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

Titus Galama†
Andrei Munteanu‡
Kevin Thom§

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Abstract

We estimate the intergenerational effects of compulsory schooling (CS) laws in the United States (1880 to 1940), exploiting the staggered roll-out of state CS laws and using a panel of linked full-count censuses. We find that *parental* exposure to CS expansion had significant positive effects on *child* outcomes, including educational attainment and later-life labor market outcomes. Intergenerational gains were largely concentrated among whites. Within families, parental exposure to CS had the greatest effects on the eldest and least educated children. We identify several mechanisms that plausibly account for persistence in these effects, including i) parental labor market outcomes (e.g. mothers attaining more education and higher income occupations), ii) assortative mating, iii) geographic mobility to neighborhoods with better human capital-related resources.

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

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†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 (UQAM).
§Department of Economics, University of Iowa.

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 in the U.S. expanded compulsory schooling (CS) requirements and enacted other educational reforms. 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.[1] These studies almost exclusively focus on the schooling and later-life outcomes of individuals directly affected by the reforms. However, the long-run consequences of these policies depend crucially on the extent to which their effects *persist or even compound across generations*. Reforms will most successfully lift people out of poverty and promote economic growth when they generate lasting effects, not only for directly exposed individuals, 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.[2] Even less is known about the channels through which these effects persist. 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. To do this, we create an intergenerational dataset using full-count decennial censuses spanning 1880-1940.[3] This allows us to observe individuals born in the late nineteenth and early twentieth centuries, locate their children in subsequent census waves, and observe the educational and labor market outcomes of these children when they are adults in 1940.[4] We exploit the staggered implementation of state CS laws to estimate intergenerational effects on education and labor market outcomes using a difference-in-differences framework. The remarkable size and richness of the linked census data permit two contributions to the literature. First, we estimate large in-

[1] See, for example, Clay et al. (2021), Goldin and Katz (2008), Lleras-Muney (2002), Stephens Jr and Yang (2014), among others.

[2] As we discuss later, several papers do examine the intergenerational effects of CS and other schooling reforms on the economic outcomes of children still living with their parents (Carneiro et al., 2013, Chevalier et al., 2013, Cornelissen and Dang, 2022, Currie and Moretti, 2003, Dickson et al., 2016, Oreopoulos et al., 2006, Piopiunik, 2014). In general this linkage strategy limits the range of outcomes that can be observed and shapes sample selection in a particular way. The papers most similar to our work are Black et al. (2005) and Sikhova (2023), which estimate intergenerational effects of CS on children's completed education in Norway and children's income in Sweden, respectively. Beyond CS-related reforms, studies have examined intergenerational effects of early childhood education (Barr and Gibbs, 2022), father's employment displacement (Oreopoulos et al., 2008), parental Medicaid exposure (East et al., 2023), parental immigration status (Cascio et al., 2024), and in-utero radiation exposure (Black et al., 2019).

[3] We link individuals across decades using matches identified by Census Tree (Buckles et al. 2023a, Price et al. 2021), which provides a higher matching rate (particularly for women) than previous crosswalks. These linked records let us work backwards from 1940 and find a census wave where individuals are observed residing with their parents. Such records reveal parental state and year of birth – the characteristics that determine CS exposure.

[4] The 1940 Census was the first to record educational attainment.

tergenerational effects of parental exposure to CS on offspring's completed adult educational attainment, wages, and occupational characteristics. Second, we explore possible mechanisms driving these intergenerational effects.

Our first contribution is to estimate the intergenerational impact of compulsory schooling (CS) reforms. We find that parental exposure to CS significantly increased the completed education of their children. In our preferred specification, maternal exposure to any nonzero level of compulsory schooling increased children's schooling by 0.082 years, while paternal exposure had an effect of 0.072 years. When we treat exposure as a continuous variable, each additional year of maternal and paternal CS exposure raised children's schooling by 0.020 and 0.017 years, respectively. Higher parental CS exposure—concentrated among families at the lower end of the parental education distribution—led to higher rates of middle school, high school, and college completion among children, suggesting a snowballing of CS effects across generations. We also find significant impacts on children's adult labor market outcomes. Maternal exposure to any CS requirement increased sons' earnings by about 1 percent. Maternal CS exposure also affected the occupational choices of both sons and daughters, shifting children to occupations associated with more education on average.

While the reduced-form intention to treat (ITT) effects of parental CS exposure may appear modest, the implied average treatment effects on the treated (ATTs) are large given the small share of parents whose schooling was directly altered by the reforms. To help interpret our main results, we use parental CS exposure as an instrumental variable for parental education to gauge the scale of the underlying causal effect, acknowledging that the exclusion restriction may not strictly hold. This exercise suggests the intergenerational effect of parental education on children's schooling is substantial, potentially as large as 1 year per year of parental schooling. Such results are consistent with the idea that educational reforms can generate long-lasting dynastic effects.

We also find that intergenerational gains were not evenly distributed across racial groups. Consistent with prior historical work (e.g., Margo, 1990, Rauscher, 2014), we find little evidence that CS exposure improved outcomes for Black Americans. The enforcement of compulsory schooling laws was often less stringent for Black students (Lleras-Muney, 2002). Moreover, the persistence of school segregation, lower teacher pay, and lower school investment likely reduced returns to schooling in predominantly Black communities (Baran et al., 2024, Cascio and Lewis, 2024). These factors could have constrained the potential benefits of compulsory schooling reforms for Black children, attenuating both direct and intergenerational effects.

Our main results hold across a series of alternate specifications and robustness checks. Alternate specifications include (i) a "stacked" event study estimator (Cengiz et al., 2019) addressing concerns related to the two-way fixed effects (TWFE) design and (ii) models that include an enriched series of time-varying controls. Across a series of robustness checks, we address threats to identification arising from: (i) the timing of CS laws relative to other educational reforms and demographic shifts; (ii) selection into cohabitation of parents and children; (iii) sample selection due to differential mortality by parental education; and (iv) mismatched census linkages.

Our second contribution is to explore the mechanisms through which these intergenerational effects operate, and whether they had heterogeneous effects across children. This task is facilitated by the richness of the census data and the large sample sizes they afford.

Household resources, such as money and time, are obvious potential mechanisms (Becker and Tomes, 1979, 1986). Thus, we first explore a series of *parental* labor market outcomes that may be linked to the persistence of schooling across generations. For mothers, we find that higher CS exposure *reduces* labor force participation (potentially freeing time for child investment), but *increases* wages and changes occupational choice among those who do work. We find that both mothers and fathers exposed to CS during their youth are more likely to sort into occupations requiring higher levels of education, though not necessarily associated with higher average incomes. For example, CS exposure increased the likelihood that mothers worked as teachers. We speculate that work in such occupations could have positively affected human capital investment in children at home, either by improving teaching skills or by shifting priorities toward education.

A second set of mechanisms relates to marriage and family formation. We find no evidence that CS affects the number of children born to a mother, or induces a quantity-quality trade-off. However, we do find that it delays the average age at marriage, which could affect child development if maternal age is a factor in skill formation. For both mothers and fathers, greater exposure to CS increases the average educational attainment of their spouses, consistent with positive assortative mating on education. Greater CS exposure can thus affect child outcomes not only through direct effects on the exposed parent's outcomes (like their own occupation), but also through the characteristics of the child's other parent.

The fine geographic detail in the censuses allow us to also study geographic sorting as a mechanism. We use linkages between the 1930 and 1940 censuses to measure parents' neighborhood sorting behaviors when most of the children in our sample were of school age (ages 5 to 14) and parents were of prime working age. Parents with more exposure to CS are more likely to gravitate towards neighborhoods with higher teacher-student ratios (i.e. school resources), higher literacy rates, higher labor force participation and employment rates, and more neighbors working in occupations associated with higher levels of schooling and earnings.

Finally, we test whether parental CS exposure had heterogeneous intergenerational effects across different kinds of children (differentiated by birth order and gender). This offers some clues as to whether parental CS exposure shifted household resources and preferences in a way that might have targeted or disproportionately benefited specific children. We estimate a series of dynastic models in which the parent is the unit of observation and the outcomes are features of the distribution of education among their children. Our main results remain robust in these models, which also reveal gender and birth-order heterogeneity in the intergenerational transmission of human capital. Parental CS exposure disproportionately benefited first-born sons and daughters, and raised the education of the least-educated child within the family in particular. This pattern suggests that CS reforms not only increased average educational attainment across generations but also reduced inequality within families, consistent with a reallocation of parental resources toward ensuring a minimum educational standard for all children.

To the best of our knowledge, we provide the first evidence from the United States on the *intergenerational* effects of CS reforms on the *completed educational attainment and adult labor market outcomes* of offspring.[5] By contrast, most of the literature estimates inter-

[5]We focus our discussion of the literature on papers that estimate the effects of policy reforms. A larger

generational effects of reforms on *early educational and health outcomes* of children during childhood. Close to our work, Oreopoulos et al. (2006) estimate that parental exposure to U.S. CS laws reduced the probability that a child was held back a year in school. Given the centrality of completed education and downstream labor market outcomes for lifetime welfare and economic success, our results complement this work and offer a distinct contribution. Elsewhere, Currie and Moretti (2003) find that mothers in the U.S. with more geographic access to colleges were more likely to have children with better infant health outcomes, such as birth weight and gestational age. Using NLSY data, Carneiro et al. (2013) find positive effects of maternal education on childhood cognitive performance and behavioral outcomes.

A number of papers estimate the intergenerational effects of education reforms in other, mostly European, contexts (Black et al., 2005, Chevalier et al., 2013, Cornelissen and Dang, 2022, Dickson et al., 2016, Holmlund et al., 2011, Piopiunik, 2014, Sikhova, 2023). These studies often rely on the co-residence of parents and children, limiting the range of outcomes to those that can be observed to those of children and young adults. For example, educational reforms affecting parents have been shown to increase the test scores of teenagers (Dickson et al., 2016) and the propensity to acquire post-compulsory schooling among co-resident children (Chevalier et al., 2013). By contrast, the linked census data allow us to estimate the effects of parental exposure not only on the completed educational attainment of the offspring, but also their adult labor market outcomes. Closest to our work is Black et al. (2005), who study a change in Norwegian CS from seventh to ninth grade rolled out gradually during the 1960s and early 1970s. They estimate an effect of at most 0.18 years (for one additional year of mother's education on sons) and interpret the small estimated effects as evidence that selection accounts for much of the intergenerational correlation in education. By contrast, our estimates suggest significantly larger intergenerational effects in the U.S. context. Sikhova (2023) offers a rare example of a study looking at the intergenerational effects of policy on adult labor market outcomes. 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.

Beyond compulsory schooling, we contribute to the broader literature on the intergenerational spillovers of interventions and environmental conditions. Barr and Gibbs (2022) find that increased maternal access to early childhood education (Head Start) improved a variety of child outcomes including educational attainment, teen pregnancy, and criminality. Examining plant closures, Oreopoulos et al. (2008) estimate negative effects of job displacement among fathers on the subsequent incomes of their children. Several studies find evidence that children's health is impacted by parental exposures including parental access to Medicaid (East et al., 2023), parental legal immigration status (Cascio et al., 2024), and parental exposure to radiation in-utero (Black et al., 2019).

Our results also relate to an established literature documenting factors that shape intergenerational mobility in the United States and other contexts. Several studies examine this by estimating whether schooling reforms had larger or smaller direct effects on individuals from different socioeconomic backgrounds. However, such studies do not provide evidence on the durability of these effects across subsequent generations. Directly related to our work,

literature, discussed in Holmlund et al. (2011), estimates causal effects of parental education on child outcomes using twin study or adoption designs.

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 et al. (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. Our approach, like that of Black et al. (2005), is distinct in that we explore the causal effect of reforms affecting parents on the outcomes of their children.

Our main results, and the analysis of mechanisms in particular, add to the larger discussion on whether the degree of intergenerational mobility in the U.S. today has changed versus the past and whether it is different from that in other contexts (Long and Ferrie, 2007, 2013). For example, Ferrie (2005) concludes that the US was occupationally and geographically more mobile than Britain in the mid-nineteenth century, but that this mobility advantage declined in the early part of the twentieth century. Long and Ferrie (2013) suggest that residential mobility offers a compelling explanation for this, since cross-county mobility in the U.S. during the late nineteenth century was substantially greater than comparable mobility in the U.K., or in the later twentieth-century U.S. context. Our results on mechanisms are consistent with geographic and neighborhood sorting playing a role in transmitting the effects of CS across generations. Separately, Ward (2023) highlights the need to understand the distinct processes shaping mobility across racial groups. Ward (2023) argues that conventional estimates of mobility in the United States in the late nineteenth and early twentieth centuries may be biased by excluding data on Black Americans, who experienced high intergenerational persistence. We find particularly low intergenerational returns to CS among Black Americans over the course of our sample, suggesting that CS policies may have been implemented unequally across different regions and groups.

2 Setting, Institutional Background and Data

2.1 Compulsory Schooling Laws

Individuals born during the late nineteenth and early twentieth centuries in the United States experienced a wave of legislative reforms regulating children's school attendance. Most states required school attendance between certain ages, while others specified a minimum age for obtaining a work permit, given the prevalence of child labor at the time. Some laws further mandated attendance in continuation or night schools until a prescribed age. Using the methodology and data from Clay et al. (2012),[6] we construct a measure of the number of years of legally required school attendance for each state–birth-year cohort (see Appendix C). This approach accounts for the staggered rollout of compulsory schooling laws across states and over time, allowing us to assign to each parent in our sample the years of compulsory schooling implied by their state and year of birth.

Figure 1 illustrates the staggered roll-out of compulsory schooling (CS) laws. States in New England and parts of the Upper Midwest were early adopters, while Southern states

[6]Building on Acemoglu and Angrist (2000), Lleras-Muney (2002), Goldin and Katz (2008), and Stephens Jr and Yang (2014), among others.

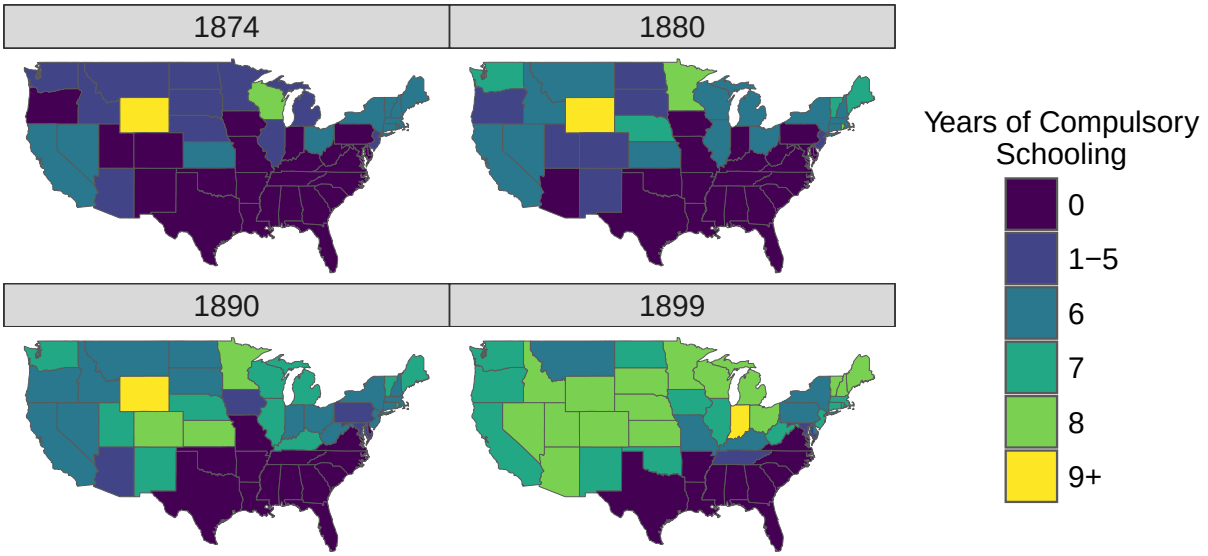

Figure 1: Compulsory schooling law exposure by state, for the 1874, 1880, 1890, and 1899 birth-year cohorts.

lagged considerably. By the time cohorts born in 1899 entered school, nearly all states outside the South required at least six years of schooling.

2.2 Building an Intergenerational Dataset

To construct the intergenerational dataset, we begin with adults aged 25–50 in the full-count 1940 Census (Ruggles et al., 2024) and identify their parents when possible. The 1940 Census is the first to report educational attainment for all adults aged 25 and older,[7] and the first to cover a generation whose parents were exposed to the late-nineteenth and early-twentieth-century wave of compulsory schooling (CS) reforms.

We link these 1940 adults to parents either through cohabitation in 1940 or by tracing them back to earlier full-count censuses (1880–1930) using the Census Tree database (Buckles et al., 2023a, Price et al., 2021). Census Tree integrates linkages from the IPUMS Multigenerational Longitudinal Panel (MLP) (Helgertz et al., 2023) and Census Linking Project (CLP) (Abramitzky et al., 2020), along with additional genealogical data and a machine-learning algorithm, yielding higher match rates—particularly for women and Black Americans. We use this feature to assess how linkage methodology influences our results and perform robustness checks restricted to records confirmed by multiple linkage sources.

[7]The 1950 Census reports education only for a subsample, while earlier censuses include literacy but not years of schooling.

Parents' state and year of birth determine their CS exposure.[8] We restrict the sample to U.S.-born individuals linked to at least one U.S.-born parent and enforce consistency checks on age, state, and birth year. The final dataset includes 10.6 million adults in 1940 successfully linked to at least one parent.

Table 1: Summary Statistics and Representativeness of Sample

	Children		Mothers		Fathers	
	Sample	Universe	Sample	Universe	Sample	Universe
Observations	10,599,296	44,262,610	4,690,568	14,382,747	3,392,081	14,432,783
Completed Schooling (Years)	9.8	9.5	7.8	8.3	7.6	7.9
Proportion Black	6.8%	10.6%	7.8%	9.0%	7.0%	9.2%
Proportion Female	40%	51%	100%	100%	0%	0%
Proportion Urban	51%	58%	50%	56%	46%	51%
Proportion Married	71%	74%	74%	69%	89%	80%
Age	31.7	31.1	55.2	53.9	56.8	55.6
Labor Force Participation Rate	71%	63%	13%	21%	88%	86%
Unemployment Rate	6.8%	7.1%	7.4%	6.9%	6.4%	7.2%
Unemployment Duration (Weeks)	66	66	78	84	97	100
Yearly Labor Earnings ($)	1,094	1,042	585	803	1,341	1,385
Weekly Hours Worked	40	39	33	34	40	40
Compulsory Schooling (Years)	-	-	3.1	3.5	2.9	3.3

Summary statistics for the matched *Parents* and *Children* samples, compared to individuals of similar ages in the 1940 Census.

Although the final sample is not fully representative—owing to non-random cohabitation, linkage challenges (especially for women), and selective mortality—it closely resembles the broader population of similarly aged individuals in geography, income, and rural status (Table 1).[9] Full details on linkage procedures, sample restrictions, and representativeness appear in Appendix A.1.

2.3 Parental CS Exposure and Completed Schooling Across Generations

Before turning to the main econometric analysis, we provide some descriptive results relating parental CS exposure to both parents' and children's outcomes. The policies we study affected both the extensive and intensive margins of CS. Some states introduced new mandates, while others expanded existing ones. To visualize the raw patterns in the data, we begin with a binary measure of exposure to any CS requirement. Let E_j represent cohort time since parent j's state of birth was exposed to any CS. For parent j, born in state s' and year y', define $E_j = y' - y^0_{s'} + 1$, where $y^0_{s'}$ is the birth year of the first cohort in birth state s' subject to any CS law. $E_j = 0$ for the last unexposed cohort, and cohorts with $E_j > 0$ were all at least partially exposed to some nonzero CS.

Figure 2 presents event-study estimates relating CS exposure to educational attainment for parents (top panels) and their children (bottom panels). Parents exposed to CS completed substantially more schooling than the last unexposed cohort—roughly 0.30–0.50 years

[8] We relax this assumption in a later section, where we test if across-state migration impacts our results.

[9] We discuss how selection into cohabitation, migration, or other sample selection issues affect our results, in Section 5.

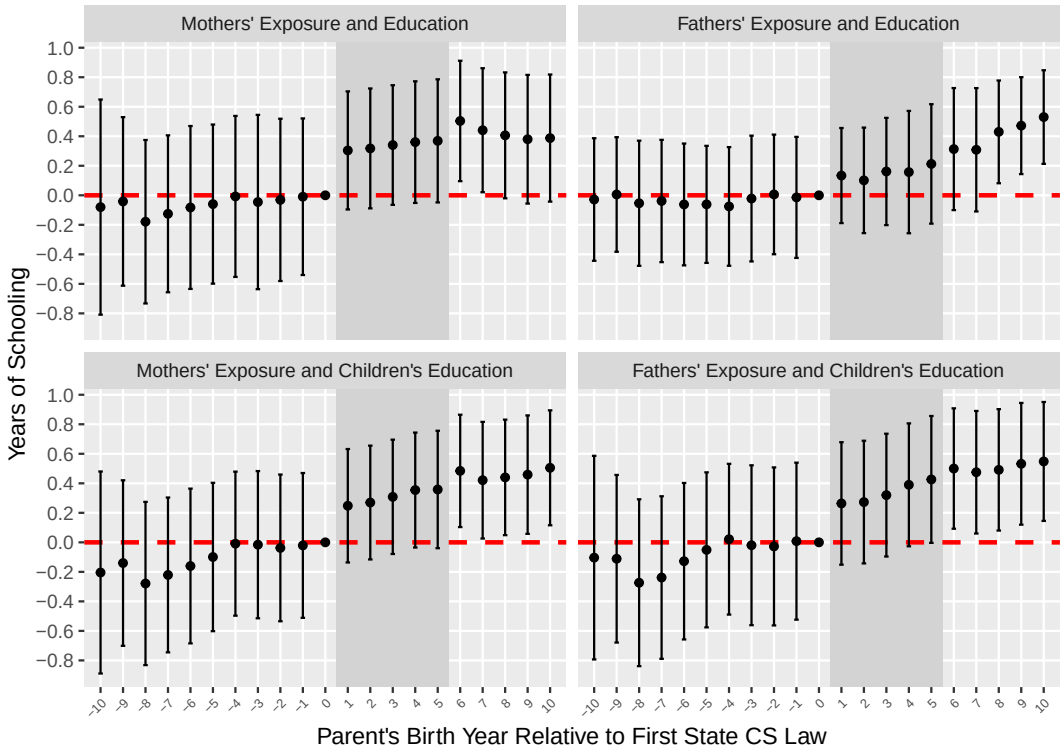

Figure 2: Event study of Parents' CS exposure and Parents' and Children's completed years of schooling. The dark gray area represents partial exposure to compulsory schooling (1–5 years), corresponding to cohorts who were already above the typical first-grade entry age when primary school—usually six years—became mandatory.

for mothers and 0.10–0.53 years for fathers.[10] The bottom panels show a clear jump in children's schooling among those whose parents were exposed to any CS law, with increases of about 0.25–0.55 years relative to children born to parents in unexposed cohorts immediately preceding them. These descriptive patterns motivate our main difference-in-differences analysis presented in the next section.

3 Empirical Strategy

Our main specifications are two-way fixed effects models that control for parental state and year of birth. In some models, we take the parent j as the primary unit of analysis and estimate the effects of parental exposure to CS on their own outcomes and on summary measures of the outcomes of their children. In others, we take the child i as the primary unit of analysis and estimate the effects of their parent's exposure to CS on individual child outcomes.

For our parent-level specifications, we relate parental outcome Y_j^p for parent j to the CS exposure ($CS^p_{s'y'}$) legally required of their birth state (s') birth year (y') cohort. In most

[10]Standard errors are clustered at the birth-state–birth-year level. The muted effects for partially exposed cohorts reflect that many had already reached school age or exited schooling before enforcement.

of our specifications, $CS^p_{s'y'}$ measures CS exposure in years of required schooling. In other specifications, we take $CS^p_{s'y'}$ to be a binary CS treatment indicating exposure to any required schooling. Parent-level outcomes Y^p_j include own (parental) years of completed education, $Educ^p_j$ as well as summary measures of the educational attainment of their children (e.g. average education among their children, $AvgEduc^{child}_j$). Our basic parent-level specification is:

$$Y^p_j = \beta^p CS^p_{s'y'} + \gamma^p_{s'} + \delta^p_{y'} + \left(\eta^p_{r'} \times \theta^p_{y'}\right) + \lambda^p Race^p_j + \mu^p Sex^p_j + \epsilon^p_j, \quad p = m, f \tag{1}$$

where we include vectors of fixed effects for j'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 j's region (r') of birth ($\eta^p_{r'}$)[11] and birth year (y') cohort ($\theta^p_{y'}$), as well as controls for parent j'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 year effects (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, $\delta^p_{y'}$). We estimate this specification using the entire *Parents* sample, as well as separate specifications for mothers and fathers.

Our primary child-level specification relates a child i's outcome, Y^c_i to the CS exposure ($CS^p_{s'y'}$) of the child's parent $p = m, f$:

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

Here we include all parental birth state / region and birth year fixed effects from Equation 1, and add a set of child-level controls: 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). These analyses use the *Children* sample and estimate effects separately for the CS exposure of mothers (m) and fathers (f). We cluster the standard errors at the child birth-state by child birth-year level.

Parental and child CS exposure are naturally correlated, since many children are born in the same state as their parents and face similar laws. The full set of child state-by-year interactions absorbs the effect of the child's own CS exposure,[12] so β^c is identified by comparing children born in the same state and year whose parents were exposed to different CS regimes. This variation arises when: (i) parents were born in the same state but different cohorts; (ii) in the same cohort but different states; or (iii) both state and cohort differ.

Our main specification addresses two key identification challenges. First, CS laws are highly persistent, so parental exposure could proxy for the child's own exposure. Controlling for child state-by-year interactions mitigates this concern. Second, as emphasized by Stephens Jr and Yang (2014), common-trend assumptions across states are often violated. Controlling for region-by-cohort interactions in parents' birth characteristics helps absorb differential regional trends in education and economic development that might otherwise be attributed to CS laws.

[11] East, South, Midwest and West.

[12] As a robustness check, we also estimate a specification without child-level controls, since these may be endogenous.

We take the two-way fixed effects model in Equation 2 to be our main specification. However, we also find it useful to present results from two alternate specifications addressing specific identification concerns. First, we estimate models adopting the "stacked difference-in-differences" method outlined by Cengiz et al. (2019). This method, described in further detail in Appendix Section B.1, can address biases that may emerge in the presence of treatment effect heterogeneity and the staggered adoption of a treatment across units. Second, we consider an alternate version of our main Equation 2 that includes a series of extra time varying controls, including i) the ratio of foreign-born children, ii) the ratio of second generation immigrant children, iii) teachers per 1,000 students, iv) literacy rates, v) school attendance rates, and vi) child labor rates. Results from both sets of alternate specifications are discussed in Section 5.

4 Results

Here we present our main findings, using the TWFE specification. We first confirm that CS exposure affected parental educational attainment, before showing the intergenerational effects of parental exposure to CS on children's educational and labor market outcomes. While we focus here on presenting and interpreting our main findings, in Section 5 we return to different specifications and conduct a series of robustness checks to assess the sensitivity of our findings to alternative samples, and estimators, including the stacked event study estimator. This will clarify what drives - and does not drive - our results. Section 6 is dedicated to exploring the behavioral mechanisms through which the effects of CS propagate across generations.

4.1 Effect of Parental CS Exposure on Parental Education

Table 2 presents estimates of the direct effects of CS reforms on the years of schooling of parents subject to these laws (Equation 1). We estimate separate specifications for mothers (top panel) and fathers (bottom panel). Column (1) reports that exposure to any CS (binary treatment) had an effect of 0.078 years for women and 0.032 years for men. Column (2) reports estimates from models where CS exposure is measured in years (continuous treatment). We find that each additional year of CS exposure had an effect of 0.013 per year for women and 0.007 per year for men. Subsequent columns estimate the continuous model across key subsamples. Effects are concentrated among whites, with no significant effects for Black parents—though standard errors are large. Regionally, CS laws had the strongest impact in the East and West, with no effects in the Midwest. We do not estimate separate effects for the South, given the lack of any compulsory schooling laws in those states affecting our Parental cohorts (with the exception of Tennessee).

These parental own-effect estimates align with prior research, which found that compulsory schooling (CS) had greater impacts on schooling for white women than for white men, and found no effects for Black Americans. This literature also shows that more recent CS laws were more effective than earlier ones.[13] Our estimates are consistent with these findings.

[13]For example, Clay et al. (2021) estimate 0.008–0.027 years of schooling per CS year for men born 1885–1912 in the 1940 Census. In appendix A1, we estimate our main specification and the ones of Clay et al. (2021) on our parents sample and on a sample similar to the one used by those authors. Our results are very

Table 2: Effect of Parental Compulsory Schooling Exposure on Parental Completed Years of Schooling

	Dependent variable: Parent's Years of Schooling						
	All (Binary Treat.)	All (Cont. Treat.)	White (Cont.)	Black (Cont.)	East (Cont.)	West (Cont.)	Midwest (Cont.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CS Years (Women)	0.078***	0.013***	0.011***	0.007	0.013***	0.071***	0.006
	(0.0124)	(0.0029)	(0.0031)	(0.0128)	(0.0036)	(0.0116)	(0.0040)
N (millions)	4.3	4.3	3.9	0.3	1.0	0.2	1.9
R^2	0.12	0.12	0.05	0.10	0.05	0.18	0.04
Outcome Means	7.8	7.8	8.0	4.8	8.3	8.5	8.2
CS Years (Men)	0.032**	0.007**	0.007*	0.002	0.022***	0.057***	-0.006
	(0.0149)	(0.0035)	(0.0037)	(0.0152)	(0.0045)	(0.0130)	(0.0047)
N (millions)	3.0	3.0	2.8	0.2	0.7	0.1	1.3
R^2	0.12	0.12	0.06	0.07	0.04	0.16	0.04
Outcome Means	7.7	7.7	7.9	4.5	8.4	8.4	8.0

Notes: Effects of exposure to CS laws on completed years of schooling for the *Parents* sample by race and census region. Each column represents a different regression. The "Binary Treatment" specification in Column (1) measures CS exposure using an indicator for exposure to any CS. The "Continuous Treatment" specification in Column (2-7) measures CS exposure as the number of years of required schooling. Controls include birth year effects, birth state effects, birth region by birth year effects, and 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.

Using a younger sample of 1940 parents aged 25–54, as in Clay et al. (2021), we find larger effects from exposure to more recent CS laws.[14]

The estimated effects in Table 2 may appear modest at first glance, but there are two key reasons why these average treatment effects (ATEs) understate the full impact of compulsory schooling (CS) laws. First, many individuals in our sample were never actually affected by the CS reforms. The laws were not binding for a large portion of the population—particularly those who already stayed in school beyond the new minimum. As we show in Appendix Table A3, only about 12–14% of children were leaving school earlier than the newly mandated minimum at the time the laws were passed. These reforms were therefore targeted at a relatively small, low-achieving subgroup.

Second, we must consider the imperfect enforcement of CS laws. Even where laws were updated, implementation varied widely by state and time, and full compliance was not guar-

similar. Lleras-Muney and Shertzer (2015), using cohorts born 1901–1925 and the 1960 Census, find gains of 0.046 and 0.062 years for white men and women, respectively, and none for Black Americans. Oreopoulos et al. (2006) estimate 0.04–0.06 years per CS year among parents cohabiting with children in the 1960–1980 censuses, exposed to more recent laws and without regional trend controls.

[14]These results are presented in Appendix Table A40, and discussed further in Section 5.

anteed. To benchmark the effectiveness of these reforms, we simulate a "perfect enforcement" scenario in which every individual stays in school up to the legally required minimum in their state and cohort. We then re-estimate the impact of CS exposure on these counterfactual schooling outcomes (see Equation 1).

As shown in Appendix Table A3, under perfect enforcement, each additional year of CS exposure would have raised schooling by 0.10 years for women and 0.12 years for men. However, actual observed gains were much lower—only 13% of this potential for women and just 4% for men, which showcases the gap between legislation and its implementation. These averages conceal important variation by race and region. Black women and men captured only 3% and 0.5% of the potential gains, respectively, consistent with systemic racial inequities in educational provision. Women in the Eastern and Western U.S. achieved 23% and 31% of the potential gains, respectively; men in these regions gained 28% and 21%, respectively. This underscores how state-level differences in policy execution shaped the effectiveness of CS reforms. Taken together, these findings help contextualize the modest average effects in Table 2 and underscore the importance of both targeting and enforcement when evaluating education policy impacts.

4.2 Effect of Parental CS Exposure on Children's Education

We now turn to our main results on the intergenerational effects of parental CS exposure (Equation 2), which are presented in Table 3. The top and bottom panels contain separate estimates for the effect of mother's and father's exposure, respectively. Column (1) presents estimates where the treatment variable $CS^{p}_{s'y'}$ is a dummy variable for parental exposure to any non-zero compulsory schooling. Here we find substantial effects, with maternal (paternal) exposure to any compulsory schooling increasing child education by 0.082 (0.072) years. Column (2) presents estimates of a model where the treatment variable $CS^{p}_{s'y'}$ is the number of years of compulsory schooling required of parents. Again both maternal and paternal CS exposure produce intergenerational gains, with one additional year of CS exposure for mothers (fathers) increasing schooling for children by 0.020 (0.017) years. Columns (3)-(6) present estimates of the Column (2) specification for different subgroups. Male children seem to benefit slightly more than female children, and maternal education is a more important channel of transmission than paternal education.

For Black children, we fail to find evidence of significant positive effects of either maternal or paternal exposure to CS laws. In fact, our point estimates suggest a negative effect of maternal CS exposure on Black children's completed education. This is consistent with evidence that early-20th-century CS laws often had limited bite in segregated, under-resourced Black schools and were unevenly enforced.

The magnitudes of the effects presented in Table 3 are difficult to interpret given the the fact that not all parents were directly affected by CS reforms facing their cohorts. To get a better sense of the size of the intergenerational effects implied by our main results, we consider an instrumental variables specification with children's years of schooling as the outcome of interest, parental years of schooling as an endogenous regressor, and parental CS exposure as an instrument for parental schooling. Appendix Section B.2 provides details about our IV models. We do this primarily to assess the scale of the intergenerational linkages consistent with our main results. These models also allow us to compare our results to those

Table 3: Effect of Parental Compulsory Schooling Exposure on Children's Completed Years of Schooling

	Dependent variable: Child's Years of Schooling					
	All (Binary Treat.)	All (Cont. Treat.)	Men (Cont.)	Women (Cont.)	White (Cont.)	Black (Cont.)
	(1)	(2)	(3)	(4)	(5)	(6)
CS Years (Mom)	0.082***	0.020***	0.023***	0.017***	0.019***	-0.026**
	(0.0172)	(0.0040)	(0.0042)	(0.0042)	(0.0043)	(0.0128)
N (millions)	7.8	7.8	4.6	3.2	7.3	0.4
R^2	0.14	0.14	0.14	0.12	0.09	0.15
Outcome Means	10.0	10.0	9.8	10.3	10.2	7.0
CS Years (Dad)	0.072***	0.017***	0.018***	0.016***	0.016***	0.021***
	(0.0168)	(0.0038)	(0.0042)	(0.0042)	(0.0042)	(0.0168)
N (millions)	5.2	5.2	3.1	2.1	4.9	0.3
R^2	0.14	0.14	0.14	0.11	0.10	0.16
Outcome Means	10.1	10.1	9.9	10.4	10.2	7.2

Notes: Effects of parental exposure to CS laws on completed years of schooling for the *Children*, by sex and race. Each column represents a different regression. The "Binary Treatment" specification in Column (1) measures CS exposure using an indicator for any non-zero CS. The "Continuous Treatment" specification in Columns (2-7) measures CS exposure as the number of years of required schooling. Controls include child birth year effects, child birth state effects, child birth state by birth year effects, parental birth year effects, parental birth state effects, parental birth region by parental birth year effects, and child 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.

of Black et al. (2005), who adopt this strategy when studying a Norwegian reform. We stress that we consider our reduced-form estimates to be our primary results. There are plausible mechanisms by which parental exposure to compulsory schooling could affect children even if the parents themselves do not change their schooling (e.g. by changing parental norms or expectations). Thus, the exclusion restriction necessary for an IV is not guaranteed to hold.

Tables A5 of the Appendix present the second stage IV estimates.[15] Column (1) replicates the exact instrumental variables (IV) specification used by Black et al. (2005). The other columns show our own specifications with additional controls and for different demographic groups and time periods. Across various specifications and samples, the effects are an order of magnitude larger than those of Black et al. (2005), who found essentially null or weak effects[16] 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

[15]Table A4 presents the first stage results.

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

effects of parental education (at least in that setting). In our main specification in Column (2), we find that every additional year of schooling obtained by mothers (fathers) as a result of CS exposure generates a 0.96-year (1.29-year) increase in children's schooling. However, these results do not hold for Black individuals (Column 6), consistent with the weak first stage results for this group. IV estimates thus suggest sizable intergenerational effects of parental exposure to CS, at least for non-Black Americans.

As mentioned earlier, we caution over-interpreting the IV results given possible violations of the exclusion restriction. However, one possible explanation for the large size of our effects relative to previous studies could be related to degree attainment. Many of the individuals in our Children sample came of age when rates of high school completion were rapidly rising and becoming normalized. In this context, increasing the human capital of parents could have particularly large effects if it induces kids not just to stay in school for an extra bit of time, but to stay long enough to complete the marginal degree program.

To better understand this, we examine binary outcomes for reaching specific milestones, such as completing middle school, high school, or college—rather than focusing solely on years of schooling. Full results for these analyses are presented in Appendix Table A11. We find that CS exposure among parents, especially mothers, significantly increases children's chances of completing middle and higher levels of education. Since high school and college were never directly targeted by CS laws, the evidence points to more complex, indirect mechanisms, such as enhanced resources or changed parental decision-making, that amplify the long-term, intergenerational impact of CS policy.

4.3 Effect of Parental CS Exposure on Children's Labor Market Outcomes

Moving beyond children's educational outcomes, we also examine effects on the adult labor market outcomes of children. Table 4 presents estimates of the intergenerational effects of parental CS exposure on two important adult labor market outcomes for Children measured in the 1940 Census: i) (100) $\times$ log of labor market wages (earnings) and ii) an occupation education score.[17] As before, the top and bottom panels present estimates for the effect of mother's and father's CS exposure, respectively. The first four columns in each panel present estimates for women while the last four present estimates for men.

While maternal CS exposure had little measurable effect on daughters' wage income, it produced statistically and economically meaningful gains for sons: exposure to any CS requirement increased sons' wages by just over 1 log point, and each additional year raised wages by 0.26 log points (around a quarter of a percent). Maternal CS exposure also improved children's occupational status, raising occupational education scores for both sons and daughters. For sons, any exposure increased average occupational rank by 0.27 percentiles (0.07 per additional year); for daughters, by 0.35 and 0.12 percentiles, respectively.

By contrast, we do not find statistically significant effects of paternal CS exposure on the

[17]The wage outcome here refers to the census variable "incwage," which measures the pre-tax wage and salary income of employees from the previous year, excluding income from self-employment and farming. To construct an occupational education score, we calculate the average level of schooling reported by all individuals in a particular occupation in the 1940 Census, separately by sex, and rank the occupations according to this measure. We measure an individual's occupational education score as the percentile of their occupation in the sex-specific occupation-level distribution of education.

Table 4: Effect of Parental Compulsory Schooling Exposure on Children's Labor Market Outcomes

	Dependent variable:							
	Wages (Log - pp)		Occ. Educ. Score		Wages (Log - pp)		Occ. Educ. Score	
	Binary	Cont.	Binary	Cont.	Binary	Cont.	Binary	Cont.
	Women:				*Men*:			
CS Years (Mom)	0.09	0.08	0.35**	0.12***	1.07***	0.26***	0.27*	0.07**
	(0.734)	(0.161)	(0.177)	(0.041)	(0.412)	(0.091)	(0.144)	(0.034)
N (millions)	1.1	1.1	3.2	3.2	3.4	3.4	4.6	4.6
R^2	0.12	0.12	0.05	0.05	0.12	0.12	0.07	0.07
Outcome Means	631.6	631.6	24.0	24.0	672.1	672.1	36.5	36.5
	Women:				*Men*:			
CS Years (Dad)	-1.39*	-0.04	0.15	0.07*	0.73*	0.19*	-0.01	-0.01
	(0.822)	(0.190)	(0.175)	(0.038)	(0.440)	(0.104)	(0.152)	(0.036)
	(0.8219)	(0.1900)	(0.1750)	(0.0379)	(0.4401)	(0.1044)	(0.1519)	(0.0363)
N (millions)	0.7	0.7	2.1	2.1	2.3	2.3	3.1	3.1
R^2	0.10	0.10	0.05	0.05	0.12	0.12	0.07	0.07
Outcome Means	630.1	630.1	23.1	23.1	670.6	670.6	36.3	36.3

Notes: Effects of parental exposure to CS laws on labor market outcomes for the *Children*, by sex. Each column represents a different regression. The "Binary Treatment" specification in Column (1) measures CS exposure using an indicator for any non-zero CS. The "Continuous Treatment" specifications measure CS exposure as the number of years of required schooling. Controls include child birth year effects, child birth state effects, child birth state by birth year effects, parental birth year effects, parental birth state effects, parental birth region by parental birth year effects, and child 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.

labor market outcomes of either sons or daughters. These results are consistent with those in Table 2 showing larger direct effects of CS exposure on the education of mothers, as well as the results in Table 3 showing larger intergenerational effects of maternal vs. paternal CS exposure on children's schooling outcomes. The lack of significant effects here might simply reflect smaller reform effects on father's education that generate smaller, more difficult to detect impacts on downstream intergenerational outcomes. It could also reflect the fact that maternal and paternal CS exposure could be operating through different intergenerational mechanisms that affect child outcomes in different ways. In Section 6, we directly examine some important mechanisms that could generate these patterns, focusing on how parental labor market, marriage, and residential outcomes were themselves shifted by exposure to CS.

5 Additional Specifications and Robustness

This section presents complementary analyses that help interpret and validate our main findings. We estimate two additional specifications: (i) a stacked event-study estimator addressing concerns about potential bias in the two-way fixed effects (TWFE) framework, and

(ii) specifications including an expanded set of demographic and policy-related controls. We then perform several robustness checks to assess whether our baseline results are affected by the following: (i) inclusion of child-level controls, (ii) the timing of compulsory schooling laws relative to other reforms and secular trends in educational and economic patterns across states, (iii) selection into the intergenerational sample based on cohabitation patterns, (iv) selection bias through mortality, (v) systematic census linkage errors, and (vi) selective migration. Last, (vii) we assess robustness to multiple potential sources of selection bias by reweighting our sample to more closely match the 1940 Census. In the following subsections, we explicitly discuss the results of robustness checks for our main intergenerational specification of interest in Column (2) of Table 3. In Sections A.4 and A.5 of the Appendix, we present expanded sets of results showing robustness checks for specifications on parental own schooling, and children's labor market outcomes.

5.1 Alternate Specifications

Stacked Event Study Specification As discussed in Section 3, the two-way fixed effects (TWFE) estimator can be biased in settings with staggered adoption and heterogeneous treatment effects. In our context, this occurs because states that implemented or expanded compulsory schooling (CS) laws earlier may inadvertently serve as controls for states that reformed later. When treatment effects are positive and persistent—as is typical for education—these comparisons can receive small or even negative weights in the TWFE aggregation, attenuating the overall estimate. To assess this, we apply the decomposition of Goodman-Bacon (2021) to our main specification. The results (Appendix Tables A6 and A7) show that such problematic comparisons account for roughly 10% of the total weights and imply smaller or negative effects, suggesting that our estimates are, if anything, conservative. Most of the identifying variation comes from comparisons with never- or always-treated states, where the weighting issue is minimal.

To complement the TWFE analysis, Appendix Table A8 reports estimates of the intergenerational effects of CS exposure using the stacked difference-in-differences approach of Cengiz et al. (2019).[18] This estimator avoids the weighting issues inherent in TWFE by comparing each treatment cohort only to units that are untreated at the same time. Using this approach, maternal (paternal) CS exposure increases children's schooling by 0.056 (0.078) years, and each additional year of exposure raises schooling by 0.022 (0.019) years. Although the stacked estimator relies on fewer reform events, its estimates are remarkably similar to those in Table 3, reinforcing that our main results are not driven by estimator bias. Appendix Tables A19 and A34 show comparable patterns for parental and intergenerational labor market outcomes.

Time-Varying State Level Controls One concern about our baseline TWFE estimates is that they could reflect correlation between the observed CS reforms and other state-level policies or more general state trends in economic or demographic characteristics of the popu-

[18] We adopt this specification because it is straightforward to implement and scales efficiently given the large number of observations and fixed effects in our setting. Specialized implementations of newer estimators are computationally infeasible at this scale.

lation.[19] The inclusion of parental region of birth by year of birth interactions in our baseline models is meant to address this. To further alleviate these concerns, we consider models with an expanded control set featuring the following variables. For each decennial census between 1880 and 1910, we compute several state-level characteristics for the cohort of 10-year old children: (i) the ratio of foreign-born children, (ii) the ratio of second generation immigrant children, (iii) teachers per 1,000 students, (iv) literacy rates, (v) school attendance rates, and (vi) child labor rates. For each state, we linearly interpolate these values for each year outside of the census years. Then, for each individual in our regressions, we control for the value of these measures in the parent's state of birth when they were 10 years old. A concern with adding controls like these is that they may be endogenous, and could reflect the effects of changes in compulsory schooling laws. In the case of literacy, child labor rates, and school attendance, which are particularly endogenous to CS laws, we control for the values of these variables during the year the parents were born (essentially a 10-year lag).

Appendix Table A9 presents results from an expanded TWFE model that includes the expanded set of time-varying controls. Many of these parental cohort-level controls are unsurprisingly statistically significant predictors of children's outcomes. However, expanding the control set only modestly changes our estimates for the intergenerational effects of parental CS exposure. In these specifications, we find that having a mother (father) exposed to any compulsory schooling increased children's years of schooling by 0.058 (0.071) years. Each extra year of CS exposure for a mother (father) increased children's schooling by 0.015 (0.015) years. All intergenerational estimates remain statistically significant. Tables A20 and A35 show similar analyses for the parental and intergenerational labor market results, respectively.[20]

5.2 Robustness Checks

Child-Level Controls We re-estimate our main intergenerational model (Equation 2) without including child-level controls, which may be endogenous due to parental migration or fertility decisions. As shown in Appendix Table A.3, the estimated effects remain robust, and are in fact larger.

Placebo Tests: Timing of CS Laws. A key identification concern is that our estimated effects might reflect other policy changes or state-specific time trends that coincide with CS reforms, rather than the reforms themselves. For example, states that increased CS requirements might also have expanded school funding or introduced other education-related

[19] In particular, passage of some compulsory schooling laws may have been in response to higher rates of immigration (Bandiera et al., 2018), raising concerns that our results might be influenced the effect of immigration on native born individuals in the parental cohorts. This motivates the inclusion of the foreign-born share as one of our expanded controls. Additionally, some states passed English-only laws and other reforms during this period. Lleras-Muney and Shertzer (2015) find that such laws had little effect on later labor market outcomes of foreign-born individuals. Since we only include parents born in the United States in our sample, this specific set of laws seems unlikely to be driving our results.

[20] Adding the expanded controls to the baseline specification (Table A20) generally attenuates the estimated effects on labor market outcomes and reduces statistical significance. Here we find that any maternal exposure to CS increases wages by 0.64 log points (p-value 0.137), while an extra year of maternal CS exposure increases wages by 0.17 log points (p-value 0.077). Parental results are very robust to introducing these controls.

reforms. To test the specificity of our estimates to the actual timing of CS laws, we conduct placebo tests using CS reforms that take effect 3 to 10 years after the parent's birth. If the true driver were broader improvements over time, these future laws might also appear predictive. However, in Table A12, we find that estimates based on future CS laws are smaller, often statistically insignificant, and even negative at longer leads. When we include future laws as controls (Table A13), our main estimates remain robust. This suggests that the timing of CS laws—not coincident trends—is key to the effects we observe.

Sample Selection: Cohabitation. Next, we address potential selection bias from over-representing adult children who cohabit with parents in 1940. Specifically, adults living with their parents in 1940 (when parents are aged 40–65 and children are 25–50) might represent a non-random group—possibly those with poor health, weak labor market outcomes, or strong family dependency norms. If these characteristics are correlated with both parental CS exposure and child education, our estimates could be biased since this group is over-represented in our sample relative to the population. To test for this, we perform three exercises: i) estimating results using only pre-1940 cohabitation matches, ii) using only 1940 cohabitation matches, and iii) using only parents cohabiting with children aged 0–10.

As shown in Table A14 (Cols. 1–4), our results are stable across all samples. The 1940 cohabitation sample—arguably the most prone to selection bias—produces the smallest effects, suggesting any bias may be downward. We further confirm robustness in Table A15 by including children whose parents were identified in earlier censuses but not re-linked in 1940. Taken together, these results highlight the potential biases that might arise from studying intergenerational effects only using samples of co-resident parents and children.

Sample Selection: Mortality. Another concern is that requiring both parents and children to be alive and observed in 1940 could bias our results through selective mortality. We expect there to be positive selection into survival, so that among older parents who make it into in our sample, those exposed to lower CS could be more skilled or have higher unobserved human capital relative to the population of older parents from such environments. We would expect this to bias the intergenerational effects of CS laws downwards, as it would understate the negative consequences of having parents exposed to lower CS. However, either negative or positive bias is possible in principle. To explore this, we stratify our sample by parental age: those 40–50 (less likely to have died before 1940) and those over 50. Table A14 (Cols. 5–6) shows that we do indeed find a difference across cohorts – with large estimated effects for the younger parental cohorts and no statistically significant effects for older parents. These results are consistent with expected patterns of selective mortality, and suggest that our main results could be understating the true intergenerational effects of CS reforms.

Mismatched Census Records. Linking individuals across censuses is inherently error-prone. If linkage errors are random, they would introduce noise and bias estimates toward zero. But if errors are systematic—say, if low-education children are more likely to be wrongly linked to low-CS parents, this could bias our results upward or downward. To address this, we replicate our analysis using only high-confidence links from Census Tree that are corroborated by at least three of seven independent sources (e.g., MLP, CLP, Family Search, machine-

learning matches). As shown in Table A16, these higher-quality linkages yield slightly larger estimates than the full sample. We also verify that results hold when using MLP and CLP linkages, providing additional reassurance that linkage error is not driving our results.

Between-State Migration Migration patterns could add noise to our measurement of parental CS exposure. We use state of birth to determine parental CS exposure, but if intra-state migration occurs when the parents are young, they could have effectively grown up under a different policy regime than the one we assign to them. To alleviate these concerns, we split the sample into: i) children of never-mover parents, ii) children of parents who moved before their birth (early movers), and iii) children of parents who moved after their birth (late movers). In Table A17, we find strong effects among never- and late-movers. For early movers, who were likely not fully exposed to the CS laws in their birth state, the effects are null—likely due to measurement error in CS exposure. This pattern suggests that any bias from parental intra-state migration likely attenuates our main estimates.

Reweighting to Address Selection Bias. Given all the potential sources of sample selection (cohabitation, mortality, linkage success, migration), we reweight our analysis sample to match the demographic structure of the full 1940 Census. Using a probit model to predict inclusion in our sample based on birth year, birth state, sex, and race, we construct inverse probability weights and apply them to our main specification. As shown in Table A18, our results are stable, indicating that the findings are not sensitive to the observed selection processes and bolstering the generalizability of our conclusions.

6 Mechanisms and Heterogeneous Effects

In this final section, we investigate the channels through which compulsory schooling (CS) may have generated the observed intergenerational effects. Parental exposure to CS could influence children's outcomes through several mechanisms operating both inside and outside the household. Using the available census data, we focus on four broad sets of explanations. First, improved parental labor market outcomes may increase household income and provide children with better schooling opportunities. Second, changes in family structure and assortative mating could alter the educational and economic environment in which children are raised. Third, shifts in the intra-household allocation of resources, such as fertility, investment per child, or time devoted to education, may reinforce intergenerational gains. Finally, changes in neighborhood characteristics—through residential mobility or selective migration—could expose children to higher-quality schools and peer environments.

Other mechanisms, such as evolving social norms, aspirations, or attitudes toward education (Piopiunik, 2014), may also play a role but cannot be directly observed in the census data and thus remain beyond the scope of this analysis.

6.1 Parental Labor Market Outcomes

A natural starting point is to consider the effects of parental CS exposure on own parental labor market outcomes, as these directly impact household monetary and time resources - the basic inputs stressed in many theories of child development. Parental exposure to CS

can increase the educational attainment and earnings of parents, providing more financial resources for the household that can be used to invest in the human capital of children. Time spent with children also matters. In a household setting with two parents, higher earnings capacity for one parent could induce an income effect, allowing the other spouse to work less (or stop working) and devote more time to child development.

Table 5: Effect of Parental Exposure to Compulsory Schooling on Parental Labor Market Outcomes

	Dependent variable: Parents' labor market outcomes						
	In Labor Force (p.p.)	Employment (p.p.)	Wage (Log, pp)	Hours Worked	Occupation Score	Teacher (p.p.)	Librarian (p.p.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CS Years (Women)	-0.108***	-0.104***	0.404**	0.060**	-0.029	0.058***	0.013**
	(0.0221)	(0.0200)	(0.1728)	(0.0250)	(0.0453)	(0.0193)	(0.0065)
N (millions)	4.3	4.3	0.4	0.5	0.6	0.6	0.6
R^2	0.03	0.02	0.09	0.02	0.10	0.00	0.00
DV Mean	13.2	12.2	579.0	41.6	30.2	2.7	0.2
IV Estimates	-8.63***	-8.29***	27.74**	5.20*	-1.77	3.54***	0.80*
CS Years (Men)	-0.049**	-0.036	0.087	-0.012	-0.077***	-0.001	0.000
	(0.0244)	(0.0280)	(0.0906)	(0.0139)	(0.0246)	(0.0040)	(5.511e-04)
N (millions)	3.0	3.0	1.6	2.2	2.7	2.7	2.7
R^2	0.04	0.03	0.10	0.03	0.06	0.00	0.00
DV Mean	88.4	82.8	677.1	46.7	31.6	0.4	0.0
IV Estimates	-6.57	-4.92	11.89	-1.63	-11.65	-0.21	-0.03

Notes: Relationship between parental exposure to more CS and labor market outcomes. Each column represents a different regression. The top panel is estimated using men and the bottom panel using women from the *Parents* sample. Outcomes are measured using the 1930 Census. 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.

To examine the effects on parental labor market outcomes in our sample, we focus on those *Parents* who can be located in the 1930 Census, when they were most likely to be prime-aged workers aged 30 to 55. Table 5 presents estimates of Equation 1 for several important outcomes using data on this 1930 subsample.[21]

For mothers, we find evidence for multiple channels that could affect the human capital of children. We find a negative effect of exposure to CS on participation in the labor force and employment, with one extra year of CS exposure reducing labor force participation and

[21]For binary outcomes we scaled the dependent variable to take values 0 and 100, so the coefficients are directly interpretable as changes in *percentage points*.

employment by approximately 0.1 percentage point. However, conditional on working in 1930, CS exposure increased the educational content of their occupation. More CS exposure for mothers also increases the probability that, conditional on working, they work in an occupation with above-median average education and earnings.[22] CS exposure also increased the likelihood that mothers are employed as teachers (conditional on working). While relatively few women became teachers, this offers one example of a specific occupation that may be associated with more human capital investment in the household.

The labor market outcomes of fathers in our *Parents* sample were less affected by exposure to CS laws. More years of CS exposure reduced their labor force participation, but this effect is smaller in magnitude and statistically weaker than the result for mothers. CS exposure increased the likelihood that fathers sorted into occupations with above-median average levels of education, but had no effect on all other outcomes.[23]

6.2 Marriage, Fertility, and Assortative Mating

Parental exposure to CS may also impact child outcomes through choices about family formation. There are several dimensions to consider. At the most basic level, CS exposure could affect whether a parent ends up married or not. A large literature addresses the effects of family structure, and in particular single parenthood, on child outcomes (Kearney, 2022). Moreover, greater CS exposure - possibly as a result of longer time spent in school and increased labor-market returns - may lead *Parents* to delay marriage, delay having children, and decide to have fewer children. The intergenerational effects of CS exposure could thus arise from a quantity-quality trade-off in fertility (Becker, 1960).

The census data are not ideal for studying marriage dynamics given the large gaps between observations. However, the 1940 Census contains a marriage and fertility history questionnaire administered to a 1% random sample of ever-married women. In Columns (1) to (3) of Table 6, we estimate the effect of CS exposure on marriage and fertility outcomes using these items. We find no statistically significant effects on the total number of children born to women in this sample.[24] However, CS exposure positively increased age at first marriage, with one extra year of CS exposure increasing marital age by 0.029 years. We also find that CS exposure reduces the probability that a woman ever remarries. This effect is difficult to interpret, as it could either indicate a reduction in the likelihood of divorce, or a reduction

[22] Occupational earnings and educational scores are IPUMS-provided percentile ranks of occupations, based on median earned incomes and education levels observed in the 1950 Census across different occupations.

[23] These results also hold in 1940 (Table A37 of the Appendix). Although measuring the outcomes in 1940 could lead to biased estimates, since many of the parents would be aged between 55 and 60 years and thus are either retired or more likely to be deceased, the results are strikingly similar to the 1930 results. Indeed, we find the same reductions in labor force participation and employment for women, coupled with sorting into high-education occupations, including teacher and librarian. Further, the 1940 Census records wages and hours worked. We find positive effects on both wages and hours worked for working women. While effects for men have similar signs they are not significant. Returns to schooling for women were very high (28% per schooling year), yet only 13% of women were in the labor force, compared to 88% of men. Perhaps, on average, working women were those who had abnormally high labor returns.

[24] A separate analysis we perform on the universe of women aged 35 to 50 in the 1940 Census shows no effect on the extensive margin of fertility - which we cannot measure on our mothers' sample - and a small positive effect on the intensive margin of fertility, which suggests that our positive intergenerational results are unlikely to be due to quantity-quality tradeoffs.

Table 6: Effect of Parental Exposure to Compulsory Schooling on Assortative Mating, Marriage and Fertility

	Dependent variable: Parent's marriage and fertility outcomes								
	Lifetime Outcomes:			***1940 Outcomes:***					
	Number of Children (1)	Age First Marriage (Years) (2)	Ever Remarried (p.p.) (3)	Married (p.p.) (4)	Widowed (p.p.) (5)	Separated (p.p.) (6)	Spouse Age Difference (Years) (7)	Spouse CS Exposure (Years) (8)	Spouse Schooling (Years) (9)
CS Years (Women)	0.006	0.029***	-0.191***	-0.081***	0.111***	-0.026***	0.000	0.335***	0.015***
	(0.0062)	(0.0085)	(0.0658)	(0.0273)	(0.0249)	(0.0085)	(0.0053)	(0.0144)	(0.0025)
N (millions)	0.2	0.2	0.2	4.3	4.2	3.2	3.0	2.8	3.0
R^2	0.05	0.08	0.01	0.06	0.07	0.00	0.03	0.57	0.10
Outcome Means	4.2	20.7	10.2	73.7	23.9	2.2	4.1	2.7	7.6
IV Estimates	0.35	1.76**	-11.58**	-6.44***	8.56***	-1.67**	0.01	17.03***	0.93***
CS Years (Men)				-0.004	0.030*	-0.017***	0.002	0.441***	0.013***
				(0.0175)	(0.0154)	(0.0072)	(0.0060)	(0.0139)	(0.0032)
N (millions)				3.0	3.0	2.7	2.6	2.5	2.6
R^2				0.02	0.02	0.00	0.07	0.59	0.10
Outcome Means				89.3	8.2	1.3	-3.7	3.4	8.0
IV Estimates				-0.48	3.94	-2.17	-0.01	43.20***	1.65***

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.

in the probability of remarriage conditional on divorce.

Columns (4)-(6) of Table 6 examine the effects of CS exposure on cross-sectional marriage outcomes for *Parents* in the 1940 Census. These require some interpretive care, since the *Parents* are older in this census, and the variables do not capture life-cycle marriage dynamics. CS exposure reduced the probability of being currently married for women and had no effect for men (Column 4). It seems likely that this is driven by spousal mortality. Indeed, in Column (5) we find a positive effect of CS exposure on the probability of widowhood, with the magnitude of the coefficient for women being roughly similar to the one for current marriage. One speculative possibility is that this reflects positive effects of CS exposure on longevity, which could increase the likelihood of outliving one's spouse. Column (6) suggests that, for both women and men, CS exposure reduced the probability of being separated from one's spouse (conditional on not being widowed). Although separations are rare in this sample, these significant effects suggest that family stability could be one possible channel contributing to our main results.

Finally, exposure to CS can also affect one's children in the presence of assortative mating. Columns (7-9) of Table 6 present the effects of CS exposure on spousal characteristics. We find no effect of CS exposure on the relative age of one's spouse. However, greater CS exposure increased the educational characteristics of one's spouse (Columns 8 and 9). For women, one extra year of CS exposure increased one's spouse's CS exposure by 0.335 years, and actual educational attainment by 0.015 years. For men, these effects are 0.441 years and 0.013 years, respectively. This may occur because of intentional choices in the marriage market (assortative mating), or mechanically if people largely pair with members of their own

birth cohorts, and those cohorts are collectively exposed to more schooling. These results are consistent with Buckles et al. (2023b), who document strong assortative mating patterns in the early twentieth century in the United States.

6.3 Neighborhood Sorting

Table 7: Effect of Parental Exposure on Neighborhood Sorting

	Dependent variable:								
		Neighborhood men characteristics				*Neighborhood women characteristics*			
	Teacher Student Ratio	Literacy	Labor Force Participation	Occupational Education Score	Occupational Earnings Score	Literacy	Labor Force Participation	Occupational Education Score	Occupational Earnings Score
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
CS Years (Female)	0.012***	0.004***	0.005***	0.004***	0.004***	0.008***	0.025***	0.007***	0.005***
	(0.0010)	(4.557e-04)	(0.0013)	(0.0010)	(7.983e-04)	(6.032e-04)	(0.0018)	(0.0015)	(0.0010)
N (millions)	2.7	2.7	2.7	2.7	2.7	2.7	2.7	2.7	2.7
R^2	0.06	0.42	0.01	0.17	0.08	0.39	0.02	0.17	0.28
Outcome Means	-0.1	0.0	-0.0	0.0	0.0	0.0	-0.1	-0.0	-0.0
IV Estimates	0.68***	0.18***	0.25***	0.11**	0.15***	0.42***	1.47***	0.43***	0.28***
CS Years (Male)	0.009***	0.001	0.002	0.000	0.001	0.004***	0.019***	0.005***	0.002**
	(9.158e-04)	(6.748e-04)	(0.0012)	(0.0010)	(8.296e-04)	(6.487e-04)	(0.0015)	(0.0015)	(8.534e-04)
N (millions)	2.3	2.3	2.3	2.3	2.3	2.3	2.3	2.3	2.3
R^2	0.06	0.42	0.01	0.18	0.08	0.40	0.01	0.16	0.27
Outcome Means	-0.1	0.0	-0.0	0.0	-0.0	0.1	-0.1	-0.0	-0.0
IV Estimates	3.34	0.09	0.57	-0.59	-0.28	1.46	7.68	1.82	0.58

Notes: Effect of exposure to different CS laws on neighborhood (defined as a census enumeration district) characteristics. Each column represents a different regression. Column (1) represents teacher-student ratios, Columns (2-5) represent characteristics male inhabitants in the neighborhood aged 18-60, and Columns (6-9) represent characteristics of female inhabitants aged 18-60. The neighborhood outcomes are standardized to be zero mean with unit standard deviations. 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.

We also examine whether parental CS exposure altered the characteristics of the neighborhoods where children were raised. Neighborhoods differ in their schooling, labor market, demographic, and household characteristics. Exposure to different neighborhood characteristics can in turn influence the human capital accumulation of children. Indeed, a large literature examines the role that neighborhoods play in this process (Chetty and Hendren 2018a,b, Chetty et al. 2016).

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 CS exposure could change neighborhood characteristics by shifting household resources, allowing families to move to areas with better amenities, like higher-quality schools. Occupational changes could also require movement to parts of the city where different kinds of jobs (e.g., those requiring more education) were located. Beyond school quality, neighborhood sorting dynamics could also influence the peer networks available to children.

To understand the relationship between parental CS exposure and geographic sorting, we take advantage of the fine geographic variation measured by the censuses. Specifically, we use information on census enumeration districts to build standardized neighborhood-level metrics on several educational, human capital and labor market dimensions. Each enumeration district corresponds to a smaller geographic area containing relatively few households.[25]

[25]An enumeration district, as used by the Bureau of the Census, was an area that could be covered by a

Then, for each parent in our sample, we identify the first census in which they were cohabiting with a child in our sample and the enumeration district they were inhabiting. We then study the relationship between parental CS exposure and the characteristics of this enumeration district, using a specification similar to Equation 2.

In Table 7, we show that parental exposure to CS is associated with sorting into neighborhoods with higher teacher-student ratios when their children are young. This suggests that these children may have had better access to schooling resources, which could explain part of our intergenerational results. Moreover, we also study the human capital and labor market characteristics of the adult inhabitants of these neighborhoods, aged 18 to 60. On average, mothers in our sample who were exposed to more CS sorted into neighborhoods where men and women were more literate, had higher labor force participation rates and worked in occupations associated with higher earnings and education levels. The effects are similar, but slightly more muted for fathers, who sorted into neighborhoods with higher female human capital and occupational status as a result of exposure to CS.

6.4 Within-Family Heterogeneity

Having found no systematic effects of parental CS exposure on fertility or family size, we next examine whether intra-household allocation of resources changed in ways that could help explain the intergenerational effects documented above. These dynastic effects represent another mechanism through which compulsory schooling (CS) reforms may influence children's education: increases in parental schooling can alter how parents allocate time, attention, and material resources across their children.

To explore these within-family dynamics, we estimate models in which the parent is the unit of observation, focusing on several dimensions of children's educational attainment—average years of schooling overall and by gender, and the schooling of the eldest, youngest, most educated, and least educated child. These specifications not only test for heterogeneity in the effects of parental CS exposure but also help address potential selection concerns related to fertility.

The results, presented in Table 8, show that parental exposure to CS laws increased children's average education, with somewhat larger gains for daughters than sons. The effects are strongest for eldest children, consistent with their role as early recipients of parental investments, but the largest relative improvements occur among the least educated siblings. This pattern suggests that CS reforms not only enhanced average educational attainment across generations but also reduced educational inequality within families, consistent with an intra-household reallocation of resources rather than changes in fertility.

6.5 Mechanisms Summary and Mediation Analysis

Taken together, the analyses in this section highlight some important mechanisms that could drive our main intergenerational results. For women, we find evidence that CS exposure reduced the probability of participating in the labor force, potentially freeing time for human

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.

Table 8: Dynastic Specifications and Within-Family Heterogeneity in the Effects of Parental Exposure to Compulsory Schooling Laws

	Dependent variable: Children's Years of Schooling										
		Son					**Daughter**				
	Average (All Children)	Average	Eldest	Youngest	Most Educated	Least Educated	Average	Eldest	Youngest	Most Educated	Least Educated
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
CS Years (Mom)	0.016***	0.016***	0.017***	0.013***	0.007**	0.024***	0.018***	0.021***	0.014***	0.010***	0.026***
	(0.0027)	(0.0030)	(0.0030)	(0.0030)	(0.0032)	(0.0030)	(0.0027)	(0.0028)	(0.0027)	(0.0028)	(0.0028)
N (millions)	4.3	3.1	3.1	3.1	3.1	3.1	2.4	2.4	2.4	2.4	2.4
R^2	0.14	0.14	0.13	0.13	0.13	0.14	0.10	0.09	0.10	0.10	0.10
Outcome Means	10.2	10.0	9.9	10.0	10.4	9.5	10.4	10.4	10.4	10.7	10.1
IV Estimates	1.30***	1.18***	1.29***	0.95***	0.55***	1.80***	1.34***	1.55***	1.06***	0.75***	1.93***
CS Years (Dad)	0.018***	0.017***	0.018***	0.015***	0.008**	0.025***	0.022***	0.025***	0.019***	0.015***	0.030***
	(0.0029)	(0.0033)	(0.0035)	(0.0032)	(0.0034)	(0.0034)	(0.0034)	(0.0034)	(0.0034)	(0.0033)	(0.0035)
N (millions)	3.0	2.2	2.2	2.2	2.2	2.2	1.6	1.6	1.6	1.6	1.6
R^2	0.12	0.13	0.12	0.12	0.11	0.13	0.09	0.09	0.09	0.09	0.09
Outcome Means	10.3	10.1	10.0	10.1	10.4	9.7	10.5	10.4	10.5	10.7	10.2
IV Estimates	2.43***	2.25**	2.46**	1.98**	1.14***	3.34**	2.26***	2.54***	1.94***	1.50***	3.03***

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.

capital investment at home. Among those women who worked, CS exposure shifted occupational characteristics, leading women to work in occupations requiring more education and offering higher pay. While we do not find evidence that CS exposure changed fertility choices, we find evidence that for both men and women, greater exposure to CS increased the average educational attainment of their partners. Thus, whatever direct effect a parent's exposure to CS might have had on their child (i.e., the parent's own skill or earnings), there is also an indirect effect coming from the characteristics of the child's other parent, through assortative mating. Beyond its direct effects on parental labor market outcomes and fertility, we also find that CS exposure affected children differently by birth order and gender, suggesting the presence of child-specific preferences and interplay between parental exposure to CS and the intra-household allocation of resources. Finally, neighborhood characteristics may also play a role, as parents exposed to more education resided in neighborhoods more human capital-promoting characteristics.

In Appendix A.6, we formalize this by conducting mediation analyses via Gelbach decomposition exercises (Gelbach, 2016).[26] These exercises highlight two things. First, parental labor market outcomes appear to be the most important mediators of the intergenerational effect of compulsory schooling. Second, assortative mating also plays a large role. In particular, father's exposure to compulsory schooling has intergenerational effects primarily through the mothers' labor market outcomes. These findings highlight the importance of compulsory schooling in shaping maternal occupational patterns, which have long-lasting effects.

[26]This approach consists of adding various controls to our baseline regressions 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 also code missing values as zero and create missingness dummies for each control variable to avoid losing too many observations due to missingness. For more details, see the appendix.

7 Conclusion

In the late nineteenth and early twentieth centuries, many states sequentially introduced CS laws, seeking to raise educational attainment and boost the social mobility of less educated and poorer families. Using the linked 1880-1940 full-count censuses and cutting-edge cross-linkages, we examine 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 the children of individuals directly exposed to the reforms. We also find evidence of intergenerational effects on labor market outcomes - particularly for the effects of maternal CS exposure on the wages of sons. The results also indicate that the benefits of compulsory schooling were unevenly distributed. For Black families, limited or null effects are consistent with the continued segregation of schools, disparities in resources, and weaker enforcement of attendance laws.

We document several plausible mechanisms explaining intergenerational persistence in the effects of CS reforms. CS exposure impacted several aspects of the labor market experience of mothers - reducing labor force participation, but increasing wages and the average educational level of their occupations when working. All of these effects could have increased time or financial resources devoted to child investment. Although we find little evidence that CS exposure affected marriage or total fertility, we do find that both mothers and fathers exposed to more CS married spouses with higher levels of educational attainment. Our dynastic analyses further indicate that the effects CS exposure may have been focused by parental preferences and the intra-household decision-making process: the gains were strongest for first-borns and for the least-educated siblings, suggesting that exposed parents placed greater emphasis on ensuring a minimum level of education for all children. Moreover, parents exposed to more CS raised their children in neighborhoods with higher teacher-student ratios, and with higher levels of education, literacy and higher overall labor force participation, amplifying local opportunities for the next generation.

The effects of CS laws on the attainment of the second generation were substantial compared to the effects on the first generation, and affected educational outcomes beyond the bottom of the educational distribution. This suggests that these educational reforms may have successfully "snowballed" to achieve rapid growth in educational attainment over the twentieth century. We find the intergenerational effects to be larger than previously thought - especially in comparison to results from more recent European cohorts. 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.

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A Appendix Tables and Figures

A.1 Additional Details on Linkages and Building Our Samples

To build an intergenerational dataset of linked children and parents, we start with adults aged between 25 and 50 in the 1940 Census. Our *Children* sample consists of these individuals, since their *Parents* were born in the late nineteenth century and directly exposed to the rapid changes in CS laws that we study. For each person in the *Children* sample, we would like to identify census records on their parents, either in 1940 or earlier. Once we locate census records for these parents, we can use their reported state and year of birth to assign them to a particular CS regime. We identify parents of the *Children* sample in two ways. First, via direct cohabitation in the 1940 Census. Some adults in 1940 will be observed living with their parents for a variety of reasons: they never left home (mainly young adults), their parents are elderly and moved in with their adult children, etc. If we cannot identify the parents in this way, we link individuals in the *Children* sample to an earlier census wave (starting with 1930 and continuing backward in time), until we find a census in which we can identify their parent via cohabitation. This level of linkage would be sufficient if we only wanted to estimate the effects of *parental CS exposure*. However, for specifications in which we estimate the effect of *completed parental education* on child education, we must also link these parents back to the 1940 Census (if they are still alive), since this was the first census to record completed education. Our *Parents* sample consists of all individuals who can be identified in this way as a parent of a 1940 Census adult.

We place a few restrictions on the set of matches that we use for our intergenerational sample. First, we restrict our attention to children who are aged between 25 and 50 in 1940 (born between 1889 and 1914), linked to at least one parent aged 40 to 65 in 1940 (born between 1874 and 1899). Both children and parents must be born in one of the continental US states. Lastly, we note that we filter out linked records with inconsistent birth states or birth years to ensure linkage quality and because we rely on these two variables to detect parental exposure to compulsory schooling.[27] This results in a dataset of 10.6 million adults in 1940 who can be linked to at least one of their parents in the 1940 Census.

We note several possible sources of selection bias that could arise given the way we construct our sample. First, it is very likely that the set of *adults* who cohabit with their parents is non-random. This may pose a potential problem because a number of individuals are matched to their parents only through cohabitation in 1940. If this is correlated with parental CS exposure, then selection bias could affect our estimates. Furthermore, using several census linkages to identify children in an earlier census and then several other linkages to re-link their parents to their 1940 Census compounds errors in census links. Relative to the overall population, the set of families with a complete series of successful linkages may be unrepresentative. Lastly, selection bias may also arise - especially for older parents - due to selective mortality that might prevent linking back to the 1940 Census. We address each of these issues in turn in our robustness section, which estimates our model on different subsamples of the data which should be less affected by these issues.

Our final sample skews slightly more rural, but has similar income levels as the universe of 1940 respondents of similar ages. Women are underrepresented in our *Children* sample (40%

[27] This last step reduces the noise in our estimates, but is not otherwise crucial.

vs 51%). This is likely due to the difficulty of linking women across censuses given changes of last names upon marriage, and possibly due to lower cohabitation rates with their parents when they are older. Black Americans are also underrepresented. While our sample is not entirely representative of the universe of 1940 Census respondents in the same age categories, we believe it is reasonably similar to the complete set of similarly-aged individuals. Our sample represents the universe of linked adults and parents in the 1940 Census, to the extent that they can be matched via currently-available state-of-the-art linking methods.

A.2 Additional Results

In this section, we present additional analyses for the effects of CS exposure on children's or parents' years of schooling. These tables are directly referenced in the main text.

A.2.1 Effect of CS on Parents (Replicating Results from Literature)

Table A1: Effect of Parental Compulsory Schooling Exposure on Parental Completed Years of Schooling (Alternative Specifications and Samples)

	TWFE Specification			
	All Men (27 to 54)	All Men (40 to 65)	All Women (27 to 54)	All Women (40 to 65)
CS Years	0.020***	0.000	0.022***	0.006***
	(0.0033)	(0.0024)	(0.0035)	(0.0024)
N (millions)	19.2	12.7	19.3	12.7
R^2	0.08	0.06	0.07	0.06
Outcome Means	9.3	8.5	9.5	8.9
	Clay et al. (2021) Specification			
1-5 Years CSL	0.050***	0.001	0.047**	0.042***
	(0.0160)	(0.0118)	(0.0194)	(0.0128)
6 Years CSL	0.057***	0.003	0.069***	0.034**
	(0.0198)	(0.0150)	(0.0225)	(0.0149)
7 Years CSL	0.105***	-0.045**	0.124***	-0.015
	(0.0212)	(0.0195)	(0.0244)	(0.0192)
8 Years CSL	0.138***	-0.003	0.144***	0.036*
	(0.0237)	(0.0226)	(0.0269)	(0.0217)
9+ Years CSL	0.260***	0.108***	0.224***	0.130***
	(0.0280)	(0.0367)	(0.0307)	(0.0388)
N (millions)	21.4	14.0	21.7	14.0
R^2	0.17	0.15	0.16	0.15
Outcome Means	8.9	8.2	9.2	8.5

Notes: Effects of exposure to CS laws on years of schooling for the *Parents* sample. Each column represents a regression on a different sample: men and women aged 27 to 54 (prime-age sample used in Clay et al., 2011) and all women and men aged 40 to 65 (the ages of our parents) in the 1940 census. The top panel estimates the main TWFE specification used in this paper and the bottom panel estimates the specification of Clay et al. (2011). 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 A2: Effect of Parental Compulsory Schooling Exposure on Parental Completed Years of Schooling (Prime Age in 1940)

0.5

	Dependent variable: *Parent's Years of Schooling*					
	All	White	Black	East	West	Midwest
	(1)	(2)	(3)	(4)	(5)	(6)
CS Years (Women)	0.020***	0.020***	0.031***	0.062***	0.076***	-0.005
	(0.003)	(0.003)	(0.007)	(0.008)	(0.009)	(0.003)
N (millions)	14.1	12.8	1.2	3.6	0.9	5.5
R^2	0.16	0.08	0.12	0.04	0.18	0.08
Outcome Means	8.9	9.2	5.9	9.3	9.9	9.4
CS Years (Men)	0.018***	0.017***	0.034***	0.058***	0.055***	-0.007*
	(0.004)	(0.004)	(0.007)	(0.010)	(0.009)	(0.004)
N (millions)	12.7	11.7	1.0	3.2	0.8	5.0
R^2	0.17	0.08	0.11	0.04	0.16	0.07
Outcome Means	8.7	9.0	5.1	9.5	9.6	9.2

Notes: Effects of exposure to CS laws on completed years of schooling for the *Parents* sample by race and census region. 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.

A.2.2 Effectiveness of CS Laws

Table A3: Compulsory Schooling Law Effectiveness

	Women					
	All	White	Black	East	West	Midwest
	(1)	(2)	(3)	(4)	(5)	(6)
Proportion Under CS Years (p.p.)	12.4	12.2	24.1	8.0	11.6	15.1
Average Schooling Deficit (Yrs)	2.3	2.3	2.6	2.3	3.2	2.2
Actual Effect of CS Exposure (Yrs)	0.013***	0.011***	0.007	0.013***	0.071***	0.006
Potential Effect of CS Exposure (Yrs)	0.10	0.09	0.20	0.06	0.23	0.11
Effectiveness (Actual/Potential)	12.7%	11.5%	3.3%	22.5%	31.4%	5.5%
Treatment on Treated (Actual/Prop. Under CS)	0.094	0.082	0.024	0.17	0.54	0.035
	Men					
	All	White	Black	East	West	Midwest
	(1)	(2)	(3)	(4)	(5)	(6)
Proportion Under CS Years (p.p.)	13.6	13.4	27.9	8.1	13.6	16.7
Average Schooling Deficit (Yrs)	2.4	2.4	2.6	2.3	3.1	2.4
Actual Effect of CS Exposure (Yrs)	0.005**	0.004*	0.001	0.019***	0.055***	-0.008
Potential Effect of CS Exposure (Yrs)	0.12	0.12	0.25	0.07	0.26	0.13
Effectiveness (Actual/Potential)	4.4%	3.7%	0.5%	28.3%	21.2%	-
Treatment on Treated (Actual/Prop. Under CS)	0.039	0.032	0.0043	0.24	0.41	-

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.

A.2.3 Instrumental Variable

Table A4: Effect of Parental Years of Schooling on Children's Years of Schooling (IV First Stage)

	Dependent Variable: Parent's Years of Schooling					
	Black et al.	All	Men	Women	White	Black
	(1)	(2)	(3)	(4)	(5)	(6)
CS Years (Mom)	0.166***	0.021***	0.022***	0.019***	0.020***	-0.004
	(0.0015)	(0.0038)	(0.0041)	(0.0038)	(0.0040)	(0.0146)
N (millions)	7.8	7.8	4.6	3.2	7.3	0.4
R^2	0.05	0.12	0.12	0.11	0.07	0.11
Outcome Means	7.6	7.6	7.6	7.7	7.8	4.8
First Stage F-stat	210,224.2	304.5	208.8	99.4	274.2	0.09
CS Years (Dad)	0.200***	0.013***	0.013***	0.013***	0.013***	0.035**
	(0.0016)	(0.0036)	(0.0038)	(0.0041)	(0.0038)	(0.0167)
N (millions)	5.2	5.2	3.1	2.1	4.9	0.3
R^2	0.05	0.11	0.12	0.11	0.08	0.09
Outcome Means	7.5	7.5	7.5	7.6	7.7	4.7
First Stage F-stat	169,472.1	57.6	33.9	23.8	53.4	4.3

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 A5: 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
	(1)	(2)	(3)	(4)	(5)	(6)
Years (Mom)	1.15***	0.96***	1.03***	0.91***	0.98***	7.38
	(0.006)	(0.103)	(0.113)	(0.136)	(0.111)	(29.149)
N (millions)	7.8	7.8	4.6	3.2	7.3	0.4
R^2	-0.04	0.13	0.08	0.16	0.07	-31.81
Outcome Means	10.0	10.0	9.8	10.3	10.2	7.0
First Stage F-stat	210,224.2	304.5	208.8	99.4	274.2	0.09
Years of Schooling (Dad)	0.96***	1.29***	1.39***	1.20***	1.28***	0.34
	(0.004)	(0.280)	(0.337)	(0.316)	(0.294)	(0.460)
N (millions)	5.2	5.2	3.1	2.1	4.9	0.3
R^2	-0.03	-0.46	-0.56	-0.41	-0.53	0.23
Outcome Means	10.1	10.1	9.9	10.4	10.3	7.0
First Stage F-stat	169,472.1	57.6	33.9	23.8	53.4	4.3

Notes: Effect of completed 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 for most specifications include child birth year effects, child birth state effects, child birth state by birth year effects, parental birth year effects, parental birth state effects, parental birth region by parental birth year effects, and child sex and race fixed effects, when possible. In the first Column, the controls are child birth year, parent birth year, and parent county of birth effects. 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.

A.2.4 Goodman-Bacon Decompositions

Table A6: Goodman-Bacon Decomposition of Effects of Parental Exposure to Compulsory Schooling on Parental Years of Schooling

	Mothers		Fathers	
Comparison	Estimate	Weight	Estimate	Weight
Treated vs Never Treated	0.10	0.26	0.37	0.26
Later vs Always Treated	0.15	0.57	0.06	0.57
Earlier vs Later Treated	0.03	0.07	0.09	0.07
Later vs Earlier Treated	-0.10	0.10	-0.21	0.10
Weighted Average	0.11	1.00	0.12	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.

Table A7: Goodman-Bacon Decomposition of Effects of Parental Exposure to Compulsory Schooling on Children's Years of Schooling

	Children-Mothers		Children-Fathers	
Comparison	Estimate	Weight	Weight	Estimate
Treated vs Never Treated	0.36	0.26	0.46	0.26
Later vs Always Treated	0.17	0.57	0.25	0.57
Earlier vs Later Treated	0.07	0.07	0.08	0.07
Later vs Earlier Treated	-0.12	0.10	-0.20	0.10
Weighted Average	0.18	1.00	0.25	0.10

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

A.3 Heterogeneity and Robustness (Intergenerational Education Results)

In this section, we present robustness and heterogeneity analyses for the effects of CS exposure on children's years of schooling. These tables are directly referenced in the main text.

A.3.1 Stacked Event Study Estimator

Table A8: Effect of Parental Compulsory Schooling Exposure on Children's Completed Years of Schooling (Stacked Event Study)

	Dependent variable: Child's Years of Schooling					
	All (Binary Treat.)	All (Cont. Treat.)	Men (Cont.)	Women (Cont.)	White (Cont.)	Black (Cont.)
	(1)	(2)	(3)	(4)	(5)	(6)
CS Years (Mom)	0.056**	0.022***	0.026***	0.016**	0.021***	0.056*
	(0.0252)	(0.0073)	(0.0079)	(0.0079)	(0.0075)	(0.0331)
N (millions)	12.9	34.5	20.6	13.9	31.2	3.3
R^2	0.13	0.13	0.13	0.11	0.08	0.16
Outcome Means	9.86	9.83	9.60	10.16	10.14	6.91
CS Years (Dad)	0.078***	0.019***	0.023***	0.013*	0.018**	0.069
	(0.0246)	(0.0070)	(0.0077)	(0.0079)	(0.0072)	(0.0439)
N (millions)	7.8	20.4	12.4	8.0	18.6	1.7
R^2	0.12	0.12	0.13	0.10	0.08	0.17
Outcome Means	9.97	9.94	9.74	10.25	10.21	7.03

Notes: Effects of parental exposure to CS completed years of schooling for the *Children* sample, using a stacked event study method. Each column represents a different regression. 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, event, 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.3.2 Additional State Controls

Table A9: Effect of Parental Compulsory Schooling Exposure on Children's Completed Years of Schooling

	Dependent variable: Child's Years of Schooling					
	All (Binary Treat.)	All (Cont. Treat.)	Men (Cont.)	Women (Cont.)	White (Cont.)	Black (Cont.)
	(1)	(2)	(3)	(4)	(5)	(6)
CS Years (Mom)	0.058***	0.015***	0.017***	0.013***	0.016***	-0.029**
	(0.0175)	(0.0041)	(0.0043)	(0.0044)	(0.0044)	(0.0135)
First Gen	0.009*	0.010**	0.012**	0.005	0.010*	-0.035
	(0.0048)	(0.0047)	(0.0050)	(0.0056)	(0.0050)	(0.0381)
Second Gen	-0.005**	-0.007***	-0.007***	-0.007***	-0.006***	-0.070***
	(0.0022)	(0.0021)	(0.0023)	(0.0025)	(0.0021)	(0.0156)
Teachers per 1000	0.002***	0.002***	0.002***	0.001**	0.002***	-0.003*
	(0.0006)	(0.0006)	(0.0006)	(0.0007)	(0.0006)	(0.0015)
Literacy	-0.012***	-0.012***	-0.013***	-0.011***	-0.012***	-0.017***
	(0.0018)	(0.0018)	(0.0017)	(0.0022)	(0.0018)	(0.0039)
School Attendance	0.024***	0.023***	0.023***	0.022***	0.021***	0.029***
	(0.0029)	(0.0029)	(0.0031)	(0.0034)	(0.0031)	(0.0055)
Child Labor	0.000	0.001	0.000	0.002	0.004	0.009**
	(0.0024)	(0.0023)	(0.0024)	(0.0027)	(0.0025)	(0.0045)
N (millions)	7.8	7.8	4.6	3.2	7.3	0.4
R^2	0.14	0.14	0.14	0.12	0.09	0.15
Outcome Means	10.0	10.0	9.8	10.3	10.2	7.0
CS Years (Dad)	0.071***	0.015***	0.015***	0.017***	0.017***	0.006
	(0.0180)	(0.0044)	(0.0047)	(0.0047)	(0.0046)	(0.0186)
First Gen	-0.012**	-0.011**	-0.011*	-0.010*	-0.010*	-0.038
	(0.0050)	(0.0050)	(0.0057)	(0.0059)	(0.0053)	(0.0588)
Second Gen	-0.001	-0.004	-0.003	-0.005	-0.003	-0.061**
	(0.0025)	(0.0025)	(0.0027)	(0.0029)	(0.0026)	(0.0256)
Teachers per 1000	0.001*	0.001**	0.001**	0.001*	0.002**	-0.008***
	(0.0006)	(0.0006)	(0.0007)	(0.0007)	(0.0006)	(0.0022)
Literacy	-0.005**	-0.006**	-0.007***	-0.004	-0.004*	-0.005
	(0.0023)	(0.0023)	(0.0025)	(0.0027)	(0.0025)	(0.0057)
School Attendance	0.022***	0.022***	0.022***	0.020***	0.019***	0.036***
	(0.0036)	(0.0036)	(0.0040)	(0.0042)	(0.0039)	(0.0080)
Child Labor	0.004	0.004	0.001	0.008**	0.008***	-0.008
	(0.0027)	(0.0028)	(0.0030)	(0.0033)	(0.0029)	(0.0063)
N (millions)	5.2	5.2	3.1	2.1	4.9	0.3
R^2	0.14	0.14	0.14	0.11	0.10	0.16
Outcome Means	10.1	10.1	9.9	10.4	10.2	7.2

Notes: Effects of parental exposure to CS completed years of schooling for the *Children* sample, using additional state-level controls. 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, event, 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.3.3 No Child Controls

Table A10: Effect of Parental Compulsory Schooling Exposure on Children's Completed Years of Schooling

	Dependent variable: Child's Years of Schooling					
	All (Binary Treat.)	All (Cont. Treat.)	Men (Cont.)	Women (Cont.)	White (Cont.)	Black (Cont.)
	(1)	(2)	(3)	(4)	(5)	(6)
CS Years (Mom)	0.070***	0.016***	0.015***	0.018***	0.017***	-0.027**
	(0.0133)	(0.0027)	(0.0029)	(0.0028)	(0.0026)	(0.0118)
N (millions)	7.8	7.8	4.6	3.2	7.3	0.4
R^2	0.08	0.08	0.09	0.06	0.05	0.08
Outcome Means	10.0	10.0	9.8	10.3	10.2	7.0
CS Years (Dad)	0.080***	0.017***	0.015***	0.021***	0.019***	0.019
	(0.0156)	(0.0033)	(0.0036)	(0.0035)	(0.0033)	(0.0150)
N (millions)	5.2	5.2	3.1	2.1	4.9	0.3
R^2	0.08	0.08	0.09	0.07	0.06	0.07
Outcome Means	10.1	10.1	9.9	10.4	10.2	7.2

Notes: Effects of parental exposure to CS laws on completed years of schooling for the *Children* sample, by sex and race. Each column represents a different regression. The "Binary Treatment" specification in Column (1) measures CS exposure using an indicator for any non-zero CS. The "Continuous Treatment" specification in Column (2) measures CS exposure as the number of years of required schooling. Controls include parental birth year effects, parental birth state effects, parental birth region by parental birth year effects, and child sex and race fixed effects, when possible. Standard errors are clustered at the parental birth state by birth year level. *p<0.1; **p<0.05; ***p<0.01.

A.3.4 Affected Margins of Schooling

Table A11: Effect of Parental Exposure to Compulsory Schooling Laws on Children's Degree Completion

	Dependent Variables: Child Attained At Least...							
	Some GS	Grade School	Some MS	Middle School	Some HS	High School	Some College	College
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CS Years Mom	0.005	0.014	0.009	0.198***	0.273***	0.309***	0.248***	0.186***
	(0.0032)	(0.0110)	(0.0168)	(0.0476)	(0.0596)	(0.0625)	(0.0442)	(0.0298)
N (millions)	7.8	7.8	7.8	7.8	7.8	7.8	7.8	7.8
R^2	0.02	0.14	0.16	0.10	0.08	0.08	0.02	0.01
DV Mean	99.2	92.5	88.5	61.2	45.3	39.7	15.9	7.3
IV Estimate	0.2	0.7	0.4	9.4***	13.1***	14.8***	11.8***	8.9***
CS Years Dad	-0.008	0.018	0.020	0.119**	0.206***	0.250***	0.241***	0.187***
	(0.0049)	(0.0129)	(0.0181)	(0.0537)	(0.0592)	(0.0613)	(0.0396)	(0.0271)
N (millions)	5.2	5.2	5.2	5.2	5.2	5.2	5.2	5.2
R^2	0.02	0.13	0.15	0.10	0.09	0.08	0.02	0.01
DV Mean	99.3	93.0	89.1	62.8	46.6	40.9	16.0	7.3
IV Estimate	-0.6	1.3	1.5	9.1***	15.6***	19.0***	18.3***	14.2***

Notes: Effect of parental exposure to CS on enrollment into 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.

A.3.5 Endogeneity of CS Laws (Placebo Tests)

Table A12: Effect of Parental Compulsory Schooling Exposure on Children's Completed Years of Schooling: Placebo Test (Future Laws As Main Variables)

	Dependent variable: Child's Years of Schooling								
Leads:	0	3	4	5	6	7	8	9	10
CS Years (Mom)	0.020***								
	(0.0040)								
CS Years (Future)		0.012***	0.008**	0.003	0.000	-0.004	-0.006**	-0.009***	-0.011***
		(0.0037)	(0.0034)	(0.0030)	(0.0028)	(0.0028)	(0.0028)	(0.0029)	(0.0030)
N (millions)	7.8	7.8	7.8	7.8	7.8	7.8	7.8	7.8	7.8
R^2	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14
Outcome Means	10.0	10.0	10.0	10.0	10.0	10.0	10.0	10.0	10.0
CS Years (Dad)	0.017***								
	(0.0038)								
CS Years (Future)		0.010***	0.006*	0.003	0.001	-0.001	-0.002	-0.004	-0.005**
		(0.0033)	(0.0030)	(0.0026)	(0.0025)	(0.0025)	(0.0026)	(0.0026)	(0.0026)
N (millions)	5.2	5.2	5.2	5.2	5.2	5.2	5.2	5.2	5.2
R^2	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14
Outcome Means	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1

Notes: Effects of parental exposure to CS laws on completed years of schooling for the *Children* sample, using exposure to future CS laws as the variable of interest. 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.

Table A13: Effect of Parental Compulsory Schooling Exposure on Children's Completed Years of Schooling: Placebo Test (Future Laws as Controls)

	Dependent variable: *Child's Years of Schooling*								
Leads:	0	3	4	5	6	7	8	9	10
CS Years (Mom)	0.020***	0.022***	0.022***	0.022***	0.021***	0.021***	0.020***	0.019***	0.018***
	(0.0040)	(0.0046)	(0.0043)	(0.0042)	(0.0041)	(0.0041)	(0.0040)	(0.0041)	(0.0041)
CS Years (Future)		-0.003	-0.004	-0.005	-0.004	-0.005*	-0.006**	-0.007**	-0.008**
		(0.0039)	(0.0033)	(0.0029)	(0.0027)	(0.0028)	(0.0028)	(0.0030)	(0.0031)
N (millions)	7.8	7.8	7.8	7.8	7.8	7.8	7.8	7.8	7.8
R^2	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14
Outcome Means	10.0	10.0	10.0	10.0	10.0	10.0	10.0	10.0	10.0
CS Years (Dad)	0.017***	0.019***	0.018***	0.018***	0.017***	0.017***	0.017***	0.017***	0.016***
	(0.0038)	(0.0042)	(0.0040)	(0.0039)	(0.0038)	(0.0038)	(0.0038)	(0.0039)	(0.0039)
CS Years (Future)		-0.003	-0.003	-0.002	-0.002	-0.002	-0.002	-0.003	-0.002
		(0.0031)	(0.0027)	(0.0025)	(0.0024)	(0.0025)	(0.0026)	(0.0027)	(0.0028)
N (millions)	5.2	5.2	5.2	5.2	5.2	5.2	5.2	5.2	5.2
R^2	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14
Outcome Means	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1	10.1

Notes: Effects of parental exposure to CS laws on completed years of schooling for the *Children* sample, using exposure to future CS laws as control variables. 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.

A.3.6 Cohabitation and Mortality

Table A14: Robustness in Effect of Parental Exposure to Compulsory Schooling on Children's Years of Schooling: by Parent Age and Linkage Timing

	Dependent variable: Child's Years of Schooling					
	All	Exclude 1940	Only 1940	0-10 y.o. Cohabitation	Young Parents	Old Parents
	(1)	(2)	(3)	(4)	(5)	(6)
CS Years (Mom)	0.020***	0.025***	0.019***	0.025***	0.020***	-0.003
	(0.0040)	(0.0038)	(0.0050)	(0.0055)	(0.0040)	(0.0059)
N (millions)	7.8	4.9	2.9	0.5	3.2	4.5
R^2	0.14	0.13	0.16	0.15	0.14	0.13
Outcome Means	10.0	10.0	10.0	9.2	10.0	10.0
CS Years (Dad)	0.017***	0.020***	0.016***	0.023***	0.019***	-0.007
	(0.0038)	(0.0039)	(0.0049)	(0.0056)	(0.0042)	(0.0050)
N (millions)	5.2	3.5	1.7	0.4	1.7	3.6
R^2	0.14	0.14	0.15	0.17	0.14	0.13
Outcome Means	10.1	10.1	10.1	9.3	10.2	10.0

Notes: Effect of parental exposure to CS laws on years of schooling for the *Children* sample, using different timing and ways to link parents and children. 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.

A.3.7 Expanded Sample

Table A15: Effect of Parental Compulsory Schooling Exposure on Children's Completed Years of Schooling: Expanded Sample

	Dependent variable: Child's Years of Schooling				
	All	Men	Women	White	Black
	(1)	(2)	(3)	(4)	(5)
CS Years (Mom)	0.017***	0.019***	0.015***	0.014***	-0.012
	(0.0034)	(0.0034)	(0.0038)	(0.0051)	(0.0143)
N (millions)	12.7	7.7	5.0	2.6	0.3
R^2	0.18	0.19	0.14	0.09	0.14
Outcome Means	9.6	9.4	10.0	10.3	7.1
CS Years (Dad)	0.015***	0.015***	0.014***	0.017***	0.007
	(0.0029)	(0.0031)	(0.0034)	(0.0053)	(0.0216)
N (millions)	8.5	5.2	3.3	1.6	0.1
R^2	0.17	0.19	0.13	0.10	0.14
Outcome Means	9.8	9.5	10.1	10.3	7.4

Notes: Robustness in the effects of parental exposure to CS laws on completed years of schooling for the *Children* sample. Here, we expand our sample to include all children with an identified parent in one of the censuses, even if we cannot re-link the parent to the 1940 census. 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.

A.3.8 Linkage Quality

Table A16: Robustness in Effect of Parental Exposure to Compulsory Schooling on Children's Years of Schooling: by Linkage Quality

	Dependent Variable: Child's Years of Schooling				
	All	High-Quality Links	Low-Quality Links	MLP	CLP
	(1)	(2)	(3)	(4)	(5)
CS Years (Mom)	0.020***	0.021***	0.018***	0.027***	
	(0.0040)	(0.0047)	(0.0045)	(0.0056)	
N (millions)	7.8	3.8	4.5	0.5	
R^2	0.14	0.14	0.16	0.11	
Outcome Means	10.0	10.1	9.8	10.2	
CS Years (Dad)	0.017***	0.015***	0.017***	0.019***	0.019***
	(0.0038)	(0.0046)	(0.0043)	(0.0064)	(0.0062)
N (millions)	5.2	2.4	2.6	0.4	0.5
R^2	0.14	0.13	0.16	0.11	0.13
Outcome Means	10.1	10.2	9.9	10.3	10.3

Notes: Effect of parental exposure to CS laws on years of schooling for the *Children* sample, for samples with different linkage quality by migration status. 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.

A.3.9 Between-State Migration

Table A17: Robustness in Effect of Parental Exposure to Compulsory Schooling on Children's Years of Schooling: by Parental Migration Status

	Dependent variable: Child's Years of Schooling			
	All	Non-Migrant	Migrated Before Child Birth	Migrated After Child Birth
	(1)	(2)	(3)	(4)
CS Years (Mom)	0.020***	0.022***	0.003	0.031***
	(0.0040)	(0.0075)	(0.0031)	(0.0095)
N (millions)	7.8	5.2	1.8	0.8
R^2	0.14	0.15	0.09	0.14
Outcome Means	10.0	9.8	10.7	10.0
CS Years (Dad)	0.017***	0.017**	0.002	0.016*
	(0.0038)	(0.0069)	(0.0035)	(0.0094)
N (millions)	5.2	3.4	1.3	0.5
R^2	0.14	0.14	0.09	0.14
Outcome Means	10.1	9.8	10.7	10.1

Notes: Effect of parental exposure to CS laws on years of schooling for the *Children* sample, by migration status. 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.

A.3.10 Reweighing Observations

Table A18: Effect of Parental Compulsory Schooling Exposure on Children's Completed Years of Schooling: Reweighing Observations

	Dependent variable: Child's Years of Schooling					
	All	Men	Women	White	Black	Binary Treatment
CS Years (Mom)	0.020***	0.023***	0.018***	0.019***	-0.026**	0.082***
	(0.0039)	(0.0041)	(0.0043)	(0.0043)	(0.0129)	(0.0172)
N (millions)	7.8	4.6	3.2	7.3	0.4	7.8
R^2	0.14	0.14	0.12	0.09	0.15	0.14
Outcome Means	10.0	9.8	10.3	10.2	7.0	10.0
CS Years (Dad)	0.020***	0.019***	0.020***	0.018***	0.018	0.072***
	(0.0036)	(0.0040)	(0.0042)	(0.0041)	(0.0196)	(0.0168)
N (millions)	5.2	3.1	2.1	4.9	0.3	5.2
R^2	0.14	0.11	0.10	0.16	0.14	0.14
Outcome Means	10.1	9.9	10.4	10.2	7.2	10.1

Notes: Effects of parental exposure to CS laws on completed years of schooling for the *Children* sample, using inverse probability sampling weights. Each column represents a different regression. "Binary Treatment" uses an indicator variable for exposure to any CS. "Stacked DiD" uses the methodology proposed by Cengiz et al. (2019). 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.

A.4 Heterogeneity and Robustness (Intergenerational Labor Results)

A.4.1 Stacked Event Study Estimator

Table A19: Effect of Parental Compulsory Schooling Exposure on Children's Labor Market Outcomes: Stacked Event Study

	Dependent variable: *Child's labor market outcomes*							
	Wages (Log - pp)		Occ. Educ. Score		Wages (Log - pp)		Occ. Educ. Score	
	Binary	Cont.	Binary	Cont.	Binary	Cont.	Binary	Cont.
	Women:				*Men*:			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CS Years Mom	0.074	-0.003	0.170	0.088	1.479**	0.435***	0.283	0.117**
	(1.1396)	(0.2905)	(0.2372)	(0.0678)	(0.6508)	(0.1631)	(0.2046)	(0.0558)
N (millions)	4.9	4.9	13.9	13.9	15.2	15.2	20.6	20.6
R^2	0.11	0.11	0.05	0.05	0.11	0.11	0.06	0.06
Outcome Means	624.29	624.29	24.34	24.34	663.64	663.64	35.38	35.38
	Women:				*Men*:			
CS Years Dad	-0.865	-0.143	0.177	0.043	0.747	0.200	0.134	0.024
	(1.2551)	(0.3268)	(0.2569)	(0.0687)	(0.5834)	(0.1602)	(0.2080)	(0.0559)
N (millions)	1.0	1.0	3.1	3.1	3.6	3.6	4.8	4.8
R^2	0.10	0.10	0.05	0.05	0.11	0.11	0.06	0.06
Outcome Means	626.44	626.44	23.97	23.97	664.55	664.55	35.75	35.75

Notes: Effects of parental exposure to CS on *Children*'s labor market outcomes, using a stacked event study method. 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, event, 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.4.2 Additional State Controls

Table A20: Effect of Parental Compulsory Schooling Exposure on Children's Labor Market Outcomes: Additional Controls

	Dependent variable: *Child's labor market outcomes*							
	Wages (Log - pp)		Occ. Educ. Score		Wages (Log - pp)		Occ. Educ. Score	
	Binary	Cont.	Binary	Cont.	Binary	Cont.	Binary	Cont.
	Women:				*Men*:			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CS Years Mom	-0.05	0.06	0.22	0.10**	0.64	0.17*	0.15	0.05
	(0.776)	(0.171)	(0.178)	(0.041)	(0.431)	(0.097)	(0.145)	(0.034)
First Gen	0.29	0.28	0.12**	0.11**	-0.09	-0.08	-0.05	-0.05
	(0.235)	(0.234)	(0.053)	(0.051)	(0.144)	(0.144)	(0.046)	(0.046)
Second Gen	-0.06	-0.07	-0.01	-0.02	-0.06	-0.08	-0.09***	-0.09***
	(0.126)	(0.126)	(0.027)	(0.026)	(0.075)	(0.074)	(0.022)	(0.021)
Teachers per 1000	-0.02	-0.03	0.00	0.00	0.01	0.01	0.01***	0.01***
	(0.022)	(0.022)	(0.006)	(0.006)	(0.014)	(0.014)	(0.005)	(0.005)
Literacy	-0.15**	-0.15*	-0.08***	-0.07***	-0.26***	-0.26***	-0.10***	-0.10***
	(0.077)	(0.076)	(0.016)	(0.016)	(0.040)	(0.040)	(0.013)	(0.013)
School Attendance	0.24**	0.24**	0.15***	0.15***	0.23***	0.22***	0.19***	0.18***
	(0.114)	(0.115)	(0.028)	(0.028)	(0.068)	(0.068)	(0.022)	(0.022)
Child Labor	-0.04	-0.04	0.01	0.02	0.05	0.06	0.04**	0.04***
	(0.088)	(0.089)	(0.020)	(0.020)	(0.051)	(0.052)	(0.017)	(0.017)
N (millions)	1.1	1.1	3.2	3.2	3.4	3.4	4.6	4.6
R^2	0.12	0.12	0.05	0.05	0.12	0.12	0.07	0.07
Outcome Means	631.6	631.6	24.0	24.0	672.1	672.1	36.5	36.5
	Women:				*Men*:			
CS Years Dad	-1.98**	-0.11	0.21	0.11***	0.59	0.16	0.09	0.00
	(0.847)	(0.201)	(0.182)	(0.039)	(0.450)	(0.110)	(0.150)	(0.037)
First Gen	0.36	0.32	0.03	0.03	-0.29	-0.28	-0.21***	-0.20***
	(0.326)	(0.327)	(0.055)	(0.055)	(0.176)	(0.176)	(0.055)	(0.055)
Second Gen	-0.15	-0.14	-0.07***	-0.09***	-0.08	-0.11	-0.05**	-0.05**
	(0.158)	(0.166)	(0.027)	(0.028)	(0.079)	(0.079)	(0.024)	(0.024)
Teachers per 1000	0.07**	0.05	0.00	0.00	0.02	0.02	0.00	0.00
	(0.031)	(0.031)	(0.007)	(0.007)	(0.017)	(0.017)	(0.005)	(0.005)
Literacy	-0.35***	-0.31***	-0.05**	-0.04**	-0.13**	-0.13***	-0.02	-0.02
	(0.109)	(0.109)	(0.018)	(0.018)	(0.052)	(0.051)	(0.016)	(0.016)
School Attendance	0.30*	0.33*	0.18***	0.17***	0.33***	0.32***	0.16***	0.16***
	(0.171)	(0.173)	(0.033)	(0.032)	(0.100)	(0.100)	(0.028)	(0.028)
Child Labor	0.03	0.06	0.08**	0.09***	0.04	0.05	0.06***	0.06**
	(0.126)	(0.125)	(0.031)	(0.031)	(0.070)	(0.071)	(0.023)	(0.023)
N (millions)	0.7	0.7	2.1	2.1	2.3	2.3	3.1	3.1
R^2	0.10	0.10	0.05	0.05	0.12	0.12	0.07	0.07
Outcome Means	630.1	630.1	23.1	23.1	670.6	670.6	36.3	36.3

Notes: Effects of parental exposure to CS on labor market outcomes for the *Children* sample, using additional state-level controls. 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, event, 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.4.3 No Child Controls

Table A21: Effect of Parental Compulsory Schooling Exposure on Children's Labor Market Outcomes: No Child Controls

	Dependent variable: *Child's labor market outcomes*							
	Wages (Log - pp)		Occ. Educ. Score		Wages (Log - pp)		Occ. Educ. Score	
	Binary	Cont.	Binary	Cont.	Binary	Cont.	Binary	Cont.
	Women:				*Men*:			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CS Years Mom	0.10	0.08	-0.80*	-0.13	1.44	0.34*	-0.11	0.00
	(0.737)	(0.157)	(0.476)	(0.104)	(0.934)	(0.205)	(0.146)	(0.032)
N (millions)	1.1	1.1	3.2	3.2	3.4	3.4	4.6	4.6
R^2	0.07	0.07	0.03	0.03	0.07	0.07	0.04	0.04
Outcome Means	631.6	631.6	24.0	24.0	672.1	672.1	36.5	36.5
	Women:				*Men*:			
CS Years Dad	-1.52*	-0.07	-0.77*	-0.13	0.88	0.20	-0.28*	-0.06*
	(0.810)	(0.181)	(0.445)	(0.098)	(1.081)	(0.242)	(0.149)	(0.036)
N (millions)	0.7	0.7	2.1	2.1	2.3	2.3	3.1	3.1
R^2	0.05	0.05	0.03	0.03	0.06	0.06	0.04	0.04
Outcome Means	630.1	630.1	23.1	23.1	670.6	670.6	36.3	36.3

Effects of parental exposure to CS on labor market outcomes for the *Children* sample, omitting child-level controls. Each column represents a different regression. 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.

A.4.4 Endogeneity of CS Laws (Placebo Tests)

Table A22: Effect of Parental Compulsory Schooling Exposure on Children's Labor Market Outcomes: Placebo Test (Future Laws As Controls) - Wages

	Dependent variable:								
	Men - Mothers								
Leads:	0	3	4	5	6	7	8	9	10
CS Years (Mom)	0.258***	0.230**	0.249***	0.246***	0.250***	0.254***	0.258***	0.259***	0.259***
	(0.0910)	(0.1033)	(0.0941)	(0.0917)	(0.0907)	(0.0906)	(0.0910)	(0.0922)	(0.0942)
CS Years (Future)		0.042	0.017	0.032	0.034	0.032	0.017	0.018	0.007
		(0.1019)	(0.0859)	(0.0762)	(0.0734)	(0.0739)	(0.0750)	(0.0758)	(0.0761)
N (millions)	3.4	3.4	3.4	3.4	3.4	3.4	3.4	3.4	3.4
R^2	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12
Outcome Means	672.1	672.1	672.1	672.1	672.1	672.1	672.1	672.1	672.1
	Women - Mothers								
CS Years (Mom)	0.075	-0.233	-0.115	-0.033	0.020	0.053	0.074	0.081	0.085
	(0.1609)	(0.2017)	(0.1810)	(0.1695)	(0.1641)	(0.1607)	(0.1601)	(0.1617)	(0.1653)
CS Years (Future)		0.468***	0.375***	0.298**	0.241**	0.197*	0.136	0.062	0.047
		(0.1651)	(0.1366)	(0.1239)	(0.1193)	(0.1185)	(0.1211)	(0.1258)	(0.1254)
N (millions)	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1
R^2	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12
Outcome Means	631.6	631.6	631.6	631.6	631.6	631.6	631.6	631.6	631.6
	Men - Fathers								
Leads:	0	3	4	5	6	7	8	9	10
CS Years (Dad)	0.193*	0.217*	0.208*	0.210**	0.199*	0.193*	0.193*	0.196*	0.203*
	(0.1044)	(0.1208)	(0.1111)	(0.1065)	(0.1051)	(0.1046)	(0.1043)	(0.1043)	(0.1048)
CS Years (Future)		-0.038	-0.032	-0.053	-0.031	0.000	0.013	0.027	0.045
		(0.1096)	(0.0927)	(0.0819)	(0.0789)	(0.0776)	(0.0782)	(0.0758)	(0.0756)
N (millions)	2.3	2.3	2.3	2.3	2.3	2.3	2.3	2.3	2.3
R^2	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12
Outcome Means	670.6	670.6	670.6	670.6	670.6	670.6	670.6	670.6	670.6
	Women - Fathers								
CS Years (Dad)	-0.045	-0.245	-0.203	-0.136	-0.085	-0.059	-0.044	-0.033	-0.036
	(0.1900)	(0.2353)	(0.2078)	(0.1955)	(0.1914)	(0.1903)	(0.1896)	(0.1900)	(0.1920)
CS Years (Future)		0.323	0.339*	0.280*	0.209	0.165	0.149	0.115	0.041
		(0.2266)	(0.1892)	(0.1670)	(0.1581)	(0.1548)	(0.1516)	(0.1516)	(0.1537)
N (millions)	0.7	0.7	0.7	0.7	0.7	0.7	0.7	0.7	0.7
R^2	0.10	0.10	0.10	0.10	0.10	0.10	0.10	0.10	0.10
Outcome Means	630.1	630.1	630.1	630.1	630.1	630.1	630.1	630.1	630.1

Notes: Effects of parental exposure to CS laws on wages for the *Children* sample, using exposure to future CS laws as the variable of interest. 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.

Table A23: Effect of Parental Compulsory Schooling Exposure on Children's Labor Market Outcomes: Placebo Test (Future Laws As Controls) - Occupations

	Dependent variable:								
	Men - Mothers								
Leads:	0	3	4	5	6	7	8	9	10
CS Years (Mom)	0.073**	0.036	0.050	0.055	0.062*	0.068**	0.073**	0.078**	0.081**
	(0.0336)	(0.0392)	(0.0358)	(0.0343)	(0.0336)	(0.0334)	(0.0335)	(0.0338)	(0.0344)
CS Years (Future)		0.056	0.044	0.049*	0.052**	0.050**	0.046*	0.043*	0.038
		(0.0354)	(0.0292)	(0.0255)	(0.0239)	(0.0231)	(0.0235)	(0.0241)	(0.0246)
N (millions)	4.6	4.6	4.6	4.6	4.6	4.6	4.6	4.6	4.6
R^2	0.07	0.07	0.07	0.07	0.07	0.07	0.07	0.07	0.07
Outcome Means	36.5	36.5	36.5	36.5	36.5	36.5	36.5	36.5	36.5
	Women - Mothers								
CS Years (Mom)	0.125***	0.063	0.085*	0.102**	0.113***	0.121***	0.125***	0.129***	0.130***
	(0.0411)	(0.0500)	(0.0449)	(0.0430)	(0.0418)	(0.0412)	(0.0409)	(0.0409)	(0.0413)
CS Years (Future)		0.094**	0.079**	0.064**	0.057*	0.047	0.043	0.037	0.023
		(0.0406)	(0.0341)	(0.0310)	(0.0306)	(0.0301)	(0.0303)	(0.0310)	(0.0316)
N (millions)	3.2	3.2	3.2	3.2	3.2	3.2	3.2	3.2	3.2
R^2	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05
Outcome Means	24.0	24.0	24.0	24.0	24.0	24.0	24.0	24.0	24.0
	Men - Fathers								
Leads:	0	3	4	5	6	7	8	9	10
CS Years (Dad)	-0.015	-0.064	-0.053	-0.040	-0.031	-0.023	-0.013	-0.001	0.010
	(0.0363)	(0.0407)	(0.0378)	(0.0366)	(0.0359)	(0.0356)	(0.0354)	(0.0354)	(0.0357)
CS Years (Future)		0.078**	0.082***	0.077***	0.086***	0.098***	0.106***	0.111***	0.112***
		(0.0305)	(0.0262)	(0.0235)	(0.0232)	(0.0243)	(0.0251)	(0.0254)	(0.0260)
N (millions)	3.1	3.1	3.1	3.1	3.1	3.1	3.1	3.1	3.1
R^2	0.07	0.07	0.07	0.07	0.07	0.07	0.07	0.07	0.07
Outcome Means	36.3	36.3	36.3	36.3	36.3	36.3	36.3	36.3	36.3
	Women - Fathers								
CS Years (Dad)	-0.045	-0.245	-0.203	-0.136	-0.085	-0.059	-0.044	-0.033	-0.036
	(0.1900)	(0.2353)	(0.2078)	(0.1955)	(0.1914)	(0.1903)	(0.1896)	(0.1900)	(0.1920)
CS Years (Future)		0.323	0.339*	0.280*	0.209	0.165	0.149	0.115	0.041
		(0.2266)	(0.1892)	(0.1670)	(0.1581)	(0.1548)	(0.1516)	(0.1516)	(0.1537)
N (millions)	0.7	0.7	0.7	0.7	0.7	0.7	0.7	0.7	0.7
R^2	0.10	0.10	0.10	0.10	0.10	0.10	0.10	0.10	0.10
Outcome Means	630.1	630.1	630.1	630.1	630.1	630.1	630.1	630.1	630.1

Notes: Effects of parental exposure to CS laws on occupations for the *Children* sample, using exposure to future CS laws as the variable of interest. 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.

Table A24: Effect of Parental Compulsory Schooling Exposure on Children's Labor Market Outcomes: Placebo Test (Future Laws As Main Variable) - Wages

	Dependent variable:								
	Men - Mothers								
Leads:	0	3	4	5	6	7	8	9	10
CS Years (Mom)	0.258***								
	(0.0910)								
CS Years (Future)		0.206**	0.150*	0.120	0.087	0.057	0.017	-0.007	-0.041
		(0.0913)	(0.0859)	(0.0779)	(0.0749)	(0.0746)	(0.0748)	(0.0744)	(0.0727)
N (millions)	3.4	3.4	3.4	3.4	3.4	3.4	3.4	3.4	3.4
R^2	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12
Outcome Means	672.1	672.1	672.1	672.1	672.1	672.1	672.1	672.1	672.1
	Women - Mothers								
CS Years (Mom)	0.075								
	(0.1609)								
CS Years (Future)		0.302**	0.313**	0.286**	0.245**	0.203*	0.137	0.055	0.033
		(0.1354)	(0.1224)	(0.1170)	(0.1154)	(0.1175)	(0.1208)	(0.1244)	(0.1209)
N (millions)	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1
R^2	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12
Outcome Means	631.6	631.6	631.6	631.6	631.6	631.6	631.6	631.6	631.6
	Men - Fathers								
Leads:	0	3	4	5	6	7	8	9	10
CS Years (Dad)	0.193*								
	(0.1044)								
CS Years (Future)		0.109	0.069	0.012	0.004	0.014	0.011	0.008	0.010
		(0.0991)	(0.0908)	(0.0829)	(0.0794)	(0.0772)	(0.0775)	(0.0751)	(0.0747)
N (millions)	2.3	2.3	2.3	2.3	2.3	2.3	2.3	2.3	2.3
R^2	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12
Outcome Means	670.6	670.6	670.6	670.6	670.6	670.6	670.6	670.6	670.6
	Women - Fathers								
CS Years (Dad)	-0.015								
	(0.0363)								
CS Years (Future)		0.036	0.057**	0.065***	0.081***	0.097***	0.107***	0.111***	0.110***
		(0.0301)	(0.0268)	(0.0239)	(0.0234)	(0.0244)	(0.0252)	(0.0254)	(0.0257)
N (millions)	3.1	3.1	3.1	3.1	3.1	3.1	3.1	3.1	3.1
R^2	0.07	0.07	0.07	0.07	0.07	0.07	0.07	0.07	0.07
Outcome Means	36.3	36.3	36.3	36.3	36.3	36.3	36.3	36.3	36.3

Notes: Effects of parental exposure to CS laws on wages for the *Children*, using exposure to future CS laws as the variable of interest. 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.

Table A25: Effect of Parental Compulsory Schooling Exposure on Children's Completed Years of Schooling: Placebo Test (Future Laws As Main Variable) - Occupations

	Dependent variable:								
	Men - Mothers								
Leads:	0	3	4	5	6	7	8	9	10
CS Years (Mom)	0.258***								
	(0.0910)								
CS Years (Future)		0.206**	0.150*	0.120	0.087	0.057	0.017	-0.007	-0.041
		(0.0913)	(0.0859)	(0.0779)	(0.0749)	(0.0746)	(0.0748)	(0.0744)	(0.0727)
N (millions)	3.4	3.4	3.4	3.4	3.4	3.4	3.4	3.4	3.4
R^2	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12
Outcome Means	672.1	672.1	672.1	672.1	672.1	672.1	672.1	672.1	672.1
	Women - Mothers								
CS Years (Mom)	0.075								
	(0.1609)								
CS Years (Future)		0.302**	0.313**	0.286**	0.245**	0.203*	0.137	0.055	0.033
		(0.1354)	(0.1224)	(0.1170)	(0.1154)	(0.1175)	(0.1208)	(0.1244)	(0.1209)
N (millions)	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.1
R^2	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12
Outcome Means	631.6	631.6	631.6	631.6	631.6	631.6	631.6	631.6	631.6
	Men - Fathers								
Leads:	0	3	4	5	6	7	8	9	10
CS Years (Dad)	0.193*								
	(0.1044)								
CS Years (Future)		0.109	0.069	0.012	0.004	0.014	0.011	0.008	0.010
		(0.0991)	(0.0908)	(0.0829)	(0.0794)	(0.0772)	(0.0775)	(0.0751)	(0.0747)
N (millions)	2.3	2.3	2.3	2.3	2.3	2.3	2.3	2.3	2.3
R^2	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12
Outcome Means	670.6	670.6	670.6	670.6	670.6	670.6	670.6	670.6	670.6
	Women - Fathers								
CS Years (Dad)	-0.015								
	(0.0363)								
CS Years (Future)		0.036	0.057**	0.065***	0.081***	0.097***	0.107***	0.111***	0.110***
		(0.0301)	(0.0268)	(0.0239)	(0.0234)	(0.0244)	(0.0252)	(0.0254)	(0.0257)
N (millions)	3.1	3.1	3.1	3.1	3.1	3.1	3.1	3.1	3.1
R^2	0.07	0.07	0.07	0.07	0.07	0.07	0.07	0.07	0.07
Outcome Means	36.3	36.3	36.3	36.3	36.3	36.3	36.3	36.3	36.3

Notes: Effects of parental exposure to CS laws on occupations for the *Children*, using exposure to future CS laws as the variable of interest. 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.

A.4.5 Cohabitation and Mortality

Table A26: Robustness in Effect of Parental Exposure to Compulsory Schooling on Children's Labor Outcomes (Wages): by Parent Age and Linkage Timing

	Dependent variable: Child's Years of Schooling					
	All	Exclude 1940	Only 1940	0-10 y.o. Cohabitation	Young Parents	Old Parents
	(1)	(2)	(3)	(4)	(5)	(6)
			Men - Mothers			
CS Years (Mom)	0.258***	0.321***	0.161	0.126	0.299**	-0.198
	(0.0910)	(0.0890)	(0.1382)	(0.1899)	(0.1172)	(0.1767)
N (millions)	3.4	2.3	1.0	0.2	1.5	1.9
R^2	0.12	0.11	0.11	0.15	0.11	0.11
Outcome Means	672.1	685.0	643.3	676.6	662.6	679.5
			Women - Mothers			
CS Years (Mom)	0.075	0.219	0.041	-0.036	-0.195	0.204
	(0.1609)	(0.2319)	(0.1803)	(0.6819)	(0.2242)	(0.3577)
N (millions)	1.1	0.4	0.7	0.0	0.5	0.6
R^2	0.12	0.05	0.17	0.09	0.13	0.11
Outcome Means	631.6	623.0	636.0	610.2	623.5	637.9
			Men - Fathers			
CS Years (Dad)	0.193*	0.334***	-0.071	0.196	0.239	-0.081
	(0.1044)	(0.0930)	(0.1859)	(0.2443)	(0.1536)	(0.1863)
N (millions)	2.3	1.7	0.6	0.2	0.8	1.6
R^2	0.12	0.12	0.10	0.15	0.11	0.12
Outcome Means	670.6	681.9	639.4	674.4	662.2	674.7
			Women - Fathers			
CS Years (Dad)	-0.045	0.342	-0.266	0.480	0.516*	-0.368
	(0.1900)	(0.2697)	(0.2229)	(0.6821)	(0.2971)	(0.4336)
N (millions)	0.7	0.3	0.4	0.0	0.2	0.4
R^2	0.10	0.05	0.15	0.10	0.11	0.09
Outcome Means	630.1	624.0	634.5	618.0	623.2	633.7

Notes: Effects of parental exposure to CS laws on wages for the *Children* sample, using different timing and ways to link parents and children. 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 A27: Robustness in Effect of Parental Exposure to Compulsory Schooling on Children's Labor Outcomes (Occupations): by Parent Age and Linkage Timing

	Dependent variable: Child's Years of Schooling					
	All	Exclude 1940	Only 1940	0-10 y.o. Cohabitation	Young Parents	Old Parents
	(1)	(2)	(3)	(4)	(5)	(6)
			Men - Mothers			
CS Years (Mom)	0.073**	0.128***	0.018	0.183***	0.140***	0.024
	(0.0336)	(0.0314)	(0.0454)	(0.0634)	(0.0413)	(0.0528)
N (millions)	4.6	3.0	1.6	0.3	2.0	2.6
R^2	0.07	0.06	0.08	0.09	0.07	0.06
Outcome Means	36.5	38.3	33.1	35.4	35.3	37.4
			Women - Mothers			
CS Years (Mom)	0.125***	0.156***	0.078	0.123*	0.111**	0.023
	(0.0411)	(0.0289)	(0.0725)	(0.0673)	(0.0522)	(0.0711)
N (millions)	3.2	1.9	1.3	0.2	1.3	1.9
R^2	0.05	0.01	0.08	0.02	0.05	0.05
Outcome Means	24.0	14.6	37.4	12.6	24.6	23.5
			Men - Fathers			
CS Years (Dad)	-0.015	0.029	-0.063	0.081	0.058	-0.041
	(0.0363)	(0.0332)	(0.0558)	(0.0727)	(0.0490)	(0.0579)
N (millions)	3.1	2.2	0.9	0.3	1.0	2.1
R^2	0.07	0.06	0.08	0.09	0.07	0.06
Outcome Means	36.3	38.0	32.4	35.8	35.5	36.7
			Women - Fathers			
CS Years (Dad)	0.071*	0.029	0.031	0.312***	0.194**	0.028
	(0.0379)	(0.0332)	(0.0774)	(0.0804)	(0.0775)	(0.0739)
N (millions)	2.1	2.2	0.7	0.2	0.6	1.4
R^2	0.05	0.06	0.07	0.04	0.05	0.05
Outcome Means	23.1	38.0	37.0	14.9	24.2	22.6

Notes: Effect of parental exposure to CS laws on occupations for the *Children* sample, using different timing and ways to link parents and children. 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.

A.4.6 Expanded Sample

Table A28: Robustness in Effect of Parental Exposure to Compulsory Schooling on Children's Labor Outcomes: Expanded Sample

	Dependent variable:							
	Wages (Log - pp)		Occ. Educ. Score		Wages (Log - pp)		Occ. Educ. Score	
	Binary	Cont.	Binary	Cont.	Binary	Cont.	Binary	Cont.
	Women:				*Men*:			
CS Years (Mom)	0.22	0.13	0.36***	0.13***	0.66**	0.19***	0.16	0.06**
	(0.629)	(0.136)	(0.140)	(0.031)	(0.320)	(0.073)	(0.113)	(0.026)
N (millions)	1.5	1.5	5.1	5.1	5.8	5.8	7.9	7.9
R^2	0.13	0.13	0.05	0.05	0.14	0.14	0.09	0.09
Outcome Means	626.8	626.8	20.6	20.6	670.8	670.8	35.1	35.1
	Women:				*Men*:			
CS Years (Dad)	-0.90	-0.12	0.18	0.08***	0.32	0.11	-0.02	0.00
	(0.716)	(0.164)	(0.151)	(0.031)	(0.303)	(0.073)	(0.116)	(0.027)
N (millions)	1.0	1.0	3.4	3.4	4.0	4.0	5.3	5.3
R^2	0.11	0.11	0.05	0.05	0.14	0.14	0.08	0.08
Outcome Means	627.8	627.8	21.0	21.0	670.3	670.3	35.6	35.6

Notes: Effect of parental exposure to CS laws on labor market outcomes for the *Children* sample. Here, we expand our sample to include all children with an identified parent in one of the censuses, even if we cannot re-link the parent to the 1940 census. 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.4.7 Linkage Quality

Table A29: Robustness in Effect of Parental Exposure to Compulsory Schooling on Children's Wages: by Linkage Quality

	Dependent Variable: Child's Years of Schooling				
	All	High-Quality Links	Low-Quality Links	MLP	CLP
	(1)	(2)	(3)	(4)	(5)
			Men - Mothers		
CS Years (Mom)	0.258***	0.308***	0.165	0.075	
	(0.0910)	(0.1087)	(0.1031)	(0.1634)	
N (millions)	3.4	1.7	1.7	0.3	
R^2	0.12	0.11	0.13	0.11	
Outcome Means	672.1	659.6	659.0	676.8	
			Women - Mothers		
CS Years (Mom)	0.075	0.027	0.109	-1.736	
	(0.1609)	(0.1765)	(0.1776)	(1.4667)	
N (millions)	1.1	0.7	0.8	0.0	
R^2	0.12	0.17	0.15	0.18	
Outcome Means	631.6	636.3	633.5	616.9	
			Men - Fathers		
CS Years (Dad)	0.193*	0.172	0.085	0.521***	0.204
	(0.1044)	(0.1384)	(0.1325)	(0.1851)	(0.1602)
N (millions)	2.3	1.1	1.0	0.3	0.4
R^2	0.12	0.11	0.12	0.11	0.11
Outcome Means	670.6	660.2	654.5	676.7	687.0
			Women - Fathers		
CS Years (Dad)	-0.045	-0.226	-0.163	-1.367	
	(0.1900)	(0.2199)	(0.2179)	(1.6087)	
N (millions)	0.7	0.4	0.5	0.0	
R^2	0.10	0.14	0.13	0.20	
Outcome Means	630.1	635.0	632.2	617.3	

Notes: Effect of parental exposure to CS laws on the wages of the *Children* sample, for samples with different linkage quality. 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 A30: Robustness in Effect of Parental Exposure to Compulsory Schooling on Children's Occupational Scores: by Linkage Quality

	Dependent Variable: Child's Years of Schooling				
	All	High-Quality Links	Low-Quality Links	MLP	CLP
	(1)	(2)	(3)	(4)	(5)
			Men - Mothers		
CS Years (Mom)	0.073**	0.033	0.046	0.057	
	(0.0336)	(0.0391)	(0.0393)	(0.0499)	
N (millions)	4.6	2.4	2.5	0.5	
R^2	0.07	0.06	0.08	0.05	
Outcome Means	36.5	34.9	34.7	36.6	
			Women - Mothers		
CS Years (Mom)	0.125***	0.077	0.098	0.044	
	(0.0411)	(0.0739)	(0.0672)	(0.1448)	
N (millions)	3.2	1.4	2.0	0.0	
R^2	0.05	0.07	0.07	0.05	
Outcome Means	24.0	37.2	29.2	14.3	
			Men - Fathers		
CS Years (Dad)	-0.015	-0.042	-0.047	0.076	0.072
	(0.0363)	(0.0438)	(0.0471)	(0.0587)	(0.0615)
N (millions)	3.1	1.6	1.5	0.4	0.5
R^2	0.07	0.06	0.08	0.05	0.05
Outcome Means	36.3	34.9	33.8	36.9	39.6
			Women - Fathers		
CS Years (Dad)	-0.015	0.049	0.059	0.083	
	(0.0363)	(0.0768)	(0.0636)	(0.1823)	
N (millions)	3.1	0.8	1.2	0.0	
R^2	0.07	0.07	0.07	0.06	
Outcome Means	36.3	36.9	29.2	15.2	

Notes: Effect of parental exposure to CS laws on occupations for the *Children* sample, for samples with different linkage quality. 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.

A.4.8 Between-State Migration

Table A31: Robustness in Effect of Parental Exposure to Compulsory Schooling on Children's Wages: by Parental Migration Status

	Dependent variable: Child's Years of Schooling			
	All	Non-Migrant	Migrated Before Child Birth	Migrated After Child Birth
	(1)	(2)	(3)	(4)
	Men - Mothers			
CS Years (Mom)	0.258***	0.111	0.194*	0.640***
	(0.0910)	(0.1635)	(0.1109)	(0.2002)
N (millions)	3.4	2.2	0.8	0.4
R^2	0.12	0.13	0.10	0.11
Outcome Means	672.1	668.1	679.7	679.8
	Women - Mothers			
CS Years (Mom)	0.075	0.071	0.183	-0.299
	(0.1609)	(0.2435)	(0.2108)	(0.4263)
N (millions)	1.1	0.7	0.2	0.1
R^2	0.12	0.15	0.08	0.11
Outcome Means	631.6	629.5	637.8	631.3
	Men - Fathers			
CS Years (Dad)	0.193*	0.004	0.251*	0.197
	(0.1044)	(0.1723)	(0.1374)	(0.2962)
N (millions)	2.3	1.5	0.6	0.3
R^2	0.12	0.13	0.10	0.12
Outcome Means	670.6	666.7	677.6	678.1
	Women - Fathers			
CS Years (Dad)	-0.045	-0.246	0.223	-0.369
	(0.1900)	(0.2655)	(0.2781)	(0.5154)
N (millions)	0.7	0.4	0.2	0.1
R^2	0.10	0.12	0.07	0.10
Outcome Means	630.1	628.9	633.4	629.3

Notes: Effect of parental exposure to CS laws on wages for the *Children* sample, by migration status. 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 A32: Robustness in Effect of Parental Exposure to Compulsory Schooling on Children's Occupations: by Parental Migration Status

	Dependent variable: Child's Years of Schooling			
	All	Non-Migrant	Migrated Before Child Birth	Migrated After Child Birth
	(1)	(2)	(3)	(4)
	Men - Mothers			
CS Years (Mom)	0.073**	0.033	0.017	0.198**
	(0.0336)	(0.0594)	(0.0402)	(0.0820)
N (millions)	4.6	3.1	1.0	0.5
R^2	0.07	0.07	0.06	0.08
Outcome Means	36.5	35.2	39.8	38.0
	Women - Mothers			
CS Years (Mom)	0.125***	0.222***	-0.023	0.100
	(0.0411)	(0.0642)	(0.0520)	(0.0984)
N (millions)	3.2	2.1	0.7	0.3
R^2	0.05	0.06	0.04	0.05
Outcome Means	24.0	23.4	25.8	23.6
	Men - Fathers			
CS Years (Dad)	-0.015	-0.096	-0.035	0.126
	(0.0363)	(0.0586)	(0.0473)	(0.0968)
N (millions)	3.1	2.1	0.7	0.3
R^2	0.07	0.07	0.06	0.09
Outcome Means	36.3	35.0	39.6	37.7
	Women - Fathers			
CS Years (Dad)	0.071*	0.087	-0.048	0.272***
	(0.0379)	(0.0578)	(0.0655)	(0.0980)
N (millions)	2.1	1.4	0.5	0.2
R^2	0.05	0.05	0.04	0.05
Outcome Means	23.1	22.5	24.8	22.3

Notes: Effect of parental exposure to CS laws on occupations for the *Children* sample, by migration status. 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.

A.4.9 Reweighing Observations

Table A33: Robustness in Effect of Parental Exposure to Compulsory Schooling on Children's Labor Market Outcomes: Reweighing Observations

	Dependent variable:							
	Wages (Log - pp)		Occ. Educ. Score		Wages (Log - pp)		Occ. Educ. Score	
	Binary	Cont.	Binary	Cont.	Binary	Cont.	Binary	Cont.
	Women:				*Men*:			
CS Years (Mom)	0.16	0.10	0.31	0.10**	1.08**	0.29***	0.27*	0.07*
	(0.767)	(0.170)	(0.191)	(0.043)	(0.455)	(0.097)	(0.155)	(0.036)
N (millions)	1.1	1.1	3.2	3.2	3.4	3.4	4.6	4.6
R^2	0.16	0.16	0.06	0.06	0.14	0.14	0.09	0.09
Outcome Means	631.6	631.6	24.0	24.0	672.1	672.1	36.5	36.5
	Women:				*Men*:			
CS Years (Dad)	-1.41	-0.02	0.20	0.07	1.09**	0.29**	0.13	0.03
	(0.887)	(0.211)	(0.204)	(0.043)	(0.487)	(0.114)	(0.166)	(0.039)
N (millions)	0.7	0.7	2.1	2.1	2.3	2.3	3.1	3.1
R^2	0.15	0.15	0.06	0.06	0.15	0.15	0.09	0.09
Outcome Means	630.1	630.1	23.1	23.1	670.6	670.6	36.3	36.3

Notes: Effects of parental exposure to CS laws on labor market outcomes for the *Children* sample, using inverse probability sampling weights. Each column represents a different regression. The "Binary Treatment" specification measures CS exposure using an indicator for any non-zero CS. The "Continuous Treatment" specification measures CS exposure as the number of years of required schooling. Controls include child birth year effects, child birth state effects, child birth state by birth year effects, parental birth year effects, parental birth state effects, parental birth region by parental birth year effects, and child 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.

A.5 Heterogeneity and Robustness (Parental Results)

In this section, we present robustness and heterogeneity analyses for the effects of CS exposure on parental years of schooling. These tables are, for the most part, not directly referenced in the main text. These analyses are similar to the ones we conducted on the intergnerational results, in the previous section of the appendix (Appendix A.3).

A.5.1 Stacked Event Study Estimator

Table A34: Effect of Parental Compulsory Schooling Exposure on Parental Completed Years of Schooling (Stacked Event Study)

	Dependent variable: Parent's Years of Schooling						
	All (Binary Treat.)	All (Cont. Treat.)	White (Cont.)	Black (Cont.)	East (Cont.)	West (Cont.)	Midwest (Cont.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CS Years (Women)	0.069***	0.019***	0.019***	-0.042**	0.021***	0.060***	0.016***
	(0.0111)	(0.0032)	(0.0033)	(0.0208)	(0.0036)	(0.0134)	(0.0046)
N (millions)	11.1	19.6	17.3	2.3	3.9	0.6	5.4
R^2	0.11	0.12	0.05	0.09	0.04	0.14	0.03
Outcome Means	7.84	7.59	7.96	4.89	8.52	8.77	8.28
CS Years (Men)	0.031***	0.014***	0.014***	-0.013	0.013**	-0.025	0.016***
	(0.0119)	(0.0036)	(0.0035)	(0.0242)	(0.0054)	(0.0184)	(0.0047)
N (millions)	6.9	12.3	11.1	1.2	2.6	0.3	3.5
R^2	0.11	0.12	0.06	0.07	0.03	0.11	0.03
Outcome Means	7.73	7.45	7.79	4.47	8.64	8.65	8.21

Notes: Effects of exposure to CS laws on completed years of schooling for the *Parents* sample, using a stacked event study estimator. 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, event, 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.5.2 Additional State Controls

Table A35: Effect of Parental Compulsory Schooling Exposure on Parental Completed Years of Schooling

	Dependent variable: Parent's Years of Schooling						
	All (Binary Treat.)	All (Cont. Treat.)	White (Cont.)	Black (Cont.)	East (Cont.)	West (Cont.)	Midwest (Cont.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CS Years (Women)	0.061***	0.011***	0.011***	0.000	0.030***	0.018	0.011**
	(0.0136)	(0.0033)	(0.0033)	(0.0132)	(0.0036)	(0.0120)	(0.0052)
First Gen	-0.013***	-0.012***	-0.010**	-0.022	0.032***	0.017	-0.002
	(0.0046)	(0.0046)	(0.0045)	(0.0453)	(0.0116)	(0.0313)	(0.0069)
Second Gen	-0.014***	-0.016***	-0.014***	-0.035**	-0.017***	-0.028***	-0.011***
	(0.0018)	(0.0018)	(0.0018)	(0.0147)	(0.0045)	(0.0063)	(0.0024)
Teachers per 1000	0.002***	0.002***	0.002***	0.001	0.002***	0.002	0.003***
	(0.0004)	(0.0004)	(0.0004)	(0.0015)	(0.0006)	(0.0015)	(0.0006)
Literacy	-0.016***	-0.017***	-0.014***	-0.013***	-0.013	-0.024***	0.003
	(0.0014)	(0.0014)	(0.0015)	(0.0038)	(0.0089)	(0.0028)	(0.0043)
School Attendance	0.021***	0.021***	0.020***	0.021***	0.023***	0.044***	0.008
	(0.0025)	(0.0025)	(0.0026)	(0.0046)	(0.0038)	(0.0066)	(0.0059)
Child Labor	0.013***	0.013***	0.016***	0.001	-0.011***	0.005	0.005
	(0.0020)	(0.0021)	(0.0021)	(0.0037)	(0.0036)	(0.0044)	(0.0035)
N (millions)	4.3	4.3	3.9	0.3	1.0	0.2	1.9
R^2	0.12	0.12	0.05	0.10	0.05	0.19	0.04
Outcome Means	7.8	7.8	8.0	4.8	8.3	8.5	8.2
CS Years (Men)	0.044***	0.010***	0.011***	-0.018	0.047***	0.008	0.007
	(0.0158)	(0.0037)	(0.0037)	(0.0166)	(0.0058)	(0.0131)	(0.0054)
First Gen	-0.015***	-0.014**	-0.012**	-0.016	0.045**	-0.042	-0.001
	(0.0057)	(0.0058)	(0.0059)	(0.0466)	(0.0175)	(0.0310)	(0.0077)
Second Gen	-0.006**	-0.007***	-0.006**	-0.038**	-0.020***	-0.004	-0.003
	(0.0022)	(0.0023)	(0.0023)	(0.0172)	(0.0059)	(0.0068)	(0.0031)
Teachers per 1000	0.000	0.000	0.000	-0.005**	-0.001	0.000	0.002**
	(0.0005)	(0.0005)	(0.0006)	(0.0019)	(0.0009)	(0.0019)	(0.0009)
Literacy	-0.003*	-0.004**	-0.003	0.000	0.001	-0.017***	0.022***
	(0.0019)	(0.0019)	(0.0021)	(0.0049)	(0.0133)	(0.0035)	(0.0054)
School Attendance	0.025***	0.025***	0.026***	0.023***	0.028***	0.048***	0.010
	(0.0032)	(0.0032)	(0.0036)	(0.0061)	(0.0063)	(0.0085)	(0.0072)
Child Labor	0.008***	0.009***	0.014***	-0.020***	-0.008	-0.017***	-0.003
	(0.0025)	(0.0026)	(0.0028)	(0.0047)	(0.0057)	(0.0060)	(0.0051)
N (millions)	3.0	3.0	2.8	0.2	0.7	0.1	1.3
R^2	0.12	0.12	0.06	0.07	0.04	0.16	0.04
Outcome Means	7.7	7.7	7.9	4.5	8.4	8.4	8.0

Notes: Effects of exposure to CS laws on completed years of schooling for the *Parents* sample, using additional state-level controls. 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, event, 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<0.1; **p<0.05; ***p<0.01.

A.5.3 Affected Margins of Schooling

Table A36: Effect of Parental Exposure to Compulsory Schooling on Parental Degree Completion

	Dependent Variables: Parent Attained At Least...							
	Some GS	Grade School (GS)	Some MS	Middle School (MS)	Some HS	High School (HS)	Some College	College
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CS Years (Moms)	0.084***	0.013	0.027	0.108**	0.145***	0.134***	0.042***	0.037***
	(0.016)	(0.042)	(0.050)	(0.044)	(0.032)	(0.029)	(0.015)	(0.006)
N (millions)	4.3	4.3	4.3	4.3	4.3	4.3	4.3	4.3
R^2	0.07	0.14	0.14	0.04	0.02	0.02	0.01	0.00
Outcome Means	97.6	79.5	71.1	28.7	17.8	15.4	5.3	1.6
CS Years (Dads)	0.093***	0.055	0.012	-0.094**	0.032	0.038	0.018	0.059***
	(0.018)	(0.056)	(0.063)	(0.046)	(0.034)	(0.030)	(0.018)	(0.013)
N (millions)	3.0	3.0	3.0	3.0	3.0	3.0	3.0	3.0
R^2	0.06	0.14	0.13	0.03	0.02	0.02	0.01	0.01
Outcome Means	97.1	76.7	68.2	25.6	16.7	14.8	7.2	3.4

Notes: Effect of parental exposure to CS on enrollment into and completion of various schooling levels by their offspring for the *Parents* 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 $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

A.5.4 Parental Labor Market Outcomes in 1940

Table A37: Effect of Parental Exposure to Compulsory Schooling on Parental Labor Market Outcomes in 1940

	Dependent variable:									
	In Labor Force (p.p.)	Employment (p.p.)	Wage (log)	Hours Worked	Occupational Education Score (0-100)	Occupational Earning Score (0-100)	Above Median Education Score (p.p.)	Above Median Earnings Score (p.p.)	Teacher (p.p.)	Librarian (p.p.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
CS Years (Women)	-0.108***	-0.104***	0.404**	0.060**	0.019	-0.012	0.089***	0.035	0.058***	0.013**
	(0.0221)	(0.0200)	(0.1728)	(0.0250)	(0.0256)	(0.0364)	(0.0331)	(0.0552)	(0.0193)	(0.0065)
N (millions)	4.3	4.3	0.4	0.5	0.6	0.6	0.5	0.5	0.6	0.6
R^2	0.03	0.02	0.09	0.02	0.04	0.11	0.01	0.03	0.00	0.00
DV Mean	13.2	12.2	579.0	41.6	13.4	29.5	6.3	17.9	2.7	0.2
IV Estimates	-8.628***	-8.293***	27.735**	5.201*	1.277	-0.820	5.242***	2.091	3.540***	0.805*
CS Years (Men)	-0.027	-0.024	0.087	-0.032**	0.030**	0.019	0.045***	0.050	-0.001	0.000
	(0.0276)	(0.0314)	(0.1027)	(0.0148)	(0.0124)	(0.0308)	(0.0132)	(0.0424)	(0.0040)	(5.511e-04)
N (millions)	2.3	2.3	1.2	1.7	2.1	2.1	2.6	2.6	2.7	2.7
R^2	0.04	0.03	0.09	0.03	0.02	0.08	0.01	0.06	0.00	0.00
DV Mean	88.7	83.3	672.6	46.9	11.3	44.9	3.9	45.6	0.4	0.0
IV Estimates	-3.909	-3.483	16.038	-5.005	5.023	3.185	7.034*	7.813	-0.210	-0.033

Notes: Effects of exposure to CS laws on labor market outcomes for the *Parents* sample. The top panel is estimated using men and the bottom panel using women. 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. 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.

A.5.5 Endogeneity of CS Laws (Placebo Tests)

Table A38: Effect of Parental Compulsory Schooling Exposure on Parent's Completed Years of Schooling: Placebo Test (Future Laws As Main Variables)

	Dependent variable: Parent's Years of Schooling								
Leads:	0	3	4	5	6	7	8	9	10
CS Years (Mom)	0.013***								
	(0.0029)								
CS Years (Future)		0.004	-0.001	-0.005*	-0.008***	-0.010***	-0.012***	-0.013***	-0.014***
		(0.0029)	(0.0029)	(0.0028)	(0.0027)	(0.0027)	(0.0026)	(0.0026)	(0.0025)
N (millions)	4.3	4.3	4.3	4.3	4.3	4.3	4.3	4.3	4.3
R^2	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12
Outcome Means	7.8	7.8	7.8	7.8	7.8	7.8	7.8	7.8	7.8
CS Years (Dad)	0.007**								
	(0.0035)								
CS Years (Future)		0.008**	0.007**	0.005*	0.005	0.004	0.004	0.003	0.003
		(0.0036)	(0.0034)	(0.0031)	(0.0029)	(0.0029)	(0.0029)	(0.0028)	(0.0027)
N (millions)	3.0	3.0	3.0	3.0	3.0	3.0	3.0	3.0	3.0
R^2	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12
Outcome Means	7.7	7.7	7.7	7.7	7.7	7.7	7.7	7.7	7.7

Notes: Effects of parental exposure to CS laws on completed years of schooling for the *Parents* sample, using exposure to future CS laws as the variable of interest. 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.

Table A39: Effect of Parental Compulsory Schooling Exposure on Parental Completed Years of Schooling: Placebo Test (Future Laws as Controls)

	Dependent variable: *Parent's Years of Schooling*								
Leads:	0	3	4	5	6	7	8	9	10
CS Years (Mom)	0.013***	0.021***	0.019***	0.017***	0.015***	0.014***	0.013***	0.011***	0.010***
	(0.0029)	(0.0040)	(0.0035)	(0.0032)	(0.0030)	(0.0029)	(0.0028)	(0.0028)	(0.0028)
CS Years (Future)		-0.012***	-0.012***	-0.012***	-0.012***	-0.012***	-0.012***	-0.012***	-0.012***
		(0.0039)	(0.0033)	(0.0029)	(0.0028)	(0.0027)	(0.0026)	(0.0025)	(0.0024)
N (millions)	4.3	4.3	4.3	4.3	4.3	4.3	4.3	4.3	4.3
R^2	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12
Outcome Means	7.8	7.8	7.8	7.8	7.8	7.8	7.8	7.8	7.8
CS Years (Dad)	0.007**	0.004	0.005	0.006*	0.007*	0.007**	0.007**	0.008**	0.008**
	(0.0035)	(0.0043)	(0.0039)	(0.0037)	(0.0036)	(0.0035)	(0.0034)	(0.0034)	(0.0034)
CS Years (Future)		0.005	0.004	0.003	0.003	0.004	0.004	0.004	0.004
		(0.0045)	(0.0038)	(0.0033)	(0.0030)	(0.0029)	(0.0029)	(0.0027)	(0.0027)
N (millions)	3.0	3.0	3.0	3.0	3.0	3.0	3.0	3.0	3.0
R^2	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12	0.12
Outcome Means	7.7	7.7	7.7	7.7	7.7	7.7	7.7	7.7	7.7

Notes: Effects of parental exposure to CS laws on completed years of schooling for the *Parents*, by sex and race, using exposure to future CS laws as a control variable. 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.

A.5.6 Cohabitation and Mortality

Table A40: Robustness in Effect of Parental Exposure to Compulsory Schooling on Parental Years of Schooling: by Parent Age and Linkage Timing

	Dependent variable: Parent's Years of Schooling				
	All	Exclude 1940	Only 1940	Young Parents	Old Parents
	(1)	(2)	(3)	(4)	(5)
CS Years (Women)	0.013***	0.014***	0.032***	0.032***	-0.006
	(0.0029)	(0.0033)	(0.0088)	(0.0088)	(0.0087)
N (millions)	4.3	3.1	0.4	0.4	2.1
R^2	0.12	0.11	0.14	0.14	0.13
Outcome Means	7.8	7.8	9.2	9.2	7.8
CS Years (Men)	0.007**	0.007*	0.012	0.012	-0.008
	(0.0035)	(0.0039)	(0.0097)	(0.0097)	(0.0078)
N (millions)	3.0	2.3	0.4	0.4	1.8
R^2	0.12	0.11	0.16	0.16	0.11
Outcome Means	7.7	7.6	9.3	9.3	7.6

Notes: Effect of parental exposure to CS laws on years of schooling for the *Parents* sample, using different timing and ways to link parents and children. 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.

A.5.7 Expanded Sample

Table A41: Effect of Parental Compulsory Schooling Exposure on Parent's Completed Years of Schooling: Expanded Sample

	Dependent variable: Child's Years of Schooling					
	All	White	Black	East	West	Midwest
	(1)	(2)	(3)	(4)	(5)	(6)
CS Years (Mom)	0.021***	0.020***	0.031***	0.062***	0.078***	-0.005
	(0.003)	(0.003)	(0.007)	(0.008)	(0.010)	(0.003)
N (millions)	14.0	12.7	1.2	3.6	0.9	5.4
R^2	0.15	0.08	0.12	0.04	0.18	0.08
Outcome Means	8.9	9.2	5.9	9.3	9.9	9.4
CS Years (Dad)	0.018***	0.017***	0.034***	0.059***	0.056***	-0.007*
	(0.004)	(0.004)	(0.007)	(0.010)	(0.009)	(0.004)
N (millions)	12.6	11.6	1.0	3.2	0.8	5.0
R^2	0.17	0.08	0.11	0.04	0.16	0.07
Outcome Means	8.7	9.0	5.2	9.5	9.7	9.2

Notes: Robustness in the effects of parental exposure to CS laws on completed years of schooling for the *Parents*, by race and region. Here, we expand our sample to include all parents aged between 25 and 54, regardless of their child's age. 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.

A.5.8 Linkage Quality

Table A42: Robustness in Effect of Parental Exposure to Compulsory Schooling on Parental Years of Schooling: by Linkage Quality

	Dependent Variable: Parents' Years of Schooling				
	All	High-Quality Links	Low-Quality Links	MLP	CLP
	(1)	(2)	(3)	(4)	(5)
CS Years (Mom)	0.013***	0.012***	0.014***	-0.005	
	(0.0029)	(0.0030)	(0.0031)	(0.0038)	
N (millions)	4.3	3.4	2.1	0.9	
R^2	0.12	0.11	0.16	0.09	
Outcome Means	7.8	7.9	7.7	8.2	
CS Years (Dad)	0.007**	0.007**	0.010***	-0.008	0.009**
	(0.0035)	(0.0037)	(0.0035)	(0.0050)	(0.0045)
N (millions)	3.0	2.5	1.2	0.8	0.9
R^2	0.12	0.10	0.16	0.09	0.10
Outcome Means	7.7	7.8	7.5	8.0	7.8

Notes: Effect of parental exposure to CS laws on years of schooling for the *Parents* sample, for samples with different linkage quality. 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.

A.5.9 Between-State Migration

Table A43: Robustness in Effect of Parental Exposure to Compulsory Schooling on Parental Years of Schooling: by Parental Migration Status

	Dependent variable: *Parent's Years of Schooling*		
	All	Non-Migrant	Migrant
	(1)	(2)	(3)
CS Years (Mom)	0.013***	0.014***	0.004
	(0.0029)	(0.0032)	(0.0033)
N (millions)	4.3	2.9	1.3
R^2	0.12	0.14	0.10
Outcome Means	7.8	7.6	8.3
CS Years (Dad)	0.007**	0.007**	-0.002
	(0.0035)	(0.0037)	(0.0048)
N (millions)	3.0	2.1	1.0
R^2	0.12	0.14	0.09
Outcome Means	7.7	7.4	8.2

Notes: Effect of parental exposure to CS laws on years of schooling for the *Parents* sample, by migration status. 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.

A.5.10 Reweighing Observations

Table A44: Effect of Parental Compulsory Schooling Exposure on Parental Completed Years of Schooling: Reweighing Observations

	Dependent variable: Parent's Years of Schooling						
	All	White	Black	East	West	Midwest	Binary Treatment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CS Years (Women)	0.010***	0.007**	0.019	0.009**	0.077***	0.003	0.078***
	(0.0028)	(0.0029)	(0.0133)	(0.0043)	(0.0123)	(0.0035)	(0.0130)
N (millions)	4.3	3.9	0.3	1.0	0.2	1.9	4.3
R^2	0.14	0.06	0.10	0.05	0.20	0.04	0.14
Outcome Means	7.8	8.0	4.8	8.3	8.5	8.2	7.8
CS Years (Men)	0.007**	0.005	0.013	0.025***	0.047***	-0.008*	0.045***
	(0.0034)	(0.0038)	(0.0162)	(0.0050)	(0.0133)	(0.0046)	(0.0158)
N (millions)	3.0	2.8	0.2	0.7	0.1	1.3	3.0
R^2	0.15	0.07	0.08	0.05	0.15	0.04	0.15
Outcome Means	7.7	7.9	4.5	8.4	8.4	8.0	7.7

Notes: Effects of exposure to CS laws on completed years of schooling for the *Parents* sample, using inverse probability sampling weights. Each column represents a different regression. "Binary Treatment" uses an indicator variable for exposure to any CS. 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. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

A.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 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 also code missing values as zero and create missingness dummies for each control variable to avoid losing too many observations due to missingness. 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 occupation education and earning scores 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).
- Neighborhood variables: Neighborhood averages when the child was 5-14 years old.[28]

Table A45 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 55% and 36% of the effects of mothers' and fathers' exposure to CS on children's years of schooling, respectively.

[28]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 A45: Decomposition of Intergenerational Effects

	Dependent variable: Child's Years of Schooling	
	Mother	Father
	Relative contribution	
Education (Own)	3.5%	1.5%
Labor (Own)	38.5%	18.1%
Education (Spouse)	2.0%	10.1%
Labor (Partner)	2.8%	0.0%
Neighborhood	8.0%	6.2%
Total Explained	54.7%	36.0%

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

The effects of mothers' exposure to CS on children's years of schooling are explained directly by mothers' labor market outcomes (38.5%%), followed by their neighborhood choice (8.0%). Fathers' exposure to CS is mainly explained by fathers' labor market outcomes (18.1%) and their spouse's education (10.1%), hinting at assortative mating effects.

B Additional Details on Methodology

B.1 Stacked Event Study Estimator

The recent econometric literature suggests that specifications like our main TWFE models 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). With staggered implementation, the TWFE estimator may include already-treated states as part of the control group. We address this issue directly in alternate specifications following the stacked event study estimator methodology of Cengiz et al. (2019). This approach aggregates clean difference-in-difference estimates for individual expansion *events* studied in the data. We define an event e as an increase in compulsory schooling taking place in state $\widetilde{s}_e$ and first affecting birth cohort $\widetilde{y}_e$. For each event e, we consider a birth-year window of 10 years (5 years before and 5 years after a policy change), with the treatment group consisting of individuals with a parent (either mother or father depending on the specification), born in state $\widetilde{s}_e$ within 5 years of the first affected birth cohort $\widetilde{y}_e$. The control group consists of individuals with a parent born in a state that did not experience a change in compulsory schooling over the same 10-year window.[29] For the single event e, we estimate the following regression:

$$\begin{aligned} Y_i^{ce} =& \beta^c \, \Delta CS_{s'y'}^{pe} + \gamma_s^{ce} + \delta_y^{ce} + \left(\eta_s^{ce} \times \theta_y^{ce}\right) + \gamma_{s'}^{pe} + \delta_{y'}^{pe} + \left(\eta_{r'}^{pe} \times \theta_{y'}^{pe}\right) \\ &+ \lambda^c Race_i^{ce} + \mu^c Sex_i^{ce} + \epsilon_i^{ce}, \quad p = m, f \end{aligned} \tag{3}$$

Here $\Delta CS_{s'y'}^{pe}$ measures the change in compulsory schooling due to event e and takes values of 0 for all children of control parents, and for the children of treatment state parents born before $\widetilde{y}_e$. For the children of treatment state parents born after $\widetilde{y}_e$, this either takes a value equal to the increase in compulsory schooling years, or 1 to indicate a reform, depending on the specification. We also normalize the birth years used in our fixed effects with respect to the year each policy event took place, such that the first birth cohort affected by the policy is assigned a birth year of 1 and the other observations in the window are assigned birth years ranging from -4 to 5 relative to the policy. The other regressors are the same as in Equation 2. Following Cengiz et al. (2019), we construct a data set required to estimate Equation 3 for each event separately, and then stack these data sets to create one data set containing variation from all events. We then estimate this stacked version of Equation 3, with all coefficients varying by e except β^c, our main parameter of interest. While this design avoids the identification problems associated with the standard TWFE specification, it does make use of less variation, since it requires having a clean window of 10 years around each event with no changes in the treatment or control groups. Thus, while there are 54 total increases in compulsory schooling in our main data set, only 21 events are used in these stacked event study specifications.

Finally, we weigh each observation by the inverse of the number of different times it

[29] We exclude states experiencing non-monotonic changes to compulsory schooling laws during our sample period. For example, states may impose high compulsory schooling requirements initially, then reduce them and increase them again.

appears in our stacked dataset, as per Wing et al. (2024). For example, if an observation appears as a control across ten different events, the weight is assigned to it is 1/10. We do this to adjust for the fact that some observations may be duplicated many times, appearing as controls across multiple events, which may give these observations unreasonably high weights.

B.2 Instrumental Variable Estimator

In addition to our main reduced form estimates, we also estimate an instrumental variable specification using parental CS exposure as an instrument for parental education in 1940. This allows us to estimate a causal effect of parental education on children's education, and thus compare our results to those in the literature, and in particular those of Black et al. (2005). One shortcoming, which we readily acknowledge, is that the exclusion restriction may well be violated in this design. Indeed, parental exposure to CS may affect children's education through other channels beyond completed parental education. For example, as shown by Piopiunik (2014), compulsory schooling could shift parental norms about the importance of education. Even if a parent's education wasn't directly affected by their exposure to CS, higher levels of CS exposure could alter what parents view as an acceptable level of education, altering their investments in or expectations for their children. On the other hand, the IV estimates of β^c have a very natural interpretation: every additional year of parental schooling induced by the CS laws increases children's educational attainment by β^c years. Keeping concerns about the exclusion restriction in mind, we find IV estimates helpful here to provide a more interpretable scale for the reduced form estimates, and to compare with results found elsewhere in the literature.

The first stage in our IV design relates the education $Educ_i^p$ of individual i's parent p (born in state s' and year y') to that parent's own exposure to CS $CS_{s'y'}^p$:

$$Educ_i^p = \beta^p \, CS_{s'y'}^p + \gamma_{s'}^p + \delta_{y'}^p + \left(\eta_{r'}^p \times \theta_{y'}^p\right) + \epsilon_i^p \quad p = m, f \tag{4}$$

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

$$Educ_i^c = \beta^c \, \widehat{Educ_i^p} + \gamma_s^c + \delta_y^c + \left(\eta_r^c \times \theta_y^c\right) + \lambda^c Race_i^c + \mu^c Sex_i^c + \epsilon_i^c \quad p = m, f \tag{5}$$

For comparability with the previous literature, we also use a specification in which we limit the fixed effects to those used by Black et al. (2005): parent's place of residence (county) and year of birth, and child's year of birth.

C Coding Compulsory Schooling Law Exposure

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 et al. (2012). 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 et al. (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:[30]

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?

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 1890 stipulating that children aged between 8 and 14 must attend school. Suppose further that in 1905, an amendment was made to this law, extending the

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

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 1890 would be legally obliged to attend school from 1898 to 1904, amounting to a CS period of 7 years. On the other hand, consider a child born in 1895, 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 1898 to 1908. However, if they were able to acquire a work permit, they would be permitted to leave school in 1906. In this case, we conservatively consider the cohort born in 1895 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).