states that set a literacy requirement instead of a minimum number of schooling years as a requirement for exemption, I treat the literacy requirement as a four years of schooling requirement.

exemption, as the CSL exit age. For example, if the CSL entry age is eight, the CSL exit age is sixteen, and children who are at least fourteen and have completed seven years of schooling are allowed to leave school before the official CSL exit age, I set the CSL-based exit age as fifteen. Similarly, I compute the effective CLR-based exit age, and define the minimum of the two as the effective school exit age.

Using the 1900-1930 census data, I construct flow measures of fertility based on the ages of children living with their mothers at time of the census.[5] To avoid downward bias due to children not being present in the household, I follow Aaronson et al. (2014) and limit the fertility horizon to the last ten years, constructing the fertility measures based only on the ages of children in the household who are at most ten years of age at the time of the census. One measure is the number of children born during the last ten years (Nchild10) and the other is an indicator for whether a woman gave birth during the last ten years (Birth10). Note that this strategy is likely to significantly reduce the downward bias in fertility but it does not guarantee it will be completely eliminated. However, there is no clear reason for the bias to be different between women who were exposed to different levels of child labor restrictions. If the identifying strategy is valid, this can only affect the efficiency (i.e increased s.e) but not the consistency of the estimated effects. The same argument can be made in regard to infant mortality. By using the ages of children living with their mother, I essentially count the number of surviving children up to a certain age, while the number of children ever born to each woman might be different.[6] I check if there is any evidence for differential mortality using 1926-1930 state level infant mortality rates from the 1931 Census of Births, Stillbirths, and Infant Mortality, and find that there is virtually no association between infant mortality rates among whites and school exit ages (ρ = -0.089).

The summary statistics for the estimating set of variables used in my main specifications are presented in table 1. 71.4% of the women in the sample gave birth to at least one child in the ten years preceding the census, while women overall had 1.7 children on average during this period. 83.4% of the women were also exposed to laws restricting child labor in their twenties; laws that allowed children to leave school and work when they were, on average, 11 years of age. Over this 30 year period sample, the literacy rate has gone up, nearing 100%, and as the nation urbanized, the fraction of farm households fell under 30%. By 1930, most of the women in the sample were living in cities, had significantly less children than their parents, and were all exposed to laws restricting child labor after they turned twenty.

[5] In the 1920 and 1930 censuses women were not asked about the total number of children they ever gave birth to.

[6] In the 1900 and 1910 Censuses where women were asked about children ever born, this number coincides with the number of surviving children in 91% of the cases.

4 Identification strategy

The causal effect of child labor restrictions on fertility cannot be simply estimated by comparing the fertility of women residing in states with and without laws restricting child labor, nor by comparing fertility in a certain state before and after such laws were enacted. Women in states that restricted child labor earlier may have lower fertility rates due to other reasons that are correlated with an earlier adoption of such laws, while women born in later cohorts in a specific state are almost certain to have lower fertility regardless of the legislation, given the downwards trend in fertility during this period in U.S history. To address this endogeneity concern, in an attempt to overcome the fact that fertility is likely correlated with unobserved determinants of compulsory schooling and child labor legislation, I take advantage of the variation in the restrictions imposed by state level legislation across time and between states in order to estimate the causal effect of child labor restrictions on fertility. I compare the fertility of women who were exposed to different child labor restrictions during their prime fertility years, by using the within state variation in exposure to the laws, while controlling for any regional time effects in fertility . Specifically, I estimate the following model:

$$Fertility_{ijts} = \alpha + \beta Exposure_{jts} + X'_{ijts}\phi + \gamma_{jt} + \delta_{tr} + \mu_s + \varepsilon_{ijst} \quad (1)$$

Where $Fertility_{ijts}$ is the fertility outcome of woman i, aged j, in year t, residing in state s; $Exposure_{jtsr}$ is the intervention exposure measure of women aged j, in year t, and state s; X_{ijts} is a vector of individual and household level characteristics; γ_{jt} are age-year fixed effects; δ_{tr} are region-year fixed effects; μ_s are state fixed effects; and ε_{ijst} are random error terms. I use two different law exposure measures. The exposure variable is defined either as (a) a dummy stating whether a law was in effect when a woman was between the ages of 20 and 30 ("Any legislation"); or as (b) the ten year average of the effective exit age during this ten year interval ("School exit age"). Individual and household level characteristic include a set of dummy variables for literacy, urban area, household farm status, and home ownership. Year interacted with census region fixed effects are included in order to capture both the downward regional level trends in fertility, as well as the effect of any nation-wide changes in fertility during this period, caused by events such as the federal legislation regarding child labor during the late 1910's, and the first world war. I use separate time effects for each of the four U.S census regions given a recent study by Stephens and Yang (2014) who show how the main findings of the literature that exploits the change in U.S. state schooling laws to infer the causal impact of extra schooling on various outcomes tend to turn insignificant and, in many instances, "wrong-signed", when controlling for differential time trends by region.

The sensitivity of the results to region-specific time effects relates to the ”common trends” assumption underlying these models, which assumes that all other changes which occur across states are uncorrelated with the law changes, educational improvements, and the outcomes under investigation. When these interacted fixed effects are included, the counterfactual is that the outcome would have been the same as other states within the same region, if not for those laws. Separate age fixed effects for each census wave are included in order to account for differential effects by age. State of residence fixed effects are included in order to account for any constant differences between states. The errors are clustered at the state of residence level. The crucial underlying assumption behind this identification strategy is that there are no confounding state specific time trends in fertility, correlated with the introduction of the laws and the changes in restrictions imposed by them over time. In section 7, I explicitly examine this assumption by running placebo regressions that test for an effect of future child labor restrictions on current fertility outcomes, and by estimating a model which predicts the probability a state will pass a compulsory schooling law as a function of state-specific total fertility rates.

5 Child labor

Prior to the estimation of the impact of the exposure to compulsory schooling and child labor legislation on the fertility of women of childbearing age, I estimate the legislation's impact on child labor. The effectiveness of CSL and CLR in limiting child labor is to some extent a prerequisite for parents to incorporate these restrictions into their fertility considerations. For if the restrictions on children labor supply are not binding, or simply not enforced, it is unlikely parents will take them into account when deciding on family size. There is mixed evidence regarding the effectiveness of these laws in restricting child labor. While Moehling (1999) concluded that state level laws have contributed little to the decline in child labor during the investigated time period, a later study by Manacorda (2006) found these laws were actually effective in limiting the labor supply of children. Notable differences in methodology and data possibly account for these contradicting results. Moehling focuses on the role of minimum age limits of manufacturing employment in restricting the labor of 14 year old children in manufacturing establishments, using the 1880, 1900 and 1910 censuses. She employs a difference-in-differences-in-differences methodology, comparing the occupation rates of 13 year-old children to that of 14 year-old children in states with and without minimum age limits of 14 for manufacturing employment, before and after states enacted such laws, and finds the triple difference is insignificant. Manacorda on the other hand, looks at the employment of children age 10 to 16 living in urban areas, using the

1920 census and a different laws database which isn't limited to manufacturing employment restrictions. He finds that the probability a child is employed significantly increases if a child's age is at least equal to the minimum working age in his state of residence, conditional on age and state fixed effects.

For the purpose of establishing this finding using the data and methods used throughout this study, I construct a sample of white native born children aged 10 to 15, using the 1900-1930 census samples. Descriptive statistics for this sample are presented in table 2. Note that while vast majority of the children in the sample are in school, and only 9% are employed, both employment and school attendance change considerably with age (see figure 3). I match each child in the sample to the laws in effect at the time of census according to his age and state of residence, and estimate the impact the laws on the probability to be (a) employed or (b) in school, using the following specification:

$$Outcome_{ijts} = \alpha + \beta CLR_{jts} + X'_{ijts}\phi + \gamma_{jt} + \delta_{tr} + \mu_s + \varepsilon_{ijst} \tag{2}$$

Where $Outcome_{ijts}$ is outcome of child i, aged j, in year t, residing in state s; CLR_{jts} is an indicator stating whether children aged j in year t and state s are required to attend school and or prohibited from working according to the CSL and CLR laws in place at the time ; X_{ijts} is a vector of individual and household level characteristics composed of a set of dummy variables for the sex of the child, urban area, household farm status, and home ownership; γ_{jt} are age-year fixed effects; δ_{tr} are region year fixed effects; μ_s are state fixed effects; and ε_{ijst} are random error terms, which are allowed to be correlated within states over time. The results are presented in table 3, and suggest that these laws were indeed effective in restricting child labor. A state law restricting child labor by requiring school attendance or a work permit decreases the probability a child will be employed and increases the probability he will be in school by approximately five percentage points. Both of these estimates are very similar in magnitude to that estimated by Manacorda (2006).

6 Fertility

In table 4 I report the estimated effect of any exposure to the laws on the number of children born during the last ten years ("Nchild10") and on the probability to have at least one child during the last ten years ("Birth10"). The results indicate that a law restricting child labor led to a 0.101 (s.e = 0.027) percentage points decrease in fertility, and a 0.025 (s.e = 0.005) percentage points reduction in the probability of at least one child. Taking into account the sample means, this translates to a 12% decrease in fertility and a 5% decrease in the probability of having at least one child in the ten years preceding the census. These effects are statistically and quantitatively significant, and consistent with the hypothesis that restrictions on child labor induced parents to decrease their number of children. The additional covariants have the expected sign: illiterate women are likelier to give birth, and have more children than literate mothers; home ownership as well as not residing in a farm, or in a rural area, are associated with lower fertility. In table 5, I report the effect of the average school exit age on the two fertility outcomes. An additional required year in school and out of the labor force has an effect of 0.008 (s.e = 0.003) percentage points decrease in the number of children born during the last ten years. Accordingly, a shift from a regime with no child labor restrictions to a school exit at the age of fourteen (i.e the mode effective exit age in the sample), is associated with a 9% decrease in fertility. The results in column (2) further suggest that an additional year in school is associated with a 0.003 (s.e = 0.001) percentage points decrease along the extensive margin of fertility.

7 Threats to validity

While the fixed effects framework is designed to address the possible endogeneity of laws, identifying the impact of child labor restrictions using changes within a state over time compared to other states in the same region, it might still be that the effect is not causal. Imagine for example that states legislated laws which merely reflected existing fertility preferences, causality could run from fertility to the laws and not vice-versa. I run a placebo test originally suggested in the context of compulsory schooling and child labor laws by Landes and Solmon (1972), and later motivated similar exercises by Lleras-Muney (2002), and Black et al. (2008), to test for the exogeneity of the laws. The test is based on the idea that if the laws are exogenous to fertility, then child labor restrictions in a given year should not explain the fertility levels of earlier cohorts. In other words, if the laws reflected pre-legislation attitudes regarding fertility, then exposure to laws in the future should also yield negative significant coefficients. Using the 1880 population census, before most states enacted laws restricting child labor, I match 30 to 40 year old women to the laws in place when they were 40 to 50 year old, and therefore could not have been affected by them. I then estimate the effect of the future legislation on the fertility outcomes of these women, using the following model[7]:

$$Fertility_{ijs} = \alpha + \beta Future\ Exposure_{ijs} + X'_{ijs}\phi + \gamma_j + \mu_s + \varepsilon_{ijs} \tag{3}$$

[7] The specification differs from the baseline model in two regards: because there is no data on home ownership in the 1880 I do not control for it, and because the estimation is based on a single census year, there is no need to control for any time effects. Variation in exposure within a state is driven solely by age.

The results are presented in table 6 and provide little evidence that long running trends in fertility are confounding my results.

In addition, to check for endogeneity due to changes in the local demand or supply of child labor, as previously discussed and formalized by Doepke and Zilibotti (2005), I estimate a simple linear probability model to test how state characteristics affect the probably that a given state will pass a compulsory schooling law. According to Doepke and Zilibotti's model, a majority supporting a child labor ban will be formed only when the gains of low skilled workers from eliminating competing child labor exceeds the gains from the income these workers children generate in the labor force. The model therefore predicts that fertility should drop prior to the enactment of a law restricting child labor.[8] While a low fertility rate prior to the introduction of laws does not affect the validity of my identification strategy (I control for state fertility levels by including state fixed effects), differential trends in fertility in states that adopted such laws earlier is a violation of the common trends assumption. To test this, I construct a state-year level panel data, based on the 1850, 1860, 1870, 1880, 1900, 1910, 1920 and 1930 US Censuses of Population samples, and merge it with data on the year of enactment of the first compulsory schooling law in each state. [9] I keep all state level observations up to the year the law was enacted, and estimate the probability a CSL will be enacted (rounded to the nearest decade) as a function of the total fertility rate (TFR)[10] ten years earlier, conditional on time-region and state fixed effects:

$$Pr(Law\ Is\ Passed)_{st} = \alpha + \beta TFR_{s,t-10} + \delta_{tr} + \mu_s + \varepsilon_{st} \tag{4}$$

The estimates are presented in table 7. State level changes in fertility rates have no significant impact on the probability a state will adopt a compulsory schooling law within the following decade, further suggesting that there are no state specific trends in fertility associated with the enactment of the laws. This result is robust to the inclusion of additional controls (share of the population residing in farms, share residing in urban areas, percent female, share of men working in manufacturing, and literacy rates) and the addition of TFR in period $t-20$ (see columns 2 and 3) . As a supplementary robustness check, I also estimate a parallel set of models without restricting the sample up to the year a law was enacted in each state, and again find insignificant effects (see columns 4-6).

[8]Family size ought to decrease to a point where the combined income of the children in each household is offset by the increase in parental income when children are banned from taking part in the labor force.

[9]Legislation data is available for the years 1880-1920, so all states that first enacted a law prior to 1880 are excluded from the analysis.

[10]I calculate the total fertility rate of each state and census year by summing the age-specific fertility rates of white married women ages 20-24, 25-29, 30-34, 35-39, 40-44, and 45-49. The data for 1880 is linearly interpolated.

Last, I check whether my results are not driven by the fact that a substantial fraction of the women in my sample might have been exposed to child labor restrictions when they were children themselves. We know from previous research that compulsory schooling and child labor regulations led to an increase in the educational attainment of the children who were young enough when these laws were enacted. The increase in human capital could have raised the opportunity cost of these women and alter their fertility preferences. To formally test this possibility, I re-estimate my main models, using a sub-sample of the main 1900-1930 sample for which I have complete data on exposure to CSL and CLR during childhood. I control for the effect of the legislation during childhood in two different ways. First, I match each individual to the laws that were in place in their state of birth when they were 14 year old, as been previously done in most studies that investigated the effect of these laws on various outcomes (see Lleras-Muney (2002) for example), and construct a dummy variable that indicates whether or not a woman was subjected to compulsory schooling at 14. As an alternative, I also compute exposure measures based on the laws in place when these women were between the ages of 9 to 13, as in Aaronson et al. (2014), and control for either the four year average school exit age, or for a dummy variable indicating any laws in place during these four years. The estimated model is:

$$Fertility_{ijts} = \alpha + \beta Exposure_{ijts} + \psi Childhood\,Exposure_{jtsr} + X'_{ijts}\phi + \gamma_{jt} + \delta_{tr} + \mu_s + \varepsilon_{ijst} \quad (5)$$

The results suggest that the laws during childhood had little or no impact on fertility, and controlling for their effect does not change the magnitude or significance of the exposure in adulthood (see appendix tables A1 and A2).

8 Additional analysis

8.1 Lifetime fertility

The fertility outcomes used throughout this study are based on the number of children under the age of ten I can link to the mother in each household. An evident limitation of using these outcome variables is they do not necessarily capture the impact of the imposed restrictions on individuals completed fertility, which might be different. It could be, for instance, that restrictions on child labor have simply led to a postponement of births and had no impact on overall fertility. Using the 1940 full count Census, where a sample line of women were asked about the number of children they ever gave birth to, I examine the effect of the legislation on the completed fertility of 50 to 60 year old women. I estimate the same model as before, with a few modifications: First, because these women are older, I use laws exposure measures that range from age 20 to 40 instead of 20 to 30 as before. Second, starting from the 1940 census, individuals were asked about their educational attainment, so I now control for completed years of schooling instead of literacy. Third, I exclude the year dummies for this model is estimated using a single census year. Age fixed effects however are still included and variation in exposure to child labor restrictions originates from variation in age within state of residence. Last, I focus solely on the effect of the average school exit age on these women's fertility, due to the fact that by the time these women were twenty, practically all states had a CSL in place, and therefore some form of child labor restrictions. The 1940 sample consist of 97,053 sample-line white native born married women ages 50-60. The results presented in table 8 suggest that lifetime fertility have been affected by laws restricting child labor in a similar magnitude to that of the laws on shorter term fertility. A requirement of an additional year in school and out of the labor force is associated with a 0.049 (s.e = 0.012) decrease in the number of children ever born, and a 0.003 (s.e = 0.001) increase in the probability to remain childless. Note that if the laws merely led to a temporary postponement of fertility, we would expect to see little or no impact on completed fertility. The fact that I find a similar impact on lifetime fertility suggests that the identified effect of child labor restrictions on the short-term fertility is not driven by an effect on the timing of births. In addition, these results provide further support to the assertion that the main results are not driven by an increase in schooling due to an impact of the legislation during childhood, for in this set of specifications I control for schooling.

8.2 Heterogeneous effects

Child labor restrictions should affect the fertility choices of parents who in the absence of such restrictions would have wanted to send their children to work. Accordingly, we should expect that the impact on fertility would be larger among households who are most likely to send their children into the labor force. To see if this is indeed the case, I start by examining the determinants of child labor. I run a linear probability model to predict the probability a child is employed as a function of available household and parental characteristics. For this purpose, I construct a sample of children ages 10 to 15, who were born to native born white married women. In particular, I examine the impact of whether the mother is illiterate, the father is employed in the agricultural sector, the father has a low occupational income score[11], urban area, farm status, and home ownership, on the probability a child is employed.

[11] In the absence of information on income or wages in these censuses, I use the father's occupational income score as a proxy for income. The IPUMS occupational income score provides a constructed income score based on the relative economic standing of occupations in 1950. I define a low occupational income

The results reported in appendix table A3 suggest that children of illiterate mothers, farm workers and workers of a low occupational standing are more likely to be working. Farm residence, living in an urban area, and being a tenant as opposed to a homeowner are also associated with a higher probability a child will be employed. I then return to the main specifications aimed at estimating the impact of the average school exit age on fertility, and allow for a differential impact by interacting the above characteristics with the average school exit age exposure measure.[12] The results are displayed in table 9. The negative impact of the legislation is significantly larger for women residing in farms, women married to men working in the agricultural sector, and to those who belong to the bottom half of the occupational income score distribution. The negative impact for home owners and for those residing in an urban area on the other hand is significantly smaller. Except for the interaction with urban area, all of the estimated heterogeneous effects are consisted with the argument that the effect should be larger in households in which the children are more likely to be employed. The results are also consistent with the luxury axiom discussed in section 2, according to which child labor is largely a result of poverty.

8.3 Migration and additional robustness checks

Due to data limitations and the nature of the exposure measures used throughout this study to infer the impact of child labor restrictions on fertility, my main sample consists of white native born women who in the time of census, resided in the same state they were born in. In other words, I omit all native born white women for which the state of birth differs from their state of residence at the time of the census. These women consist roughly a third of the native born white married women population at the time. The results so far are therefore representative of the population of native born women who have chosen not to migrate to another state during the investigated period. While census data for these years is not very suitable for investigating geographic mobility (all we know is the state of birth and the current state of residence), I attempt to examine whether child labor restrictions were in some way associated with different migration patterns. Estimates from the basic specifications, with an indicator for state of birth being different than the current state of residence as the dependent variable are presented in appendix table A4. The results show that the probability a woman was born in a different state than the one she currently resides in is not affected by exposure to child labor restrictions, and provide no evidence for selective migration. Next, I re-estimate the baseline model with fertility as the dependent

score as a score lower than the median.

[12] I do this for each of the covariants except the mother literacy indicator, due to the fact that less than 3% of the women in this sample are illiterate.

variable, only this time I do not exclude from the sample women who were born in a different state. The results are presented in appendix table A5. The estimated coefficients from the baseline model which excludes interstate migrants are presented in column 1 for the sake of comparison. Expanding the sample to include migrants, while controlling for migration, does not seem to affect the results (column 2). I also examine what happens when I assign individuals to states according to their state of birth rather than their state of residence. The estimate of the effect of child labor restrictions remains unchanged (column 3).

Last, I examine the sensitivity of the main results to the inclusion of region-specific and state-specific linear time trends. The results are presented in appendix table A6. The estimated coefficients from the baseline region year fixed effects model are presented in column 1 for the sake of comparison. Replacing the region year fixed effects with either regional linear time trends (column 2), or state-level linear time trends (column 3), does not considerably change the magnitude or significance of the main estimate, suggesting the results are not sensitive to the way I account for time effects in fertility.

9 Summary

In this chapter, I have shown that the labor market restrictions on working children in the early 20th century U.S were not only effective in reducing the prevalence of child labor, but also had spillover effects on young parents fertility rates. By exploiting the variation in restrictions imposed by states across time, I estimate the impact of different exposure measures to legislation aimed at reducing child labor and increasing education attainment on the fertility of women of reproductive age. I find that couples have internalized the implicit increase in the price of children induced by these restrictions and consequently chose to have less children, and that the impact was larger among parents who are more prone to send their children to work. I address several threats to the validity of my identification strategy and show my results are robust to various robustness checks. My results provide additional empirical support to the premise that fertility responds to economic incentives, and that parents weigh the gains of having children against their costs. In addition, this study contributes to the understating of the role of social legislation in the U.S in shaping the modern labor market and 20th century family. While the demographic transition could be justly attributed to many factors, my findings imply that compulsory schooling laws and child labor regulation have also played a nuanced role in lowering fertility rates during this period in American history.

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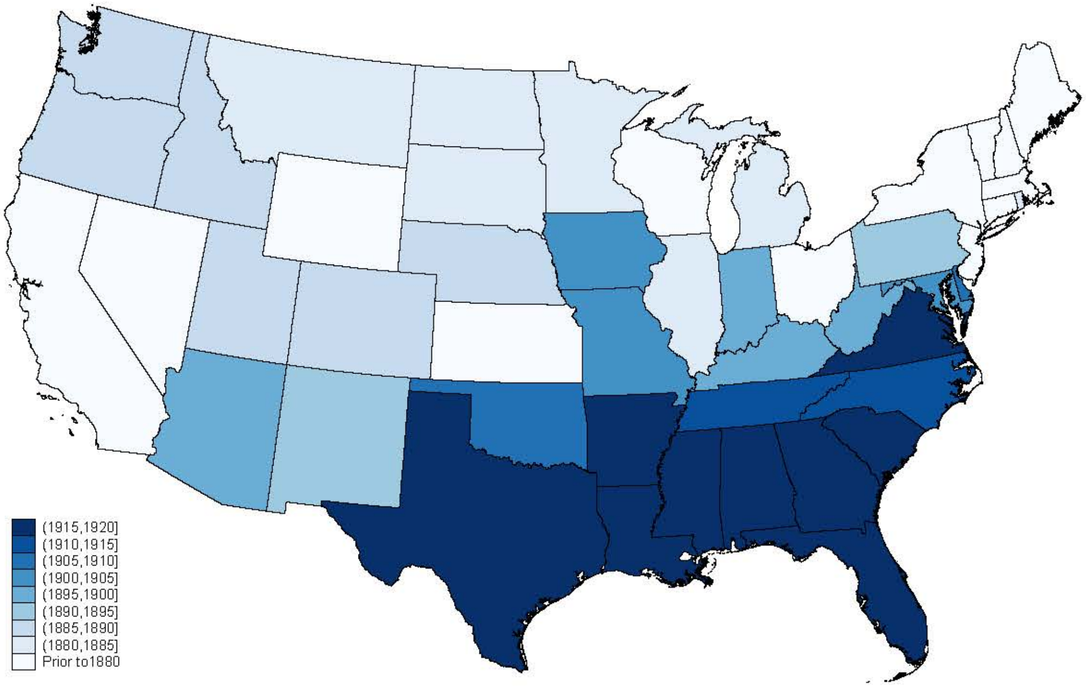
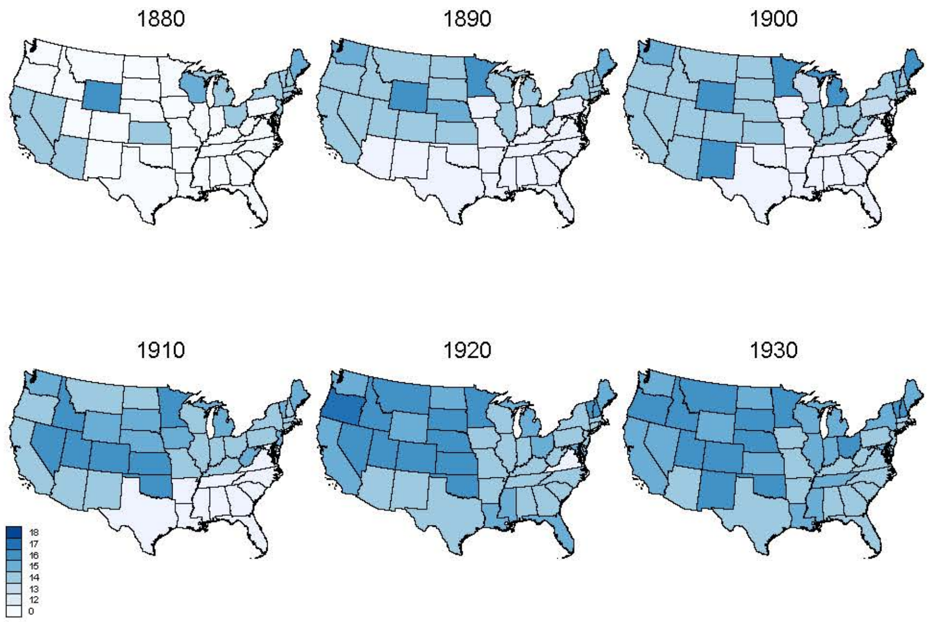
Figure 1: Introduction of Compulsory Schooling Laws, 1880-1930



Notes: The time in which each state first introduced Compulsory Schooling Laws, in 5 years intervals. Source of laws data: Clay et al. 2016.

Figure 2: The age at which children could stop attending school and work full time (effective exit age), by state, 1880-1930



Notes: The effective exit age is the minimal age a child can drop out school and enter the labor force in each state, according to Compulsory Schooling Laws and Child labor regulation in place at the the time, for the years 1880, 1890, 1990, 1910, 1920 and 1930. Exit age is set to zero when there are no child labor restrictios of any sort.

Table 1: Summary Statistics, married women ages 30-40, 1900-1930

	Total (1)	1900 (2)	1910 (3)	1920 (4)	1930 (5)
			Individual and household characteristics		
Age	34.794	34.732	34.774	34.775	34.851
	(3.182)	(3.239)	(3.182)	(3.172)	(3.161)
Literacy	0.975	0.947	0.968	0.981	0.989
	(0.156)	(0.225)	(0.175)	(0.136)	(0.104)
Urban area	0.463	0.371	0.414	0.465	0.538
	(0.499)	(0.483)	(0.493)	(0.499)	(0.499)
Home ownership	0.501	0.502	0.511	0.497	0.496
	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)
Farm residence	0.325	0.396	0.354	0.337	0.263
	(0.468)	(0.489)	(0.478)	(0.473)	(0.440)
			Fertility Measures		
Nchild10	1.683	1.949	1.768	1.663	1.512
	(1.543)	(1.615)	(1.577)	(1.534)	(1.466)
Birth10	0.714	0.763	0.726	0.707	0.687
	(0.452)	(0.426)	(0.446)	(0.455)	(0.464)
			Exposure Measures		
Any legislation	0.834	0.573	0.746	0.858	0.999
	(0.372)	(0.495)	(0.435)	(0.349)	(0.032)
School exit age	11.000	6.912	9.424	11.287	13.796
	(5.632)	(6.703)	(6.084)	(5.469)	(2.350)
Observations	359317	101592	25609	31731	200385

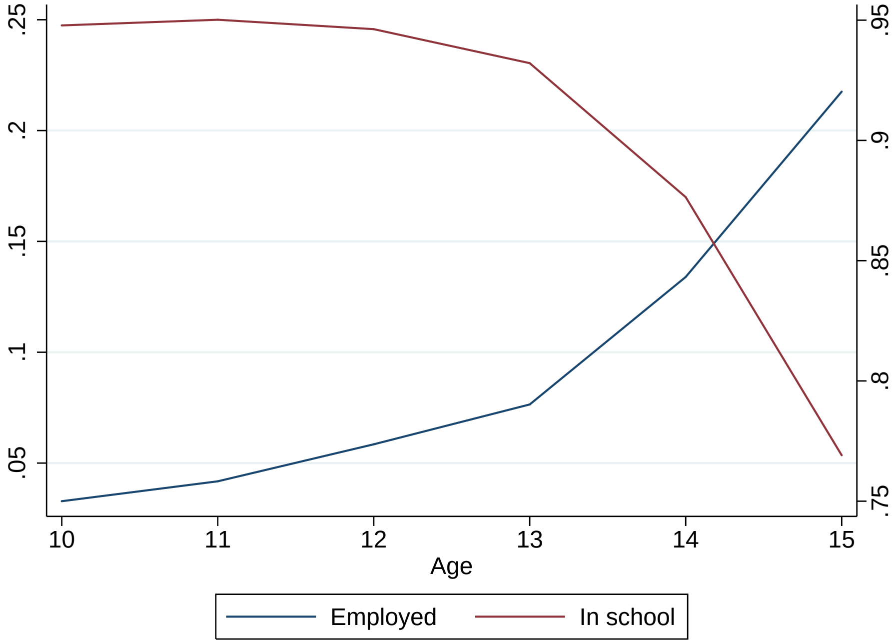
Notes: The table reports the main sample characteristics in regard to individual and household characteristic, fertility outcomes, and exposure to child labor restrictions, separately for the 1900, 1910, 1920 and 1930 censuses. Sample consists of native born white married women between 30 and 40 years of age, who resided in their state of birth at the time of the census. Nchild10 is the number of children born during the last ten years. Birth10 is a dummy variable stating whether a woman gave birth during the last years. Any legislation is a dummy variable stating whether a law restricting child labor was in place when each woman was in her twenties. School exit age is the mean school exit age imposed by child labor restrictions when each woman was in her twenties. The table reports mean values. Standard deviations are reported in parentheses.

Table 2: Summary Statistics, children ages 10-15, 1900-1930

	Total (1)	1900 (2)	1910 (3)	1920 (4)	1930 (5)
	Individual and household characteristics				
Age	12.446	12.422	12.488	12.405	12.465
	(1.705)	(1.716)	(1.707)	(1.693)	(1.704)
Male	0.505	0.505	0.508	0.505	0.504
	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)
Urban area	0.436	0.345	0.397	0.442	0.516
	(0.496)	(0.475)	(0.489)	(0.497)	(0.500)
Home ownership	0.545	0.555	0.557	0.543	0.531
	(0.498)	(0.497)	(0.497)	(0.498)	(0.499)
Farm residence	0.371	0.455	0.400	0.367	0.300
	(0.483)	(0.498)	(0.490)	(0.482)	(0.458)
	Outcomes				
Employed	0.092	0.146	0.148	0.067	0.037
	(0.288)	(0.353)	(0.356)	(0.250)	(0.189)
In school	0.906	0.783	0.924	0.942	0.939
	(0.292)	(0.412)	(0.266)	(0.234)	(0.239)
	Exposure Measures				
Law in effect	0.667	0.455	0.575	0.754	0.794
	(0.471)	(0.498)	(0.494)	(0.430)	(0.405)
Observations	1179613	389918	87349	102608	599738

Notes: The table reports the main sample characteristics in regard to individual and household characteristic, labor market outcomes, and exposure to child labor restrictions, separately for the 1900, 1910, 1920 and 1930 censuses. The sample includes native born white children between 10 and 15 years of age. Employed is a dummy variable stating whether a child is currently employed. In school is a dummy variable stating whether a child is currently in school. Law in effect is a dummy variable indicating whether a law restricting the child's labor supply (according to his age, and state of residence) is currently in place. Standard deviations are reported in parentheses.

Figure 3: Share of children employed and children attending school, by age



Notes: Sample consists of white native born children from the 1900, 1910, 1920, and 1930 IPUMS

Table 3: The effect of child labor restrictions on children's labor supply

	Employed (1)	In school (2)
Law in effect	-0.040***	0.047***
	(0.006)	(0.007)
Farm residence	0.070***	-0.005
	(0.011)	(0.004)
Urban area	0.016***	0.002
	(0.002)	(0.004)
Home ownership	-0.026***	0.039***
	(0.003)	(0.005)
Male	0.084***	-0.006***
	(0.011)	(0.001)
Age * year F.E	Yes	Yes
State F.E	Yes	Yes
Year * region F.E	Yes	Yes
R2	0.165	0.118
Outcome mean	0.092	0.906
Observations	1179562	1179562

Notes: : The table reports the effect of work eligibility on the probability a child is either employed or in school. Each column denotes a separate regression. The sample includes native born white children between 10 and 15 years of age, from the 1900, 1910, 1920, and 1930 IPUMS. Law In Effect is a dummy variable stating whether a child is required to be in school and is not allowed to work. Employed is a dummy variable stating whether a child is currently employed, and In school is a dummy variable stating whether a child is currently in school. Law in effect is a dummy variable stating whether a law restricting the child's labor supply (according to his age, and state of residence) is currently in place. Standard errors clustered at the state level in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 4: The effect of the extensive margin of child labor restrictions on fertility

	Nchild10 (1)	Birth10 (2)
Any legislation	-0.101***	-0.025***
	(0.027)	(0.005)
Literacy	-0.338***	-0.047***
	(0.058)	(0.010)
Farm residence	0.398***	0.068***
	(0.034)	(0.004)
Urban area	-0.298***	-0.069***
	(0.033)	(0.007)
Home ownership	-0.088***	-0.003
	(0.018)	(0.005)
Age * year F.E	Yes	Yes
State F.E	Yes	Yes
Year * region F.E	Yes	Yes
R2	0.109	0.058
Outcome mean	1.683	0.714
Observations	359306	359306

Notes: The table reports the effect of child labor restrictions extensive margin on women's fertility. Each column denotes a separate regression. Sample consists of native born white married women between 30 and 40 years of age, who resided in their state of birth at the time of the census, from the 1900, 1910, 1920, and 1930 IPUMS. Nchild10 is the number of children born during the last ten years. Birth10 is a dummy variable stating whether a woman gave birth or not during the last years. Any legislation is a dummy variable stating whether a law restricting child labor was in place when each woman was in her twenties. Standard errors clustered at the state level in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 5: The effect of the intensive margin of child labor restrictions on fertility

	Nchild10 (1)	Birth10 (2)
School exit age	-0.008**	-0.003***
	(0.003)	(0.001)
Literacy	-0.339***	-0.047***
	(0.059)	(0.010)
Farm residence	0.398***	0.068***
	(0.034)	(0.004)
Urban area	-0.299***	-0.069***
	(0.033)	(0.007)
Home ownership	-0.088***	-0.003
	(0.018)	(0.005)
Age F.E	Yes	Yes
State F.E	Yes	Yes
Year * region F.E	Yes	Yes
R2	0.109	0.058
Outcome mean	1.683	0.714
Observations	359306	359306

Notes: The table reports the effect of child labor restrictions intensive margin on women's fertility. Each column denotes a separate regression. Sample consists of native born white married women between 30 and 40 years of age, who resided in their state of birth at the time of the census, from the 1900, 1910, 1920, and 1930 IPUMS. Nchild10 is the number of children born during the last ten years. Birth10 is a dummy variable stating whether a woman gave birth or not during the last years. School exit age is the mean school exit age imposed by child labor restrictions when each woman was in her twenties. Standard errors clustered at the state level in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 6: The effect of the future child labor restrictions on fertility

	Nchild10		Birth10	
	(1)	(2)	(3)	(4)
Future legislation	-0.007		-0.007	
	(0.023)		(0.006)	
Future school exit age		0.005		-0.0001
		(0.006)		(0.001)
Controls	Yes	Yes	Yes	Yes
Age F.E	Yes	Yes	Yes	Yes
State F.E	Yes	Yes	Yes	Yes
R2	0.113	0.113	0.044	0.044
Outcome mean	2.176	2.176	0.801	0.801
Observations	112319	112319	112319	112319

Notes: The table reports the effect of future child labor restrictions on women's fertility. Each column denotes a separate regression. Sample consists of native born white married women between 30 and 40 years of age, who resided in their state of birth at the time of the census, from the 1880 IPUMS. Nchild10 is the number of children born during the last ten years. Birth10 is a dummy variable stating whether a woman gave birth or not during the last years. Future legislation is a dummy variable stating whether a law restricting child labor was in place when each woman was in her fourties. The future school exit age is the mean school exit age imposed by child labor restrictions, 10 years later, when each woman is in her fourties. Standard errors clustered at the state level in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 7: Total fertility rates and the enactment of compulsory schooling laws

	(1)	(2)	(3)	(4)	(5)	(6)
TFR (t-10)	-0.075	-0.047	-0.012	-0.010	-0.026	0.009
	(0.064)	(0.084)	(0.090)	(0.033)	(0.050)	(0.076)
TFR (t-20)			-0.055			-0.048
			(0.111)			(0.086)
State F.E	Yes	Yes	Yes	Yes	Yes	Yes
Year * region F.E	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes
R2	0.796	0.806	0.822	0.854	0.860	0.858
Observations	156	156	123	211	211	176

Notes: The table reports the effect of lagged state level total fertility rates on the probability a compulsary schooling law is passed\is in place. Each column denotes a separate regression. The sample consists of a panel of state-year observations, from the 1850, 1860, 1870, 1880, 1900, 1910, 1920, and 1930 IPUMS. Data on 1890 is linearly interpolated. In coulmns 1-3, each state is observed up to the year the law is passed. State level controls include share of the population residing in farms, share residing in urban areas, percent female, share of men working in manufacturing, and literacy rates. TFR is calculated by summing the age-specific fertility rates of women ages 20-24, 25-29, 30-34, 35-39, 40-44, and 45-49, for each state-year cell. 30 cells with non-positive age-specific fertility rates are omitted. Standard errors clustered at the state level in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 8: The effect of child labor restrictions on completed fertility

	Nchild (1)	Birth (2)
School exit age	-0.049***	-0.003**
	(0.012)	(0.001)
Schooling	-0.188***	-0.008***
	(0.007)	(0.001)
Farm residence	0.614***	0.040***
	(0.049)	(0.004)
Urban area	-0.500***	-0.027***
	(0.043)	(0.003)
Home ownership	-0.423***	-0.014***
	(0.041)	(0.003)
Age F.E	Yes	Yes
State F.E	Yes	Yes
R2	0.140	0.026
Outcome mean	3.249	0.853
Observations	95894	95894

Notes: The table reports the effect of child labor restrictions intensive margin on women's fertility. Each column denotes a separate regression. Sample consists of native born white married women between 30 and 40 years of age, who resided in their state of birth at the time of the census, from the 1900, 1910, 1920, and 1930 IPUMS. Nchild is the number of children ever born. Birth is a dummy variable stating whether a woman ever gave birth. School exit age is the mean school exit age imposed by child labor restrictions when each woman was in her twenties and thirties. Standard errors clustered at the state level in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 9: The heterogeneous effects of child labor restrictions on fertility

	Interacted with:				
	Farm residence	Rural Area	Home ownership	Spouse -low occupational income score	Spouse - farmer
	(1)	(2)	(3)	(4)	(5)
School exit age	-0.001	0.003	-0.011***	0.001	-0.001
	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)
Interaction	-0.015***	-0.016***	0.006***	-0.012***	-0.014***
	(0.004)	(0.004)	(0.002)	(0.003)	(0.003)
Literacy	-0.327***	-0.323***	-0.332***	-0.317***	-0.327***
	(0.060)	(0.059)	(0.060)	(0.061)	(0.061)
Farm residence	0.544***	0.391***	0.401***	0.326***	0.257***
	(0.053)	(0.032)	(0.034)	(0.030)	(0.026)
Urban area	-0.308***		-0.297***	-0.256***	-0.271***
	(0.032)		(0.034)	(0.031)	(0.033)
Home ownership	-0.090***	-0.088***	-0.158***	-0.047**	-0.056***
	(0.018)	(0.018)	(0.021)	(0.019)	(0.019)
Rural Area		0.491***			
		(0.040)			
Spouse - low income score				0.290***	
				(0.035)	
Spouse - farmer					0.308***
					(0.048)
Age * year F.E	Yes	Yes	Yes	Yes	Yes
State F.E	Yes	Yes	Yes	Yes	Yes
Year * region F.E	Yes	Yes	Yes	Yes	Yes
R2	0.110	0.110	0.109	0.111	0.110
Outcome mean	1.683	1.683	1.683	1.737	1.737
Observations	359306	359306	359306	338297	338297

Notes: The table reports the effect of child labor restrictions intensive margin on women's fertility. Each column denotes a separate regression, in which the school exit age is interacted with a different covariant. Sample consists of native born white married women between 30 and 40 years of age, who resided in their state of birth at the time of the census, from the 1900, 1910, 1920, and 1930 IPUMS. The outcome variable is Nchild10, which is the number of children born during the last ten years. School exit age is the mean school exit age imposed by child labor restrictions when each woman was in her twenties. Standard errors clustered at the state level in parentheses. * p<0.10, ** p<0.05, *** p<0.01

10 Appendix

Table A1: The effect of the extensive margin of child labor restrictions on fertility, controlling for earlier exposure

	Nchild10 (1)	Nchild10 (2)	Nchild10 (3)	Birth10 (4)	Birth10 (5)	Birth10 (6)
Any legislation	-0.124***	-0.130***	-0.130***	-0.031***	-0.037***	-0.032***
	(0.030)	(0.032)	(0.032)	(0.008)	(0.008)	(0.008)
Any legislation during childhood		-0.019			-0.017**	
		(0.035)			(0.008)	
Compulsory schooling at 14			-0.028			-0.009
			(0.029)			(0.006)
Literacy	-0.342***	-0.342***	-0.341***	-0.047***	-0.046***	-0.046***
	(0.060)	(0.060)	(0.060)	(0.011)	(0.011)	(0.011)
Farm residence	0.412***	0.412***	0.412***	0.071***	0.071***	0.071***
	(0.034)	(0.034)	(0.034)	(0.004)	(0.004)	(0.004)
Urban area	-0.317***	-0.317***	-0.317***	-0.073***	-0.073***	-0.073***
	(0.030)	(0.030)	(0.030)	(0.007)	(0.007)	(0.007)
Home ownership	-0.081***	-0.081***	-0.081***	0.002	0.002	0.002
	(0.020)	(0.020)	(0.020)	(0.005)	(0.005)	(0.005)
Age F.E	Yes	Yes	Yes	Yes	Yes	Yes
State F.E	Yes	Yes	Yes	Yes	Yes	Yes
Year * region F.E	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.106	0.106	0.106	0.057	0.058	0.057
Outcome mean	1.636	1.636	1.636	0.708	0.708	0.708
Observations	249173	249173	249173	249173	249173	249173

Notes: The table reports the effect of child labor restrictions extensive margin on women's fertility, controlling for exposure to labor market restrictions exposed to during childhood. Each column denotes a separate regression. Sample consists of native born white married women between 30 and 40 years of age with available exposure data at an early age, who resided in their state of birth at the time of the census, from the 1910, 1920, and 1930 IPUMS. Nchild10 is the number of children born during the last ten years. Birth10 is a dummy variable stating whether a woman gave birth or not during the last years. Any legisltation is a dummy variable stating whether a law restricting child labor was in place when each woman was in her twenties. Any legisltation during childhood is a dummy variable stating whether a law restricting child labor was in place when each woman was between the age of 7 and 13. Compulsory schooling at 14 is dummy variable indicating whether when a woman was 14 she was required to be in school or not. Standard errors clustered at the state level in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table A2: The effect of the intensive margin of child labor restrictions on fertility, controlling for earlier exposure

	Nchild10			Birth10		
	(1)	(2)	(3)	(4)	(5)	(6)
School exit age	-0.013***	-0.014***	-0.013***	-0.004***	-0.004***	-0.004***
	(0.004)	(0.004)	(0.004)	(0.001)	(0.001)	(0.001)
School exit age during childhood		-0.001			-0.001**	
		(0.003)			(0.000)	
Compulsory schooling at 14			-0.019			-0.007
			(0.029)			(0.006)
Literacy	-0.341***	-0.341***	-0.341***	-0.046***	-0.046***	-0.046***
	(0.060)	(0.060)	(0.060)	(0.011)	(0.011)	(0.011)
Farm residence	0.412***	0.412***	0.412***	0.071***	0.071***	0.071***
	(0.034)	(0.034)	(0.034)	(0.004)	(0.004)	(0.004)
Urban area	-0.317***	-0.317***	-0.317***	-0.073***	-0.073***	-0.073***
	(0.030)	(0.030)	(0.030)	(0.007)	(0.007)	(0.007)
Home ownership	-0.081***	-0.081***	-0.081***	0.002	0.002	0.002
	(0.020)	(0.020)	(0.020)	(0.005)	(0.005)	(0.005)
Age * year F.E	Yes	Yes	Yes	Yes	Yes	Yes
State F.E	Yes	Yes	Yes	Yes	Yes	Yes
Year * region F.E	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.106	0.106	0.106	0.058	0.058	0.058
Outcome mean	1.636	1.636	1.636	0.708	0.708	0.708
Observations	249173	249173	249173	249173	249173	249173

Notes: The table reports the effect of child labor restrictions intensive margin on women's fertility, controlling for exposure to labor market restrictions exposed to during childhood. Each column denotes a separate regression. Sample consists of native born white married women between 30 and 40 years of age with available exposure data at an early age, who resided in their state of birth at the time of the census, from the 1910, 1920, and 1930 IPUMS. Nchild10 is the number of children born during the last ten years. Birth10 is a dummy variable stating whether a woman gave birth or not during the last years. School exit age is the mean school exit age imposed by child labor restrictions when each woman was in her twenties. School exit age during childhood is the mean school exit age imposed by child labor restrictions when each woman was between the age of 7 and 13. Compulsory schooling at 14 is dummy variable indicating whether when a women was 14 she was required to be in school or not. Standard errors clustered at the state level in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A3: The determinants of child labor

	Employed (1)
Mother is literate	-0.068***
	(0.007)
Farm residence	0.042***
	(0.009)
Urban area	0.022***
	(0.002)
Home ownership	-0.017***
	(0.004)
Father - low income score	0.016***
	(0.002)
Father is a farmer	0.026***
	(0.007)
Age * year F.E	Yes
State F.E	Yes
Year * region F.E	Yes
R2	0.154
Outcome mean	0.086
Observations	289744

Notes: : The table reports the effect of individual and household level characteristics on the probability a child is employed. Each column denotes a separate regression. The sample includes children ages 10 to 15, of native born white married women between 30 and 40 years of age, from the 1900, 1910, 1920, and 1930 IPUMS. The father's occupational score is an IPUMS constructed occupational income score, which provides a continuous measure of occupations, according to the economic rewards enjoyed by people working at them in 1950. A low score is defined as lower than the sample median. Father is a farmer is a dummy variable indicating whether the father is employed in the agricultural sector. Standard errors clustered at the state level in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table A4: Checking for endogenous migration

	(1)	(2)
Any legislation	-0.010	
	(0.021)	
School exit age		-0.002
		(0.002)
Literacy	0.054***	0.054***
	(0.014)	(0.014)
Farm residence	-0.069***	-0.069***
	(0.006)	(0.006)
Urban area	0.057***	0.057***
	(0.008)	(0.008)
Home ownership	-0.052***	-0.052***
	(0.005)	(0.005)
Age * year F.E	Yes	Yes
State F.E	Yes	Yes
Year * region F.E	Yes	Yes
R2	0.206	0.206
Outcome mean	0.315	0.315
Observations	525345	525345

Notes: The table reports the effect of child labor restrictions on the probability a woman currently resides in a state different than her state of birth. Each column denotes a separate regression. Sample consists of native born white married women between 30 and 40 years of age, from the 1900, 1910, 1920, and 1930 IPUMS. Any legislation is a dummy variable stating whether a law restricting child labor was in place when each woman was in her twenties. School exit age is the mean school exit age imposed by child labor restrictions when each woman was in her twenties. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A5: The effect of child labor restrictions on fertility, including migrants

	(1)	(2)	(3)
School exit age - state of residence	-0.008**	-0.007**	
	(0.003)	(0.003)	
School exit age - state of birth			-0.007**
			(0.003)
Literacy	-0.339***	-0.353***	-0.354***
	(0.059)	(0.048)	(0.053)
Farm residence	0.398***	0.425***	0.429***
	(0.034)	(0.032)	(0.028)
Urban area	-0.299***	-0.327***	-0.343***
	(0.033)	(0.028)	(0.030)
Home ownership	-0.088***	-0.062***	-0.059***
	(0.018)	(0.017)	(0.016)
Migrant		-0.087***	-0.114***
		(0.018)	(0.020)
Age * year F.E	Yes	Yes	Yes
State F.E	Yes	Yes	Yes
Year * region F.E	Yes	Yes	Yes
R2	0.109	0.115	0.113
Outcome mean	1.683	1.622	1.622
Observations	359306	525345	525244

Notes: The table reports the effect of child labor restrictions intensive margin on women's fertility. Each column denotes a separate regression. Sample consists of native born white married women between 30 and 40 years of age, from the 1900, 1910, 1920, and 1930 IPUMS. In column 1, the sample is restricted to women who resided in their state of birth at the time of the census. School exit age is the mean school exit age imposed by child labor restrictions when each woman was in her twenties. Standard errors clustered at the state level in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table A6: The effect of child labor restrictions on fertility, with linear time trends

	Nchild10		
	(1)	(2)	(3)
School exit age	-0.008**	-0.009***	-0.006***
	(0.003)	(0.003)	(0.002)
Literacy	-0.339***	-0.337***	-0.341***
	(0.059)	(0.059)	(0.057)
Farm residence	0.398***	0.399***	0.398***
	(0.034)	(0.034)	(0.034)
Urban area	-0.299***	-0.298***	-0.301***
	(0.033)	(0.034)	(0.033)
Home ownership	-0.088***	-0.088***	-0.091***
	(0.018)	(0.018)	(0.018)
Age * year F.E	Yes	Yes	Yes
State F.E	Yes	Yes	Yes
Year * region F.E	Yes	No	No
Region specific linear time trend	No	Yes	No
State specific linear time trend	No	No	Yes
R2	0.109	0.109	0.111
Outcome mean	1.683	1.683	1.683
Observations	359306	359306	359306

Notes: The table reports the effect of child labor restrictions intensive margin on women's fertility, while controling for either regional year fixed effects, or regional and state specific linear time trends. Each column denotes a separate regression. Sample consists of native born white married women between 30 and 40 years of age, who resided in their state of birth at the time of the census, from the 1900, 1910, 1920, and 1930 IPUMS. Nchild10 is the number of children born during the last ten years. School exit age is the mean school exit age imposed by child labor restrictions when each woman was in her twenties. Standard errors clustered at the state level in parentheses. * p<0.10, ** p<0.05, *** p<0.01