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

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Abstract

We estimate the intergenerational effects of compulsory schooling (CS) laws in the United States (1875 to 1940), exploiting the staggered roll-out of state CS laws and using difference-in-differences and instrumental variable (IV) approaches in a linked panel of full-count US census data. Exposure to CS laws led to comparable increases in education levels for both directly affected individuals and their children, suggesting a strong intergenerational persistence of schooling gains. These gains were largely concentrated among whites, with no significant intergenerational effects for black Americans. Within families, parental exposure to CS had the greatest effect on the eldest and least educated children. We identify several mechanisms that could explain persistence in the effects of CS: i) parental labor market outcomes (e.g. mothers sorting into higher education and higher income occupations) ii) assortative mating on education, and iii) geographic mobility to higher human capital neighborhoods.

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

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

Horace Mann (1849)

1 Introduction

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

A large literature estimates the effects of changes in CS requirements on education, earnings, and other outcomes in the United States and around the world (Lleras-Muney, 2002, Stephens Jr and Yang, 2014). These studies almost exclusively focus on the schooling and later-life outcomes of individuals directly affected by CS reforms. However, the long-run consequences of these reforms depend crucially on the extent to which their effects *persist or even compound across generations*. Policies will most successfully lift people out of poverty and promote human-capital-based economic growth when they generate lasting effects, not only for those directly exposed to the policy, but also for their children and subsequent descendants. Yet, very little is known about such intergenerational effects, precisely because of the scarcity of data linking outcomes across multiple generations. Even less is known about the channels through which these effects persist across generations. Understanding these mechanisms is important 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. We create an intergenerational dataset using full-count decennial Censuses spanning 1880-1940. We link records across decades using the matches identified by Census Tree (Buckles et al. 2023, Price et al. 2021), which provides a higher matching rate (particularly for women) than previous crosswalks. Because the 1940 Census was the first to record educational attainment, we focus on individuals who were at least 25 years old in 1940 (born before 1915) and their parents (born before 1900). The Census Tree crosswalk allows us to link records on the 1940 adults with their records in previous Censuses, including those covering years in which they resided with their parents. Census items on birth year and state of residence then allow us to measure both own and parental exposure to CS laws for this sample of 1940 adults.

We exploit the staggered implementation of state CS laws to estimate their intergenerational effects using a difference-in-differences framework. To aid the interpretation of these results, we also use parental CS exposure as an instrumental variable to estimate the causal effect of parental education on children's education. The remarkable size and richness of the linked census data permit three contributions to the literature: we (i) estimate large intergenerational effects of parental exposure to CS on offspring's completed adult educational attainment, (ii) characterize heterogeneity in these effects within families, and (iii) explore the mechanisms through which such intergenerational effects operated.

Our first contribution is to estimate the intergenerational impact of CS reforms. We find that parental exposure to CS has significant effects on completed education and related outcomes. In fact, the effect of parental exposure to CS on their children's education is larger than the effect on parents themselves. In our preferred difference-in-differences specification, we find that maternal exposure to any non-zero level of compulsory schooling increased children's schooling by 0.082 years, while any paternal exposure increased children's schooling by 0.072 years. In models where the treatment is multivalued (years of CS), we find that each year of maternal and paternal compulsory schooling exposure increased children's schooling by 0.020 years and 0.017 years, respectively. Although these reduced-form results on CS *exposure* may appear small, they imply substantial causal effects of parental *educational attainment* on child educational attainment, since not all parents were affected by changes in the CS laws. Indeed, when we use parental CS exposure as an instrumental variable for parental educational attainment, our results imply that an extra year of a mother's education increases a child's education by 0.96 years, while an extra year of a father's education increases a child's education by 1.29 years. Higher parental CS exposure - which largely affected outcomes at the bottom of the parental education distribution - led to higher rates of middle school, high school, and college attendance and graduation among children, which suggests a snowballing effect of CS laws across generations.

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

Our main results hold across a series of robustness checks designed to test several threats to identification. These include potential bias stemming from i) two-way fixed effects (TWFE) specifications, ii) the timing of CS laws relative to other educational reforms, iii) selection into cohabitation of parents and children (which helps us identify parent-child pairs), iv) sample selection bias due to different mortality rates by parental education, and v) mismatched census linkages. We address these concerns by: i) estimating a stacked event-study design to rule out bias from problematic control groups in TWFE, ii) conducting placebo tests around exposure to future CS laws (which should not have an effect on schooling), iii) measuring cohabitation between parents and children when children are very young, iv) estimating specifications on samples with different parental age restrictions, v) using census links corroborated by different linkage methodologies, and vi) reweighting our sample to better match characteristics in the 1940 Census population.

Our second contribution is to document heterogeneity in the effects of CS laws within families. In a series of dynastic specifications, we take the treated parents as the unit of observation and ask how their exposure to CS affected the distribution of education across their children. We find evidence for gender and birth-order effects in the intergenerational transmission of human capital. Parental exposure to CS disproportionately benefited first-born male children. Moreover, we find that parental CS exposure raises the education level of the *least* educated child the most, which could suggest that CS modifies norms regarding a minimally acceptable education level.

Our third contribution is to explore the mechanisms through which these intergenerational effects operate - a task 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 also 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, suggesting 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.

Finally, the fine level of geographic detail in the censuses allows us to study geographic sorting. 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. These neighborhoods tend to be in less populous localities, but are not less urban or metropolitan, possibly indicating moves out of overcrowded and congested areas to, e.g., suburbs.

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

A number of papers estimate the intergenerational effects of education reforms in European contexts (Black, Devereux and Salvanes, 2005, Chevalier et al., 2013, Dickson, Gregg and Robinson, 2016, Holmlund, Lindahl and Plug, 2011, Piopiunik, 2014, Sikhova, 2023). Using UK data, Dickson, Gregg and Robinson (2016) find that parental exposure to more CS increased test scores for teenagers. Examining multiple policies, including changes in CS laws, Chevalier et al. (2013) estimate causal effects of parental income and education on the propensity for children to acquire post-compulsory schooling. Studies like these examine the outcomes of children residing with their parents in order to match child outcomes to parental CS exposure. This data requirement necessitates a focus on childhood academic outcomes completed before the end of formal education. By contrast, the linked census data allow us to estimate the effects of parental exposure on the completed educational attainment of the offspring. Sikhova (2023) offers a rare example of a study looking at the intergenerational effects of policy on the adult outcomes of children. Using a schooling reform in Sweden as a source of exogenous variation in parental income, Sikhova (2023) estimates the contributions of parental income and education to the intergenerational correlation in earnings.

Our results also contribute to an established literature documenting factors that shape intergenerational mobility in the United States and other contexts. Several studies examine whether schooling reforms affected intergenerational mobility by estimating whether schooling reforms had larger or smaller direct effects on individuals from different socioeconomic backgrounds. However, such studies do not provide evidence on the durability of these effects across subsequent generations. Our approach, like that of Black, Devereux and Salvanes (2005), is distinct in that we explore the causal effect of reforms that affected parents on the outcomes of their children. Directly related to our work, Rauscher (2016) finds that while CS laws made school attendance more equal, they initially *reduced* intergenerational occupational mobility, although this effect subsequently vanished after about a decade. Using the full count 1940 Census, Card, Domnisoru and Taylor (2022) find that higher quality education in a state (proxied by teacher's wages) promotes greater educational mobility for the children of parents in the bottom quartile of the education distribution. Both studies examine heterogeneity in the effects of educational institutions on the outcomes of directly affected children. By contrast, we study whether CS reforms had effects across generations, specifically on the children of directly affected individuals.

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

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

2 Setting, Institutional Background and Data

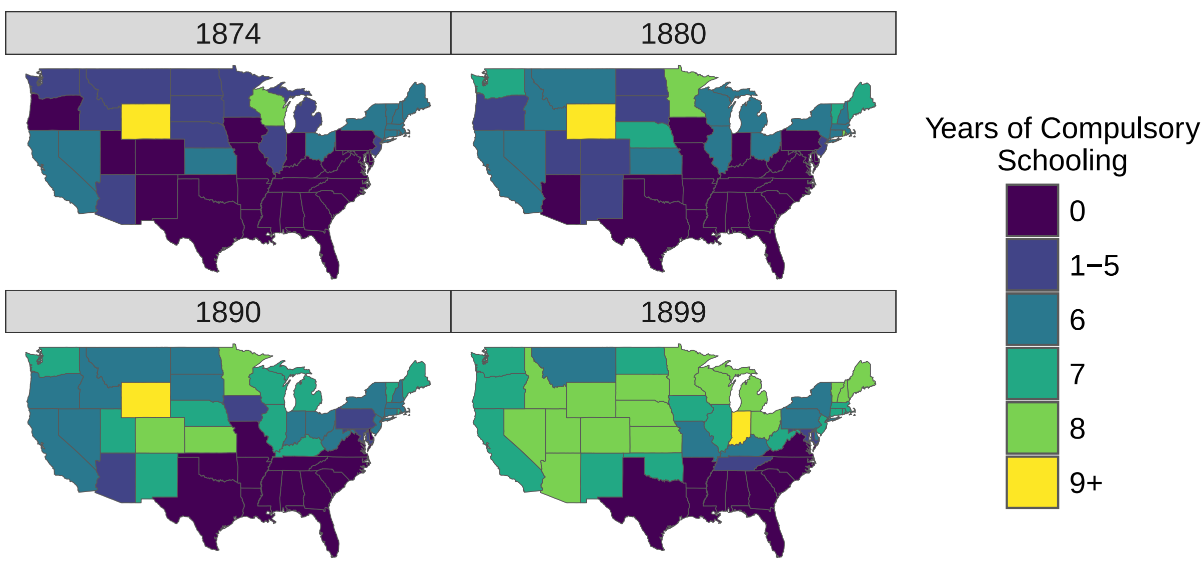

Figure 1: Compulsory schooling law exposure by state, for the 1874, 1890, 1890, and 1899 birth-year cohorts.

2.1 Compulsory Schooling Laws

Individuals born in the late nineteenth and early twentieth centuries in the United States lived through a number of substantial changes to laws regulating school attendance for children. The most common laws required children to attend school between certain ages. 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.

Using the methodology and data from Clay et al. (2012),[1] we use the timing of state laws to compute the years of legally required school attendance for each birth year-birth state cohort in our data. Appendix B provides more details. Figure 1 shows the geographic distribution of the roll-out of CS laws in the United States, based on this coding of CS laws. When cohorts born in 1874 reached the school-entry age, states in New England, and some in the Upper Midwest and Western United States, already had some form of CS law in place. The New England states in particular were early adopters of CS laws, with Massachusetts enacting the first such law in 1647. Cohorts born in 1890 in the New England states generally had to complete at least 6 years of CS and, in some cases, up to 9 years. By the time the cohorts born in 1899 were in school, the norm in most states was 6-8 years of CS, with only Southern states significantly lagging in the adoption of any CS laws.

2.2 Full Count Census Data

We measure the impact of CS laws using linked records from decennial censuses spanning the period 1880 to 1940. Individuals born in the late nineteenth century were exposed to rapidly changing CS environments during their childhoods. The linked Census data allow us to track these individuals, link them to their children, and observe the impact of CS exposure on their children's outcomes. Using the full count censuses, we identify individuals' state of birth and year of birth (which together identify their exposure to compulsory schooling) and measure a number of outcomes, such as completed education, labor market outcomes (including occupation, employment status, earnings, etc.), geographic mobility, etc.

We use the 1940 census as our main source for adult outcomes, since this is the first census that directly measures educational attainment for all individuals aged at least 25.[2] Moreover, the 1940 census is the first census in which we can plausibly measure the outcomes of the children of individuals born in the late nineteenth century at adulthood. Indeed, the parent of a 25-year-old individual in the 1940 census - born in 1914 - must be born before 1900, even if this parent was a teen at the time of their child's birth. Using an earlier census - for example, 1930 - would push back the birthdates of parents towards the mid-nineteenth century, when few states had introduced compulsory schooling laws.

2.3 Census Linkages (Census Tree)

We use a state-of-the-art census linkage database called Census Tree (Price et al. 2021, Buckles et al. 2023) to link records between 1880-1940 census waves. This crosswalk identifies many more matches and produces a more representative panel than other linkage databases. In particular, Census Tree more successfully matches records for women and black Americans,

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

[2] The 1950 census, released at the time of writing, only measures education for a subsample of respondents, while earlier censuses only measure literacy, but do not include information on educational attainment.

groups that are notoriously difficult to link. Other popular linkage databases include the IPUMS Multigenerational Longitudinal Panel - MLP (Helgertz et al., 2023) and the Census Linking Project - CLP (Abramitzky et al., 2020). Census Tree uses a variety of methods to build its database, including the MLP and CLP linkages, but also information provided by descendants of census respondents via the Family Search genealogy website and an original state-of-the-art machine learning algorithm. The Census Tree database thus subsumes linkages produced by the MLP and CLP and allows us to more directly test the influence of linkage methodology on our results. If a particular linkage is confirmed by more methodologies, we might have greater confidence that it is accurate. We can thus perform robustness checks that restrict the sample to only include records confirmed by multiple methods.

2.4 Building an Intergenerational Dataset

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.[3] 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

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

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.

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.

Table 1 presents summary statistics for our intergenerational sample separately for *Children* (Column 1), their *Mothers* (Column 3), and their *Fathers* (Column 5). We compare each of these groups to the complete set of similarly aged individuals in the 1940 census (Columns 2, 4, and 6). This provides some information on the representativeness of the sample. First, we note that close to a quarter of 1940 adults satisfying the appropriate age and birthplace restrictions show up in our *Children* sample. About one third of older women and one quarter of older men in the 1940 census appear as mothers and fathers in our sample.

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% vs 51%). This is likely due to the difficulty of linking women across censuses given the practice of changing last names upon marriage, and possibly due to lower cohabitation rates with their parents when they are older. Black Americans are also underrepresented in our samples. 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.

2.5 Parental CS Exposure and Completed Schooling Across Generations

Before estimating more fully specified econometric models, we first provide a descriptive account of the relationship between parental exposure to CS and the outcomes of both parents and children in our sample. The policies we study affected both the extensive and intensive margins of CS. Some reforms established a CS requirement when none had previously existed, while others expanded the number of required years. To ease visualization, we start with a simple binary measure of exposure to any compulsory schooling. Consider a parent j born in state s' with birth year y', and let $y^0_{s'}$ refer to the birth year of the first cohort born in state s' that could have faced any legal requirement to attend school. For each parent, we define the exposure index $E_j = y' - y^0_{s'} + 1$. That is, $E_j = 1$ indicates that j was among the first cohort in their birth state to face any possibility of compulsory schooling, while $E_j = 0$ if j was in the last cohort in their birth state to face no requirement, and so on.

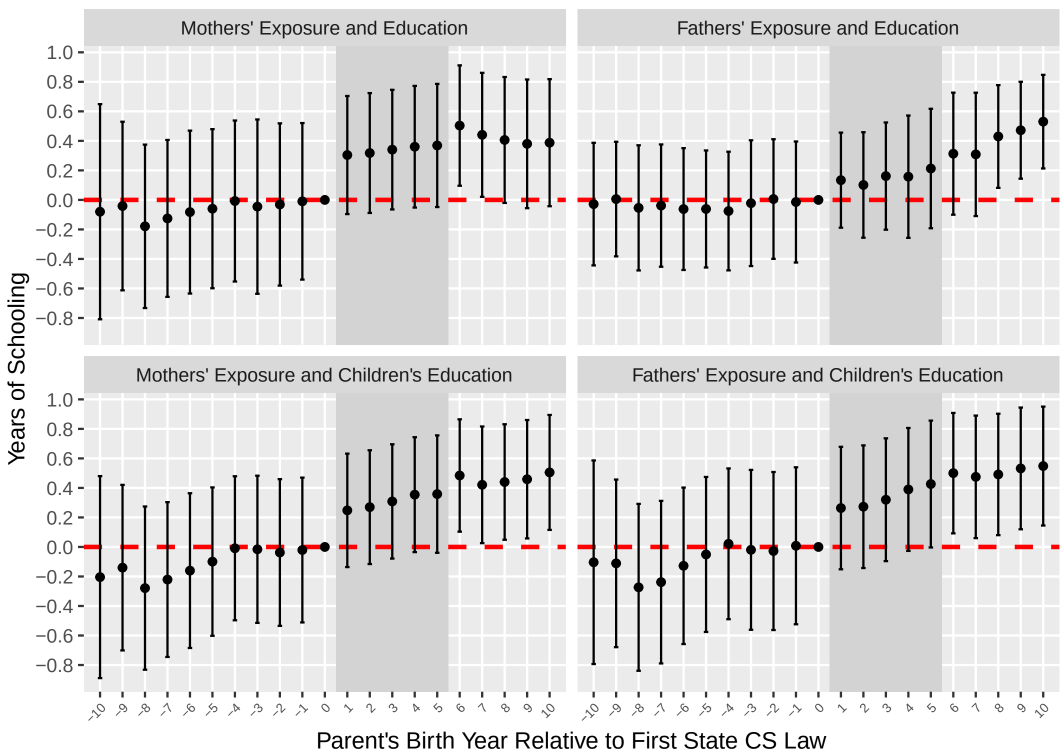

Figure 2: Event study of Parents' CS exposure and Parents' and Children's completed years of schooling.

Figure 2 presents event study plots showing the relationship between average own education and average children's education for parents in our sample with different exposures E_j. The top panels show that parents who were part of the first cohorts exposed to compulsory schooling in their state of birth (cohort 1 and beyond), completed significantly more years of schooling than the last unexposed cohort (cohort 0, and reference point), ranging from 0.30 - 0.50 for mothers in our sample to 0.10 to 0.53 for fathers.[4] The bottom panels repeat this

[4]We cluster standard errors at the level of the treatment, i.e. birth state × birth year level. The gray

exercise, but now plot the average years of schooling for the children of each parent j. These plots suggest that parental exposure to compulsory schooling had large and significant effects on their children's completed schooling. There is a clear jump in the average education of children born to parents with any exposure, relative to those born to parents with no exposure. Compared to parents born in the last cohort unexposed to CS, the children of parents exposed to any CS complete an additional 0.25 - 0.55 years of schooling. This applies to both maternal and paternal exposure to CS, which may be surprising at first glance, given the more modest effect of CS on fathers' education levels. However, as we will show in later sections, fathers who are exposed to CS are also more likely to marry women who are more educated and more exposed to CS.

3 Empirical Strategy

3.1 Two-Way Fixed Effects

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 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 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_j^p include own (parental) years of completed education, $Educ_j^p$ as well as summary measures of the educational attainment of their children (e.g. average education among their children, $AvgEduc_j^{child}$). Our basic parent-level specification is:

$$Y_j^p = \beta^p\, CS^p_{s'y'} + \gamma^p_{s'} + \delta^p_{y'} + \left(\eta^p_{r'} \times \theta^p_{y'}\right) + \lambda^p Race_j^p + \mu^p Sex_j^p + \epsilon_j^p, \quad p = m, f \qquad (1)$$

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 ($\eta^p_{r'}$)[5] 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

area of the event study plots shows the effects on parents who were exposed to 1 to 5 years of compulsory schooling. In most states, the first CS law consisted of a minimum of 6 years of compulsory schooling (i.e., primary schooling). A 1 to 5-year exposure to CS indicates that these are cohorts who would have been of school age when the first CS law was passed and may have i) never been enrolled in school in the first place, ii) may have dropped out before the law came into effect, or iii) may have reached the minimum school-leaving age before completing six years of schooling. Not surprisingly, the effects of CS on these cohorts are more muted than on subsequent cohorts, which were exposed to CS since first grade.

[5] East, South, Midwest and West.

separate specifications for mothers and fathers.

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

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

Here we include all the 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 to mothers (m) and fathers (f). We cluster the standard errors at the child birth-state by child birth-year level.

Parental CS exposure and child CS exposure are naturally correlated, since many children will be born in the same state as their parents and face similar CS laws. The full set of interactions between child birth state and child birth year in our main specification absorbs the effects of the child's own direct exposure to CS laws. Thus, we identify the intergenerational effect of parental exposure to CS laws, β^c, by comparing children born in the same state and in the same year, but whose parents were exposed to different levels of CS. This variation arises because (1) the parents were born in the same state, but belong to different birth cohorts and faced different legal regimes within that state, (2) the parents were born in the same year, but in different states with different CS requirements, or (3) the parents grew up in different CS regimes due to differences in both state of birth and birth cohort.

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

The second challenge is highlighted by Stephens Jr and Yang (2014) who suggest that the standard assumption of common trends across states is generally not valid. Stephens Jr and Yang (2014) find that estimated effects of own exposure to CS on downstream outcomes (e.g. wages) are often not robust to controlling for region-level time trends. In other words, the effects measured in the CS literature may be driven by regional time trends, which are then incorrectly attributed to CS laws. Controlling for parental region of birth by birth year interactions helps address this challenge.

3.2 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.[6] 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.

3.3 Instrumental Variable Specification

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, in particular, those of Black, Devereux and Salvanes (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 ex-

[6] 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.

ample, 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, Devereux and Salvanes (2005): parent's place of residence (county) and year of birth, and child's year of birth.

4 Results

4.1 Main Specifications

Effect of Parental Exposure to CS on Parental Outcomes: Before turning to our main intergenerational results, we first estimate the direct effect of CS reforms on the educational attainment of *Parents* directly subject to these laws, as in Equation 1. The top and bottom panels of Table 2 display separate estimates for women (mothers) and men (fathers), respectively. Column (1) presents estimates when the main regressor of interest is a binary indicator for exposure to any non-zero compulsory schooling. We find that any exposure to CS increased own educational attainment by 0.078 years for women and 0.032 years for men. In Column (2) the treatment variable of interest is set to years of compulsory schooling. Our results indicate that each additional year of CS exposure increased own educational attainment by 0.013 years of schooling for women and 0.007 years for men. The subsequent columns of Table 2 present estimates of the model in Column (2) for different subsamples. The effects of CS exposure were largely concentrated among whites - we find no statistically significant effects for black men or black women, but given larger standard errors, we cannot rule out similar-sized effects. Across geographic regions, compulsory schooling laws had the largest effects in the East and especially the West, with no evidence of an effect in the Midwest. We also cannot estimate an effect for the South, where states had not yet passed any CS laws affecting our parents' cohorts (with the exception of Tennessee).

The parental own-effects we estimate are generally consistent with previous studies on

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) 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.

this topic. This literature tends to show that CS has positive impacts on schooling for white women more than for white men, while finding no effects for black Americans. It also finds that more recent compulsory schooling laws were more effective than earlier laws.[7] Our results are consistent with these estimates, especially considering that our sample of parents (born between 1874 and 1899), is relatively old and was thus exposed to the first, less effective CS laws. In Appendix Table A1, we confirm that the timing of the laws, and

[7]For example, Clay, Lingwall and Stephens Jr (2021) find estimates between 0.008 and 0.027 years per CS years for men born between 1885 and 1912 in the 1940 census. Lleras-Muney and Shertzer (2015), using cohorts born between 1901 and 1925 and the 1960 census, find increases of 0.046 and 0.062 years of schooling per year of CS for white men and white women, respectively, while finding no effects on black men and women. Oreopoulos, Page and Stevens (2006) find estimates roughly between 0.04 and 0.06 years per CS year on parents who cohabit with children in the 1960, 1970, and 1980 censuses (and are thus exposed to more recent CS laws), without controlling for regional trends in the outcome variable.

not the particularities of selection into our child-parent sample via cohabitation, is driving these differences. We estimate our first stage results using a younger sample consisting of 1940 parents aged 25 to 54, similar to the one used in Clay, Lingwall and Stephens Jr (2021). We find that using this sample of parents exposed to more recent CS laws yields results that are larger in magnitude.

Interpreting the magnitude of the estimates in Table 2 can be challenging, since they reflect average treatment effects across entire populations. We highlight two factors that explain the seemingly modest average effects of CS exposure in our *Parents* sample. First, for many individuals, changes in CS laws were not binding and were thus not expected to have an effect. In Appendix Table A2, we characterize the population treated by the CS laws. We find that each time a new compulsory schooling law was passed, only an average of 12-14% of children were quitting school earlier than the newly-mandated minimum. Thus, these laws were aimed at a relatively small fraction of low-achieving students. For black women and men, these figures were higher (24% and 28%), consistent with lower educational attainment of this demographic.

A second factor to consider when interpreting the average effects in Table 2 is enforcement or effectiveness - even when states increased CS requirements, these reforms were not always implemented perfectly or with total compliance. To better understand the *effectiveness* of the schooling laws in producing schooling gains, we next measure them against *potential* schooling gains. To do this, we create a counterfactual "perfect enforcement" scenario, in which we impose on all individuals in our data that they stay in school up to at least the mandated years of CS in their state s' for their specific cohort y'. Next, we re-estimate the main effect of own exposure to CS on these simulated years of schooling (Equation 1) to obtain effect sizes under perfect enforcement of CS laws.[8] This provides us with a benchmark to which we can compare the observed effects of the CS laws.

In Appendix Table A2, we find that even under perfect enforcement, each additional year of CS exposure would have increased years of schooling by 0.10 years for women and 0.12 for men. In reality, CS laws achieved 13% of these potential educational gains for women, and only 4% for men, although regional and racial heterogeneity in that estimate is substantial. Black women captured only 3% of the potential schooling gains expected from CS schooling, while black men only 0.5%, highlighting the racial disparities in the provision of education during this period. On the other hand, women in the Eastern and Western regions of the US captured 23% and 31% of the potential gains, respectively, highlighting once again the more robust implementation of CS laws in these regions. This is similar for Eastern and Western men, who achieved gains equivalent to 28% and 21% of the "perfect enforcement" counterfactual.

Effect of Parental Exposure to CS on Offspring Outcomes (Intergenerational): 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

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

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

	Dependent variable: Child's Years of Schooling							
	TWFE						Stacked Event Study	
	All (Binary Treat.)	All (Cont. Treat.)	Men (Cont.)	Women (Cont.)	White (Cont.)	Black (Cont.)	All (Binary)	All (Cont.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CS Years (Mom)	0.082***	0.020***	0.023***	0.017***	0.019***	-0.026**	0.058*	0.018**
	(0.0172)	(0.0040)	(0.0042)	(0.0042)	(0.0043)	(0.0128)	(0.0302)	(0.0087)
N (millions)	7.8	7.8	4.6	3.2	7.3	0.4	12.9	34.5
R^2	0.14	0.14	0.14	0.12	0.09	0.15	0.14	0.14
Outcome Means	10.0	10.0	9.8	10.3	10.2	7.0	9.9	9.8
CS Years (Dad)	0.072***	0.017***	0.018***	0.016***	0.016***	0.020	0.079***	0.021***
	(0.0168)	(0.0038)	(0.0042)	(0.0042)	(0.0042)	(0.0168)	(0.0271)	(0.0065)
N (millions)	5.2	5.2	3.1	2.1	4.9	0.3	7.8	20.4
R^2	0.14	0.14	0.14	0.11	0.10	0.16	0.13	0.16
Outcome Means	10.1	10.1	9.9	10.4	10.2	7.2	10.0	9.9

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 Column (2) measures CS exposure as the number of years of required schooling. The "Stacked Event Study" specifications use the methodology proposed by Cengiz et al. (2019). 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.

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 set to the number of years of compulsory schooling required of parents. We again find that 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.

As discussed in Section 3, the TWFE estimator can be biased in settings with staggered implementation and heterogeneous treatment effects. For a binary treatment adopted at different times by different units, the TWFE estimator can be decomposed into a weighted average of various 2×2 TWFE estimates based on a single treated unit and a single control group. Bias can arise because some of these 2×2 estimates could be given negative weights, and some may involve problematic comparisons using already treated units as controls. To understand whether this is likely to bias our estimates here, we apply the decomposition method of Goodman-Bacon (2021) to our binary specification in Column (1) of Table 2 and Column (1) of Table 3.[9] We present the results of the decomposition in Appendix Tables A3 and A4. This analysis suggests that our results do not suffer from a negative weighting problem. Moreover, our results are largely driven by comparisons using never-treated and always-treated states as controls. The problematic comparisons between later-treated states using earlier-treated states as controls are assigned relatively small weights in our estimates (10%) and on average these comparisons suggest *negative* treatment effects. Thus, our estimates of the effects of CS laws on schooling are likely conservative.[10]

An additional step in addressing the challenges associated with the TWFE is to estimate stacked event-study estimates as in Cengiz et al. (2019) (see section 3.2). Column (7) of Table 3 presents estimates of the stacked event-study estimator when the treatment of interest is any parental CS exposure. Having a mother (father) exposed to any compulsory schooling increased children's years of schooling by 0.058 (0.079) years of schooling. In Column (8), we present estimates from this design when the treatment of interest is extra years of compulsory schooling mandated. Here each extra year of CS exposure by a mother (father) increased children's years of schooling by 0.018 (0.021) years. It is difficult to directly compare the stacked event-study estimates to the baseline TWFE estimates, since the stacked estimates rely on a smaller set of reform events for identification. Nevertheless, the estimates in Columns (1)-(2) are reasonably similar to those in Columns (7)-(8), suggesting that our main results are unlikely to be driven by the kinds of biases associated with TWFE.

4.2 Instrumental Variables Results

We now turn to our IV design and use parental CS exposure as an instrumental variable to estimate the causal effect of parental schooling on children's schooling. This approach rescales the estimates to calculate the effect of parental education on children's education, making it easier to interpret the magnitudes of our reduced form estimates. However, the previously discussed problems with the exclusion restriction threaten a clean LATE interpretation. Thus, although TWFE is our preferred approach, we report IV estimates where possible to facilitate the interpretation and comparison of results with the existing literature.

Table 4 presents our second-stage IV results (see Appendix Table A5 for the first-stage results). Column (1) replicates the exact instrumental variables (IV) specification used by Black, Devereux and Salvanes (2005). The other columns show our own specifications with additional controls (afforded by our very large Census sample sizes) and for different demographic groups and time periods. Across various specifications and samples, the effects are an order of magnitude larger than those of Black, Devereux and Salvanes (2005), who found

[9]To efficiently implement this, we aggregated our data at the birth state - birth year level for tractability. This precludes us from using sex, race and other individual controls.

[10]In other words, schooling levels increased more in earlier-treated states compared to later-treated states, which is in line with our argument that CS laws raised education levels. This also means that our main TWFE estimates are slightly biased towards zero by using these comparisons.

Table 4: 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.

essentially null or weak effects[11] for a 1960 reform in Norway and concluded, as a result, that the strong intergenerational correlation in education between parents and offspring reflects selection rather than causal effects of parental education (at least in that setting). 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. We revisit possible explanations for this in the Mechanisms section 5.

[11] 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.

4.3 Degrees and Attainment Margins

Thus far, we have treated educational attainment as a continuous variable without distinguishing between levels of schooling or degrees. One extra year of schooling may have very different effects on downstream outcomes depending on whether or not it represents the completion of a degree. To understand the intergenerational effects of parental CS exposure - and what it reveals about the process of intergenerational transmission - it is useful to estimate the impacts of CS laws on enrollment into, and graduation from, different levels of schooling for the children of treated parents.

Appendix Table A6 presents results in which we re-estimate our basic *Child* Sample regression (Equation 2), but instead of analyzing completed years of schooling as the dependent variable, we instead use a series of dummy variables indicating whether or not individuals had at least: some grade school (1 to 5 years of schooling), completed grade school (6 years), some middle school (7 or 8 years), completed middle school (9 years), some high school (10 or 11 years), completed high school (12 years), some college (13 or 14 years), and completed college (15 years and above). Parental exposure to CS significantly increased children's propensity to enroll in and graduate from middle school, high school and college. Both maternal and paternal exposure matter, with maternal effects typically larger in magnitude. We find null effects on lower levels of degree attainment.

Different channels linking parents and children should generate different sets of results. It could be that parental completion of a degree establishes a floor for the expected educational attainment of the children. Parents may wish to ensure their children obtain at least as much formal schooling as they themselves were legally obligated to obtain. If this is true, we might expect intergenerational effects to be largely confined to the lower end of the distribution of completed outcomes. Alternatively, increases over time in mandated levels of schooling could increase the chances that children obtain substantially more educational attainment than their parents. This could arise for a variety of reasons. For example, higher parental incomes could reduce the opportunity cost of children spending time on education, or could free up time for greater parental investment (through income effects). Alternately, higher levels of compulsory education could increase the value that parents place on educational success, as argued by Piopiunik (2014) in the German context.

Since high school graduation and college enrollment and graduation were never margins specifically targeted by CS laws, it seems that there are more complex mechanisms in place beyond simply raising the floor of expected education. CS exposure may increase resources available to, and/or change decision-making in, the household in such a way that the effects "snowball," shifting the educational distribution of subsequent generations above and beyond minimum-mandated levels. Results for paternal and maternal transmission are similar, which, as we will see later, could be driven by parental assortative mating on education or other mechanisms, even if compulsory schooling had small effects on paternal years of schooling. In Section 5, we return to these questions and make use of the richness of the Census data to test for the presence of several different mechanisms.

4.4 Dynastic Specifications and Within-Family Heterogeneity

Up to this point, we have thought of parental exposure to CS as a treatment that is quasi-randomly assigned to different children with different intensities. This focus on the child as

Table 5: 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.

the unit of analysis creates at least two interpretational challenges. First, fertility may be affected by CS, potentially creating a selection issue in our *Children* sample. On the one hand, this could be part of a genuine intergenerational causal effect - more education could change fertility and alter investment per child, for example. Additionally, pure selection could generate child-level results spuriously if CS laws do not actually affect child outcomes, but differentially increase fertility for positively selected families, for example.

A second challenge with taking the child as the unit of analysis is that it ignores the fact that educational investments are made at the household level. The effects of parental exposure to CS on children from the same household will be jointly determined. More parental resources could increase one child's completed education while leaving that of another unaffected. It could even be that greater resources increase one child's completed educational attainment at the expense of another child's attainment. Household choices could thus create complex patterns of heterogeneity in the effects of parental exposure to CS on child outcomes. It may therefore be useful to think of this intergenerational problem at the dynastic level.

We now shift the unit of analysis to the level of the family dynasty and ask whether parental exposure to CS changed different features of the distribution of educational attainment among their children. Specifically, we take the parent as the unit of analysis and estimate specifications that follow Equation 1, but now consider the effect of exposure to CS on the following family level outcomes: the maximum, minimum, and average years of schooling of their children and the years of schooling of the eldest and youngest sons and daughters. These results are presented in Table 5.

The first column in Table 5 suggests that one extra year of parental exposure to CS increases the average educational attainment of children by 0.016 years for mothers and 0.018 years for fathers. Reassuringly, these estimates are quite consistent with the child-level estimates found in Table 3, suggesting that our main intergenerational results are not driven by the kinds of selection biases discussed previously, or by particular choices for child-level controls in Equation 2.

The subsequent columns of Table 5 break down these effects by birth order and sex of children. We present evidence that oldest (first-born) children benefit more from parental exposure to CS than the youngest children in a family. This result is noteworthy given the existing literature on birth-order effects, which finds that first-born children tend to have higher levels of educational attainment, skills, and better adult outcomes - perhaps because of greater levels of parental investment (Black, Grönqvist and Öckert, 2018, Pavan, 2016). Our results suggest that first-born children also appear to gain more from parental exposure to CS. This suggests a complementarity between parental exposure to CS and the inputs or environmental factors that give rise to first-born effects.

Additional results in Table 5 show that the effects of parental exposure to CS on the attainment of the least educated sons and daughters in a family are at least two times larger than those of the most educated sons and daughters. This may reflect a number of behaviors. For example, it could be the case that CS exposure anchors parental expectations about the minimum level of education their children should obtain, thus naturally affecting the least educated children. Alternately, the extra resources provided by greater parental CS exposure could be used purposely by the family to reduce educational inequalities across children.

5 Mechanisms

Theoretically, parental exposure to CS could influence children's outcomes in many ways. The Census data allow us to empirically test for three important groups of explanations: (1) parental labor market outcomes, (2) family structure / assortative mating, and (3) neighborhood characteristics. Of course, other channels may be important, like the influence on norms or expectations suggested by Piopiunik (2014), but they are not well measured in the Census data and are therefore beyond the scope of this paper.

5.1 Parental Labor Market Outcomes

Table 6: Effect of Parental Exposure to Compulsory Schooling on Parental Labor Market Outcomes

	Dependent Variable:							
	In Labor Force (p.p.)	Employment (p.p.)	Occupational Education Score (0-100)	Occupational Earnings 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)
CS Years (Women)	-0.108***	-0.104***	0.083***	0.046	0.120***	0.141**	0.066***	0.005
	(0.0221)	(0.0200)	(0.0299)	(0.0432)	(0.0379)	(0.0632)	(0.0240)	(0.0061)
N (millions)	4.3	4.3	0.5	0.5	0.5	0.5	0.5	0.5
R^2	0.03	0.02	0.05	0.13	0.01	0.03	0.01	0.00
DV Mean	13.2	12.2	13.3	28.3	6.3	16.9	3.4	0.2
IV Estimates	-7.97***	-8.02***	3.47***	1.92	5.00***	5.85**	2.83***	0.21
CS Years (Men)	-0.049**	-0.036	0.018	-0.038	0.046***	0.011	0.003	0.000
	(0.0244)	(0.0280)	(0.0120)	(0.0322)	(0.0125)	(0.0475)	(0.0036)	(3.219e-04)
N (millions)	3.0	3.0	2.6	2.6	2.6	2.6	2.7	2.7
R^2	0.04	0.03	0.02	0.09	0.00	0.07	0.00	0.00
DV Mean	88.4	82.8	11.5	46.2	3.7	44.6	0.4	0.0
IV Estimates	-5.81*	-6.79	2.55	-5.35	6.41**	1.56	0.49	0.01

Notes: Relationship between parental exposure to more CS and labor market outcomes. 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.

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.

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 6 presents estimates of Equation 1 for several important outcomes using data on this 1930 subsample. 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*. 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 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.[12] 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.[13]

5.2 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 7, 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.[14] 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 in the probability of remarriage conditional on divorce.

Columns (4)-(6) of Table 7 examine the effects of CS exposure on cross-sectional marriage outcomes for *Parents* in the 1940 Census. These require some interpretive care, since

[12] 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.

[13] These results also hold in 1940 (Table A15 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.

[14] 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 7: Effect of Parental Exposure to Compulsory Schooling on Assortative Mating, Marriage and Fertility

	Dependent Variable:								
	Lifetime Outcomes:			***1940 Outcomes:***					
	Number of Children	Age First Marriage (Years)	Ever Remarried (p.p.)	Married (p.p.)	Widowed (p.p.)	Separated (p.p.)	Spouse Age Difference (Years)	Spouse CS Exposure (Years)	Spouse Schooling (Years)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(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.

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 7 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, Price and Ward (2023), who document strong assortative mating patterns in the early twentieth century in the United States.

5.3 Neighborhood Sorting

Table 8: 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 characteristics. The neighborhood outcomes are standardized to be zero mean with unit standard deviations. 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.

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 2018*a*,*b*, Chetty, Hendren and Katz 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.[15] Then, for each parent in our sample, we identify the first census in which they were cohabitating with a child in our sample and the enumeration district they were inhabiting. We

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

then study the relationship between parental CS exposure and the characteristics of this enumeration district, using a specification similar to Equation 2.

In Table 8, 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 inhabitants of these neighborhoods. 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.

Mechanisms Summary Taken together, the analyses in this section highlight some important mechanisms that could be driving 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 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 about from the characteristics of the child's other parent, through assortative mating. Finally, neighborhood characteristics could be one pathway linking all of these mechanisms to child outcomes. Parents exposed to more education resided in neighborhoods with characteristics that appear to be human capital enhancing.

6 Additional Specifications and Robustness Exercises

We perform several robustness checks to assess whether our results are affected by the following: (i) timing of compulsory schooling laws relative to other reforms, (ii) selection into the intergenerational sample based on cohabitation patterns, (iii) mortality selection, (iv) systematic census linkage errors, and (v) selective migration. Last, (vi) 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 Section A.3 of the Appendix, we present an expanded set of results showing robustness checks for specifications on parental own schooling.

6.1 Timing and Endogeneity of Compulsory Schooling Laws: Placebo Tests

One concern is that our results do not reflect the causal effects of changes in compulsory schooling laws, but rather the effects of other policy changes or changes in state-level economic conditions that are correlated with CS reforms. To some extent, our existing controls (especially region - birth year effects) mitigate this concern. Nevertheless, this issue may still exist if within regions, states increasing CS also increased educational spending or undertook other reforms at the same time. Placebo tests can help diagnose this issue by determining how specific our results are to the exact timing of the CS reforms.

If our results reflect correlation between parental CS exposure and other reforms or processes unfolding over time, then we might estimate positive coefficients even if we mis-specify the coding of the laws. In particular, we estimate the effect of parental exposure to *future* CS reforms on the educational attainment of children. If variation in CS laws simply captures some positive time trends in educational attainment in some states, we would expect to find positive relationships between future CS laws and children's schooling levels. We use schooling laws 3 to 10 years in the future in two separate placebo tests (we expect leads of 1 or 2 years to be highly correlated with actual laws). In the first, we replace CS exposure with exposure to future CS laws. In the second one, we use these future laws as controls and observe whether the significance of our results survives the inclusion of these controls.

In Table A7, we re-estimate Equation 2 with child years of schooling as the dependent variable. Column (1) presents our baseline estimates from Table 3 (zero lag). The successive columns of Table A7 present estimates where we replace our main treatment variable of interest with the years of CS required of someone born in the parent's state X years later than this parent. Even with relatively small leads (e.g., 3 years in the future), our estimate drops by nearly half. For leads of 5-7 years, we find no statistically significant coefficient for using either mother's or father's exposure. At very long leads (e.g., 10 years), we actually estimate statistically significant *negative* coefficients, though these are much smaller in magnitude than our main estimated results. In Table A8 we present results where we add leads of parental CS exposure as additional controls to our main specification. We find that the coefficient on true parental CS exposure is remarkably robust. Taken together, these results suggest that the positive effects we find are quite specific to the actual timing of CS reforms faced by the parents in our sample. If anything, the negative estimates for long leads in Table A7 suggests that parental exposure could be *negatively* correlated with future reforms or trends that positively impacted the children of our sample, implying that our estimates may be conservative.

6.2 Sample Selection: Cohabitation

Recall that we construct parent-child linkages using two separate methods: either going backward in time from 1940 to find a Census where a 1940 adult was cohabiting with their parents in childhood, or by directly finding adults who were cohabiting with their parents in 1940. Since a large fraction of our sample (37% and 32% of our mother-child and father-child links, respectively) is composed of 1940 cohabitation matches, one might be worried that the individuals who live with their parents as adults are unrepresentative, and could be selected in ways that bias our results. Indeed, in 1940, the parents in our sample were between 40 and 65 years old, while the children were between 25 and 50 years of age. Cohabitation at these ages could indicate poor parental (or child) health, adverse parental (or child) labor market outcomes, particular family norms, etc. Any one of these factors could bias our results if they are systematically correlated with parental exposure to compulsory schooling and the educational attainment of children.

We perform three robustness tests to alleviate these concerns. First, we estimate our results using only children who we link to their parents via cohabitation in censuses excluding the 1940 census. This addresses the concern that there could be pronounced selection into cohabitation in the 1940 census between adult children and their elderly parents, in particular by income and education. Second, we estimate our model using only the 1940 cohabiting child-parent pairs, to show that these results are similar to our main results, ruling out bias from selection into cohabitation in 1940. Lastly, for our children's sample, we estimate the intergenerational results on the sample of parents who we identify as living with their children when their children were aged 0 to 10 years old to ensure that cohabitation is measured at the same ages for all our observed individuals.

In columns 1-4 of Table A9, we show that no matter when we measure parent-child cohabitation, the intergenerational results are robust to the exercises described above. In fact, the sample of 1940 adults cohabiting with their elderly parents - which is plausibly the most concerning in terms of selection - exhibits the lowest intergenerational transmission of education out of all the samples we use and therefore may actually be attenuating our results. Additionally, in Table A10, we estimate our main results on a sample that includes all 1940 children whose parents are identified in a prior census - even if these parents cannot be re-linked to the 1940 census. In this way, we avoid sample selection issues deriving from i) differential parental mortality rates in 1940, ii) cohabitation of parents and children in 1940, and iii) differential census linkage rates for parents, which could bias our results. We show that the intergenerational effects of CS derived from this sample are very similar to our main results.

6.3 Sample Selection: Mortality

Because many of our parent-child linkages are formed by observing both individuals in the 1940 Census, one may be concerned that the results could be biased since the sample excludes parent-child pairs where the parent has died before 1940. If mortality risks are systematically affected by (or otherwise correlated with) parental exposure to CS, then our intergenerational results could be biased. For example, there could be a spurious positive relationship between parental CS exposure and child outcomes if parents who invest heavily in their children are more likely to die when exposed to low levels of CS.

We believe this kind of mortality bias is unlikely to explain our results. First, the most plausible scenario would likely result in a downward bias in our estimated results. Education and human capital tend to be positively correlated with health and longevity. If anything, among cohorts exposed to low levels of CS, we would be worried about there being positive selection into our sample, which would bias our results toward zero.

As a more formal exercise, we estimate our results using only children-parent pairs where the parents are aged between 40 and 50 in 1940, who are relatively young and less likely to be subject to high mortality rates. We compare these to results from child-parent pairs with parents aged over 50 years. In columns 5 and 6 of table A9 we find virtually no heterogeneity in our results with respect to the age of mothers. Moreover, we find *larger* intergenerational transmission of education in the sample of young fathers and their children, again suggesting that different rates of mortality are not driving our results.

6.4 Mismatched Census Records

Next, we show that our results are robust to alternate census linking rules. Recall that some of the parents in our sample are identified by linking the 1940 children's records to earlier censuses, identifying their parents in that earlier census (and then re-linking the parent to their 1940 census record to obtain additional information, such as their educational attainment). Linking errors - either i) between the children's 1940 census records and earlier census records or ii) between the parents' 1940 census records and earlier census records - could bias our results. Random linking errors would introduce measurement error and lead to attenuation bias in our results as long as mismatches occur unsystematically. More seriously, however, systematic linking errors could bias our results away from zero. For example, if low-education children are more likely to be incorrectly linked to parents from cohorts with low CS exposure, this would incorrectly lead us to conclude that there is an association between low parental exposure to CS and low education levels of children.

We address linkage error concerns by re-estimating our models on different samples varying by linkage quality. First and foremost, we estimate our results on a subsample of high-quality links. Census Tree linkages include flags indicating if the linkages are corroborated by other linkage procedures, such as the Multigenerational Longitudinal Panel (MLP) and Census Linking Project (CLP) (often used in the literature), as well as Family Search users manually linking the census records of their relatives and ancestors and a machine-learning approach. We retain only links which are corroborated by at least three (of seven possible) sources. We also show how our results change if we use the MLP linkages instead of the Census Tree Linkages. We also show that our results are robust to using the Census Linking Project (CLP) linkages for fathers (these are not available for women). In Table A11, we replicate our main intergenerational specification using a sample of parents identified using at least three high-quality links. In fact, our estimates using the high-quality linkages (for parents and children) are of slightly higher magnitudes than the low-quality linkages. Our results hold across all samples defined by different linkage procedures.

6.5 Between-State Migration

We also show that our results are not driven by migration and selection into migration. To illustrate a potential concern, suppose that the implementation of CS laws is correlated with state-level economic developments that changed selection into migration out of the state. For example, suppose that in states adopting more CS, there were greater incentives for more skilled individuals to leave the state, possibly for destinations where their children would be relatively more skilled compared to their peers. A scenario like this could result in a spurious relationship between parental CS exposure and children's education levels (conditional on child state of birth fixed effects).

In Table A12 of the appendices, we show that, if anything, migration actually biases our intergenerational results *towards* zero. We divide our child-parent sample into three subsamples: (1) children whose parents never moved from their state of birth (never-movers), (2) children whose parents moved from their state of birth before the child was born (early movers), and (3) children whose parents moved to a different state after their child was born (late movers). We find strong intergenerational effects for the children of never-movers and late-movers. Meanwhile, for the children of early-mover parents - whose year of migration to a different state is unobserved to us - we estimate null effects. This is likely due to the fact that early-mover parents were not exposed or were exposed only partially to the CS laws in their state of birth. This kind of migration introduces measurement error in parental CS exposure that plausibly biases our results downwards.

6.6 Reweighting

Given the multiple potential sources of selection bias affecting our sample, one potentially useful exercise is to reweight our sample to more closely match the demographics of the overall 1940 Census population. We construct probability weights by first estimating a probit model that predicts whether a respondent of the 1940 Census is included in our parent-child sample based on sex, race, birth state and birth year. We use the predicted inverse probabilities generated by this model and winsorize them to handle extreme values. We then use these inverse probabilities as probability weights for our main intergenerational results, with results reported in Table A13. We find that our results and conclusions are very robust to this reweighing scheme.

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 novel, cutting-edge cross-linkages, we examine a large number of outcomes across the entire life cycle for both parents and children. Using a difference-in-differences approach, we find that CS laws increased the educational attainment of both individuals directly exposed to the reforms, as well as that of their children.

The effects of CS laws on the attainment of the second generation were larger than the effects on the first generation, suggesting that educational reforms may have successfully "snowballed" to achieve the rapid growth in educational attainment over the 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.

We explore channels that may explain the very strong intergenerational effects we obtain. Several plausible mechanisms emerge. CS exposure affects 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. We find that parents exposed to more CS raised their children in neighborhoods with higher teacher-student ratios, and with higher levels of education, literacy and labor force participation.

A Appendix Tables and Figures

A.1 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.1.1 Parental Age

Table A1: Effect of Parental Compulsory Schooling Exposure on Parental Completed Years of Schooling (Prime Age in 1940)

	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.1.2 Effectiveness of CS Laws

Table A2: 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.1.3 Goodman-Bacon Decompositions

Table A3: 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 A4: 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.1.4 Instrumental Variable

Table A5: 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.

A.2 Heterogeneity and Robustness (Intergenerational 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.2.1 Affected Margins of Schooling

Table A6: 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 entry and completion of various schooling levels by their offspring for the *Children* sample. Each column represents a different regression. Dependent variables are coded as 0 (education level not attained) or 100 (education level attained) so that the regression coefficients can be interpreted as percentage point increases in entry and completion. Controls include parent birth year, birth state, birth region, birth region by birth year and race fixed effects and child birth year, birth state, birth state by birth year, sex and race fixed effects. Standard errors are clustered at the child's birth state by birth year level. *p<0.1; **p<0.05; ***p<0.01.

A.2.2 Endogeneity of CS Laws (Placebo Tests)

Table A7: 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 *Parents*, by sex and race, 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 A8: 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*, by sex and race, 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.2.3 Cohabitation and Mortality

Table A9: 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 of the child. Each column represents a different regression. Controls include parent birth year, birth state, birth region, birth region by birth year and race fixed effects and child birth year, birth state, birth state by birth year, sex and race fixed effects. Standard errors are clustered at the child's birth state by birth year level. *p<0.1; **p<0.05; ***p<0.01.

A.2.4 Expanded Sample

Table A10: Effect of Parental Compulsory Schooling Exposure on Parent'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*, by sex and race. 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.2.5 Linkage Quality

Table A11: 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 of the child. Each column represents a different regression. Controls include parent birth year, birth state, birth region, birth region by birth year and race fixed effects and child birth year, birth state, birth state by birth year, sex and race fixed effects. Standard errors are clustered at the child's birth state by birth year level. *p<0.1; **p<0.05; ***p<0.01.

A.2.6 Between-State Migration

Table A12: 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 of the child. Each column represents a different regression. Controls include parent birth year, birth state, birth region, birth region by birth year and race fixed effects and child birth year, birth state, birth state by birth year, sex and race fixed effects. Standard errors are clustered at the child's birth state by birth year level. *p<0.1; **p<0.05; ***p<0.01.

A.2.7 Reweighing Observations

Table A13: Effect of Parental Compulsory Schooling Exposure on Children's Completed Years of Schooling

	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*, by sex and race. 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.3 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.2).

A.3.1 Affected Margins of Schooling

Table A14: 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 exposure to CS on entry and completion of various schooling levels for individuals in the Parent sample. Each column represents a different regression. Dependent variables are coded as 0 (education level not attained) or 100 (education level attained) so that the regression coefficients can be interpreted as percentage point increases in entry and completion. Controls include birth year, birth state, birth region, birth region by birth year, sex and race fixed effects, when possible. Standard errors are clustered at the birth state by birth year level *p<0.1; **p<0.05; ***p<0.01.

A.3.2 Parental Labor Market Outcomes in 1940

Table A15: 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: Relationship between parental exposure to more CS and labor market outcomes. The top panel is estimated using men and the bottom panel using women from the *Parents* sample. Other outcomes explored and unreported because of a lack of statistical significance are: labor-force participation rate, employment rate, living on a farm, urban status, and living in a multifamily household. Controls include birth state, birth year, birth region, birth region and birth year interactions, and self-reported race. Standard errors are clustered at the birth state by birth year level. *p<0.1; **p<0.05; ***p<0.01.

A.3.3 Endogeneity of CS Laws (Placebo Tests)

Table A16: 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*, by sex and race, 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 A17: 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.3.4 Cohabitation and Mortality

Table A18: 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 of the child. Each column represents a different regression. Controls include parent birth year, birth state, birth region, birth region by birth year and race fixed effects and child birth year, birth state, birth state by birth year, sex and race fixed effects. Standard errors are clustered at the child's birth state by birth year level. *p<0.1; **p<0.05; ***p<0.01.

A.3.5 Expanded Sample

Table A19: 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.3.6 Linkage Quality

Table A20: 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 of the child. Each column represents a different regression. Controls include parent birth year, birth state, birth region, birth region by birth year and race fixed effects and child birth year, birth state, birth state by birth year, sex and race fixed effects. Standard errors are clustered at the child's birth state by birth year level. *p<0.1; **p<0.05; ***p<0.01.

A.3.7 Between-State Migration

Table A21: 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 of the child. Each column represents a different regression. Controls include parent birth year, birth state, birth region, birth region by birth year and race fixed effects and child birth year, birth state, birth state by birth year, sex and race fixed effects. Standard errors are clustered at the child's birth state by birth year level. *p<0.1; **p<0.05; ***p<0.01.

A.3.8 Reweighing Observations

Table A22: Effect of Parental Compulsory Schooling Exposure on Parental Completed Years of Schooling

	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 by race and census region. 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.3.9 Stacked Event Study Estimator

Table A23: Effect of Parental Exposure to Compulsory Schooling on Parental Years of Schooling: Stacked Event Study

	Dependent Variable:			
	Parents's Years of Schooling			
	Binary	All	White	Black
	(1)	(2)	(3)	(4)
CS Years (Women)	0.064***	0.015***	0.016***	-0.062**
	(0.0131)	(0.0029)	(0.0029)	(0.0299)
N (millions)	11.1	19.6	17.3	2.3
R^2	0.12	0.13	0.05	0.07
Outcome Means	7.84	7.59	7.96	4.89
CS Years (Men)	0.012***	0.033**	0.012***	-0.011
	(0.0040)	(0.0137)	(0.0034)	(0.0261)
N (millions)	12.3	6.9	11.1	1.2
R^2	0.13	0.12	0.07	0.05
Outcome Means	7.45	7.73	7.79	4.47

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

B 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, Lingwall and Stephens Jr (2021), finding whether this law amends or replaces a previously-existing schooling law, and moving backward in time in this manner.

Exposure to CS is defined for each individual based on their state of birth and cohort year sy. For each state-cohort sy, we ask the following questions each year they are aged between 1 and 18:[16]

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

[16]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).

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