

Intergenerational Persistence in the Effects of Compulsory Schooling*

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Abstract

Using linked records from the 1880 to 1940 full-count United States decennial censuses, we estimate the effects of parental exposure to compulsory schooling laws on the human capital outcomes of their children, exploiting the staggered roll-out of state compulsory schooling (CS) laws in the second half of the nineteenth and beginning of the twentieth century. Parental exposure to CS not only increased the educational attainment of parents, but also that of their children. The magnitudes of these effects are similar, suggesting much stronger intergenerational transmission of human capital than found in other settings. We find particularly large effects of parental CS exposure for black families, as well as for first-born sons. Exploring mechanisms of intergenerational transmission, we find evidence that higher parental CS exposure could have affected child outcomes through higher parental labor-market earnings, marriage to more educated spouses, and a greater propensity to reside in neighborhoods with greater school resources (teacher to student ratios) and higher average educational levels.

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Education ... beyond all other devices of human origin, is a great equalizer of conditions of men – the balance wheel of the social machinery

Horace Mann (1849)

1 Introduction

Public education has long been considered a critical engine of social mobility. Starting in the late nineteenth century, against the backdrop of large-scale industrialization and demographic change, nearly every state expanded its compulsory schooling (CS) requirements and enacted other educational reforms to improve the skills of their populations. Indeed, while very few states had any kind of CS law in 1880, all states required at least six years of schooling by 1930.

A large literature estimates the effects of changes in CS requirements on education, earnings, and other outcomes in the United States and around the world (Lleras-Muney, 2002, Stephens Jr and Yang, 2014). These studies almost exclusively focus on the schooling and later-life outcomes of individuals directly affected by CS reforms. However, the long-run consequences of these reforms depend crucially on the extent to which their effects *persist or even compound across generations*. Policies will most successfully lift people out of poverty and promote human-capital-based economic growth when they generate lasting effects not only for those directly exposed to the policy but also for their children and subsequent descendants. Yet, very little is known about such intergenerational effects, precisely because of the scarcity of data linking outcomes across multiple generations. Even less is known about the channels through which these effects persist across generations. Understanding these mechanisms is important if policymakers aim to target policies towards the margins that have the most persistent intergenerational effects.

In this study, we estimate the intergenerational effects of CS reforms in the United States during the late nineteenth and early twentieth centuries using linked records from full count decennial U.S. Censuses spanning 1880-1940 and observations on completed education from the 1940 Census (the first Census which collected it). Critically, cross-wave identifiers allow us to track individuals over time across census waves. By locating records of individuals during their childhoods, we observe the characteristics of their parents. Birth year and state of residence then allow us to construct parental and child exposure to CS laws.

We exploit the staggered implementation of state CS laws to estimate their intergenerational effects using a difference-in-differences framework. The remarkable size and richness of the linked census data permit three main contributions to the literature: we (i) estimate large intergenerational effects of parental exposure to CS on offspring's completed adult educational attainment, (ii) characterize heterogeneity in these effects, and (iii) explore the mechanisms through which such intergenerational effects operate.

First, we uncover strong intergenerational persistence, with the effects of parental exposure to CS on offspring's education actually being larger than the effects on the parents themselves. One extra year of maternal (paternal) exposure to CS increased children's educational attainment by 0.015 (0.016) years. By contrast, women (men) directly exposed to one additional year of CS experienced a 0.008 (0.005) year increase in years of schooling (or about

half of the intergenerational effect). These estimates are substantial. To better interpret their magnitude, we use CS exposure as an instrument for parental years of schooling, following Black, Devereux and Salvanes (2005). We estimate that a one-year increase in maternal and paternal schooling resulted in, respectively, a 0.8-year and a 1.1-year increase in children's years of schooling. These effects are much larger than those found by Black, Devereux and Salvanes (2005) in Norway, who exploit a change in CS from seventh to ninth grade that was rolled out gradually across municipalities during the 1960s and early 1970s. They obtain an effect of at most 0.18 years (for the effect of mother's education on sons; significant at the 5% level). While Black, Devereux and Salvanes (2005) interpreted their small estimated effects as evidence that selection (versus causation) accounts for much of the raw intergenerational correlation in education, our results suggest a sizable causal relationship between parental exposure to CS laws and offspring schooling. This difference in results could arise for many reasons, including a potentially larger role for residential sorting and neighborhood resource disparities in the United States, lower returns to education at higher levels of education in Norway, and higher returns during a period of rapid industrialization in the 19th and early 20th century, to name but a few.

Our second contribution consists in documenting heterogeneity in the effects of compulsory schooling. We find that maternal and paternal exposure to compulsory schooling have similar effects on children's educational attainment. However, schooling laws were more successful in urban areas and particularly effective in later years, specifically after 1900 when enforcement of compulsory schooling laws improved. Moreover, compulsory schooling had a greater impact on raising the educational attainment of Black Americans compared to other groups. This particular finding is attributed to the significantly lower educational attainment of Black Americans, particularly in the South, which resulted in compulsory schooling becoming binding for approximately half of the Black population. Furthermore, our research reveals strong evidence for gender and birth-order effects in the intergenerational transmission of human capital from both mothers and fathers. Parental exposure to compulsory schooling disproportionately benefited first-born male children.

Our third contribution consists in exploring mechanisms through which these intergenerational effects operate. The richness of the census data, and the large sample sizes they afford, allow us to not only test hypotheses about these mechanisms but also to quantify their relative importance. Household resources, such as money and time, are obvious potential mechanisms (Becker and Tomes, 1979, 1986). Both fathers and mothers directly exposed to more years of CS had higher observed wages, with women experiencing larger wage increases. Higher CS exposure also reduced female labor-force participation, which could have ambiguous effects on child outcomes by lowering monetary resources yet increasing time for child investment. More exposure to CS increased men's home-ownership rates and was associated with lower home values for both men and women. The latter result may reflect selection into home ownership: CS exposure could have induced poorer households on the margin to become homeowners.

Parental exposure to CS could also affect child outcomes through partner choice. Indeed, we find that women exposed to more CS married more educated men, who were more likely to be employed and had higher earnings. On the other hand, men exposed to more CS married more educated women who were less likely to participate in the labor market, but had higher earnings conditional on employment.

The fine level of geographic detail in the censuses allow us to study geographic sorting and mobility over time. Specifically, we construct neighborhood measures of school resources, labor-market, educational, dwelling, and demographic characteristics, at the enumeration district level, and study how exposure to more CS affected sorting across neighborhoods and across time. Even after accounting for state and cohort effects, as well as regional trends, we find that parents with more exposure to CS tended to choose neighborhoods with distinct characteristics. Specifically, when their children reached school age, these parents gravitated towards neighborhoods with higher teacher-student ratios. Importantly, this phenomenon is not driven by pre-existing conditions in the parents' initial neighborhoods. In fact, when the parents themselves were of school age, compulsory schooling laws in their state showed no association with teacher-student ratios in their neighborhoods. This suggests that parents exposed to more CS had relocated to neighborhoods with better-resourced schools by the time they had children.

We find similar evidence of parents sorting into neighborhoods with housing markets, labor markets, and demographic characteristics that reflect urbanization. Between 1910 and 1940, individuals exposed to more CS were more likely to cross state lines and transition to neighborhoods that were more populous, urban, and metropolitan. Consistent with this, their neighbors were more likely to be Black, immigrants, less likely to own their homes, and more likely to live in larger and multifamily households. Last, individuals exposed to more CS migrated to locations with higher employment rates. These results suggest that more education allowed individuals to move to more urbanized areas, thereby potentially benefiting from the rapid industrialization during the first half of the twentieth century in the United States.

We quantify the relative importance of each of these channels using a decomposition method (Gelbach, 2016). We find that the effect of paternal exposure to CS on the child's years of schooling is accounted for by the father's education (15%), his spouse's education (30%), home ownership and home value (6%), and sorting into a neighborhood with more educated inhabitants (22%). School resources, measured as teacher-student ratios, own labor-market outcomes, and other neighborhood characteristics, appear to explain very little of the effects. The decomposition also suggests that the effect of maternal exposure to CS is accounted for by maternal education (21%), her spouse's education (17%), and the average level of education in the neighborhood (21%).

To the best of our knowledge, we provide the first evidence from the United States on the *intergenerational* effects of compulsory schooling reforms on *completed adult educational attainment outcomes* of the offspring. By contrast, most of the literature estimates the effects of parental education on the *early* educational outcomes of children. Currie and Moretti (2003) find that mothers in the U.S. who had easier access to colleges were more likely to have children with better infant health outcomes, such as for birth weight and gestational age. Using NLSY data, Carneiro, Meghir and Parey (2013) find positive effects of maternal education on childhood cognitive performance and behavioral outcomes. Closer to our work, Oreopoulos, Page and Stevens (2006) estimate that parental exposure to U.S. compulsory schooling laws reduced the probability that a child was held back a year in school.

A number of papers estimate the intergenerational effects of education reforms in European contexts (Black, Devereux and Salvanes, 2005, Chevalier et al., 2013, Dickson, Gregg and Robinson, 2016, Holmlund, Lindahl and Plug, 2011, Piopiunik, 2014, Sikhova, 2023).

Using UK data, Dickson, Gregg and Robinson (2016) find that parental exposure to more compulsory schooling increased test scores for teenagers. Examining multiple policies, including changes in compulsory schooling laws, Chevalier et al. (2013) estimate causal effects of parental income and education on the propensity for children to acquire post-compulsory schooling. Studies like these examine the outcomes of children residing with their parents in order to match child outcomes to parental compulsory schooling exposure. This data requirement necessitates a focus on childhood academic outcomes completed before the end of formal education. By contrast, the linked census data allow us to estimate the effects of parental exposure on completed educational attainment of the offspring. Sikhova (2023) offers a rare example of a study looking at intergenerational effects of policy on the adult outcomes of children. Using a schooling reform in Sweden as a source of exogenous variation in parental income, Sikhova (2023) estimates the contributions of parental income and education to the intergenerational correlation in earnings.

Our results also contribute to an established literature documenting factors that shape intergenerational mobility in the United States and other contexts. Several studies examine whether schooling reforms affected intergenerational mobility by estimating whether schooling reforms had larger or smaller direct effects on individuals from different socioeconomic backgrounds. Our approach and that of Black, Devereux and Salvanes (2005), is distinct in that we explore the causal effect of reforms that affected parents on the outcomes of their children. Directly related to our work, Rauscher (2016) finds that while CS laws made school attendance more equal, they initially *reduced* intergenerational occupational mobility, although this effect subsequently vanished after about a decade. Using the full count 1940 Census, Card, Domnisoru and Taylor (2022) find that higher quality education in a state (proxied by teacher’s wages) promotes greater educational mobility for the children of parents in the bottom quartile of the education distribution. Both studies examine heterogeneity in the effects of educational institutions on the outcomes of directly affected children. By contrast, we study whether compulsory schooling reforms had effects across generations, specifically on the children of directly affected individuals.

Our main results, and the analyses of mechanisms in particular, also contribute to the larger discussion on whether the degree of intergenerational mobility in the U.S. today has changed versus the past and whether it is different from that in other contexts (Long and Ferrie, 2007, 2013). For example, Ferrie (2005) concludes that the US was occupationally and geographically more mobile than Britain in the mid-nineteenth century, but that this mobility advantage declined in the early part of the twentieth century. Long and Ferrie (2013) suggest that residential mobility offers a compelling explanation for this, since cross-county mobility in the U.S. during the late nineteenth century was substantially greater than comparable mobility in the U.K., or in the later twentieth-century U.S. context. Our results provide direct evidence for this hypothesis, showing that indeed parental exposure to CS accelerated migration to more metropolitan and urban areas. This migration may have been one of the key mechanisms leading to persistence in the intergenerational transmission of education.

Our results further suggest that the intergenerational transmission of human capital is larger than we previously thought. In particular, in environments with high social mobility and rapidly increasing educational levels, policies aiming to increase the educational levels of low-education individuals can have very large intergenerational effects. Such “snowballing” may have contributed to the observed rapid growth in educational attainment over the twen-

tieth century.

The rest of the paper is structured as follows. Section 2 describes in detail the setup, institutional background, and data. Section 3 outlines the empirical strategy. Section 4 presents the main results. Section 5 provides several robustness checks and Section 6 concludes.

2 Institutional Background and Data

2.1 Compulsory Schooling Laws

Individuals born in the late nineteenth and early twentieth centuries in the United States lived through a number of substantial changes to compulsory schooling laws. Several distinct laws operated together to influence the schooling required for a particular birth-year cohort y in state s . Using the taxonomy of Lleras-Muney (2002), these included laws on the oldest age at which a child could start schooling (Entrance Age) and the youngest age at which a child could end schooling (Dropout Age). Some laws provided a school leaving exemption, allowing children to drop out of school before the Dropout Age, as long as they completed sufficiently many years of schooling.

Given the prevalence of child labor during this period, several states also specified a minimum age after which a child could obtain a work permit and leave school (Work Age). In some cases, these children were still required to attend continuation schooling (a type of after-work night school) until a certain age. The literature has typically combined information on these laws to create a single variable that measures the *years of compulsory schooling* faced by a state (s) birth-year (y) group, sy .

We code state compulsory schooling laws and child labor laws following the methodology of Clay, Lingwall and Stephens Jr (2021).¹ Using state law archives for each individual state, these authors collect state laws between 1880 and 1930 to determine the number of years of compulsory schooling each individual born in state s and birth year cohort y was subject to. We use their data and extend it by including information about cohorts born as early as 1845 using state law archives. We do this by accessing state archives online to find the oldest schooling law documented by Clay, Lingwall and Stephens Jr (2021), finding whether this law amends or replaces a previously-existing schooling law, and moving backward in time in this manner.

Exposure to compulsory schooling is defined for each individual based on their state of birth and cohort year sy . For each state-cohort sy , we ask the following questions each year they are aged between 1 and 18:²

1. Is the child's age between the maximum Entrance Age and the minimum Dropout Age?
2. If so, does an exemption to the Dropout Age apply? For example:
 - was the child already required to attend school for a sufficient number of years such that it could qualify for an early Dropout exemption?

¹This builds on previous work by Acemoglu and Angrist (2000), Lleras-Muney (2002), Goldin and Katz (2008) and Stephens Jr and Yang (2014), among others.

²The school leaving age is at most 18 in all states during our sample period.

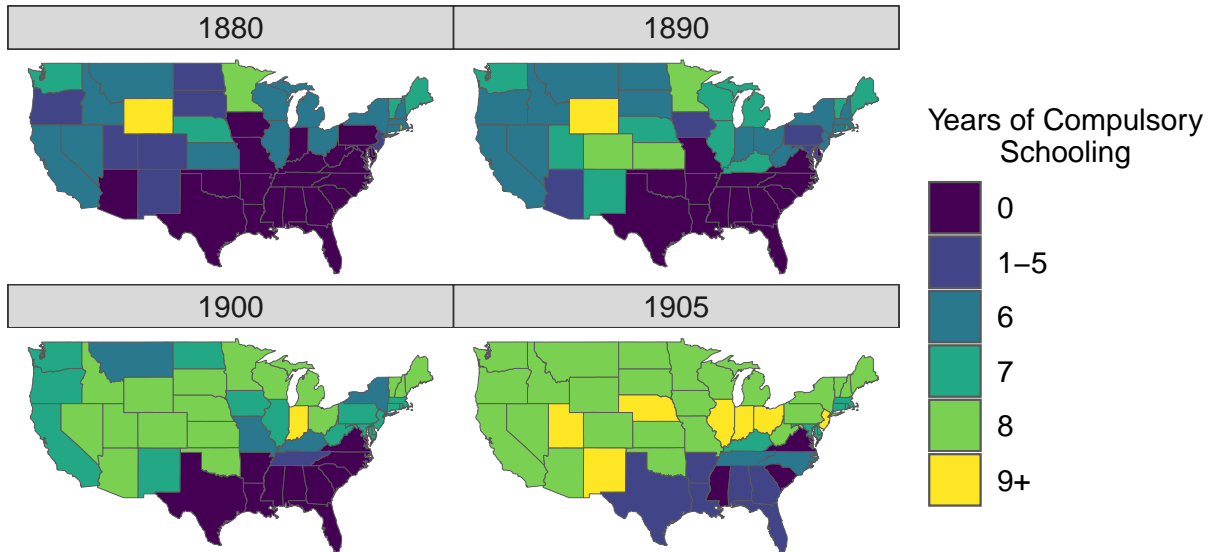


Figure 1: Compulsory schooling law exposure by state, for the 1880, 1890, 1900, and 1905 birth-year cohorts.

- is the child's age equal to, or greater than, the age at which a work permit could be obtained (Work Age exemption)? If so, has the child been required to attend school for a sufficient number of years such that it would satisfy the Work Age exemption?
3. If a Work Age exemption applies, is the child's age less than the Dropout Age? If so, has the child completed sufficient schooling to be exempt from continuation school if such an exemption exists?

The answers to these questions determine the number of years the individuals in our data were legally required to stay in school. For instance, suppose that a certain state s implemented legislation in the year 1900 stipulating that children aged between 8 and 14 must attend school. Suppose further that in 1915, an amendment was made to this law, extending the age range to include all children between 8 and 18, with a provision for exempting those who are over 16 and have a job. According to these laws, a child born in this state in 1900 would be legally obliged to attend school from 1908 to 1914, amounting to a compulsory schooling period of 7 years. On the other hand, consider a child born in 1905, who was still attending school at the age of 10 when the new schooling law came into effect. This child would be obliged to attend school from 1908 to 1918. However, if they were able to acquire a work permit, they would be permitted to leave school in 1916. In this case, we conservatively consider the cohort born in 1905 to be exposed to 9 years of compulsory schooling. This

accounts for 11 years of compulsory schooling initially but is reduced to 9 years due to the work permit exemption (since we do not know for whom this exemption applied we apply it conservatively to everyone).

Figure 1 shows the geographic distribution of the roll-out of compulsory schooling laws in the United States, based on the previously-described coding of compulsory schooling laws. By the time the cohorts born in 1880 reached the school-entry age, states in New England, around the Great Lakes, and in the Western United States, already had some form of compulsory schooling law enacted. The New England states in particular were early adopters of compulsory schooling laws, with Massachusetts enacting the first such law in 1647. This law, called the Old Deluder Satan Act, was meant to provide basic literacy to everyone, as the early Puritan settlers put great value on each individual being able to read and interpret the Bible for themselves. Cohorts born in 1890 in the New England states generally had to complete at least 6 years of compulsory schooling and, in some cases, up to 9 years. By the time the cohorts born in 1900 were in school, only Southern states did not yet require children to attend any compulsory schooling. The last cohort in our sample, born in 1905, was subject to some form of compulsory schooling in all states except Louisiana, South Carolina, and Virginia. By this time, the norm in most states was 8 years of compulsory schooling.

2.2 Full Count Census Data

Table 1: Summary Statistics

	Parents	Children
Observations	9,756,597	9,382,509
Minimum Birth Year	1880	1896
Maximum Birth Year	1905	1921
Compulsory Schooling (Years)	4.0	7.2
Completed Schooling (Years)	8.1	10.2
Proportion Black	8.9%	10.0%
Proportion Female	60%	45%
Proportion Urban	50%	50%
Proportion Married	90%	10%
Age	48.4	22.4
Labor Force Participation Rate	50%	70%
Unemployment Rate	5.6%	16.9%
Unemployment Duration (Weeks)	89	53
Yearly Labor Earnings (\$)	1,314	627
Weekly Hours Worked	39	36
Percent Own Home	50%	50%
Home Value (\$)	3,291	3,241
Monthly Rent (\$)	67	66

Summary statistics for the matched *Parents* and *Children* samples used in this paper.

Our key question of interest is whether changes in compulsory schooling laws had *inter-generational* effects on completed education. To this end, we use linked census data from 1880 to 1940, allowing us to track individuals affected by the introduction of compulsory

schooling laws in the late 1800s and early 1900s, link them to their children, and observe how parental exposure to compulsory schooling laws affected outcomes of their children.³

The 1940 census is the most recent full-count census available at the time of writing and the first one to ask questions on educational attainment. We focus on individuals aged over 18 in 1940 and use 1880 to 1940 census linkages constructed by Ruggles et al. (2019) to identify individuals across census waves. Measuring parental exposure to compulsory schooling requires data on the birth year and birth state of the parents of the “children” in the 1940 Census. For the vast majority of individuals, this information can only be ascertained by making use of cross-walks that link respondents across consecutive censuses (for example, between 1940 and 1930), as most of the “children”, when they are adults, no longer co-reside with their parents. Since parent-children links between respondents can only be identified if the respondents are part of the same household, we identify the parents of 1940 “children” in at least one of the 1940 to 1880 censuses, using the moment in time when they were still co-residing. Survey items from the censuses then allow us to determine the year of birth and state of birth of the parents of the 1940 respondents that we are able to link in this way. This, in turn, enables us to determine parental exposure to compulsory schooling, using the compulsory schooling law dataset described in the previous section 2.1.

The linked census data offer several advantages in studying the intergenerational effects of compulsory schooling laws. First, the very large sample sizes help to increase the precision of estimated effects beyond what might be offered in survey data. Second, the census data are very rich. We explore a multitude of outcomes, from years of schooling to marriage and family structure, occupational, employment, and other labor-market outcomes, to name a few. Third, because we can track individuals across time, we can observe changes in their outcomes across census waves. In particular, we explore geographic mobility across census tracts from one census wave to another, and we zero in on particular ages (e.g., early adulthood) when these changes are most likely to happen.

We build two main samples of interest:

1. the *Children* sample: contains all 9,382,509 individuals in the 1940 Census born in one of the 48 continental states, or D.C., who are at least 18 years old and who have at least one identified parent in the *Parent* sample below.
2. *Parents* sample: contains all 9,756,597 parents of the individuals in the *Children* sample who were born between 1880 and 1905 in one of the 48 continental states, or D.C., and were aged at least 16 when their child was born.

Table 1 presents some basic summary statistics on demographics, education, and selected labor-market outcomes for the two samples of interest.

Of particular note is the average education level of the children (10.2 years of schooling), which is significantly higher than that of parents (8.1 years). This highlights how this era was defined by rapid increases in educational attainment across generations. Females are under-represented in our *Children* sample (45%). This may arise because of difficulties in matching women across censuses when their last names change as a result of marriage. Women are slightly over-represented in our *Parents* sample, and there may be several reasons for this:

³Note that the 1890 census is not available, as the population schedules were lost in a fire.

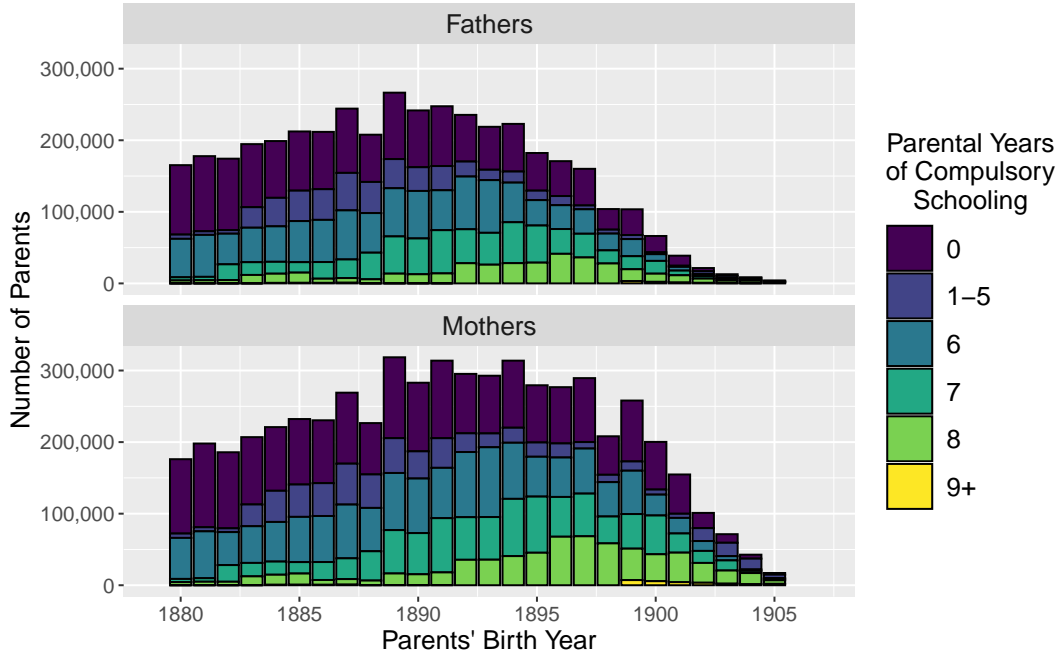


Figure 2: US-born parents by birth year and own exposure to compulsory schooling in the *Parent* sample.

mothers are on average younger than fathers, they have a higher life expectancy, and are more likely to live with their children in case of separation. For these reasons, it is easier to link mothers to their children.

Figure 2 shows the distribution of birth years and exposure to compulsory schooling of the parents in the *Parent's* sample, for fathers (top-panel) and mothers (bottom-panel), separately. As the Figure shows, the census allows us to link tens of thousands to several hundred thousands of parents in each birth year cohort to their children. Most of our parents are born in the 1880 to 1900 period, with some younger ones being born as late as 1905. Parents born between 1880 and 1900 were ages 41 to 61 in 1940, a prime age range for having adult children in the 1940 census. The exposure of these parents to compulsory schooling varies significantly, both within and across cohorts.

We restrict the *Parent* sample to those born after 1880 because most of our parent-child matches come from the 1940 census. Incorporating older parents observed in the later censuses may lead to bias since they are likely to be positively selected on health, but may be disproportionately exposed to older legal regimes requiring fewer years of CS. At the other end of the age distribution, parents born after 1905 are too young to have adult children in 1940.

Figure 3 shows the distribution of birth years and *parental* exposure to compulsory schooling for the *Children's* sample. The children's sample is born between 1896, when their 1880-born parents were 16, and 1921, after which 1940 respondents are too young to be adults in the 1940 census. The Figure shows that parents of children in every cohort experienced exposure to compulsory schooling ranging from no compulsory schooling to 9 years and more. Further, the share of children exposed to more parental compulsory schooling increases with

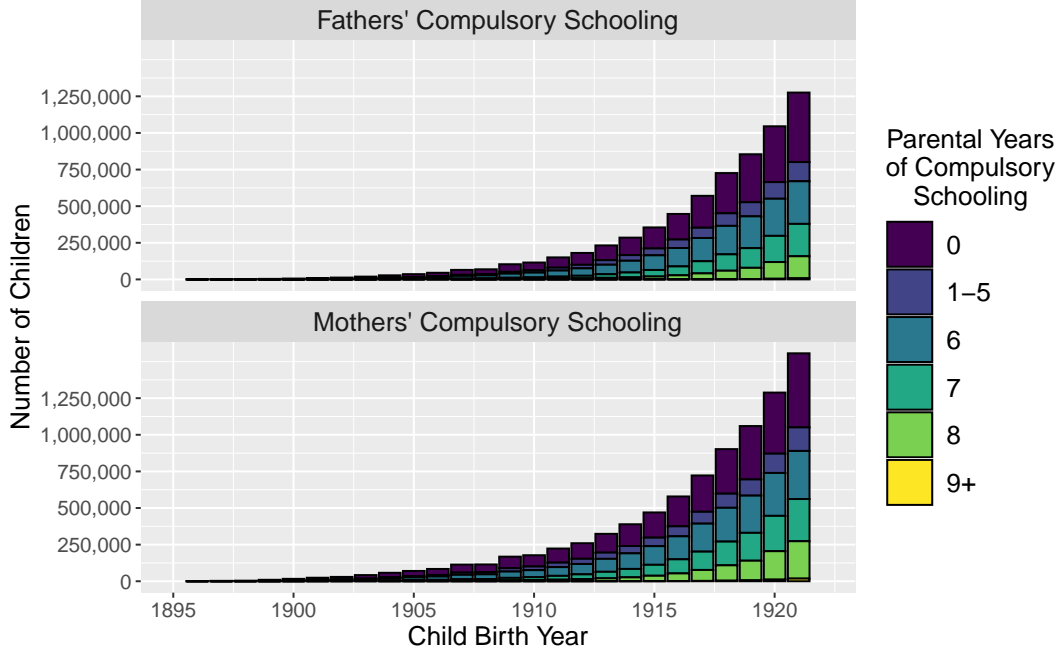


Figure 3: Children's exposure to parental years of compulsory schooling by child birth year.

each cohort.

3 Empirical Strategy

3.1 Difference-in-Differences

Our main empirical strategy consists of two estimating equations. The first relates the parental (p) years of schooling ($Educ_i^p$) of parent i to the number of years of compulsory schooling ($CS_{s'y'}^p$) required of their birth state (s') birth year (y') cohort:

$$Educ_i^p = \beta^p CS_{s'y'}^p + \gamma_{s'}^p + \delta_{y'}^p + (\eta_{r'}^p \times \theta_{y'}^p) + \lambda^p Race_i^p + \mu^p Sex_i^p + \epsilon_i^p, \quad p = m, f \quad (1)$$

where we include vectors of fixed effects for i 's state of birth (s') and birth year (y') cohort ($\gamma_{s'}^p$ and $\delta_{y'}^p$, respectively), interactions ($\eta_{r'}^p \times \theta_{y'}^p$) between parent i 's region (r') of birth ($\eta_{r'}^p$)⁴ and birth year (y') cohort ($\theta_{y'}^p$), as well as controls for parent i 's race (λ^p) and sex (μ^p). The effect β^p of parental exposure to compulsory schooling laws $CS_{s'y'}^p$ is identified from variation across states of birth (s') and birth year (y') cohorts, conditional on regional trends (captured by the region and birth year cohort interactions $\eta_{r'}^p \times \theta_{y'}^p$), state differences in levels (captured by state fixed effects, $\gamma_{s'}^p$) and cohort differences in levels (captured by birth year cohort fixed effects, $\delta_{y'}^p$). These analyses use the *Parents* sample and estimate separate effects for mothers' and fathers' exposure to CS.

Our main focus is on the intergenerational effects of exposure to compulsory schooling laws. Therefore, the second estimating equation relates the child's (c) years of schooling

⁴West, Southwest, Midwest, Southeast and Northeast.

($Educ_i^c$) to the compulsory schooling exposure ($CS_{s'y'}^p$) of the child's parents:

$$\begin{aligned}
 Educ_i^c &= \beta^c CS_{s'y'}^p + \gamma_s^c + \delta_y^c + (\eta_s^c \times \theta_y^c) + \gamma_{s'}^p + \delta_{y'}^p + (\eta_{r'}^p \times \theta_{y'}^p) \\
 &+ \lambda^c Race_i^c + \mu^c Sex_i^c + \epsilon_i^c, \quad p = m, f
 \end{aligned} \tag{2}$$

where, analogous to Equation 1, we include vectors of fixed effects for the child's state of birth s (γ_s^c) and birth year y (δ_y^c), and interactions ($\eta_s^c \times \theta_y^c$) between the child's state of birth (η_s^c) and birth year (δ_y^c), as well as controls for the child's race (λ^c) and sex (μ^c).

Unlike Equation 1, we control for children's birth state and birth year trends, as opposed to trends by region r . These controls capture state birth year effects, such as children's own exposure to compulsory schooling. We are able to introduce these controls because children's birth states and birth years are not co-linear with parental exposure to compulsory schooling.

These state-year controls are important because children often live in the same state as their parents, so child exposure to compulsory schooling is likely correlated with that of their parents. Indeed, as Figure 4 demonstrates, children whose parents were exposed to 9 or more years of compulsory schooling, are almost 50% more likely to be themselves exposed to that same level of compulsory schooling. Meanwhile, fewer than 10% of children whose parents were not exposed to any compulsory schooling received 9 or more years of compulsory schooling. Thus, omitting child-level state and birth-year controls would bias our results, as parental exposure to compulsory schooling also captures the effects of children's own exposure to compulsory schooling.

Further, we include vectors of fixed effects for the parent's state of birth s' ($\gamma_{s'}^p$) and parent's birth year y' ($\delta_{y'}^p$), and interactions ($\eta_{r'}^p \times \theta_{y'}^p$) between the parents' region (r') of birth ($\eta_{r'}^p$) and birth year ($\delta_{y'}^p$). Therefore, the parameter of interest, β_c , is identified, across children born in the same states and years, via variation in their parents' birth states and birth years.

These analyses use the *Children* sample and estimate effects separately for mothers (m) and fathers (f). The effect of parental exposure to compulsory schooling laws β^c on the child is here identified across children who live in the state and are born in the same year, but whose parental exposure to compulsory schooling - which varies at the parental state of birth s' and parental year of birth y' level - varies.

Our main specification helps us address three main identification challenges. First, compulsory schooling laws are persistent over time (states rarely reduce their level of compulsory schooling). Thus, the measured effect of parental exposure to these laws may simply be picking up children's exposure to similar laws. Indeed, Figure 4 demonstrates that parents' and children's exposure to compulsory schooling is highly correlated. By controlling for interactions between the children's birth state s and birth year y , we take into account the effects of children's own exposure to CS laws, allowing us to separately measure the effects of parental exposure to CS.

The second challenge is highlighted by Stephens Jr and Yang (2014). This study finds that the standard assumption of common trends across states is generally not valid: controlling for birth region and birth year fixed effects interactions, to allow for differential changes across states, most of the effects of compulsory schooling laws on various outcomes (ranging from health to educational and labor-market outcomes) become insignificant. In other words, the effects measured in the CS literature may be driven by regional (and not state-specific)

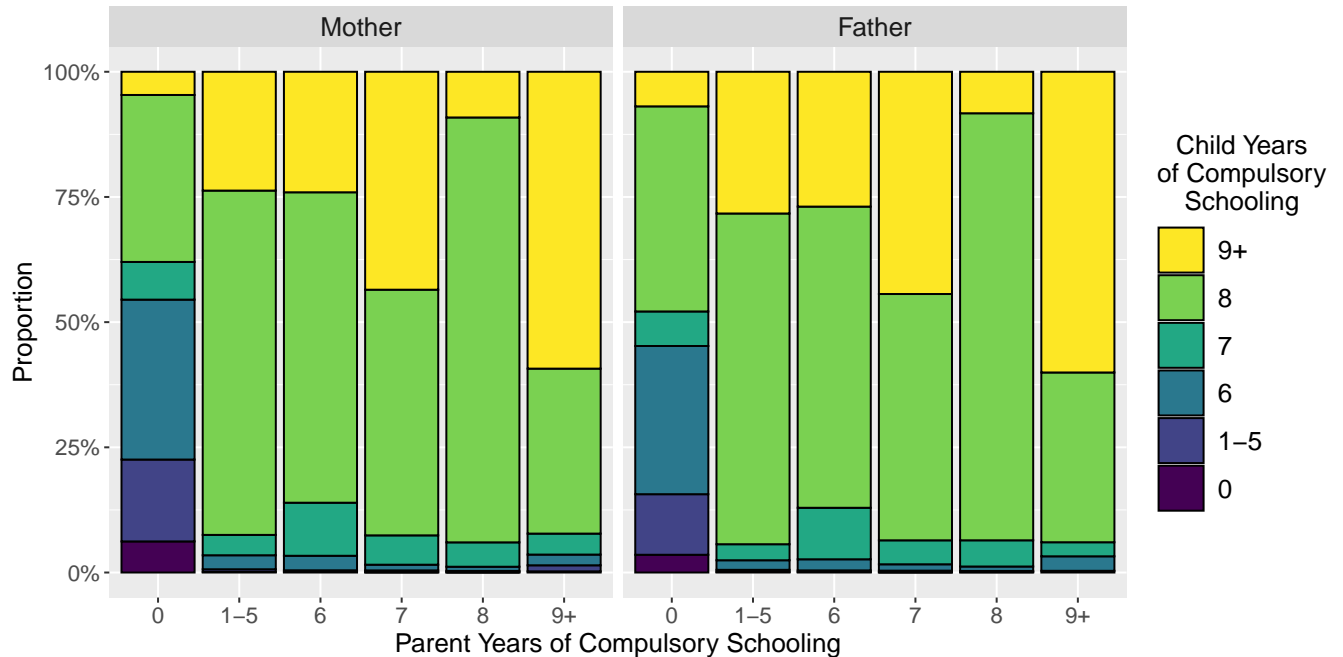


Figure 4: Relationship between parental years of compulsory schooling (horizontal axis) and proportion of child exposure to compulsory schooling (vertical axis; color-coded years of compulsory schooling), separate for mothers (left) and fathers (right).

time trends, which are then incorrectly attributed to CS laws. To address this, we control in equation 1 for region of birth interactions, and in equation 2 for both parent birth region (r') and birth-year cohort (c') interactions, as well as child birth state (s) and child birth year (y) interactions. Moreover, we cluster the standard errors conservatively, using two-way birth state and birth-year cohort clustering.⁵ Our results are robust to the inclusion of these rich sets of controls.

Last, in section 7.1, we address possible issues with the two-way fixed effects (TWFE) specification used in this paper, as per De Chaisemartin and d'Haultfoeuille (2020), Callaway and Sant'Anna (2021), Goodman-Bacon (2021) and Sun and Abraham (2021).

3.2 Instrumental Variable

We also set up an alternate instrumental variable specification, in which we use compulsory schooling exposure as an instrument for parental education in 1940, and use this to predict the child's education. The advantage of this approach is that it allows us to compare our results to those in the literature, in particular, those of Black, Devereux and Salvanes (2005). However, a shortcoming is that the exclusion restriction is probably violated. Indeed, parental exposure to compulsory schooling may affect children's education through other channels than purely parental education. Because compulsory schooling laws affect many cohorts and

⁵We cluster equation 1's standard errors at the less conservative birth year and birth state levels for consistency with the literature.

entire cohorts of parents, this may cause spillovers and may have general equilibrium effects on, e.g., labor markets. Nonetheless, the instrumental variable approach has a very natural interpretation, causally linking increases in education to increases in children’s education. It, therefore, provides a useful point of reference.

In this approach, the first stage relates education $Educ_i^p$ of parent p born in state s' and year y' to their own exposure to compulsory schooling $CS_{s'y'}^p$:

$$Educ_i^p = \beta^p CS_{s'y'}^p + \gamma_{s'}^p + \delta_{y'}^p + (\eta_{r'}^p \times \theta_{y'}^p) + \lambda^p Race_i^p + \mu^p Sex_i^p + \epsilon_i^p \quad p = m, f \quad (3)$$

In the second stage, we used the fitted parental education \widehat{Educ}_i^p to predict children’s educational attainment:

$$Educ_i^c = \beta^c \widehat{Educ}_i^p + \gamma_s^c + \delta_y^c + (\eta_r^c \times \theta_y^c) + \lambda^c Race_i^c + \mu^c Sex_i^c + \epsilon_i^c \quad p = m, f \quad (4)$$

A last potential drawback of this specification is that years of schooling are only reported starting with the 1940 census. Thus, there is a significant drop in sample size, but also a potential selection issue, as this specification relies mostly on parents living with their adult children in 1940 and to a lesser extent, parents who are identified in previous censuses and are linked back to the 1940 census. For comparability, we also use a specification in which we limit the fixed effects to those used by Black, Devereux and Salvanes (2005): parent’s place of residence (county) and year of birth, and child’s year of birth.

4 Main Effects of Compulsory Schooling Laws

In this section, we present the main effects of exposure to CS laws on educational outcomes. First, we present the results of one’s own exposure to CS laws on one’s own years of schooling (Equation 1) in the *Parents* sample (section 4.1). Second, we explore the effectiveness of CS laws (section 4.2) by focusing on the relatively small share of the population that was affected by these laws, demonstrating that the seemingly small average effects we find are in fact substantial. Third, we explore the effects of exposure to compulsory schooling laws on enrollment in and graduation from grade, middle, and high school, as well as college (section 4.3). Fourth, we explore the intergenerational effects of parental exposure to CS laws on their offspring’s schooling (Equation 2) in the *Children* sample (section 4.4). Fifth, in section 4.5, following Black, Devereux and Salvanes (2005), we instrument parental years of schooling in 1940 with the parent’s exposure to compulsory schooling laws, to get a sense for the potential size of the intergenerational effects of parental exposure to CS on the offspring. Sixth, in section 4.6 we explore the effects of parental exposure to CS on offspring enrollment and degree completion. Last, in section 4.7 we present the results of an analysis of heterogeneity in outcomes for different types of children (e.g., first-born males).

4.1 Effects of Own Compulsory Schooling (Parents sample)

Table 2 presents estimates of the effect of compulsory schooling laws on own years of schooling for individuals directly exposed to them in the *Parents* sample (equation 1). One additional year of CS exposure is associated with a 0.008 and 0.005 increase in women’s and men’s years of schooling, respectively. The largest effects on years of schooling are found for Blacks

Table 2: Effect of Own Exposure to Compulsory Schooling on Years of Schooling

	<i>Dependent variable: Years of Schooling</i>			
	All	White	Black	Post-1900
CS Years (Women)	0.008*** (0.003)	0.007** (0.003)	0.038*** (0.008)	0.044*** (0.010)
N (millions)	5.5	5.0	0.5	0.6
R ²	0.15	0.06	0.10	0.17
Outcome Means	8.1	8.4	5.4	7.7
CS Years (Men)	0.005* (0.003)	0.004 (0.003)	0.046*** (0.011)	0.093*** (0.021)
N (millions)	4.0	3.6	0.3	0.1
R ²	0.15	0.07	0.08	0.24
Outcome Means	8.0	8.3	4.7	7.5

Notes: Effects of exposure to compulsory schooling laws on years of schooling for the *Parents* sample. Each column represents a different regression. Controls include birth year, birth state, birth region, birth region by birth year, sex and race fixed effects, when possible. Standard errors are clustered at the birth state by birth year level. * p<0.1; ** p<0.05; *** p<0.01.

(more than five and nine times larger than the average effect size for women and men, respectively). This may be because compulsory schooling was more binding for this demographic and because a large proportion lived in the South, where compulsory schooling laws were implemented later (in the early twentieth century) and were plausibly more effective. This increased effectiveness was mainly due to an increase in state government capacity and bureaucratization, which allowed proper enforcement of these schooling laws (Katz, 1976).

4.2 Effectiveness of CS laws on own schooling (*Parent* sample).

To put these seemingly small effects into perspective, we must consider that for many high-achieving students, the CS laws had no effect as these students would already have stayed in school for longer, regardless of the law. Indeed, typically only a small fraction of students had educational attainment levels lower than what was mandated by the CS law in their area at the time the law was in effect and thus only a small fraction of students had to remain in school longer than desired.

In addition, a 6-year compulsory schooling law would not induce all individuals to stay in school for an extra six years. For example, an individual with a desired educational attainment of five years would only be induced to stay in school for one additional year. These two reasons explain why, even if the CS laws were well-enforced, the expected estimates in Table 2 would be small. A third reason then is that CS laws were not always effectively enforced.

To get a better sense of the magnitude of our estimates and to understand the scope and effectiveness of the CS laws, we first attempt to characterize the population for whom these

Table 3: Compulsory Schooling Law Effectiveness

	All	White	Black	Post-1900
Proportion Under CS Years (p.p.)	12.5	12.0	23.0	18.4
Average Schooling Deficit (Yrs)	2.3	2.2	2.5	2.2
Women Actual Effect of CS Exposure (Yrs)	0.008***	0.007**	0.038***	0.044***
Potential Effect of CS Exposure (Yrs)	0.099	0.090	0.187	0.174
Effectiveness (Actual/Potential)	8%	8%	20%	26%
Proportion Under CS Years (p.p.)	12.8	12.2	26.2	21.0
Average Schooling Deficit (Yrs)	2.4	2.3	2.6	2.3
Men Actual Effect of CS Exposure (Yrs)	0.005*	0.004	0.046***	0.093***
Potential Effect of CS Exposure (Yrs)	0.109	0.100	0.261	0.255
Effectiveness (Actual/Potential)	5%	4%	18%	36%

Notes: Effects of exposure to compulsory schooling laws on years of schooling for the *Parents* sample. Each column represents a different regression. Controls include birth year, birth state, birth region, birth region by birth year, sex and race fixed effects, when possible. Standard errors are clustered at the birth state by birth year level. * p<0.1; ** p<0.05; *** p<0.01.

laws were binding and then simulate what the policy effects would have been under perfect enforcement of the CS laws.

Although we cannot observe desired years of schooling of individuals, we can estimate the fraction of students that would have preferred to stay in school for fewer years than what was mandated, by measuring the proportion of a cohort that was not yet exposed to the CS law and that had fewer years of schooling completed in adulthood than what was subsequently mandated. For example, if a state passed a new 6-year CS law affecting cohorts born after 1900, the relevant marginal individuals are those of the 1899 pre-reform cohort that had fewer than 6 years of schooling completed in adulthood (in 1940).

From these pre-reform cohorts, we extract two pieces of information relevant to the reform: i) the fraction of individuals who had lower than mandated educational attainment levels and ii) how many years of schooling these undereducated individual had compared to the reform’s mandated minimum. For example, the 1899 cohort may have 10% of individuals who received less than 6 years of schooling and, on average, these individuals may have an average achievement level of 4 years of schooling (that is to say, a 2-year schooling deficit with respect to the reform). Both these data points tell us how ambitious these schooling laws were and what the potential gains from these policies were.

We aggregate this information for all schooling laws in order to get as sense of the scope of the schooling reforms. We estimate in Table 3 that at the time CS laws came into effect, an average of only 12.8% of men and 12.5% of women had educational attainment levels that were under the mandated minimum. This percentage was higher for Black men (26.1%) and Black women (23.0%). The non-compliant, low-attainment individuals received typically 2.3 to 2.4 fewer years of schooling than the minimum schooling levels demanded, with Black Americans more than 2.5 fewer years than the minimum schooling levels demanded. The remaining individuals were already compliant with the schooling laws before they came into

effect.

Thus, only a small section of the population was bound by by CS laws. These are the individuals whose desired years of schooling were lower than the legally-mandated years of schooling. Therefore, the estimates in Table 2 on these marginal individuals are underestimated (because they represent an average over the entire population).

To better understand the *effectiveness* of the schooling laws in producing schooling gains, we measure them against *potential* schooling gains. To do this, we perform the following simulation exercise, which effectively imposes on all individuals in our data that they stay in school up to at least the mandated years of compulsory schooling in their state s' for their specific cohort y' .

We do this as follows. First, in the *Parent* sample, we set the years of schooling $Educ_i^p$ to the minimum mandated years of compulsory schooling $CS_{s',y'}^p$ in their state s' and cohort y' for those who in actuality completed fewer years of schooling $Educ_i^p$ than were mandated $CS_{s',y'}^p$. At the same time, we leave the years of schooling $Educ_i^p$ the same for those that completed more than the minimum mandated years of compulsory schooling $CS_{s',y'}^p$. Second, we re-estimate the main effect of own exposure to compulsory schooling on these simulated years of schooling (Equation 1).

In this way, we obtain an estimate of the maximum *potential* schooling gains from CS under perfect enforcement of schooling laws.⁶ This provides us with a theoretical yardstick against which we can measure the actual effects of CS laws on educational attainment. This also provides us with an answer to the question “*Had enforcement been perfect, by how many years of schooling would the average level of education be increased?*”.

These potential schooling gains are shown in Table 3. Even under perfect enforcement, the schooling laws would have produced at most an average of a 0.099 and 0.109 years of schooling gain for men and women. Comparing our estimated gains from Table 2 to these potential gains, we estimate that the schooling laws were roughly 8% effective for women and 5% effective for men. These laws were, however, more effective in increasing the educational attainment for Black Americans of both sexes and for people born after 1900, especially men. This was influenced by increased bureaucratization and state capacity of state governments, who became more able and willing to enforce new and existing compulsory schooling laws after the turn of the century.

4.3 Enrollment and Degree Completion.

Table 4 shows that the main effect of compulsory schooling laws was to increase enrollment in and graduation from grade school, as well as enrolment into middle school.⁷ We define a set of indicator variables $Attainment_i^\ell$ that take a value 100 if an individual ever reaches educational level ℓ , and 0 otherwise so that the estimates are in percentage points. We consider degree outcomes $\ell \in \{\text{Some Grade School, Grade School, Some Middle School, Middle School, Some High School, High School, Some College, and College}\}$. One additional year of maternal compulsory schooling exposure increased the probability of attending grade

⁶The assumption here is that more stringent enforcement does not induce people to stay in school longer than mandated.

⁷We define grade school as grades 1 through 6, middle school as grades 7 through 9, and high school as grades 10 through 12.

Table 4: Effect of Compulsory Schooling Laws on Own Educational Attainment and Degree Completion

	<i>Dependent Variables: Entry, Completion (p.p.)</i>							
	Some GS	Grade School (GS)	Some MS	Middle School (MS)	Some HS	High School (HS)	Some College	College
CS Years (Moms)	0.029** (0.011)	0.121*** (0.032)	0.090** (0.037)	0.048 (0.043)	0.048 (0.035)	0.053* (0.032)	0.026 (0.018)	0.033*** (0.009)
N (millions)	5.5	5.5	5.5	5.5	5.5	5.5	5.5	5.5
R ²	0.07	0.17	0.17	0.05	0.03	0.03	0.01	0.00
Outcome Means	97.9	81.9	74.1	33.9	21.2	18.3	6.5	2.0
CS Years (Dads)	0.061*** (0.012)	0.120*** (0.034)	0.055 (0.041)	-0.066 (0.046)	-0.023 (0.037)	0.009 (0.034)	-0.019 (0.019)	0.037** (0.014)
N (millions)	4.0	4.0	4.0	4.0	4.0	4.0	4.0	4.0
R ²	0.06	0.19	0.18	0.05	0.03	0.03	0.01	0.01
Outcome Means	97.4	79.5	71.6	30.8	19.9	17.5	8.6	4.3

Notes: Effect of exposure to compulsory schooling on entry and completion of various schooling levels for individuals in the Parent sample. Each column represents a different regression. Dependent variables are coded as 0 (education level not attained) or 100 (education level attained) so that the regression coefficients can be interpreted as percentage point increases in entry and completion. Controls include birth year, birth state, birth region, birth region by birth year, sex and race fixed effects, when possible. Standard errors are clustered at the birth state by birth year level *p<0.1; **p<0.05; ***p<0.01.

school, graduating from grade school, attending some middle school, and completing college by 0.03, 0.12, 0.09, and 0.03 p.p., respectively. Fathers' exposure led to increases of 0.06 and 0.12 p.p. in grade school attendance and graduation, respectively, and 0.04 p.p. in completing college. These results are consistent with the compulsory schooling laws of the early twentieth century, which imposed between 6 and 9 years of mandatory schooling, explaining weaker results for high school (though still for college). Interestingly, our results suggest that CS laws were more effective on the intensive than on the extensive margin: the effect was to encourage those who were enrolled in school to pursue more years of schooling, rather than inducing students who never attended school to enroll in the first place.

Note that our results are robust to the Stephens Jr and Yang (2014) critique, who found that causal estimates of the benefits of compulsory schooling, which tended to rely on the assumption of common trends across regions, were not robust to allowing for such trends to differ across regions. When including region fixed effects and region by birth year interactions, the compulsory schooling laws have statistically significant effects on years of schooling. We cluster standard errors at the birth state by birth year level, following Abadie et al. (2022).⁸

⁸The authors show that when estimating effects on the entire population, and not just on a subsample of individuals drawn from a subsample of clusters, clustering conservatively leads to unnecessarily and very large confidence intervals that do not shrink even when sample sizes are large. Moreover, in our setup, treatment assignment is at the birth state by birth year level, which further reduces the need to cluster at the state level.

Table 5: Effect of Parental Exposure to Compulsory Schooling on Children’s Years of Schooling

<i>Dependent Variable: Child’s Years of Schooling</i>						
	All	Men	Women	White	Black	Post-1900
CS Years (Mom)	0.015*** (0.003)	0.016*** (0.003)	0.015*** (0.004)	0.014*** (0.004)	0.021** (0.008)	0.018* (0.009)
N (millions)	8.3	4.8	3.6	7.5	0.8	0.7
R ²	0.19	0.20	0.16	0.10	0.15	0.20
Outcome Means	10.3	10.0	10.7	10.6	7.5	9.5
CS Years (Dad)	0.016*** (0.003)	0.018*** (0.003)	0.012*** (0.004)	0.015*** (0.003)	0.039*** (0.009)	0.059*** (0.023)
N (millions)	5.5	3.2	2.3	5.0	0.5	0.2
R ²	0.18	0.19	0.15	0.11	0.15	0.20
Outcome Means	10.4	10.1	10.8	10.6	7.7	9.5

Notes: Effect of parental exposure to compulsory schooling laws on years of schooling of the child. Each column represents a different regression. Controls include parent birth year, birth state, birth region, birth region by birth year and race fixed effects and child birth year, birth state, birth state by birth year, sex and race fixed effects. Standard errors are clustered at the child’s birth state by birth year level. * p<0.1; ** p<0.05; *** p<0.01.

4.4 Intergenerational Effects of Parental Exposure to Compulsory Schooling on Child Schooling

We now turn to the intergenerational effects of compulsory schooling. The successive columns of Table 5 provide estimates of the effect of parental exposure to compulsory schooling on years of education of the child using our main specification (Equation 2) in five sub-samples of interest: all individuals, men, women, White Americans, Black Americans, and those born after 1900. Effects are significant and generally larger in magnitude than the effects of compulsory schooling on parents’ own educational attainment, except for Blacks and Post 1990 (compare with Table 2). The largest intergenerational effects of parental exposure to CS are for the offspring of Black American fathers and for offspring born after 1900 of fathers that were exposed to more CS. In section 7.1, we explore our two-way fixed effects estimator to understand how it is identified and if it suffers from any of the issues highlighted in the recent difference-in-difference literature.

4.5 Intergenerational Effects of Parental Exposure to Compulsory Schooling on Child Schooling (IV approach)

Following Black, Devereux and Salvanes (2005), we estimate the causal effects on child years of schooling of parental exposure to compulsory schooling by instrumenting parental years of

Table 6: Effect of Parental Years of Schooling on Children’s Years of Schooling (IV Second Stage)

	<i>Dependent Variable: Child’s Years of Schooling</i>						
	Black et al.	All	Men	Women	White	Black	Post-1900
Years of Schooling (Mom)	1.089*** (0.005)	0.930*** (0.132)	1.014*** (0.162)	0.833*** (0.139)	0.951*** (0.149)	0.595*** (0.206)	0.379** (0.171)
N (millions)	8.3	8.3	4.7	3.5	7.4	0.8	0.7
R ²	0.00	0.18	0.15	0.19	0.05	0.33	0.35
First Stage F-stat	387,238.3	257.3	135.8	122.5	210.5	31.0	32.5
Outcome Means	10.3	10.3	10.0	10.7	10.6	7.5	9.6
Years of Schooling (Dad)	0.848*** (0.003)	1.044*** (0.199)	1.188*** (0.243)	0.826*** (0.216)	1.061*** (0.227)	0.866*** (0.211)	0.613*** (0.215)
N (millions)	5.8	5.9	3.4	2.5	5.4	0.5	0.2
R ²	0.08	-0.14	-0.27	0.02	-0.33	0.16	0.25
First Stage F-stat	286,739.8	119.4	68.2	49.4	93.8	27.9	23.2
Outcome Means	10.4	10.4	10.1	10.8	10.7	7.7	9.3

Notes: Effect of parental years of schooling on years of schooling of the child using an instrumental variable approach, where parental compulsory schooling exposure is used as an instrument for parental education. Each column represents a different regression. Controls include parent birth year, birth state, birth region, birth region by birth year and race fixed effects and child birth year, birth state, birth state by birth year, sex and race fixed effects, except for the first column, where controls are child birth year and county of residence, parent birth year. Standard errors are clustered at the child’s birth state by birth year level, except for the first column, where they are clustered at the county-by-parent birth year level. * p<0.1; ** p<0.05; *** p<0.01.

schooling in 1940 with the parent’s exposure to compulsory schooling laws.⁹ Table 6 presents the results. Column 1, replicates the exact specification used by Black, Devereux and Salvanes (2005). The other columns show our own specifications with additional controls (afforded by our very large Census sample sizes) for different demographic groups and time periods. Across various specifications and samples, the effects are an order of magnitude larger than those of Black, Devereux and Salvanes (2005), who found essentially no or weak effects¹⁰ for a 1960 reform in Norway and concluded, as a result, that the strong intergenerational correlation in education between parents and offspring reflects selection rather than causal effects of parental education (at least in that setting).

Remarkably, we find instead very large causal effects. A one-year increase in maternal (paternal) education resulting from exposure to compulsory schooling increases children’s schooling by 1.09 (0.85) years using the Black, Devereux and Salvanes (2005) specification. When using the full set of fixed effects from our main specification, the effects are 0.93 (1.04). They range from 0.60 years for the effects of maternal exposure to CS for Black American children to 1.19 years for paternal exposure to CS of men.

These large effects may arise because the compulsory schooling laws in the United States

⁹First stage results are presented in Table 16 of the Appendix. All first-stage F-stats are highly significant, ruling out weak instruments.

¹⁰The effects are significant only for low education mother-son pairs. The coefficient linking mothers’ instrumented years of schooling to sons’ years of schooling is 0.11.

in this era, unlike Norway, mainly targeted students who had between zero and six years of schooling at a time when levels of schooling were very low. Some of these parents might otherwise not have completed any schooling. This is in stark contrast with the Norwegian context, where the studied reform took place in the 1960s and increased compulsory schooling from 7 to 9 years at a time when the population was relatively highly educated. Period differences in the returns to additional schooling may also play a role. In Section 5 we explore some of the potential pathways through which these large educational effects may have operated.

Table 7: Effect of Compulsory Schooling Laws on Child’s Educational Attainment

	<i>Dependent Variables: Entry, Completion (p.p.)</i>							
	Some GS	Grade School	Some MS	Middle School	Some HS	High School	Some College	College
CS Years Mom	0.012*** (0.003)	0.063*** (0.010)	0.062*** (0.011)	0.191*** (0.034)	0.231*** (0.048)	0.247*** (0.057)	0.106** (0.043)	0.043** (0.021)
N (millions)	8.3	8.3	8.3	8.3	8.3	8.3	8.3	8.3
R ²	0.01	0.15	0.18	0.14	0.13	0.12	0.04	0.04
Outcome Means	99.2	92.6	89.0	71.2	55.9	46.8	14.3	3.6
CS Years Dad	0.006 (0.004)	0.062*** (0.014)	0.084*** (0.017)	0.166*** (0.035)	0.191*** (0.043)	0.214*** (0.052)	0.157*** (0.042)	0.077*** (0.025)
N (millions)	5.5	5.5	5.5	5.5	5.5	5.5	5.5	5.5
R ²	0.01	0.13	0.16	0.13	0.12	0.13	0.05	0.04
Outcome Means	99.3	93.5	90.3	73.6	58.4	48.8	14.7	3.4

Notes: Effect of parental exposure to compulsory schooling on entry and completion of various schooling levels by their offspring for the *Children* sample. Each column represents a different regression. Dependent variables are coded as 0 (education level not attained) or 100 (education level attained) so that the regression coefficients can be interpreted as percentage point increases in entry and completion. Controls include parent birth year, birth state, birth region, birth region by birth year and race fixed effects and child birth year, birth state, birth state by birth year, sex and race fixed effects. Standard errors are clustered at the child’s birth state by birth year level. *p<0.1; **p<0.05; ***p<0.01.

The results in Tables 5 and 6 also raise the question of which margin of schooling was affected by parental exposure to compulsory schooling laws. It could be that the effects of parental exposure to CS on the schooling of the child are largely confined to the lower end of the distribution of parental educational attainment. This could arise if, for example, parental educational attainment establishes a floor for the expected educational attainment of the children. Parents may, for example, wish to ensure their children obtain at least as much formal schooling as they themselves were legally obligated to obtain. Alternatively, an increase in required schooling could increase the chances that children obtain substantially more educational attainment than their parents. For example, higher levels of compulsory education could increase the value that parents place on educational success, as argued by Piopiunik (2014) in the German context. We next test the children’s educational attainment margins affected by higher levels of parental compulsory schooling exposure. As before, we define a set of indicator variables $\text{Attainment}_i^{\ell}$ that take a value 100 if an individual ever

reaches educational level ℓ , and 0 otherwise, so that the estimates are in percentage points. We consider degree outcomes $\ell \in \{\text{Some Grade School, Grade School, Some Middle School, Middle School, Some High School, High School, Some College and College}\}$.

4.6 Effects of Parental Exposure to CS on Offspring Enrollment and Degree Completion.

Table 7 shows the effects of parental exposure to CS on children’s enrolment and degree completion. Parental exposure to compulsory schooling had positive effects on children entering a schooling level (Some GS, Some MS, Some HS, Some College) and completing it (Grade School, Middle School, High School, College) across the entire distribution of educational attainment. The largest effects were on completing middle school (Middle School), attending (Some HS) and completing high school (High School), and attending college (Some College), with an extra year of maternal exposure to compulsory schooling increasing the probability of these outcomes by between 0.11 and 0.25 percentage points. The effects of paternal exposure to compulsory schooling are remarkably similar to those of maternal exposure.

4.7 Family-Level Effects

Table 8: Heterogeneity between Children in the Effects of Parental Exposure to Compulsory Schooling Laws

	<i>Dependent Variable: Years of Schooling</i>							
	Son				Daughter			
	Eldest	Youngest	Most Educated	Least Educated	Eldest	Youngest	Most Educated	Least Educated
CS Years (Mom)	0.018*** (0.003)	0.015*** (0.003)	0.018*** (0.003)	0.015*** (0.003)	0.012*** (0.003)	0.011*** (0.002)	0.011*** (0.002)	0.012*** (0.002)
N (millions)	3.8	3.8	3.8	3.8	3.0	3.0	3.0	3.0
R ²	0.17	0.18	0.18	0.18	0.14	0.14	0.14	0.15
Outcome Means	10.1	10.0	10.2	9.9	10.7	10.7	10.8	10.6
CS Years (Dad)	0.023*** (0.003)	0.021*** (0.003)	0.022*** (0.003)	0.022*** (0.003)	0.015*** (0.003)	0.014*** (0.002)	0.014*** (0.003)	0.015*** (0.002)
N (millions)	2.8	2.8	2.8	2.8	2.1	2.1	2.1	2.1
R ²	0.16	0.16	0.16	0.16	0.13	0.13	0.13	0.13
Outcome Means	10.2	10.1	10.3	10.0	10.7	10.7	10.8	10.6

Notes: Effect of parental exposure to compulsory schooling on the minimum, maximum, and mean years of schooling of their children and on the years of schooling of their eldest and youngest child. Controls include parent birth year, birth state, birth region, birth region by birth year, and race fixed effects. Standard errors are clustered at the child’s birth state by birth year level. *p<0.1; **p<0.05; ***p<0.01.

Up to this point, we have thought of parental exposure to compulsory schooling as a treatment assigned to different children in different amounts. This focus on the child as the unit of analysis creates at least two interpretational challenges. First, fertility may be affected by

compulsory schooling, potentially creating a selection issue in our *Children* sample. Second, taking the child as the unit of analysis ignores the fact that educational investments are made at the household level. The effects of parental exposure to CS on children from the same household will be jointly determined. More parental resources could increase one child's completed education while leaving that of another unaffected. It could even be that greater resources increase one child's completed educational attainment at the expense of another child's attainment. Household choices could thus create complex patterns of heterogeneity in the effects of parental exposure to CS on child outcomes. This suggests that it may be useful to think of this intergenerational problem at the dynastic level.

We now shift the unit of analysis to the level of the family dynasty and ask whether parental exposure to CS changed different features of the distribution of educational attainment among their children. Specifically, we estimate the effect of parental exposure to compulsory schooling on the following family-level outcomes: the maximum, minimum, and average years of schooling of their children and the years of schooling of the eldest and youngest sons and daughters. These results are presented in Table 8.

First, we find that sons benefit more from parental exposure to CS than daughters. While one extra year of exposure to maternal and paternal compulsory schooling leads to a 0.015 to 0.018 and a 0.021 to 0.023 year increase in boys' years of schooling, respectively, these effects are only 0.011 to 0.012 and 0.014 to 0.015 for girls.

Second, eldest sons are the beneficiaries of the largest effects. Eldest sons receive an educational boost of 0.018 and 0.023 years of schooling for each additional year of maternal and paternal compulsory schooling exposure, respectively. In contrast, youngest sons (0.015), youngest daughters (0.011), and eldest daughters (0.012) receive lower benefits from maternal exposure. Paternal exposure yields similar patterns: 0.021, 0.014, and 0.015 for youngest sons, youngest daughters, and eldest daughters, respectively.

Third, fathers' exposure to CS yields higher effects than mothers' exposure, across children of all genders and birth orders. Paternal exposure yields 0.03 to 0.05 years larger effects on children's years of schooling than does maternal exposure.

5 Mechanisms

In this section, we explore plausible channels through which parental exposure to compulsory schooling influences children's outcomes. We first explore effects on parental labor-market outcomes that could directly affect human-capital investments through the monetary and time resources of the household. We then explore spousal choice. As individuals become more educated, assortative matching suggests that they are more likely to marry more educated individuals, which could in turn affect child outcomes through household resources and the productivity of parental time. We also study the geographic mobility of households as a result of CS exposure. We find that parents exposed to compulsory schooling are more likely to live in enumeration districts with high teacher-student ratios when their children are of school age, suggesting that these more educated parents desire, and have the means, to relocate to areas with better-resourced schools. We also find that between 1910 and 1940, parents exposed to more CS moved to urban and metropolitan centers, where their neighbors are more likely to be immigrants, or racially diverse, where households are larger and home-ownership rates lower. This suggests that CS exposure allowed individuals to partake in urbanization.

5.1 Parental Labor-Market Outcomes and Living Arrangements

Compulsory schooling has significant effects on wages and occupational choices (Table 9). Conditional on working, one year of compulsory schooling exposure increases wage earnings by 0.24% and 0.66% for men and women, respectively, a finding that confirms wage effects of CS laws found by Clay, Lingwall and Stephens Jr (2021).¹¹

Women exposed to more CS are less likely to be employed, but when they are, they earn more. Exposure to CS has positive effects on home ownership for men, but these men purchase homes with lower home values. Overall, this suggests improved access to home ownership. Rent paid is also higher for men and women, although imprecisely estimated.

These results are important not only because they hint at CS laws increasing social mobility, but also because they represent potential channels through which changes in parental behaviors and outcomes may affect children’s outcomes and shape intergenerational mobility.

Table 9: Compulsory Schooling Exposure vs Other Outcomes

	log(Wage) (p.p.)	Employment (p.p.)	log(Rent) (p.p.)	log(Home Value) (p.p.)	Home Ownership (p.p.)
CS Years (Male)	0.237*** (0.074)	-0.009 (0.016)	0.075 (0.094)	-0.384*** (0.103)	0.130*** (0.034)
N (millions)	2.6	4.1	1.9	2.1	4.1
R ²	0.13	0.01	0.17	0.17	0.04
Outcome Means	\$1,427.9	89.6	\$69.2	\$3,314.1	52.7
CS Years (Female)	0.664*** (0.140)	-0.076*** (0.020)	0.119 (0.088)	-0.375*** (0.100)	0.047 (0.029)
N (millions)	0.7	5.7	2.6	2.9	5.7
R ²	0.12	0.03	0.18	0.18	0.05
Outcome Means	\$570.2	14.6	\$66.0	\$3,273.6	51.7

Notes: Relationship between exposure to more CS and labor market outcomes and living arrangements. The top panel is estimated using men and the bottom panel using women from the *Parents* sample. Other outcomes explored and unreported because of a lack of statistical significance are: labor-force participation rate, employment rate, living on a farm, urban status, and living in a multifamily household. Controls include birth state, birth year, birth region, birth region and birth year interactions, and self-reported race. Standard errors are clustered at the birth state by birth year level. * p<0.1; ** p<0.05; *** p<0.01.

5.2 Assortative Matching

We find that exposure to CS affected assortative matching. More specifically, it changed the characteristics of a person’s future spouse. Individuals exposed to more compulsory schooling married more educated and higher-earning spouses, on average (see Table 10). A

¹¹When interpreting these results, one must keep in mind that women’s labor market participation is only around 10% during this period.

Table 10: Effect of Compulsory Schooling on Assortative Matching

	<i>Dependent Variable: Spouse's Characteristics</i>			
	Schooling (Years)	log(Wage) (p.p.)	Participation Rate (p.p.)	Employment (p.p.)
CS Years (Female)	0.012*** (0.003)	0.255*** (0.094)	-0.007 (0.013)	-0.026 (0.018)
N (millions)	3.4	2.2	3.5	3.5
R ²	0.15	0.13	0.00	0.01
Outcome Means	8.1	\$1,456.1	95.2	90.7
CS Years (Male)	0.010*** (0.002)	0.614*** (0.220)	-0.039** (0.016)	-0.043*** (0.016)
N (millions)	3.4	0.3	3.5	3.5
R ²	0.14	0.10	0.02	0.02
Outcome Means	8.4	\$552.1	9.6	9.0

Notes: Effect of exposure to different compulsory schooling laws on spousal characteristics. Each column represents a different regression. The regressions include individuals in the *Parents* sample. Controls include birth year, birth state, birth region and birth year interactions, and race. Standard errors are clustered at the birth state by birth year level. *p<0.1; **p<0.05; ***p<0.01.

one-year increase in compulsory schooling exposure is associated with marrying a wife with 0.010 more years of schooling and 0.61% higher wages, and a husband with 0.012 more years of schooling and 0.26% higher wages. These effects on education are similar in size to the effects of compulsory schooling on own educational attainment, i.e. they are substantial as demonstrated earlier in sections 4.2 and 4.5.

Lastly, men's exposure to CS is linked to a 0.04 p.p. decrease in spousal employment and a 0.61% increase in their working spouses' wages, suggesting that more educated couples were able to support a family with a single income, with the female working only if their potential wages were relatively high.

5.3 Geographic Mobility and Neighborhood Characteristics

The quality of local schools is a potentially important mechanism linking parental education to child outcomes. This is particular true in the American context, where education policies and the resources of school districts vary considerably at the local level. Parental exposure to compulsory schooling could induce parents to move to better school districts for several reasons. For example, higher parental education could increase the incentives to move geographically where jobs and school are better. Alternately, as suggested by Piopiunik (2014), parental exposure to compulsory schooling could shift attitudes about the importance of childhood education, increasing the desire to locate in areas with better schools.

We test this hypothesis about the role of local schools by examining whether parental exposure to CS predicts sorting into areas with more resource-rich schools. Lacking detailed data on school district budgets or resources, we proxy school resources by computing district-

Table 11: Sorting Into Better School Districts

<i>Dependent variable: Teacher-student ratios</i>						
	All	Men	Women	White	Black	Post-1900
CS Years (Mom)	0.002*** (0.000)	0.003*** (0.001)	0.001 (0.001)	0.002*** (0.000)	0.000 (0.001)	0.007* (0.004)
N (millions)	5.6	3.6	2.1	5.1	0.4	0.4
R ²	0.00	0.00	0.00	0.02	0.02	0.00
Outcome Means	0.00	-0.00	0.01	0.01	-0.08	-0.03
CS Years (Dad)	0.002*** (0.000)	0.003*** (0.001)	0.002*** (0.001)	0.002*** (0.000)	0.001 (0.002)	0.008 (0.005)
N (millions)	4.1	2.6	1.6	3.8	0.3	0.1
R ²	0.00	0.00	0.01	0.00	0.02	0.00
Outcome Means	0.00	-0.00	0.01	0.01	-0.08	-0.03

Notes: Effect of parental exposure to compulsory schooling laws on teacher-student ratios in the enumeration district of the child when the child was aged 5-14. Dependent variable is standardized. Each column represents a different regression. Controls include parent birth year, birth state, birth region, birth region by birth year, and race fixed effects, as well as child birth year, birth state, birth state by birth year, sex, and race fixed effects. Standard errors are clustered at the child's birth state by birth year level. *p<0.1; **p<0.05; ***p<0.01.

specific teacher-student ratios in the 1900, 1910, 1920, and 1930 censuses, as follows. We use the occupation question in the censuses to infer working adults' occupations and the school-enrollment question to measure school attendance. Next, we create a standardized measure of teachers per student for each enumeration district in the United States for each of the four censuses.¹² Last, we match each individual in the *Children* sample to the teacher-student ratio in the enumeration district they inhabited at ages 5-14. For example, if an individual was born in 1900, we match them to the teacher-student ratio in the enumeration district they inhabited in 1910 (when they were 10 years old). This gives us a measure of the school resources each child was exposed to.

In Table 11, we show that compulsory schooling exposure leads parents to sort into enumeration districts with higher teacher-student ratios. On average, a one-year increase in maternal (paternal) compulsory schooling exposure is related to living in a census tract with a small but highly significant 0.002 (0.002) standard deviation higher teacher-student ratio. These results are conditional on parent birth state, birth year, regional time trends in teacher-student ratios and child birth year, birth state and birth year by state fixed effects. Interestingly, we find no effects for Black Americans. This is perhaps unsurprising given that much of the country was segregated during our sample time frame - either de facto or de jure - creating barrier to black families wishing to choose neighborhoods on the basis of school

¹²Enumeration districts are good proxies for school districts. For example, there were 151,000 enumeration districts in the 1940 census, while there were an estimated 117,000 school districts in the United States in 1939-1940 (Barnard et al., 1947).

characteristics.

Table 12: Effect of Parental Exposure to Compulsory Schooling on Children’s Years of Schooling with Teacher Controls

	<i>Dependent Variable: Child’s Years of Schooling</i>					
	All	Men	Women	White	Black	Post-1900
CS Years (Mom)	0.014*** (0.003)	0.014*** (0.003)	0.014*** (0.004)	0.013*** (0.003)	0.015 (0.009)	0.015 (0.011)
Teacher-Student Ratio	0.114*** (0.034)	0.113*** (0.034)	0.115*** (0.034)	0.101*** (0.032)	1.170*** (0.136)	0.026 (0.017)
N (millions)	5.5	3.5	2.0	5.1	0.4	0.4
R ²	0.16	0.17	0.12	0.11	0.18	0.19
Outcome Means	10.3	10.0	10.7	10.6	7.5	9.5
CS Years (Dad)	0.013*** (0.003)	0.015*** (0.003)	0.009** (0.004)	0.013*** (0.003)	0.030*** (0.011)	0.052 (0.034)
Teacher-Student Ratio	0.172*** (0.041)	0.165*** (0.049)	0.185*** (0.045)	0.161*** (0.039)	1.149*** (0.152)	0.031 (0.024)
N (millions)	4.1	2.5	1.5	3.7	0.3	0.1
R ²	0.17	0.17	0.13	0.11	0.18	0.21
Outcome Means	10.4	10.1	10.8	10.7	7.7	9.5

Notes: Effect of parental exposure to compulsory schooling laws on years of schooling, including estimates for standardized teacher-student ratios in the county \times metropolitan area the child inhabited when aged 4-15. Each column represents a different regression. Controls include parent birth year, birth state, birth region, birth region by birth year and race fixed effects and child birth year, birth state, birth state by birth year, sex and race fixed effects. Standard errors are clustered at the child’s birth state by birth year level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Results in Table 12 demonstrate that teacher-student ratios may indeed be a relevant proxy for school resources and quality. We find that, after controlling for parental CS exposure, a one-standard deviation increase in teacher-student ratios is associated with a 0.10 to 0.19 years of schooling increase, for most demographics. For Black Americans, this figure is much larger, with each one-standard deviation increase being linked to a 1.17-year increase in educational attainment.

In Appendix Table 17, we verify that parents exposed to more compulsory schooling were not living in neighborhoods with higher teacher-student ratios when they *themselves* were of school age (5 to 14 years old). Conditional on our state and year controls and trends in regional teacher-student ratios over time, we find no evidence that the parents were living in high teacher-student ratio enumeration districts to begin with, i.e., at baseline, suggesting that they subsequently sorted into such neighborhoods in anticipation of their children attending school. Thus, the relationship between parental exposure to compulsory schooling and sorting into neighborhoods with higher teacher-student ratios is plausibly driven by parental choices as a result of exposure to more CS, and not spurious.

In section 6, we conduct a more detailed mediation analysis to better quantify how re-

sources (teacher-student ratios) as well as many other variables we include, explain our intergenerational estimates.

The results in the previous section suggest that compulsory schooling could have influenced child outcomes by affecting parental neighborhood choices and the local context in which children were raised. Neighborhood choice represents just one facet of a broader process of geographic mobility, which can also include rural-urban and inter-state migration. Parental CS exposure could influence child outcomes not only through local neighborhood choice, but also through these larger scale movements. The longitudinal nature of the linked census samples allows us to investigate these channels.

Beyond school resources, other aspects of geographic location in childhood could influence educational attainment, including other neighborhood characteristics, and moves from rural to urban areas, or across state lines. We again make use of the enumeration district variable, which allows us to identify all households living in the same, fine-grained geographic area and measure the characteristics of these households at the tract level. For each household, we construct four dimensions of neighborhood characteristics and household migration, where the neighborhood is the enumeration district where the household lives. These dimensions are: (i) labor market and human capital, (ii) demographic, (iii) household, and, last, (iv) migration and urbanity.¹³

To explore neighborhood sorting, we set up the following equation, which relates individual i 's neighborhood of residence n 's characteristics (Y_{ni}) to individual i 's exposure to compulsory schooling years in their state s and for their birth cohort y (CS_{sy}):

$$Y_{ni} = \beta CS_{sy} + \gamma_s + \delta_y + (\eta_r \times \theta_y) + \lambda Race_i + \mu Sex_i + \epsilon_i, \quad (5)$$

This specification is identical to Equation (1) and includes vectors of fixed effects for i 's state of birth (s) and birth year (y) cohort (γ_s and δ_y respectively), interactions ($\eta_r \times \theta_y$) between individual i 's region (r) of birth (η_r)¹⁴ and birth year (y) cohort (θ_y), as well as controls for individual i 's race (λ) and sex (μ). The effect β of individual exposure to compulsory schooling laws CS_{sy} is identified from variation across states of birth (s) and birth year (y) cohorts, conditional on regional trends (captured by the region and birth year cohort interactions $\eta_r \times \theta_y$), state differences in levels (captured by state fixed effects, γ_s) and cohort differences in levels (captured by birth year cohort fixed effects, δ_y).

We estimate how compulsory schooling affects neighborhood sorting between 1910 and 1940 for all individuals in the *Parent* sample who are linked between the 1910, 1920, 1930, and 1940 censuses. The results are presented in Table 13.

¹³Alternatively, as a robustness check, we make use of the full census population schedules of the 1910-1940 censuses, to explore how individuals' exposure to compulsory schooling allows them to sort into more favorable neighborhoods. The population schedules were filled out by enumerators going from door to door, allowing us to identify each household's nearest neighbors. We use this to build a more granular definition of neighborhood, by using each household's 20 nearest neighboring households. This is analogous to the procedure used in Card et al. (2022). The results are robust to this different definition of neighborhood.

¹⁴West, Southwest, Midwest, Southeast and Northeast.

Table 13: Compulsory Schooling and Neighborhood Characteristics

	<i>Labor Market and Human Capital</i>					
	Labor Force Participation 18-60 (p.p.)	Employment Men 18-60 (p.p.)	In School (6-18 p.p.)	Schooling (Years)		
	CS Years	-0.007 (0.006)	0.017*** (0.006)	-0.094*** (0.015)	0.014*** (0.001)	
N (millions)	1.9	1.9	1.9	1.9		
R ²	0.10	0.04	0.11	0.06		
Outcome Means	14.4	-3.4	3.6	0.9		
	<i>Demographic Characteristics</i>					
	Black (p.p.)	White (p.p.)	Immigrant (p.p.)	Average Age (Years)		
	CS Years	0.020*** (0.005)	-0.023*** (0.006)	0.117*** (0.009)	0.003 (0.003)	
N (millions)	1.9	1.9	1.9	1.9		
R ²	0.06	0.06	0.20	0.06		
Outcome Means	-1.4	1.5	-5.5	4.8		
	<i>Household Characteristics</i>					
		Home Ownership (p.p.)	Household Members	Multifamily Household		
	CS Years		-0.114*** (0.021)	0.002*** (0.001)	0.097*** (0.005)	
N (millions)		1.9	1.9	1.9		
R ²		0.10	0.05	0.06		
Outcome Mean		-2.6	-0.8	-1.4		
	<i>Migration and Urbanity</i>					
	Urban (p.p.)	Metropolitan (p.p.)	Population (000s)	State Mover (p.p.)	County Mover (p.p.)	Farm Dweller (p.p.)
	CS Years	0.080** (0.039)	0.094*** (0.030)	2.123*** (0.660)	0.135*** (0.033)	0.061 (0.042)
N (millions)	1.9	1.9	1.9	1.9	1.9	1.9
R ²	0.03	0.02	0.10	0.03	0.03	0.02
Outcome Means	9.8	11.7	-11.4	14.6	36.7	-9.0

Notes: Relationship between individual exposure to CS and neighborhood occupational and labor-market characteristics: i) log wage earnings of working individuals, ii) men's labor-force participation rate (ages 18-60), iii) men's unemployment rate (ages 18-60), iv) educational score of occupation and v) earnings score of occupation. Analyses include all individuals in the *Parents* sample observed in each census between 1910 and 1940. Controls include birth state, birth year, birth region, birth region and birth year interactions, sex and race. Standard errors are clustered at the birth state by birth year level. * p<0.1; ** p<0.05; *** p<0.01.

The top panel of Table 13 shows that compulsory schooling encouraged mobility to neighborhoods with slightly higher employment levels (0.017 p.p.) and educational attainment levels (0.014 p.p.), but lower school enrollment levels for children 6 to 18 years old (-0.094 p.p.). Demographically, as the second panel shows, exposure to compulsory schooling is asso-

ciated with mobility to more diverse neighborhoods. One extra year of CS is associated with moving to neighborhoods having higher proportions of Black (0.020 p.p.) and immigrant (0.117 p.p.) inhabitants and lower proportions of White inhabitants (-0.023 p.p.). In terms of living arrangements and household characteristics (third panel), one additional year of CS exposure is associated with moving to neighborhoods with lower home-ownership rates (0.11 p.p.), somewhat larger households (0.002 individuals), and a higher probability of neighbors living in multifamily households (0.097 p.p.). The evidence in these first three panels hints at migration towards urban areas, which at the time had larger shares of rental properties and multi-family housing units and were more ethnically and racially diverse. This hypothesis is confirmed by the fourth panel, which shows that exposure to CS is associated with moving to more urban neighborhoods. Between 1910 and 1940, individuals exposed to one additional CS year were more likely to transition to localities that were more urban (0.08 p.p.), more metropolitan (0.09 p.p.), and with larger populations (2.1 thousand). They were also 0.14 p.p. more likely to move across state lines, and 0.11 p.p. less likely to live on farms.

Overall, these findings suggest that CS exposure is associated with mobility to more urban and more diverse neighborhoods, with higher labor-force participation, and where households were more likely to live in rented and more crowded spaces. Thus, a potential mechanism that explains the intergenerational transmission of education in the United States during this period is urban migration, which presumably enabled the second generation to benefit from better, urban schooling.

To summarize the findings so far, compulsory schooling exposure positively impacted individuals' educational attainment and outcomes related to financial well-being, such as wages, marrying more educated spouses, and home ownership. Exposure to compulsory schooling also allowed individuals to sort into more urban, higher-wage neighborhoods with lower student-teacher ratios. These three channels may explain why parental exposure to compulsory schooling had such a large impact on their children's outcomes. This exposure is associated with growing up in more educated, higher-earning households and in more urban neighborhoods.

6 Decomposition

In the previous section, we explored several channels that may explain the intergenerational effects of CS laws. Specifically, parental exposure to CS did not only increase parental years of schooling, but it also allowed individuals to obtain higher-paying jobs, marry more educated and higher-earning spouses, and migrate to more urban/metropolitan areas.

To quantify the relative importance of each of these channels on the intergenerational effects of parental exposure to CS, we perform a Gelbach decomposition (Gelbach, 2016). This approach consists of adding different controls to our baseline regressions and quantifying how the estimate of the effect of CS exposure on children's education changes as these controls are included. We control for parental education, dwelling, and labor-market characteristics, parent's spouse's education and labor-market characteristics, and neighborhood characteristics.¹⁵

¹⁵Parental education includes parental years of schooling, labor market includes wages, employment, labor market participation, dwelling includes home ownership, housing value and rent paid and neighborhood characteristics includes all these variables averaged out over each neighborhood (census tract), and additional

We restrict our sample to children who are linked to a previous census in which they were 5-14 years old - allowing us to measure school resources in the census tract of attendance - and with both parents identified in the 1940 census. This leaves us with a sample of 3.7 million individuals (out of our initial 9.4 million children). This way we can control for parental features that are only measured in 1940 (such as wage and education). Additionally, including children with both parents identified allows us to quantify the relative importance of assortative matching on education.

Table 14: Decomposition of Intergenerational Effects

	<i>Dependent variable:</i> <i>Child's Years of Schooling</i>	
	(1)	(2)
CS Years (Mother)	0.014***	
CS Years (Father)		0.013***
N (millions)	3.7	3.7
	<i>Relative contribution</i>	
Education (Own)	20.9%	14.9 %
Labor Market (Own)	-0.3%	0.2 %
Housing (Own)	4.3%	6.1%
Education (Spouse)	16.9%	30.0%
Labor Market (Spouse)	3.9%	-0.2%
Labor Market (Neighborhood)	-2.4%	-2.7%
Housing (Neighborhood)	-2.0%	-1.9%
Urbanization (Neighborhood)	-0.3%	-2.8%
Demographics (Neighborhood)	0.8%	2.4%
Education (Neighborhood)	21.0%	22.2%
Teacher-Student Ratio (Enumeration District)	0.2%	0.3%
Total	63.1%	68.6%

Notes: Relative Contribution of different channels using a Gelbach decomposition. * p<0.1; ** p<0.05; *** p<0.01.

Table 14 presents the results of the Gelbach decomposition. First, note that the estimated effects of mothers' and fathers' exposure to CS are similar to those in our full sample (compare with Table 5). Second, the controls for parental, parent's spousal, and neighborhood characteristics, together explain roughly 63% and 69% of the effects of mothers' and fathers' exposure to CS on children's years of schooling, respectively.

The effects of mothers' exposure to CS on children's years of schooling are explained directly by mothers' education (21%), followed by that of their spouse (17%), hinting at strong assortative mating effects. Neighborhood education levels (measured as the level of education of the people in the same census tract) explain almost 21% of the effects,

variables: household size, proportion of multifamily households, urban status of neighborhood, metropolitan status of neighborhood, proportion of Black, White and immigrant inhabitants, proportion of farm dwellers, school enrollment rates for those aged 6-18, CS exposure of inhabitants, average years of schooling and teacher/student ratios. The Gelbach decomposition controls for each of these variables separately. We show the aggregate explanatory power of each of these categories.

while other neighborhood characteristics, such as neighborhood labor-market, housing, and demographic characteristics matter little. Additionally, the parental housing situation (4%) and the father’s labor-market characteristics (4%) explain a small portion of the total effect. Last, Mothers’ own labor-market outcomes matter very little.

For the effect of fathers’ exposure to CS on children’s years of schooling, only 17% is explained by the father’s own education level, while 30% is explained by that of their spouse, 6% by parental housing characteristics, and 2% and 22%, respectively, by neighborhood demographic and education characteristics.

Interestingly, mothers’ labor market characteristics (which include employment, participation, and wages), explain almost none of the effects of parental CS exposure on children’s education. This is perhaps not surprising, since women’s labor-force participation was around 10% for the mothers in our sample in 1940. Moreover, despite earlier evidence that CS exposure led to moving to more urban and more diverse areas, we find that these channels explain relatively little of the variation in children’s schooling. The one neighborhood characteristic that matters most is the education level of the neighbors.

7 Robustness

7.1 Difference-in-Differences Estimator

In this section, we address possible issues with the two-way fixed effects (TWFE) specification used in this paper. Recent econometric literature suggests that staggered difference-in-difference estimators may be biased in the presence of heterogeneous treatment effects across time and treated units (De Chaisemartin and d’Haultfoeuille 2020, Callaway and Sant’Anna 2021, Goodman-Bacon 2021 and Sun and Abraham 2021). The main issue with a staggered difference-in-difference approach is that, with staggered implementation, the TWFE estimator sometimes includes already-treated states as part of the control group. Moreover, this estimator can be shown to be a weighted average of all possible two-state, two-period (2×2) difference-in-differences (DD) estimators in the data. The weights assigned by the TWFE to each of these comparisons are determined by the length of the panel and the treatment timing, with units treated close to the middle of the panel being assigned more weight. This is sub-optimal and may even lead to some of these comparisons receiving negative weights.

7.1.1 Goodman-Bacon Decomposition

We address these issues in two ways. First, we conduct a decomposition of our TWFE estimator as per Goodman-Bacon (2021).¹⁶ We show the results of this decomposition in Table 15 below. Several main takeaways emerge. When using never-treated states as controls, the difference-in-difference estimators are very large and every single 2×2 comparison yields positive estimates for the effect of parental CS exposure on parent and children’s years of schooling. These particular 2×2 comparisons suggest that exposure to compulsory schooling

¹⁶In order to conduct the Goodman-Bacon decomposition, we first change our compulsory schooling variable to a binary treatment variable that takes the value of 1 for all individuals who were exposed to at least one year of compulsory schooling. We also collapse our data to birth state - birth year cells in order to speed up computation.

Table 15: Goodman-Bacon Decomposition

Comparison	Parents		Children-Fathers		Children-Mothers	
	Estimate	Weight	Estimate	Weight	Estimate	Weight
Treated vs Never Treated	0.38	0.07	0.43	0.07	0.37	0.07
Later vs Always Treated	0.16	0.70	0.13	0.70	0.26	0.70
Earlier vs Later Treated	0.13	0.10	0.09	0.13	0.12	0.10
Later vs Earlier Treated	0.09	0.13	0.09	0.10	0.05	0.13
Weighted Average	0.11	1.00	0.13	1.00	0.20	1.00

Notes: This table shows the Goodman-Bacon decomposition of the TWFE in Equation 1. *p<0.1; **p<0.05; ***p<0.01.

increased parental and children’s educational attainment by 0.37-0.43 years of schooling, or roughly 0.06-0.07 years of schooling per year of compulsory schooling, which is much greater than our TWFE estimates (0.005-0.008 for parents and to 0.015 for children). While these effects are larger they are still smaller than the effects we found by instrumenting parental exposure to CS (section 4.5).

Further, when using always-treated states as controls, the estimates also tend to be positive and significant. These later-treated versus always-treated comparisons account for about 70% of the weight of the TWFE estimator, as many large states were already treated by the time the 1880 birth cohort started attending school.

Last, the later-treated vs earlier-treated comparisons, which are “forbidden” because they are contaminated by the earlier-treated states already having received treatment during the pre-period, account for only about 10% of the weight in our TWFE estimator. Unsurprisingly, these particular estimates are much smaller in magnitude.

In summary, the issues associated with staggered difference-in-differences likely cause a downward bias in our TWFE estimates, which means that our results represent conservative estimates of the true effect of CS laws on educational attainment and on the intergenerational transmission of education.

7.1.2 Stacked Difference-in-Differences Estimator (Cengiz et al., 2019)

The second approach we use to validate our results is akin to the methodology used by Cengiz et al. (2019). This approach aims to manually eliminate all the problematic control group units (i.e. units with varying treatment status).

We first modify our compulsory schooling variable to an indicator for being exposed to no compulsory schooling (0) or any amount of compulsory schooling (1). This allows us to simplify our model to avoid the issue of varying treatment intensities and makes it possible to apply the methodology in Cengiz et al. (2019).

As a benchmark, we first estimate versions of equations 1 and 2 with this new compulsory schooling variable. We then apply the Cengiz et al. (2019) methodology. More specifically, we create an *event-specific* dataset for each state which introduced its first compulsory schooling law in 1880-1905.

Each dataset consists of a 10-year panel of data. It includes the data from the treated state introducing the new schooling law in year y between years $y - 3$ and $y + 7$. It also

includes data from *clean* control states. These clean control states consist of states which did *not* introduce their first schooling law between $y - 3$ and $y + 7$. By including only these states in the control group, we prevent the “forbidden” comparisons between the treated state and control states who change their treatment status at other points in time and bias the results. We then stack all the resulting datasets and perform the difference-in-difference estimation. The advantage of this approach is that it eliminates all problematic control states that have variation in their schooling laws and may create biases in the results.

We report the results for the baseline estimates and the Cengiz et al. (2019) estimates in appendix Tables 18 and 19, both for the direct effects on parental years of schooling and the intergenerational effect on the childrens’ years of schooling. First, we find that the baseline specifications also show positive and significant effects of parental exposure to compulsory schooling on parental and childrens’ years of schooling.

Lastly, the Cengiz et al. (2019) estimator is similar in magnitude to the baseline estimator and the basic patterns hold. In particular, the intergenerational effects of compulsory schooling on childrens’ educational attainment are slightly larger than the direct effects on parental educational attainment. Moreover, the direct and intergenerational effects on Black Americans are the largest of all subgroups.

8 Conclusion

In the late nineteenth and early twentieth centuries, states across the United States sequentially introduced compulsory schooling laws, hoping to raise educational attainment and boost the social mobility of less educated and poorer families, with ever-increasing years of schooling demanded.

Using the linked 1880-1940 full-count censuses and linkages, we examine a large number of outcomes across the entire life cycle, for both parents and children. The panel nature of the data allows us to explore social and geographic mobility across the census years.

Using a difference-in-differences approach, we find those compulsory schooling laws increased the educational attainment of individuals directly exposed to them as well as those of their children. Encouragingly, the effects of compulsory schooling laws on the attainment of the second generation are similar in magnitude to the effects on the first generation, suggesting that such “snowballing” may have contributed to the observed rapid growth in educational attainment over the 20th century.

We explore potential channels that may explain the very strong intergenerational estimates we found. We show that exposure to CS had effects on several parental outcomes that could explain such transmission. First, CS enabled individuals to earn higher wages and gain access to home ownership. It also enabled individuals to marry more educated and higher-earning spouses. Last, exposure to CS impacted migration and neighborhood sorting. Exposure to CS was associated with migration across state lines between 1910 and 1940 and sorting into more urban, more metropolitan, and more diverse neighborhoods, with neighbors with higher-earning professions.

The results suggest that the intergenerational transmission of human capital is larger than we previously thought. In particular, in environments with high social mobility and rapidly increasing educational levels, policies aiming to increase the educational levels of low-education individuals can have very large intergenerational effects.

A Appendix Tables and Figures

Table 16: Effect of Parental Years of Schooling on Children’s Years of Schooling (IV First Stage)

	Black et al.	All	Men	Women	White	Black	Post-1900
CS Years (Mom)	0.222*** (0.002)	0.017*** (0.004)	0.016*** (0.004)	0.018*** (0.005)	0.015*** (0.005)	0.035*** (0.008)	0.047*** (0.010)
N (millions)	8.3	8.3	4.7	3.5	7.4	0.8	0.7
R ²	0.07	0.16	0.15	0.16	0.07	0.12	0.19
First Stage F-stat	387,238.3	257.3	135.8	122.5	210.5	31.0	32.5
Outcome Means	8.0	8.0	7.9	8.1	8.3	5.4	7.6
CS Years (Dad)	0.253*** (0.002)	0.015*** (0.004)	0.014*** (0.004)	0.015*** (0.004)	0.013*** (0.004)	0.051*** (0.011)	0.089*** (0.029)
N (millions)	5.8	5.9	3.4	2.5	5.4	0.5	0.2
R ²	0.07	0.16	0.16	0.16	0.08	0.11	0.26
First Stage F-stat	286,739.8	119.4	68.2	49.4	93.8	27.9	23.2
Outcome Means	7.9	7.9	7.8	8.0	8.2	4.7	7.4

Notes: Instrumental variable first stage showing the effect of parental exposure to CS on parental years of schooling. Each column represents a different regression. Controls include parent birth year, birth state, birth region, birth region by birth year and race fixed effects and child birth year, birth state, birth state by birth year, sex, and race fixed effects, except for the first column, where controls are child birth year and county of residence, parent birth year. Standard errors are clustered at the child’s birth state by birth year level, except for the first column, where they are clustered at the county-by-parent birth year level. *p<0.1; **p<0.05; ***p<0.01.

Table 17: Sorting Into Better School Districts

<i>Dependent variable: Teacher-student ratios</i>						
	All	Men	Women	White	Black	Post-1900
CS Years (Mom)	0.003 (0.004)	0.006 (0.005)	-0.003 (0.006)	0.002 (0.004)	0.069** (0.032)	-0.039** (0.016)
N (millions)	0.11	0.07	0.04	0.11	0.00	0.01
R ²	0.03	0.04	0.04	0.03	0.27	0.06
Outcome Means	10.3	10.0	10.7	10.6	7.5	9.5
CS Years (Dad)	-0.001 (0.002)	-0.003 (0.002)	0.001 (0.003)	-0.002 (0.002)	-0.001 (0.007)	0.002 (0.007)
N (millions)	0.89	0.53	0.36	0.86	0.03	0.03
R ²	0.01	0.02	0.01	0.01	0.07	0.10
Outcome Means	10.4	10.1	10.8	10.7	7.7	9.5

Notes: Relationship between parental exposure to compulsory schooling laws on teacher-student ratios in the enumeration district of residence when a parent was aged 5-14. Each column represents a different regression. Controls include parent birth year, birth state, birth region, birth region by birth year, and race fixed effects and child birth year, birth state, birth state by birth year, sex, and race fixed effects. Standard errors are clustered at the child's birth state by birth year level. *p<0.1; **p<0.05; ***p<0.01.

Table 18: Effect of Own Exposure to Compulsory Schooling on Years of Schooling: Cengiz et al. (2019)

	Baseline					
	All	Men	Women	White	Black	Post-1900
CS Indicator	0.062*** (0.015)	0.032* (0.017)	0.066*** (0.016)	0.053*** (0.015)	0.149*** (0.028)	0.214*** (0.034)
N (millions)	9.5	4.0	5.5	8.6	0.9	2.5
R ²	0.15	0.15	0.15	0.06	0.10	0.15
Outcome Means	8.1	8.0	8.1	8.4	5.1	6.8

	Cengiz et al., 2019					
	All	Men	Women	White	Black	Post-1900
CS Indicator	0.030* (0.018)	0.041* (0.023)	0.025 (0.020)	0.023 (0.016)	0.167*** (0.036)	0.012 (0.038)
N (millions)	24.3	10.1	14.2	20.0	4.1	2.1
R ²	0.14	0.14	0.14	0.04	0.07	0.11
Outcome Means	7.3	7.2	7.5	7.9	4.9	7.8

Effects of exposure to compulsory schooling laws on years of schooling for the *Parents* sample. Each column represents a different regression. Top panel shows baseline estimates of compulsory schooling exposure binary indicator. The bottom panel shows a similar estimation using Cengiz et al. (2019)'s methodology. Controls include birth year, birth state, birth region, birth region by birth year, sex and race fixed effects, when possible. Standard errors are clustered at the birth state by birth year level. * p<0.1; ** p<0.05; *** p<0.01.

Table 19: Effect of Parental Exposure to Compulsory Schooling on Children’s Years of Schooling: Cengiz et al. (2019)

	Baseline					
	All	Men	Women	White	Black	Post-1900
CS Indicator (Mom)	0.063*** (0.013)	0.067*** (0.013)	0.059*** (0.015)	0.046*** (0.015)	0.081*** (0.028)	0.058** (0.029)
N (millions)	8.4	4.8	3.6	7.5	0.8	2.2
R ²	0.19	0.19	0.16	0.10	0.15	0.17
Outcome Means	10.3	10.0	10.7	10.6	7.5	8.9
	Cengiz et al., 2019					
	All	Men	Women	White	Black	Post-1900
CS Indicator (Mom)	0.045* (0.026)	0.034 (0.025)	0.060** (0.030)	0.025 (0.030)	0.127*** (0.030)	0.055 (0.037)
N (millions)	21.5	12.3	9.2	17.4	3.9	1.9
R ²	0.18	0.18	0.15	0.08	0.13	0.14
Outcome Means	9.5	9.2	10.0	10.1	7.4	9.7
	Baseline					
	All	Men	Women	White	Black	Post-1900
CS Indicator (Dad)	0.052* (0.028)	0.056* (0.029)	0.045 (0.031)	0.055* (0.032)	0.146*** (0.045)	0.170*** (0.049)
N (millions)	14.3	8.3	5.9	11.8	2.4	0.5
R ²	0.17	0.17	0.14	0.09	0.13	0.15
Outcome Means	9.6	9.3	10.2	10.1	7.5	9.5
	Cengiz et al., 2019					
	All	Men	Women	White	Black	Post-1900
CS Indicator (Dad)	0.049*** (0.013)	0.056*** (0.014)	0.039** (0.016)	0.046*** (0.015)	0.103*** (0.028)	0.036 (0.035)
N (millions)	6.5	3.8	2.7	5.9	0.5	1.6
R ²	0.18	0.18	0.15	0.11	0.15	0.16
Outcome Means	10.3	10.1	10.8	10.6	7.6	9.0

Effects of parental exposure to compulsory schooling laws on years of schooling for the *Children* sample. Each column represents a different regression. First and third panels show baseline estimates of compulsory schooling exposure of mothers and fathers, respectively. The second and fourth panels show similar estimations using Cengiz et al. (2019)’s methodology. Controls include birth year, birth state, birth region, birth region by birth year, sex and race fixed effects, when possible. Standard errors are clustered at the birth state by birth year level. * p<0.1; ** p<0.05; *** p<0.01.

B Data Appendix

This appendix provides details on how to obtain, clean and transform the data used in this study in order to replicate its results.

B.1 Census Data

First, download the census data and linkages following the instructions below:

- IPUMS 1880-1940 US full count census¹⁷
- 1880-1900 to 1930-1940 US Census cross-walk¹⁸

B.2 Compulsory Schooling Law Data

For compulsory schooling laws, we extend the data used by Clay, Lingwall and Stephens Jr (2021), which builds on work by Lleras-Muney (2002), Stephens Jr and Yang (2014) and Goldin and Katz (2011), among others. The original code used in Clay, Lingwall and Stephens Jr (2021) is extended in “~/DATA/ClayLingwallStephens2021/”. Here is a brief overview of how this dataset is constructed:

1. The authors searched state law archives and created a dataset of compulsory school entry and exit ages and child labor laws between 1880 and 1930 in each U.S. state “state_age_limits_1880_1930_17oct2016.dta”.
2. The authors use the code “cohort_requirements_oct_2016.do” to compute, iteratively, how many years of compulsory schooling each birth cohort was exposed to in each state.
3. The code yields a list (“cohort_requirements_17oct2016.dta”) of compulsory years of schooling for each birth cohort in each state, for cohorts born between 1875 and 1912. These data can be merged to the census data, by year and state of birth of individuals, yielding compulsory schooling laws for all census individuals born between 1880 and 1930.

For more detailed information on this code, please refer to the replication files of Clay, Lingwall and Stephens Jr (2021).

¹⁷Ruggles et al. (2021), obtained at <https://usa.ipums.org/usa/index.shtml>. Select the variables and follow the instructions listed in “Variables.txt”.

¹⁸Ruggles et al. (2019), obtained at https://usa.ipums.org/usa/mlp_downloads.shtml. follow the instructions listed in “Variables.txt”.

B.3 Replication

Once all census data is downloaded and the compulsory schooling data is obtained, run the following codes:

1. Run all codes in `~/CODE/00 clean and merge/` in order. These
 - read the census and crosswalk files
 - create one large crosswalk between 1880 and 1940 to identify individuals and their parents across time¹⁹
 - extract the relevant parent and children samples
 - clean the samples
2. Open the main.R file in `~/CODE/Figures and Tables/` and change the wd (working directory) variable to the relevant path on your machine.
3. Run the main.R file preamble and the lines related to your desired figure/table replication file. Each table and figure can be replicated separately. Each individual replication file is stored in `~/CODE/Figures and Tables/`.
4. Some exceptions:
 - for neighborhood sorting results, first open:
`~/CODE/Figures and Tables/07 Tables Neighborhood Sorting/` and run `create_neighbor_stats_1910.R` and `create_neighbor_stats_1940.R` to create neighborhood-level measures, before running the .Rmd replication scripts from main.R.

¹⁹At the time of the analysis, IPUMS did not allow downloading linked samples from their website. This is now possible and some of these steps may be avoided by downloading the linked samples directly.

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