Chaire de recherche
sur les enjeux économiques
intergénérationnels

24-02

Intergenerational Persistence in the Effects of Compulsory Schooling in the U.S.

Titus Galama, Andrei Munteanu and Kevin Thom

Cahier de recherche
Working paper
Janvier / January 2024

ESG UQÀM

Chaire de recherche
sur les enjeux économiques intergénérationnels

est une chaire qui s'appuie sur un partenariat avec les organisations suivantes :

CIRANO
Allier savoir et décision
Centre interuniversitaire de recherche en analyse des organisations

Retraite Québec

Les opinions et analyses contenues dans les cahiers de recherche de la Chaire ne peuvent en aucun cas être attribuées aux partenaires ni à la Chaire elle-même et elles n'engagent que leurs auteurs.

Opinions and analyses contained in the Chair's working papers cannot be attributed to the Chair or its partners and are the sole responsibility of the authors.

Dépôt légal : Bibliothèque et Archives nationales du Québec et Bibliothèque et Archives Canada, 2024. ISSN 2563-0008

Intergenerational Persistence in the Effects of Compulsory Schooling in the U.S.[*]

Titus Galama[†]
Andrei Munteanu[‡]
Kevin Thom[§]

January 29, 2024

Abstract

Using linked records from the 1880 to 1940 full-count United States decennial censuses, we estimate the effects of parental exposure to compulsory schooling (CS) laws on the human capital outcomes of children, exploiting the staggered roll-out of state CS laws in the late nineteenth and early twentieth centuries. CS reforms not only increased the educational attainment of exposed individuals, but also that of their children. We find that one extra year of maternal (paternal) exposure to CS increased children's educational attainment by 0.015 (0.016) years - larger than the average effects on the parents themselves, and larger than the few existing intergenerational estimates from studies of more recent reforms. We find particularly large effects on black families and first-born sons. Exploring mechanisms, we find suggestive evidence that higher parental exposure to CS affected children's outcomes through higher own human capital, marriage to more educated spouses, and a higher propensity to reside in neighborhoods with greater school resources (teacher-to-student ratios) and with higher average educational attainment.

Keywords: education, inequalities, compulsory schooling, human capital, intergenerational transmission, geographic sorting, assortative mating, racial inequalities
JEL Codes: E24, H52, I24, I25, I26, J15

[*]Acknowledgements: Research reported in this publication was supported by the National Institute On Aging of the National Institutes of Health (RF1055654, R56AG058726, R01AG078522, and R01AG079554) and the Dutch National Science Foundation (016.VIDI.185.044).

[†]University of Southern California's (USC) Center for Economic and Social Research (CESR), USA; USC Department of Economics, USA; Vrije Universiteit Amsterdam, Department of Economics, The Netherlands, and Tinbergen Institute, The Netherlands.

[‡]Department of Economics, Université du Québec à Montréal (UQÀM).

[§]Department of Economics, University of Wisconsin-Milwaukee.

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

Horace Mann (1849)

1 Introduction

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

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

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

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

Our first contribution is to estimate the intergenerational effects of CS reforms. We find substantial effects of parental exposure to CS on offspring's education that are larger than the effects on the parents themselves. Women (men) directly exposed to one additional year of CS experienced a 0.008 (0.005) year increase in years of schooling. By contrast, one extra year of maternal (paternal) exposure to CS increased children's educational attainment by 0.015 (0.016) years (or about double the intergenerational effect).

These seemingly small estimates are, in fact, economically substantial. Parental exposure to CS reforms substantially increased the probability of their children entering the next schooling level (grade school, middle school, high school, and college) and completing it (with effects ranging between 4 and 24 p.p) over the full range from grade school to college, despite these laws binding only for a small portion of the population (12.5%/12.8% for women/men). This is corroborated by subsequent analyses in which we decompose our two-way fixed effects analyses following Goodman-Bacon (2021). These analyses suggest exposure to CS increased parental and children's educational attainment by at least 0.06 to 0.07 years of schooling per year of CS exposure (averaged over all groups).

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

Our second contribution consists in documenting heterogeneity in the intergenerational effects of CS. Schooling laws were more successful for more recent cohorts, in particular for cohorts born after 1900 when enforcement of CS laws improved, and in raising the educational attainment of the offspring of Black Americans. We attribute this to the significantly lower educational attainment of Black Americans, particularly in the South, which resulted in CS becoming binding for approximately half of the Black population. CS laws were particularly effective at increasing educational attainment in the Western United States, and again especially for the offspring of Black Americans. By contrast, the Eastern regions of the US saw very small educational gains from exposure to CS. This may be due to the already high levels of educational attainment, with CS laws dating back as far as the early-to-mid nineteenth century. Last, we find evidence for gender and birth-order effects in the intergenerational transmission of human capital. Parental exposure to CS disproportionately benefited first-born male children.

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

Parental exposure to CS could also affect child outcomes through partner choice. Indeed, we find that women exposed to more CS married more educated men, who were more likely to be employed and had higher earnings. On the other hand, men exposed to more CS married more educated women who were less likely to participate in the labor market, but when they did had substantially higher earnings. This suggests a time versus money trade-off, where increased family resources may have allowed women to stay home (time devoted to home production) unless they were particularly high earners (in which case labor-force participation was more attractive).

The fine level of geographic detail in the censuses allows us to study geographic sorting. We use linkages between the 1910 to 1940 censuses to measure parents' neighborhood sorting behaviors when their children were of school age (ages 5 to 14). Parents with more exposure to CS gravitated towards neighborhoods with higher teacher-student ratios (i.e. school resources), more literate neighbors, and higher rates of primary and secondary school enrollment. Importantly, this phenomenon is not driven by pre-existing conditions in the parents' initial neighborhoods: parents exposed to more CS indeed relocated to neighborhoods with better-resourced schools, higher literacy, and higher school enrollment rates by the time they had children.

We quantify the relative importance of each of these channels using a decomposition method (Gelbach, 2016). We find that the effect of paternal exposure to CS on the child's years of schooling is accounted for by the father's education (22%), his spouse's education (49%), home ownership and home value (7%), and sorting into a neighborhood with more school resources and higher human capital (9%). The effect of maternal exposure to CS is accounted for by maternal education (33%), her spouse's education (25%), and neighborhood school resources (10%). These findings are consistent with the importance of maternal education in the intergenerational transmission of human capital (Currie and Moretti 2003, Black, Devereux and Salvanes 2005).

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

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

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

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

Our main results, and the analyses of mechanisms in particular, also contribute to the larger discussion on whether the degree of intergenerational mobility in the U.S. today has changed versus the past and whether it is different from that in other contexts (Long and Ferrie, 2007, 2013). For example, Ferrie (2005) concludes that the US was occupationally and geographically more mobile than Britain in the mid-nineteenth century, but that this mobility advantage declined in the early part of the twentieth century. Long and Ferrie (2013) suggest that residential mobility offers a compelling explanation for this, since cross-county mobility in the U.S. during the late nineteenth century was substantially greater than comparable mobility in the U.K., or in the later twentieth-century U.S. context. The expansion of compulsory schooling in this era may have played a role in this process.

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

The rest of the paper is structured as follows. Section 2 describes details of the 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 CS laws. Several distinct laws operated together to influence the schooling required for a particular birth-year cohort y in state s. Using the taxonomy of Lleras-Muney (2002), these included laws on the oldest age at which a child could start schooling (Entrance Age) and the youngest age at which a child could end schooling (Dropout Age). Some laws provided a school leaving exemption, allowing children to drop out of school before the Dropout Age, as long as they completed sufficiently many years of schooling.

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

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

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

1. Is the child's age between the maximum Entrance Age and the minimum Dropout Age?
2. If so, does an exemption to the Dropout Age apply? For example:
    - was the child already required to attend school for a sufficient number of years such that it could qualify for an early Dropout exemption?
    - is the child's age equal to, or greater than, the age at which a work permit could be obtained (Work Age exemption)? If so, has the child been required to attend school for a sufficient number of years such that it would satisfy the Work Age exemption?
3. If a Work Age exemption applies, is the child's age less than the Dropout Age? If so, has the child completed sufficient schooling to be exempt from continuation school if such an exemption exists?

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

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

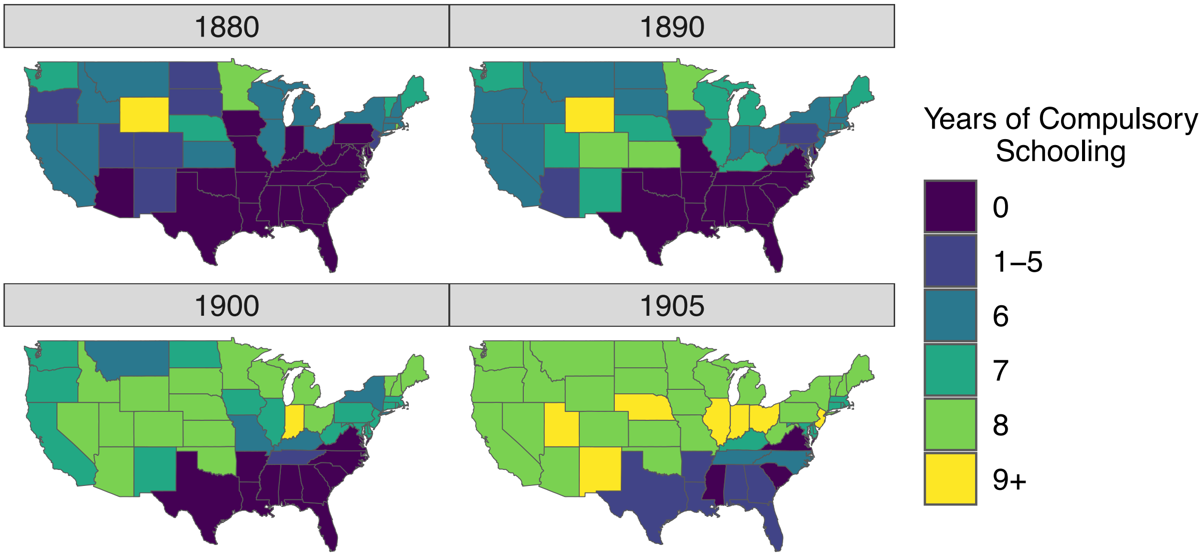

Figure 1: Compulsory schooling law exposure by state, for the 1880, 1890, 1900, and 1905 birth-year cohorts.

The answers to these questions determine the number of years the individuals in our data were legally required to stay in school. For instance, suppose that a certain state s implemented legislation in the year 1900 stipulating that children aged between 8 and 14 must attend school. Suppose further that in 1915, an amendment was made to this law, extending the age range to include all children between 8 and 18, with a provision for exempting those who are over 16 and have a job. According to these laws, a child born in this state in 1900 would be legally obliged to attend school from 1908 to 1914, amounting to a CS period of 7 years. On the other hand, consider a child born in 1905, who was still attending school at the age of 10 when the new schooling law came into effect. This child would be obliged to attend school from 1908 to 1918. However, if they were able to acquire a work permit, they would be permitted to leave school in 1916. In this case, we conservatively consider the cohort born in 1905 to be exposed to 9 years of CS. This accounts for 11 years of CS initially but is reduced to 9 years due to the work permit exemption (since we do not know for whom this exemption applied we apply it conservatively to everyone).

Figure 1 shows the geographic distribution of the roll-out of CS laws in the United States, based on the previously-described coding of CS laws. By the time the cohorts born in 1880 reached the school-entry age, states in New England, around the Great Lakes, and in the Western United States, already had some form of CS law enacted. The New England states in particular were early adopters of CS laws, with Massachusetts enacting the first such law in 1647. This law, called the Old Deluder Satan Act, was meant to provide basic literacy to everyone, as the early Puritan settlers put great value on each individual being able to read and interpret the Bible for themselves. Cohorts born in 1890 in the New England states generally had to complete at least 6 years of CS and, in some cases, up to 9 years. By the time the cohorts born in 1900 were in school, only Southern states did not yet require children to attend any CS. The last cohort in our sample (see section 2.2), born in 1905, was subject to some form of CS in all states except Louisiana, South Carolina, and Virginia. By this time, the norm in most states was 8 years of CS.

2.2 Full Count Census Data

Table 1: Summary Statistics

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

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

Our key question of interest is whether changes in CS laws had *intergenerational* effects on completed education, as this gets to the heart of whether each subsequent generation does better and whether human-capital based economic growth operated during a period of rapid industrialization and consecutive reforms aimed at raising education levels of the population. To this end, we use linked census data from 1880 to 1940, allowing us to track individuals affected by the introduction of CS laws in the late 1800s and early 1900s, link them to their children, and observe how parental exposure to CS laws affected outcomes of their children.[3]

The 1940 census is the most recent full-count census available at the time of writing and the first one to ask questions on educational attainment. We focus on individuals aged over 18 in 1940 and use 1880 to 1940 census linkages constructed by Ruggles et al. (2019) to identify individuals across census waves. Measuring parental exposure to CS requires data on the birth year and birth state of the parents of the "children" in the 1940 Census. For

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

the vast majority of individuals, this information can only be ascertained by making use of cross-walks that link respondents across consecutive censuses (for example, between 1940 and 1930), as most of the "children", when they are adults, no longer co-reside with their parents. Since parent-children links between respondents can only be identified if the respondents are part of the same household, we identify the parents of 1940 "children" in at least one of the 1940 to 1880 censuses, using the moment in time when they were still co-residing. Survey items from the censuses then allow us to determine the year of birth and state of birth of the parents of the 1940 respondents that we can link in this way. This, in turn, enables us to determine parental exposure to CS, using the compulsory schooling law dataset described in the previous section 2.1.

The linked census data offer several advantages in studying the intergenerational effects of CS laws. First, the very large sample sizes help to increase the precision of estimated effects beyond what might be offered in survey data. Second, the census data are very rich. We explore a multitude of outcomes, from years of schooling to marriage and family structure, occupational, employment, and other labor-market outcomes, to name a few. Third, because we can track individuals across time, we can observe changes in their outcomes across census waves. In particular, we explore geographic mobility across enumeration districts 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.

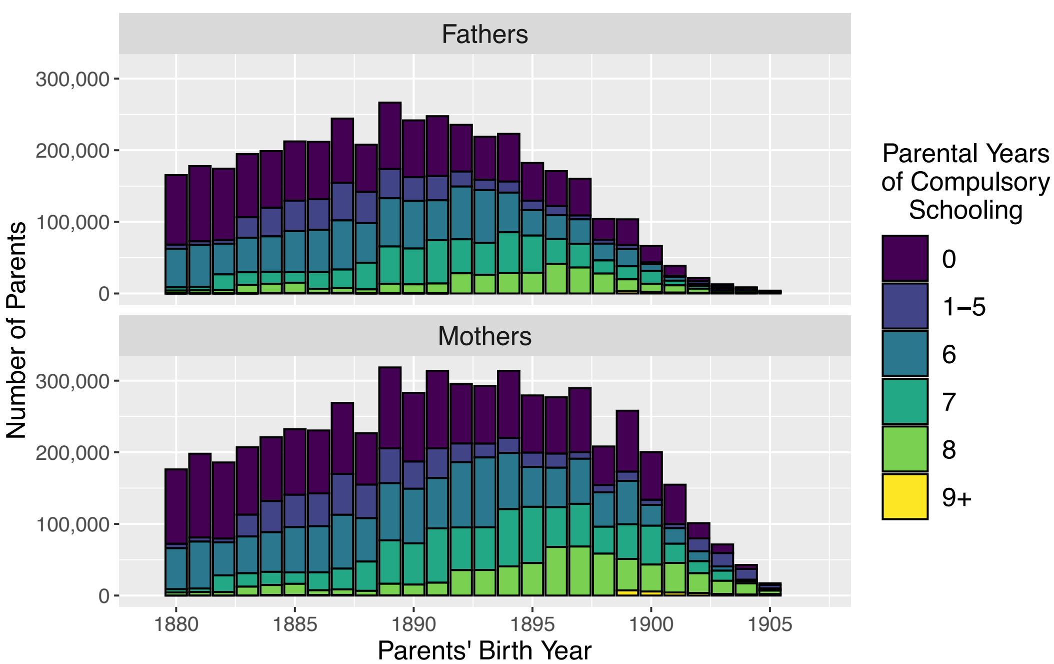

Figure 2: US-born parents by birth year and own exposure to compulsory schooling in the *Parent* sample.

We build two main samples of interest:

1. *Children* sample: contains all 9,382,509 individuals in the 1940 Census born in one of the 48 continental states, or D.C., who are at least 18 years old and who have at least

one identified parent in the *Parents* sample below.

2. *Parents* sample: contains all 9,756,597 parents of the individuals in the *Children* sample who were born between 1880 and 1905 in one of the 48 continental states, or D.C., and were aged at least 16 when their child was born.

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

Of particular note is the average education level of the children (10.2 years of schooling), which is significantly higher than that of parents (8.1 years). This highlights how this era was defined by rapid increases in educational attainment across generations. Females are under-represented in our *Children* sample (45%). This may arise because of difficulties in matching women across censuses when their last names change as a result of marriage. Women are slightly over-represented in our *Parents* sample, and there may be several reasons for this: mothers are on average younger than fathers, they have a higher life expectancy, and are more likely to live with their children in case of separation. For these reasons, it is easier to link mothers to their children.

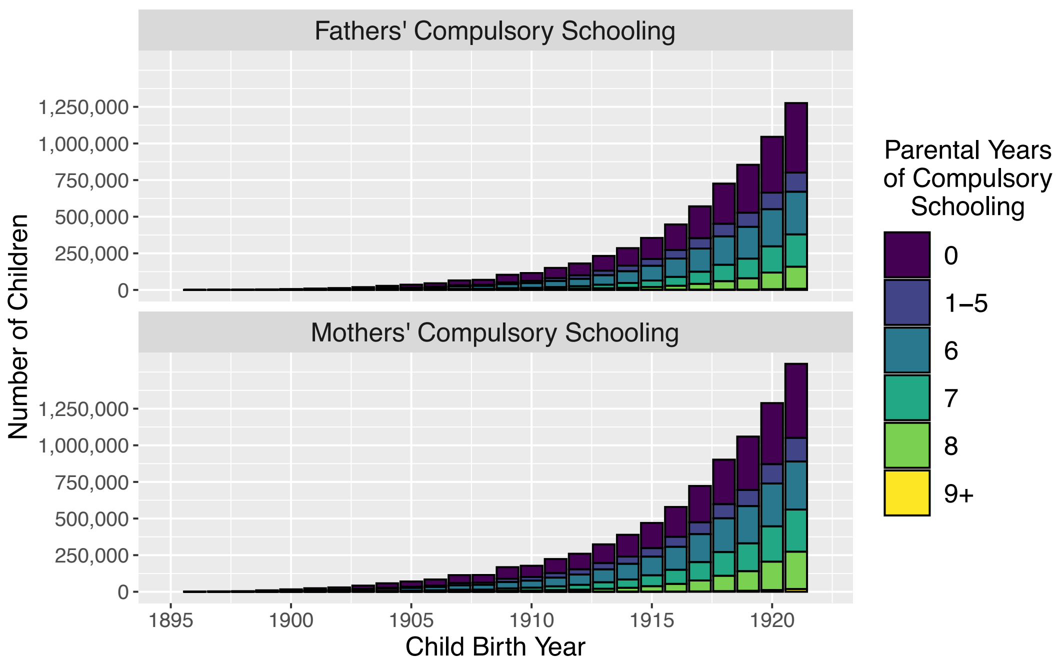

Figure 3: Children's exposure to parental years of compulsory schooling by child birth year.

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

1940 census. The exposure of these parents to CS varies significantly, both within and across cohorts.

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

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

3 Empirical Strategy

3.1 Difference-in-Differences

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

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

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

Our main focus is on the intergenerational effects of exposure to CS laws. Therefore, the second estimating equation relates the child's (c) years of schooling ($Educ_i^c$) to the CS exposure ($CS^p_{s'y'}$) of the child's parents:

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

where, analogous to Equation 1, we include vectors of fixed effects for the child's state of birth s (γ^c_s) and birth year y (δ^c_y), and interactions ($\eta^c_s \times \theta^c_y$) between the child's state of birth (η^c_s) and birth year (δ^c_y), as well as controls for the child's race (λ^c) and sex (μ^c).

Unlike Equation 1, we control for children's birth state and birth year trends, as opposed to trends by region r. These controls capture state birth year effects, such as children's own

[4] East, South, Center and West.

exposure to CS. We are able to introduce these controls because children's birth states and birth years are not co-linear with parental exposure to CS.

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

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

These analyses use the *Children* sample and estimate effects separately for mothers (m) and fathers (f). The effect of parental exposure to CS 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 CS - which varies at the parental state of birth s' and parental year of birth y' level - varies.

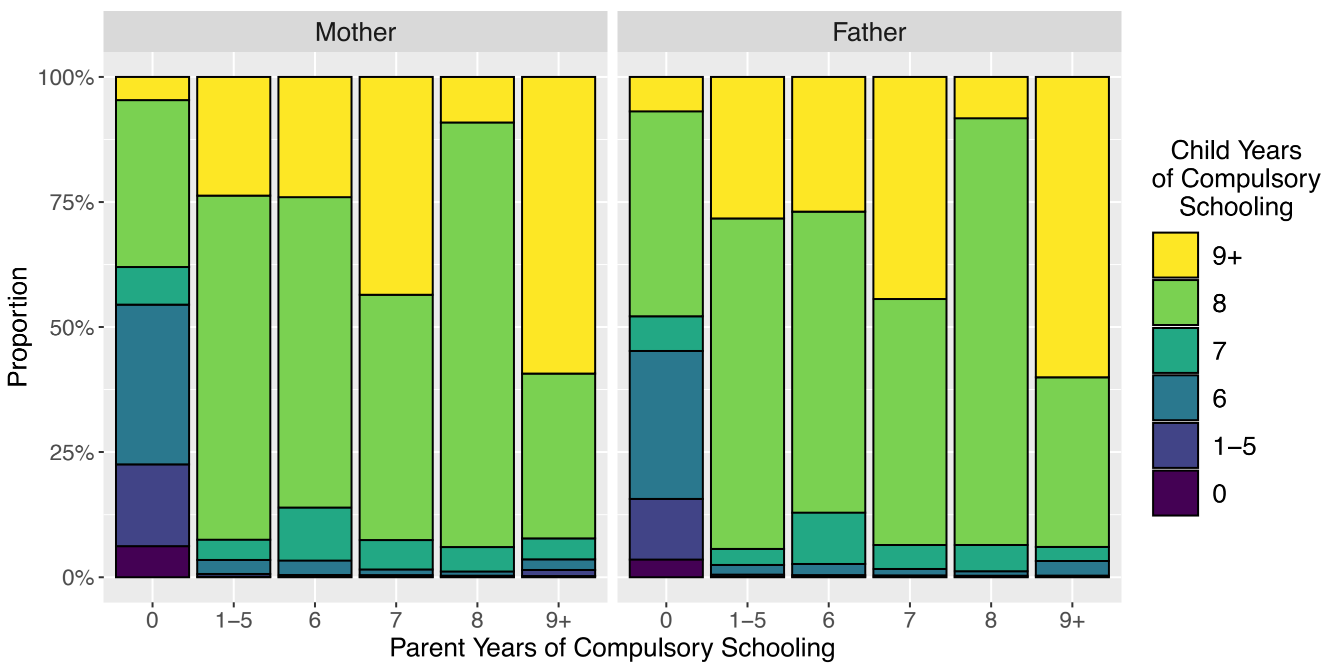

Figure 4: Relationship between parental years of CS (horizontal axis) and proportion of child exposure to CS (vertical axis; color-coded years of CS), separate for mothers (left) and fathers (right).

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

The second challenge is highlighted by Stephens Jr and Yang (2014). This study finds that the standard assumption of common trends across states is generally not valid: controlling for birth region and birth year fixed effects interactions, to allow for differential changes across states, most of the effects of CS laws on various outcomes (ranging from health to educational and labor-market outcomes) become insignificant. In other words, the effects measured in the CS literature may be driven by regional (and not state-specific) time trends, which are then incorrectly attributed to CS laws. To address this, we control in equation 1 for region of birth interactions, and in equation 2 for both parent birth region (r') and birth-year cohort (c') interactions, as well as child birth state (s) and child birth year (y) interactions. Moreover, we cluster the standard errors conservatively, using two-way birth state and birth-year cohort clustering.[5] Our results are robust to the inclusion of these rich sets of controls.

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

3.2 Instrumental Variable Specification

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

Nonetheless, the instrumental variable (IV) approach has a very natural interpretation, causally linking increases in parental education to increases in their children's education. The interpretation of the second-stage β^c coefficient is that for every additional year of parental schooling induced by the CS laws, β^c years of schooling were transmitted to the child. In other words, the IV approach is informative of the intergenerational transmission of schooling from parents to children, providing a useful point of reference. Further, these IV estimates are around one (see section 4.2), suggesting that children benefit from their parent's exposure to CS by roughly the same amount of schooling as their parents did. In addition, these children were themselves exposed to CS. Indeed, our main intergenerational effect estimates for children are larger, roughly double those of their parents (see section 4.2).

In this approach, the first stage relates education $Educ_i^p$ of parent p born in state s' and

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

year y' to their own exposure to CS $CS^p_{s'y'}$:

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

In the second stage, we used the fitted parental education $\widehat{Educ^p_i}$ to predict children's educational attainment:

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

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

4 Main Effects of Compulsory Schooling Laws

In this section, we present the main effects of exposure to CS laws on educational outcomes. First, we present the results of one's own exposure to CS laws on one's own years of schooling (Equation 1) in the *Parents* sample (section 4.1.1). Second, we explore the effectiveness of CS laws (section 4.1.2) by focusing on the relatively small share of the population that was affected by these laws, demonstrating that the seemingly small average effects we find are in fact substantial. Third, we explore the effects of exposure to CS laws on enrollment in and graduation from grade, middle, and high school, as well as college (section 4.1.3). Fourth, we explore the intergenerational effects of parental exposure to CS laws on their offspring's schooling (Equation 2) in the *Children* sample (section 4.2). Fifth, in section 4.2.2, following Black, Devereux and Salvanes (2005), we instrument parental years of schooling in 1940 with the parent's exposure to CS laws, to get a sense for the potential size of the intergenerational effects of parental exposure to CS on the offspring. Sixth, in section 4.2.3 we explore the effects of parental exposure to CS on offspring enrollment and degree completion. Last, in section 4.2.4 we present the results of an analysis of heterogeneity in outcomes for different types of children (e.g., first-born males).

4.1 Effects of Own Exposure to Compulsory Schooling (Parent sample)

4.1.1 Parental Years of Schooling

Table 2 presents estimates of the effect of CS laws on own years of schooling for individuals directly exposed to them in the *Parents* sample (equation 1). One additional year of CS exposure is associated with a 0.008 and 0.005 increase in women's and men's years of schooling, respectively. The largest effects on years of schooling are found for Blacks (more than five and nine times larger than the average effect size for women and men, respectively). This may be because CS was more binding for this demographic and because a large proportion lived in the South, where CS laws were implemented later (in the early twentieth century)

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

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

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

and were plausibly more effective. This increased effectiveness was mainly due to an increase in state government capacity and bureaucratization, which allowed proper enforcement of these schooling laws (Katz, 1976).

4.1.2 Effectiveness of CS Laws

To put these seemingly small effects into perspective, consider that for many high-achieving students, the CS laws had no effect as these students would have stayed in school for longer, regardless of the law. Indeed, typically only a small fraction of students had educational attainment levels lower than mandated by the CS law in their area at the time the law was in effect and thus only a small fraction of students had to remain in school longer than desired. In addition, a 6-year CS law would not induce all individuals to stay in school for an extra six years. For example, an individual with a desired educational attainment of five years would only be induced to stay in school for one additional year at most. These two reasons explain why, even if the CS laws were well-enforced, the expected estimates in Table 2 would be small. A third reason then is that CS laws were not always effectively enforced.

To get a better sense of the magnitude of our estimates, and to understand the scope and effectiveness of the CS laws, we first characterize the population for whom these laws were binding and then simulate what the policy effects would have been under perfect enforcement of the CS laws. Although we cannot observe desired years of schooling, we can estimate the fraction of students that would have preferred to stay in school for fewer years than what was mandated, by measuring the proportion of a cohort that was not yet exposed to the CS law and that had fewer years of schooling completed in adulthood than what was subsequently

Table 3: Compulsory Schooling Law Effectiveness

	Women			
	All	White	Black	Post-1900
Proportion Under CS Years (p.p.)	12.5	12.0	23.0	18.4
Average Schooling Deficit (Yrs)	2.3	2.2	2.5	2.2
Actual Effect of CS Exposure (Yrs)	0.008***	0.007**	0.038***	0.044***
Potential Effect of CS Exposure (Yrs)	0.099	0.090	0.187	0.174
Effectiveness (Actual/Potential)	8%	8%	20%	26%
Treatment on Treated (Actual/Prop. Under CS)	0.064	0.058	0.165	0.239
	Men			
	All	White	Black	Post-1900
Proportion Under CS Years (p.p.)	12.8	12.2	26.2	21.0
Average Schooling Deficit (Yrs)	2.4	2.3	2.6	2.3
Actual Effect of CS Exposure (Yrs)	0.005*	0.004	0.046***	0.093***
Potential Effect of CS Exposure (Yrs)	0.109	0.100	0.261	0.255
Effectiveness (Actual/Potential)	5%	4%	18%	36%
Treatment on Treated (Actual/Prop. Under CS)	0.039	0.033	0.176	0.443

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

mandated. For example, if a state passed a new 6-year CS law affecting cohorts born after 1900, the relevant marginal individuals are those of the 1899 pre-reform cohort that had fewer than 6 years of schooling completed in adulthood (in 1940).

From these pre-reform cohorts, we extract two pieces of information: i) the proportion of individuals who had lower than mandated educational attainment levels (Proportion Under CS Years) and ii) how many years of schooling these undereducated individuals had compared to the reform's mandated minimum (Average Schooling Deficit). Both data points tell us how ambitious these schooling laws were and their potential gains. We aggregate this information over all schooling laws. At the time CS laws came into effect, an average of only 12.8% of men and 12.5% of women had educational attainment levels under the mandated minimum (see Table 3). This percentage was higher for Black men (26.1%) and Black women (23.0%). Non-compliant, low-attainment individuals received on average 2.3 to 2.6 fewer years of schooling than minimum schooling levels demanded.

Thus, only a small fraction of the population was bound by CS laws. Therefore, the estimates in Table 2 on these marginal individuals (Actual Effect of CS Exposure in Table 3) are underestimated (because they represent an average over the entire population). To better understand the *effectiveness* of the schooling laws in producing schooling gains, we measure them against *potential* schooling gains (Potential Effect of CS Exposure). To this end, we perform the following simulation exercise, which effectively imposes on all individuals in our data that they stay in school up to at least the mandated years of CS in their state s' for their specific cohort y'. In the *Parent* sample, we set the years of schooling $Educ_i^p$ to the minimum mandated years of CS $CS^p_{s'y'}$ in their state s' and cohort y' for those who in actuality completed fewer years of schooling $Educ^p_i$ than were mandated $CS^p_{s',y'}$, and leave the years of schooling $Educ^p_i$ the same for those that completed more than the minimum mandated years of CS $CS^p_{s'y'}$. Next, we re-estimate the main effect of own exposure to CS on these simulated years of schooling (Equation 1). In this way, we obtain an estimate of the maximum *potential* schooling gains from CS under perfect enforcement of schooling laws.[6] It provides us with an answer to the question, *"Had enforcement been perfect, by how many years of schooling would the average level of education have been increased?"*.

Even under perfect enforcement, the schooling laws would have produced at most an average of a 0.099 and 0.109 years of schooling gain for men and women (see Table 3). Comparing our estimated gains from Table 2 to these potential gains, the schooling laws were roughly 8% effective for women and 5% effective for men (Effectiveness, Table 3). The ratio of the actual effect divided by the proportion of the population with fewer years of schooling than demanded (the target population), is a rough measure of the effect on the treated (Treatment on Treated, Table 3). First, these effects are large, ranging from 0.033 to 0.443 years of schooling (or between 0.4 and 5.3 additional months of schooling). Second, these laws were more effective in increasing the educational attainment for Black Americans of both sexes and people born after 1900, especially men. This was likely influenced by increased bureaucratization and improvements in the capacity of state governments, who became more able and willing to enforce new and existing CS laws after the turn of the century (Katz, 1976).

Table 4: Effect of Own Exposure to Compulsory Schooling on Degree Completion

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

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

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

4.1.3 Parental Enrollment and Degree Completion

Table 4 shows that the main effect of CS laws was to increase enrollment in, and graduation from, grade school and enrolment into middle school, with smaller effects persisting all the way to high school and college completion.[7] We define a set of indicator variables Attainment_i^ℓ that take a value 100 if an individual ever reaches educational level ℓ, and 0 otherwise so that the estimates are in percentage points. We consider degree outcomes $\ell \in$ {Some Grade School, Grade School, Some Middle School, Middle School, Some High School, High School, Some College, and College}. One additional year of maternal CS exposure increased the probability of attending grade school, graduating from grade school, attending some middle school, and completing college by 0.029, 0.121, 0.090, and 0.033 p.p., respectively. Fathers' exposure led to increases of 0.061 and 0.120 p.p. in grade school attendance and graduation, respectively, and 0.037 p.p. in completing college. These results are consistent with the CS laws of the early twentieth century, which imposed between 6 and 9 years of mandatory schooling.

Note that our results are robust to the Stephens Jr and Yang (2014) critique, who found that causal estimates of the benefits of CS, 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 CS laws have statistically significant effects on years of schooling. We cluster standard errors at the birth state by birth year level, following Abadie et al. (2022).[8]

4.2 Intergenerational Effects of Parental Exposure to Compulsory Schooling On Children's Outcomes (Parent and Children Samples Combined)

4.2.1 Children's Years of Schooling (DiD)

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

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

[8] The authors show that when estimating effects on the entire population and using all clusters available in the population, clustering conservatively leads to unnecessarily large confidence intervals that do not shrink even when sample sizes are large. Moreover, in our setup, treatment assignment is at the birth state by birth year level, which further reduces the need to cluster at the state level.

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

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

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

4.2.2 Children's Years of Schooling (IV approach)

Following Black, Devereux and Salvanes (2005), we estimate the causal effects on child years of schooling of parental exposure to CS by instrumenting parental years of schooling in 1940 with the parent's exposure to CS laws.[9] Table 6 presents the results. Column 1, replicates the exact specification used by Black, Devereux and Salvanes (2005). The other columns show our own specifications with additional controls (afforded by our very large Census sample sizes) for different demographic groups and time periods. Across various specifications and samples, the effects are an order of magnitude larger than those of Black, Devereux and Salvanes (2005), who found essentially no or weak effects[10] for a 1960 reform in Norway and concluded, as a result, that the strong intergenerational correlation in education between parents and offspring reflects selection rather than causal effects of parental education (at least in that setting).

Remarkably, we find very large causal effects. A one-year increase in maternal (paternal) education resulting from exposure to CS increases children's schooling by 1.089 (0.848) years using the Black, Devereux and Salvanes (2005) specification. When using the full set of fixed effects from our main specification (All), the effects are 0.93 (1.04). They range from 0.60 years for the effects of maternal exposure to CS for Black American children to 1.19 years

[9]First stage results are presented in Table 17 of the Appendix. All first-stage F-stats are highly significant, ruling out weak instruments.

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

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

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

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

for paternal exposure to CS for men.

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

The results in Tables 5 and 6 also raise the question of which margin of schooling was affected by parental exposure to CS laws. It could be that the effects of parental exposure to CS on the schooling of the child are largely confined to the lower end of the distribution of parental educational attainment. This could arise if, for example, parental educational attainment establishes a floor for the expected educational attainment of the children. Parents may, for example, wish to ensure their children obtain at least as much formal schooling as they themselves were legally obligated to obtain. Alternatively, an increase in required schooling could increase the chances that children obtain substantially more educational attainment than their parents. For example, higher levels of compulsory education could increase the value that parents place on educational success, as argued by Piopiunik (2014) in the German context. We next test the children's educational attainment margins affected by higher levels of parental CS exposure. As before, we define a set of indicator variables Attainment_i^ℓ that take a value 100 if an individual ever reaches educational level ℓ, and 0 otherwise, so that the estimates are in percentage points. We consider degree outcomes $\ell \in$ {Some Grade School, Grade School, Some Middle School, Middle School, Some High School, High School, Some College and College}.

4.2.3 Children's Enrollment and Degree Completion

Table 7: Effect of Parental Exposure to Compulsory Schooling Laws on Children's Grade Entry and Completion

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

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

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

4.2.4 Family-Level Intergenerational Effects

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

	Dependent Variable: Years of Schooling							
	Son				Daughter			
	Eldest	Youngest	Most Educated	Least Educated	Eldest	Youngest	Most Educated	Least Educated
CS Years (Mom)	0.016***	0.014***	0.016***	0.014***	0.011***	0.011***	0.011***	0.011***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.002)
N (millions)	3.8	3.8	3.8	3.8	3.0	3.0	3.0	3.0
R^2	0.17	0.18	0.18	0.18	0.14	0.14	0.14	0.15
Outcome Means	10.1	10.0	10.2	9.9	10.7	10.7	10.8	10.6
CS Years (Dad)	0.018***	0.016***	0.017***	0.017***	0.012***	0.012***	0.012***	0.012***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.002)
N (millions)	2.8	2.8	2.8	2.8	2.1	2.1	2.1	2.1
R^2	0.16	0.16	0.16	0.16	0.13	0.13	0.13	0.13
Outcome Means	10.2	10.1	10.3	10.0	10.8	10.8	10.9	10.6

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

Up to this point, we have thought of parental exposure to CS as a treatment assigned to different children in different amounts. This focus on the child as the unit of analysis creates at least two interpretational challenges. First, fertility may be affected by CS, potentially creating a selection issue in our *Children* sample. Second, taking the child as the unit of analysis ignores the fact that educational investments are made at the household level. The effects of parental exposure to CS on children from the same household will be jointly determined. More parental resources could increase one child's completed education while leaving that of another unaffected. It could even be that greater resources increase one child's completed educational attainment at the expense of another child's attainment. Household choices could thus create complex patterns of heterogeneity in the effects of parental exposure to CS on child outcomes. This suggests that it may be useful to think of this intergenerational problem at the dynastic level.

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

First, we find evidence that sons benefit more from parental exposure to CS than daughters. While one extra year of exposure to maternal and paternal CS leads to a 0.014 to 0.016

and a 0.017 to 0.018 year increase in boys' years of schooling, respectively, these effects are only 0.011 and 0.012 for girls. Second, eldest sons are the beneficiaries of the largest effects. Eldest sons receive an educational boost of 0.016 and 0.018 years of schooling for each additional year of maternal and paternal CS exposure, respectively. In contrast, youngest sons (0.014 and 0.016), youngest daughters (0.011 and 0.012), and eldest daughters (0.011 and 0.012) receive lower benefits from maternal and paternal exposure. Third, fathers' exposure to CS yields slightly higher, but not statistically significantly different effects from mothers' exposure, across children of all genders and birth orders.

4.2.5 Heterogeneity in the Intergenerational Effects of Compulsory Schooling

Table 9 shows significant variation in the effects of parental exposure to CS laws on offspring years of schooling by parental region of birth, gender, and race. Parental exposure to CS had the largest effects, across race and gender, in the Western United States. These ranged from 0.061 years of schooling gained for every additional year of parental exposure to CS for the children of White men, to 0.243 years for the children of Black men. Exposure to more CS also had sizeable effects on the offspring of Black mothers and Black fathers in the Central United States (respectively, 0.040 and 0.071 years). Generally, effects were larger for the offspring of Black parents than for the offspring of White parents. Effects were small in the Eastern region. This is perhaps not surprising given that the Eastern region - in particular New England - already had very high levels of educational attainment, potentially reflecting diminishing returns.

In the Appendix (Table 18), we present similar results for the effects of exposure to CS laws on years of schooling of the parents themselves. These closely mirror the intergenerational effects, with the exception that exposure to Eastern CS laws appears to have had small negative effects on the educational attainment of Eastern White males and that effects for Blacks are also relatively large in the South.

5 Mechanisms

In this section, we explore plausible channels through which parental exposure to CS influences children's outcomes. We first explore effects on parental labor-market outcomes that could directly affect human-capital investments through the monetary and time resources of the household. We then explore spousal choice. As individuals become more educated, assortative matching suggests they may marry more educated individuals, which could in turn affect child outcomes through increased household resources and higher productivity of parental time. We also study the geographic mobility of households as a result of exposure to more CS. For example, parents exposed to more CS may relocate to areas with better-resourced schools or where there are more job opportunities.

Table 9: Heterogeneity in the Intergenerational Effects of CS Laws

	Dependent Variable: Years of Schooling							
	White				Black			
	Central	East	South	West	Central	East	South	West
CS Years (Mom)	0.015	0.008**	0.022	0.063***	0.040***	-0.009	0.034**	0.102***
	(0.011)	(0.004)	(0.022)	(0.011)	(0.012)	(0.012)	(0.016)	(0.036)
N (thousands)	2,122	3,176	1,827	332	46	49	717	5
R^2	0.05	0.10	0.06	0.14	0.14	0.13	0.12	0.22
Outcome Mean	11.0	10.8	9.7	11.4	9.1	9.6	7.3	9.3
	White				Black			
	Central	East	South	West	Central	East	South	West
CS Years (Dad)	0.024**	0.007**	0.029	0.061***	0.070***	0.013	0.034*	0.243***
	(0.010)	(0.003)	(0.038)	(0.013)	(0.015)	(0.014)	(0.019)	(0.057)
N (thousands)	1,349	2,196	1,259	192	24	27	416	3
R^2	0.06	0.10	0.07	0.15	0.16	0.13	0.12	0.28
Outcome Mean	11.1	10.8	9.7	11.5	9.2	9.7	7.5	9.3

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

5.1 Parental Labor-Market Outcomes and Living Arrangements

Compulsory schooling had significant effects on wages, female employment, home values, and home ownership (see Table 10). Conditional on working, one year of CS exposure increased wage earnings by 0.24% and 0.66% for men and women, respectively, consistent with wage effects of CS laws found by Clay, Lingwall and Stephens Jr (2021).[11]

Women exposed to more CS were less likely to be employed (-0.076%), but when they were, they earned more. Exposure to CS had positive effects on home ownership for men, but these men purchased homes with lower home values. This suggests improved access to home ownership.

These results hint at CS laws increasing social mobility, but also represent potential channels through which changes in parental behaviors and outcomes may have affected children's outcomes and shaped intergenerational mobility.

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

Table 10: Compulsory Schooling Exposure vs Other Outcomes

	Dependent Variable:				
	log(Wage) (p.p.)	Employment (p.p.)	log(Rent) (p.p.)	log(Home Value) (p.p.)	Home Ownership (p.p.)
CS Years (Male)	0.237***	-0.009	0.075	-0.384***	0.130***
	(0.074)	(0.016)	(0.094)	(0.103)	(0.034)
N (millions)	2.6	4.1	1.9	2.1	4.1
R^2	0.13	0.01	0.17	0.17	0.04
Outcome Means	$1,427	89.6	$69	$3,314	52.7
CS Years (Female)	0.664***	-0.076***	0.119	-0.375***	0.047
	(0.140)	(0.020)	(0.088)	(0.100)	(0.029)
N (millions)	0.7	5.7	2.6	2.9	5.7
R^2	0.12	0.03	0.18	0.18	0.05
Outcome Means	$570	14.6	$66	$3,273	51.7

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

5.2 Assortative Matching

Exposure to higher levels of CS changed the characteristics of a person's future spouse (see Table 11). Individuals exposed to more CS married more educated and higher-earning spouses, on average. A one-year increase in exposure to CS is associated with marrying a wife with 0.010 more years of schooling and 0.61% higher wages, and a husband with 0.012 more years of schooling and 0.26% higher wages. These assortative mating effects on the education of the spouse are similar in size to the effects of CS on own educational attainment, i.e. they are substantial, as demonstrated earlier in sections 4.1.2 and 4.2.2.

Lastly, men's exposure to CS is linked to a 0.043 p.p. decrease in spousal employment and a 0.61 p.p. increase in their spouses' wages, if working, suggesting that more educated couples were able to support a family with a single income, with the female working only if her potential wages were relatively high.

5.3 Fertility

Compulsory schooling may impact children's outcomes through parental fertility decisions. Indeed, as individuals become more educated - possibly in conjunction with longer time spent in school and increased labor-market returns - they may have fewer children. This allows parents to invest more time and other resources per child. Thus, there is a so-called quantity-quality trade-off concerning the number of children (Becker, 1960). This trade-off may have

Table 11: Effect of Compulsory Schooling on Assortative Matching

	Dependent Variable: Spouse's Characteristics			
	Schooling (Years)	log(Wage) (p.p.)	Participation Rate (p.p.)	Employment (p.p.)
CS Years (Female)	0.012***	0.255***	-0.007	-0.026
	(0.003)	(0.094)	(0.013)	(0.018)
N (millions)	3.4	2.2	3.5	3.5
R^2	0.15	0.13	0.00	0.01
Outcome Means	8.1	$1,428	95.2	90.7
CS Years (Male)	0.010***	0.614***	-0.039**	-0.043***
	(0.002)	(0.220)	(0.016)	(0.016)
N (millions)	3.4	0.3	3.5	3.5
R^2	0.14	0.10	0.02	0.02
Outcome Means	8.4	$570	9.6	9.0

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

influenced the intergenerational effects described previously.

Moreover, extensive margin changes in fertility due to CS may bias our results. Indeed, if parents exposed to more CS are less likely to become parents, they will be less likely to appear in our parent-child sample. To measure the effects of the CS laws on fertility, we use a question in the 1940 census asking a 1% randomly selected sample of women the number of live births born to them. We report the results in Table 12. For women aged 35 to 65 in 1940, exposure to more CS did not affect the probability of having ever been married or having at least one child. Thus our results do not appear to suffer from sample selection bias due to changes in maternity. On the intensive, margin, however, we do find a 0.015 decrease in the number of children per woman exposed to more CS. This represents a roughly 0.4% decrease in the number of children a woman has per year when exposed to more CS.

5.4 Neighborhood Sorting

Neighborhood choice is a potentially important mechanism linking parental education to child outcomes. Neighborhoods differ in their schooling, labor-market, demographic, and household characteristics, as well as their levels of urbanity. Associated differences in the environments that children are exposed to may in turn influence their outcomes. In the American context, education policies, and the resources of school districts, vary considerably at the local level, resulting in differences in the quality of local schools. Parental exposure to more CS could shift attitudes about the importance of childhood education, increasing the desire to move to areas with better schools (Piopiunik, 2014). Higher parental exposure to CS could also increase geographic mobility in search for better jobs, with better jobs being

Table 12: Effect of Compulsory Schooling on Fertility

	Dependent Variables:		
	Ever Married	Any Child	Number of Children
CS Years	-0.013	-0.022	-0.015***
	(0.029)	(0.032)	(0.003)
Observations	910,455	799,577	706,878
R^2	0.01	0.02	0.06
DV Mean	89.8	88.4	3.9

Notes: Effect of exposure to different CS laws on women's marriage and fertility decisions. Sample include the 1% randomly selected sample of women aged 35 to 65 in the 1940 census. Controls include birth year, birth state, birth region and birth year interactions, and race. Standard errors are clustered at the birth state by birth year level. *p<0.1; **p<0.05; ***p<0.01.

located in areas where schools are also better. Beyond school quality, CS could influence child outcomes by the local context in which children were raised, such as neighborhood demographic characteristics, neighborhood household characteristics, and levels of urbanity.

We make use of the enumeration district variable, which allows us to identify all households living in the same, fine-grained geographic area (the enumeration district) and to measure the characteristics of these households.[12] For each household in our *Children* and *Parents* samples, we construct five dimensions of neighborhood characteristics, by calculating averages over all households in the neighborhood, where the neighborhood is the enumeration district where the household lives. These dimensions are: (i) neighborhood schooling characteristics, (ii) neighborhood labor-market characteristics, (iii) neighborhood demographic characteristics, (iv) neighborhood housing characteristics, and, last, (v) neighborhood urbanity. We describe these neighborhood characteristics in more detail below.

We test the role of neighborhood sorting by examining whether parental exposure to CS predicts sorting into different types of enumeration districts when their children were of school age (ages 5-14), i.e. when neighborhood characteristics were more likely to impact child development. To this end, we compute average enumeration-district level characteristics in the 1910, 1920, and 1930 censuses. We then match each individual in the *Children* sample to the enumeration district they (and their parents) inhabited at ages 5-14. For example, if a child was born in 1900, we match them to the enumeration district they inhabited in 1910 (when they were 10 years old). Thus, we use the Children's sample that can be linked back to a census year when the children were 5 to 14 years old.[13] We then measure the effect of

[12] An enumeration district, as used by the Bureau of the Census, was an area that could be covered by a single enumerator in one census period. These enumeration districts varied in size, from several city blocks to an entire county in less densely populated areas. They also have the desirable feature that they do not cross the boundaries of a county, township, incorporated place, ward, or other political subdivision.

[13] Note that the Children sample is by definition older than 18 in 1940, so that the 1940 census cannot be

the parent's exposure to CS laws on the characteristics of the enumeration district they (and their child) inhabited.

To explore neighborhood sorting, we set up the following equation, which relates child c's neighborhood of residence n's characteristics (Y_{nc}) to parents p's exposure to CS years in their state s' and for their birth cohort y' ($CS_{s'y'}$):

$$\begin{aligned} Y_{nc} =& \beta\, CS^{p}_{s'y'} + \gamma^{c}_{s} + \delta^{c}_{y} + \left(\eta^{c}_{s} \times \theta^{c}_{y}\right) + \gamma^{p}_{s'} + \delta^{p}_{y'} + \left(\eta^{p}_{r'} \times \theta^{p}_{y'}\right) \\ &+ \lambda^{c} Race^{c}_{i} + \mu^{c} Sex^{c}_{i} + \epsilon^{c}_{i}, \quad p = m, f \end{aligned} \tag{5}$$

This specification is identical to Equation (2) and includes vectors of fixed effects for the parent p's state of birth (s') and birth year (y') cohort ($\gamma^{p}_{s'}$ and $\delta^{p}_{y'}$ respectively), interactions ($\eta_{r'} \times \theta_{y'}$) between parent p's region (r') of birth ($\eta_{r'}$)[14] and birth year (y') cohort ($\theta_{y'}$). We include controls for the child's birth year y (δ^{c}_{y}) and state s (γ^{c}_{s}) as well as controls for child c's race (λ) and sex (μ).

The effect β of parental exposure to CS laws $CS_{s'y'}$ is identified from variation across states of birth (s') and birth year (y') cohorts, conditional on regional trends (captured by the region and birth year cohort interactions $\eta_{r'} \times \theta_{y'}$), state differences in levels (captured by state fixed effects, $\gamma_{s'}$) and cohort differences in levels (captured by birth year cohort fixed effects, $\delta_{y'}$) and on the child's birth year y (δ^{c}_{y}) and state s (γ^{c}_{s}) as well as controls for child c's race (λ) and sex (μ). In other words, we compare children of the same state-cohort, exploiting variation in their parents' birth state and birth year, which impacts their exposure to CS and measure the effect of CS on neighborhood sorting when the child is between 5 and 14 years old.[15] We estimate equation 5 separately for maternal and paternal exposure and present the results in Tables 13 and 14.

Neighborhood Schooling Characteristics: Lacking detailed data on school district budgets or resources, we proxy school resources by computing enumeration-district teacher-student ratios in the 1910-1930 censuses, as follows. We use the occupation question in the censuses to identify teachers from working adults' occupations and the school-enrollment question to measure school attendance. Next, we create a standardized measure of teachers per student for each enumeration district in the United States for each of the four censuses.[16] Further, the census only contains educational attainment information starting in 1940. Since we wish to study neighborhood characteristics when the children in our sample were of school age (5-14), we proxy the levels of schooling in each enumeration district using literacy rates and school enrollment data in one of the 1910 - 1930 censuses, depending on the birth year of the child. We separately compute school enrollment rates for children aged 6 to 18 and 19 to 25, which proxy primary to secondary and tertiary school-enrollment rates, respectively.

Parents exposed to one additional year of CS sorted into neighborhoods, with on average higher teacher-student ratios, higher literacy rates, and higher school enrollment rates (Tables 13 and 14). Maternal (paternal) exposure to one additional year of CS is associated with a 0.0069 (0.0071) standard deviation (sd) higher neighborhood teacher-student ratio and

used to compute neighborhood characteristics during childhood (ages 5-14).

[14]East, Center, South and West.

[15]We estimate specifications with no child controls and obtain similar results.

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

Table 13: Mother's Exposure to CS and Geographic Sorting

			Neighborhood Schooling Characteristics			
			Teacher-Student Ratio	Literacy Rate	School Enrol. (6-18)	School Enrol. (19-25)
CS Years			0.0069***	0.0046***	0.0020***	0.0038***
			(0.0008)	(0.0006)	(0.0006)	(0.0008)
N (millions)			5.6	5.6	5.6	5.6
R^2			0.04	0.40	0.25	0.08
			Neighborhood Labor-Market Characteristics			
			LFP (Men)	Employment (Men)	LFP (Women)	Employment (Women)
CS Years			-0.0023***	0.0029***	-0.0019***	0.0021***
			(0.0005)	(0.0009)	(0.0006)	(0.0006)
N (millions)			5.6	4.4	5.6	4.4
R^2			0.04	0.12	0.15	0.03
			Neighborhood Demographic Characteristics			
	Female	Black	White	Immigrant	State Mover	Average Age
CS Years	0.0026***	0.0009***	-0.0009***	-0.0095***	-0.0001	0.0089***
	(0.0007)	(0.0003)	(0.0003)	(0.0012)	(0.0012)	(0.0009)
N (millions)	5.6	5.6	5.6	5.6	5.6	5.6
R^2	0.08	0.58	0.57	0.48	0.44	0.31
			Neighborhood Household Characteristics			
		Home Ownership	Home Value	Rent	Household Size	Multifamily Household
CS Years		0.0030**	-0.0022	-0.0003	-0.0042***	0.0069***
		(0.0013)	(0.0016)	(0.0009)	(0.0006)	(0.0008)
N (millions)		5.6	4.0	4.0	5.6	5.6
R^2		0.21	0.23	0.06	0.25	0.09
			Urban/Rural Status and Own Migration			
			Locality Population (sd)	Metropolitan Area (pp)	Urban Area (pp)	State Mover (Own, pp)
CS Years			-0.0149***	-0.1674***	-0.1238***	0.1759**
			(0.0027)	(0.0411)	(0.0445)	(0.0742)
N (millions)			5.6	5.6	5.6	5.6
R^2			0.26	0.25	0.16	0.04
DV Mean			-0.1	42.9	48.5	13.5

Notes: Relationship between maternal exposure to CS and neighborhood characteristics when child is of school age. Sample includes all mothers who are linked to the census in which their child was aged 5 to 14. Controls include parent birth year, birth state, birth region, birth region by birth year and race fixed effects and child birth year, birth state, birth state by birth year, sex and race fixed effects. Standard errors are clustered at the child's birth state by birth year level. *p<0.1; **p<0.05; ***p<0.01. A locality is considered metropolitan if its population is at least 50,000, and urban if its population is at least 2,500.

Table 14: Father's Exposure to CS and Geographic Sorting

			Neighborhood Schooling Characteristics			
			Teacher-Student Ratio	Literacy Rate	School Enrol. (6-18)	School Enrol. (19-25)
CS Years			0.0071***	0.0042***	0.0005	0.0023***
			(0.0008)	(0.0006)	(0.0007)	(0.0007)
N (millions)			4.4	4.4	4.4	4.4
R^2			0.04	0.41	0.26	0.08

			Neighborhood Labor-Market Characteristics			
			LFP (Men)	Employment (Men)	LFP (Women)	Employment (Women)
CS Years			-0.0019***	0.0047***	-0.0010	0.0029***
			(0.0005)	(0.0009)	(0.0007)	(0.0006)
N (millions)			4.4	3.6	4.4	3.6
R^2			0.04	0.11	0.15	0.03

			Neighborhood Demographic Characteristics			
	Female	Black	White	Immigrant	State Mover	Average Age
CS Years	0.0015**	0.0012***	-0.0011***	-0.0086***	-0.0002	0.0082***
	(0.0007)	(0.0003)	(0.0003)	(0.0011)	(0.0012)	(0.0009)
N (millions)	4.4	4.4	4.4	4.4	4.4	4.4
R^2	0.08	0.58	0.57	0.48	0.43	0.32

			Neighborhood Household Characteristics			
		Home Ownership	Home Value	Rent	Household Size	Multifamily Household
CS Years		0.0019	-0.0007	0.0001	-0.0034***	0.0085***
		(0.0012)	(0.0011)	(0.0007)	(0.0007)	(0.0009)
N (millions)		4.4	3.2	3.3	4.4	4.4
R^2		0.21	0.22	0.06	0.26	0.09

			Urban/Rural Status and Own Migration			
			Locality Population (sd)	Metropolitan Area (pp)	Urban Area (pp)	State Mover (Own, pp)
CS Years			-0.0125***	-0.1248**	-0.1906***	0.1394
			(0.0023)	(0.0484)	(0.0532)	(0.0907)
N (millions)			4.4	4.4	4.4	4.4
R^2			0.24	0.25	0.15	0.05
DV Mean			-0.1	42.7	47.9	13.7

Notes: Relationship between paternal exposure to CS and neighborhood characteristics when child is of school age. Sample includes all fathers who are linked to the census in which their child was aged 5 to 14. Controls include parent birth year, birth state, birth region, birth region by birth year and race fixed effects and child birth year, birth state, birth state by birth year, sex and race fixed effects. Standard errors are clustered at the child's birth state by birth year level. *p<0.1; **p<0.05; ***p<0.01. A locality is considered metropolitan if its population is at least 50,000, and urban if its population is at least 2,500.

a 0.0046 (0.0042) sd higher neighborhood literacy rate. Neighborhood school enrollment between the ages of 6 and 18 is also higher (0.0020) for women exposed to CS and both mothers and fathers sort into neighborhoods with higher postsecondary enrollment rates (0.0038 and 0.0023 sd, respectively). More favorable neighborhood schooling characteristics may have contributed to the intergenerational persistence of the effects of CS exposure on the human-capital accumulation of their offspring.

Neighborhood Labor-Market Characteristics: Next, we measure how CS impacted sorting into neighborhoods with different labor market characteristics. Exposure to more CS (Tables 13 and 14) led to sorting into neighborhoods with lower labor-force participation rates for men (-0.0019 to -0.0023 sd) - fewer inhabitants working or looking for work - but higher employment rates (between 0.0021 and 0.0047 sd) for those in the labor force. One interpretation of this combination of results is that parents exposed to more CS could have been living in more affluent neighborhoods where there were more opportunities for work (higher employment rate), but where more prime-aged individuals also chose not to work - perhaps due to higher rates of school attendance. Lower labor-force participation could, however, also indicate a larger share of unemployed individuals who have given up on seeking work. This would be consistent with the effects of the Great Depression.

Neighborhood Demographic Characteristics: We use the sex, race, age, and country (or state) of birth of respondents living in the same enumeration district to construct neighborhood demographic characteristics, including immigrant and state-mover status. Parents exposed to more CS (Tables 13 and 14) sorted into neighborhoods with higher proportions of female (0.0015 to 0.0026 sd) and Black residents (0.0009 to 0.0012 sd) and lower proportions of Whites (-0.0009 to -0.0011 sd) and immigrants (-0.0086 to -0.0095 sd). Their inhabitants were also, on average, older (0.0082 to 0.0089 sd).

Neighborhood Housing Characteristics: We make use of questions about home ownership, home value, and monthly rent paid. Homeownership is a potentially important savings channel and thereby a measure of wealth. The amount of rent paid, and home value, are more direct measures of neighborhood income and wealth. Questions on the number of household members and multifamily household status provide additional information about living arrangements and family size. Exposure to more CS (Tables 13 and 14) sorted parents into neighborhoods with marginally higher rents (0.0015; fathers only) and higher homeownership rates (0.0030 sd; mothers only). Moreover, CS-exposed individuals were more likely to live in multifamily households (0.0069 to 0.0085 sd), while at the same time having households with fewer individuals (-0.0034 to -0.0042 sd). This is suggestive evidence for sorting into neighborhoods with lower fertility rates, while, at the same time, these residents have the means to take in relatives (e.g., grandparents) or had to because of the Great Depression.

Neighborhood Urbanity: Finally, we find that CS exposure makes it more likely for parents to live in rural areas. Exposure to compulsory schooling led to living in a locality with a smaller population (-0.0125 to -0.0149 sd) and less likely to be metropolitan (-0.12 to -0.17 pp) or urban (-0.12 to -0.19 pp). Meanwhile, CS-exposed women were more likely to live in a different state than the one they were born in, when their child was of school age (0.18 pp per CS year exposure).

While this positive link between CS and migration toward rural areas is surprising at first, this could be a consequence of the Great Depression. First, given that our children's sample mean age is 22.4 in 1940 (see Table 1), most of the outcomes measured in this section come from the 1930 census, i.e., at the onset of the Great Depression. The period following the Wall Street Crash of 1929 saw a reversal of the rapid urbanization that had characterized the early twentieth century (Lively and Taeuber, 1939). Metropolitan areas were hardest hit by the Great Depression and many individuals and families returned to the countryside to find economic relief. Separately analyzing each Census, we find the same pattern in the 1940 census, but the opposite in the 1910 and 1920 censuses, when CS exposure is positively associated with migration toward less urban areas.[17]

These findings suggest that exposure to compulsory schooling could have enhanced individuals' geographic mobility and allowed them to more flexibly react to economic shocks. This translated into a higher likelihood to sort into cities in 1910 and 1920, taking advantage of work opportunities driving urbanization. In 1930 and 1940, this pattern was reversed, as individuals exposed to more CS were more likely to migrate to less urban areas, taking refuge from the economic hardships that were hitting cities particularly hard.

6 Mediation Analysis

In the previous section, we explored several channels that may have contributed to the intergenerational persistence of CS laws. Parental exposure to more CS not only increased parental years of schooling, but also allowed parents to obtain higher-paying jobs, increase home ownership, and marry more educated and higher-earning spouses. Parents were also more likely to sort into neighborhoods with systematically different characteristics, including higher education levels, more school resources, higher school attendance, and higher literacy rates. To quantify the relative importance of each of these channels on the intergenerational effects of parental exposure to more CS, we perform a Gelbach decomposition (Gelbach, 2016). This approach consists of adding various controls to our baseline regressions (Column 1 in Table 15) and quantifying how the estimate of the effect of parental CS exposure on children's years of schooling changes as these controls are included. We include the following set of potential mediators:

- Parental Own Education: Years of schooling for mother (Col. 1) or father (Col. 2)

- Parental Own Labor Market Outcomes: labor force participation, employment, and wages for the mother (Col. 1) or father (Col. 2).

- Parental Own Housing Outcomes: home ownership, home value, and rent paid for the mother (Col. 1) or father (Col. 2).

- Spouse's Education: Years of schooling for father (Col. 1) or mother (Col. 2)

- Spouse's Labor Market Outcomes: labor force participation, employment, and wages for the father (Col. 1) or mother (Col. 2).

[17] We estimated the link between CS and urban/rural and metropolitan status using all adults observed in the 1910, 1920, 1930, and 1940 censuses, as well as the subsample of our parent sample observed in 1930 and 1940. Since identifying parents in 1920 and 1910 relies on several linkages, which significantly reduces the sample size, we did not run analyses in the 1910 and 1920 censuses of our parents sample.

- Neighborhood variables: Neighborhood averages when the child was 5-14 years old.[18]

We restrict this decomposition exercise to individuals in the *Children* sample who 1) can be linked to a previous census in which they were observed to be 5-14 years old, and 2) whose parents can both be identified in the 1940 census. These requirements reduce the sample size considerably, but they allow us to fully observe the list of mediating channels explored in the previous sections. The top panel of Table 15 presents the baseline estimates of the effects of maternal and paternal CS exposure on a child's years of schooling in this restricted sample. Note that the estimated intergenerational effects are similar, but not identical, to those in our full sample (compare with Table 5).

The bottom panel of Table 15 presents the results of the Gelbach (2016) decomposition exercise. While the Gelbach decomposition controls for each mediating variable separately, we show the aggregate explanatory power of groups of related mediators. Taken together, all of the observables at the parental, spousal, and neighborhood characteristics can explain roughly 74.4% and 76.4% of the effects of mothers' and fathers' exposure to CS on children's years of schooling, respectively. The effects of mothers' exposure to CS on children's years of schooling are explained directly by mothers' education (38.7%), followed by that of their spouse (21.7%), hinting at strong assortative mating effects. Neighborhood education levels and housing characteristics (measured as the average level of education in the enumeration district) explain 14.0% and 9.4% of the effects, respectively, while other neighborhood characteristics, such as neighborhood labor-market characteristics matter little. Neighborhood demographics (greater diversity), however, have a negative effect, of -13.2%, meaning that including controls for these characteristics actually increases the coefficient on the intergenerational effect of compulsory schooling exposure. Additionally, the parental housing situation (5.6%) and the father's labor-market characteristics (5.6%) explain a small portion of the total effect. Last, Mothers' own labor-market outcomes matter very little. This is not surprising, given the low female labor force participation rates around 1940 of around 10%.

For the effect of fathers' exposure to CS on children's years of schooling, only 22.0% is explained by the father's own education level, while 46.9% is explained by that of their spouse, 6.6% by parental housing characteristics, and 13.8% and 16.9%, respectively, by neighborhood housing and education characteristics - including school resources. Here too, neighborhood demographics, however, have a negative effect, of -17.2%.

Interestingly, parents' labor market characteristics (which include employment, participation, and wages), explain relatively little of the effects of parental CS exposure on children's education, once we condition on parental education. Indeed, parental labor market characteristics and neighborhood demographics inflate our estimates of interest when controlled for, instead of dampening these effects. Lastly, only neighborhood housing characteristics - such

[18] Education (Neighborhood) includes teacher-student ratios, literacy rates, and school enrollment rates for those aged 6-18 and 19-25; Labor-Market (Neighborhood) includes neighborhood-level employment rates and labor force participation rates for men and women separately; Housing (Neighborhood) includes the rate of home ownership, average home values, average rent, average household size and the proportion of multifamily households; Demographic (Neighborhood) includes the proportion of Female, Black, White, immigrant, and out of state inhabitants, and average age; Urbanization (Neighborhood) includes the population of the locality the neighborhood is situated in, metropolitan status of neighborhood, urban status of the locality, and fraction State Movers. A locality is metropolitan if its population is at least 50,000, while a locality is considered urban if its population is at least 2,500.

Table 15: Decomposition of Intergenerational Effects

	Dependent variable: Child's Years of Schooling	
	Mother	Father
CS Years (Parent)	0.017***	0.012***
N (millions)	4.4	4.4
	Relative contribution	
Education (Own)	38.7%	22.0%
Labor Market (Own)	-0.4%	-5.9%
Housing (Own)	5.6%	6.6%
Education (Spouse)	21.7%	46.9%
Labor Market (Spouse)	0.4%	-0.4%
Education (Neighborhood)	14.0%	16.9%
Labor Market (Neighborhood)	-0.5%	-2.1%
Housing (Neighborhood)	9.4%	13.8%
Demographics (Neighborhood)	-13.2%	-17.2%
Urbanization (Neighborhood)	-1.3%	-4.1%
Total	74.4%	76.4%

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

as home ownership, rent levels, other housing characteristics, which are probably proxies for wealth - and neighborhood education characteristics - such as school attendance rates, literacy rates, and teacher-student ratios - seem to be the channels through which geographical sorting explains the intergenerational effects of CS.

7 Robustness

7.1 Difference-in-Differences Estimator

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

Table 16: Goodman-Bacon Decomposition

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

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

7.1.1 Goodman-Bacon Decomposition

We address these issues in two ways. First, we conduct a decomposition of our TWFE estimator as per Goodman-Bacon (2021).[19] We show the results of this decomposition in Table 16. Several main takeaways emerge. When using never-treated states as controls (top row), the difference-in-difference estimators are very large and every single 2×2 comparison yields positive estimates for the effect of parental CS exposure on parent and children's years of schooling. These particular 2×2 comparisons suggest that exposure to CS increased parental and children's educational attainment by 0.37-0.43 years of schooling, or roughly 0.06-0.07 years of schooling per year of CS, which is much greater than our TWFE estimates (0.005-0.008 for parents and 0.015 for children). While these effects are larger they are still smaller than the effects we found by instrumenting parental exposure to CS (section 4.2.2).

Further, when using always-treated states as controls, the estimates also tend to be positive and significant. These later-treated versus always-treated comparisons account for about 70% of the weight of the TWFE estimator, as many large states were already treated by the time the 1880 birth cohort started attending school. The later-treated vs earlier-treated comparisons, which are "forbidden" because they are contaminated by the earlier-treated states already having received treatment during the pre-period, account for only about 10% of the weight in our TWFE estimator. Unsurprisingly, these particular estimates are much smaller in magnitude. In summary, the issues associated with staggered difference-in-differences likely cause a downward bias in our TWFE estimates. Our results therefore represent conservative estimates of the true effect of CS laws on educational attainment and on the intergenerational transmission of education.

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

The second approach to validate our results is akin to the methodology used by Cengiz et al. (2019). It aims to manually eliminate all the problematic control group units (i.e. units with varying treatment status). We turn our CS variable into an indicator of being exposed to no CS (0) or any amount of CS (1). This simplification allows us to avoid the issue of varying

[19]To conduct the Goodman-Bacon decomposition, we first change our CS variable to a binary treatment variable that takes the value of 1 for all individuals who were exposed to at least one year of CS. We also collapse our data to birth state - birth year cells to speed up computation.

treatment intensities and makes it possible to apply the methodology in Cengiz et al. (2019). As a benchmark, we first estimate versions of equations 1 and 2 with this new CS variable. We then apply the Cengiz et al. (2019) methodology.

More specifically, for each event consisting of a treated state introducing their first CS law, we define a control group comprising all states which have not yet introduced a CS law. In this way, we can compare our treated state to the control states and obtain a simple event-study estimate of the effect of the policy. We repeat this procedure for each event (i.e. introduction of a new CS law in a state) in our data, defining an event-specific control group as a basis of comparison. We then normalize the time of CS law implementation to zero across all these events and stack all the treatment and control groups to estimate one meta-event study measuring the effect of the introduction of new CS laws.

To be more specific, each event-specific dataset consists of a 10-year panel of data. It includes the data from the treated state introducing the new schooling law in year y between years $y-3$ and $y+7$. It also includes data from *clean* control states. These clean control states consist of states which did *not* introduce their first schooling law between $y-3$ and $y+7$. By including only these sates in the control group, we prevent the "forbidden" comparisons between the treated state and control states who change their treatment status at other points in time and bias the results. We then stack all the resulting datasets and perform the difference-in-difference estimation. The advantage of this approach is that it eliminates all problematic control states that have variation in their schooling laws and may create biases in the results.

We report the results for the baseline estimates and the Cengiz et al. (2019) estimates in appendix Tables 19 and 20, both for the direct effects on parental years of schooling and the intergenerational effect on the childrens' years of schooling. First, we find that the baseline specifications also show positive and significant effects of parental exposure to CS on parental and childrens' years of schooling. Additionally, the Cengiz et al. (2019) estimates are similar in magnitude to the baseline estimates and basic qualitative patterns hold. In particular, the intergenerational effects of CS on children's educational attainment are slightly larger than the direct effects on parental educational attainment. Moreover, the direct and intergenerational effects on Black Americans are the largest of all subgroups.

8 Conclusion

In the late nineteenth and early twentieth centuries, states across the United States sequentially introduced CS laws, seeking to raise educational attainment and boost the social mobility of less educated and poorer families, with ever-increasing years of required schooling. Using the linked 1880-1940 full-count censuses and cross-linkages, we examine a large number of outcomes across the entire life cycle, for both parents and children. Using a difference-in-differences approach, we find that CS laws increased the educational attainment of both individuals directly exposed to the reforms, as well as that of their children. The effects of CS laws on the attainment of the second generation were larger than the effects on the first generation, suggesting that educational reforms may have successfully "snowballed" to achieve the rapid growth in educational attainment over the 20^{th} century. Exploring various ways of quantifying the intergenerational effects of CS laws, we find the intergenerational effects to be larger than previously thought. In environments with high social mobility and rapidly increasing educational levels, policies aiming to increase the educational levels of low-education individuals may have very large intergenerational effects.

We explore channels that may explain the very strong intergenerational effects we obtain. Our decomposition exercise suggests a set of potentially important mediators. Exposure to more CS enabled both males and females to marry more educated and higher-earning spouses and reduced the number of children, which may have allowed for increased investments per child. Men exposed to more CS earned higher wages and gained access to home ownership. Women on the other hand reduced their employment, but when they worked earned substantially higher pay, suggesting a trade-off between home production (afforded by a higher-income male) and the wage potential of the female. Exposure to more CS also sorted people into better neighborhoods when their children were of school age, with higher teacher-student ratios (i.e. school resources), higher literacy rates (more educated population), and higher school enrollment rates.

A Appendix Tables and Figures

Table 17: Effect of Parental Years of Schooling on Children's Years of Schooling (IV First Stage)

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

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

Table 18: Heterogeneity in Effects of CS Laws on Parental Years of Schooling

	Dependent Variable: Years of Schooling							
	White				Black			
	Central	East	South	West	Central	East	South	West
CS Years (Women)	0.006*	-0.006*	0.036**	0.119***	0.044***	0.001	0.051***	0.157***
	(0.004)	(0.003)	(0.016)	(0.011)	(0.014)	(0.013)	(0.012)	(0.038)
N (thousands)	1,371	2,159	1,205	238	29	32	462	4
R^2	0.03	0.05	0.03	0.08	0.14	0.09	0.05	0.13
DV Mean	8.7	8.6	7.7	9.3	6.8	7.4	5.1	7.1
	White				Black			
	Central	East	South	West	Central	East	South	West
CS Years (Men)	0.023***	-0.011***	0.029*	0.101***	0.033	-0.015	0.119***	0.179**
	(0.004)	(0.004)	(0.016)	(0.013)	(0.024)	(0.015)	(0.019)	(0.069)
N (thousands)	1,003	1,610	876	152	16	19	287	2
R^2	0.03	0.04	0.02	0.08	0.15	0.09	0.03	0.11
DV Mean	8.8	8.5	7.3	9.2	6.5	6.8	4.5	6.5

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

Table 19: Effect of Own Exposure to Compulsory Schooling on Years of Schooling: Cengiz et al. (2019)

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

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

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

Table 20: Effect of Parental Exposure to Compulsory Schooling on Children's Years of Schooling: Cengiz et al. (2019)

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

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

B Data Appendix

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

B.1 Census Data

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

- IPUMS 1880-1940 US full count census[20]
- 1880-1900 to 1930-1940 US Census cross-walk[21]

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).

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

[21] 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 the following codes:

1. Run all codes in `~/CODE/00 clean and merge/` in order. These
   - read the census and crosswalk files
   - create one large crosswalk between 1880 and 1940 to identify individuals and their parents across time[22]
   - extract the relevant parent and children samples
   - clean the samples
2. Open the main.R file in `~/CODE/Figures and Tables/` and change the wd (working directory) variable to the relevant path on your machine.
3. Run the main.R file preamble and the lines related to your desired figure/table replication file. Each table and figure can be replicated separately. Each individual replication file is stored in `~/CODE/Figures and Tables/`.
4. Some exceptions:
   - for neighborhood sorting results, first open: `~/CODE/Figures and Tables/07 Tables Neighborhood Sorting/` and run `create_neighbor_stats_1910.R` and `create_neighbor_stats_1940.R` to create neighborhood-level measures, before running the .Rmd replication scripts from main.R.

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

References

Abadie, Alberto, Susan Athey, Guido W Imbens, and Jeffrey M Wooldridge. 2022. "When should you adjust standard errors for clustering?" *The Quarterly Journal of Economics*, 138(1): 1–35.

Acemoglu, Daron, and Joshua Angrist. 2000. "How large are human-capital externalities? Evidence from compulsory schooling laws." *NBER macroeconomics annual*, 15: 9–59.

Barnard, Henry, John Eaton, Nathaniel Dawson, and William Harris. 1947. *Biennial Survey of Education 1938-1940.* U.S. Office of Education.

Becker, Gary S. 1960. "An economic analysis of fertility." In *Demographic and economic change in developed countries.* 209–240. Columbia University Press.

Becker, Gary S, and Nigel Tomes. 1979. "An equilibrium theory of the distribution of income and intergenerational mobility." *Journal of political Economy*, 87(6): 1153–1189.

Becker, Gary S, and Nigel Tomes. 1986. "Human capital and the rise and fall of families." *Journal of labor economics*, 4(3, Part 2): S1–S39.

Black, Sandra E, Paul J Devereux, and Kjell G Salvanes. 2005. "Why the apple doesn't fall far: Understanding intergenerational transmission of human capital." *American economic review*, 95(1): 437–449.

Callaway, Brantly, and Pedro HC Sant'Anna. 2021. "Difference-in-differences with multiple time periods." *Journal of Econometrics*, 225(2): 200–230.

Card, David, Ciprian Domnisoru, and Lowell Taylor. 2022. "The intergenerational transmission of human capital: Evidence from the golden age of upward mobility." *Journal of Labor Economics*, 40(S1): S39–S95.

Carneiro, Pedro, Costas Meghir, and Matthias Parey. 2013. "Maternal education, home environments, and the development of children and adolescents." *Journal of the European Economic Association*, 11(suppl_1): 123–160.

Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer. 2019. "The effect of minimum wages on low-wage jobs." *The Quarterly Journal of Economics*, 134(3): 1405–1454.

Chevalier, Arnaud, Colm Harmon, Vincent O'Sullivan, and Ian Walker. 2013. "The impact of parental income and education on the schooling of their children." *IZA Journal of Labor Economics*, 2(8).

Clay, Karen, Jeff Lingwall, and Melvin Stephens Jr. 2021. "Laws, educational outcomes, and returns to schooling evidence from the first wave of US state compulsory attendance laws." *Labour Economics*, 68: 101935.

Currie, Janet, and Enrico Moretti. 2003. "Mother's Education and the Intergenerational Transmission of Human Capital: Evidence from College Openings." *The Quarterly Journal of Economics*, 118(4): 1495—1532.

De Chaisemartin, Clément, and Xavier d'Haultfoeuille. 2020. "Two-way fixed effects estimators with heterogeneous treatment effects." *American Economic Review*, 110(9): 2964–96.

Dickson, Matt, Paul Gregg, and Harriet Robinson. 2016. "Early, Late or Never? When does Parental Education Impact Child Outcomes?" *The Economic Journal*, 126: F184—F231.

Ferrie, Joseph P. 2005. "History Lessons: The End of American Exceptionalism? Mobility in the United States since 1850." *The Journal of Economic Perspectives*, 19(3): 199–215.

Gelbach, Jonah B. 2016. "When do covariates matter? And which ones, and how much?" *Journal of Labor Economics*, 34(2): 509–543.

Goldin, Claudia, and Lawrence F Katz. 2008. "Mass secondary schooling and the state: the role of state compulsion in the high school movement." In *Understanding long-run economic growth: Geography, institutions, and the knowledge economy.* 275–310. University of Chicago Press.

Goldin, Claudia, and Lawrence F Katz. 2011. *9. Mass Secondary Schooling and the State: The Role of State Compulsion in the High School Movement.* University of Chicago Press.

Goodman-Bacon, Andrew. 2021. "Difference-in-differences with variation in treatment timing." *Journal of Econometrics*, 225(2): 254–277.

Holmlund, Helena, Mikael Lindahl, and Erik Plug. 2011. "The causal effect of parents' schooling on children's schooling: A comparison of estimation methods." *Journal of economic literature*, 49(3): 615–51.

Katz, Michael S. 1976. *A History of Compulsory Education Laws. Fastback Series, No. 75. Bicentennial Series.* ERIC.

Lively, Charles Elson, and Conrad Taeuber. 1939. *Rural migration in the United States.* Works Progress Administration, Division of Research.

Lleras-Muney, Adriana. 2002. "Were compulsory attendance and child labor laws effective? An analysis from 1915 to 1939." *The Journal of Law and Economics*, 45(2): 401–435.

Long, Jason, and Joseph Ferrie. 2007. "The Path to Convergence: Intergenerational Occupational Mobility in Britain and the US in Three Eras." *The Economic Journal*, 117(519): C61–C71.

Long, Jason, and Joseph Ferrie. 2013. "Intergenerational Occupational Mobility in Great Britain and the United States Since 1850." *The American Economic Review*, 103(4): 1109–1137.

Oreopoulos, Philip, Marianne E Page, and Ann Huff Stevens. 2006. "The intergenerational effects of compulsory schooling." *Journal of Labor Economics*, 24(4): 729–760.

Piopiunik, Marc. 2014. "Intergenerational Transmission of Education and Mediating Channels: Evidence from a Compulsory Schooling Reform in Germany." *The Scandinavian Journal of Economics*, 116(3): 878—907.

Rauscher, Emily. 2016. "Does Educational Equality Increase Mobility? Exploiting Nineteenth-Century U.S. Compulsory Schooling Laws." *American Journal of Sociology*, 121(6): 1697–1761.

Ruggles, Steven Catherine A Fitch, Ronald Goeken, J David Hacker, Matt A Nelson, Evan Roberts, Megan Schouweiler, and Matthew Sobek. 2021. "IPUMS Ancestry Full Count Data: Version 3.0 [dataset]."

Ruggles, Steven, Catherine Fitch, Ron Goeken, J David Hacker, Jonas Helgertz, Evan Roberts, Matt Sobek, Kelly Thompson, John Robert Warren, and Jacob Wellington. 2019. "IPUMS Multigenerational Longitudinal Panel."

Sikhova, Aiday. 2023. "Understanding the Effect of Parental Education and Financial Resources on the Intergenerational Transmission of Income." *Journal of Labor Economics*, 41(3): 771–811.

Stephens Jr, Melvin, and Dou-Yan Yang. 2014. "Compulsory education and the benefits of schooling." *American Economic Review*, 104(6): 1777–92.

Sun, Liyang, and Sarah Abraham. 2021. "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects." *Journal of Econometrics*, 225(2): 175–199.