Intergenerational Persistence in the Effects of Compulsory Schooling in the United States*

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Abstract

We examine the intergenerational persistence in socio-economic outcomes by exploring the effects of parental exposure to compulsory schooling laws on the outcomes of their children, exploiting the staggered rollout of state compulsory schooling (CS) laws in the second half of the nineteenth and beginning of the twentieth century. We use the linked records from the 1860 to 1940 full-count United States' decennial censuses, which allow us to study i) outcomes of individuals across the full lifespan, ii) a rich set of individual outcomes ranging from educational attainment, to occupational choice and marriage and fertility decisions, and iii) geographic mobility. 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, hinting at much stronger intergenerational transmission of human capital than found in other settings. CS exposure has long-lasting impacts on several key parental outcomes and behaviors (labor market, marriage, fertility, and geographical mobility outcomes) that may explain its inter-generational persistence. Individuals exposed to more CS sort into occupations with higher incomes and higher educational requirements. They also delay marriage, marry more highly-educated spouses, and sort into socio-economically advantaged neighborhoods with: higher rents, higher property prices, lower school dropout rates, and whose inhabitants are more likely to be in the labor force, literate and in higher-earning professions.

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1 Introduction

Public education has long been considered a critical engine of social mobility. The words of nineteenth century American reformer Horace Mann exemplify this idea: *"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). The late nineteenth and early twentieth centuries witnessed the rapid development of public education in the United States (Rauscher, 2016). Against the backdrop of large-scale industrialization and demographic change, nearly every state expanded compulsory schooling requirements and adopted other educational reforms aimed at improving the skills of the population. Indeed, while very few states had any kind of compulsory schooling law in 1880, all states required at least six years of schooling by 1930.

A large existing literature estimates the effects of changes in compulsory schooling 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 effects of compulsory schooling laws on the individuals directly affected by them as children. However, the long-run consequences of these reforms depend crucially on the extent to which their effects persist across multiple generations and thereby reshape the intergenerational transmission of human capital. 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 compulsory schooling reforms in the United States during the late nineteenth and early twentieth centuries using linked records from full count decadal U.S. Censuses spanning 1860-1940. Critically, cross-wave identifiers allow us to track many individuals over time across Census waves. By locating records of individuals during their childhoods, we observe the characteristics of their parents - including parental exposure to compulsory schooling laws.

We exploit the staggered implementation of state compulsory schooling laws to estimate their intergenerational effects using a difference-in-differences framework. The remarkable size and richness of the linked Census data permit several contributions to the literature. First, we are able to estimate the intergenerational effect of exposure to compulsory schooling on adult educational attainment. We find that one extra year of maternal and paternal exposure to compulsory schooling increases children's educational attainment by 0.016 and 0.010 years, respectively. By contrast, cohorts directly exposed to one additional year of compulsory schooling experience a 0.014 year increase in years of schooling. The similarity in the magnitudes of these effects suggests a substantial degree of dynastic persistence in the effects of compulsory schooling.

Second, the richness of the census data and the large sample sizes they afford allow us to test hypotheses about the mechanisms through which intergenerational effects operate. We find evidence of effects on marriage and fertility behaviors. Individuals exposed to more compulsory schooling marry at higher rates, marry at older ages, and marry more educated spouses. We find evidence of effects on labor-market outcomes for women but not men. An extra year of compulsory school exposure increases average wages of women by 0.005 log points for working women, and significantly increases sorting into professions associated with higher earnings and higher average educational levels. The fine level of geographic detail in the censuses allows us to study mobility and sorting across census tracts. Exposure to compulsory schooling increases sorting into census tracts with significantly different housing-market, labor-market and demographic characteristics that reflect higher socio-economic status. Specifically, individuals exposed to more compulsory schooling live in neighborhoods with lower school drop-out rates, lower unemployment levels, higher home ownership rates, higher housing values, higher rent prices, and inhabitants who are more likely to be white, in the labor force, working in higher-earning and higher-education professions, foreign-born and older. Up to one half of these differences in observed census tract characteristics can be explained by inter-neighborhood upward mobility as observed over the decade between sequential censuses. Indeed, respondents who are exposed to more compulsory schooling transition towards more desirable areas between 1920 and 1930 on almost all the dimensions listed above. This upward mobility is especially pronounced for people who move across county lines and in particular, across state lines. Interpreting our findings in the context of literature exploring the importance of neighborhood effects,[1] suggests that sorting into neighborhoods is an important mediator of human-capital transmission.

Our findings contribute to multiple strands of literature on human capital accumulation, education policy, and intergenerational linkages. 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. Several studies estimate the effect of parental education on the *early* educational outcomes of children. Currie and Moretti (2003) find that mothers in the U.S. with easier access to colleges are more likely to have children with better infant health outcomes like birth weight and gestational age. Using NLSY data, Carneiro, Meghir and Parey (2013) estimate 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 reduces the probability that a child is held back a year in school. These studies all focus on the outcomes of children residing with their parents in order to match child outcomes to parental compulsory schooling exposure,. Necessarily, this limits these studies to academic outcomes in childhood before the completion of formal education. By contrast, the linked Census data allow us to estimate the effects of such exposure on the completed educational attainment of the children of exposed individuals.

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 a number policies including changes in compulsory schooling laws, Chevalier et al. (2013) estimate causal effect of parental income and education on the propensity for children to acquire post-compulsory schooling. 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. Black, Devereux and Salvanes (2005) use exposure to this reform as an instrumental variable for parental education, and find little evidence of a causal effect of father's

[1]For example, Chetty, Hendren and Katz (2016).

education, but some evidence of a causal effect of mother's education on the education of sons (but not daughters). By contrast, we find evidence of relatively large causal effects for both father's and mother's education on the attainment of their children. When adopting the same IV strategy as Black, Devereux and Salvanes (2005), we estimate that a one year increase in maternal and paternal schooling result in 0.75 and 1.03 year increases 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.

First, using compulsory schooling as an instrument for parental education in the 1940 census, we find that a one year increase in maternal and paternal schooling result in 0.75 and 1.03 year increases in children's years of schooling, respectively. Notably, these estimates are much larger than those found by Black, Devereux and Salvanes (2005) and robust to including only the fixed effects used by these authors.

In the American context, however, this has remained largely unstudied. An important exception is Oreopoulos, Page and Stevens 2006, who find that parental exposure to more compulsory schooling increases the probability that their co-resident children are the appropriate age for their grade level. The large and significant estimates of the intergenerational transmission of human capital represent a departure from previous studies,[2] that generally find small and often insignificant results. One reason for these differences is plausibly the fact that we study an era of low, but rapidly increasing educational attainment and schooling laws, mainly mandating 1 to 6 years of schooling. These laws thus affected individuals with extremely low levels of formal schooling at a very young age, unlike more recent compulsory schooling legislation (from which most, if not all, of the extant body of evidence is derived), which was instituted during an era of higher educational levels. Compulsory schooling policies in our setup did not only potentially affect a large proportion of the population, but their effects had the potential to compound across generations. First, mechanically, as generally the children faced at least the same number of years of CS as their parents. Second, educational policies, especially in contexts of low initial levels of education, rapid expansion of the schooling system, strong demand for education due to industrialization, and rapid increases in years of compulsory schooling, may have stronger intergenerational effects than previously believed.

Since our baseline estimates are averages across cohorts they include individuals for whom compulsory schooling laws were not binding, and thus may understate relevant marginal treatment effects. We conduct two further analyses that quantify different aspects of the distribution of marginal treatment effects. First, using compulsory schooling as an instrument for parental education in the 1940 census, we find that a one year increase in maternal and paternal schooling result in 0.75 and 1.03 year increases in children's years of schooling, respectively. Notably, these estimates are much larger than those found by Black, Devereux and Salvanes (2005) and robust to including only the fixed effects used by these authors. Second, leveraging the educational distributions of cohorts immediately preceding the introduction of compulsory schooling laws in each state, and comparing these to XXX, we estimate the effect of schooling laws on the students who were compelled to stay in school longer than desired. We find that an additional year's exposure to compulsory schooling for parents who

[2]See, for example Black, Devereux and Salvanes (2005).

were thus compelled to stay in school longer than desired, leads to an increase of 0.39 in own years of schooling and 0.32 and 0.27 years in children's educational attainment for mothers and fathers respectively.

Ferrie (2005), exploring occupational and geographic mobility across generations for the mid nineteenth and early twentieth centuries, using data on fathers and sons linked across US censuses between 1850 and 1920, and contrasting these results with those from Britain and with more recent US cohorts, concludes that the US was occupationally and geographically more mobile than Britain in the mid 19^{th} century, but that this mobility has since diminished (see also, Long and Ferrie, 2007, 2013). What explains this decline? Long and Ferrie (2007, 2013) speculate it may have been due to declines in widely-available public education in the US and in geographic mobility, as economic activity became more homogeneously distributed across the States.

For example, 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. Conversely, Rauscher (2016) found that while compulsory laws made school attendance more equal, they initially reduced (not increased) intergenerational mobility, and this effect subsequently vanished after about a decade.

Our focus is different. Most existing studies in this literature focus on an individual's own exposure to compulsory schooling or other educational reforms, and how this varies by parental levels of education. Intergenerational mobility is then reflected in the individual's obtaining higher levels of education than their parents.

As a robustness check, and in order to better understand which laws were the most effective and therefore drive our identification, we decompose our estimator using the approach developed by Goodman-Bacon (2021). We find that our results are not driven by any particular group of states. However, a general pattern emerges. Compulsory schooling laws affecting cohorts born after 1890 were effective at reducing educational gaps, in particular between the early-adopter states in New England and the late-adopters in the South. This is plausibly due to the fact that compulsory schooling laws in the South affected a large proportion of lower-achieving students compared to other regions, where educational achievement was higher and the laws had less "bite".

This study contributes to the literature on the intergenerational transmission of human capital. Several studies measure the intergenerational effects of school policies on educational outcomes, in particular in Scandinavia (Black, Devereux and Salvanes 2005, Holmlund, Lindahl and Plug 2011, Lundborg, Nilsson and Rooth 2014) and the United States (Oreopoulos, Page and Stevens 2006). Our rich data allow us to study much richer dimensions of human capital than other studies in the United States, including completed years of schooling in adulthood, marriage and fertility, occupational choice and geographic mobility. The effects we uncover are substantially larger in magnitude than those found in the literature. In particular, using a similar instrumental variable approach as Black, Devereux and Salvanes (2005), we find intergenerational effects that are more than ten times larger than those found in that particular and in other similar studies. We propose several explanations. First, the compulsory schooling laws we study target students with lower educational attainments (mostly between 0 and 8 years of schooling) than in Norway (7 to 9 years of schooling). Second, we study a period with very rapid increases in educational attainments across time. In this context, small changes in parental human capital can have amplifying effects on children's education. Lastly, the United States provides us with a context where individuals face fewer safety nets and transfers and in which the incentives to achieve higher levels of education are strong and undistorted, which contrasts with the generous welfare state of Scandinavian countries.[3]

We also contribute to the growing literature on the determinants of, mechanisms of, and heterogeneities in human-capital transmission. We uncover links between compulsory schooling reforms and occupational choice, and delaying marriage. Moreover, we uncover a link between exposure to compulsory schooling and geographic mobility. Recent studies have emerged that highlight pronounced differences in mobility across geographic locations (see Chetty, Hendren and Katz 2016, Connolly, Corak and Haeck 2019, Corak 2020 and Connolly et al. 2020), supporting our finding that geographic mobility may be an important channel through which higher-educated individuals cement the academic and human-capital success of their children.

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, y, cohort, born in a particular 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).[4] 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

[3]See, for example Hertz et al. (2008). The estimated correlation between parental and children's years of schooling, 0.46 in the United States, is significantly higher than that observed in Denmark (0.30), Finland (0.33), Norway (0.35), Sweden (0.4) or the United Kingdom (0.3), while the general pattern of high earnings immobility in the U.S. has long been documented.

[4]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.

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:[5]

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. With the exception of Connecticut, D.C., Massachusetts, Nevada, New Hampshire, Vermont and Wyoming, no states required any years of compulsory schooling for those born in 1860. Indeed, 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. By the time the 1880 cohort was in school, most North-Eastern and Western states had adopted some form of compulsory schooling. For the cohort born in 1900, only those in Southern states were still not required to attend any compulsory schooling. The last cohort in our sample, born in 1907, was subject to some form of compulsory schooling in all states. By 1930 the vast majority of states required 8 years of schooling, with Illinois, Indiana, Nebraska, New Jersey, New Mexico, Ohio and Utah requiring 9 or more years.

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

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

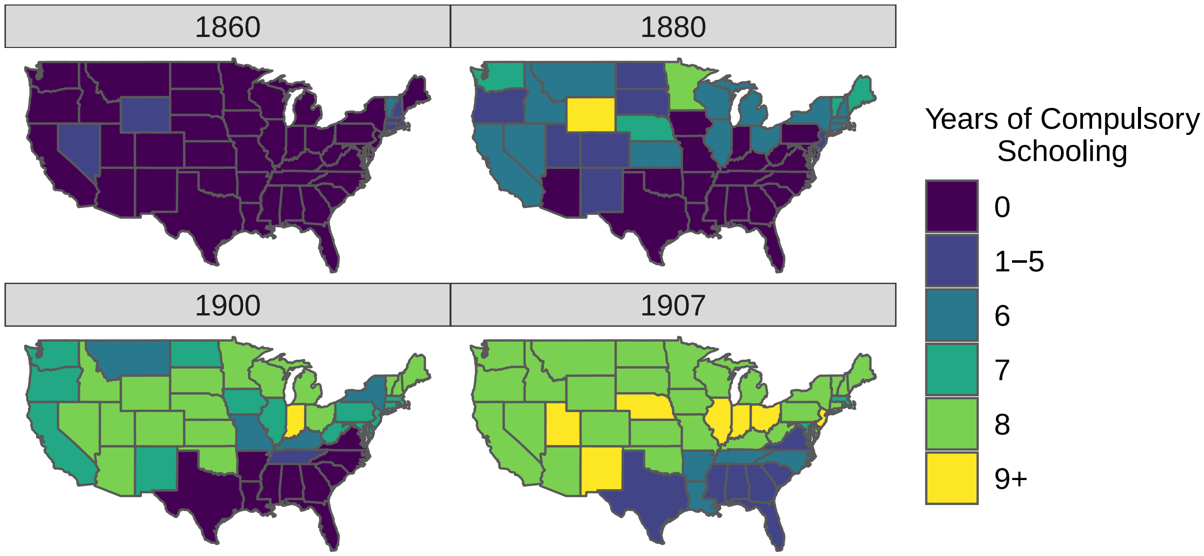

Figure 1: Compulsory schooling law exposure by state and birth year cohort.

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.

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 1930 to 1860 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 and allow us to explore a multitude of outcomes over time, 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.

Table 1: Summary Statistics

	Adults	Children (with Dads)	Children (with Moms)	Couples	Dads	Moms
Observations	46,276,708	9,588,303	7,460,985	28,444,274	4,156,943	5,753,244
Compulsory Schooling (Years)	5.5	6.9	7.1	5.4	4.0	4.1
Completed Schooling (Years)	8.9	10.2	10.3	8.9	8.0	8.1
Proportion Black	10.1%	9.5%	8.1%	8.8%	8.2%	9.8%
Proportion Female	50%	40%	40%	50%	0%	100%
Proportion Urban	55%	50%	50%	55%	50%	55%
Proportion Married	75%	10%	5%	100%	95%	80%
Age	41.5	23.6	22.9	41.7	49.8	48.0
Labor Force Participation Rate	60%	70%	65%	55%	95%	15%
Unemployment Rate	6.6%	16.0%	16.4%	4.7%	5.2%	7.4%
Unemployment Duration (Weeks)	81	55	53	84	92	77
Yearly Labor Earnings ($)	904	504	473	984	962	425
Weekly Hours Worked	40	37	37	41	41	33
Percent Own Home	55%	45%	45%	55%	45%	50%
Home Value ($)	3,402	3,271	3,286	3,354	3,320	3,277
Monthly Rent ($)	70	73	77	71	75	71

Summary statistics for all samples used in this paper: Adults, Children (with Dads, with Moms) and Couples. We also show summary statistics for the Dads and Moms of the Children sample.

We build four main samples of interest:

1. the *Adult* sample: contains all 46,276,708 individuals in the 1940 Census born in the 48 continental states or D.C. who are between 28 and 60 years old in 1940 (i.e. born between 1880 and 1912). These correspond to working-age people who are directly affected by compulsory schooling law changes in the late 1800s and early 1900s.[6]

2. the *Children with Dads* sample: consists of the 9,588,303 individuals in the 1940 Census who are born before 1922 (i.e. at least 18 years of age in 1940) and can be successfully linked to a U.S.-born father in at least one of the U.S. censuses conducted between 1860 and 1940. We further impose restrictions on the father's age: he must be at most 60 years old in the census linking him to his child[7] and must be at least 15 years old at the time of his child's birth.

3. the *Children with Moms* sample: consists of 7,460,985 individuals defined analogously to the *Children with Dads* sample.

[6]We exclude people older than 60, as the strong gradient of mortality in education may bias regression results when using older people. Since we have compulsory schooling laws coded until 1930 and we use exposure to these laws between birth and 18, the youngest cohort we can include is the one born in 1912, who was 18 in 1930 and 28 in 1940.

[7]For example, if a 1940 census respondent's parent is identified in the 1920 census, we include the parent-child pair if the parent is born after 1860. If a child's parent is identified in the 1930 census, we include the parent-child pair if the parent is born after 1870. If the child's parent is only identified in 1940, we include the parent-child pair if the parent is born after 1880. These restrictions ensure that parents are observed when they are at most 60 years old, to prevent high mortality which may be correlated with education, leading to selection bias.

4. the *Couples* sample: consists of all 28,444,274 married individuals in the Adult sample and is used to study assortative mating.

Table 1 presents some basic summary statistics on demographics, education, and selected labor-market outcomes for the four samples of interest. In addition, the columns Dads and Moms provide basic summary statistics on the fathers and mothers in the Children sample.

Of particular note is the average education level of the Children with Dads and Children with Moms samples (10.2 years and 10.3 years of schooling, respectively), which is significantly higher than that of parents (8.0 year for fathers and 8.1 years for mothers). This highlights how this era was defined by rapid increases in educational attainment across generations. Further, even though mothers had similar levels of educational attainment as fathers, their labor-force participation was very low (15%). Lastly, males are over-represented in our Children sample (60%). 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.

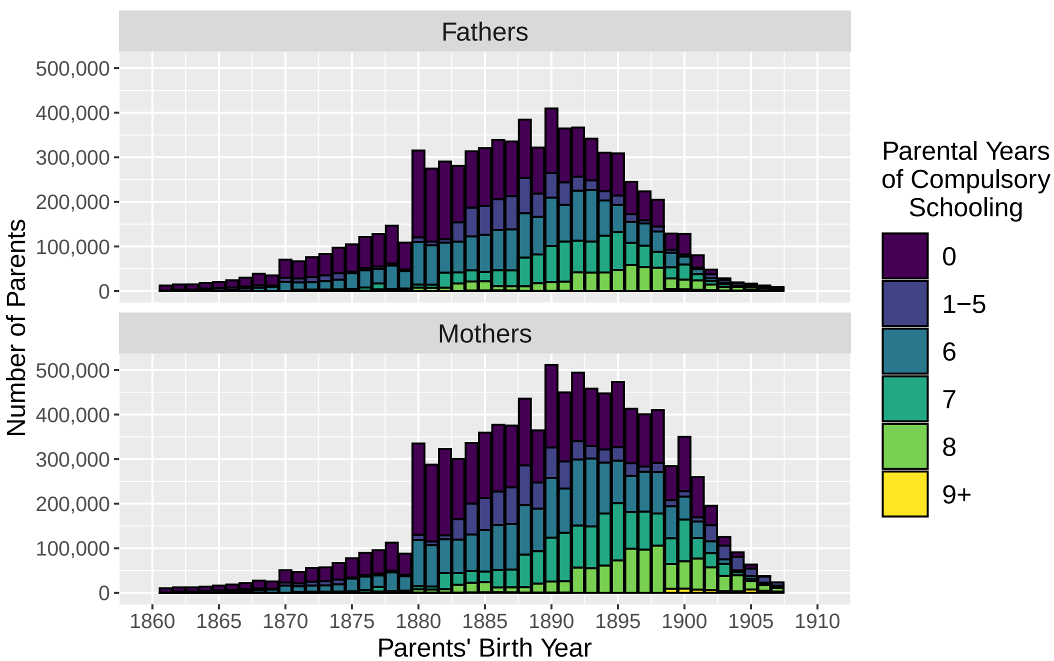

Figure 2: US-born parents by birth year and own exposure to compulsory schooling in the Children with Dads (top) and Children with Moms (bottom) samples.

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 of parents is in 1890. Parents born in this year were age 50 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.

The bell-curve patterns in Figure 2 can be explained by the selection of children to be aged at least 18 in the 1940 census (in order for them to have completed their schooling) and for parents to be at most 60 in the census used to link the parent to his/her child, i.e. 1940, 1930, etc. (to avoid selection issues that may arise due to the education-mortality gradient).

For example, a mother from the 1908 cohort would roughly have had children between 1929-1933 (when she was between the ages of 21 to 25)[8] and be of age 32 in the 1940 census, when her children were between ages 7 and 11 and therefore dropped from the sample, explaining the lack of observations past 1908. Conversely, a mother from the 1880 cohort would roughly have had children between 1901-1905 and be of age 60 in the 1940 census, with children between the ages of 35 to 39. Therefore, parents born before 1880 (who were older than 60 in 1940) were dropped from the 1940 census, explaining the sharp drop-off in the number of parents identified before 1880 (see Figure 2). However, some of these parents and children could still be linked in the 1930 census. Parents in the 1930 census were older than 60 for cohorts before 1870 (indeed a small drop is here visible too), etc. Last, a mother from the 1860 cohort would have had children between 1881 and 1885. These would be aged 55 to 59 in 1940, i.e., these children are about to be dropped from the sample, as they are nearing 60, explaining how the sample drops for parental cohorts before 1860.

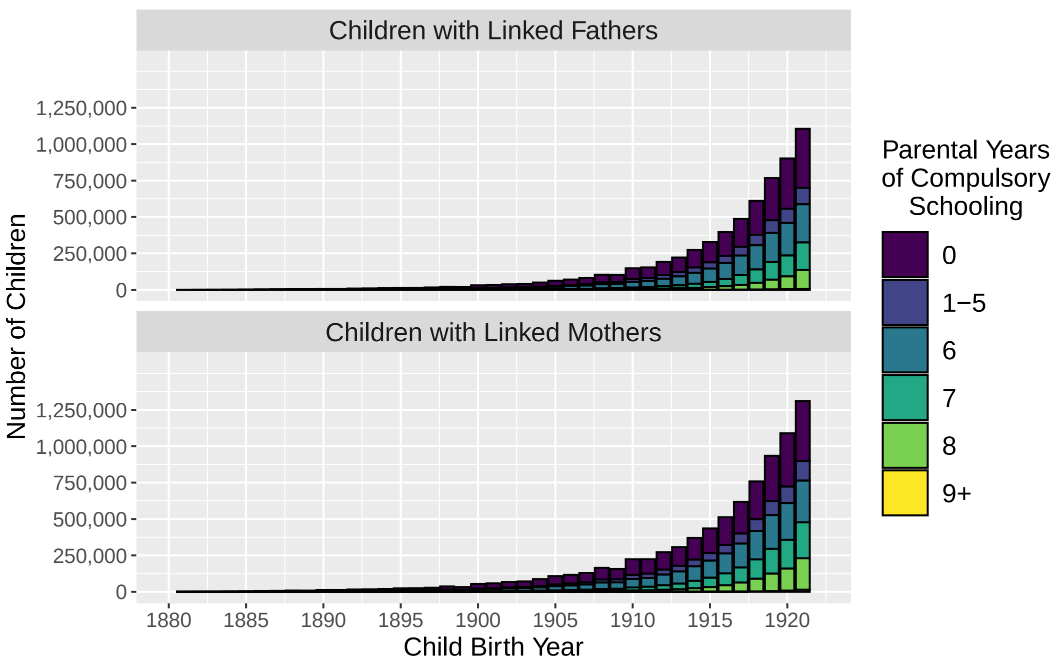

Figure 3: Children's exposure to parental years of compulsory schooling by child birth year in the Children with Dads (top) and Children with Moms (bottom) samples.

Figure 3 shows the distribution of birth years and *parental* exposure to compulsory schooling for the Children's sample. The Children sample grows rapidly with birth year, for two main reasons. First, this was an era of relatively rapid growth in the United States popula-

[8]In 1910 the mother's age at first birth was 21 and 70% of children were born before age 25 (Kirmeyer and Hamilton, 2011).

tion, from 76.2 million in 1900 to 106.0 million people in 1920 (census.gov), through natural births and immigration. Second, as the earlier discussion of Figure 2 revealed, the size of the Children with Parents sample drops with every census we lose. Here this plays out in a slightly different way. For example, a child born in 1922 would have been 18 year old in the 1940 census and may still live at home. A child born in 1917, however, is 23 in the 1940 census and less likely to still live with her parents. The 1940 census can then not be used to link her to her parents. She then has to be linked to her parents in the 1930 (or an earlier) census. As we move to earlier cohorts, the sample size falls as successive censuses are effectively removed. 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 (CS_{sc}^p) their birth state (s) birth year (y) cohort were exposed to:

$$Educ_i^p = \beta^p\, CS_{sy}^p + \gamma_s^p + \delta_y^p + \left(\eta_r^p \times \theta_y^p\right) + \lambda^p Race_i^p + \mu^p Sex_i^p + \epsilon_i^p, \quad p = m, f \tag{1}$$

where we include vectors of fixed effects for i's state of birth (s) and birth year (y) cohort (γ_s^p and δ_y^p respectively), interactions ($\eta_r^p \times \theta_y^p$) between individual i's region (r) of birth (η_r^p)[9] and birth year (y) cohort (δ_y^p), 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_{sy}^p is identified from variation across states of birth (s) and birth year (y) cohorts, conditional on regional trends (captured by the region and birth year cohort interactions $\eta_r^p \times \delta_y^p$), state differences in levels (captured by state fixed effects, γ_s^p) and cohort differences in levels (captured by birth year cohort fixed effects, δ_y^p). These analyses use the parents of the Children with Dads and Children with Moms sample and estimate effects separately for mothers (m) and fathers (f).

Our main focus is on the inter-generational 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_{sy}^p) of the child's parents:

$$Educ_i^c = \beta^c\, CS_{sy}^p + \gamma_s^c + \delta_y^c + \left(\eta_s^c \times \theta_y^c\right) + \gamma_s^p + \delta_y^p + \left(\eta_r^p \times \delta_y^p\right) + \lambda^c Race_i^c + \mu^c Sex_i^c + \epsilon_i^c, p = m, f \tag{2}$$

where, analogous to Equation 1, we include vectors of child fixed effects for the child's state of birth (γ_s^c) and birth year (δ_y^c), and interactions ($\eta_s^c \times \theta_y^c$) between the child's state (s) of birth (η_s^c) and birth year (δ_s^c), 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 for trends that differ between states, as opposed to trends by region. These controls capture state-birth year effects, such as children's own exposure to compulsory schooling. This is important because children

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

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, in Equation 1 we also include vectors of parent fixed effects for the parents' state of birth (γ_s^p) and birth year (δ_y^p), and interactions ($\eta_r^c \times \theta_y^c$) between the parents' region (r) of birth (η_r^p) and birth year (δ_s^p).

The effect of parental exposure to compulsory schooling laws CS_{sy}^p is identified from variation across states of birth (s) and birth year (y) cohorts.

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 and year of birth level - varies.

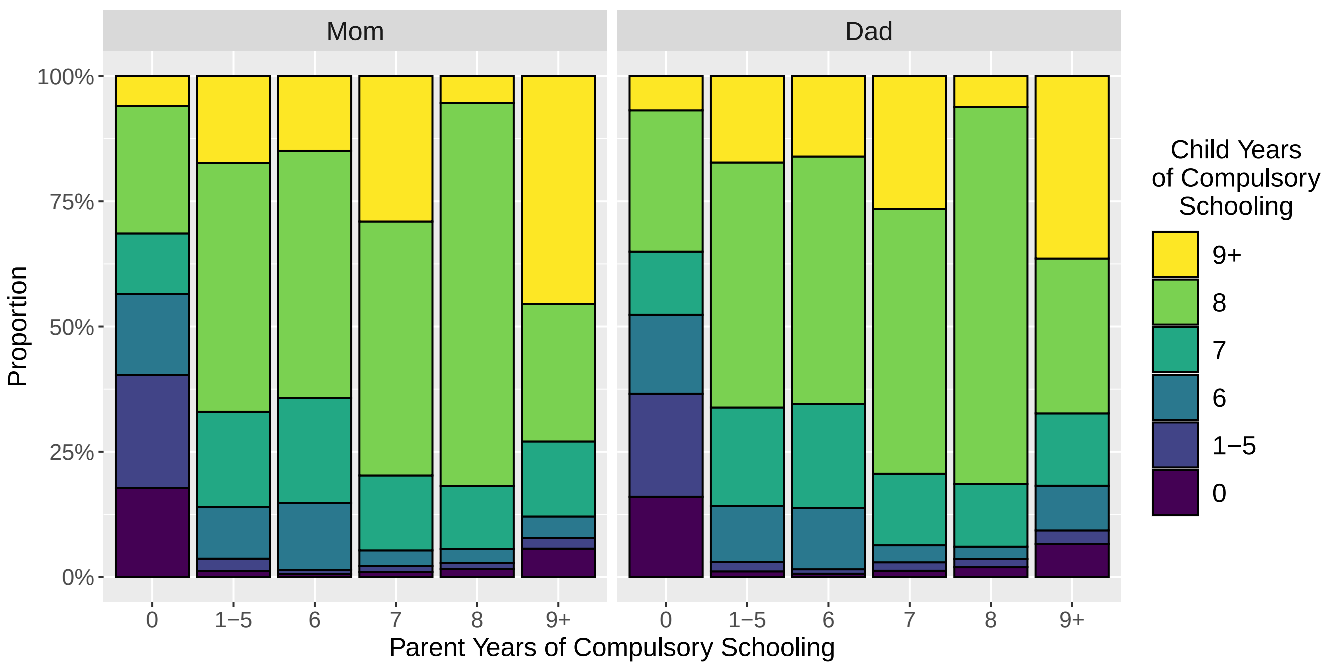

Figure 4: Relationship between parental (horizontal axis) and child exposure to compulsory schooling (color-coded years of CS), separate for mothers (left) and fathers (right). We include respondents (the "children") in the Children with Mons and Children with Dads samples, where the children are at least 18 at the time of our latest observed compulsory schooling reforms of 1930 and compare their exposure to years of compulsory schooling (color code) with the exposure to years of compulsory schooling of their mothers (left) and fathers (right) (horizontal axis).

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 of the children's birth state and birth year thus becomes very important.

The second challenge is highlighted by Stephens Jr and Yang (2014). This study finds that when controlling for birth region and birth year fixed effects interactions, most of the effects of compulsory schooling laws on various outcomes (ranging from health to educational and labor-market outcomes) are not significant. To address this, in equation 1, we control for region of birth interactions. In equation 2, we control for parent birth region (r) and birth-year cohort (c) interactions, as well as child birth state and birth year interactions in equation. Moreover, we cluster the standard errors conservatively, using two-way birth state (s) and birth-year cohort (c) clustering.[10]

Lastly, our specification is a two-way fixed effects strategy that is potentially sensitive to heterogeneous treatment effects and the differential timing of treatments across states, highlighted in the recent difference-in-differences literature.[11] Our setup is the most complicated case of difference-in-differences because it features the following complications: staggered implementation of treatments (different states pass compulsory schooling laws at different times), treatments with different intensities (laws can mandate different numbers of compulsory years of schooling), varying treatment intensities across time (states pass different compulsory schooling laws at different points in time) and non-permanent treatments (states can scale down compulsory schooling or abandon it altogether it). To address these potential complications, we estimate a version of our model with the compulsory schooling law exposure coded as a binary treatment. This allows us to use the framework of Goodman-Bacon (2021) and Sun and Abraham (2021), for example, to better understand which treated states drive our results and to confirm that they are robust to the weighing and heterogeneous treatment effects highlighted by this literature.

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 poreidt 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 Black, Devereux and Salvanes (2005). However, the shortcoming of this approach is that the exclusion restriction is probably violated. Indeed, parental exposure to compulsory schooling may affect children's education through other channels that parental education, as it affects entire cohorts of parents, for several cohorts. This may cause spillovers and may have general equilibrium effects on labor markets, in particular. Nonetheless, the instrumental variable approach has a very natural interpretation,

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

[11]For example, De Chaisemartin and d'Haultfoeuille (2020), Callaway and Sant'Anna (2021), Goodman-Bacon (2021) and Sun and Abraham (2021).

causally linking increases education to increases in children's education.

In this approach, the first stage relates education $Educ_p$ of parent p born in state s and year yp to their exposure to compulsory schooling CS^p_{sy}:

$$Educ^p_i = \beta^p\, CS^p_{sy} + \gamma^p_s + \delta^p_y + \left(\eta^p_r \times \theta^p_y\right) + \lambda^p Race^p_i + \mu^p Sex^p_i + \epsilon^p_i \quad p = m, f \tag{3}$$

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

$$Educ^c_i = \beta^c\, \widehat{Educ^p_i} + \left(\gamma^c_s \times \delta^c_y\right) + \gamma^p_s + \delta^p_y + \left(\eta^p_r \times \delta^p_y\right) + \lambda^c Race^c_i + \mu^c Sex^c_i + \epsilon^c_i \quad p = m, f \tag{4}$$

One 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 (county) of residence and birth year and child's year of birth.

4 Main Results

4.1 Direct Effects of Compulsory Schooling (Adults sample)

Table 2 presents the estimates of the effect of compulsory schooling laws on years of schooling for individuals directly exposed to them in the Adults sample (equation 1). Column 1 shows an effect across all groups of a 0.014 increase in years of schooling that is only moderately statistically significant (at the 10 percent level). These results are driven by women, who experience an increase by an average of 0.016 years of schooling for each year of compulsory schooling they were exposed to. For men, this figure is a statistically insignificant 0.012 years.

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

	Dependent Variable: Years of Schooling						
	All	Men	Women	Urban	Rural	Black	Post-1890
CS Years	0.014*	0.012	0.016*	0.016**	0.010	0.027**	0.023**
	(0.008)	(0.008)	(0.009)	(0.008)	(0.009)	(0.010)	(0.010)
Observations	46,175,175	22,960,049	23,215,126	26,316,448	19,858,727	4,675,506	36,788,697
R^2	0.176	0.178	0.174	0.138	0.229	0.130	0.172
Age	X	X	X	X	X	X	X
Birth State	X	X	X	X	X	X	X
Region	X	X	X	X	X	X	X
Region-Age	X	X	X	X	X	X	X
Sex	X			X	X	X	X
Race	X	X	X	X	X		X

Notes: Effects of exposure to compulsory schooling laws on years of schooling for the Adult sample. Each column represents a different regression. Standard errors are clustered using birth year and birth state two-way clustering. *p<0.1; **p<0.05; ***p<0.01.

We do, however, obtain statistically significant results (at the 5 percent level) for Urban dwellers, African Americans and those born after 1890. 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. The effects on African Americans are the largest at 0.027 years of schooling, 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. Consistent with this, we find significant effects on cohorts born after 1890, highlighting the more adequate enforcement of these laws in the late nineteenth and twentieth centuries. Our results are generally 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 for the overall Adult sample.

Table 3 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.[12] Indeed, one additional year of compulsory schooling exposure increases the probability of attending grade school, graduating from grade school and attending some middle school by 0.07 p.p., 0.33 p.p. and 0.32 p.p., respectively. This is consistent with the compulsory schooling laws of this era, which imposed between 6 and 9 years of mandatory schooling. Interestingly, this suggests that compulsory schooling was more effective on the intensive than on the extensive margin: the effect was to encourage those who were enrolled into schools to pursue more years of schooling, rather than inducing student who never attended school to enrol in the first place.

Table 3: Effect of Compulsory Schooling Laws on Completion

	Dependent Variable: Completion							
	Some GS	Grade School (GS)	Some MS	Middle School (MS)	Some HS	High School (HS)	Some College	College
CS Years	0.069*	0.326**	0.324**	0.117	-0.045	-0.027	-0.046	-0.005
	(0.038)	(0.123)	(0.147)	(0.151)	(0.110)	(0.103)	(0.042)	(0.021)
Observations	46,276,708	46,276,708	46,276,708	46,276,708	46,276,708	46,276,708	46,276,708	46,276,708
R^2	0.054	0.201	0.204	0.101	0.070	0.060	0.017	0.010
Race	X	X	X	X	X	X	X	X
Gender	X	X	X	X	X	X	X	X
Birth State	X	X	X	X	X	X	X	X
Birth Year	X	X	X	X	X	X	X	X
Birth Region	X	X	X	X	X	X	X	X
Birth Birth R-Y	X	X	X	X	X	X	X	X

Notes: Effect of exposure to compulsory schooling on completion for individuals in the Adult 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 educational attainment. Standard errors are clustered using birth year and birth state two-way clustering. *p<0.1; **p<0.05; ***p<0.01.

[12] 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 Parental Exposure to Compulsory Schooling on Years of Schooling

	Dependent Variable: Child's Years of Schooling						
	All	Men	Women	Urban	Rural	Black	Post-1890
CS Years Mom	0.013***	0.013***	0.013***	0.008*	0.013***	0.015	0.016***
	(0.004)	(0.005)	(0.003)	(0.004)	(0.003)	(0.011)	(0.005)
Observations	9,588,303	5,551,201	4,037,102	4,990,107	4,598,196	906,868	5,258,931
R^2	0.185	0.191	0.156	0.121	0.228	0.150	0.202
CS Years Dad	0.013***	0.013***	0.012***	0.012***	0.005	0.026**	0.019***
	(0.004)	(0.004)	(0.003)	(0.002)	(0.004)	(0.010)	(0.004)
Observations	7,460,985	4,360,606	3,100,379	3,613,250	3,847,735	605,226	3,124,068
R^2	0.176	0.183	0.144	0.099	0.214	0.149	0.195
Race	X	X	X	X	X	X	X
Birth State	X	X	X	X	X	X	X
Birth Year	X	X	X	X	X	X	X
Birth State-Year	X	X	X	X	X	X	X
Parent Birth State	X	X	X	X	X	X	X
Parent Birth Region	X	X	X	X	X	X	X
Parent Birth Year	X	X	X	X	X	X	X
Parent Birth Region-Year	X	X	X	X	X	X	X

Notes: This table shows the effect of parental exposure to different compulsory schooling laws on years of schooling for the Children sample. Each column represents a different regression. Standard errors are clustered using birth year and birth state two-way clustering. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

4.2 Intergenerational Effects of Compulsory Schooling

We present our main results regarding the intergenerational transmission of compulsory schooling. We first show the instrumental variable results Table 5, where we instrument parental years of schooling in 1940 with their exposure to compulsory schooling laws.[13] Across various specifications and samples, the effects are an order of magnitude larger than the ones obtained Black, Devereux and Salvanes (2005). Column 1, in particular, replicates the exact specification used in the aforementioned-study. Remarkably, a 1 year increase in maternal (paternal) education resulting from exposure to compulsory schooling is responsible for a 1 year (0.80 years) increase in children's schooling. These very large estimates can be explained by several factors. First, the Progressive Era of the United States is characterized by very rapid increases in mean educational attainment. The effects of parental schooling on children are then plausibly compounded by the mere fact that attainment is exploding with each successive generation. Second, the compulsory schooling laws in the United States, unlike the Norwegian studies, mainly target students who have between zero and six years of schooling. Thus, the policy affects very low attainment individuals, some of whom would have not completed any schooling and might have 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 to 9 years, further increasing the educational attainment of individuals who had already spent at least 7 years in school.

Second, we present our main results in Table 4. It contains estimates of Equation 2, highlighting the effects of parental exposure to compulsory schooling on children's education,

[13]First stage results are presented in Table 15 of the Appendix. All first stage F-stats are highly significant, ruling out weak instruments.

Table 5: Effect of Parental Years of Schooling on Children's Years of Schooling (IV Second Stage)

	Black et al.	All	Men	Women	Urban	Rural	Black	Post-1890
Years of Schooling (Mom)	0.995***	0.750***	0.777***	0.689***	0.657***	0.956***	0.425***	0.909***
	(0.005)	(0.145)	(0.165)	(0.126)	(0.134)	(0.250)	(0.135)	(0.240)
Observations	5,749,267	5,749,267	3,329,889	2,419,378	2,754,333	2,994,934	408,493	3,583,198
Years of Schooling (Dad)	0.802***	1.025***	1.177**	0.805***	0.754***	-1.110	0.559***	1.450***
	(0.003)	(0.300)	(0.481)	(0.026)	(0.100)	(2.736)	(0.158)	(0.470)
Observations	5,749,267	5,749,267	3,329,889	2,419,378	2,754,333	2,994,934	408,493	2,544,870
Race		X	X	X	X	X	X	X
Birth State		X	X	X	X	X	X	X
Birth Year	X	X	X	X	X	X	X	X
Birth State-Year		X	X	X	X	X	X	X
Parent Birth State		X	X	X	X	X	X	X
Parent Birth Region		X	X	X	X	X	X	X
Parent Birth Year	X	X	X	X	X	X	X	X
Parent Birth Region-Year		X	X	X	X	X	X	X
County of Residence	X							

Notes: This table shows the effect of parental years of schooling on years of schooling for the Children sample using an instrumental variable approach, where parental compulsory schooling exposure is an instrument for parental education. Each column represents a different regression. Standard errors are clustered using birth year and birth state two-way clustering, except for the first column, which uses county-by-parent birth year clustering. *p<0.1; **p<0.05; ***p<0.01.

separately for mother's and father's exposure (Top and Bottom Panels respectively). Column (1) of each panel contains our estimates for the baseline Children sample.[14] The results suggest statistically significant effects of parental exposure to compulsory schooling on the educational attainment. Maternal exposure to an additional year of compulsory schooling increased completed education by 0.013 years. The results in the Bottom Panel suggest similar effects associated with paternal exposure to compulsory schooling: an additional year of parental schooling is associated with an increase of 0.013 years of completed education for the child. All estimates, save for the exposure of urban mothers, Black mothers and rural fathers to compulsory schooling and are statistically significant at a 95% confidence level.

The successive columns of Table 4 provide estimates of our main specification in five subsamples of interest: men, women, urban, rural, black and those born after 1890, respectively. The magnitude of the estimates is similar for these subsamples, with some notable exceptions. First, paternal exposure to education for rural men does not have significant effects on completed years of schooling. Second, paternal exposure to compulsory schooling for blacks yields substantially higher effects than the baseline estimates for men. Lastly, the estimates for respondents with both parents born after 1890 are higher for both men and women. This is in line with literature suggesting that early compulsory schooling laws were not effective due to to low accessibility to schools, little budget allocated to enforcement mechanisms and unwillingness or inability of local officials to implement the new laws.[15] Additionally, the last column of the table raises questions regarding which state laws are driving our results.

[14]Table 16 of the Appendix shows similar results, estimated on an expanded version of the Children's sample, which also includes children with only one identified parent in the Adults sample.

[15]See, for example Katz (1976) and Harris (1893).

Table 6: Effect of Compulsory Schooling Laws on Child's Educational Attainment

	Dependent Variable: Child's Years of Schooling							
	Some GS	Grade School	Some MS	Middle School	Some HS	High School	Some College	College
CS Years Mom	0.012***	0.073***	0.072***	0.169***	0.191***	0.195***	0.073	0.037
	(0.004)	(0.020)	(0.022)	(0.057)	(0.053)	(0.051)	(0.047)	(0.033)
Observations	9,588,303	9,588,303	9,588,303	9,588,303	9,588,303	9,588,303	9,588,303	9,588,303
R^2	0.015	0.145	0.176	0.145	0.129	0.124	0.041	0.038
CS Years Dad	0.008**	0.067**	0.081**	0.146**	0.145**	0.162**	0.102***	0.061**
	(0.004)	(0.025)	(0.031)	(0.070)	(0.063)	(0.066)	(0.038)	(0.026)
Observations	7,460,985	7,460,985	7,460,985	7,460,985	7,460,985	7,460,985	7,460,985	7,460,985
R^2	0.014	0.131	0.162	0.138	0.125	0.123	0.043	0.041
Race	X	X	X	X	X	X	X	X
Gender	X	X	X	X	X	X	X	X
Birth State	X	X	X	X	X	X	X	X
Birth Year	X	X	X	X	X	X	X	X
Birth State-Year	X	X	X	X	X	X	X	X
Parent Birth State	X	X	X	X	X	X	X	X
Parent Birth Region	X	X	X	X	X	X	X	X
Parent Birth Year	X	X	X	X	X	X	X	X
Parent Birth R-Y	X	X	X	X	X	X	X	X

Notes: This table shows the effect of parental exposure to compulsory schooling on educational attainment for individuals in 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 attainment. The regressions include observations in the Children sample. Standard errors are clustered using birth year and birth state two-way clustering. *p<0.1; **p<0.05; ***p<0.01.

We dedicate a whole latter section to exploring our two-way fixed effects estimator in order to understand how it is identified and if it suffers from any of the issues highlighted in the recent difference-in-difference literature.

The results in Table 4 also raise the question of which margin of schooling is affected by parental exposure to compulsory schooling laws. It could be the case that the effects of parental compulsory schooling are largely confined to the lower end of the distribution of educational attainment. This could arise if, for example, parental educational attainment establishes a floor for the expected educational attainment of children. Parents may find it desirable to ensure that their children obtain at least as much formal schooling as they themselves received. Alternately, an increase in required schooling could increase the changes that children obtain even more educational attainment than their parents. We next test for the margins along which higher levels of parental compulsory schooling exposure affected children's educational attainment. We define a set of indicator variables $\text{Attainment}_i^{\ell}$ that take a value 100 if an individual ever reaches educational level ℓ, and 0 otherwise, so that our 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 6 presents estimates of our main specification (Column 1 in Table 4) with these degree dummies as the main outcomes of interest.

The results in Table 6 suggest that parental exposure to compulsory schooling had positive effects on degree completion across the entire distribution of educational attainment. We find evidence that an increase in parental exposure to compulsory schooling increased the probability of attaining all of the eight categories considered. The largest effects are on attending and completing middle school and high school and attending college, with an extra year of maternal exposure to compulsory schooling increasing the probability of these outcomes by between 0.17 to 0.20 percentage points. The effects of paternal exposure to compulsory schooling mirror those of maternal exposure. However, the magnitudes of the estimates are smaller. On the other hand, parental exposure to compulsory schooling leads to statistically significant increases in child attainment all the way to college graduation.

4.3 Dynastic Concerns

We have seen that parental exposure to compulsory schooling leads to increases in children's education. However, parents may choose to invest unequally into their children's education. This could arise for several reasons, including: complementarities between parental investment and individual children's abilities, parents maximizing their family (or dynasty) well-being by heavily investing into the education of one child or preferences over children's birth order and gender (e.g. preferential treatment for the first-born son). We test this hypothesis by asking if parental exposure to compulsory schooling affects different family-level outcomes: the maximum, minimum, average years of schooling of all their children and the years of schooling of the eldest and youngest sons and daughters. These results are presented in Table 7. Although there is suggestive evidence that exposure to compulsory schooling of both mothers and fathers

Table 7: Parental Exposure to Compulsory Schooling vs Dynasty Education

	Any Child			Oldest			Youngest		
	Max	Min	Mean	Any	Male	Female	Any	Male	Female
CS Years Mom	0.016	0.014	0.015	0.016	0.016	0.013	0.014	0.015	0.012
	(0.010)	(0.009)	(0.009)	(0.010)	(0.010)	(0.009)	(0.009)	(0.010)	(0.009)
N	6,637,787	6,637,787	6,637,787	6,637,787	4,446,000	3,449,226	6,637,787	4,446,000	3,449,226
R^2	0.12	0.12	0.13	0.12	0.13	0.10	0.12	0.14	0.10
CS Years Dad	0.016*	0.015*	0.016*	0.017*	0.016*	0.014*	0.015*	0.014*	0.014*
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Num.Obs.	5,151,934	5,151,934	5,151,934	5,151,934	3,480,794	2,633,986	5,151,934	3,480,794	2,633,986
R^2	0.11	0.11	0.11	0.11	0.12	0.09	0.11	0.12	0.09

Notes: This table shows the 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. Each column represents a different regression. The regressions include observations in the Children's sample, aggregated by Mother and Father. Controls include parent's birth state, age in years, birth region and birth region interacted with birth year. Standard errors are clustered using birth year and birth state two-way clustering. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table 8: Effect of Compulsory Schooling on Assortative Matching by Years of Schooling

	Husband's Education		Wife's Education	
CS Years	0.013	0.013	0.015*	0.016*
	(0.008)	(0.008)	(0.008)	(0.008)
Observations	14,222,137	14,222,137	14,222,137	14,222,137
R2	0.183	0.166	0.173	0.163
Own Birth Year	X	X	X	X
Own Birth State	X	X	X	X
Own Birth Region-Year	X	X	X	X
Own Race	X	X	X	X
Spouse Birth Year	X		X	
Spouse Birth State	X		X	
Spouse Birth Region-Year	X		X	
Spouse Race	X		X	

Notes: This table shows the 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. Standard errors are clustered using birth year and birth state two-way clustering. *p<0.1; **p<0.05; ***p<0.01.

5 Mechanisms

5.1 Assortative Matching

In this section, we explore channels through which parental exposure to compulsory schooling plausibly affects children's outcomes. First, in Table 8, we show that individuals who are exposed to more compulsory schooling take a more educated spouse, on average. This is particularly true of men. A one year increase in men's compulsory schooling exposure is associated with marrying a woman with a 0.012 more years of schooling, on average. This effect persist even after controlling for spousal birth year, birth state, race and interactions of birth year and region of birth. We find similar effects for women exposed to more compulsory schooling, but these are not precisely estimated. Overall, these are indications that there is positive assortative matching on education in the marriage market.

5.2 Employment, Living Arrangements and Marriage Decisions

Next, we show that exposure to compulsory schooling affects employment outcomes, living arrangements and marriage decisions, in particular for women. Table 9 shows that while women who were exposed to an additional year of compulsory schooling show weak evidence of lower employment rates, fewer hours per week worked and fewer weeks per year worked, they also exhibit slightly higher weekly wages (0.005 log points) in 1940. Moreover, women who were subject to more compulsory schooling, sort into professions associated with higher average education levels and higher earnings, both in 1930 and in 1940. Table 10 shows that there are no statistically significant effects of compulsory schooling on men's labor market outcomes, although the direction of the estimates is, in all cases, the same as the women's estimates. Compulsory schooling also predicts lower home ownership rates in 1930, perhaps as a result of higher-educated individuals' later labor market entry or their sorting into neighborhoods with more expensive real estate.

In terms of living arrangements, an extra year of compulsory schooling decreases a woman's probability of living in a house she owns by 0.19 percentage points (which is mirrored by a similar estimate for men's home ownership rates), but this gap disappears by 1940. Compulsory schooling has no effect on home values, but women who rent their homes live in dwellings with \$0.39 higher monthly rents in 1940 (the average rent is \$70 for the adults sample) per additional year of compulsory schooling. In 1940, they live in households that have on average 0.008 more people per additional year of schooling, a figure that is similar to men's 0.006 larger households per additional year of schooling.

Lastly, compulsory schooling improves the marriage probability, while delaying marriage: for women and men, respectively, the probability of getting married increases by 0.442 and 0.310 percentage points, while the average marriage age increases by 0.026 and 0.036 years per year of compulsory schooling. These effects are substantial, as they suggest that being exposed to nine years of compulsory schooling can increase the marriage probability by more than 3 percentage points for women, while for men this figures stand at 2.2 percentage points.

Table 9: Compulsory Schooling Exposure vs Other Outcomes (Women)

	CS Years	SE	Observations	R^2
Employment (p.p.)	−0.029	0.072	18 625 586	0.026
Hours Worked (Last Week)	−0.003	0.023	18 625 586	0.000
Weeks Worked (1939)	−0.011	0.029	18 625 586	0.021
Log Weekly Wage	0.005**	0.002	3 762 101	0.241
Occupational Education Score 1930 (0-100)	0.291***	0.091	7 496 325	0.101
Occupational Education Score 1940 (0-100)	0.161***	0.054	6 439 750	0.000
Occupational Earnings Score 1930 (0-100)	0.198***	0.068	7 535 135	0.259
Occupational Earnings Score 1940 (0-100)	0.098**	0.041	6 439 750	0.193
Home Ownership 1930 (p.p.)	−0.187**	0.080	32 079 100	0.083
Home Ownership 1940 (p.p.)	0.049	0.094	23 274 378	0.000
Home Value 1930 (\$)	−2.228	18.944	11 426 505	0.014
Home Value 1940 (\$)	−8.635	11.217	10 708 850	0.013
Monthly Rent 1930 (\$)	−0.057	0.143	11 844 391	0.002
Monthly Rent 1940 (\$)	0.393	0.310	11 830 480	0.000
Household Size 1930	−0.006	0.006	32 779 118	0.031
Household Size 1940	0.008**	0.003	23 737 752	0.020
Ever Married 1930 (p.p.)	0.442***	0.076	29 416 455	0.247
Marriage Age (Years)	0.026**	0.011	19 650 583	0.000

Notes: This table shows the relationship between individual exposure to compulsory years of schooling and a series of individual outcomes in 1930 and 1940. We include all female individuals in the Adults sample. We include birth state fixed effects, birth year fixed effects, region of birth fixed effects and region of birth and year of birth interactions. Standard errors are clustered using birth year and birth state two-way clustering. *p<0.1; **p<0.05; ***p<0.01.

Table 10: Compulsory Schooling Exposure vs Other Outcomes (Men)

	CS Years	SE	Observations	R^2
Employment (p.p.)	−0.062	0.054	18 590 320	0.013
Hours Worked (Last Week)	−0.037	0.042	18 590 320	0.000
Weeks Worked (1939)	−0.047	0.037	18 590 320	0.016
Log Weekly Wage (1939)	0.002	0.002	12 138 203	0.145
Occupational Education Score 1930 (0-100)	0.028	0.041	22 447 566	0.043
Occupational Education Score 1940 (0-100)	0.004	0.031	22 048 027	0.000
Occupational Earnings Score 1930 (0-100)	0.058	0.097	22 547 550	0.131
Occupational Earnings Score 1940 (0-100)	0.059	0.075	22 048 027	0.090
Home Ownership 1930 (p.p.)	−0.188**	0.082	31 375 055	0.082
Home Ownership 1940 (p.p.)	0.030	0.087	22 871 000	0.000
Home Value 1930 ($)	6.013	24.109	10 378 037	0.016
Home Value 1940 ($)	−0.400	9.553	9 866 976	0.017
Monthly Rent 1930 ($)	−0.030	0.121	11 257 885	0.002
Monthly Rent 1940 ($)	0.223	0.214	12 075 532	0.000
Household Size 1930	−0.006	0.005	32 516 332	0.025
Household Size 1940	0.006*	0.003	23 564 030	0.009
Ever Married 1930 (p.p.)	0.310***	0.052	29 128 453	0.374
Marriage Age (Years)	0.036***	0.012	18 159 820	0.000

Notes: This table shows the relationship between individual exposure to compulsory years of schooling and a series of individual outcomes in 1930 and 1940. We include all male individuals in the Adults sample. We include birth state fixed effects, birth year fixed effects, region of birth fixed effects and region of birth and year of birth interactions. Standard errors are clustered using birth year and birth state two-way clustering. *p<0.1; **p<0.05; ***p<0.01.

5.3 Neighborhood Sorting

We explore whether compulsory schooling has an effect on neighborhood sorting. We hypothesize that if compulsory schooling is allowing individuals to increase their level of education, marry more educated spouses and work in professions wit higher earnings, they may sort into more affluent neighborhoods. This decision could have an important impact on children's outcomes, by exposing them to an environment with more opportunities to thrive. We make use of the very fine grained census tracts in the 1920 and 1930 censuses, to first explore how individuals' neighborhood characteristics are impacted by their exposure to compulsory schooling. More specifically, we explore four dimensions of neighborhoods: housing and living arrangements, occupational choices, labor market and human capital indicators and demographics. To do this, we set up the following equation, which relates 1930 individual i's neighborhood of residence n characteristics (Y_{ni}^{1930}) to their exposure to compulsory schooling years (CS_i):

$$Y_{ni}^{1930} = \beta_k CS_i + \left(\kappa_{ci}^{1930} \times \delta_{byi}\right) + \lambda Race_i + \mu Sex_i + \epsilon_i \tag{5}$$

The specification includes fixed effects for county of residence (κ_{ci}) and birth year (δ_{byi}), their interaction and controls for individual i's race and sex.

Table 11 shows that, controlling for county of residence, year of birth and their interaction,

Table 11: Compulsory Schooling Exposure vs Census Tract Characteristics (1930)

	CS Years	SE	Observations	R^2
Home Ownership (p.p.)	0.791***	0.141	58 412 339	0.404
Housing Value ($)	112.990**	52.058	53 606 935	0.034
Monthly Rent ($)	0.851***	0.189	54 992 924	0.051
Household Size	−0.010***	0.002	58 621 947	0.247
Radio in Home (p.p.)	0.934***	0.122	58 621 947	0.675
Occupational Education Score (0-100)	0.309***	0.036	58 605 178	0.224
Occupational Earnings Score (0-100)	0.538***	0.087	58 605 184	0.488
Participation Rate (p.p.)	0.024*	0.013	58 609 352	0.076
Unemployment Rate (p.p.)	−0.186***	0.034	58 599 027	0.235
Proportion 6-18 in School (p.p.)	0.156***	0.016	58 604 612	0.283
Literacy (p.p.)	0.115***	0.014	58 604 612	0.515
Teachers per 100 Students (Aged 6-18)	0.152***	0.028	58 515 433	0.025
Black (p.p.)	−1.806***	0.372	58 621 947	0.537
White (p.p.)	1.802***	0.372	58 621 947	0.037
Native English (p.p.)	−0.101	0.079	58 621 947	0.593
Immigrant (p.p.)	0.150*	0.085	58 621 947	0.743
Average Age (Years)	0.068***	0.008	58 621 947	0.450
Urban (p.p.)	−0.055	0.039	58 621 947	0.488
Farmer (p.p.)	−0.032	0.024	58 621 947	0.455

Notes: This table shows the relationship between individual exposure to compulsory years of schooling and the characteristics of their census tract of residence in 1930. We include all 1930 individuals who are born in 1907 at the latest. We include county of residence and birth year fixed effects and their interaction. Standard errors are clustered using birth year and 1930 county of residence two-way clustering. *p<0.1; **p<0.05; ***p<0.01.

individuals exposed to an additional year of compulsory schooling live in census tracts with 0.8 p.p. higher home ownership rates, $113 higher housing prices, $0.82 higher rents and where households have, on average, 0.01 fewer members and 0.9 p.p. likelier to have a radio in their home. All of these estimates are significant at the 0.01 significance level, with the exception of the housing value estimate, which is significant at the 0.05 level. In terms of occupational choice, people exposed to one additional year of compulsory schooling live in neighborhoods where the average occupational education score is 0.3 higher and the earnings score is 0.5 higher (on a scale from 0 to 100). They inhabit census tracts with a higher (but imprecisely estimated labor force participation rate), a -0.2 p.p. lower unemployment rate and whose inhabitants are 0.1 p.p. more likely to be literate and 0.16 p.p. more likely to be in school if they are between the ages of 6 and 18.

From a demographic standpoint, an additional year of compulsory schooling is associate to living in a neighborhood where the average inhabitant is 1.8 p.p. less likely to be Black, 1.8 p.p. more likely to be White, 0.2 p.p. less likely to be born outside the United States and whose average age is 0.07 years older.

Although the previous results show us that there are systematic differences in neighborhood characteristics predicted by exposure to compulsory schooling in 1930, they only provide a snapshot at one point in time and do not shed any light on when these changes occur. To that end, we set up a similar equation to equation 5, where the independent variable is the difference between respondent i's neighborhood characteristics in 1930 and those in 1920 (i.e. $Y_{ni}^{1930} - Y_{ni}^{1920}$):[16]

$$Y_{ni}^{1930} - Y_{ni}^{1920} = \beta_k CS_i + \left(\kappa_{ci}^{1920} \times \delta_{byi}\right) + \lambda Race_i + \mu Sex_i + \epsilon_i \tag{6}$$

The specification includes fixed effects for county of residence in 1920 ($\kappa_{1920,ci}$) and birth year (δ_{byi}), their interaction and controls for individual i's race and sex. In Table 12, we present the results for those neighborhood outcomes that are available in both the 1920 and the 1930 censuses. We find strong evidence that exposure to compulsory schooling predicts upward geographic (neighborhood) mobility. One additional year of compulsory schooling is causally associated to transitioning, between 1920 and 1930, to a census tract with a 0.1 p.p. higher home ownership rate, and whose inhabitants have occupations with 0.07 and 0.02 higher occupational education and earnings score, respectively. For every year of additional compulsory schooling an individual receives, their 1930 census tracts have 0.07 p.p. lower school leaving for those between 6 and 18 years old and a 0.02 p.p. higher literacy, are 0.27 p.p. less likely to be Black and 0.27 p.p. more likely to be White, 0.07 p.p. less likely to be native English speakers, 0.1 p.p. more likely to be born outside the United States and 0.1 p.p. more likely to live in an urban area.

In the Appendix Table 17, we restrict our attention only to those who moved across states between 1920 and 1930. for these migrants, higher exposure to compulsory schooling predicts changes in neighborhood characteristics even more strongly. In particular, an extra year of compulsory schooling exposure for 1920-1930 migrants across states predicts a 0.27 p.p. increase in neighborhood home ownership, a 0.19 p.p. increase in the population age 6 to 18 enrolled in school, a 0.29 p.p. increase in the fraction of foreign-born individuals and a 0.43 p.p. increase in the fraction of White inhabitants (mirrored by a 0.43 p.p. decrease in the proportion of Black inhabitants).

6 Robustness

6.1 Difference-in-Differences Estimator Decomposition

As previously discussed, the two-way fixed effects (TWFE) specification we estimate in this paper is at the forefront of a recent and growing literature highlighting some potential issues with his approach. For example, Goodman-Bacon (2021) suggests that, with staggered treatment timing and when there are heterogeneous treatment effects across different units, the causal interpretation of the TWFE estimator can be problematic. In addition, this estimator can be shown to be a weighted average of all possible two-group/two-period difference-in-differences (DD) estimators in the data. In addition, 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.

Moreover, our setup features several complications which make the analysis of the TWFE estimator more difficult. Indeed, our data feature staggered implementation of treatments

[16] In the 1920 census, there are no questions regarding rent, home value, radio in home or employment.

Table 12: Compulsory Schooling Exposure vs Census Tract Characteristics (1920 vs 1930)

	CS Years	SE	Observations	R^2
Home Ownership (p.p.)	0.105***	0.029	26 069 113	0.176
Household Size	−0.004***	0.001	26 095 197	0.048
Occupational Education Score (0-100)	0.048***	0.008	26 043 760	0.015
Occupational Earnigs Score (0-100)	0.114***	0.016	26 043 764	0.057
Participation Rate (p.p.)	0.000	0.004	26 095 119	0.182
Proportion 6-18 in School (p.p.)	0.066***	0.012	26 093 105	0.144
Literacy (p.p.)	0.019***	0.003	26 095 197	0.149
Teachers per 100 Students (Aged 6-18)	0.036***	0.005	26 095 197	0.020
Black (p.p.)	−0.268***	0.063	26 095 197	0.042
White (p.p.)	0.267***	0.063	26 095 197	0.001
Native English (p.p.)	−0.065***	0.010	26 095 197	0.091
Immigrant (p.p.)	0.098***	0.013	26 095 197	0.106
Average Age (Years)	0.032***	0.004	26 095 197	0.057
Urban (p.p.)	0.097***	0.024	26 095 197	0.049
Farmer (p.p.)	−0.090***	0.015	26 095 197	0.049

Notes: This table shows the relationship between individual exposure to compulsory years of schooling and the changes in characteristics of their census tract of residence between 1920 and 1930. We include all matched 1920 to 1930 individuals who are born in 1907 at the latest. We include county of residence and birth year fixed effects and their interaction. Standard errors are clustered using birth year and 1930 county of residence two-way clustering. *p<0.1; **p<0.05; ***p<0.01.

(different states pass compulsory schooling laws at different times), treatments with different intensities (laws can mandate different numbers of compulsory years of schooling), varying treatment intensities across time (states pass different compulsory schooling laws at different points in time) and non-permanent treatments (states can scale down compulsory schooling or abandon it altogether it).

To better understand our estimator and how it is identified, we first conduct a Goodman-Bacon decomposition. More specifically, we decompose our TWFE estimator into all possible two-period/two-unit DD estimations, where each units are defined as groups of states that passed their first compulsory schooling laws at the same time. To conduct the Goodman-Bacon decomposition, which is intended for binary-value treatments, we group our data into state-year level cells and code our treatment as an indicator variable that takes the value of 1 for each state-birth cohort exposed to a compulsory schooling law and 0 otherwise. In other words, we estimate the following model, which relates state-birth cohort average years of schooling ($Educ_{sby}$) to whether or not that state-birth cohort (CS_{sy}):

$$Educ_{sby} = \beta_k CS_{sby} + \gamma_s + \delta_{by} + \epsilon_{sby} \tag{7}$$

The specification includes fixed effects for state (γ_{si}), birth year (γ_{si}). Note that the Goodman-Bacon decomposition does not support time-invariant controls, such as region fixed effects. We exclude states where some early birth year cohorts that are sufficiently

Table 13: Goodman-Bacon Decomposition

	Parents		Children-Mothers		Children-Fathers	
Comparison	Weight	Estimate	Weight	Estimate	Weight	Estimate
Earlier vs Later Treated	0.309	0.180	0.258	0.496	0.296	0.465
Later vs Always Treated	0.193	0.846	0.360	0.619	0.189	0.781
Later vs Earlier Treated	0.498	0.113	0.382	0.146	0.514	0.060
Total		0.275		0.406		0.317

Notes: This table shows the Goodman-Bacon decomposition of the TWFE in Equation 7. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

small that no respondent reports non-missing educational information in 1940.[17]

Table 13 shows the Goodman-Bacon decomposition of this particular TWFE estimator. Columns 1 and 2 show a decomposition of the Adult sample DD estimate into three categories: effects of compulsory schooling on early treated states versus later treated states later-treated states versus always treated states and later versus earlier treated states. By far the largest effect is observed in states that pass relative schooling laws relative to the always treated states of New England. A possible explanation for this is that New England states had very high and stable educational achievement to begin with, so the introduction of compulsory schooling laws in other states in the country helped narrow this gap. The early- and late-treated states in our sample tend to be underdeveloped in the mid-nineteenth century, but the educational achievement is rapidly increasing. While compulsory schooling laws help accelerate this process, the measured effect of compulsory schooling when comparing these type of states is not as pronounced as when they are compared to New England states.

Columns 3 to 6 in the table shows a similar decomposition of the DD estimate of the effect of parental exposure to compulsory schooling on the Children's years of schooling. The introduction of new schooling laws helped these later adopters to catch up to early-adopter states. Parental and maternal exposure to new schooling laws is respectively responsible for a 0.6 year and 0.8 year reduction in the educational attainment gap between children born in these states compared to the states that already had schooling laws in place before 1860. Moreover, children of parents exposed to early compulsory schooling laws complete 0.5 years more schooling than those in states with no compulsory schooling. Finally, the decomposition suggests that parental exposure to late-adopted compulsory schooling laws only marginally helped in reducing the education gap between these states and early-adopters, by approximately 0.1 years. However, this could be driven by the binary treatment definition used in the Goodman-Bacon decomposition. We know that early-adopters typically continued to increase the number of compulsory schooling years long after the adoption of the first schooling laws. This would explain why late-adopter states failed to narrow educational gaps even after passing their first schooling laws.[18]

In order to understand which states drive our results, Figure 10 of the Appendix shows event study plots of the average educational attainment in years across state-cohorts around

[17] These are Arizona, Nevada and Wyoming.

[18] A further breakdown of these DD estimators, where we plot each 2-by-2 Goodman-Bacon estimate is provided in the Appendix, in Figures 7, 8 and 9.

the implementation of first schooling laws in different states. By far the most visible effects of the laws are in Southern states, in particular in Texas, Florida, Georgia, Alabama, North Carolina and Louisiana. These states are also the states that implemented their first schooling laws relatively late, in the early twentieth century. Moreover, these are states in which the pre-schooling law levels of educational attainment is low. For example, the average educational attainment in Alabama for cohorts born in the late 1890s is below 7 years of completed schooling, which is which is almost two years lower than the attainment in Massachusetts for cohorts born forty years earlier. This means that compulsory schooling laws in the south were binding for a large proportion of the population, which may explain the large estimated effects. In the next section, we explore this issue in more depth: we move from the TWFE estimator, which gives us average effects of compulsory schooling for the entire population, to an estimator that takes into account pre-schooling laws state-specific educational attainments to estimate an effect on individuals for whom the laws are binding.

6.2 Effects to Exposure to Compulsory Schooling vs Effects to Exposure to Compulsory Schooling Past Desired Education Level

Next, we move from the TWFE estimate, which estimates an average effect of exposure to compulsory schooling laws on entire state-cohorts, to an average effect on the treated children in those cohorts for whom the schooling laws are binding and who are thus compelled to attend school past their desired educational attainment level (henceforth EPD). Conceptually, these children are low-attainment students who would have dropped out of school early in the counterfactual scenario in which no compulsory schooling law was passed. This gets us closer to the true causal impact of schooling laws on the relevant population targeted by the laws.

To illustrate the difference between TWFE and EPD, consider the following example with two states and two periods. Suppose a state mandates two years of compulsory schooling and, in the absence of schooling laws, 80% of children would exceed this level of attainment, 10% of children would not attend school at all and 10% would complete only one year of schooling. The estimated TWFE effect of the law, under perfect enforcement is simply a weighted average of the increase in years of schooling induced by the law, divided by the total number of years of compulsory schooling. Since 10% of children see a one year increase in their attainment and another 10% see a two year increase, the effect is $(0.1 \times 1 + 0.1 \times 2)/2 = 0.15$ years of schooling per compulsory schooling year.[19] Contrast this with a second state, where 25% of students would complete no schooling and another 25% of students would only complete one year. The effect of the law for this state is $(0.25 \times 1 + 0.25 \times 2)/2=0.375$ per year of compulsory schooling. Therefore, the TWFE estimate for the two states is different, despite the fact that the direct effect (EPD) on low-attainment students is, by construction, the same for both states (1 year per compulsory schooling year exposure beyond desired attainment). This is due to the fact that the proportion of students actually affected by the law across the two states is very different.

[19] We assume that the counterfactual educational distribution is the same as the pre-compulsory schooling law one. that the laws have no effects on students who would have attained at least the minimum level of required schooling in absence of the law and the law does not induce low-achieving students to stay on past the mandated years of schooling.

We derive a relationship between the pooled TWFE estimator and EPD estimator. Suppose there are N states with $n_{s,by}$ individuals in state s and birth year-cohort by. Assuming a homogeneous effect EPD to being exposed to one year of compulsory schooling past your desired level of education, a compulsory schooling law mandating $c_{s,by}$ years of schooling will yield a TWFE effect that depends on each state-cohort's s, by distribution of individuals' desired level of attainment in absence of the law. This is defined by $p_{s,by,e}$, which is the fraction of individuals in state-cohort s, by which desires to attain education level e. We estimate:[20]

$$TWFE = \frac{\sum_{s}\sum_{by} \mathbb{1}(c_{s,by} > 0) \times n_{s,by} \times \frac{\sum_{e \leq c_{s,y}} e \times p_{s,by,e} \times EPD}{c_{s,by}}}{\sum_{s}\sum_{by} \mathbb{1}(c_{s,by} > 0) \times n_{s,by}} \tag{8}$$

We can back out an average treatment effect EPD:

$$EPD = TWFE \times \frac{\sum_{s}\sum_{by} \mathbb{1}(c_{s,by} > 0) \times n_{s,by}}{\sum_{s}\sum_{by} \mathbb{1}(c_{s,by} > 0) \times n_{s,by} \times \frac{\sum_{e \leq c_{s,y}} e \times p_{s,by,e} \times EPD}{c_{s,by}}} \tag{9}$$

We interpret EPD as the average effect of exposure to one year of compulsory schooling past desired years of schooling. If a compulsory schooling law is higher than the desired yeas of schooling only for a small fraction of individuals, the TWFE estimate will be small compared to EPD. Table 14 shows the adult TWFE estimates of compulsory schooling exposure on years of schooling from Table 2 and the corresponding EPD estimates. We find that for the entire adults sample, the average response to a year of compulsory schooling past desired attainment is a 0.39 year increase. This effect is larger for women (0.45) than men (0.34) and for urban (0.45) vs rural (0.26) dwellers. It is largest for Blacks (0.74) and for cohorts born after 1890 (0.64), which indicates that the compulsory schooling laws in the South, which took place largely after 1890, were particularly effective.

Table 14: Comparison of Exposure to Compulsory Schooling (TWFE) vs Exposure to Compulsory Schooling Past Desired Years of Schooling (EPD)

	Dependent Variable: Years of Schooling										
	All	Men	Women	Urban	Rural	Black	Post-1890	East	Center	South	West
TWFE	0.014*	0.012	0.016*	0.016**	0.010	0.027**	0.023**	0.034	−0.006	0.031*	0.076***
EPD	0.394	0.340	0.451	0.448	0.263	0.738	0.642	0.937	−0.167	0.840	2.083

Notes: This table shows the effect of exposure to one extra year of compulsory schooling on years of schooling for the entire Adult sample using the TWFE estimator (Equation 1) and the effect of exposure to one extra year of compulsory schooling past desired years of schooling (Equation 9).

[20] We assume that the distribution of educational attainment in a state in the cohorts immediately preceding the passing of each law is the counterfactual educational attainment in that state in absence of the law. This is a conservative estimate, since there is a strong secular trend of increasing education in this period in all states. Furthermore, we assume that the laws have no impact on individuals with counterfactual educational levels above the legally mandated ones.

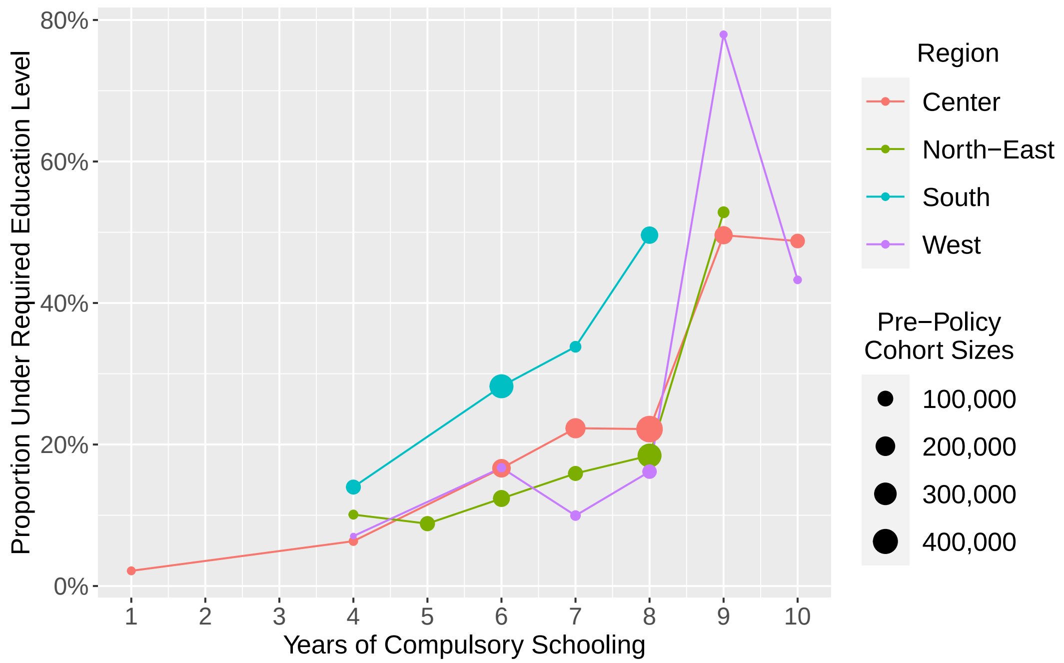

Figure 5: This figure shows what proportion of each state-birth year cohort had lower educational attainment than the legally mandated one before the law was passed, aggregated at the regional level.

Looking at different geographic regions of the US, the relatively small TWFE estimates in the North-East are driven by the fact pre-compulsory schooling law educational attainments are high and the laws themselves are not binding for the vast majority of individuals, as per Figure 5. Therefore, while the TWFE estimated using the entire population is relatively small for the North-East (0.034), EPD is very large (1.06 years of schooling for every binding year of compulsory schooling individuals are subject to) and in fact about twice as large as the effect for Southern individuals (0.50). In the Center region, both the TWFE and the EPD are small, indicating issues with the implementation of school compulsion. Lastly, the TWFE is largest in the West (0.076) and the effects on the treated are extremely large (2.173), possibly signalling that schooling laws can in fact induce individuals to remain in school longer than mandated or that there the laws cause spillovers that induce those not directly compelled by the laws to further increase their educational attainment, perhaps as a way to compete for higher-paid jobs. Similar patterns merge for the estimates of parental exposure to compulsory schooling on children's educational attainment (see Table 14).

Lastly, Figure 6 provides a possible explanation for the relative success of the North-East and West in enforcing the schooling laws. Using the occupations reported in full count censuses from 1850 to 1940, we show that the ratio of teachers to students across different states, around the timing of the first schooling laws. While the South and Center see increases in the number of teachers per enrolled students after the first schooling laws are implemented, signalling an expansion in teacher hiring that more than meets the increase in enrolment, the teacher-student ratios in these areas generally remain below those in the West and North-East. Thus, these latter regions were better positioned to accommodate an influx of students

	Dependent Variable: Child's Years of Schooling										
	All	Men	Women	Urban	Rural	Black	Post-1890	East	Center	South	West
TWFE Mom	0.016***	0.015***	0.018***	0.013***	0.015***	0.011	0.020***	0.021**	0.003	0.024***	0.042***
EPD Mom	0.323	0.295	0.348	0.214	0.301	0.403	0.485	0.562	0.079	0.633	1.147
TWFE Dad	0.010**	0.009**	0.012***	0.011***	0.003	0.019	0.019***	0.023***	0.000	0.032***	0.047***
EPD Dad	0.272	0.240	0.297	0.240	0.103	0.536	0.505	0.623	0.011	0.866	1.258

Notes: This table shows the effect of exposure to one extra year of parental compulsory schooling exposure on years of schooling for the Chilren's sample using a TWFE estimator (Equation 1) and the effect of exposure to one extra year of compulsory schooling past desired years of schooling (Equation 9).

stemming from compulsory schooling laws.

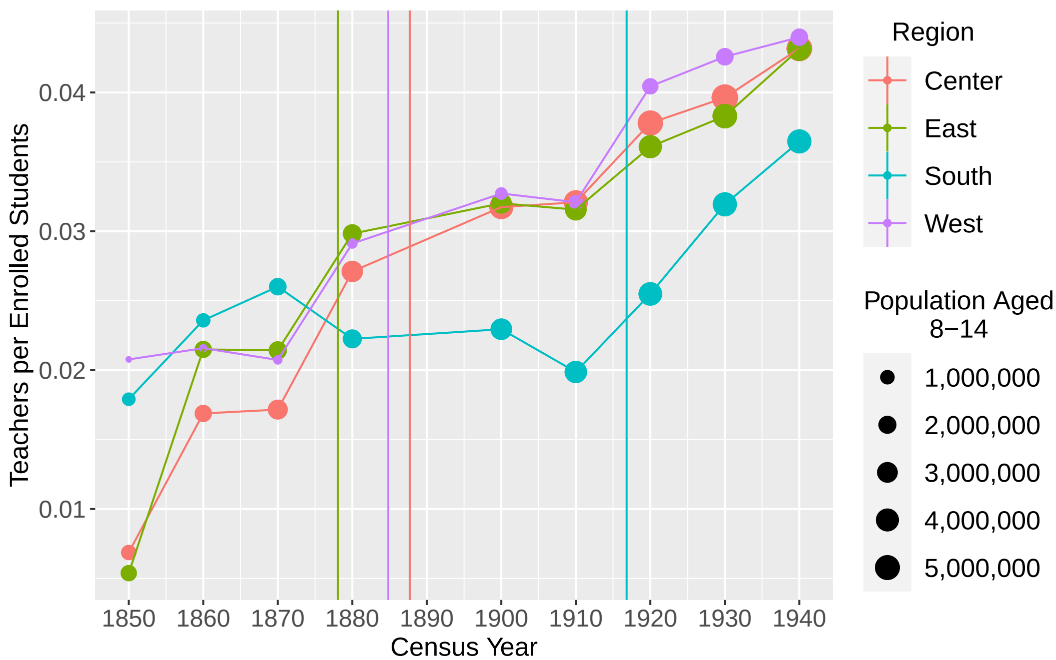

Figure 6: This figure shows the proportion of individuals reporting teaching as their profession relative to those reporting being enrolled in school in each state, in the US censuses of 1850 to 1940. The vertical lines represent the enactment of average implementation year of six years compulsory schooling

7 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 1920, 1930 and 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. Lastly, the large sample sizes allow us to to use a number of control variables and a level of standard error clustering that other studies were not able to include due to statistical power limitations.

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.

We find that exposure to compulsory schooling had an effect on several parental outcomes that could explain intergenerational transmission. Compulsory schooling enabled individuals to marry more educated spouses, delay marriage work in high earning and higher education occupations and sort into neighborhoods with higher employment rates, housing values and rents, lower school dropout rates and whose inhabitants are likelier to be white and born outside the Unites States.

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.

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

	Black et al.	All	Men	Women	Urban	Rural	Black	Post-1890
CS Years Mom	0.210***	0.028***	0.027***	0.029***	0.022***	0.022***	0.048***	0.025**
	(0.001)	(0.007)	(0.007)	(0.007)	(0.006)	(0.008)	(0.016)	(0.011)
Observations	5,749,267	5,749,267	3,329,889	2,419,378	2,754,333	2,994,934	408,493	3,583,198
R^2	0.066	0.144	0.142	0.145	0.089	0.191	0.118	0.155
F-stat	239,447.918	554.554	305.633	242.131	174.701	194.479	41.615	135.167
Years of Schooling (Dad)	0.802***	1.025***	1.177**	0.805***	0.754***	-1.110	0.559***	1.450***
	(0.003)	(0.300)	(0.481)	(0.026)	(0.100)	(2.736)	(0.158)	(0.470)
Observations	5,749,267	5,749,267	3,329,889	2,419,378	2,754,333	2,994,934	408,493	2,544,870
R^2	0.073	0.153	0.149	0.156	0.090	0.192	0.111	0.172
F-stat	275,725.983	124.907	59.827	61.943	130.611	5.509	29.390	25.832
Race		X	X	X	X	X	X	X
Birth State		X	X	X	X	X	X	X
Birth Year	X	X	X	X	X	X	X	X
Birth State-Year		X	X	X	X	X	X	X
Parent Birth State		X	X	X	X	X	X	X
Parent Birth Region		X	X	X	X	X	X	X
Parent Birth Year	X	X	X	X	X	X	X	X
Parent Birth Region-Year		X	X	X	X	X	X	X
County of Residence	X							

Notes: This table shows the first stage describing the effect of parental exposure to compulsory schooling on parental years of schooling. Each column represents a different regression. Standard errors are clustered using birth year and birth state two-way clustering, except for the first column, which uses county-by-parent birth year clustering. *p<0.1; **p<0.05; ***p<0.01.

Table 16: Effect of Parental Exposure to Compulsory Schooling on Years of Schooling (with Single Parents)

	Dependent Variable: Years of Schooling						
	All	Men	Women	Urban	Rural	Black	Post-1890
CS Years Mom	0.012***	0.011**	0.013***	0.008*	0.011***	0.015*	0.018***
	(0.004)	(0.004)	(0.003)	(0.004)	(0.003)	(0.008)	(0.005)
Female	0.735***			0.352***	0.945***	1.266***	0.709***
	(0.062)			(0.042)	(0.046)	(0.045)	(0.065)
Observations	12,591,618	8,200,944	4,390,674	6383452	208,166	1,015,371	5,785,597
R^2	0.175	0.173	0.159	0.116	0.218	0.160	0.204
Birth State-Year	X	X	X	X	X	X	X
Mother Birth State	X	X	X	X	X	X	X
Mother Birth Region-Year	X	X	X	X	X	X	X
Race	X	X	X	X	X	X	X
CS Years Dad	0.010***	0.009**	0.011***	0.009***	0.004	0.020**	0.019***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.010)	(0.004)
Female	0.715***			0.356***	0.916***	1.277***	0.659***
	(0.057)			(0.040)	(0.048)	(0.047)	(0.057)
Observations	10,310,951	6,899,124	3,411,827	4,993,959	5,316,992	700,904	3,441,762
R^2	0.166	0.163	0.146	0.099	0.205	0.163	0.197
Birth State-Year	X	X	X	X	X	X	X
Father Birth State	X	X	X	X	X	X	X
Father Birth Region-Year	X	X	X	X	X	X	X
Race	X	X	X	X	X	X	X

Notes: This table shows the effect of parental exposure to different compulsory schooling laws on children's years of schooling using an expanded version of the Children's sample, where we include children with at least one-identified parent from the Adult sample (rather than both parents). Each column represents a different regression. Standard errors are clustered using birth year and birth state two-way clustering. *p<0.1; **p<0.05; ***p<0.01.

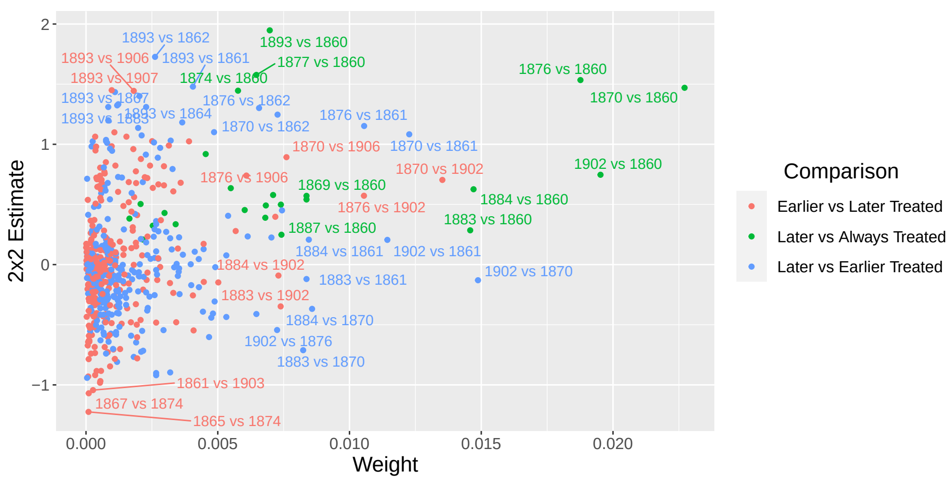

Figure 7: This figure shows the two-by-two simple DD comparisons of the effects of compulsory schooling laws on years of schooling for exposed adults. Each data point is a DD comparison between two groups, where each group is defined as a group of states sharing identical timing of their first compulsory schooling law. Each point is labelled according to the first birth year cohort exposed to compulsory schooling.

Table 17: Compulsory Schooling Exposure vs Census Tract Characteristics Changes (1920-1930): Across-State Movers

	CS Years	SE	Observations	R^2
Home Ownership (p.p.)	0.273***	0.037	2 358 587	0.231
Household Size	−0.013***	0.001	2 363 285	0.101
Occupational Education Score (0-100)	0.073***	0.007	2 359 397	0.041
Occupational Earnings Score (0-100)	0.239***	0.021	2 359 397	0.218
Participation Rate (p.p.)	−0.011	0.007	2 363 273	0.109
Proportion 6-18 in School (p.p.)	0.189***	0.020	2 362 795	0.161
Literacy (p.p.)	0.080***	0.007	2 363 285	0.315
Black (p.p.)	−0.429***	0.033	2 363 285	0.158
White (p.p.)	0.429***	0.033	2 363 285	0.003
Native English (p.p.)	−0.193***	0.012	2 363 285	0.199
Immigrant (p.p.)	0.290***	0.014	2 363 285	0.243
Average Age (Years)	0.092***	0.006	2 363 285	0.149
Urban (p.p.)	0.463***	0.054	2 363 285	0.204
Farmer (p.p.)	−0.318***	0.037	2 363 285	0.222

Notes: This table shows the relationship between individual exposure to compulsory years of schooling and the changes in characteristics of their census tract of residence between 1920 and 1930, for those living in different states in 1920 and 1930. We include all matched 1920 to 1930 individuals who are born in 1907 at the latest and live in different states in the two census years. We include county of residence in 1920 and birth year fixed effects and their interaction. Standard errors are clustered using birth year and 1920 county of residence two-way clustering. *p<0.1; **p<0.05; ***p<0.01.

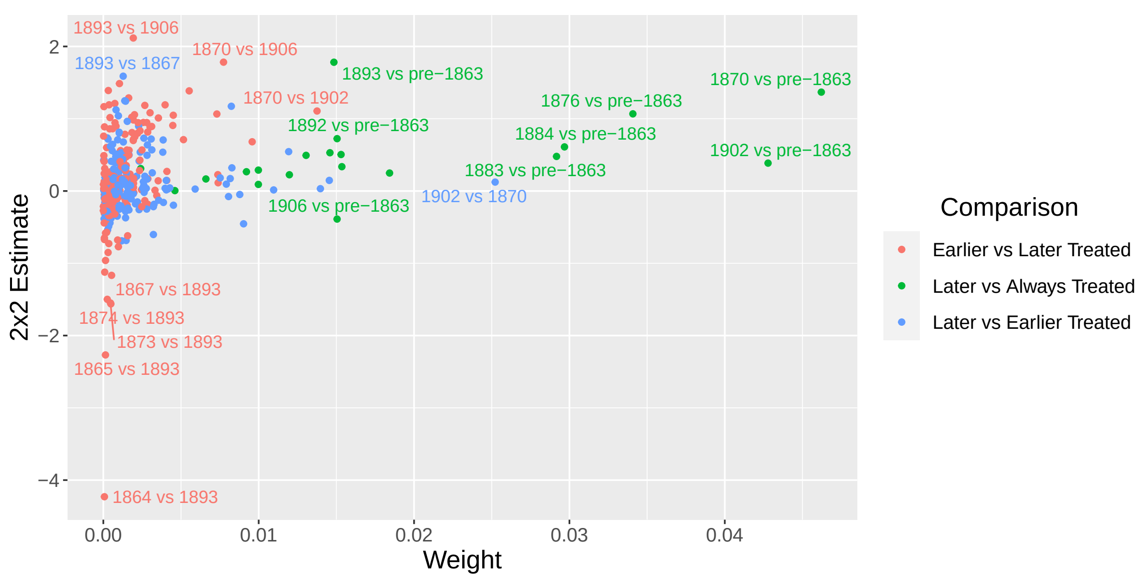

Figure 8: This figure shows the two-by-two simple DD comparisons of the effects of compulsory schooling laws on years of schooling for children whose mothers were exposed to compulsory schooling. Each data point is a DD comparison between two groups, where each group is defined as a group of states sharing identical timing of their first compulsory schooling law. Each point is labelled according to the first birth year cohort exposed to compulsory schooling.

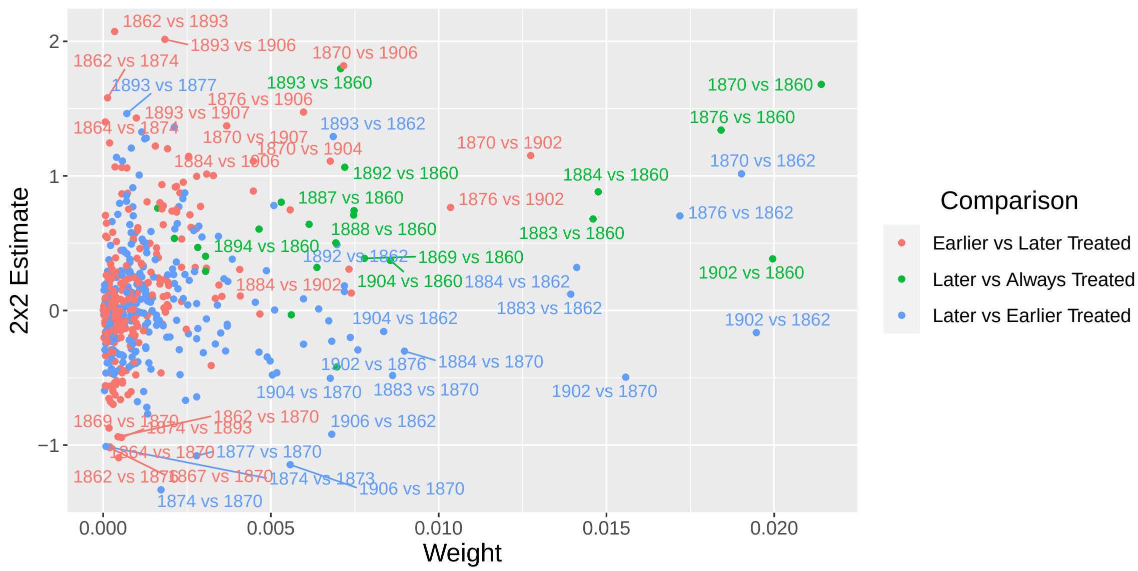

Figure 9: This figure shows the two-by-two simple DD comparisons of the effects of compulsory schooling laws on years of schooling for children whose fathers were exposed to compulsory schooling. Each data point is a DD comparison between two groups, where each group is defined as a group of states sharing identical timing of their first compulsory schooling law. Each point is labelled according to the first birth year cohort exposed to compulsory schooling.

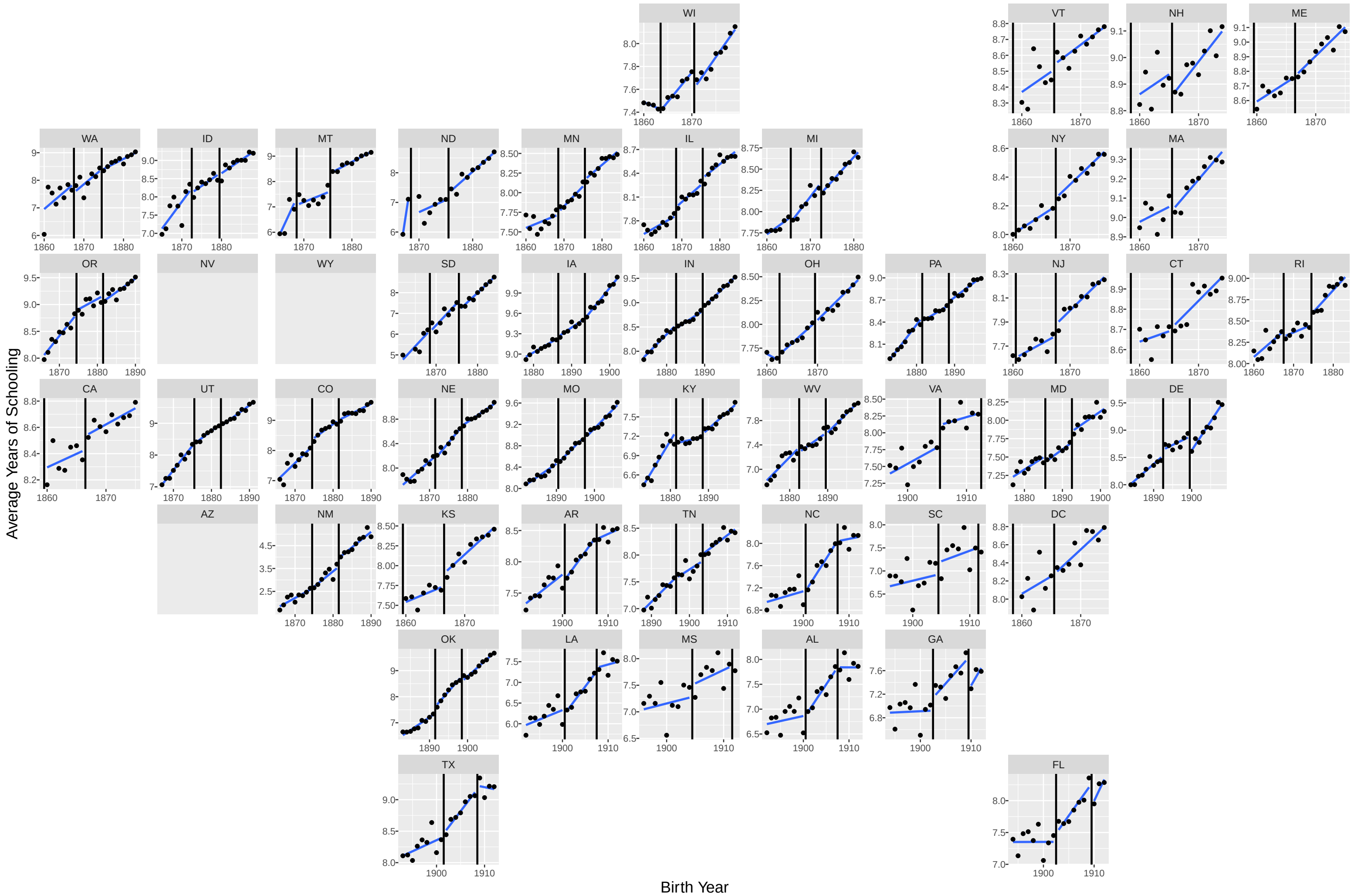

Figure 10: This figure shows the effect of the introduction of six years of compulsory schooling on the average years of schooling by state and birth year, using the 1940 census. The cohorts between the two vertical lines in the graph represent the transitional cohorts: the first vertical line marks the first cohort exposed to one year of schooling in that state and the second vertical line the first cohort exposed to six years of schooling. Nevada, Wyoming and Arizona are excluded due to lacking sufficient observations.

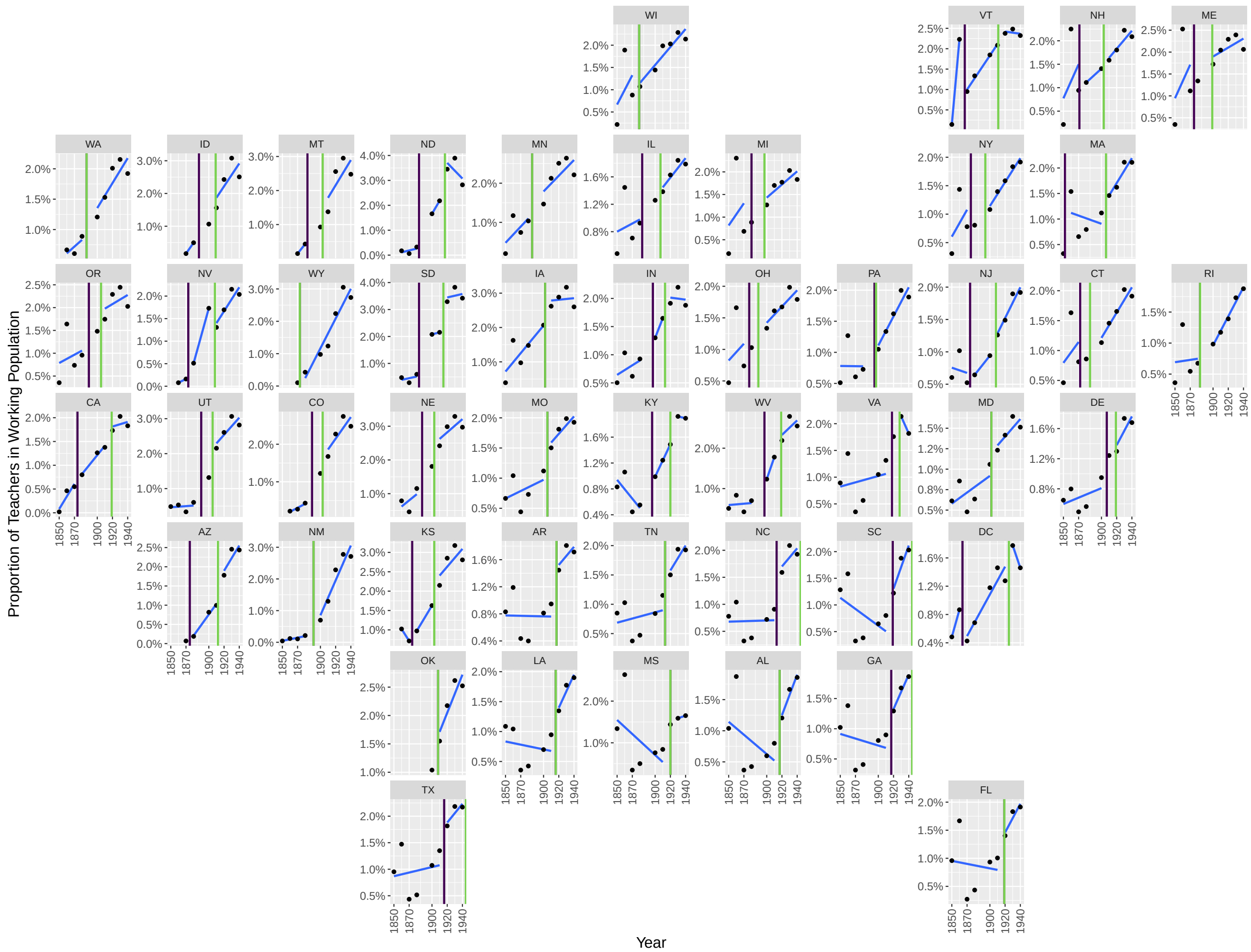

Figure 11: This figure shows the proportion of individuals reporting teaching as their profession relative to all workers in each state in the US censuses of 1850 to 1940. The vertical dark bar represents the enactment of the fist compulsory schooling law. The light green bar represents the enactment of the first eight-year compulsory schooling law.

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 1860-1940 US full count census[21]
- 1860-1870 to 1930-1940 US Census cross-walk[22]

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).

We extend this work by exploring state archives for compulsory schooling laws between 1850 and 1880 "`state_age_limits_1850_1879.dta`" and then use them to create birth-year by state exposure to compulsory schooling data for individuals born between 1845 and 1874. Together with the original data in Clay, Lingwall and Stephens Jr (2021), this means that we have compulsory schooling law exposure for all US cohort born between 1845 and 1930.

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

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

B.3 Replication

Once all census data is downloaded and the compulsory schooling data is obtained, run preliminary codes found under `~/CODE/00 Clean and merge/`, which read and create some helper files and samples:

1. `01_read.R`: read the full count census and linkage .zip files and create corresponding .csv files:

2. `02_merge_1860_1940.R`: links census records

Open the `main.R` file located in `~/CODE/Figures and Tables/` and follow these steps:

1. Change the variable 'wd' to your working directory path.

2. Run the preamble of the `main.R` file, which sets some global variables and functions needed to run the codes.

3. Select the table or figure you want to replicate, commenting out the rest of the `main.R` file. For example, if you wish to replicate Table 2, simply leave the two lines referring to Table 2 uncommented and run them. This should create a file called `Table_02.html` with the code, table and latex table, under `~/CODE/Figures and Tables/Table 02 Parent Years/`. Note that there are no dependencies between the replication files, so each table figure can be replicated individually without relying on any intermediate results from other files.

B.4 Samples Used

In this paper we use two main samples:

- the Adults sample: This sample consists of 1940 census adults who are directly affected by the compulsory schooling laws and are old enough to have children over 18 in 1940. Thus, they must be born between 1880 and 1907 in one of the 48 continental U.S. states and D.C. The 1940 census contains 131,903,910 observations. 44,802,767 individuals are born between 1880 and 1907 (inclusively). 37,291,513 of them are born in continental U.S. states or D.C. and 36,381,675 have non-missing educational attainment. information.

- the second generation sample: This sample consists of 1940 census individuals 18 or older and born in one of the 48 continental U.S. states and D.C., with both linked parents who are in the Adults sample. The parents can be linked in either the 1940 census or any other census via crosswalks. We additionally exclude individuals with at least one parent who:
  - is linked to them both in the 1930 and 1940 census AND
  - has inconsistent birth states across the 1930 and 1940 censuses OR
  - has inconsistent birth years across the 1930 and 1940 censuses (we allow a difference in birth years of at most 2 years between the two censuses and use the 1930 birth year when these are different)

The 1940 census contains 131,903,910 observations. 79,930,943 individuals are at least 18 years old. 78,026,726 of them have non-missing educational attainment. Of these, 11,231,856 are linked to a U.S.-born mother either in the 1940 or the 1930 census, with non-conflicting birth years and states. 10,483,910 of these have mothers born between 1845 and 1912. 6,402,215 of these respondents linked to their mothers are also linked to a U.S.-born father. 6,107,891 are linked to a father that is born between 1845 and 1912.

For some results, we use refined versions of the two main samples:

- the couples sample: this sample consists of all first generation sample individuals who are married (MARST=1) and who live in the same dwelling as another first generation individual (SPLOC$\neq$0). This sample is used for assortative mating results and covers 39,224,106 individuals from the 55,667,919 eligible adult sample described above.

- the labor force sample: this sample consists of first generation sample individuals who are born between 1875 and 1912 (and are thus at most 65 in 1940). This sample is used or labor outcome results.

Lastly, we use two enumeration district samples to study neighborhood-level sorting:

- the 1930 enumeration district sample: Consists of all 1930 census data aggregated into cells. Each cell contains average characteristics all individuals in a given cell. Cells are defined at the enumeration district x age bin level.

- the 1920-1930 enumeration district sample: Consists of all linked 1920-1930 census data aggregated into cells. Each cell contains average characteristics all individuals in a given cell. Cells are defined at the 1920 enumeration district x 1930 enumeration district x age bin level.

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