Intergenerational Persistence in the Effects of Compulsory Schooling*

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Abstract

We examine the effects of parental exposure to compulsory schooling laws on the socio-economic 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. Using linked records from the 1880 to 1940 full-count United States' decennial censuses, we find that parental exposure to CS increased not only the educational attainment of parents but also of their children and the magnitudes of these effects are similar, suggesting much stronger intergenerational transmission of human capital than found in other settings. Temporal and spatial differences in school resources, such as teacher-student ratios do not explain these effects. Exploring the mechanisms of intergenerational transmission, we find that higher CS exposure leads to higher parental wage earnings, marrying more educated spouses, higher probabilities of moving across state lines and to more urban and cosmopolitan areas. Parental education and assortative mating on education explain most of the estimated intergenerational effects, while geographic sorting had a limited impact, suggesting family characteristics are more important than neighborhood effects.

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PRELIMINARY: DO NOT CIRCULATE OR QUOTE

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

Mann (1849), Rauscher (2016)

1 Introduction

Public education has long been considered a critical engine of social mobility. Against the backdrop of large-scale industrialization and demographic change, nearly every state expanded its compulsory schooling (CS) requirements, alongside other educational reforms, to improve the skills of their population. 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 across generations*. In order to lift people out of poverty, and more generally for human-capital based economic growth, it is essential that each subsequent generation does better than the preceding one. Yet, very little is known about such intergenerational effects, precisely because of the scarcity of data linking outcomes across multiple generations.

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 the intergenerational effect of exposure to CS on educational attainment, (ii) explore mechanisms through which such intergenerational effects operate, and (iii) provide, to our knowledge, the first evidence from the United States on the *intergenerational* effects of compulsory schooling reforms on *completed adult educational attainment outcomes* of the offspring. We discuss this below in more detail.

First, we are able to estimate the intergenerational effect of exposure to CS on completed educational attainment. One extra year of maternal and paternal exposure to CS increases children's educational attainment by 0.015 and 0.016 years, respectively. Contrasting these with the effects of one's own exposure to CS, the cohorts directly exposed to one additional year of CS experienced a 0.008 year increase in years of schooling for women and 0.005 years for men (or about half of the intergenerational effect). These estimates hint at strong intergenerational persistence in the effects of CS, with the effects of parental exposure to CS on the offspring larger than the effects on the parents themselves. Such "snowballing" may have contributed to the observed rapid growth in educational attainment over the 20^{th} century.

But, how to interpret these estimates? Since CS laws were binding only for a small fraction of the population, their effects are actually much larger for those that were affected than the difference-in-difference estimators suggest (with estimates ranging from 0.005 to 0.016 years). We calculated, using pre-policy educational attainments in each state, the proportion of treated children, for whom compulsory schooling laws were binding. At the time these CS laws came into force, they were only binding for slightly more than 20% of children, who, on average, had between two and three fewer years of schooling than the newly-mandated minimum years of CS. Thus, even under full enforcement, the average educational attainment of the population would have only modestly increased. Further, we find that early CS laws were only about 6-10% effective in bringing individuals with less than the mandated level of CS up to the mandated level. The combination of CS laws affecting only a small proportion of children, and weak enforcement, may well explain the small observed effects.

To get a sense of the potential effect size, we use CS exposure as an instrument for parental years of schooling, like Black, Devereux and Salvanes (2005). We estimate that a one-year increase in maternal and paternal schooling results 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. These authors exploit a change in CS from seventh to ninth grade that was rolled out gradually across municipalities during the 60s and early seventies, and 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 finding of small effects as evidence for selection (versus causation), operating in the intergenerational transmission of education, our results suggest a causal relationship between parental exposure to CS laws and offspring schooling. This could reflect that returns to education were higher at lower levels of education and/or during the 19^{th} and early 20^{th} century in a rapidly industrializing economy, versus the 60s and early 70s.

States' inability and, in some cases, unwillingness to fully enforce these laws (Katz, 1976), may explain the gap between our estimates and the expected effects under full enforcement. The modest effects of CS exposure on parental education should be viewed in light of the limited scope and limited effectiveness of the implementation of CS laws in the late 19^{th} and early 20^{th} centuries. At the same time, those affected by CS reforms, as well as their offspring, saw substantial gains in years of schooling, much larger than in other settings, such as Norway in the 60s and early 70s.

Our second contribution consists in exploring mechanisms through which 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. Both men and women exposed to more years of CS had higher wages, even though more CS is associated with men sorting into slightly lower-earning occupations. Women experienced larger wage increases and sorted into occupations associated with higher education but were less likely to work after exposure to higher levels of CS. More CS increased men's homeownership rates and was associated with lower home values for both men and women, hinting at lower barriers to home purchasing.

We also find evidence of effects on spousal characteristics. Both sexes exposed to more CS marry more educated and higher-earning spouses. Specifically, women exposed to higher

CS marry men who are more likely to be employed, have higher earnings, and are more likely to work in a higher-earning occupation. Men exposed to more CS marry women who are less likely to participate in the labor market, but, when they do, have higher earnings and sort into occupations associated with higher education levels.

Further, the fine level of geographic detail in the censuses allows us to study geographic sorting, while the crosswalks enable us to study social geographic mobility over time. Specifically, we construct neighborhood measures of labor-market, educational, dwelling, and demographic characteristics, at the census-tract level, and study how exposure to more CS affects sorting across neighborhoods and across time. Parents exposed to more CS sort into neighborhoods with significantly different housing markets, labor markets, and demographic characteristics that reflect urbanization. At baseline in 1910, individuals exposed to more CS tended to live in more rural areas, with fewer immigrants and with fewer Black neighbors. The neighborhoods these individuals grew up in have higher home-ownership rates and more farm-dwelling families. Between 1910 and 1940, individuals exposed to more CS were more likely to cross state lines and to transition to neighborhoods that are more populous, urban, and metropolitan. Their neighbors were more likely to be Black, immigrant, less likely to own their homes, and more likely to live in larger and multifamily households. Last, individuals exposed to more CS increasingly migrated to locations with higher employment rates and with neighbors working in lower-educated, but higher-earning, professions. These results suggest that more education allowed individuals to move to more urbanized areas, potentially benefiting from the rapid industrialization of the first half of the twentieth century in the United States.

Last, we quantify the relative importance of each of these channels. Using a Gelbach decomposition (Gelbach, 2016), we find that roughly two-thirds of the estimated effects are explained by the various channels we explore. In particular, paternal exposure to compulsory schooling is explained by the father's education (16%), his spouse's education (32%), home ownership and home value (7%) and sorting into a neighborhood with more educated inhabitants (24%). School resources, measured as teacher-student ratios, own labor market outcomes, and other neighborhood characteristics, explain very little of the effects. Maternal exposure to CS is explained by maternal education (21.6%), her spouse's education (17.5%), her own labor-market outcomes (4.0%), and the average level of education in the neighborhood (21.7%).

Our third contribution is that, to 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). 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. These studies all 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.

Methodologically, our analysis most closely relates to Black, Devereux and Salvanes (2005), who study the intergenerational effects of an increase in compulsory schooling in Norway during the 1960s and early 70s. They use exposure to the reform as an instrumental variable for parental education and find little evidence of a causal effect of father's education, but some evidence of a causal effect of mother's education on the education of sons (but not for daughters). Black, Devereux and Salvanes (2005) therefore conclude that the observed high correlations between parental and children's education are due primarily to selection and not causation. By contrast, we find evidence of large and highly statistically significant and robust causal effects for both father's and mother's education on the educational attainment of their children. When adopting the same instrumental variable strategy as Black, Devereux and Salvanes (2005), we estimate that a one-year increase in maternal and paternal schooling results in 0.8- and 1.1-year increase in children's years of schooling, respectively. 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 19^{th} and early 20^{th} century, to name a few.

Our results also contribute to an established literature documenting factors that shape intergenerational mobility in the United States and across contexts. Several studies examine whether schooling reforms affected intergenerational mobility by estimating whether they had larger or smaller effects on individuals from different family socio-economic backgrounds. Our approach, and that of Black, Devereux and Salvanes (2005), is distinct in that we explore the causal effect of parental status whereas these studies rely on associations.

Directly related to our work, Rauscher (2016) finds that while compulsory 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 of these 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 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. 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 to this set of facts, since cross-county mobility in the U.S. during the late nineteenth century was substantially greater than comparable migration in the U.K., or in the later twentieth century U.S. context. Our results provide direct evidence on this showing that parental exposure to CS accelerates migration to more metropolitan and urban areas. This migration may have been one of the mechanisms leading to persistent effects of compulsory schooling reforms.

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

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

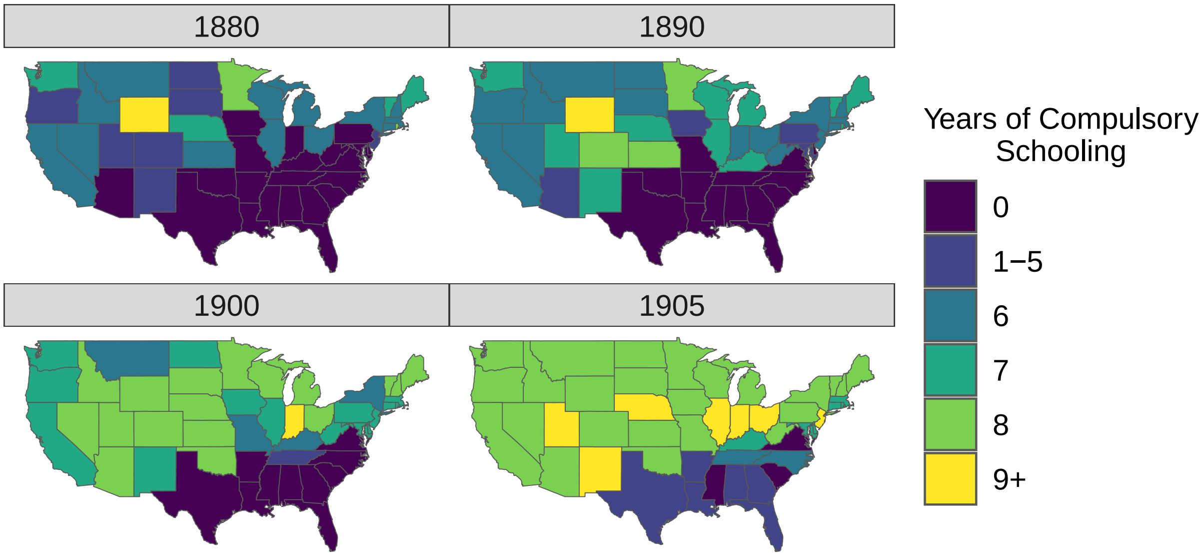

Figure 1: Compulsory schooling law exposure by state and birth year cohort.

1. Is the child's age between the maximum compulsory school entry age and the minimum compulsory school leaving age?

2. If so, does an exemption to the school leaving 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 school leaving exemption?
   - 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 permit requirement?

3. If a work age exemption applies, is the child's age less than the continuation schooling age? If so, has the child completed sufficient schooling to be exempt from continuation school if such an exemption exists?

By using the answers to these questions we can determine for how many years the individuals in our data were legally required to stay in school.

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 school-entering age, states in New England, around the Great Lakes and in the Western United States, 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 these 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 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, although children born in other Southern states in 1905 were already in school by the time the first compulsory schooling laws passed and were thus only partially affected by them. Meanwhile, the norm in most states was 8 years of compulsory schooling by this time.

2.2 Full Count Census Data

Our key question of interest is whether changes in compulsory schooling laws had *intergenerational* effects on completed education. To this end, we use linked census data from 1860 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 1860 to 1940 census linkages constructed by Ruggles et al. (2019) to identify individuals across census waves.

These linkages are crucial for several reasons. First, 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.

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 only name only a few.

Third, because we can track individual 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.

Last, the combined full-count censuses allow us to harness great statistical power.

[3]Note that the 1890 census is excluded, as the population schedules were lost in a fire.

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.

We build four main samples of interest:

1. the *Chilren* sample: contains all 9,382,509 individuals in the 1940 Census born in one 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 are who are born between 1880 and 1905 in one the 48 continental states or D.C. and are aged at least 16 when their child is born.

Table 1 presents some basic summary statistics on demographics, education, and selected labor-market outcomes for the four 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. Lastly, males are over-represented in our Children sample (55%). This is because women are more difficult to match across censuses, in particular due to their changing of last names as a result of marriage. Meanwhile, women are slightly overrepresented in our parents sample for several possible reasons: mothers are on average younger than fathers, they have higher life expectancy and are more likely to live with their children in case of separation. All these reasons make it easier to link mothers to their children.

Figure 2 shows the distribution of birth years and exposure to compulsory schooling of the parents of our Children's sample, for fathers (top-panel) and mothers (bottom-panel), separately. The census allows us to link tens of thousands to several hundred thousands of parents in each birth year cohort to their children. The mode of the birth year distribution

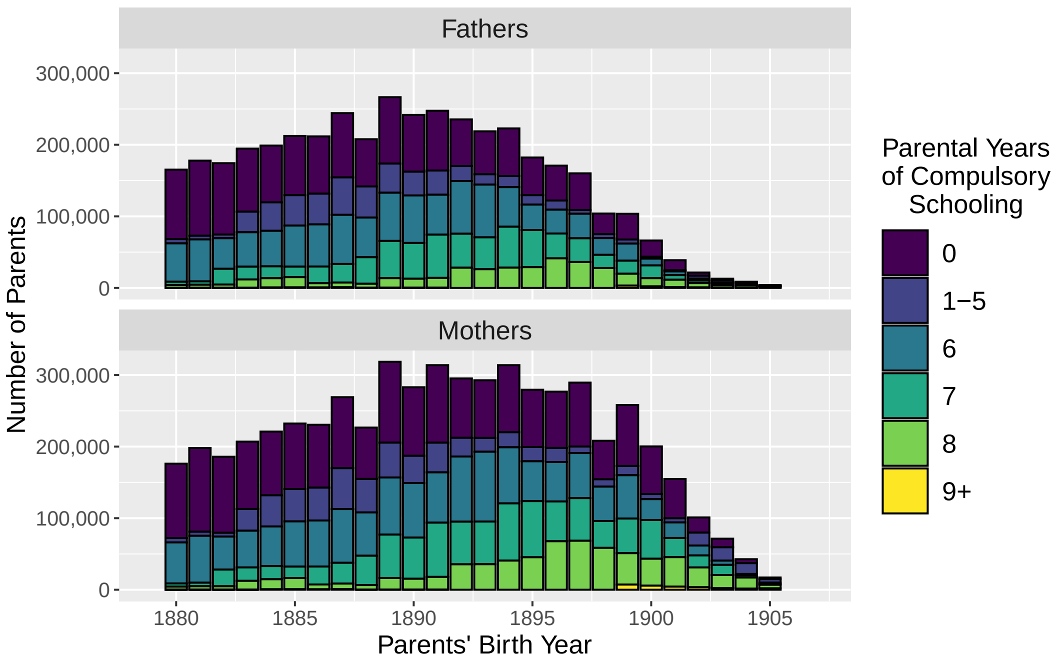

Figure 2: US-born parents by birth year and own exposure to compulsory schooling in the Children sample.

of parents is in 1888. Parents born in this year were aged 51 in 1940, a prime age for having adult children in the 1940 census. The exposure of these parents to compulsory schooling varies significantly, both within and across cohorts. While older parents are exposed to fewer years of compulsory schooling, younger ones are often exposed to 7 or more years of schooling.

We restrict the parent sample to those born after 1880 because most of our parent-child matches come from the 1940 census. Using older parents may lead to attrition via mortality. To the extent that this attrition may be inversely correlated with schooling, including older parents may introduce unwanted bias in our results. 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 each cohort.

3 Empirical Strategy

3.1 Difference-in-Differences

Two estimating equations serve as our main empirical strategy. The first relates the parental (p) years of schooling ($Educ_i^p$) of individual i to the number of years of compulsory schooling

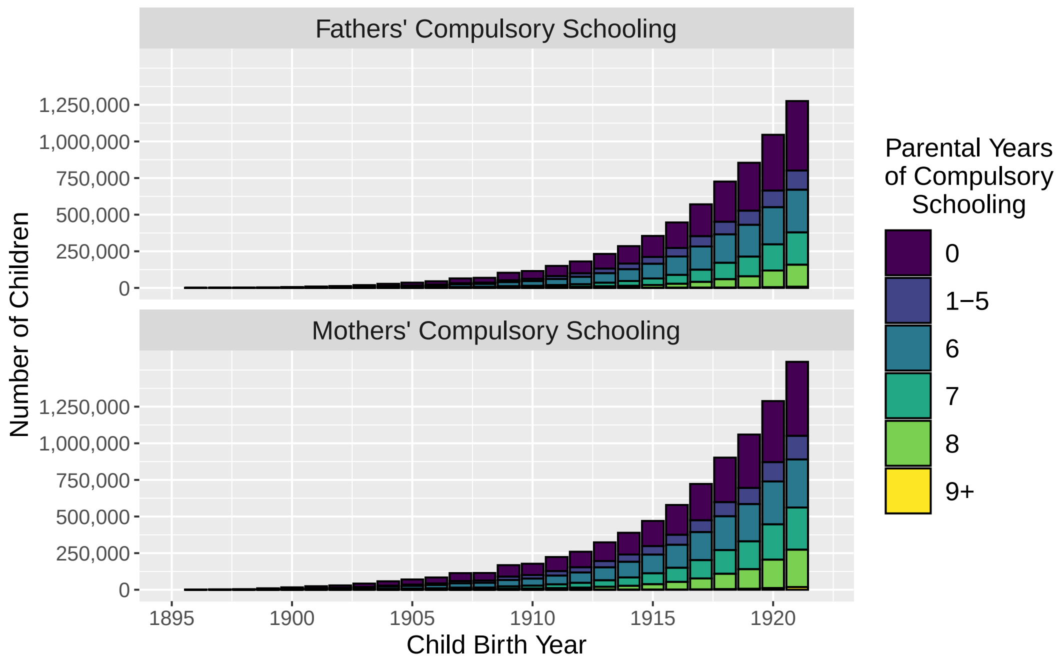

Figure 3: Children's exposure to parental years of compulsory schooling by child birth year.

($CS^p_{s'y'}$) their birth state (s') birth year (y') cohort were exposed to:

$$Educ_i^p = \beta^p CS^p_{s'y'} + \gamma^p_{s'} + \delta^p_{y'} + \left(\eta^p_{r'} \times \theta^p_{y'}\right) + \lambda^p Race_i^p + \mu^p Sex_i^p + \epsilon_i^p, \quad p = m, f \tag{1}$$

where we include vectors of fixed effects for i's state of birth (s') and birth year (y') cohort ($\gamma^p_{s'}$ and $\delta^p_{y'}$ respectively), interactions ($\eta^p_{r'} \times \theta^p_{y'}$) between individual i's region (r') of birth ($\eta^p_{r'}$)[4] and birth year (y') cohort ($\theta^p_{y'}$), as well as controls for individual i's race (λ^p) and sex (μ^p). The effect β^p of parental exposure to compulsory schooling laws $CS^p_{s'y'}$ 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 $\eta^p_{r'} \times \theta^p_{y'}$), state differences in levels (captured by state fixed effects, $\gamma^p_{s'}$) and cohort differences in levels (captured by birth year cohort fixed effects, $\delta^p_{y'}$). These analyses use the Children's sample and estimate separate effects for mother and father's 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 ($Educ_i^c$) to the compulsory schooling exposure ($CS^p_{s'y'}$) of the child's parents:

$$\begin{aligned} Educ_i^c &= \beta^c CS^p_{s'y'} + \gamma^c_s + \delta^c_y + \left(\eta^c_s \times \theta^c_y\right) + \gamma^p_{s'} + \delta^p_{y'} + \left(\eta^p_{r'} \times \theta^p_{y'}\right) \\ &+ \lambda^c Race_i^c + \mu^c Sex_i^c + \epsilon_i^c, \quad p = m, f \end{aligned} \tag{2}$$

where, analogous to Equation 1, we include vectors of child fixed effects for the child's state of birth s (γ^c_s) and birth year y (δ^c_y), and interactions ($\eta^c_s \times \theta^c_y$) between the child's state of

[4] West, Southwest, Midwest, Southeast and Northeast.

birth (η^c_s) and birth year (δ^c_s), as well as controls for the child's race (λ^c) and sex (μ^c). Unlike Equation 1, here we have sufficient power to control at the state level s for trends that differ between states, as opposed to trends by region r. These controls capture state-birth year effects, such as children's own exposure to compulsory schooling. This is important because children often live in the same state as their parents, so that their exposure to compulsory schooling is likely correlated with that of their parents. Indeed, as Figure 4 demonstrates, children whose parents were exposed to 9 years or more 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 also include vectors of fixed effects for the parent's state of birth s' ($\gamma^p_{s'}$) and parent's birth year y' ($\delta^p_{y'}$), and interactions ($\eta^p_{r'} \times \theta^p_{y'}$) between the parents' region (r') of birth ($\eta^p_{r'}$) and birth year ($\delta^p_{y'}$).

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 year of birth y' level - varies.

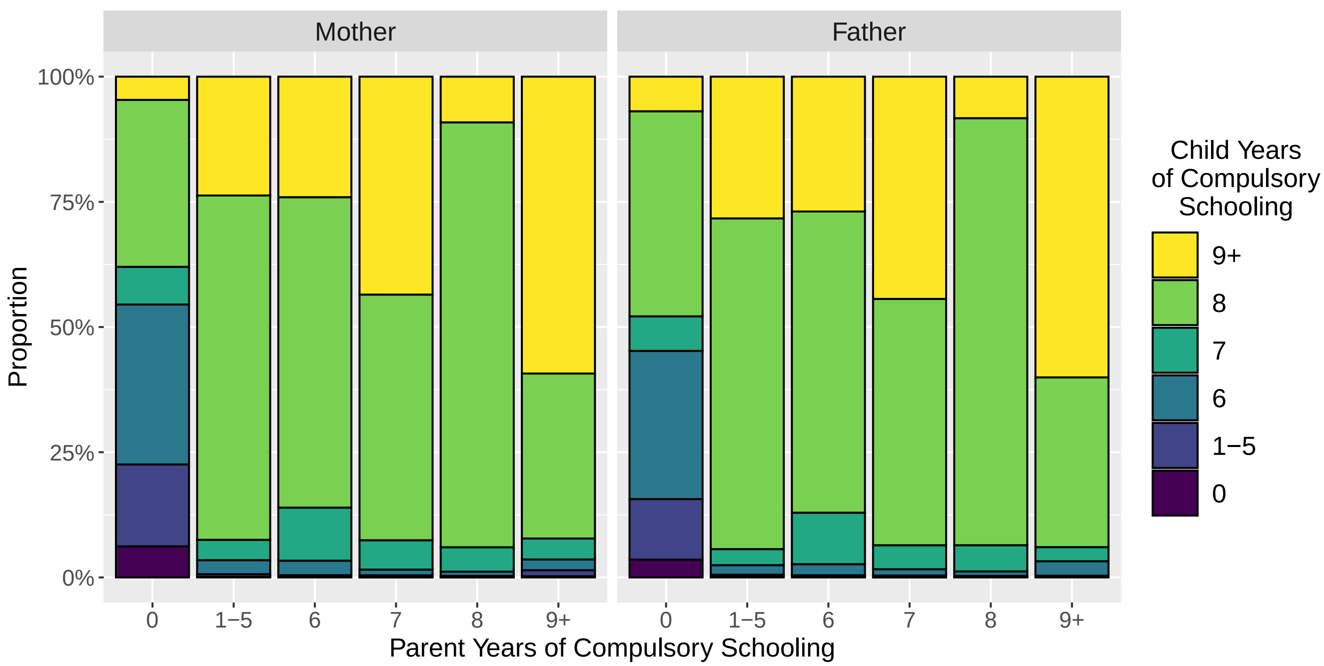

Figure 4: Relationship between parental years of compulsory schooling (horizontal axis) and proportion of child exposure to compulsory schooling (vertical axis; color-coded years of compulsory schooling), separate for mothers (left) and fathers (right).

Our main specification helps us address three main identification challenges. First, compulsory schooling laws are persistent over time (hardly ever are compulsory years of schooling reduced and more generally over the period considered, they increased). 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 parental and children's exposure to compulsory schooling are highly correlated. Controlling for interactions between the children's birth state s and birth year y thus becomes very important.

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) time trends, which are then incorrectly attributed to CS laws. To address this, we do 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.

Lastly, 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 children'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 parental education. Because compulsory schooling laws affects many cohorts and 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 usual 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 $CS_{s'y'}^p$:

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

In the second stage, we used the fitted parental education to predict children's educational attainment:

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

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

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. 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 Results

We now present our main results. First, we show the impact of compulsory schooling laws on the parents sample. On average, compulsory schooling caused small increases in years of schooling, primarily driven by additional enrolment and graduation from grade school and enrolment into middle school and, to a lesser extent, high school and college graduation. Individuals in rural areas did not benefit from schooling laws, hinting and enforcement problems.

We then show that *parental* exposure to CS has even larger effects on children's educational attainments. These effects are driven especially by high school enrolment and high school graduation increases. Sons, especially eldest sons, are the main beneficiaries of parental exposure to CS laws.

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

	Dependent variable: Years of Schooling				
	All	Urban	Rural	Black	Post-1900
CS Years (Women)	0.008***	0.007**	0.004	0.038***	0.044***
	(0.003)	(0.003)	(0.003)	(0.008)	(0.010)
N (millions)	5.5	2.9	2.6	0.5	0.6
R^2	0.15	0.10	0.20	0.10	0.17
Outcome Means	8.1	8.6	7.5	5.4	7.7
CS Years (Men)	0.005*	0.010***	-0.006	0.046***	0.093***
	(0.003)	(0.003)	(0.004)	(0.011)	(0.021)
N (millions)	4.0	2.0	2.0	0.3	0.1
R^2	0.15	0.09	0.20	0.08	0.24
Outcome Means	8.0	8.8	7.2	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.

To put these results into perspective, we first instrument parental years of schooling using CS law exposure and find that a one-year increase in parental schooling induces children's attainment by approximately one year. Second, we estimate that across different states and schooling laws, only about 20% of pre-policy cohorts had educational attainments lower than the newly-mandated ones. Even under perfect enforcement of the compulsory schooling policies, we would only find a 0.08 increase in years of schooling for each additional compulsory schooling year. The relatively small difference-in-difference effects are due to the limited scope of these laws and imperfect enforcement.

Lastly, we explore other channels through which parental exposure to CS may affect children's outcomes. We find that CS exposure affects assortative mating, parental labor market and living arrangements and parental geographic sorting and mobility between 1910 and 1940.

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 to a 0.008 and 0.005 increase in women's and men's years of schooling, respectively. Rural-dwellers in 1940 were not affected by CS. This is in accordance with Katz (1976), who documents poor enforcement of early compulsory schooling laws in rural areas, in particular due to a lack of rural schools. However, it could also indicate that those who achieved higher education through compulsory schooling moved to urban areas before 1940.

We obtain statistically significant results for urban dwellers and Black Americans. The effects on the Black population's years of schooling is are the largest, possibly 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.

Table 3: Compulsory Schooling Law Effectiveness

		All	Urban	Rural	Black	Post-1900
Women	Actual Effect of CS Exposure (Yrs)	0.008***	0.007**	0.004	0.038***	0.044***
	Proportion Under CS Years (p.p.)	23.1	20.4	26.4	45.6	37.6
	Average Schooling Deficit (Yrs)	2.4	2.3	2.5	3.0	2.6
	Potential Effect of CS Exposure (Yrs)	0.099	0.080	0.115	0.187	0.174
	Effectiveness (Actual/Potential)	8%	9%	5%	20%	26%
Men	Actual Effect of CS Exposure (Yrs)	0.005*	0.010***	-0.006	0.046***	0.093***
	Proportion Under CS Years (p.p.)	21.8	17.7	26.5	51.2	42.6
	Average Schooling Deficit (Yrs)	2.5	2.3	2.6	3.2	2.9
	Potential Effect of CS Exposure (Yrs)	0.109	0.080	0.131	0.261	0.255
	Effectiveness (Actual/Potential)	5%	13%	-	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.

To put these results into perspective, we must consider that for many high-achieving students, the CS laws had no effect. Indeed, only a small fraction of students had educational attainment that was lower than the one mandated by the compulsory schooling law and thus had to remain in school longer than desired. For example, if a state passed a new 6-year compulsory schooling law affecting cohorts born after 1900, the relevant marginal individuals are those who would have completed fewer than 6 years of schooling without the reform.

Although we cannot observe desired years of schooling, we can estimate this fraction by measuring which proportion of the 1899 cohort had fewer than 6 years of schooling completed in adulthood (in 1940). Moreover, 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 extra year. This explains why, even if the CS laws were well-enforced, the expected estimates in Table 2 would be small.

To better understand potential schooling gains, estimate the following model, which imposes all individuals in our data to get educated at least up to the compulsory schooling-mandated education level in their state:

$$\max(Educ_i, CS^p_{s'y'}) = \beta^p\, CS^p_{s'y'} + \gamma^p_{s'} + \delta^p_{y'} + \left(\eta^p_{r'} \times \theta^p_{y'}\right) + \lambda^p Race^p_i + \mu^p Sex^p_i + \epsilon^p_i,\ \ p = m, f \quad (5)$$

In this way, we can estimate maximum potential schooling gains from CS under perfect enforcement of schooling laws. This provides us with a theoretical yardstick against which we can measure the actual effects of CS laws on educational attainment.

These potential schooling gains, and other statistics summarizing the scope and effectiveness of compulsory schooling laws across different regions of the United States are presented in Table 3. First, we observe that at the time CS laws came into effect, an average of 23.1% of men and 21.8% of women had educational attainment levels that were under the mandated minima. This percentage was much higher for Black men (45.6%) and Black women (51.2%).

These low-attainment individuals were typically 2.4 to 2.5 years below the minimum schooling level, with Black Americans being more than 3 years behind this minimum. Putting this information together, the potential schooling gains from compulsory schooling, even under full enforcement of compulsory schooling laws, would have been around 0.10 years per person per compulsory schooling year. Comparing our estimated gains from Table 2 to these potential gains, we estimate that the schooling laws were roughly 8% effective for men and 5% effective for women. These laws were particularly effective in increasing the educational attainment for urban women, Black Americans of both sexes and for people born after 1900, especially women.

Next, 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.[6] Indeed, one additional year of maternal compulsory schooling exposure increases the probability of attending grade school, graduating from grade school and attending some middle school by 0.03, 0.12 and 0.09 p.p., respectively. Mothers' exposure leads to increases of 0.06 and 0.12 in grade school attendance and graduation, respectively. Moderate gains are also observed in high school and college graduation, suggesting that perhaps compulsory schooling induced some individuals to complete their degrees. These results are consistent with

[6]We 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

	Dependent Variables: Entry, Completion (p.p.)							
	Some GS	Grade School (GS)	Some MS	Middle School (MS)	Some HS	High School (HS)	Some College	College
CS Years (Moms)	0.029**	0.121***	0.090**	0.048	0.048	0.053*	0.026	0.033***
	(0.011)	(0.032)	(0.037)	(0.043)	(0.035)	(0.032)	(0.018)	(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***	0.120***	0.055	-0.066	-0.023	0.009	-0.019	0.037**
	(0.012)	(0.034)	(0.041)	(0.046)	(0.037)	(0.034)	(0.019)	(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 $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

the compulsory schooling laws of the early twentieth century, which imposed between 6 and 9 years of mandatory schooling. 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 schools to pursue more years of schooling, more than inducing students who never attended school to enrol in the first place.

Lastly, we 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).[7]

[7]The 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 extremely large confidence intervals that do not shrink even when sample sizes are large. Moreover, in our setup, the level of treatment assignment is the birth state by birth year, which further decreases the need to cluster at the state level.

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

	Dependent Variable: Child's Years of Schooling						
	All	Men	Women	Urban	Rural	Black	Post-1900
CS Years (Mom)	0.015***	0.016***	0.015***	0.010**	0.015***	0.021**	0.018*
	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.008)	(0.009)
N (millions)	8.3	4.8	3.6	4.4	4.0	0.8	0.7
R^2	0.19	0.20	0.16	0.12	0.23	0.15	0.20
Outcome Means	10.3	10.0	10.7	11.0	9.5	7.5	9.5
CS Years (Dad)	0.016***	0.018***	0.012***	0.014***	0.009***	0.039***	0.059***
	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.009)	(0.023)
N (millions)	5.5	3.2	2.3	2.7	2.8	0.5	0.2
R^2	0.18	0.19	0.15	0.10	0.22	0.15	0.20
Outcome Means	10.4	10.1	10.8	11.1	9.6	7.7	9.5

Notes: Effect of parental exposure to compulsory schooling laws on 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. Standard errors are clustered at the child's birth state by birth year level. *p<0.1; **p<0.05; ***p<0.01.

4.2 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 subsamples of interest: all individuals, men, women, urban and rural-dwellers in 1940 and Black Americans. All effects are significant and larger in magnitude than the effects of compulsory schooling on parents' own educational attainment. The largest intergenerational effects are from Black Americans' fathers' exposure to compulsory schooling, while the smallest are effects of paternal exposure to CS on rural children's education levels. 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.

Next, 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 schooling in 1940 with the parent's exposure to compulsory schooling laws.[8] 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) and for different demographic groups and time period. Across various specifications and samples, the effects are an order of magnitude larger than those of Black, Devereux and Salvanes (2005), who found

[8] First stage results are presented in Table 19 of the Appendix. All first stage F-stats are highly significant, ruling out weak instruments.

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

	Dependent Variable: Child's Years of Schooling							
	Black et al.	All	Men	Women	Urban	Rural	Black	Post-1900
Years of Schooling (Mom)	1.089***	0.930***	1.014***	0.833***	0.953***	1.407***	0.595***	0.379**
	(0.005)	(0.132)	(0.162)	(0.139)	(0.215)	(0.451)	(0.206)	(0.171)
N (millions)	8.3	8.3	4.7	3.5	4.3	4.0	0.8	0.7
R^2	0.00	0.18	0.15	0.19	-0.07	-0.21	0.33	0.35
First Stage F-stat	387,238.3	257.3	135.8	122.5	52.6	55.7	31.0	32.5
Outcome Means	10.4	10.3	10.0	10.7	11.0	9.5	7.5	9.6
Years of Schooling (Dad)	0.848***	1.044***	1.188***	0.826***	0.862***	-7.908	0.866***	0.613***
	(0.003)	(0.199)	(0.243)	(0.216)	(0.203)	(26.388)	(0.211)	(0.215)
N (millions)	5.8	5.9	3.4	2.5	1.6	1.8	0.5	0.2
R^2	0.08	-0.14	-0.27	0.02	-0.15	-57.82	0.16	0.25
First Stage F-stat	286,739.8	119.4	68.2	49.4	55.3	0.4	27.9	23.2
Outcome Means	10.4	10.4	10.1	10.8	11.1	9.6	7.7	9.3

Notes: Effect of parental years of schooling on years of schooling 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. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

essentially no or weak effects 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 this setting).

Remarkably, we find instead very large causal effects. A 1 year increase in maternal (paternal) education resulting from exposure to compulsory schooling increases children's schooling by 1.09 (0.85) years, with effects ranging from 0.60 years for the effects of maternal exposure to compulsory schooling for African American children to 1.41 years for maternal exposure to CS of rural-dwellers. Paternal exposure to CS in rural areas yields the only non-significant result, as CS exposure does not predict paternal educational attainment well in rural areas.

These large effects may be because the compulsory schooling laws in the United States, unlike Norway, mainly targeted students who had between zero and six years of schooling and at a time when levels of schooling were very low. Some of these parents would otherwise not have completed any schooling. They may have, e.g., been illiterate in the absence of the policy. This is in stark contrast with the Norwegian setup, where the studied reform took place in the 1960s and increased compulsory schooling from 7 to 9 years. 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.

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

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

	Dependent Variables: Entry, Completion (p.p.)							
	Some GS	Grade School	Some MS	Middle School	Some HS	High School	Some College	College
CS Years Mom	0.012***	0.063***	0.062***	0.191***	0.231***	0.247***	0.106**	0.043**
	(0.003)	(0.010)	(0.011)	(0.034)	(0.048)	(0.057)	(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	0.062***	0.084***	0.166***	0.191***	0.214***	0.157***	0.077***
	(0.004)	(0.014)	(0.017)	(0.035)	(0.043)	(0.052)	(0.042)	(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 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. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

parental compulsory schooling 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 did. Alternatively, an increase in required schooling could increase the chances that children obtain substantially more educational attainment than their parents. We next test the children's educational attainment margins affected by higher levels of parental compulsory schooling exposure. We define a set of indicator variables Attainment_i^ℓ 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 $\ell \in$ {Some Grade School, Grade School, Some Middle School, Middle School, Some High School, High School, Some College and College}.

Table 7 shows 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 and completing it across the entire distribution of educational attainment. The largest effects are on completing middle school, attending and completing high school and attending college, with an extra year of maternal exposure to compulsory schooling increasing the probability of these outcomes by between 0.19 and 0.25 percentage points. The effects of paternal exposure to compulsory schooling are remarkably similar to those of maternal exposure.

4.3 Dynastic Effects

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

	Dependent Variable: Years of Schooling							
	Son				Daughter			
	Eldest	Youngest	Most Educated	Least Educated	Eldest	Youngest	Most Educated	Least Educated
CS Years (Mom)	0.018***	0.015***	0.018***	0.015***	0.012***	0.011***	0.011***	0.012***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(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***	0.021***	0.022***	0.022***	0.015***	0.014***	0.014***	0.015***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(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 oldest and youngest children. 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.

We have established that parental exposure to compulsory schooling increases children's education. But are educational outcomes similar across children? Differences in children's educational outcomes may arise in the presence of complementarities between school or parental investments and children's abilities, and parents may have preferences for certain children. For example, parents may focus on one particular child because of limited resources, or may have preferences over children's birth order and gender (e.g., preferential treatment of the first-born son).

We test for such differences by comparing how parental exposure to compulsory schooling affects different 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. We first 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 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. This suggests that parents prefer investing in male children's human capital, especially eldest sons.

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 maternal exposure. This possibly suggests that in this period, men had more say in decisions regarding children's education.

5 Mechanisms

In this section, we explore plausible channels through which parental exposure to compulsory schooling influences children's outcomes, such as school resources, parental assortative mating, parental employment, living arrangements, marriage decisions, and neighborhood sorting.

5.1 School Resources

Table 9: Effect of Parental Exposure to Compulsory Schooling on Children's Years of Schooling with Teacher Controls

	Dependent Variable: Child's Years of Schooling						
	All	Men	Women	Urban	Rural	Black	Post-1900
CS Years (Mom)	0.014***	0.014***	0.014***	0.008**	0.016***	0.015	0.015
	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.009)	(0.011)
Teacher-Student Ratio	0.114***	0.113***	0.115***	0.069***	0.276***	1.170***	0.026
	(0.034)	(0.034)	(0.034)	(0.023)	(0.058)	(0.136)	(0.017)
N (millions)	5.5	3.5	2.0	2.8	2.7	0.4	0.4
R^2	0.16	0.17	0.12	0.09	0.21	0.18	0.19
Outcome Means	10.3	10.0	10.7	11.0	9.5	7.5	9.5
CS Years (Dad)	0.013***	0.015***	0.009**	0.012***	0.008***	0.030***	0.052
	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.011)	(0.034)
Teacher-Student Ratio	0.172***	0.165***	0.185***	0.110***	0.266***	1.149***	0.031
	(0.041)	(0.049)	(0.045)	(0.030)	(0.069)	(0.152)	(0.024)
N (millions)	4.1	2.5	1.5	2.0	2.1	0.3	0.1
R^2	0.17	0.17	0.13	0.09	0.21	0.18	0.21
Outcome Means	10.4	10.1	10.8	11.2	9.6	7.7	9.5

Notes: Effect of parental exposure to compulsory schooling laws on years of schooling, including estimates for teacher-student ratios in the county × 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.

First, we explore how school impact the intergenerational transmission of education. We do this for two reasons. First, to alleviate the worry that our CS exposure results are actually driven by other educational policies and investments, such as expanding the teacher workforce. Second, we want to understand how much, if any, of the effect of compulsory schooling is potentially explained by the school resource channel.

We proceed by creating state-statistics on teacher-student ratios in the 1900, 1910, 1920 and 1930 censuses. We use the occupation question in the censuses to infer working adults' occupations and the school enrolment question to measure school attendance. Then, we create a standardized measure of teachers per students. We create this measure for each enumeration district in the United States for each of these four censuses. Enumeration 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).

Then, we match each individual in our children's sample to the teacher-student ratio in the enumeration district they inhabited at ages 5-14. For example, if an individual is 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 sense of school resources each child was exposed to.

In Table 10, we find that, first of all, after controlling for parental CS exposure, a one-standard deviation increase in teacher-student ratios are associated with a 0.1 to 0.3 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 1.2-year increase in educational attainment. The variation in teacher-student ratios for Black Americans explains a large portion of the intergenerational effects of compulsory schooling. For other demographics, this variation explains relatively little of the intergenerational effect of compulsory schooling.

In section 6, we conduct a more detailed mediation analysis to quantify the impact of including this control and many others, on our integenerational estimates.

5.2 Other Outcomes: Labor Market and Living Arrangements

Compulsory schooling has significant effects on wages and occupational choices (Table 10). Conditional on working, one year of compulsory schooling exposure increases wage earnings by 0.24% and 0.67% for men and women, respectively, a finding that confirms wage effects of CS laws found by Clay, Lingwall and Stephens Jr (2021). When interpreting these results, one must keep in mind that women's labor market participation is only around 10% during this period.

Working women sort into occupations associated to higher education levels, although they are less likely to be employed. Compulsory schooling also has positive effects home ownership for men, while home values are negatively affected. 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 10: Compulsory Schooling Exposure vs Other Outcomes

	log(Wage) (p.p.)	Education Score (0-100)	Earnings Score (0-100)	Employment (p.p.)	log(Rent) (p.p.)	log(Home Value) (p.p.)	Home Ownership (p.p.)
CS Years (Male)	0.237***	-0.002	-0.051**	-0.009	0.075	-0.384***	0.130***
	(0.074)	(0.013)	(0.023)	(0.016)	(0.094)	(0.103)	(0.034)
N (millions)	2.6	3.9	3.9	4.1	1.9	2.1	4.1
R^2	0.13	0.03	0.11	0.01	0.17	0.17	0.04
Outcome Means	$1,427.9	12.3	49.5	89.6	$69.2	$3,314.1	52.7
CS Years (Female)	0.664***	0.102***	0.040	-0.076***	0.119	-0.375***	0.047
	(0.140)	(0.026)	(0.028)	(0.020)	(0.088)	(0.100)	(0.029)
N (millions)	0.7	0.9	0.9	5.7	2.6	2.9	5.7
R^2	0.12	0.06	0.17	0.03	0.18	0.18	0.05
Outcome Means	$570.2	13.5	31.0	14.6	$66.0	$3,273.6	51.7

Notes: Relationship between individual exposure to compulsory years of schooling and labor market and living arrangement outcomes. The top panel is estimated using men from the parents' sample. The bottom panel is estimated using women from parents' sample. Other outcomes explored and unreported because of lack of significance are: labor force participation, 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.

5.3 Assortative Matching

Table 11: Effect of Compulsory Schooling on Assortative Matching

	Dependent Variable: Spouse's Characteristics					
	Schooling (Years)	log(Wage) (p.p.)	Participation Rate (p.p.)	Employment Rate (p.p.)	Education Score (0-100)	Earnings Score (0-100)
CS Years (Female)	0.012***	0.255***	-0.007	-0.026	0.042***	0.072**
	(0.003)	(0.094)	(0.013)	(0.018)	(0.016)	(0.029)
N (millions)	3.4	2.2	3.5	3.5	3.4	3.4
R^2	0.15	0.13	0.00	0.01	0.03	0.10
Outcome Means	8.1	$1,456.1	95.2	90.7	12.5	49.7
CS Years (Male)	0.010***	0.614***	-0.039**	-0.043***	0.075**	0.021
	(0.002)	(0.220)	(0.016)	(0.016)	(0.035)	(0.038)
N (millions)	3.4	0.3	3.5	3.5	0.4	0.4
R^2	0.14	0.10	0.02	0.02	0.05	0.17
Outcome Means	8.4	$552.1	9.6	9.0	14.3	33.5

Notes: Effect of exposure to different compulsory schooling laws on spouse's years of schooling. Each column represents a different regression. The regressions include individuals in the Couples 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.

We find that exposure to CS affects assortative matching. More specifically, it changes the characteristics of a person's future spouse. Individuals exposed to more compulsory schooling marry more educated spouses, on average (see Table 11). A one year increase in compulsory schooling exposure is associated with marrying a wife with 0.010 more years of schooling and a husband with 0.012 more years of schooling. These effects are similar in size to the effects of compulsory schooling on own educational attainment.

We also find that exposure to CS affects other spousal characteristics. Men with more exposure to CS marry higher-earning spouses. Each additional year of compulsory schooling exposure for men is associated to a 0.26% and a 0.61% increase in spousal wages for women and men, respectively. Women marry spouses who work in higher-earning professions and professions associated with higher education levels. Men marry spouses working in higher-education level occupations.

Lastly, men's exposure to CS is linked to 0.04 p.p. decrease in spousal labor force participation and employment. Coupled with the 0.61% increase in their working spouses' wages associated with higher CS exposure, this suggests that men who are more educated and are more likely to earn sufficiently high wages to support a family with a single income. Their spouses would only work if their potential wages were relatively high.

5.4 Neighborhood Sorting

Table 12: Compulsory Schooling and Neighbor Occupational and Labor Market Characteristics

	log(Wage) (p.p.)	Labor Force Participation Men 18-60 (p.p.)	Employment Men 18-60 (p.p.)	Occupational Education Score (0-100)	Occupational Earnings Score (0-100)
CS Years (1910)		0.004	0.008	-0.004	-0.130***
		(0.005)	(0.005)	(0.004)	(0.019)
CS Years (1940)	-0.276***	-0.003	0.024***	-0.016***	-0.093***
	(0.051)	(0.003)	(0.004)	(0.006)	(0.015)
CS Years (1910-1940)		-0.007	0.017***	-0.012**	0.037***
		(0.006)	(0.006)	(0.005)	(0.014)
N (millions)	1.9	1.9	1.9	1.9	1.9
R^2 (1910)		0.16	0.02	0.12	0.25
R^2 (1940)	0.25	0.02	0.09	0.08	0.17
R^2 (1910-1940)		0.10	0.04	0.01	0.03
Outcome Means (1910)		77.6	95.7	10.2	35.1
Outcome Means (1940)	$876.9	91.7	92.5	13.1	41.4
Outcome Means (1910-1940)		14.4	-3.4	3.4	8.4

Notes: Relationship between individual exposure to compulsory years of schooling and neighbor 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 i) 1910, ii) 1940 and iii) 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.

Table 13: Compulsory Schooling and Neighbor Housing and Living Arrangement Characteristics

	Home Ownership (p.p.)	log(Home Value) (p.p.)	log(Rent) (p.p.)	Household Members
CS Years (1910)	0.099***			0.000
	(0.022)			(0.001)
CS Years (1940)	-0.015	-0.305***	0.053	0.002***
	(0.013)	(0.070)	(0.087)	(0.001)
CS Years (1910-1940)	-0.114***			0.002***
	(0.021)			(0.001)
N (millions)	1.9	1.9	1.9	1.9
R^2 (1910)	0.22			0.20
R^2 (1940)	0.13	0.23	0.11	0.21
R^2 (1910-1940)	0.10			0.05
Outcome Means (1910)	51.7			4.5
Outcome Means (1940)	49.8	$3,080.4	82.7	3.7
Outcome Means (1910-1940)	-2.6			-0.8

Notes: Relationship between individual exposure to compulsory years of schooling and neighbor housing and living arrangement characteristics: i) home ownership rate of neighbors (p.p. neighbors living in home owned by household), ii) log of the home value (for owned homes), iii) log of rent paid (for rented homes), iv) outstanding mortgages (for owned homes) and v) number of individuals in the household. Analyses include all individuals in the Parents sample observed in i) 1910, ii) 1940 and iii) in each census between 1910 and 1940. Controls include birth state, birth year, birth region, birth region and birth year interactions, sex and race. SStandard errors are clustered at the birth state by birth year level. *p<0.1; **p<0.05; ***p<0.01.

We hypothesize that if compulsory schooling enables individuals to increase their level of education, get married, marry more educated spouses (see Table 11) and work in professions with higher levels of education and earnings, they may also be able to sort into better neighborhoods. This decision could have important impacts in turn on their children's outcomes, by exposing them to environments with more opportunities to thrive.

We make use of the census tract 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 U.S. household, we construct five dimensions of neighborhood characteristics using the 20 nearest neighboring households living in the same locality. These dimensions are occupational and labor market, housing and living arrangements, demographics, urbanity and, finally, education and human capital. [9]

To explore neighborhood sorting, we set up the following equation, which relates individual i's neighborhood n of residence's characteristics (Y_{ni}) to their exposure to compulsory

[9] Alternatively, 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. This is analogous to the procedure used in Card et al. (2022). These results are presented in the appendix.

Table 14: Compulsory Schooling and Urbanity of Neighborhood

	Urban (p.p.)	Metropolitan (p.p.)	Population (000s)	State Mover (p.p.)	County Mover (p.p.)	Farm Dweller (p.p.)
CS Years (1910)	-0.405***	-0.281***	-7.022***			0.338***
	(0.057)	(0.047)	(1.299)			(0.045)
CS Years (1940)	-0.325***	-0.187***	-4.900***			0.226***
	(0.046)	(0.049)	(0.897)			(0.034)
CS Years (1910-1940)	0.080**	0.094***	2.123***	0.135***	0.061	-0.113***
	(0.039)	(0.030)	(0.660)	(0.033)	(0.042)	(0.033)
N (millions)	1.9	1.9	1.9	1.9	1.9	1.9
R^2 (1910)	0.15	0.23	0.19			0.22
R^2 (1940)	0.07	0.17	0.10			0.13
R^2 (1910-1940)	0.03	0.02	0.10	0.03	0.03	0.02
Outcome Means (1910)	41.6	35.0	244.2			35.3
Outcome Means (1940)	45.7	41.4	173.9			30.9
Outcome Means (1910-1940)	9.8	11.7	-11.4	14.6	36.7	-9.0

Notes: Relationship between individual exposure to compulsory years of schooling and urbanity of neighborhood: i) urban neighborhood, ii) rural neighborhood, iii) population of locality, whether the respondent moved iv) states or v) counties between 1910 and 1940 and vi) proportion of farm-dwelling neighbors. Analyses include all individuals in the Parents sample observed in i) 1910, ii) 1940 and iii) 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. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

schooling years (CS_i):

$$Y_{ni} = \beta\, CS_{s'y'} + \gamma_s + \delta_y + (\eta_r \times \theta_y) + \lambda Race_i + \mu Sex_i + \epsilon_i, \tag{6}$$

The specification is identical to the one in 1 and includes vectors of fixed effects for i's state of birth (s) and birth year (y) cohort ($\gamma^p_{s'}$ and δ_y respectively), interactions ($\eta_r \times \theta_y$) between individual i's region (r) of birth (η_r)[10] 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 in 1910, 1940 and the changes in neighborhood characteristics between 1910 and 1940 for all individuals in the parent samples who are linked linked between the 1910, 1920, 1930 and 1940 censuses. The results are presented in tables 12 to 16. Because of changes in the questions asked in the 1910 and 1940 censuses, the effects on some of the variables can only be measured in one of the census years.

Table 12 shows that compulsory schooling encouraged mobility to neighborhoods with better paying jobs and slightly higher employment levels between 1910 and 1940. Lastly, both

[10] West, Southwest, Midwest, Southeast and Northeast.

Table 15: Compulsory Schooling and Neighbor Demographic

	Black (p.p.)	White (p.p.)	Immigrant (p.p.)	Average Age (Years)	Multifamily Household (p.p.)
CS Years (1910)	-0.013***	0.013**	-0.186***	0.001	-0.060***
	(0.005)	(0.005)	(0.014)	(0.004)	(0.009)
CS Years (1940)	0.007*	-0.010**	-0.068***	0.004	0.037***
	(0.004)	(0.005)	(0.007)	(0.003)	(0.006)
CS Years (1910-1940)	0.020***	-0.023***	0.117***	0.003	0.097***
	(0.005)	(0.006)	(0.009)	(0.003)	(0.010)
N (millions)	1.9	1.9	1.9	1.9	1.9
R^2 (1910)	0.56	0.55	0.41	0.37	0.14
R^2 (1940)	0.52	0.51	0.39	0.30	0.08
R^2 (1910-1940)	0.06	0.06	0.20	0.06	0.06
Outcome Means (1910)	5.7	94.0	14.4	26.8	20.9
Outcome Means (1940)	5.8	94.0	6.1	31.2	11.2
Outcome Means (1910-1940)	-1.4	1.5	-5.5	4.8	-8.4

Notes: Relationship between individual exposure to compulsory years of schooling and demographic characteristics: i) proportion of black neighbors, ii) proportion of white neighbors, iii) proportion born outside the US, iv) proportion of native English-speaking neighbors, v) average age and vi) proportion of multifamily households. Analyses include all individuals in the Parents sample observed in i) 1910, ii) 1940 and iii) 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.

in 1910 and 1940, individuals exposed to more compulsory schooling live in neighborhoods with higher occupational education scores, which are a measure of the average education levels required by occupations.

In terms of living arrangements (Table 13), we find that one addition al year of CS exposure is associated to moving to a neighborhoods with lower home ownership rates (0.11 p.p.) and where household sizes are larger (0.002 individuals), potentially hinting towards migration towards urban areas.

This hypothesis is confirmed by Table 14. CS is also associated to moving towards more urban neighborhoods. Between 1910 and 1940, individuals exposed to one additional CS year are more likely to transition towards localities that are more urban (0.08 p.p.), more metropolitan (0.09 p.p.) and with higher populations (2.1 thousands). They are also 0.14 p.p. more likely to move across state lines. Lastly, their neighbors are 0.11 p.p. less likely to live on farms.

Demographically, exposure to compulsory schooling is associated with mobility towards more diverse neighborhoods. One extra year of CS is associated to moving to neighborhoods having higher proportions of Black (0.007 p.p.) and immigrant (0.07 p.p.) inhabitants and lower proportions of White inhabitants (-0.01 p.p.). These neighborhoods tend to have a higher proportion of multifamily households (0.04 p.p.).

Lastly, CS exposure is associated to moving towards neighborhoods whose inhabitants were also more exposed to compulsory schooling, but where the proportion of children aged 6-

Table 16: Compulsory Schooling and Neighbor Education Characteristics

	In School (6-18, p.p.)	Compulsory Schooling (Years)	Schooling (Years)	Literacy (p.p.)
CS Years (1910)	0.016**	-0.018***		-0.005
	(0.008)	(0.002)		(0.010)
CS Years (1940)	-0.078***	-0.004***	-0.005**	
	(0.017)	(0.001)	(0.002)	
CS Years (1910-1940)	-0.094***	0.014***		
	(0.015)	(0.001)		
N (millions)	1.9	1.9	1.4	1.9
R^2 (1910)	0.25	0.83	0.47	
R^2 (1940)	0.21	0.80	0.34	
R^2 (1910-1940)	0.11	0.06		
Outcome Means (1910)	80.1	4.4		94.4
Outcome Means (1940)	83.1	5.0	8.3	
Outcome Means (1910-1940)	3.6	0.9		

Notes: Relationship between individual exposure to compulsory years of schooling and neighbor educational characteristics: i) proportion aged 6-18 attending school, ii) average years of neighbor compulsory schooling exposure, iii) neighbor years of schooling (over 18) and iv) neighbor literacy rate. Analyses include all individuals in the Parents sample observed in i) 1910, ii) 1940 and iii) 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.

18 who are attending school is lower than in 1910. (Table 16). Overall, these findings suggest that CS exposure is associated to mobility towards more urban, more diverse, higher-wage neighborhoods.

To summarize the findings so far, compulsory schooling exposure positively impacts individuals' educational attainment and outcomes related to financial well-being, such as wages and home ownership. Exposure to compulsory schooling also allows individuals to sort into more urban, higher-wage neighborhoods. These three channels may explain why parental exposure to compulsory schooling has such a large impact on children's outcomes. This exposure is associated to 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 does not only increase parental years of schooling, but it also allows individuals to obtain higher-paying jobs, to marry more educated and higher-earning spouses and to move to migrate to metropolitan areas.

We perform a Gelbach decomposition (Gelbach, 2016) to quantify the relative importance of each of these channels. 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 education and labor market characteristics and neighborhood characteristics.

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). We do this so that 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 17: Decomposition of Intergenerational Effects

	Dependent variable:	
	Child's Years of Schooling	
	(1)	(2)
CS Years (Mother)	0.014***	
CS Years (Father)		0.013***
N (millions)	3.7	3.7
	Relative contribution	
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 17 presents the results of the Gelbach decomposition. First, we find that the estimated effects of mothers' and fathers' CS exposure is similar to our full sample. Second, the decomposition reveals several striking patterns. The controls for parental, parent's spouse and neighborhood characteristics explain roughly 63% and 69% of mothers' and fathers' of the effects of mothers' and fathers' exposure to CS on children's years of schooling, respectively.

The effects of mothers' exposure to CS are explained directly by mothers' education (21%), followed by father's education (17%), hinting at strong assortative mating effects. Mothers' own labor market outcomes matter very little. Additionally, parental housing situation (4%), father's labor market characteristics (4%) explain a small portion of the total effects. Lastly, neighborhood education levels explain almost 21% of the effects, while other neighborhood characteristics, such as neighbor's labor market, housing and demographics matter little.

For the effect of fathers' exposure to CS, only 17% is explained by the father's own education level, while 30% is explained by the wife's schooling, 6% by parental housing characteristics and 2% and 22% by neighborhood demographic and education characteristics.

Interestingly, mother's labor market outcomes, including employment, participation and wages, explain almost none of the effects of parental CS exposure on children's education. This is 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 channel explains relatively little of the variation in children's schooling. The one characteristic that matters most is the neighbors' education levels.

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 is 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, which is sub-optimal and may even lead to some of these comparisons to receive negative weights.

We address these issues in two ways. First, we conduct a decomposition of our TWFE estimator as per Goodman-Bacon (2021).[11] We show the results of this decomposition in Table 18 below and Figures 5 and 6 of the Appendix. Several main takeaways emerge. First, 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 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).

Second, 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 are already treated by the time the 1880 birth cohort starts attending school.

[11] In 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 times.

Table 18: Goodman-Bacon Decomposition

Comparison	Parents Estimate	Parents Weight	Children-Fathers Estimate	Children-Fathers Weight	Children-Mothers Estimate	Children-Mothers 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.

Lastly, The later-treated vs earlier-treated comparisons, which are "forbidden" because they are contaminated by the earlier-treated states receiving treatment during the pre-period despite being part of the control group, receive only about 10% 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 the intergenerational transmission of education.

The second approach we use to validate our results is akin to the methodology used by Cengiz et al. (2019). We manually exclude all treated states from the control group and estimate a simple difference-in-difference model one treated state at a time, for states who first implemented compulsory schooling laws affecting the 1880-1905 cohorts. We plot the estimates in Figures 7 and 8. The main takeaway from this exercise is that the effects of unambitious CS laws (i.e. laws that mandated few compulsory schooling years compared to the pre-law educational attainment distribution) on educational attainment were, as expected, low. For example, many CS laws in New England yield negative estimates because these states had very high educational attainment levels to begin with. Meanwhile, control states with no compulsory schooling laws in 1880 started catching up in terms of educational attainment. At the other end of the spectrum, relatively ambitious CS laws, for example in Louisiana and Florida, had large positive effects, allowing these states to catch up to the rest of the country.

In summary, the staggered implementation of CS laws raises some issues with the difference-in-differences estimator method. However, our estimate does not suffer from some state-by-state comparisons receiving negative weights. Moreover, to the extent that the difference-in-differences estimator uses some "forbidden" comparisons between later-treated and earlier-treated states, this causes downward bias in our estimates. Thus, we are probably underestimating both the direct and the intergenerational effects of compulsory schooling laws.

8 Conclusion

In this paper, we study the intergenerational transmission of education. In the late nineteenth and early twentieth centuries, states across the United States introduced compulsory schooling laws, hoping to raise the educational attainment and boost the social mobility of uneducated and poor families.

Using the linked 1880-1940 full-count censuses and linkages, we can examine 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 that the compulsory schooling laws increased the educational attainment of individuals directly exposed to compulsory schooling laws and 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 fist generation.

The intergenerational effects we find are very large. In fact, compulsory schooling laws has intergenerational effects on educational attainment that were almost as large as the direct effects of the individuals exposed to the laws, across a variety of specifications, including specifications replicating preexisting literature. We attribute these large effects to the particular educational margins the laws affected (0 to 8 years of schooling) and to the rapid secular increases during the early twentieth century in United States, which amplified the effects of compulsory schooling across generations.

We explore potential channels which explain the very strong intergenerational estimates found in the data. We how that exposure to CS had an effect on several parental outcomes that could explain intergenerational transmission. First, CS enabled individuals to earn higher wages and have easier access to home ownership. It also enabled individuals to marry more educated and higher-earning spouses, who work in professions associated with higher educational levels. Lastly, CS impacted migration and neighborhood sorting. Exposure to CS is associated with migration across state lines between 1910 and 1940 and sorting into more urban, more metropolitan and more diverse neighborhoods, where neighbors sort into 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 educational levels of low-education individuals can have very large integenerational effects.

A Appendix Tables and Figures

Table 19: Effect of Parental Years of Schooling on Children's Years of Schooling (IV First Stage)

	Dependent Variable: Parent's Years of Schooling							
	Black et al.	All	Men	Women	Urban	Rural	Black	Post-1900
CS Years (Mom)	0.222***	0.017***	0.016***	0.018***	0.010***	0.011**	0.035***	0.047***
	(0.002)	(0.004)	(0.004)	(0.005)	(0.004)	(0.005)	(0.008)	(0.010)
N (millions)	8.3	8.3	4.7	3.5	4.3	4.0	0.8	0.7
R^2	0.07	0.16	0.15	0.16	0.11	0.21	0.12	0.19
First Stage F-stat	387,238.3	257.3	135.8	122.5	52.6	55.7	31.0	32.5
Outcome Means	10.4	8.0	7.9	8.1	8.5	7.4	5.4	7.6
CS Years (Dad)	0.253***	0.015***	0.014***	0.015***	0.019***	-0.001	0.051***	0.089***
	(0.002)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.011)	(0.029)
N (millions)	5.8	5.9	3.4	2.5	1.6	1.8	0.5	0.2
R^2	0.07	0.16	0.16	0.16	0.09	0.20	0.11	0.26
First Stage F-stat	286,739.8	119.4	68.2	49.4	55.3	0.4	27.9	23.2
Outcome Means	7.9	7.9	7.8	8.0	8.7	7.1	4.7	7.4

Notes: Instrumental variable first stage showing the effect of parental compulsory schooling exposure 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.

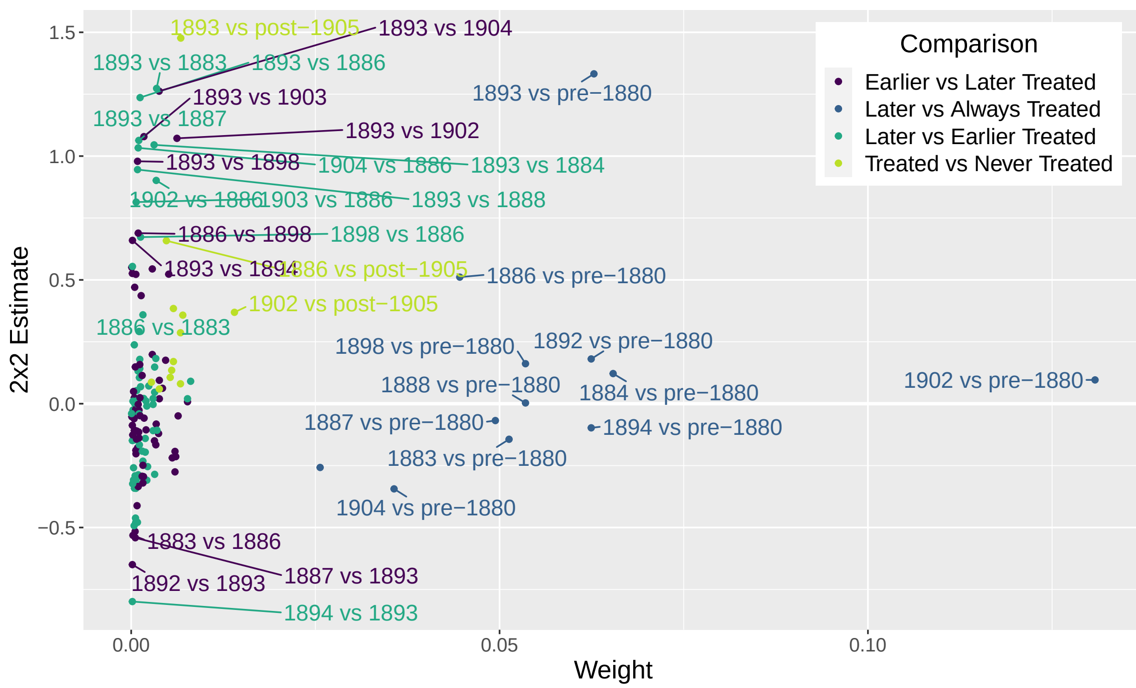

Figure 5: This figure shows the two-by-two simple DD comparisons of the effects of compulsory schooling laws on years of schooling for exposed adults. Each data point is a DD comparison between two groups, where each group is defined as a group of states sharing identical timing of their first compulsory schooling law. Each point is labelled according to the first birth year cohort exposed to compulsory schooling.

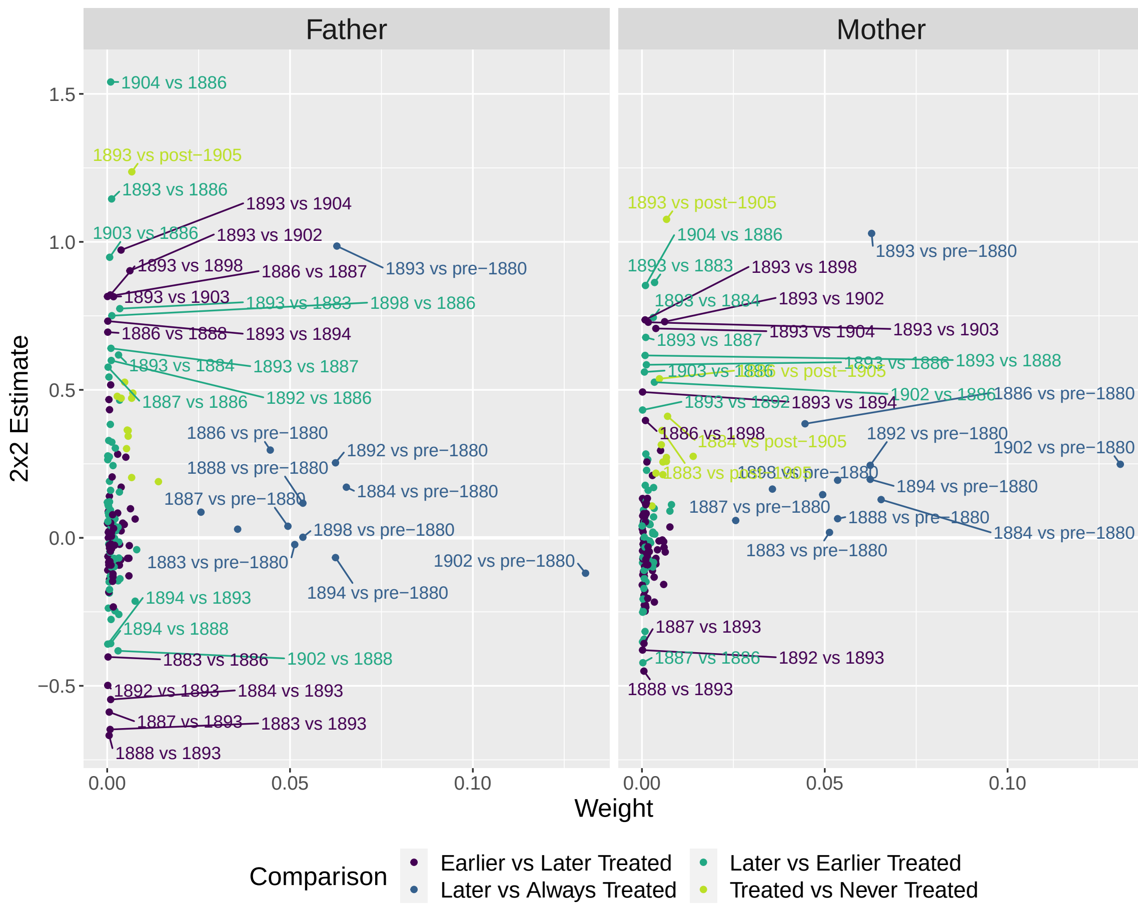

Figure 6: This figure shows the two-by-two simple DD comparisons of the effects of compulsory schooling laws on years of schooling for children whose parents were exposed to compulsory schooling. Each data point is a DD comparison between two groups, where each group is defined as a group of states sharing identical timing of their first compulsory schooling law. Each point is labelled according to the first birth year cohort exposed to compulsory schooling.

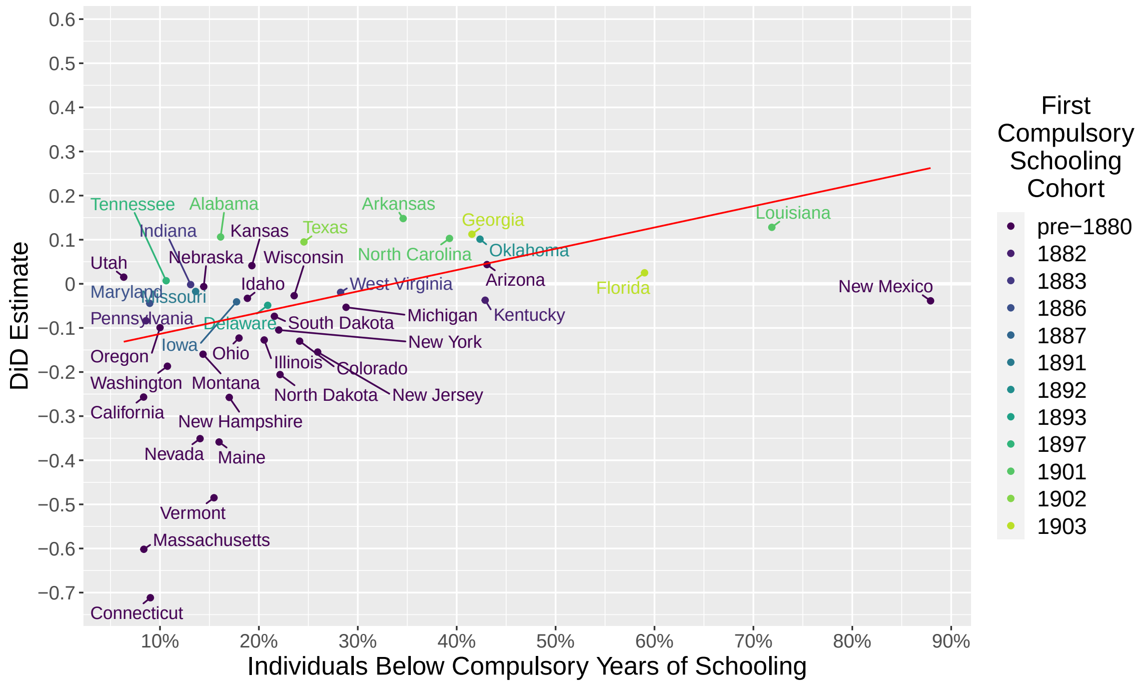

Figure 7: This DiD estimates of exposure to compulsory schooling on parental education for individual states. The control group contains only never-treated states.

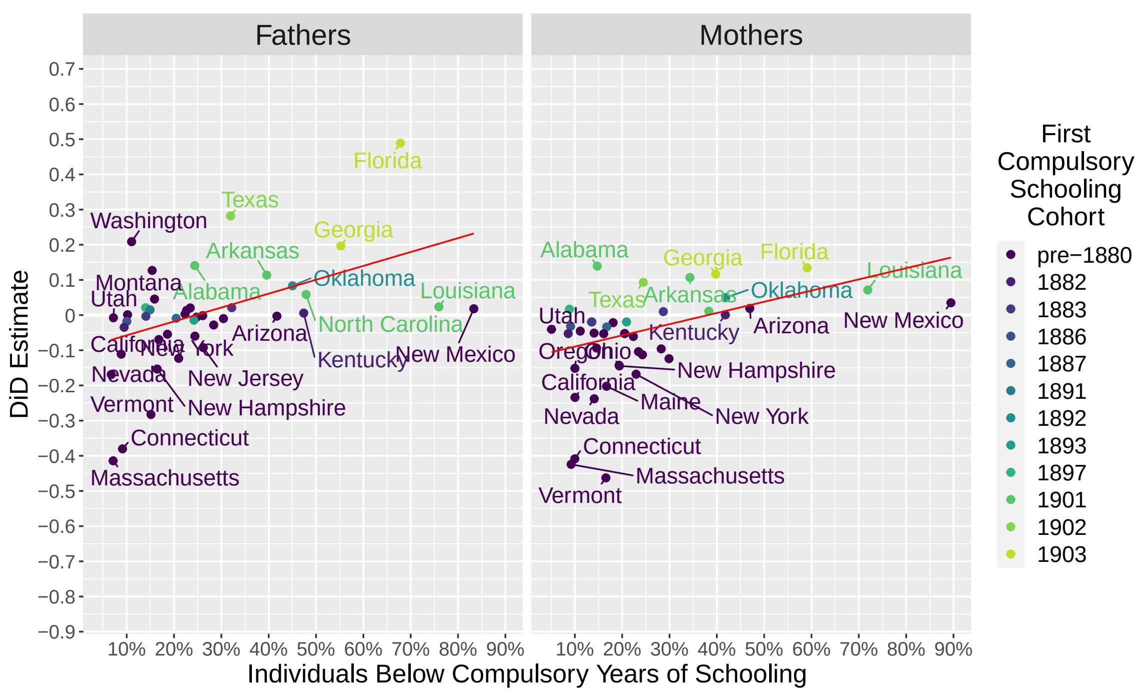

Figure 8: This DiD estimates of parental exposure to compulsory schooling on children's education for individual states. The control group contains only never-treated states.

A.1 Neighborhood Sorting Results Using 20 Closest Households

Table 20: Compulsory Schooling and Neighbor Occupational and Labor Market Characteristics

	log(Wage) (p.p.)	Labor Force Participation Men 18-60 (p.p.)	Employment Men 18-60 (p.p.)	Occupational Education Score (0-100)	Occupational Earnings Score (0-100)
CS Years (1910)		0.016*	0.015***	0.000	-0.149***
		(0.008)	(0.006)	(0.005)	(0.020)
CS Years (1940)	-0.315***	-0.005	0.022***	-0.011*	-0.095***
	(0.056)	(0.005)	(0.005)	(0.006)	(0.015)
CS Years (1910-1940)		-0.021**	0.006	-0.012**	0.053***
		(0.009)	(0.008)	(0.005)	(0.015)
N (millions)	1.9	1.9	1.9	1.9	1.9
R^2 (1910)		0.04	0.01	0.06	0.20
R^2 (1940)	0.22	0.01	0.04	0.06	0.15
R^2 (1910-1940)		0.03	0.02	0.01	0.03
Outcome Means (1910)		78.5	95.8	10.2	35.2
Outcome Means (1940)	$931.3	91.8	92.4	13.8	43.3
Outcome Means (1910-1940)		13.3	-3.4	3.6	8.1

Notes: Relationship between individual exposure to compulsory years of schooling and neighbor 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 i) 1910, ii) 1940 and iii) 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.

Table 21: Compulsory Schooling and Neighbor Housing and Living Arrangement Characteristics

	Home Ownership (p.p.)	log(Home Value) (p.p.)	log(Rent) (p.p.)	Household Members
CS Years (1910)	0.105***			-0.003***
	(0.025)			(0.001)
CS Years (1940)	-0.010	-0.259***	0.056	0.002***
	(0.015)	(0.070)	(0.091)	(0.001)
CS Years (1910-1940)	-0.116***			0.005***
	(0.026)			(0.001)
N (millions)	1.9	1.9	1.9	1.9
R^2 (1910)	0.14			0.10
R^2 (1940)	0.08	0.20	0.10	0.13
R^2 (1910-1940)	0.05			0.03
Outcome Means (1910)	52.3			4.6
Outcome Means (1940)	50.2	$3,251.5	85.0	3.7
Outcome Means (1910-1940)	-2.1			-0.9

Notes: Relationship between individual exposure to compulsory years of schooling and neighbor housing and living arrangement characteristics: i) home ownership rate of neighbors (p.p. neighbors living in home owned by household), ii) log of the home value (for owned homes), iii) log of rent paid (for rented homes), iv) outstanding mortgages (for owned homes) and v) number of individuals in the household. Analyses include all individuals in the Parents sample observed in i) 1910, ii) 1940 and iii) in each census between 1910 and 1940. Controls include birth state, birth year, birth region, birth region and birth year interactions, sex and race. SStandard 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 census[12]
- 1880-1900 to 1930-1940 US Census cross-walk[13]

[12]Ruggles et al. (2021), obtained at `https://usa.ipums.org/usa/index.shtml`. Select the variables and follow the instructions listed in "Variables.txt".

[13]Ruggles et al. (2019), obtained at `https://usa.ipums.org/usa/mlp_downloads.shtml`. follow the instructions listed in "Variables.txt".

Table 22: Compulsory Schooling and Urbanity of Neighborhood

	Urban (p.p.)	Metropolitan (p.p.)	Population (000s)	State Mover (p.p.)	County Mover (p.p.)	Farm Dweller (p.p.)
CS Years (1910)	-0.405***	-0.281***	-7.022***			0.394***
	(0.057)	(0.047)	(1.299)			(0.048)
CS Years (1940)	-0.325***	-0.187***	-4.900***			0.230***
	(0.046)	(0.049)	(0.897)			(0.035)
CS Years (1910-1940)	0.081**	0.094***	2.098***	0.136***	0.062	-0.165***
	(0.039)	(0.030)	(0.660)	(0.033)	(0.041)	(0.037)
N (millions)	1.9	1.9	1.9	1.9	1.9	1.9
R^2 (1910)	0.15	0.23	0.19			0.18
R^2 (1940)	0.07	0.17	0.10			0.12
R^2 (1910-1940)	0.03	0.02	0.10	0.03	0.03	0.02
Outcome Means (1910)	41.6	35.0	244.2			36.4
Outcome Means (1940)	45.7	41.4	173.9			27.3
Outcome Means (1910-1940)	9.8	11.7	-11.4	14.6	36.7	-9.0

Notes: Relationship between individual exposure to compulsory years of schooling and urbanity of neighborhood: i) urban neighborhood, ii) rural neighborhood, iii) population of locality, whether the respondent moved iv) states or v) counties between 1910 and 1940 and vi) proportion of farm-dwelling neighbors. Analyses include all individuals in the Parents sample observed in i) 1910, ii) 1940 and iii) 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.

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).

Table 23: Compulsory Schooling and Neighbor Demographic

	Black (p.p.)	White (p.p.)	Immigrant (p.p.)	Average Age (Years)	Multifamily Household (p.p.)
CS Years (1910)	-0.007	0.017***	-0.208***	0.009**	-0.061***
	(0.006)	(0.007)	(0.015)	(0.004)	(0.011)
CS Years (1940)	0.007*	-0.010**	-0.070***	0.007*	0.053***
	(0.004)	(0.004)	(0.007)	(0.004)	(0.007)
CS Years (1910-1940)	0.015**	-0.027***	0.137***	-0.002	0.114***
	(0.006)	(0.008)	(0.010)	(0.004)	(0.013)
N (millions)	1.9	1.9	1.9	1.9	1.9
R^2 (1910)	0.55	0.54	0.34	0.23	0.06
R^2 (1940)	0.59	0.58	0.31	0.18	0.04
R^2 (1910-1940)	0.02	0.02	0.13	0.03	0.02
Outcome Means (1910)	6.2	93.6	11.1	26.5	18.8
Outcome Means (1940)	4.7	95.0	14.0	26.9	20.0
Outcome Means (1910-1940)	-0.9	0.9	-6.4	5.3	-8.4

Notes: Relationship between individual exposure to compulsory years of schooling and demographic characteristics: i) proportion of black neighbors, ii) proportion of white neighbors, iii) proportion born outside the US, iv) proportion of native English-speaking neighbors, v) average age and vi) proportion of multifamily households. Analyses include all individuals in the Parents sample observed in i) 1910, ii) 1940 and iii) 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.

Table 24: Compulsory Schooling and Neighbor Education Characteristics

	In School (6-18, p.p.)	Compulsory Schooling (Years)	Schooling (Years)	Literacy (p.p.)
CS Years (1910)	0.014	-0.019***		-0.001
	(0.009)	(0.002)		(0.011)
CS Years (1940)	-0.076***	-0.006***	-0.002	
	(0.017)	(0.001)	(0.002)	
CS Years (1910-1940)	-0.090***	0.013***		
	(0.017)	(0.001)		
N (millions)	1.9	1.9	1.4	1.9
R^2 (1910)	0.12	0.80	0.37	
R^2 (1940)	0.11	0.74	0.23	
R^2 (1910-1940)	0.05	0.04		
Outcome Means (1910)	80.2	4.4		94.9
Outcome Means (1940)	83.9	5.3	8.9	
Outcome Means (1910-1940)	3.6	0.9		

Notes: Relationship between individual exposure to compulsory years of schooling and neighbor educational characteristics: i) proportion aged 6-18 attending school, ii) average years of neighbor compulsory schooling exposure, iii) neighbor years of schooling (over 18) and iv) neighbor literacy rate. Analyses include all individuals in the Parents sample observed in i) 1910, ii) 1940 and iii) 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.

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 time
    - 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.

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