**Hidden Gains or Hidden Gaps?: Revisiting the Effects of Early U.S. Compulsory Schooling Laws on Educational Inequality**

**Michael Lachanski and Xi Song**

Abstract

This paper examines whether compulsory schooling laws (CSLs) in the 19th and early 20th centuries narrowed the educational attainment gap between high and low socioeconomic status children and proximate causes. Drawing on newly available data from the IPUMS Multigenerational Longitudinal Panel and the NBER Census Linking Project, we construct a novel longitudinal sample of White, native-born father-son pairs that were exposed to CSLs. Using a modeling strategy that exploits variation in the timing and intensity of the laws across states within regions, we provide short and longer-run causal estimates of the effects of compulsory schooling policies on the relationship between family background and education utilizing a natural experiment counterfactual approach. As in prior work, we find that laws requiring more years of schooling modestly increased educational attainment for the population and equalized attendance. After their initial imposition, all but the most aggressive compulsory schooling regimes generated larger gains from higher socioeconomic status (SES) households. Among children participating in the school system at older ages, occupational attainment continues to predict 8th grade attendance, high school entry, and years of school completed in all cross-sections. Two mechanisms contribute to these results. First, lower SES students were disproportionately likely to be retained, reducing gains from the laws. Second, higher SES students were disproportionately likely to benefit from "spillovers" in the form of completing more years of schooling than required by the law in response to the law. A rational choice framework emphasizing class differences in relative risk-aversion can explain these findings. Although teacher/student ratios decline in response to the laws, we find no evidence of "coordination failure" in the form of reduced resources per student at the state-level in explaining their contribution to educational inequality. Historical social scientists and education researchers should not assume that policies equalizing school participation necessarily translated to equalized educational attainment.

## Introduction

Historical compulsory schooling laws make a compelling policy instrument for studying how educational reforms change the unequal distribution of educational opportunities among children of from varying socioeconomic backgrounds. These policies, which enforced school attendance during the elementary school years, generated significant shifts in the levels of school participation (Rauscher 2014; 2016; Clay et al. 2021). Educational historians generally concur that the initial surge of compulsory schooling, enacted in most U.S. states between 1852 and 1895 and universally adopted by all states by 1929, represented a concerted effort to assimilate lower social classes and immigrant populations into a swiftly industrializing American society (Meyer, Ramirez, and Soysal 1979, 1992; Tyack 1974; Richardson 1980; Rauscher 2014; Lleras-Muney and Shertzer 2015; Rauscher 2016; Bandiera et al. 2019). Although compulsory schooling laws (CSLs) were introduced to an agrarian U.S. over a century ago, the new CSL literature draws contemporary interest because social scientists typically study the effects of small policy changes, such as reforms mandating an additional year of school attendance, on a variety of economic and health outcomes (Angrist and Krueger 1991; Lleras-Muney 2005). The initial imposition of CSLs exemplify large structural reforms, in which we go from a regime that required no school attendance to one in which several years of school attendance are required.

With the explosion of digital historical archives based on individual-level administrative and Census data a small but rapidly growing literature has emerged documenting how these laws affected occupational stratification, social mobility, assortative mating, historical immigrant assimilation, wages, school attendance inequalities by family background and race, and historical spatial mobility (Rauscher 2014; Rauscher 2015; Rauscher 2017; Rauscher 2016; Lleras-Muney and Shertzer 2015; Mazumder 2019; Clay et al. 2021; Rauscher and Oh 2021). However, no studies have examined whether and to what extent CSLs equalized educational attainment by socioeconomic status in the 19$^{th}$ century. Such an analysis would elucidate the potential role CSLs might have played in mitigating the high levels income inequality witnessed in the early 20$^{th}$ century as documented by Piketty (2014) and Piketty and Saez (2003) (also see Autor, Golden, and Katz 2019).

U.S. CSLs passed in the 19th century provide a unique quasi-experiment for us to answer a classic sociological question: what happens to the relationship between social background and educational attainment when the school system expands (Raftery and Hout 1993; Lucas 2001; Hout and DiPrete 2006; Lucas and Byrne 2017)? Some studies advanced the Maximally Maintained Inequality (MMI) hypothesis, which postulated that family background-driven inequalities in education are typically reduced when education expands but would reappear at higher levels of educational attainment (Collins 1971; Raftery and Hout 1993). Others challenged this view, believing that equalized quantities of education at any given level obscure differences in the quality of education obtained (Lucas 2001; Rauscher 2016; Lucas and Byrne 2017). In what follows, we exploit recent innovations in historical Census linking to evaluate whether historical CSLs reduced educational inequalities by social origin (Abramitzky et al. 2020; Helgertz et al. 2021). If so, at what level of the education system did the inequality-reduction effect of CSLs occur? Answers to these questions shed light on the efficacy of policymakers' attempts to reduce socioeconomic status inequalities in education in the 19th century and its potential implications for long-term trends in socioeconomic educational achievement gaps (Reardon 2011; Reardon, Robinson-Cimpian, and Weathers 2014).

**Compulsory Schooling Laws in the U.S.**

Most states had compulsory educational policies prior to the passage of CSLs, but those policies did not compel parents to send their children to public or private schools (Katz 1976). Instead, compulsory education was left to parents and voluntary community institutions. One of the earliest known compulsory education laws from Massachusetts in 1674 required that parents teach their "children to read and understand the principles of religion and the capital laws of this country" (Katz 1976: 11). CSLs were imposed at the state-level in the U.S., starting in Massachusetts in 1852, and spreading to cover all 50 states by 1929. CSLs compelled parents to send their children to public or private educational institutions from a mandated entry age, often 7 or 8, and, on paper, only allowed children to leave school only when

they had reached a minimum exit age, typically 14 or 16, completed particular thresholds, e.g. 8th grade, or spent a certain number of years in school.

Schooling levels in the 19th and early 20th centuries were much lower than today, although each cohort tended to obtain more education than prior cohorts (Tyack 1974: 56 – 57). For instance, although high school dropouts are in the bottom quintile of the U.S. schooling distribution today, completing high school would have put you in the top quartile of the national educational attainment distribution at any point in the 19th century for which we have data (Golden and Katz 2008; Tyack 1974). In this environment, a CSL compelling students to attend school until an age at which elementary school could be completed might be expected to mechanically increase school attendance and total educational attainment. Did these policy interventions matter? Exemptions were often made for children who were distant from a school, in poverty, or had attended a certain number of years and reached a minimum working age, often 12. These exemptions may have been consequential for some groups; Bandiera et al. (2019) hypothesizes that one reason for the exemptions was to prevent Black children from attending and limit their educational attainment when they did attend. Beyond the exemptions written into the laws, two other social forces might have reduced the effect of U.S. CSLs on school attendance.

First, researchers empirically assessed early CSLs as largely "ineffective or symbolic" (Tyack 1976: 359, 361, 370; Landes and Solomon 1972) because CSLs were usually not strictly enforced at the time of their passage (Lassonde 1996; Rauscher 2014; 2016; Steffes 2012; Mazumder 2019). The situation gradually changed. By the turn of the 20th century, Steffes (2012: 3 - 4) reports that compulsory elementary school attendance became "a national policy despite the absence of a federal government role" because "compulsory attendance statutes…borrowed language, enforcement techniques, and attendance practices from examples in other places" to "converge on common solutions". Overall, limited state capacity did not allow policymakers to enforce fines or impose penalties for noncompliance until the early 20th century in many locales even when increasing school attendance was a widely-agreed upon objective. For instance, it is now well-established that 19th century CSLs targeted the foreign born and their children as part of an "nation-building", assimilationist policy (Bandiera et al. 2019). Mazumder

(2019) found no effect on educational attainment for this group from CSLs while Clay et al. (2021) found near zero effects on years of schooling completed even for the children of the foreign-born. Lleras-Muney and Shertzer's (2015) findings in Tables 5 and 6 are particularly interesting: they show that that early 20th century CSLs appeared to induce higher levels of school attendance among the foreign born in pre-1940 Censuses. However, those same immigrant cohorts reported no statistically significant increase in years of schooling when they were subsequently surveyed in the 1940 Census. For European immigrants at least, it appears that compelled attendance did not translate into higher levels of educational attainment. Why? School attendance questions on the census asked whether a student had attended school any time in the past several months, where the exact length of time varied across Censuses. During the 19th and early 20th centuries, the number of years students attended school did not directly correspond with their level of educational achievement either measured by credential or years of schooling completed. This discrepancy had its proximate cause in frequent instances of student retention, the lack of a consistent age-grading system in schools, and the fact that students sometimes completed more than one grade level within a single year (Ayres 1909; Terman 1926; Finis Welch 1973; Card and Krueger 1992; Tyack 1974). Even when CSLs yielded increased school attendance, they may not have been enforced consistently or rigorously enough to generate greater educational attainment among targeted populations.

Second, school attendance and educational attainment were rising throughout the 19th and 20th century prior to CSLs' passage (Landes and Solomon 1972; Clay et al. 2021; Tyack 1974: 56 – 57; Groeger 2021). Economists theorize that the choice to obtain more schooling followed from rational decision making to invest in human capital rather than legal compulsion (Goldin and Katz 2008; Goldin and Katz 2011; Feigenbaum and Tan 2020). Although other social scientists believe that the reasons for the increase in schooling over the last two centuries are more complex, a broad consensus holds that the bulk of U.S. educational expansion in the 19th and 20th centuries was not primarily driven by direct government compulsion (Collins 1971; 1979; Baker 1999; Goldin and Katz 2008; Rauscher 2014: 505; Caplan 2019).

The fact that compulsory schooling did not play a preeminent role in boosting population-level educational attainment does not mean that it had no effects, especially for subpopulations unlikely to attend school prior to CSLs. Rauscher (2014) argues CSLs generated "hidden gains" for groups previously excluded from the school system (Rauscher 2014; 2016).[1] Even large effects of CSLs for excluded groups might be difficult to detect if we focused only on CSLs' effects for the overall population. Rauscher (2014) argues for equalizing effects of CSLs on school attendance for Whites of low social origin, who like European immigrants, were a target of the laws, using cross-sectional decennial Censuses. Rauscher (2016) replicates this finding using a longitudinal sample of households linked across U.S. Censuses but finds that social mobility went down after educational equalization, a finding she attributes to decreasing school quality in the wake of CSLs. Clay et al. (2021) using a careful design to study the impact of CSLs on White, male native-born birth cohorts born in the late 19$^{th}$ and early 20$^{th}$ century, found that CSLs modestly boosted average educational attainment, a finding consistent with larger equalizing effects for Whites coming from low social backgrounds. However, neither Rauscher (2014; 2016) nor Clay et al. (2021) demonstrated that educational attainment was equalized by social origin. Rauscher (2014; 2016) did not have access to the linked 1940 Decennial Census, the first to record the highest grade level completed, while Clay et al. (2021) was primarily concerned with population-level effects rather than changing educational inequalities.

As this review shows, demonstrating when, where, how, and for whom CSLs influence educational attainment is non-trivial. Studies of these laws over different periods and using different methods find different results for different groups. Three perspectives on early compulsory schooling's effect on educational attainment have empirical support. First, one perspective argues that early compulsory schooling had no effect because it was subject to implementation failure in a low-state capacity environment, because attending school was the result of rational choice on the part of attendees, and because the laws had numerous exemptions (Landes and Solomon 1972; Mazumder 2019: 1 - 3). A

[1] Historians from the start of the critical history of American education literature worried about potentially heterogeneous, population-specific effects of CSLs (Tyack 1976: 376).

second perspective emphasizes the "modest" changes in years of schooling and attendance brought about by compulsory school reforms (Clay et al. 2021; Lleras-Muney and Shertzer 2015). A third perspective, which tends to focus on school attendance after the laws passed, concludes that the modest or no population-level effects concealed large "hidden gains" for those from marginalized class backgrounds (Rauscher 2014: 502; 2016). No research we are aware of has attempted to test the hidden gains perspective with respect to class for both school attendance and educational attainment simultaneously.

**Educational Reforms and Historical Equality of Educational Opportunity: A Brief Overview**

Hout and DiPrete (2006: 9) summarized their evaluation of education's role in the contemporary stratification process by concluding that, "Education is the main factor in…the reproduction of status from generation to generation" because children from families of high social origin often obtain more schooling and schooling is correlated with higher occupational attainment. No similar consensus exists for the role of education in historical stratification: van Leeuwen and Maas (2010: 430, 434) note that "There was little variance in formal educational attainment" in historical Europe and the U.S. but report a "growing importance of education" during industrialization that upended social elites of that period. A voluminous literature on post-World War II changes in the class-educational attainment relationship has been accompanied by far less work on the presumably changing role of family background in educational attainment in the 19th and early 20th century for the U.S. (Katz 1968; Collins 1979; van Leeuwen and Maas 2010; Groeger 2021; Rauscher 2014). School attendance at older ages initially rose faster for White elite than non-elite households, at least in the second half of the 19th century, making it a plausible contributor to the rise in gilded age inequality (Groeger 2021; Rauscher 2014; 2016; Tyack 1974; Katz 1968).

Rauscher (2016) stands out as the first attempt to test whether equalized education would yield increased social mobility using modern causal inference methods. However, education in Rauscher's (2016) study did not mean the same thing as it did in Hout and DiPrete's (2006) review. Education in the studies summarized by Hout and DiPrete (2006: 6) typically consisted of "years of

schooling…successfully completed" in the U.S. context and particular credential milestones, e.g. vocational schooling, college, or graduate work in a global context. Rauscher's reliance on pre-1940 Censuses limits her to measuring age-specific school attendance for different socioeconomic status (SES) groups. Although the effect of school expansion on the relationship between social origins and school attendance and attainment has been extensively studied in Massachusetts and New Haven (Katz 1968; Collins 1979; Field 1976; Lassonde 1996; Groeger 2021), we are not aware of many systematic, quantitative explorations of how *policy* changed the *effect* of social origin on *educational attainment*, whether defined by years of schooling or various thresholds like high school completion, for the United States as a whole in the 19th and early 20th centuries, when educational attainment was lower. Given the central role played by educational inequality in theories of intergenerational mobility, moving towards more plausibly causal research designs of the causes of educational attainment is imperative for contextualizing prior descriptive findings in social stratification research (Breen 2019).

By linking sons in the 1940 Decennial Census and to their fathers in earlier Censuses, we can test whether CSLs equalized educational attainment. If educational attainment was equalized for Whites coming from different social origins, then this would bolster the arguments made in Rauscher (2016) in particular: equalized educational attainment did not increase intergenerational occupational mobility. If we fail to find any equalization effect from CSLs, then Rauscher's (2016) findings should not be interpreted as challenging classical models of social stratification (Blau and Duncan 1967; Jencks et al. 1972; Torche 2011) predicting that equalized educational attainments would increase social mobility for the simple reason that CSLs did not actually equalize educational attainment in the sense that is typically relevant for occupational mobility outcomes. This study uses a novel dataset constructed from linked, complete count decennial Censuses and plausibly causal designs to uncover whether and when CSLs altered the relationship between social origin and both school attendance and years of schooling completed. Specifically, we investigate the effects of CSLs using a novel dataset spanning two generations to study potential heterogeneities in family educational trajectories before and after the imposition of CSLs (Ferrie et al. 2021). To clarify our analysis, we refer to the father's generation as G1

and the son's generation as G2. We study how large structural policy changes differentially affected the trajectories of families starting from low and high social backgrounds, as indexed by G1's occupational attainment. We will study potential (dis)equalizing effects using G1-G2 pairs and their proximate causes in our main analysis.

**Research Question**

In what follows, we estimate the effects of 19th and early 20th century CSLs on equalization of school attendance and educational attainment for G2 across social origins. We use G1's occupational attainment to define G2's social origin. We focus on education as our primary outcome for several reasons. First, the contemporary stratification literature emphasizes the role of educational inequalities in the reproduction of social inequality (Blau and Duncan 1967; Raftery and Hout 1993; Lucas 2001; Hout and DiPrete 2006). Second, comparable measures of education have been used in prior studies of long-term intergenerational mobility (Song et al. 2020). Third, education is a common measure of socioeconomic mobility (Blau and Duncan 1967; Pfeffer and Hertel 2015; Anderson et al. 2018). Several possible findings would be consistent with the prior literature.[2]

i. The U.S.'s CSLs might have had no causal effect on either school attendance or educational attainment, for a variety of reasons (Mazumder 2019; Landes and Solomon 1972). Historical CSLs were irrelevant.

ii. CSLs might have reduced the importance of origin in educational attainment by increasing years of schooling for families of low social origin relative to families of high social origin (Rauscher 2016). Historical CSLs equalized educational outcomes by social origin.

[2] We are not aware of any published research suggesting that CSLs contributed to an increase in either school attendance or educational attainment for households with a high social background, who were more likely to attend school throughout the period we study nor are we are aware of research claiming that CSLs reduced educational attainment, but these remains logical possibilities.

iii. CSLs might have increased the effect of origin in educational attainment. School attendance exemptions, institutional factors, or differential behavioral responses might have yielded greater educational attainment for high social origin families than low social origin families.

Possibility (i) would raise questions about the statistically significant findings uncovered in the recent literature on compulsory schooling. Possibility (ii) above points to an important role for CSLs in facilitating equality of educational opportunity, i.e. a reduction in the role of father G1's social origins in the determination of the distribution of a G2's educational attainment. Possibility (iii) gives credence to critical accounts of the history of schooling highlighting how policies justified by their equalizing potential can backfire.

The remainder of the paper is structured as follows. First, we discuss our dataset of linked father-son Census records and CSLs. Next, we describe our modeling strategy for estimating the effects CSLs at the population-level and for different socioeconomic status groups. Then, we present results from our main analysis and document several important mechanisms through which compulsory schooling affected educational inequality. Finally, we discuss those results considering the existing literature and conclude.

## Data, Measures, and Methods

### *Data*

We use linked 100% complete count sample Decennial Census datasets from the 1850–1940 period to examine the effects of CSLs on educational inequality. Our design treats 19$^{th}$ and early 20$^{th}$ century CSLs as a natural experiment for the cohorts exposed to them to simultaneously estimate: (1) the effects of CSLs on cohorts first subject to them, i.e., the "main effect" for the CSL-exposed population as a whole, (2) the differential effects of CSLs on groups with different social origins. Comparison (2) is a test of the "hidden gains" hypothesis. The large sample size of our dataset in terms of observations, birth states, and birth cohorts enables us to select subpopulations with different exposures to CSL policies. In this way, we can consistently estimate single generation effects of CSLs on the relationship between G1's occupational origin and G2's educational destination.

The sample is restricted to White, native-born G2 males completing the 1940 Census and their fathers. We make this choice first because we plan to certain give full attention to other subpopulations, e.g. nonwhite males and females in future work. Second, it is currently easier to link male generations. Males in this time period retained their father's last names while females typically changed their last name to their after marriage. Third, we want to avoid conflating our results with historical changes to the racial and immigrant-specific stratification processes. Fourth, White, native-born G2 males were a substantial fraction of the U.S. population in this period. To analyze the G1-G2 social origin-education relationship changes, we must create full linkages between fathers' G1 and sons' G2 across all available complete Censuses. To do this, we study the newly released linkages between the full-count U.S. Censuses from 1850 to 1940 in both the IPUMS Multigenerational Longitudinal Panel (MLP) (Helgertz et al. 2021; Ruggles et al. 2021) and the NBER Census Linking Project (CLP) (Abramitzky et al. 2021). Post-1940 Censuses are not yet publicly available for linkage to prior Censuses. Thus, we implement the linkage "backwards" from G2s (and where they appear, G1s) in the full count 1940 Decennial Census to prior Censuses. We will refer to G1-G2 as a father-son pair and it will be sons that are exposed to the CSLs or who comprise the control group. Linked census data enable an analysis of changing relationship between origin and educational destination through an examination of parts of father-son lineages.

Our procedure for constructing lineages follows that of Tian, Song, and Xie (2021). We summarize the procedure in Figure 1. The procedure for constructing the sample is intricate and we present Table 1 as a reference for how we pool the CLP and MLP datasets. We first extract the male offspring's socioeconomic position in 1940, the year of the first U.S. Census to collect detailed educational information. All individuals' information in the 1940 Census is retained and individuals are traced back to earlier Censuses when the data is available. We use the first available Census for constructing fixed characteristics, e.g. race, sex, and nativity. With 1940 as the reference year, we use both the MLP and CLP to construct a linkage.

We start our sample with the MLP. First, we trace back individuals using the 1930–1940 linkage. We construct a multiyear linkage using all available MLP files (1850-1860, 1860-1870, 1870-1880, 1880-

1900, 1900–1910, 1910–1920, 1920–1930, and 1930–1940). For example, if the same 1930 individual-level identifier appears in both the 1920–1930 MLP linkage and the 1930 - 1940 MLP linkage, we trace the individual from 1920 to 1940 via the 1930 Census. We continue this process until we have identified all of a 1940 individual's prior appearances in the MLP. For the CLP, we link individuals back to 1850, 1860, 1870, 1880, 1900, 1910, 1920, and 1930 respectively via the CLP linkage files 1850-1940, 1860-1940, 1870-1940, 1880-1940, 1900–1940, 1910–1940, 1920–1940, and 1930–1940. An individual in 1940 can thus be traced to 1920 or earlier without information from 1930 if they appear in the CLP 1920–1940 but not in the CLP 1930–1940. The CLP offers linkage results via various algorithms, which generally rely on matching people by similar name, sex, and cohort between Censuses. To maximize the sample size, we keep linked records if they are linked by any of the CLP's linking algorithms. For G2s, we have all 1940 complete count census respondents' highest grade of schooling completed and can trace a significant proportion of them to earlier Census years.

Full G1-G2 lineages are only available via the CLP. Specifically, we identify all G2s when they were children in the 1850, 1860, 1870, 1880, 1900, 1910, 1920, and 1930 Censuses. The father in the household in which G2 lives in these Censuses is identified as G1 and his occupation (using 1950 IPUMS coding) is recorded. Although MLP does not contain any G1-G2 linkages, strictly speaking, it provides G1 linkages across Censuses and G2 linkages across Censuses that are not present in the CLP. It is sometimes possible to find a parent-child (G1-G2) linkage in the CLP for a G2 only traced back via the MLP. We combine the CLP and MLP pairs to make a complete G1-G2 linkage whenever this occurs.[3]

Our base sample consists of G2s completing the 1940 complete count Decennial Census in birth cohorts from 1865 to 1915 and their ancestors G1 with a recorded occupation. We have access to the birth state, age, nativity, race, and occupation of G2s' fathers (G1s). We will use our data to estimate single-generation effects using G2 data alone and analyze the potential equalizing effects of CSLs by splitting

[3] In this way, we combine the CLP and MLP datasets to maximize the number of U.S. lineages that we track. Winship and Radbill (1994) argue against the use of weighting when the correct weights are unclear. Therefore, throughout the analysis, we give every lineage in our sample equal weight. In Appendix B, we reweight our sample and replicate key results in the main text.

our sample into categories indexed by G1's occupational attainment as well. All estimates are based on outcomes derived from lineages with individuals whose G2s completed the 1940 Decennial Census education question.[4] We will analyze one continuous variable, mean years of schooling completed, and three dichotomous outcome variables: whether the G2 (i) completed elementary school, (ii) completed 10th grade, and (iii) completed high school. We will look at whether the children of G1s with lower social background were able to close the educational attainment gap with the children of higher social background before and after CSLs were imposed.

Figure 1 presents a schematic of our linking strategy for our G1-G2 tests. We will study G2s' educational attainment as a function of their exposure to the CSL and social background. We build our sample from native-born Americans G2s aged 24 to 71 who answered an educational attainment question in the 1940 Census. Race-ethnicity is defined based on an individual's sex and race-ethnicity in the 1940 Census where the data is reported for G2s. We drop those who do not report that they are White and male in the 1940 Census. We construct G1's occupational status from the first available Census including this information for G1s. Nativity for G1s is constructed similarly.

Our linkage process is not without uncertainty; we sometimes have two potential fathers for a given son. Those potential fathers usually share a name, birthdate, birthplace, and race with someone identified as the father of a G2 in an earlier Census. When this happens we select a father at random to be retained and drop the others. The potential fathers often share many other characteristics, e.g. occupation and current location and so our results are not especially sensitive to which fathers are selected across random draws. Similarly, lineages with G2s reporting no occupational status in any Census are dropped from the sample. This process yields 4,581,016 lineages.

In Table 2, we report our linked sample characteristics. Bailey, Cole, and Massey (2019) recommend that researchers compare their unweighted samples with cross-sectional complete count Censuses using regression to ensure their representativeness, generate custom weights to create

[4] We also drop all lineages in which the gap between birth cohorts for G1s and G2s is 10 years to reflect biological limits to male reproduction and to eliminate lineages that were likely incorrectly specified.

representative samples from the linked population, and report weighted and unweighted results. I report weighted and unweighted results.

### *Occupational Measures of Social Origin*

Researchers in contemporary studies of stratification and inequality have moved away from the use of occupation as an indicator of social status (Sakamoto and Wang 2020). Rising intragenerational mobility in recent decades (Jarvis and Song 2017), better measures of socioeconomic status for contemporary cohorts (Chetty et al. 2014; Chetty et al. 2020; DiPrete 2020), and longstanding unresolved conceptual problems have pushed researchers away from occupational measures in recent work on stratification (Sakamoto and Wang 2020). However, no education or income measures were recorded in the decennial Censuses prior to 1940. Sakamoto and Wang (2020), in a review of recent research, conclude that occupational measures of social status can still be useful for historical research or when analyzing stratification within a limited demographic category. Both conditions characterize the present work.

An additional criticism is that commonly used measures like Duncan's SEI were not designed for historical stratification research, especially for time periods prior to industrialization. As a result, the average SEI shifted within cohorts over the 19$^{th}$ century (Rauscher 2014; 2016). Researchers attempting to apply measures like the SEI to historical stratification contexts often adopt ad hoc normalization procedures. Therefore, we use a new measure of occupational status designed to be comparable between historical periods in the U.S. (Song and Xie 2023). The measure dynamically computes percentile ranks of a birth cohort's occupation in question by ranking them according to how much education individuals within the occupation had. The measure is bounded between 0 and 99 for every occupation. This procedure naturally allows the ranking of an occupation to vary by birth cohort and by changes to educational attainment required to participate in the occupation. Because education was not recorded prior to the 1940 Decennial Census, Song and Xie use literacy levels by occupation to construct a historical measure of occupational status dating to 1785. We will use this measure to create a comparable measure of the family background of G2s in our main analysis, since the former do not generally survive to 1940

and therefore do not participate in the 1940 Census. Song and Xie's (2021) measures are not estimated for cohorts born before 1785 and so we will drop all G1s born before this time.

In our analysis, we will report results based on treating Song and Xie's (2021) measure as as a dichotomous measure to create a high and low class (setting all those with an occupation score equal to or greater than 49 into the high class and those below into the lower class). In the main text, dichotomized results are reported with continuous results in the Appendix. Rauscher (2014; 2015; 2016) and Richardson (1980) report that farming households' school attendance was rising before CSLs. Therefore, in Appendix C, I will replicate key analyses after dropping all fathers listing farming as an occupation. Farmers' rank, according to the Song and Xie measure, varies from 44 (for birth cohorts born around 1800) to 24 (for birth cohorts born around 1910) over the decades. Thus, this procedure changes the composition of (only) the lower socioeconomic status group. Otherwise, it has no qualitative impact on the results.

***Defining CSL Exposure***

Clay et al. (2021) develop a CSL policy simulator, which models how much compelled schooling a birth cohort born in a particular state would have been exposed to from 1880 to 1912. We combine this with records collected by Rauscher (2014) for earlier cohorts. Clay et al.'s (2021) files contain notes on cohorts born as early as 1870 and changes to the legal code as late as 1930. We compared Clay et al.'s detailed codings, which reflect potential exemptions for and subsequent changes to each state's law, with those used in Rauscher (2014; 2016) and found that they were similar, but Clay et al.'s records implied fewer years of compelled schooling. Where there is a conflict, e.g. whether New Mexico passed its first CSL in 1872 or 1891, we defer to Clay et al. (2021). In Appendix A, we list the states adopting CSLs from 1852 to 1895, the year that each CSL was adopted, as well as the ages at which children were required to attend school if only ages were used. We also include a column listing the earliest leaving age based on available exemptions reported in Clay et al. (2021). We see a variety of geographic areas represented as well as considerable variation in when CSLs were passed (Richardson 1980). In Figure 2, we plot selected states'

years of compelled schooling by cohort as output by our modification of Clay et al.'s (2021) simulation. In general, we find a "policy ratchet" effect in which the number of years of required schooling typically, but not always increases. The initial imposition of CSLs results in a rapid and sharp increase in the number of years of required schooling by birth cohort year for most states in our sample. It should be noted that the CSL policy simulator's output is driven entirely by coding of CSLs in state legislatures. At no point does our simulation take in information on the number of years of school a student obtained. Additional details on our CSL simulation can be found in Appendix A. Following other work on the effect of policies on educational status (Black, Devereux, and Salvanes 2005), the simulation takes in an individual's state of birth and year of birth to assign the number of years of compelled schooling for that individual. This procedure assumes that the individual did not move from their birth state and that the laws were implemented as written. Results based on an alternative coding that attempts to identify the state in which the child resided from ages 0 to 11 based on their reported location in an early Census are reported in Appendix C.

**Method: A Multiple Regression Design with Regional Time Trends and State Fixed Effects**

We employ multiple methods to study the effects of CSLs. Our main analysis uses a multiple regression design to study the effects of CSLs. The key assumption is that, after adjusting for regional time trends, specifically a birth cohort by region interaction, and state fixed effects, that the years of compelled schooling are exogeneous to other factors correlated with years of school in our sample.[5] To use the language of causal inference, the "treatment" is differential exposure to the CSL with unobserved state and region-year confounds. Since not all children complied with the CSL policy, our analysis focuses on "intent-to-treat" effects. Birth year cohorts are specified in five year age groups as in Rauscher (2014;

[5] Researchers might be more familiar with a standard two-way fixed effects design in which a national time trend and state fixed effects are included. Relying solely on within-state comparisons might be confounded by unobservable trends in population and institutional characteristics common across states (Stephens and Yang 2014; Clay et al. 2021).

2016). By including region interacted with birth year cohort fixed effects, potential confounding from unobserved differences between groups of states adopting CSLs at a particular time and those adopting them later, at least within a particular region, is minimized. This strategy's thus compares the effect of compelling a certain number of school years versus no school years (or relative to other coefficients) within a particular region.

The analysis proceeds in three steps. First, I investigate population-level and equalization effects of the CSLs on G2s' educational attainment as reported in the 1940 Census. [6] Second, I investigate population-level and equalization effects of the CSLs on G2s' school attendance (Clay et al. 2021). An equalizing effect in our framework would be a larger effect of CSLs on our school attendance and educational attainment measures for G2 sons coming from G1 fathers with low social origin than for G1s with higher social origin. This equalizing effect would constitute evidence for the hidden gains hypothesis. Third, we will investigate proximate causes through which CSLs affected attendance and educational inequality.

Our strategy requires a careful identification of the cohorts subject to CSLs to identify treatments because it combines information on the birth cohorts born before and after CSL passage to construct a statistical counterfactual for the exposed cohorts.[7] We code birth year cohort as: census year – age – 1 following Clay et al. (2021) and code state treatment using reported birth state. In Appendix A, we refine

[6] Several recent papers have used regression discontinuity designs to study the imposition of compulsory schooling laws, following the more contemporary literature on their use (Rauscher 2014; 2016; Rauscher and Oh 2021). In the analysis of historical CSLs, we found this strategy problematic. We discuss regression discontinuity designs as a causal inference strategy in Appendix C.

[7] For example, Rauscher (2016) notes that the secondary literature is contradictory on whether Texas imposed its first CSL in 1873 or 1915. Although this is what the secondary literature says, neither is technically correct. Using primary sources, we resolved this confusion: Texas imposed a controversial and rigorous CSL in 1871 on all children 8 to 18. However, no Texas cohort was compelled to attend the full ten years of compulsory education that the 1871 law would have required because Texas's CSL was revoked in 1876 with the end of post-Civil War Reconstruction. It was substantially weakened in 1873. We follow Clay et al. (2021) in modeling Texas as first passing a CSL in 1915 because this CSL was not contested and never subsequently repealed. Similarly, Rauscher (2016) finds that the secondary sources do not agree on whether New Mexico passed a CSL in 1872 or 1891. Again, our scan of primary documents indicates that neither is correct: the first CSL in New Mexico passed in 1860, but excused kids for any employment at all, specified no age limits, and was likely never enforced (Everett 1984) often because there were no public schools in the area to which children could be compelled to attend (Katz 1976: 19). We choose to code New Mexico as passing its first law in 1891 because most historians identify this as the first CSL that was enforced (Laurence 1977; Carlile 1926; Katz 1976). Everett (1984: 127, 134) gives a more mixed assessment: she agrees that most attendance growth in the New Mexico public school system came after the 1891 law which was "intended to be more rigidly enforced than the 1867 statute" but cites a near contemporaneous federal government assessment worrying that the law was not well-worded enough to be operative.

these estimates using G2's location from age 0 to 5 and reported birth year in the 1900 Census. We obtain results consistent with those reported in the main text.

After applying our simulation to every individual in our sample, we create the treatment sample for our main analysis by assigning every individual the number of years of compelled schooling. The cohorts born just before the selected treatment cohorts in each state, which would have had no compelled schooling, comprise the control sample, which our design uses to construct a counterfactual predicted cohort education level. Our main analysis will focus on G2's mean years of schooling as a continuous outcome measure. We also analyze three dichotomous outcomes in the G2 generation: completing 8th grade, 9th grade, 10th grade, 11th grade, and high school (12th grade). As can be seen in Figure 1, high school graduation was uncommon in the labor force in this period and would have been a major achievement. For the G1-G2 analysis, we have 277,282 G2s who complete the 1940 Decennial Census question who were schooled before 1895. The modal state in the main G1-G2 analysis is Pennsylvania.

To test whether the CSLs increased educational attainment among the average individual in our sample, we will start with the population-level analysis making no distinction across social classes. Then, we will look for equalizing effects. We first use a conventional fixed effects regression like that used in Clay et al. (2021):

$$Y_{ist} = \beta_0 + \gamma_s + \alpha_{rt} + CSL_{1-2}\beta_1 + CSL_{3-5}\beta_2 + CSL_6\beta_3 + CSL_7\beta_4 + CSL_8\beta_5 + CSL_{9+}\beta_6 + \epsilon_{ist} \quad (1)$$

where $i$ indexes an individual, $s$ indexes a state (which implicitly indexes a region $r$), and $t$ indexes a birth cohort. The variable $CSL_{1-2}$ equals 1 when individual $i$ born in state $s$ is born at a time $t$ that they would be assigned to one or two years of compulsory schooling and zero otherwise. $CSL_{3-5}$ equals 1 when individual would be assigned between 3 and 5 years of schooling and zero otherwise. $CSL_6$ equals 1 when individual $i$ born in state $s$ is born at a time $t$ that they would be assigned to six years of compulsory schooling and zero otherwise. $CSL_7$ and $CSL_8$ are defined analogously while $CSL_{9+}$ equals 1 whenever an individual in the sample would be compelled 9 or more years of schooling. Our parameter

vector of interest is $(\beta_1, \beta_2, \beta_3, \beta_4, \beta_5, \beta_6)$. This coding scheme is borrowed from Clay et al. (2021), who reports nonlinearities on the years of schooling obtained from compulsion but we find qualitatively similar results when we code compelled years of schooling linearly.

We operationalize years of education as continuous and whether the respondent completed 8th grade, completed 10th grade, or completed high school as binary in which 1 represents completion and zero noncompletion of the threshold. We estimate (1) and cluster standard errors by the state of exposure. We note that (1), when it has a dichotomous variable, is an example of the linear probability model, which trades off ease-of-interpretation, estimation, and simplicity at a cost of restricting the support of our explanatory variables. To test for equalizing effects, we initially estimate:

$$\begin{aligned} Y_{ist} = \beta_0 + \lambda_s + \lambda_s FatherOcc_{ist} + \alpha_{rt} + \alpha_{rt} FatherOcc_{ist} + CSL_{1-2}\beta_1 + CSL_{3-5}\beta_2 \\ + CSL_6\beta_3 + CSL_7\beta_4 + CSL_8\beta_5 + CSL_{9+}\beta_6 + CSL_{1-2}FatherOcc_{ist}\beta_7 \\ + CSL_{3-5}FatherOcc_{ist}\beta_8 + CSL_6 FatherOcc_{ist}\beta_9 + CSL_7 FatherOcc_{ist}\beta_{10} \\ + CSL_8 FatherOcc_{ist}\beta_{11} + CSL_{9+} FatherOcc_{ist}\beta_{12} + \epsilon_{ist} \end{aligned} \quad (2)$$

We also estimate model (1) separately a partitioned social origin variable, e.g. farming, those with lower occupational rank scores than farming ("low social origin"), and those with higher occupational rank scores than farming ("high social origin"). We report those results in an Appendix and they are consistent with the results we report in the main text.[8]

In a second analysis in Appendix C, we focus on CSLs passed before 1895 CSLs. Rauscher (2014; 2016) and Clay et al. (2021) argue that early CSLs had larger effects than later CSLs. Our designs, described in detail below, focus on comparing cohorts across all states passing CSLs. The timing and geography of CSL rollouts are confounded. Because Southern states were slow to adopt CSLs, passing

[8] As an alternative to this strategy, we considered a difference-in-difference study design using the time that the law passes as an event. Unfortunately, it is well-known that current difference-in-difference designs yield biases in the presence of variable treatment timing and heterogeneous treatment effects. At the moment, no clear solution exist for credibly estimating difference-in-difference designs with continuous or multivalued treatments when treatment timing varies, and the treatment is likely to have nonlinear effects (Callaway, Goodman-Bacon, and Sant'Anna 2021; Clay et al. 2021). All three conditions characterize the current study.

them only in the early 20th century, researchers often analyze these states separately (Rauscher 2014). The pre-1895 analysis includes no Southern states.

**Results**

In Figure 2, the growth in schooling is plotted. In Figure 3, Panel A (Panel B) we plot the fraction of the male population who completed high school (8th grade) by cohort as measured in the full count of the 1940 Decennial Census in red.[9] We observe a nearly continuous increase in the fraction of a given cohort completing these educational milestones. To demonstrate the comparability of our initial sample to the overall data, Figure 3 Panel A (B) presents the fraction of each cohort completing at least 8th (10th) in our sample G2s in blue. We can see that our samples are broadly similar, but the linked sample appears relatively more educated: a higher fraction of birth cohorts in our multigenerational linked sample completes the educational milestones in most cohorts. The mean years of school completed rises across cohorts over time; again it rises faster in the linked sample. This positive selection, which has been found in other studies, may bias our estimates of the effects of CSLs on educational attainment downwards. Suppose the CSL succeeds in generating higher levels of educational attainment for everyone. Individuals with less education prior to the CSL might have been systematically underrepresented in the Census compared to individuals with more education. Nonetheless, the series track each other closely over time.

Figure 3 shows the spread of compulsory schooling over time across different educational regimes. There is heterogeneity by region, with the Southern states being laggards and the North and Midwest leading in the passage of compulsory schooling. There is a general policy ratchet effect in which increase the years of compulsory years required tend to be reversed only rarely. Overall however, we observe considerable heterogeneity across states over time.

[9] Tyack (1974: 183) reports that 1940 was the first year more than half of the population of seventeen years old completed high school and continued to increase, reaching 60% in the 1950s (Goldin and Katz 2008: 26). As with the elementary school movement, these trends obscure considerable state-by-state heterogeneity: Goldin and Katz (2008: 28) estimate that more than 60 percent of 18-year-old outside of the South had a high school education by 1940.

We present our population-level and equalization results across three figures. Figure 4 contains our population-level results for the effect of compulsory schooling on educational attainment. Figure 5 contains our results exploring whether CSLs equalized school grades and particular grade milestones completed. Figure 6 contains our results on how CSLs affected school attendance. Coefficients are plotted in each panel for both weighted and unweighted samples.

Figure 4 shows that compulsory schooling increases the years of schooling completed at the population level and for selected samples. In general, more intense compulsory schooling laws. i.e. a move from 3 to 5 years of compelled schooling to 9 years of schooling or more has a larger effect. The effects are concentrated in grades 3 through 7. All other CSL regimes were statistically indistinguishable from 0 except for the aggressive regime compelling 9 years of schooling. Unsurprisingly, the aggressive regime tended to induce the largest effect across outcomes. Both weighted and unweighted sample estimates track each other closely.

Figure 5 plots the effect of compulsory schooling laws on educational inequalities. The first six estimates of each panel show the effects of the CSL on low SES sons' grade completion. The bottom six estimates are the additional effect for high SES households. If the bottom six estimates are less than zero, then the CSL has an equalizing effect compared to a baseline with no CSL. In terms of years of school completed, CSLs were never equalizing and sometimes disequalizing. In terms of grade thresholds completed, CSLs had small equalizing effects at lower grade levels, e.g. 2$^{nd}$ and 3$^{rd}$ grade and larger disequalizing effects at higher grade levels. Notably, the most aggressive CSL compelling 9 or more years never had a statistically significant disequalizing effect, but CSLs compelling 6 – 8 years of schooling were often disequalizing for the outcomes of completing 8$^{th}$ grade, 9$^{th}$ grade, and 10$^{th}$ grade. The positive coefficients for the interactions of the CSL and high SES status on the Panels displaying the effect of the CSL on 9$^{th}$ and 10$^{th}$ grade completion shows that compulsory schooling laws tended to induce higher grade completion by high SES students beyond what was compelled by the law itself. Clay et al. (2021) call the increase in grade completion beyond that compelled by the law a "spillover effect". The estimates in Figure 5 strongly suggest that such spillovers disproportionately accrued to high SES children.

Figure 6 plots the additional propensity to attend school in response to the CSL. Each panel shows a different outcome-population group. The top left panel shows that the CSLs did indeed induce greater school attendance for low SES children aged 8 to 13 relative to the non-CSL regime. However, these children were unlikely to obtain spillovers in the form of school attendance at ages 14 through 17. On the other hand, high SES children aged 8 to 13 were also more likely to attend school in response to the laws (although point estimates are slightly smaller for this group than for the low SES group), but also to attend school at ages 14 through 17. This highlights the importance of spillovers as a mechanism through which educational inequality was maintained under the CSL even when school attendance at lower ages and grade levels was in fact equalized.

**Discussion**

Using a novel multicohort longitudinal dataset constructed from linked decennial Censuses, we provide causal estimates of whether CSLs reduced class gaps in educational attainment and school attendance. In contrast to earlier work suggesting that CSLs play a role in educational equalization, we found that CSLs made a small but statistically significant contribution to educational inequality in all but the most aggressive regimes. CSL-induced education expansions have been considered an important policy instrument for generating upward mobility (Breen 2019; Rauscher 2016). The logic is that if educational attendance was equalized, then children's educational attainment would depend less on whether they were born into low or high socioeconomic status families. In our analysis, we have shown that 19$^{th}$ and early 20$^{th}$ century CSLs had small population-level effects and, if anything, increased educational inequalities.

These results contrast with prior work. Rauscher (2014; 2016) primarily studied *school attendance* equalization by class, but the translation of compelled school attendance into years of schooling completed, subsequent literacy, or educational milestones is an open question for this period (Collins 1979: 112). In a study of New Haven schools in the 19$^{th}$ and early 20$^{th}$ century, Lassonde (1996: 849) points out that students who attended school because of the CSL might have been especially likely to drop out at the first available opportunity without making academic progress:

> [L]arge numbers of students…kept back for lack of progress in the course of during the 1890s. These "laggards" filled the lower grades of the elementary schools, biding their time until they reached the legal working age, forsaking the classroom periodically as the opportunity to earn beckoned.

For contemporary policymakers, the distinction between school attendance, years of schooling completed, and credential attainment is important because the credentials are a more direct predictor of income and labor market inequality among individuals than the former two (Collins 1979; Grenet 2013; Caplan 2019). The effect of differential retention by socioeconomic status has long been recognized in historical educational research and post-war quantitative educational research, but the potential interaction of retention and compulsory schooling has not been assessed in revisits of the U.S. history of education literature.

Our results suggest that CSLs had a small but statistically significant effect on the attainment gap between children from unequal social backgrounds. Like Rauscher (2014) and Rauscher (2016), we find that CSLs broadly reduced the school attendance gap families of low social origin and high social origin as shown in Figures 4 and 6. However, when subsequently surveyed, the sons whose school attendance had been equalized did not report equalized educational attainment. As shown in the results section, the failure of CSLs to close educational attainment gaps by social origin likely had two proximate causes.

First, as discussed above, policies that elevated school attendance at a particular age should not be assumed to have elevated educational attainment for the same individual at that age. Differential retention could have lowered the probability of advancing to the next grade at elementary school grades. Second, the marginal high SES compelled to attend school might have been more likely to attend grades beyond those compelled. Thus, the "costs" in the form of retention in compulsory schooling are concentrated in the low SES group and the "benefits" in the form of spillovers are concentrated in the higher SES group. To evaluate this hypothesis, I estimated Mare (1981) models of the form:

$$
\begin{aligned}
&logit(NextGrade_{ist}) \qquad (3)\\
&= \beta_0 + \lambda_s + \lambda_s FatherOcc_{ist} + \alpha_{rt} + \alpha_{rt} FatherOcc_{ist} + CSL_{1-2}\beta_1\\
&+ CSL_{3-5}\beta_2 + CSL_6\beta_3 + CSL_7\beta_4 + CSL_8\beta_5 + CSL_{9+}\beta_6\\
&+ CSL_{1-2} FatherOcc_{ist}\beta_7 + CSL_{3-5} FatherOcc_{ist}\beta_8 + CSL_6 FatherOcc_{ist}\beta_9\\
&+ CSL_7 FatherOcc_{ist}\beta_{10} + CSL_8 FatherOcc_{ist}\beta_{11} + CSL_{9+} FatherOcc_{ist}\beta_{12}\\
&+ \epsilon_{ist}
\end{aligned}
$$

where $logit(NextGrade_{ist})$ is the probability of advancing to the next grade given that one is in the current grade. If differential retention as at play, then we should see that children from low SES backgrounds are less likely to comply with the law. For instance, a low SES child in 7th grade should be less likely to make the transition to 8th grade even under a CSL compelling 8 years of school attendance.

We computed the marginal odds ratio for each treatment for the high and low SES origin groups separately for grades 7 through 9 (Karlson and Jann 2023). The marginal odds ratio that we compute is defined as:

$$
MOR(Y_G) = odds\left(\frac{E_x\left[P\left\{Y^G_{CSL=x} = 1 \middle| Y^{G-1} = 1, State, BirthYear\right]\right\}}{E_x\left[P\left\{Y^G_{CSL=0} = 1 \middle| Y^{G-1} = 1, State, BirthYear\right]\right\}}\right) \qquad (4)
$$

where $Y^G = 1$ when the respondent completes grade level $G$ and $Y^G = 0$ when a respondent fails to complete grade level $G$. Clearly, (4) captures how the treatment of compulsory schooling at each level affects the odds of advancing to the next level. A marginal odds ratio greater (less) than 1 indicates that the odds of advancing to the next grade given the current grade increase (decrease) under the CSL relative to the non-CSL baseline. Results are presented in Table 4 for selected grades. These results show that, at earlier grades, the marginal odds ratio falls for low levels of compulsion for making the transition from grade 6 to grade 7 and from grade 7 to 8 for sons with low social origin parents. On the other hand, the effects of CSLs on the marginal odds ratio of making a grade transition is always for higher SES sons, although sometimes it too falls below 1. This indicates that the CSL was pushing students into levels at which, relative to the non-CSL baseline, they would be less likely to advance to the next level. Consider

for instance, a CSL inducing 8 years of compulsion. At the transition to 8th grade from 7th grade, a high social origin child had 2 percentage points greater odds of making the transition relative to a child with a child in the hypothetical world without a CSL. On the other hand, a low social origin child have about 2.3 percentage points less odds of making the transition to 8th grade given that they are in 7th grade. In other words, a child in 7th grade with low social origin was likely to meet the threshold required by the law than a high social origin child. This highlights the critical role of noncompliance. Next, consider the transition to 9th grade given 8 years of compulsion. A high social origin child had 10 percentage points greater odds of making this transition relative to the non-compulsory world. The low social origin child only had 2 percentage points greater odds of making this transition relative to the non-compulsory world. Thus, differential spillovers in the form of high SES children being more likely to beyond what was compelled by the law in response to the law maintained a critical role in keeping the laws from reaching their equalizing potential. This exercise highlights the role of both differential grade retention and differential spillovers in sometimes sustaining inequality under compulsory schooling.

A number of social forces underlie these two mechanisms of differential noncompliance and differential spillovers. Rauscher (2016) documented a fall in resources per student immediately after CSL passage, which might help explain why low-social origin students fell behind their better educated counterparts. In Appendix C, we consider a counterfactual in which teachers per elementary school student were equalized. Second, the infrastructure to enforce CSLs typically passed years after CSLs were first introduced and was rarely if ever implemented at the same time as CSLs (Steffes 2012). Lassonde (1996) reports that a state-wide bureaucratic infrastructure required to enforce anti-truancy policies developed nearly a decade after the first imposition of CSLs in Connecticut, whereas Collins (1979: 110) notes that the first law against school truancy was passed two years before the first CSL in Massachusetts. Perhaps high SES children were more likely to comply with the letter of the law absent a state apparatus. Third, and contra Rauscher (2014; 2016), we speculate that CSLs sometimes suffered from a mass "implementation failure," even when an enforcement apparatus was available (Mazumder 2019; Katz 1976: 19). Working and lower classes in the mid-19th century often resisted even non-compulsory

schooling expansions because they saw little benefit from schooling after they had achieved literacy (Katz 1968; Collins 1979: 104 – 109). Policymakers were hesitant to disrupt the lives of children who they believed would obtain few years of schooling at any rate. Teachers and principals often sought ways to avoid enforcement of compulsory schooling or discharge (likely lower class children) to alternative schools (Tyack 1974). Under this functionalist interpretation, equalized school attendance did not translate into equalized educational attainment because of differential enforcement of truancy codes after children of lower socioeconomic status had been integrated into the system.

Our data do not allow a detailed exploration of these three possibilities. Even with available data, we suspect that the importance of each cause may vary by state.[10] Nonetheless, these possibilities suggest that future historical stratification research utilizing educational reforms in particular might benefit from moving beyond the assumption that everything passed into law was implemented exactly as written. One place to start might be by including more details on development and deployment of enforcement mechanisms, such as funding for state truancy officers, although constructing a detailed implementation history for CSLs is outside the scope of this paper.

The "hidden gains" interpretation of CSLs, at least when measured using educational attainment, is rejected. Our results align with Clay et al. (2021), who report significant effects of CSLs on educational attainment mostly from cohorts entering school several years after the passage of CSLs, not for the cohorts first compelled to attend school that we study. The most parsimonious interpretation of both the archival and quantitative literatures is that, on average, the strictness of CSL enforcement rose with time.

Each aspect of our design invites multiple responses, some of which could be addressed in future research. First, Beller (2009) points out that in the 21st century mother's occupational status is likely to be relevant for son's outcomes. The relevance of mother's occupational status in the 19th century for social stratification remains an open empirical question. Second, our main analyses have focused on Whites and

[10] A fourth, intriguing but unlikely possibility, is that the increase in attendance was offset by a decrease in attainment for students already attending school. Notice that several of the educational attainment coefficients for students of high family background are negative.

native-born to avoid confounding changes to the processes of racial stratification and immigrant assimilation with the effects of CSLs, but researchers should continue documenting potential demographic heterogeneities in the effects of CSLs. Third, we have conducted our analysis from a retrospective perspective. Demographic biases are likely present, but the size and relevance of these biases is difficult to assess. It remains possible that differential effects of CSLs on fertility have biased our findings (Breen 2018; Puerta 2009), but constructing a prospective sample from all possible pre-1940 Census respondents to those appearing in the 1940 Decennial Census remains a computationally intractable problem. The bias induced by this omission is unclear. Retrospective and prospective analyses of social mobility often yield different results. As better fertility data become available for linked datasets, future research can reconcile mobility estimates from our retrospective approach with a prospective approach using methods proposed by Song and Mare (2015).

**Conclusion and Future Work**

The centrality of education to social stratification and mobility has pushed scholars to study the effects of school system expansions on the equalization of educational opportunities and outcomes. Using CSLs as a policy shock to existing inequality in educational opportunities between individuals from different social class origins, we examine whether equalized educational *attendance*, at least according to the initial design of CSLs, reduced educational *attainment* gaps. Findings from the present study suggest that CSLs had modest population-level effects on educational attainment when they were first passed but the effects it did have were disequalizing. Specifically, we report statistically significant evidence that compulsory schooling increased gaps between high- and low-status origin children in years of schooling completed, primary school completion, and high school entry. Using marginal odds ratios, we show that these effects are primarily driven by two effects: differential retention at lower grades for low SES children and greater "spillovers" into higher grades for high SES children.

Several limitations of the present study are also worth noting. Prior work on CSL induced educational equalization included many state-level covariates (Oreopoulos et al. 2006: Table 6; Rauscher

2014; Rauscher 2016; Rauscher and Oh 2021; Clay et al. 2021), while we have only included those that we can construct from our linked sample.[11] Finally, beyond CSL policy changes, we have not explored mechanisms through which social class origins affect educational destinations. This leaves us unable to capture changes in other drivers of educational attainment, such as subsequent occupational attainment (reflecting the returns to education), parents' locational choice (urban versus rural), parental assortative mating, or changes to the post-schooling assortative mating process induced by CSLs (Rauscher 2017). Similarly, although it has been used in highly cited articles (Song et al. 2020), and was designed for historical stratification research, the occupational measure we have used to capture family background is new (Song and Xie 2023). It has not been used in a wide variety of settings yet. Although it is unlikely, we cannot rule out the possibility that including additional variables or an alternative measure of occupational standing might uncover large effects of CSLs on the reduction of class inequalities in the school system over the time period we study.

Research on inequality thus far has given center stage to how social inequalities are reproduced via subsequent educational inequalities. By studying policy shocks from the distant past, we can learn about the role of the state in shaping historical processes while simultaneously testing theories about the generation and maintenance of social inequality. In this case, we have found that the typical historical CSL had at most a modest, negative role to play in shaping the evolution of educational inequalities, but the most aggressive CSLs either had no effect or only slightly reduced educational inequality. For non-immigrant White male population that we study here, CSLs did not fundamentally alter the structure of educational opportunity.

[11] To threaten inference in our design, covariates within states must be both time-varying and not captured by time-varying regional trends. Manufacturing per capita and teachers per elementary school aged student are analyzed in Appendix C. While this makes it unlikely for unmeasured long-run trends to affect our results, e.g. industrialization and the spread of teacher's associations across adjacent states, we cannot rule out unmeasured time-varying confounding in general.

**References**


Ran Abramitzky, Leah Boustan, Katherine Eriksson, Santiago Pérez and Myera Rashid. Census Linking Project: Version 2.0 [dataset]. 2020. https://censuslinkingproject.org

Abramitzky, Ran, Boustan, Leah, Eriksson, K., Feigenbaum, J. and Pérez, S., 2021. "Automated Linking of Historical Data." *Journal of Economic Literature*, *59*(3), pp.865-918.

Alkire, S. and Jahan, S. 2018. The New Global MPI 2018: Aligning with the Sustainable Development Goals.

Aguinis, H., 1995. "Statistical Power Problems with moderated multiple regression in Management Research." *Journal of Management*, *21*(6), pp.1141-1158.

Anderson, Lewis R., Paula Sheppard, and Christiaan W.S. Monden. "Grandparent Effects on Educational Outcomes: a Systematic Review." *Sociological Science* 5 (2018): 114-142.

Autor, David H. 2014. "Skills, education, and the rise of earnings inequality among the 'other 99 Percent'". *Science*, *344*(6186), pp. 843-851.

Autor, David. H, Claudia Goldin and Lawrence F. Katz. 2020, May. "Extending the Race Between Education and Technology." In *AEA Papers and Proceedings* (Vol. 110, pp. 347-51).

Baker, David P. 1999. "Schooling All the Masses: Reconsidering the Origins of American Schooling in the Postbellum Era." *Sociology of Education* 72(4): 197–215.

Baker, David, Gerald K. LeTendre, and Brian Goesling. 2005. "Symbiotic Institutions: Changing Global Dynamics between Family and Schooling." Chapter 3 in National Differences, Global Similarities: World Culture and the Future, edited by David P. Baker and Gerald K. LeTendre. Stanford University Press.

Bandiera, O., Mohnen, M., Rasul, I. and Viarengo, M. 2019. "Nation-building through Compulsory Schooling during the Age of Mass Migration." *The Economic Journal*, *129*(617), pp.62-109.

Barranti, Chrystal C. Ramirez. 1985. "The Grandparent/grandchild Relationship: Family Resource in an Era of Voluntary Bonds." *Family Relations* : 343-352.

Barry, Kathleen. 1989. "Reconstructing American Education." *American Journal of Sociology* 95: 243-244.

Becker, G.S. and Tomes, Nigel. 1986. Human Capital and the Rise and Fall of Families. *Journal of Labor Economics*, *4*(3, Part 2), pp.S1-S39.

Beckfield, Jason. 2020. Rising Inequality Is Not Balanced by Intergenerational Mobility. *Proceedings of the National Academy of Sciences*, *117*(1), pp.23-25.

Beller, Emily. 2009. "Bringing Intergenerational Social Mobility Research into the Twenty-first Century: Why Mothers Matter." *American Sociological Review*, *74*(4), pp.507-528.

Bernardi, F. and Ballarino, G. eds., 2016. *Education, Occupation and Social Origin: A Comparative Analysis of the Transmission of socio-economic Inequalities*. Edward Elgar Publishing.

Black, S.E., Devereux, P.J. and Salvanes, K.G., 2005. "Why the Apple Doesn't Fall Far: Understanding Intergenerational Transmission of Human Capital." *American Economic Review*, *95*(1), pp.437 - 449.

Blau, Peter and Otis Dudley Duncan. 1967. Chapter 5 "The Process of Stratification." *The American Occupational Structure*. New York: Wiley.

Bloome, Deirdre, Shauna Dyer, and Xiang Zhou. 2018. "Educational Inequality, Educational Expansion, and Intergenerational Income Persistence in the United States." *American*

Bloome, Deirdre., 2015. "Income inequality and intergenerational income mobility in the United States." *Social Forces*, *93*(3), pp. 1047-1080.

Bills, David. 2016. "Contested Credentials: The Material and Positional Economies of Schooling." Research in Social Stratification and Mobility 43: 65-70.

Bowles, Samuel, and Herbert Gintis. 1976. *Schooling in Capitalist America:Educational reform*

*and the contradictions of economic Life*. New York: Basic Books.
Brand, Jennie E. and Yu Xie. 2010. "Who Benefits Most from College? Evidence for Negative Selection in Heterogeneous Economic Returns to Higher Education." *American Sociological Review* 75: 273-302.
Breen, Richard. 2011. "Educational Assortative Mating and Earnings Inequality in the United States." *American Journal of Sociology* 117(3): 808-843.
Breen, Richard. 2018. Some Methodological Problems in the Study of Multigenerational Mobility. *European Sociological Review*, *34*(6), pp.603-611.
Breen, Richard. 2019. "Education and Intergenerational Social mobility in the US and four European Countries." *Oxford Review of Economic Policy*, *35*(3), pp.445-466.
Burton, Linda M., and Vern L. Bengtson. 1985. "Black Grandmothers: Issues of Timing and Continuity of Roles." Pp. 61–77 in *Grandparenthood.*, *Sage focus editions, Vol. 74.* Thousand Oaks, CA, US: Sage Publications, Inc.
Card, David, Domnisoru, Ciprian and Taylor, Lowell. 2018. *The Intergenerational Transmission of Human Capital: Evidence from the Golden Age of Upward Mobility* (No. w25000). National Bureau of Economic Research.
Calonico, S., Cattaneo, M.D., Farrell, M.H. and Titiunik, R. 2019. "Regression discontinuity Designs Using Covariates." *Review of Economics and Statistics*, *101*(3), pp.442-451.
Cattaneo, M.D., Idrobo, N. and Titiunik, R. 2019. *A Practical Introduction to Regression Discontinuity Designs: Foundations*. Cambridge University Press.
Cattaneo, M.D., Keele, L. and Titiunik, R. 2021. "Covariate Adjustment in Regression Discontinuity Designs." *arXiv preprint arXiv:2110.08410*.
Carlile, A.B. 1926. *Compulsory Attendance Laws in the United States* (Vol. 1). University of Wisconsin-Madison.
Carlson, Robert A. 1975. *The Quest for Conformity: Americanization through Education*. New York: Wiley & Sons.
Chan, Tak Wing and Vikki Boliver. 2013. "The Grandparents Effect in Social Mobility: Evidence from British Birth Cohort Studies." *American Sociological Review* 78: 662-678.
Cherlin, Andrew and Furstenberg Jr., Frank F. 1992. *The New American Grandparent*. Harvard University Press.
Chetty, Raj, Hendren, Nathan, Kline, Patrick, & Saez, E. 2014. "Where Is the Land of Opportunity? The Geography of Intergenerational Mobility in the United States." *The Quarterly Journal of Economics*, *129*(4), 1553-1623.
Chetty, Raj, Hendren, Nathan, Jones, Maggie R. and Porter, Sonya R. 2020. Race and economic Opportunity in the United States: An Intergenerational Perspective. *The Quarterly Journal of Economics*, *135*(2), pp.711-783.
Choi, J.Y. and Lee, M.J. 2021. Basics and Recent Advances in Regression Discontinuity: Difference Versus Regression Forms. *Journal of Economic Theory and Econometrics*, *32*(3), pp.1 – 68.
Clay, Karen, Lingwall, Jeff, and Stephens Jr, Melvin. 2021. "Laws, Educational Outcomes, and Returns to Schooling Evidence from the First Wave of US State Compulsory Attendance Laws." *Labour Economics*, *68*, p.101935.
Coleman, James S. 1988. "Social Capital in the Creation of Human Capital." A*merican Journal of Sociology* 94: S95-S120.
Collins, Randall. 1971. Functional and Conflict Theories of Educational Stratification. *American Sociological Review*, pp.1002-1019.
Collins, Randall. 1979. *The Credential Society*. New York: Academic Press.
Daw, Jonathan, Gaddis, S. Michael and Morse, Anne R. 2020. "3Ms of 3G: Testing three mechanisms of three-Generational Educational Mobility in the U.S." *Research in Social Stratification and Mobility*, *66*, p.100481.
DiPrete, Thomas A. 2020. "The Impact of inequality on intergenerational Mobility." *Annual*

*Review of Sociology*, *46*, pp.379-398.
Downey, Doug and Dennis J. Condron. 2016. "Fifty Years since the Coleman Report: Rethinking the Relationship between schools and Inequality." *Sociology of Education* 89: 207-220.
Duncan, O.D. 1961. "A Socioeconomic Index for All Occupations." *Class: Critical Concepts*, *1*(1), pp. 388-426.
Elsayed, A., Namoro, S.D. and Roushdy, R., 2021. "Empowering women in conservative Settings:
Evidence from an Intervention in Rural Egypt." *Review of Economics of the Household*, pp. 1-36.
Erikson, R. 2016. Is It Enough to be Bright? Parental Background, Cognitive Ability and Educational Attainment. *European Societies*, *18*(2), pp.117-135.
Everett, Dianna. 1984. "The Public School Debate in New Mexico: 1850-1891." *Arizona and the West*, *26*(2), pp.107-134.
Featherman, David L., F. Lancaster Jones, and Robert M. Hauser. 1975. "Assumptions of Social Mobility Research in the U.S.: The Case of Occupational Status." *Social Science Research* 4:329-360.
Feigenbaum, J.J. and Tan, H.R. 2020. "The Return to Education in the Mid-twentieth Century: Evidence from Twins." *The Journal of Economic History*, *80*(4), pp.1101-1142.
Ferrie, Joseph, Massey, Catherine and Rothbaum, Jonathan. 2021. "Do Grandparents Matter? Multigenerational Mobility in the United States, 1940–2015." *Journal of Labor Economics*, *39*(3), pp.000-000.
Field, Alexander James. 1976. "Educational Expansion in Mid-nineteenth-century Massachusetts: Human-Capital Formation or Structural Reinforcement?" *Harvard Educational Review*, *46*(4), pp. 521-552.
Fuller, Bruce and Rubinson, Richard. 1992. *The Political Construction of Education: The State, School Expansion, and Economic Change*. Praeger Publishers, One Madison Avenue, New York, NY 10010.
Gelman, Andrew, Hill, Jennifer and Vehtari, Aki Vehtari. 2020. *Regression and Other Stories*. Cambridge University Press.
Goh, E.C. and Kuczynski, L. 2010. "'Only children' and their coalition of parents: Considering grandparents and parents as joint caregivers in urban Xiamen, China." *Asian Journal of Social Psychology*, *13*(4), pp. 221-231.
Goldin, Claudia and Katz, Lawrence F. 2008. *The Race Between Education and Technology*. Harvard University Press.
Goodhue, Dale, Lewis, William and Thompson, Ronald. 2007. "Research note—Statistical power in analyzing interaction effects: Questioning the advantage of PLS with product indicators." *Information Systems Research*, *18*(2), pp.211-227.
Grenet, Julien. 2013. "Is Extending Compulsory Schooling Alone Enough to Raise Earnings? Evidence from French and British compulsory schooling laws." *The Scandinavian Journal of Economics*, *115*(1), pp.176-210.
Groeger, Christina V., 2021. *The Education Trap: Schools and the Remaking of Inequality in Boston*. Harvard University Press.
Grubb, W. Norton and Lazerson, Marvin. 2004. *The Education Gospel: The Economic Power of Schooling*. Harvard University Press.
Lassonde, Stephen. 2008. *Learning to Forget: Schooling and Family Life in New Haven?'s Working Class, 1870-1940*. Yale University Press.
Lazerson, Marvin. 2013. *Origins of the Urban School*. Harvard University Press.
Liu, Yujia and David B. Grusky. 2013. "The Payoff to Skill in the Third Industrial Revolution." *American Journal of Sociology* 118: 1330-1374.
Hällsten, Martin and Fabian T. Pfeffer. 2017. "Grand Advantage: Family Wealth and Grandchildren's Educational Achievement in Sweden." *American Sociological Review* 82: 328-360.
Hank, Karsten, Cavrini, Giulia, Di Gessa, Giorgia. and Tomassini, Cecilia. 2018. "What Do We

Know about Grandparents? Insights from Current Quantitative Data and Identification of Future Data Needs." *European Journal of Ageing*, *15*(3), pp. 225-235.
Heckman, James. 1995. "Lessons from the Bell Curve." Journal of Political Economy 103:1091–1120.
Heckman, J.J. 2006. "Skill Formation and the Economics of Investing in Disadvantaged Children." *Science*, *312*(5782), pp.1900-1902.
Helgertz, Jonas, Ruggles, Steven, Warren, John Robert, Fitch, Catherine A., Goeken, Ronald, Hacker, J. David, Nelson, Matt A., Price, Joseph P., Roberts, Evan, and Sobek, Matthew. 2020. IPUMS Multigenerational Longitudinal Panel: Version 1.0 [dataset]. Minneapolis, MN: IPUMS, 2020. https://doi.org/10.18128/D016.V1.0
Herrnstein, Richard J. and Murray, C., 2010. *The Bell Curve: Intelligence and Class Structure in American Life*. Simon and Schuster.
Hout, M. and DiPrete, T.A., 2006. What we have learned: RC28's contributions to knowledge about social stratification. *Research in social stratification and mobility*, *24*(1), pp.1-20.
Hout, Michael. 2018. "American's Occupational Status Reflects the Status of Both of Their Parents." *Proceedings of the National Academy of Sciences of the United States of America (PNAS)* 115(38): 9527–9532.
Jargowsky, Paul A. 1997. *Poverty and Place: Ghettos, Barrios, and the American City*. Russell Sage Foundation.
Jencks, C. 1972. *Inequality: A Reassessment of the Effect of Family and Schooling in America*.
Karlson, K.B. and Jann, B., 2023. "Marginal Odds Ratios: What They Are, How to Compute Them, and Why Sociologists Might Want to Use Them." *Sociological Science, 10,* pp. 332 – 347.
Katz, Michael B. 1968/2001. *The Irony of Early School Reform: Educational Innovation in Mid-Nineteenth Century Massachusetts*. Teachers College Press.
Katz, Michael B. 1987. *Reconstructing American Education*. Harvard University Press.
Katz, Michael S. 1976. *A History of Compulsory Education Laws*. Fastback Series, No. 75. Bicentennial Series.
Kaestle, Carl F. 1973. *The Evolution of an Urban School System*. Harvard University Press.
Kaestle, Carl F. 1983. *Pillars of the Republic: Common Schools and American Society, 1780-1860* (Vol. 154). Macmillan.
Keister, Lisa A. and Moller, Stephanie. 2000. "Wealth inequality in the United States." *Annual Review of Sociology*, *26*(1), pp.63-81.
Killewald, Alexandra., Pfeffer, Fabian T. and Schachner, Jared N. 2017. "Wealth Inequality and Accumulation." *Annual Review of Sociology*, *43*, pp.379-404.
Knigge, Antonie. 2016. "Beyond the Parental Generation: The Influence of Grandfathers and Great-Grandfathers on Status Attainment." *Demography* 53(4):1219–44. https://doi.org/10.1007/s13524-016-0486-6.
Laurence, R. 1977. "Indian Education: Federal Compulsory School Attendance Law Applicable to American Indians: The Treaty-Making Period: 1857-1871." *American Indian Law Review*, *5*(2), pp.393 - 413.
Landes, William M., and Solomon, Lewis C. 1972. "Compulsory Schooling Legislation: An Economic Analysis of Law and Social Change in the Nineteenth Century." *Journal of Economic History* 22: 54–91.
Lassonde, Stephen. 1996. "Learning and Earning: Schooling, Juvenile Employment, and the Early Life Course in Late Nineteenth-century New Haven." *Journal of Social History*, pp. 839-870.
Lawrence, Matthew and Richard Breen. 2016. "And Their Children after Them? The Effect of College on Educational Reproduction." *American Journal of Sociology* 122: 532-572.
Lleras-Muney, Adriana. 2002. "Were compulsory attendance and child labor laws effective? An Analysis from 1915 to 1939." *The Journal of Law and Economics*, *45*(2), pp.401-435.
Lleras-Muney, Adriana and Shertzer, Allison. 2015. "Did the Americanization movement

Succeed? An Evaluation of the Effect of English-only and Compulsory Schooling Laws on Immigrants." *American Economic Journal: Economic Policy, 7*(3), pp.258-90.
Lindahl, M., Palme, M., Sandgren-Massih, S. and Sjögren, A. 2014. "A Test of the Becker-Tomes Model of Human Capital Transmission Using Microdata on Four Generations." *Journal of Human Capital*, *8*(1), pp.80-96.
Lucas, Samuel R. 2001. "Effectively Maintained Inequality: Education Transitions, Track Mobility, and Social Background Effects." *American Journal of Sociology* 106: 1642-1690.
Lucas, Samuel R. and Byrne, D. 2017. "Effectively Maintained Inequality in Education: An Introduction." *American Behavioral Scientist*, *61*(1), pp. 3-7.
Maurin, E. and McNally, S., 2008. "Vive la revolution! Long-term educational returns of 1968 to the Angry Students." *Journal of Labor Economics*, *26*(1), pp.1-33.
Mare, Robert D. 1981. "Change and Stability in Educational Stratification." *American Sociological Review* 46:72-87
Mare, Robert D. 2011. "A Multigenerational View of Inequality." *Demography* 48:1-23.
Mare, Robert D. 2014. "Multigenerational Aspects of Social Stratification: Issues for Further Research." *Research in Social Stratification and Mobility* 35: 121-128.
Margolis, Rachel and Laura Wright. 2017. "Healthy Grandparenthood: How long is it, and How Has it Changed?" *Demography* 54: 2073-2099.
Massey, Douglas and Denton, Nancy A. 1993. *American Apartheid: Segregation and the Making of the Underclass*. Harvard University Press.
Mayer, Susan. 1998. What Money Can't Buy: Family Income and Children's Life Chances. Harvard University Press.
Mazumder, Soumyajit. 2019. "No Nation Left Behind? Assessing the Impact of Compulsory Schooling Laws on Immigrant Assimilation." SocArXiv. May 23. doi:10.31235/osf.io/k687f.
Dayioğlu, Meltem and Kïrdar, Murat G. 2020. "Keeping Kids in School and Out of Work: Compulsory Schooling and Child Labor in Turkey."
McNeal, Ralph B., Jr. 1999. "Parental Involvement as Social Capital: Differential Effectiveness on Science Achievement, Truancy, and Dropping Out." Social Forces 78: 117-144.
Meng, Xin and Zhao, Guochang. 2021. "The Long Shadow of a Large Scale Education Interruption: The Intergenerational Effect." *Labour Economics*, *71*, p.102008.
Meyer, John W., Ramirez, Francisco O. and Soysal, Y.N. 1992. World Expansion of Mass Education, 1870-1980. *Sociology of Education*, pp. 128-149.
Meyer, John W., Tyack, D., Nagel, J. and Gordon, A. 1979. "Public education as nation-building in America: Enrollments and bureaucratization in the American states, 1870-1930." *American Journal of Sociology*, *85*(3), pp. 591-613.
Moneyhon, C.H. 1989. "Public Education and Texas Reconstruction Politics, 1871-1874." *The Southwestern Historical Quarterly*, *92*(3), pp.393-416.
Mundial, G.B. and UNICEF, 2016. Education 2030: Incheon declaration and framework for action: towards inclusive and equitable quality education and lifelong learning for all.
Neckerman, Kathryn M. and Torche, Florencia. 2007. "Inequality: Causes and Consequences." *Annual Revolution Sociology*, *33*, pp. 335-357.
Neem, Johann N. 2017. *Democracy's Schools: The Rise of Public Education in America*. JHU Press.
Nybom, Martin and Stuhler, Jan. 2019. "Steady-state Assumptions in Intergenerational Mobility Research." *The Journal of Economic Inequality*, *17*(1), pp. 77-97.
Oreopoulos, Philip, Page, Marianne E. and Stevens, Ann Huff. 2006. "The Intergenerational Effects of Compulsory Schooling." *Journal of Labor Economics*, *24*(4), pp.729-760.
Pfeffer, F.T. 2014. Multigenerational Approaches to Social Mobility. A Multifaceted Research Agenda. *Research in Social Stratification and Mobility*, *35*, p.1.

Pfeffer, F.T. and Hertel, F.R. 2015. "How Has Educational Expansion Shaped Social Mobility Trends in the United States?" *Social Forces, 94*(1), pp.143-180.
Puerta, Juan Manuel. 2009. "'The Fewer, the Merrier': Compulsory Schooling Laws, Human Capital, and Fertility in the United States." Paper presented at Population Association of America annual meeting April 17, 2010, Dallas, Tex. http://paa2010.princeton.edu/download.aspx?submissionId5102057.
Piketty, Thomas. 2014. Capital in the 21st Century. *Cambridge, MA: President and Fellows, Harvard College*.
Putnam, Robert D. 2016. *Our Kids: The American Dream in Crisis*. Simon and Schuster.
Raftery, Adrian E., and Michael Hout. 1993. "Maximally Maintained Inequality: Expansion, Reform, and Opportunity in Irish Education." *Sociology of Education* 66:41-62.
Rauscher, Emily. 2014. "Hidden gains: Effects of Early US Compulsory Schooling Laws on Attendance and Attainment by Social Background." *Educational Evaluation and Policy Analysis, 36*(4), pp.501-518.
Rauscher, Emily. 2015. "Educational Expansion and occupational Change: US compulsory Schooling Laws and the Occupational Structure 1850–1930." *Social Forces, 93*(4): 1397-1422.
Rauscher, Emily. 2016. "Does Educational Equality Increase Mobility? Exploiting Nineteenth-Century U.S. Compulsory Schooling Laws." *American Journal of Sociology* 121: 1697-1761.
Rauscher, Emily and Oh, Byeongdon. 2021. "Going Places: Effects of Early US Compulsory Schooling Laws on Internal Migration." *Population Research and Policy Review, 40*(2): 255-283.
Reardon, Sean F. 2011. "The Widening Academic Achievement Gap Between the Rich and the Poor: New Evidence and Possible Explanations." in *Whither Opportunity* pp. 91-116. NY: Russell Sage Foundation.
Reardon, Sean F., Joseph P. Robinson-Cimpian, and Ericka S. Weathers. 2014. "Patterns and Trends in Racial/Ethnic and Socioeconomic Academic Achievement Gaps." In *Handbook of Research in Education Finance and Policy*, pp. 507-525. Routledge.
Richardson, J.G. 1980. "Variation in Date of Enactment of Compulsory School Attendance Laws: An Empirical Inquiry." *Sociology of Education*, pp.153-163.
Rivera, Lauren A. 2011. "Ivies, Extracurriculars, and Exclusion: Elite Employers' Use of Educational Credentials. *Research in Social Stratification and Mobility*", *29*(1), pp. 71-90.
Rosenbaum, James E. 2001. *Beyond College for All: Career Paths for the Forgotten Half*.
Ruggles, Steven. 2003. "Multigenerational Families in Nineteenth-Century America." *Continuity and Change*, *18*(1): 139-165.
Ruggles, Steven, Fitch, Catherine A., Goeken, Ronald, Hacker, J. David, Nelson, Matt A., Robert, Evan, Schouweiler, Megan, and Matthew Sobek. IPUMS Ancestry Full Count Data: Version 3.0 [dataset]. Minneapolis, MN: IPUMS, 2021.
U.S. Bureau of the Census. 1924. "School Attendance in 1920: An Analysis of School Attendance in the United States and in the Several States, with a Discussion of the Factors Involved." In Census Monographs V, by Frank Alexander Ross. Washington, D.C.: Government Printing Office. https://www2.census.gov/library/publications/decennial/1920/monograph-5/04097225no5ch1.pdf
Sakamoto, Arthur and Wang, Sharron Xuanren. 2020. "The Declining Significance of Occupation in Research on Intergenerational Mobility." *Research in Social Stratification and Mobility, 70,* p.100521.
Sauder, Ulrich. 2006. "Education Transmission across Three Generations—New Evidence from NCDS Data." Manuscript, University of Warwick
Saez, Emmanuel 2017. "Income and Wealth Inequality: Evidence and Policy Implications." Contemporary Economic Policy 35: 7-25.
Schneider, Daniel, Orestes P. Hastings, and Joe LaBriola. 2018. "Income Inequality and Class Divides in Parental Investments." American Sociological Review 83: 475-507.

Sellik, H., van Paridon, O., Gousios, G. and Aniche, M. 2021, May. Learning Off-by-one Mistakes: An Empirical Study. In *2021 IEEE/ACM 18th International Conference on Mining Software Repositories (MSR)* (pp. 58-67). IEEE.

Sharkey, Patrick and Felix Elwert. 2011. "The Legacy of Disadvantage: Multigenerational Neighborhood Effects on Cognitive Ability." *American Journal of Sociology* 116: 1934-1981.

Shavit, Yossi and Hyunjoon Park. 2016. "Introduction to the Special Issue: Education as a Positional Good." Research in Social Stratification and Mobility 43: 1-3.

Song, Xi. "Multigenerational Social Mobility: A Demographic Approach." *Sociological Methodology* 51, no. 1 (2021): 1-43.

Song, Xi, and Robert D. Mare. 2015. "Prospective versus Retrospective Approaches to the Study of Intergenerational Social Mobility." *Sociological Methods & Research* 44(4): 555-584.

Song, Xi and Yu Xie. 2021. "Occupation-Based Socioeconomic Index with Percentile Ranks."

Song Xi, Catherine G. Massey, Karen A. Rolf, Joseph Ferrie, Jonathan Rothbaum and Yu Xie. 2020. "Long-Term Decline in Intergenerational Mobility in the United States since the 1850s." *Proceedings of the National Academy of Sciences of the United States of America (PNAS)* 117(1): 251–258.

Song, Xi and Mare, Robert D. 2019. "Shared Lifetimes, Multigenerational Exposure, and Educational Mobility." *Demography*, *56*(3), pp. 891-916.

Song, Xi and Campbell, Cameron D. 2017. "Genealogical Microdata and Their Significance for Social Science." *Annual Review of Sociology*, *43*, pp.75-99.

Suhonen, T. and Karhunen, H. 2019. "The Intergenerational Effects of Parental Higher Education: Evidence from Changes in University Accessibility." *Journal of Public Economics*, *176*, pp. 195-217.

Stella, Luca. 2013. "Intergenerational Transmission of Human Capital in Europe: evidence from SHARE." *IZA Journal of European Labor Studies*, *2*(1), pp. 1-24.

Steffes, Tracy L. 2011. *School, Society, and State*. University of Chicago Press.

Sturgis, P. and Buscha, F., 2015. "Increasing inter-generational social mobility: is educational expansion the Answer?" *The British Journal of Sociology*, *66*(3), pp.512-533.

Torche, Florencia. 2011. "Is a College Degree Still the Great Equalizer? Intergenerational Mobility across Levels of Schooling in the United States." *American Journal of Sociology* 117: 763-807.

Tian, Ziyao, Xi Song, and Yu Xie. 2021. "Intergenerational Social Mobility of Asian Americans Under the Shadow of Asian Exclusion (1882 – 1943)". *Unpublished working paper.*

Tyack, David B. 1974. *The One Best System: A History of American Urban Education* (Vol. 95). Harvard University Press.

Tyack, David. 1976. Ways of Seeing: An Essay on the History of Compulsory Schooling. *Harvard Educational Review*, *46*(3), pp. 355-389.

Tyack, David and Hansot, Elizabeth. 1980. From Social Movement to Professional Management: An Inquiry into the Changing Character of Leadership in Public Education. *American Journal of Education*, *88*(3), pp. 291-319.

Tyack, David. and Hansot, Elizabeth. 1981. Conflict and Consensus in American Public Education. *Daedalus*, pp. 1-25.

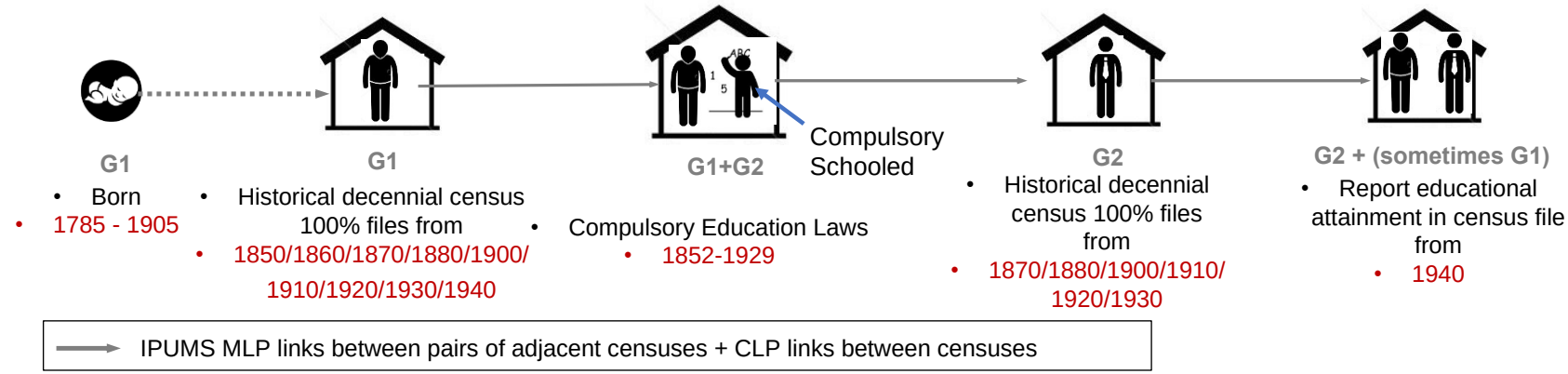


Figure 1. Illustrating Our Sample Linking Strategy Across Decennials with Linkages from the MLP and CLP Data Structure.
NOTE. - Individuals in birth cohorts from 1870 to 1915 who have reported a level of education are identified in the 1940 complete count Census and linked to their household in the 1850, 1860, 1870, 1880, 1900, 1910, 1920, or 1930 complete count Censuses. They are assigned to generation G2 and their fathers, inferred by reported demographic relationships within the household at the time of the Census are assigned to generation G1. The father's generation G2, in turn, is linked to their household in the 1850, 1860, 1870, 1880, 1900, 1910, 1920, 1930, and 1940 Censuses. Although all G2s must appear in the 1940 Decennial Census, our linking algorithm searches over all prior full count Censuses to construct a G1-G2 link. G2s' CSL exposure is inferred using their birth cohort and reported birthplace in subsequent Censuses. In our sample, we score G2 as being sometimes compelled to attend school and sometimes not, depending on their state of birth and birth cohort.


Figure 2. Comparing the Linked Decennials Sample to the 1940 Full Count Sample by Educational Attainment by Birth Cohort
NOTE. – This Figure compares the educational attainment of our sample population (G2) with that of the equivalent from the complete count 1940 decennial Census. The cohorts plotted are the children of native-born White males. We can see that those who were able to link generally have higher educational attainment than the typical U.S. resident in 1940. In general however, the trajectories of educational attainment by cohort track each other closely. The fraction of the native-born White male population completing high school only reached 50% in our linked sample during World War I and never reached it in the full count decennial Census for cohorts in our sample.

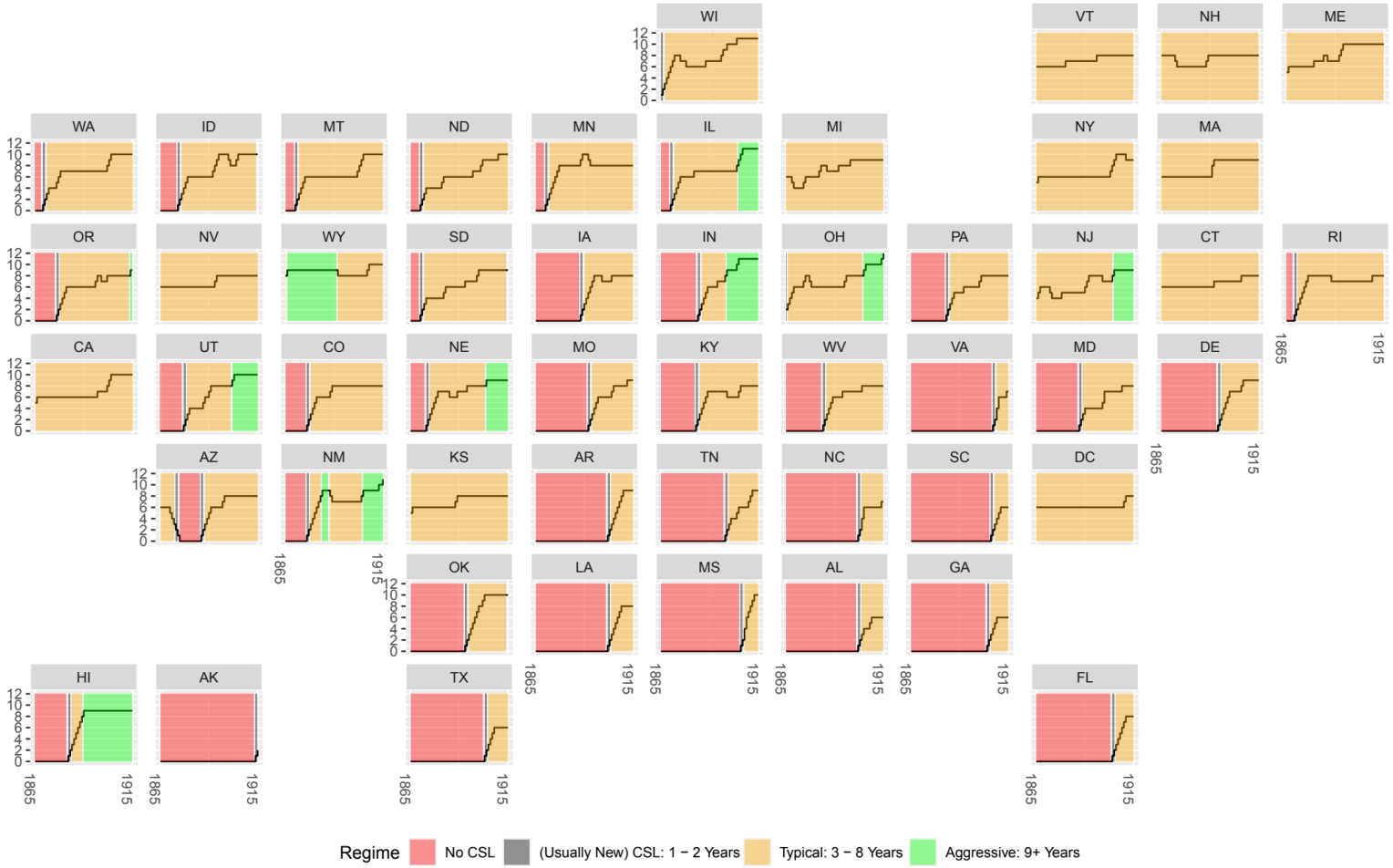


Figure 3. The Expansion of Compulsory Schooling by State and Cohort

NOTE. – This Figure graphs the years of school required for each state by birth cohort. Compulsory schooling regimes are color coded. A state – birth cohort with no CSL, like many of the Southern states for birth cohorts born in 19th century, is colored in red. States implementing only 1 – 2 years of compulsory schooling are colored in grey. States with 3 – 8 years of compulsory schooling, which characterized a "typical" regime are colored in yellow. States with aggressive compulsory schooling regimes mandating 9 or more years are colored in green. We observe a policy ratchet effect in which the number of years of compulsory schooling tended to go up rather than down.

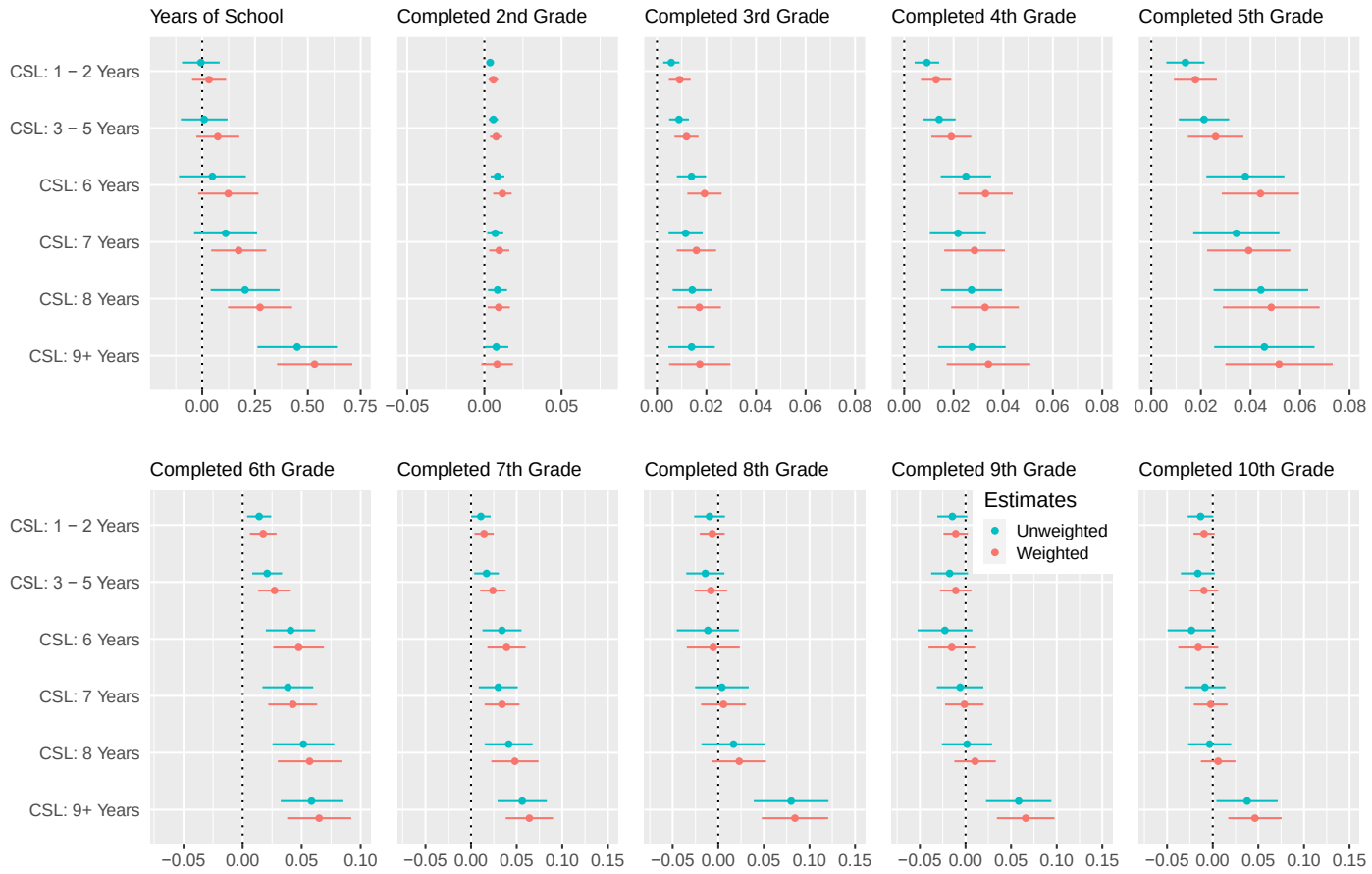


Figure 4. The Effects of Compulsory Schooling on Grade Completion at the Population-level
Data source: IPUMS MLP 2021; Princeton University CLP 2021.
NOTE. – This table displays the coefficient estimates of the effect of CSLs from equation (1) for a different outcome. The outcome analyzed is the displayed at the top of each panel. Standard errors clustered on G2's state of birth are used to generate 95% confidence intervals around each point estimate. The effects of compulsory schooling on overall years completed are positive for more aggressive laws and concentrated in grades 3 through 7. For grades 8 through 10, only more aggressive laws compelling 8 or 9 years of school generated additional completion at the population-level. Estimates weighted to the 1940 Decennial Census as well as unweighted estimates are shown.

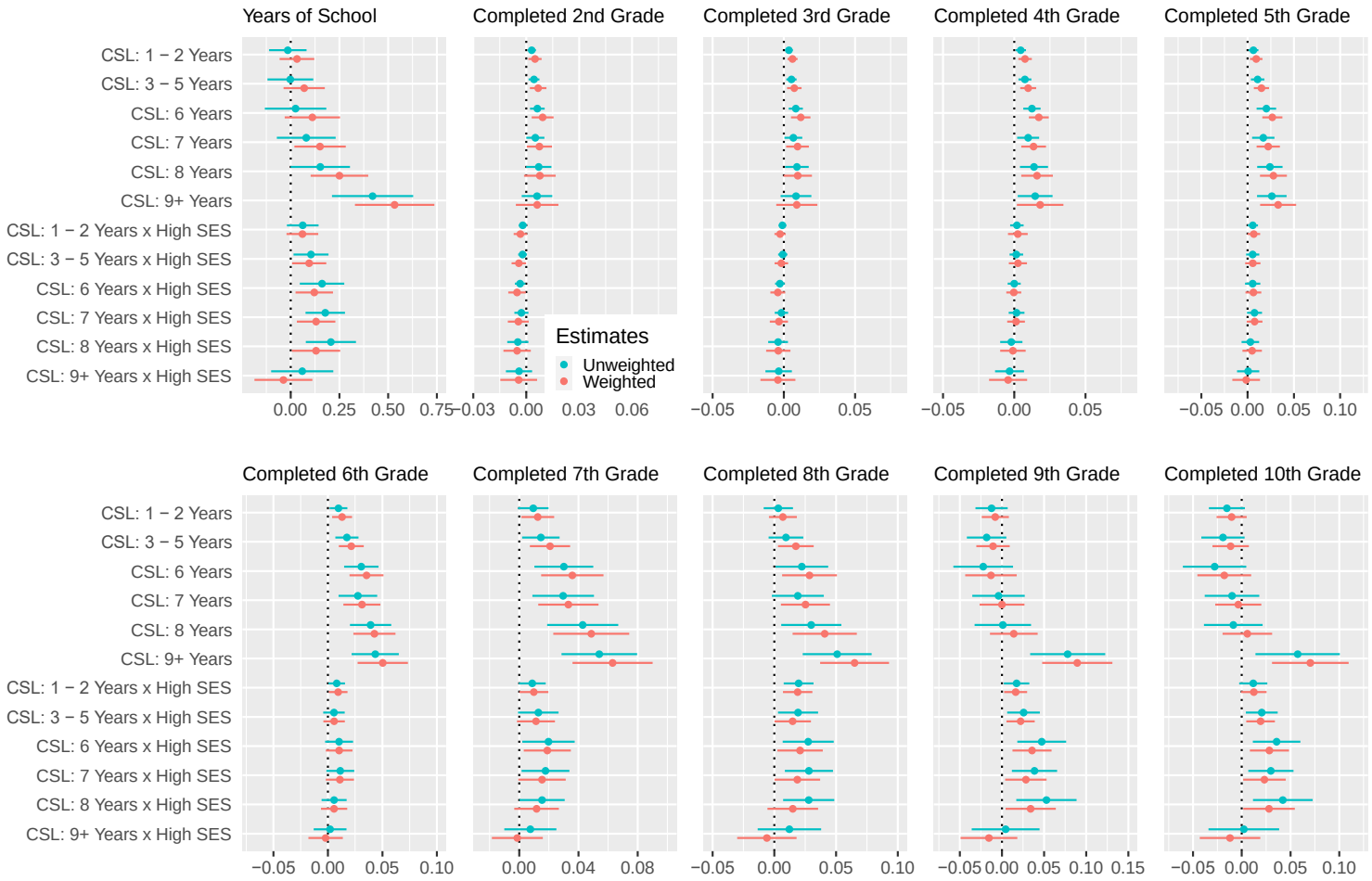


Figure 5. The Effects of Compulsory Schooling on Educational Inequalities
Data source: IPUMS MLP 2021; Princeton University CLP 2021.
NOTE. – This table displays the coefficient estimates of the effect of CSLs from equation (2) for a different outcome. The outcome analyzed is the displayed at the top of each panel. Standard errors clustered on G2's state of birth are used to generate 95% confidence intervals around each point estimate. The first six estimates of each panel show the effects of the CSL on low SES sons' grade completion. The bottom six estimates are the additional effect for high SES households. If the bottom six estimates are less than zero, then the CSL has an equalizing effect compared to a baseline with no CSL. In terms of years of school completed, CSLs were never equalizing and sometimes disequalizing. In terms of grade thresholds completed, CSLs had small equalizing effects at lower grade levels and larger disequalizing effects at higher grade levels. Notably, the most aggressive CSL compelling 9 or more years never had a statistically significant disequalizing effect, but CSLs compelling 6 – 8 years of schooling were often disequalizing for the outcomes of completing 8$^{th}$ grade, 9$^{th}$ grade and 10$^{th}$ grade. Estimates weighted to the 1940 Decennial Census as well as unweighted estimates are shown.

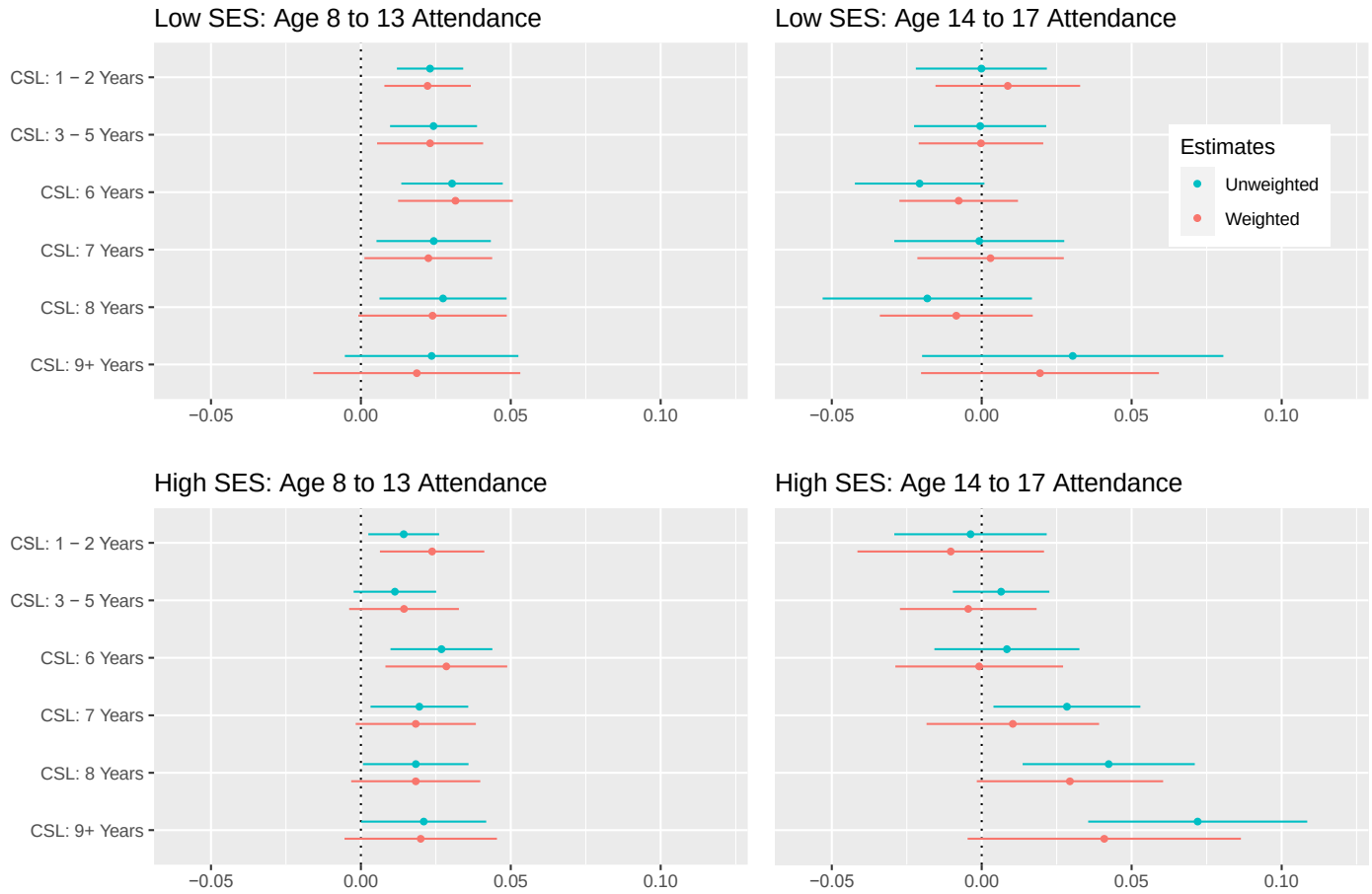


Figure 6. The Effects of Compulsory Schooling on School Attendance by SES Group
Data source: IPUMS MLP 2021; Princeton University CLP 2021.
NOTE. – This table displays the coefficient estimates of the effect of CSLs from equation (1) school attendance propensity for different age – SES groups. The SES group and outcome analyzed is displayed at the top of each panel. Standard errors clustered on G2's state of birth are used to generate 95% confidence intervals around each point estimate. The top left panel shows that the laws increase the attendance of the low SES group at age 8 to 13 relative to a baseline. However, they have noisy effects, centered at 0, on the low SES's propensity to attend school at age 14 to 17. On the other hand, the high SES group is also more likely to attend school at ages 8 to 13 (although the point estimates are slightly smaller than for the low SES group in all cases), but also more likely in some cases to attend school at ages 14 through 17 in response to a CSL compelling 7 to 8 years of attendance. Estimates weighted to the 1940 Decennial Census as well as unweighted estimates are shown.

## Tables

Table 1: Understanding the Sources for the 2-Generation Linked Sample

| Data Source | Matches Possible |
|---|---|
| NBER Census Linking Project (CLP) | G1-G2, G1 to itself across Censuses, G2 to itself across Censuses |
| IPUMS Multigenerational Linking Project (MLP) | G1 to itself across Censuses, G2 to itself across Censuses |

NOTE. – This table illustrates the data sources that we pooled for this project. We pool all possible father-child linkages from the NBER Census Linking Project. Then, we add in all of the father-child linkages (G1-G2) from the IPUMS Multigenerational Linking Project (MLP). It is common to find a G1-G1 or G2-G2 linkage from the IPUMS MLP not available in the G1-G1 linkage from the NBER Census Linking Project and vice-versa. When this happens, we add all potential G1-G1 and G2-G2 links to our initial sample.

Table 2: State Compulsory Schooling Laws in the Main Sample Analysis (Pre-1895)

| State | Year | Mandatory Entry Age | School Leaving Age |
|---|---|---|---|
| Massachusetts | 1852 | 8 | 14 |
| District of Columbia | 1864 | 8 | 14 |
| Vermont | 1867 | 8 | 14 |
| Michigan | 1871 | 8 | 14 |
| New Hampshire | 1871 | 6 | 16 |
| Connecticut | 1872 | 8 | 16 |
| Nevada | 1873 | 8 | 14 |
| California | 1874 | 8 | 14 |
| Kansas | 1874 | 8 | 14 |
| New York | 1874 | 8 | 14 |
| Maine | 1875 | 8 | 15 |
| New Jersey | 1875 | 8 | 14 |
| Wyoming | 1876 | 7 | 16 |
| Ohio | 1877 | 8 | 14 |
| Wisconsin | 1879 | 7 | 15 |
| Illinois | 1883 | 8 | 15 |
| Montana | 1883 | 8 | 14 |
| North Dakota | 1883 | 10 | 14 |
| South Dakota | 1883 | 10 | 14 |
| Rhode Island | 1883 | 7 | 15 |
| Minnesota | 1885 | 8 | 16 |
| Washington | 1886 | 8 | 18 |
| Idaho | 1887 | 8 | 18 |
| Nebraska | 1887 | 8 | 18 |
| Oregon | 1889 | 8 | 18 |
| Colorado | 1889 | 8 | 14 |
| Utah | 1890 | 10 | 18 |
| New Mexico | 1891 | 7 | 16 |
| Pennsylvania | 1895 | 8 | 13 |

NOTE. – Data are from Clay et al. (2021) and augmented with Rauscher (2016: 1709 – 1711) for years before 1870. Contradictions between Clay et al. (2021) and Rauscher (2016), e.g. New Mexico's first effective CSL, are always resolved in favor of Clay et al. (2021). The **Year** column shows the year that the CSL was passed for a particular state. The **Mandatory Entry Age** shows the age at which children were compelled to attend school at the time of the CSL's passage. The **School Leaving Age** shows the age at which children were allowed to leave school when the law was first passed, without accounting for potential exemptions.

Table 3: Sample Descriptive Statistics

| Variable | G1 | G2 |
|---|---|---|
| Count Appearing in 1940 Census | 1456094 | 4661639 |
| Count Appearing in 1930 Census | 2096276 | 4327381 |
| Count Appearing in 1920 Census | 2571684 | 3979047 |
| Count Appearing in 1910 Census | 2363792 | 2555093 |
| Count Appearing in 1900 Census | 1773264 | 1320511 |
| Count Appearing in 1880 Census | 820504 | 280657 |
| Count Appearing in 1870 Census | 419014 | 59718 |
| Count Appearing in 1860 Census | 215837 | 0 |
| Count Appearing in 1850 Census | 181008 | 0 |
| Count Farming | 1863204 | 884274 |
| Minimum Birth Year | 1786 | 1865 |
| Mean Birth Year | 1869 | 1901 |
| Mean Years of Schooling | 7.40 | 9.46 |
| Required Schooling: | | |
| Percent Zero Years | 0.667 | 0.014 |
| Percent One to Two Years | 0.031 | 0.024 |
| Percent Three to Five Years | 0.064 | 0.052 |
| Percent Six Years | 0.177 | 0.178 |
| Percent Seven Years | 0.041 | 0.122 |
| Percent Eight Years | 0.020 | 0.369 |
| Percent Nine or More Years | 0.000 | 0.117 |

NOTE. – Data are from author's calculations from our linked G1-G2 sample

Table 4. Understanding Grade Retention and Spillovers with Mare Models

Marginal Odds Ratios of Advancing to Next Grade Conditional on Advancing to Previous Grade

| | **Grade 7 Completion (Completed Grade 6)** | | **Grade 8 Completion (Completed Grade 7)** | | **Grade 9 Completion (Completed Grade 8)** | |
|---|---|---|---|---|---|---|
| **Compelled Years of Schooling** | Low Social Origin | High Social Origin | Low Social Origin | High Social Origin | Low Social Origin | High Social Origin |
| 3 to 5 Years | 0.939 | 1.02 | 0.945 | 1.02 | 0.950 | 0.981 |
| | (0.127) | (0.026) | (0.012) | (0.023) | (0.007) | (0.013) |
| 6 Years | 0.941 | 1.007 | 0.923 | 0.964 | 0.950 | 0.995 |
| | (0.013) | (0.025) | (0.011) | (0.021) | (0.008) | (0.012) |
| **7** Years | 0.991 | 1.023 | 0.927 | 1.01 | 1.02 | 1.041 |
| | (0.016) | (0.031) | (0.013) | (0.027) | (0.010) | (0.015) |
| 8 Years | 1.053 | 1.191 | 0.977 | 1.022 | 1.075 | 1.104 |
| | (0.040) | (0.039) | (0.014) | (0.029) | (0.011) | (0.016) |
| 9 Years or More | 1.446 | 1.542 | 1.050 | 1.161 | 1.351 | 1.264 |
| | (0.039) | (0.074) | (0.023) | (0.045) | (0.019) | (0.024) |

*Data source*: IPUMS MLP 2021; Princeton University CLP 2021.
*Notes*: This table displays the coefficient estimates of the effect of CSLs from equation (3). Standard errors, clustered on G2's state, are in parentheses. Control variables, including state and birth year cohort, are omitted from the Table.

## Appendix A: Extending Clay et al. (2021) and Refining School Effect Estimates

Modeling the number of years of compelled schooling is non-trivial because children were not just subject to minimum leaving ages and mandated entry ages. Early CSLs often exempted children from them CSLs if they had completed a certain number of years of schooling already (effectively lowering the minimum leaving ages, given a cohort's educational history) or could obtain a job (setting a minimum leaving age of whatever the minimum child labor law age was). Therefore, understanding the effective number of compelled years of schooling required requires both simulating changes in the laws and the life course of a child born in each cohort from ages 5 to 17 to incorporate how many years of schooling they would have already had.

We first reproduce Clay et al.'s (2021) simulation in R from their raw coding of the laws. Next, we augment Clay et al.'s CSL policy simulator with additional primary and secondary sources, e.g. those used in Rauscher et al. (2016) to score every possible birth year cohort and birth state combination from 1838 to 1912 by how many years of compelled schooling they would have needed. This procedure should in general be expected to yield more precise estimates compared to prior work and yields a difference in how we code some states mandated years of schooling compared to Rauscher (2016), such as New Mexico. Within a state, all birth cohorts born prior to the first cohort with 1 compelled year of schooling are assumed to have zero years of compelled schooling.

Clay et al.'s (2021) policy simulation models every legal change we found in the life course of every individual in our sample from age 5 to age 17. The details of this simulation attempt to account for all legal changes that take place during the life of the child. For instance, consider a child who had been attending school from a mandated entry age of 7 and a minimum school leaving age of 14. A law passed when that child was 12 that exempting children from CSLs after 5 years of schooling would result in everyone from that child's birth cohort only being assigned 5 years of compelled schooling rather 7, even though the minimum school leaving age remained at age 14 for that state. Clay et al. (2021) point out that the constraint that kids could leave school after achieving a certain number of years of education was more often the binding constraint for many children, rather than the CSL's exit age. Therefore, modeling exposure to CSLs requires modeling the child's life prior years of compelled school exposure from ages 5 to 17. Because Clay et al.'s (2021) simulation rules are complex, we direct readers to their Appendix for additional details.

**Appendices B and C are in progress**