The Labor Market Impact of K-11 vs. K-12

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Abstract

A high school education for students in several U.S. states spanned only eleven grades until as late as 1960. When these states transitioned to a twelve-year system, adjacent birth cohorts holding the same educational credential exogenously received different amounts of schooling. We analyze this natural experiment to evaluate the long-run labor market impact of this extra year. Using a two-stage difference-in-differences analysis, we find that cohorts exposed to one more year of education earn about 5-8% more in real labor income, though they are no more or less likely to graduate from high school. Occupational income scores increase as well, which suggests occupational upgrading. Consistent with human capital accumulation, a life cycle analysis finds the strongest effect in middle age.

Keywords: secondary education, human capital, labor market signaling, policy evaluation, returns to education

JEL Codes: H7, I26, I28, J24

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1 Introduction

There was no high school class of 1949 in Baton Rouge, Louisiana. Turn the pages of the 1948 yearbook for Istrouma High School and you flip directly from the senior class photo on page 25 to the sophomores on page 26. There are no sophomores in the 1947 edition; no freshmen in 1946. The gap marks the transition in the state's public school system from an eleven-year elementary and secondary education program to the twelve-year one that had become the national standard. For a cohort of children born in Louisiana in the early 1930s, a few months' variation in birthday meant the difference between attainment of a high school diploma representing eleven years of actual education or twelve.

Louisiana was not a special case. Although places like Chicago offered twelve grades going back to at least 1878 (Daily Inter Ocean 1878), a survey from the Bureau of Education of the U.S. Department of Interior found that ten states still had a total of 861,228 pupils enrolled in an eleven-year school system as of the 1926-1927 school year (Jessen 1929). Many schools in Utah, Maryland, New Hampshire, and Missouri had already added the twelfth year, yet full statewide take-up did not occur until the early 1940s. North and South Carolina each eventually adopted the twelve-year program around the same time as Louisiana. Texas transitioned primarily from 1936 to 1946, and Virginia and Georgia were the last U.S. states to finish switching over in the 1950s.

The "K-12" program has been the standard for so long that from a contemporary perspective it may be surprising to learn that its adoption was contentious, with its utility hotly debated for decades. A survey commissioned by the Louisiana State Legislature found that while 81% of teachers favored adding an eighth grade between junior high and high school, not a single school superintendent favored the idea (Louisiana Educational Survey Commission 1942). Schools in Salt Lake City, Utah even cut the twelfth year from the curriculum in 1928 to reduce costs, not restoring it until the fall of 1946 (Buchanan 1993). Another concern was the opportunity cost. Lower-income parents surveyed in Texas worried that a high school education was a prerequisite to many jobs, and an extra year meant a delay in their children's ability to help support the family (Watlington 2012).[1]

This transition among the last set of states to adopt the twelve-year program motivates our research question of interest: what is the human capital content of a twelve-year public education versus one completed in eleven? By comparing wages, occupational choices, and educational attainment in these states across birth cohorts adjacent to each side of the policy change, we analyze the economic impact of receiving what on paper or in census records would appear to be the same high school education except it contains an additional year of schooling.

[1] State legislatures occasionally still question the cost effectiveness of the twelfth year of public school. See `https://abcnews.go.com/WN/utah-mulls-eliminating-12th-grade/story?id=9853553` for news of a recent example in Utah.

We find that incomes are between 5-8% higher for individuals exposed to the twelve-year program. The change does not appear to significantly alter educational attainment, either of completion of high school or subsequent completion of college, suggesting that the labor market rewards the human capital earned in the extra year even though workers born on either side of the cutoff may share the same credentials. We also find evidence of occupational upgrading as measured by an increase in occupational earnings scores, which only vary at the occupation level. If workers were merely earning more for doing the same jobs as before, these scores would be unaffected.

The null effect on education suggests that student behavior is more in line with their following a "sheepskin" stopping rule in choosing their educational attainment as opposed to a year-by-year comparison of the costs and marginal rate of return. We find null or negative effects of exposure to the additional year of schooling on later mobility or employment, which we interpret as indicating that the shift in human capital was enough to increase labor market productivity but not so monumental as to alter major life cycle decisions. This may be unsurprising considering that comparable high school graduates were observationally equivalent; the families of many affected high school graduates were later not even aware of the change (Miller 2023).[2] Depending on the speed of employer learning at the time, early career life cycle events such as migration may have been determined before the returns to the increased human capital were reflected in higher wages. When we conduct the analysis interacting treatment with age to investigate the policy impact over the life cycle, we see the strongest earnings effects for those in their mid-30s or older, which again supports a theory that they are accumulating additional human capital that becomes evident over time as opposed to being immediately observed by firms at the start of their careers.

Education plays a key role in shaping economic outcomes, with proposed channels including the development of human capital (Oreopoulos and Petronijevic 2013; Psacharopoulos and Patrinos 2018; Hanushek and Woessmann 2020) and signaling of already developed human capital to employers (Spence 1973; Lang and Kropp 1986; Weiss 1995; Arcidiacono et al. 2010). Accordingly, a different amount of educational content underlying a high school diploma could affect labor market outcomes for several reasons. Under the classical model of education as an investment in human capital (Becker 1964), the additional year of schooling would increase wages for degree completers in the treated birth cohorts by the marginal return to education. Estimates of the average marginal return, based on Mincer models (1974), generally range from 10% to 20% (Card 1999). These models assume that individuals continue schooling until the internal rate of return equals the rate of discount. However, social pressures or institutional breakpoints, such as the division between middle and high school, could lead students to complete high school regardless of whether

[2] When shown the missing classes in the pages of the archived yearbooks, the genealogist at the East Baton Rouge Parish Public Library called her 93-year-old mother from the reading room to verify.

it requires eleven or twelve years. Evidence in favor of the hypothesis that an additional compulsory year of high school increases human capital is that it raises Armed Forces Qualification Test (AFQT) scores for minorities by about 0.3 standard deviations (Cascio and Lewis 2006).

An alternative model of education's value is based on the signaling effect of the diploma and how earnings rise sharply after completing milestones like high school or college (Hungerford and Solon 1987; Belman and Heywood 1991; Jaeger and Page 1996; Park 1999; Aryal et al. 2022).[3] To explain a wage increase resulting from the shift to the twelve-year program, such a model would require a substantial change in the composition of who completed high school. Moreover, the literature on employer learning argues that firms without any information about an individual may rely on an external signal such as a degree at first, but then as that worker's productivity is learned over time wages increasingly adjust to reflect that revealed information (Lange 2007; Khoo and Ost 2018; Aryal et al. 2022; Hansen et al. 2024). Due to the decennial structure of the U.S. census, we first observe many birth cohorts in the labor force once they reach several years of experience.

Evidence from Kroch and Sjoblom (1994) indicates that the content of human capital matters more for earnings than the level of credential obtained, and empirical studies of individuals with high school credentials generally support this, finding that the amount of education received matters more than the credential itself. Findings in both Cameron and Heckman (1993) and Jepsen et al. (2016) are that traditional high school graduates earn more than observationally similar individuals who obtain a high-school equivalent (GED), though Tyler et al. (2000) find a positive signaling effect in the labor market from obtaining a GED for White high school dropouts. By contrast, Clark and Martorell (2014) compare students who barely pass or fail a high school exit exam and find no earnings effect from actually obtaining a degree.

Because years of schooling are often endogenous, many studies estimate the causal impact on earnings by instrumenting for years of schooling (Angrist and Krueger 1991; Card 1999; Oreopoulos 2006; Brunello et al. 2009). While these studies focus on marginal years of education, our setting allows for a clean causal estimate of the effect of structurally adding a year of school without changing the terminal degree for a broader population than that identified by a local average treatment effect. By leveraging an exogenous shift in the amount of education underlying attainment of the same diploma, we avoid confounders such as selection bias, credential changes or other types of distinctions. Our approach to identification is conceptually similar to how Arteaga (2018) examines a change in the length of the college curriculum and the earnings impact after the amount of coursework needed to graduate was reduced—a change in the opposite direction from the policy in Louisiana. In that case, wages for students with the diploma requiring less education were about 13-16% lower for business and economics majors, though the scope of the policy change in that

[3] Also called "sheepskin effects."

study was limited specifically to students from those majors at one university in Colombia. Gong and Pan (2023) exploit a discontinuity leading some college graduates in Singapore to receive an additional year of undergraduate education, estimating that it produces a 12% increase in earnings. However, by design, they estimate together the value of the additional year with that from earning honors, as only honors students could qualify for the program. Khoo and Ost (2018) show that an honors designation has value in the labor market in and of itself.

Expanding beyond these specific treated populations, we examine a policy that altered the structure of high school uniformly across several entire states without altering the diploma itself. Unlike reforms that target specific schools or programs, these shifts affected all students and operated at the scale of full local labor markets. The focus on high school is especially salient in this historical context, when completing high school marked the end of formal education for most individuals and directly preceded labor market entry. These statewide policy changes in our sample are more similar in scope to a 2003 German reform that eliminated the thirteenth year of schooling. Previous literature has examined the effect of that reduction in years of schooling on test scores (Büttner and Thomsen 2015), grade point averages (Huebener and Marcus 2017), and university enrollment (Marcus and Zambre 2019). Besides being a change in the opposite direction, the German setting also differs from that in Louisiana because the curriculum remained unchanged: students covered the same material in less time, making that a question more of educational effectiveness than of quantity. Krashinsky (2014) also analyzes a one-year reduction in high school requirements in Ontario, Canada, though the outcome of interest is performance in university courses and is therefore limited in scope to the subpopulation of individuals who select into college. For neither case has the literature examined post-schooling labor market outcomes reflecting potential changes in human capital, as we do here.

A closely related literature synthesized by Kraft and Novicoff (2024) examines how the quantity of instructional time within a given curriculum affects outcomes, whether that be by extending the length of the school day (Figlio et al. 2018; Padilla-Romo 2022), the number of days in the school week (Thompson 2021), or the length of the school year within a grade (Parinduri 2014). Our setting differs in that it adds an entire additional year of new curricular content without altering the credential obtained, but this still raises the question of whether the timing and distribution of the extra time in school matter. Most school districts in our sample independently converged on expanding elementary school instead of "pyramiding" a year onto the end of high school. Does one year's worth of education inserted between elementary and secondary school produce the same marginal increase in human capital as one year's worth of education allocated at the end of high school, or as an extra hour per day over many years, or as extra days within each year? The answer holds important implications for the debate about lengthening the public school curriculum by adding pre-kindergarten. While we cannot resolve this question definitively with our setting, the fact that the 7% return we observe emerges gradually over the life cycle among otherwise observationally equivalent individuals from adjacent cohorts for whom the additional year was not reflected in any observable credential suggests that the human capital accumulation from the extra schooling operates even when it is invisible on a resume. We return to the question of generalizability in Section 6.

2 State variation in the transition to the twelve-year program

Education policy in the United States is set by the individual states, and to a lesser extent the local school districts within those states. Hence it was possible for nearly every high school graduate from the class of 1926 in New Hampshire, along with about 80% of all other graduates nationwide, to have completed the twelfth grade, while every diploma in Louisiana and South Carolina was issued to eleventh graders. In that year, for the first time, Texas offered a twelve-year program exclusively to students enrolled in the Port Arthur district of Jefferson County. By 1936 there were only 13 Texas districts with a twelve-year program, and it did not become a statewide accreditation requirement until 1943 (Watlington 2012). President Jimmy Carter graduated from Plains High School in Georgia after the eleventh grade in 1941 (National Park Service 2026), nine years before full statewide adoption of the twelve-year program, although he would have been a twelve-year graduate if he were from Bibb, Chatham, Glynn, or Polk County (covering about 17.5% of Georgia students) where the system had already been in place for decades.[4]

By 1892 the "Committee of Ten", a group of educators assembled by the National Education Association to promulgate best curricular practices, was recommending twelve years as the standard elementary and secondary course (National Education Association of the United States. Committee of Ten on Secondary School Studies and United States. Bureau of Education 1893). The persistence of eleven-year systems in a handful of states reflected the decentralized and path-dependent nature of American education policy. During the late nineteenth and early twentieth centuries, secondary education expanded rapidly across the country in what Goldin (1998) terms the "high school movement." As Goldin and Katz (2008) document, this expansion was driven primarily by local communities responding to rising demand for educated workers, with relatively little federal coordination. The result was substantial cross-state (and sometimes within-state) variation in the structure of the curriculum. Given the high fixed costs of changing systems once a curriculum was in place, absent external pressure states that had settled on an eleven-year plan had little incentive to change, particularly in the earlier part of the 20th century when high school was less of a standard expected degree of educational attainment.

[4]Details on individual state enrollment levels and implementation of the twelve-year program come from the authors' personal review of archived copies of historical state department of education reports, which are typically published on an annual or biennial basis.

When pressure to change did rise, the motivation was strikingly similar across states. Education officials emphasized that their graduates were less well-prepared than peers from twelve-year states, citing lower performance on standardized tests or in college. In South Carolina, surveys found that their eleven-year graduates had lower achievement scores than graduates of twelve-year systems, and even as college sophomores they performed on par with freshmen from twelve-year states (South Carolina State Superintendent of Education (1945) pp. 96–97). In Texas, early data from the Port Arthur district, which was the first to switch over, showed that college failure rates were higher among its eleven-year graduates (Watlington 2012). Louisiana's Educational Survey Commission likewise concluded that the state's students were at a competitive disadvantage, a view shared by gubernatorial candidate Jimmie Davis, who argued that "most other states give their children twelve years of schooling and so our children have that competitive disadvantage" (The Times-Picayune 1944c). Proponents across states also emphasized that seventeen-year-old graduates were too young to enter the workforce or adapt to college, that a twelve-year curriculum allowed for more elective coursework, and that only a minority of students continued to post-secondary education, making the quality of the high school experience all the more consequential. Opposition centered primarily on financial cost for the district, opportunity cost for the students and their families, and, unrelated to academics, concerns that schools maintaining an eleven-year system would be at a competitive disadvantage in interscholastic athletics. Encompassing several of these objections, the Salt Lake City district in Utah originally had a twelve-year system, dropped it in the 1920s to cut costs, and then re-added it in 1945, with one motivating factor purportedly being that their sports teams were at a disadvantage because their seniors were a year younger than the competition (Buchanan 1993).

The consistency of these motivations across states and over two decades suggests that the transitions were driven by gradual convergence toward a national norm rather than by state-specific economic conditions, supporting the plausibility of treating the timing of adoption as exogenous to local labor market trends. Although there were no uniform standards for how to implement the change, records indicate that the preponderance of schools expanded by inserting an additional grade at the end of the elementary curriculum, before the first year of high school. In most cases this was not simply a matter of stretching the existing content across an additional year; state education officials emphasized that the reorganization involved new preparatory material at the elementary level and curricular changes across all grades (South Carolina State Superintendent of Education (1944) p. 71). Over time, many districts further restructured along the lines of the increasingly standard 6-3-3 model of six elementary, three junior high, and three senior high school grades. Further details on the motivation, timeline, and implementation process for each state appear in Appendix B.

Even after its adoption, the value of the additional year remained contested. In 1955, the Georgia

General Assembly introduced a resolution to eliminate the twelfth grade, with some concurring letters to the editor calling it a "loafing" grade and others arguing that the extra year made students more mature and better able to compete in college and the business world (Athens Banner-Herald 1948; Montjoy 1955). In 1983, a North Carolina legislative committee studied whether to eliminate the twelfth grade, citing concerns that some students were not benefiting from it and that the state was spending over $55 million annually on a year many used to "slide" through with minimal coursework (North Carolina. General Assembly. Legislative Research Commission 1983). These debates mirror some of the questions our paper seeks to answer empirically, namely, "What were the returns to this additional amount of time invested in school?"

As there is no pre-existing secondary source documenting the timing or process of the adoption of the twelve-year program for most states, we assemble our own.[5] We take as a starting point the 1926 federal government survey indicating which states still had any students graduating high school under an eleven-year program (Jessen 1929), reproduced as Table 1. Ten states still had students in an eleven-year system at the time, though they were in various stages of transitioning. For all intents and purposes, New Hampshire and Missouri had already fully switched over. Maryland was halfway to statewide coverage. Utah as noted above originally had a statewide twelve-year system, and the less-than-full adoption intensity in the figure reflects the Salt Lake City district having dropped the twelfth year around 1925 before switching back again in 1945.

Table 1: Reported distribution of students in 11-year and 12-year school systems as of 1926, by state (Jessen 1929)

State	Students in 11-year systems	Students in 12-year systems	% 12-year system
Georgia	652,907	40,000	5.8%
Louisiana	400,402	-	0.0%
Maryland	118,064	141,541	54.5%
Missouri	13,367	412,534	96.9%
New Hampshire	3,426	74,248	95.6%
North Carolina	782,602	41,549	5.0%
South Carolina	471,701	-	0.0%
Texas	1,210,127	7,945	0.7%
Utah	32,143	106,614	76.8%
Virginia	512,520	36,797	6.7%
Total	4,197,259	861,228	17.0%

To document each state's transition path, we collected digital scans of historical annual statistical reports from each state's Department of Education or equivalent institution from the early 1920s through the 1950s. From these reports, we constructed annual measures of the fraction of a state's students exposed to a twelve-year program in their district. Our preferred method was to count enrollment in districts listing twelve grades and divide by total enrollment.[6] When district-level grade enrollment was unavailable, we imputed

[5] With the exception of a dissertation on the process in Texas (Watlington 2014).

[6] Certain states report White and Black enrollment separately in many years. Although the proportions generally appear

the fraction by dividing total twelfth-grade enrollment by adjusted eleventh-grade enrollment, allowing for year-to-year attrition.[7] During their main transitions, North Carolina and South Carolina directly reported the number of graduates from eleven- and twelve-year programs, which we used where available.

We present our estimated adoption paths visually in Figure 1. For each state and birth cohort, the degree of shading indicates our estimate of the fraction of students in that cohort who would face a twelve-year program in order to graduate from high school. To assign birth cohorts, we identify the spring graduation year of each affected class from the state reports and subtract 18, based on the typical age at high school graduation.[8] Louisiana and South Carolina transitioned quickly statewide over a two-year period in the mid-1940s, affecting cohorts born in the early 1930s. North Carolina did as well, though a fraction of districts had a twelve-year program for many years prior, consistent with Jessen (1929). In Georgia, a subset of districts maintained a twelve-year program for many years, but the statewide mandate came in the early 1950s, affecting birth cohorts in the mid-1930s. Like most states, Georgia gave districts several years to come into compliance. Virginia followed a similar trajectory with a longer grace period. Watlington (2012) reports that as of 1936 only 13 schools in all of Texas had adopted a twelve-year program; adoption rose rapidly after the state legislature made it the standard in the early 1940s.

Because the twelve-year program was primarily implemented by adding a new year of schooling, the "eighth grade," at the end of elementary school and before secondary school, students already in high school at the official time of adoption could still graduate under the eleven-year system. Only those in the seventh grade or below at the time would ultimately face twelve grades in order to obtain a diploma, meaning there is a lag between a district's official adoption year and the first graduating class affected. For example, the majority of Louisiana districts held their first eighth-grade class in fall 1945; those students graduated from the twelfth grade in spring 1950, making the 1932 birth cohort the first to be fully treated. A small number of districts, including Caddo and Orleans Parishes, started one year earlier, partially treating the 1931 birth cohort. For South Carolina, the first partially treated cohorts are those born in 1930, with the 1931 birth cohort being the first nearly fully treated.[9] In Georgia, we assign our calculated annual average of 17.5% for all cohorts born before 1934, reflecting the share of districts that had maintained a twelve-year program for decades; adoption then rises to 77.5% for the 1934 birth cohort before becoming universal for those born in 1935 and later. Given natural variation in when students start school, repeat grades, or skip grades, these

consistent across racial groups, in cases of ambiguity in reporting we use White enrollment counts and shares.

[7]In states where the historical reports indicated the change by listing enrollment in an eighth grade where previously they had listed enrollment in seventh grade followed by first year of high school, we divided eighth-grade enrollment by adjusted seventh-grade enrollment.

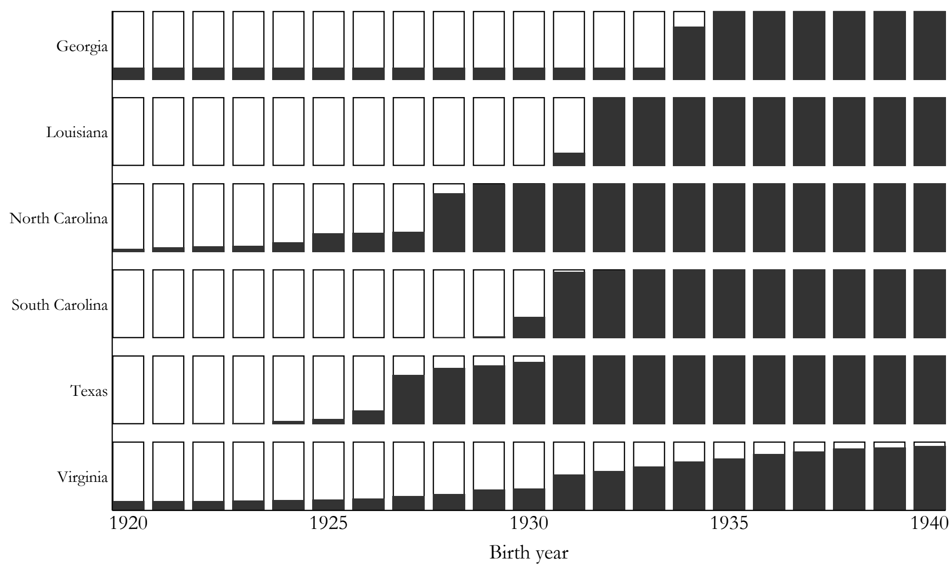
[8]Equivalently, when the change is reported as enrollment in a new eighth grade rather than a twelfth grade, we add four years of high school before applying the same calculation.

[9]Given a few early adopters and a few straggling districts, our measured intensities for South Carolina are specifically 1.1% for the 1928 cohort, 2.4% for 1929, 30.9% for 1930, 96.3% for 1931, 98.7% for 1932, and 99.7% for 1933.

assignments represent intention-to-treat status rather than strict individual treatment. Measurement error in individual treatment status will attenuate our estimated impact.

In our main empirical specification, we consider the six states with no or very low adoption rates as of the 1920 birth cohort to be our treated set, comparing them against states that always had a twelve-year system. In the Appendix, we test robustness to several alternative conceptions of treatment status.

Figure 1: Rates of twelve-year program exposure for each state, by birth cohort.



Notes: Adoption rates calculated by the authors based on information reported in the archival reports of each state's Department of Education or equivalent. They approximate the fraction of students in each birth cohort who are in a school district where they will face a twelve-year program to graduate from high school.

3 Data

Testing for the post-schooling impact of the shift to the twelve-year system requires data on labor market outcomes for individuals born in a wide range of states and observed at different points over the life cycle. We use the public-use U.S. decennial census microdata samples from 1950 through 1990 (Ruggles et al. 2024), which provide repeated cross-sections of individuals across cohorts and states. To encompass the staggered treatment adoption among our treated states, we restrict the sample to individuals born between 1920 and 1940, and to focus on individuals after schooling ages and before retirement, we include adults observed at ages 25–59.[10]

The census data provide information on earnings, occupation, education, and geographic mobility, along with basic demographic characteristics. Our main outcome is log wage and salary income, measured in

[10]In the appendix, we show that the estimates are not sensitive to this particular window of birth cohorts.

constant 2010 dollars. Because the outcome is defined in logs, it is only observed for individuals with positive wage income; however, we also look at an indicator for employment that is based on the full sample of individuals. We also consider occupational income scores and occupational education scores. The occupational income score assigns to each individual the median income associated with their occupation based on 1950 Census data and can therefore capture changes in occupation, or occupational upgrading. The occupational education score measures the share of workers in an occupation who completed at least one year of college, enabling a test for occupational upgrading on the basis of education. In addition, we examine indicators for any college attendance and whether an individual resides outside their birth state.

Summary statistics are reported in Appendix Table A1. The table describes individuals in always-12 states and in switcher states, separately for earlier and later cohorts. Observable characteristics differ across state groups. In particular, individuals in switcher states are more likely to be nonwhite and less likely to be homeowners or veterans than those in always-12 states. These differences reflect underlying regional variation in the sample rather than changes across cohorts within states.

4 Empirical framework

Our objective is to estimate the causal relationship between exposure to a high school education that includes one additional year of schooling and a set of economic outcomes that are plausibly affected by one's quantity of human capital. The key features of our empirical setting to account for in conducting causal inference are that the policy treatment of an additional year of education is staggered in timing across states, is continuous rather than binary in intensity within states, and increases in intensity within states over time through gradual adoption.

As widely documented in the econometrics literature, in this setting a traditional two-way fixed effects estimator would implicitly use later-treated units as controls for earlier-treated units, generating negative weights on some group-time comparisons and yielding estimates that are difficult to interpret and may lie outside the range of the underlying cohort-specific effects (Roth et al. 2023). To accommodate our specific empirical setup, we apply the two-stage difference-in-differences estimator proposed by Gardner (2022).[11] This approach separates the estimation of baseline outcome structure from the identification of treatment effects, ensuring that treatment effects are identified exclusively from comparisons between untreated cohorts and cohorts newly exposed to the policy.[12]

[11] We thank Kyle Butts for discussions of this estimator and its implementation with the coding package described in Butts and Gardner (2022).

[12] Although estimators such as that proposed by De Chaisemartin and d'Haultfoeuille (2020) could provide identification from within-unit variation in exposure levels, we prefer the two-stage approach because it most directly identifies the policy-relevant parameter: the effect of transitioning from an 11-year to 12-year education program. This method can still accommodate non-linear effects through binning of the continuous treatment variable in the second stage. We show in the appendix that

Let Y_{isc} denote the outcome of individual i born in state s belonging to birth year cohort c. Exposure to the 12-year program varies at the state–cohort level depending on the degree of adoption within a state at the time. For each state and birth cohort, define $D_{sc} \in [0, 1]$ as the fraction of students in birth cohort c born in state s who were exposed to a 12-year rather than an 11-year education system, based on the timing of policy implementation relative to their school progression. Our parameter of interest, β, is the causal effect of exposure to the 12-year program on earnings:

$$Y_{isc} = \alpha_s + \lambda_c + \delta_t + X'_{isc}\gamma + \beta D_{sc} + \varepsilon_{isc} \tag{1}$$

where α_s are state fixed effects, λ_c are birth-cohort fixed effects, δ_t are observation time (census year) fixed effects, and X_{isc} includes individual controls.

In the first stage of the estimation, we partial out permanent differences across states and common cohort and time effects by estimating:

$$Y_{isc} = \alpha_s + \lambda_c + \delta_t + X'_{isc}\gamma + \varepsilon_{isc}, \tag{2}$$

only using observations where $D_{sc} = 0$, which includes all birth cohorts from states that maintained 12-year programs throughout the sample period (as they never experienced the policy transition) and pre-treatment birth cohorts from states that adopted 12-year programs during our observation window. States that maintained 12-year programs throughout our sample period are coded as $D_{sc} = 0$ because our treatment variable measures exposure to the policy transition from 11 to 12 years, not the level of education itself. While these always-12-year states may have different baseline earnings levels than transitioning states, the state fixed effects α_s absorb these permanent differences under a parallel trends assumption.

Using these estimated effects, we construct residual outcomes $\tilde{Y}_{isc} = Y_{isc} - (\hat{\alpha}_s + \hat{\lambda}_c + \hat{\delta}_t + X'_{isc}\hat{\gamma})$. Under a cohort parallel trends assumption, removing the estimated fixed effects from untreated observations creates residuals that are orthogonal to treatment status, allowing unbiased estimation of the treatment effect in the second stage, where we regress the residualized outcomes on cohort-level treatment intensity:

$$\tilde{Y}_{isc} = \beta D_{sc} + u_{isc}. \tag{3}$$

The coefficient β captures the "dosage response" from exposure to the 12-year program: an average treatment effect on the treated of moving a cohort from zero to full exposure weighted across the treated states and cohorts. This coefficient more precisely represents an intention-to-treat (ITT) effect, as treatment assignment is based on birth state and cohort rather than the actual educational environment facing each specific

point estimates are similar in magnitude across the distribution of adoption intensity.

individual in their grade progression. Standard errors are clustered at the state level and adjusted using Gardner's (2022) variance formula that accounts for the two-stage estimation procedure. As an additional check, we also report inference based on resampling and randomization-based procedures that are more robust in settings with a limited number of clusters. All regressions are weighted using the person-level sampling weights from the public use microdata census samples.

4.1 Identifying assumptions

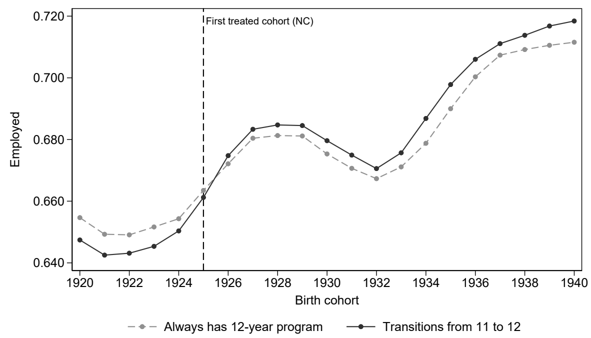
Our baseline two-stage difference-in-differences specification follows Gardner (2022) and assumes that, in the absence of the change in the number of years required to obtain a high school degree, untreated potential outcomes can be written as the sum of state-of-birth fixed effects and birth-cohort fixed effects. Under this assumption, persistent differences across states are absorbed by state fixed effects, and common cohort-specific shocks are absorbed by birth-cohort fixed effects. Conditional on these components and observed covariates, untreated potential outcomes are assumed to evolve similarly across states. Identification, therefore, comes from deviations from this state–cohort structure that coincide with exposure to the reform.

As a preliminary assessment of these assumptions, Figure 2 reports three-year rolling averages of mean outcomes in switcher states and always-12 comparison states. Across outcomes, both groups exhibit broadly similar patterns over time, with gradual increases in income and occupational measures across cohorts. For log income and occupational income scores, outcomes evolve in parallel for several cohorts prior to exposure, although levels differ between switcher and comparison states. For employment, any college attendance, and interstate migration, trends are generally smooth, with no sharp divergences prior to the reform. Because these figures aggregate across states and cohorts and do not condition on covariates or fixed effects, they should be interpreted as descriptive.

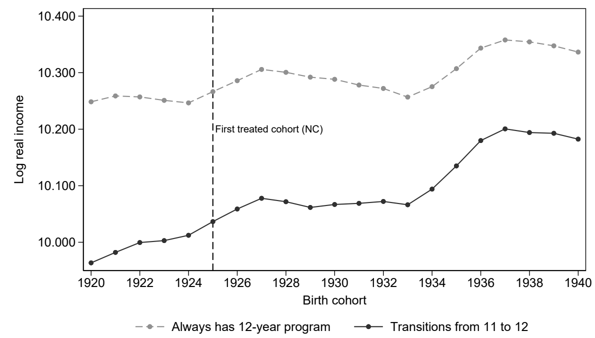
We next formally assess the parallel trends assumption following Gardner (2022) by estimating event-study specifications in the second stage and examining pre-treatment coefficients. In the two-stage framework, these coefficients are estimated using residuals from the first-stage regression and therefore measure average deviations from the maintained state–cohort structure. Pre-treatment coefficients should be interpreted as tests of whether these residuals are systematically different from zero prior to exposure. In contrast to the standard TWFE event-study interpretation, they do not directly estimate average differences in trends between treated and untreated states, but rather assess whether the additive structure assumed for untreated potential outcomes is consistent with the data (Gardner 2022).

Figure 3 presents the event-time estimates. Focusing on the pre-treatment periods, the coefficients for log income and occupational income scores are small in magnitude and statistically indistinguishable from zero,

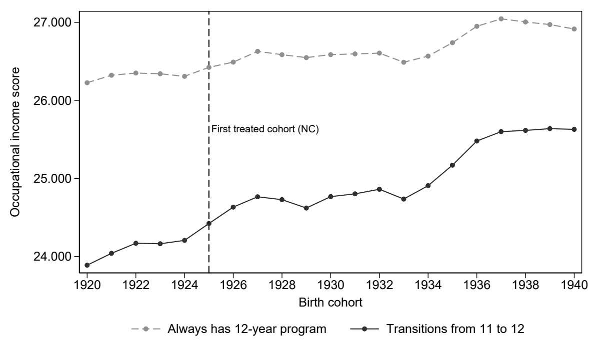
Figure 2: Mean Outcomes in treatment and control states



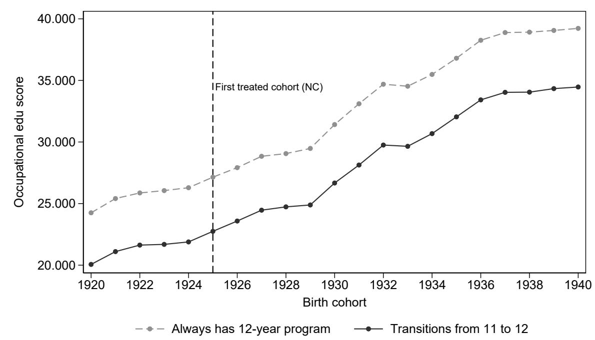
(a) Employed



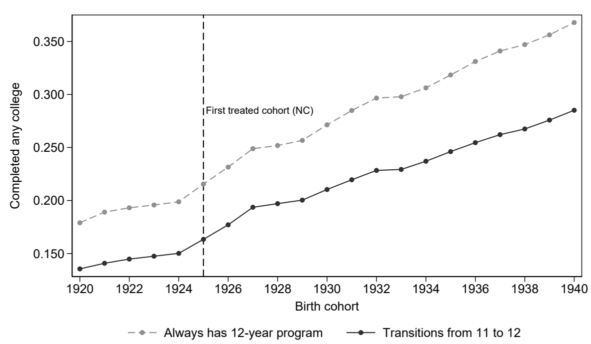
(b) Log real labor income



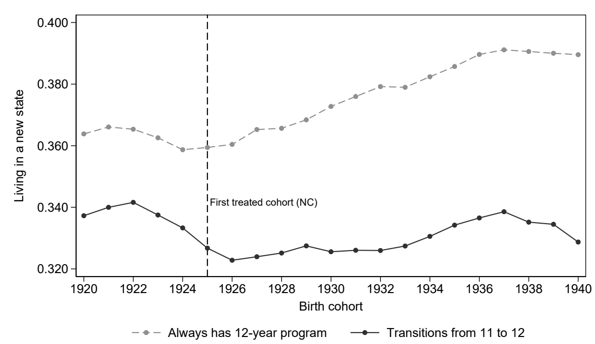
(c) Occupational income score



(d) Occupational education score



(e) Completed any college



(f) Living outside birth state

Notes: The figure reports three-year rolling averages of mean outcomes for individuals born between 1920 and 1940 and observed at ages 25–59 in the 1950–1990 IPUMS census samples. The outcomes shown in each panel are log income, occupational income score, occupational education score, an indicator for any college attendance, employment, and interstate migration. The estimation sample excludes Maryland, Missouri, Utah, New Hampshire, Alaska, and Hawaii. "Switcher" states are Georgia, Louisiana, North Carolina, South Carolina, Texas, and Virginia; all other states serve as always-12 comparison states.

providing supportive evidence for the state–cohort structure. For the remaining outcomes, pre-treatment coefficients are also generally close to zero, although in some cases, confidence intervals are wider and individual estimates fluctuate modestly around zero. Importantly, we do not observe systematic patterns of pre-treatment deviations that would indicate persistent departures from the assumed structure. While the absence of statistically significant pre-treatment coefficients does not prove the validity of the parallel trends assumption, these results are consistent with it and do not suggest meaningful violations in the pre-exposure period.

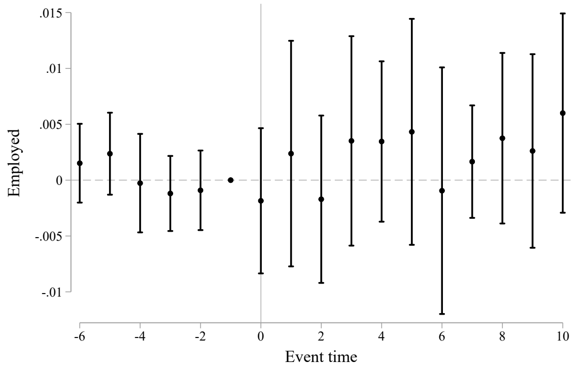
As an additional diagnostic, we implement the alternative first-stage procedure described in Section 2.6 of Gardner (2022). Rather than estimating the first stage using all untreated observations, we restrict it to (i) all never-treated states and (ii) eventually treated states observed only in their last pre-treatment cohort. Under the normalization that parallel trends hold in this final pre-exposure period, the event-time coefficients for earlier pre-treatment periods identify average deviations of eventually treated states relative to never-treated trends, analogous to the conventional interpretation of event-study pre-trend figures.

Figure A2 reports the corresponding estimates. Pre-treatment coefficients remain generally small and imprecisely estimated across outcomes, and we do not observe systematic evidence that earlier cohorts in treated states diverge from always-12 states trends prior to exposure. Confidence intervals are wider than in Figure 2, reflecting the reduced identifying variation under the restricted first stage. As before, these results provide suggestive evidence in support of the parallel trends assumption.

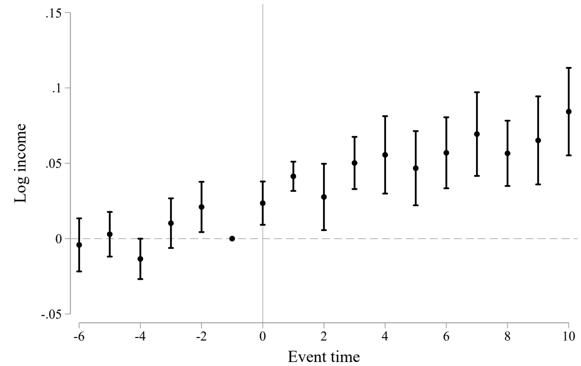
5 Results

Table 2 reports estimates of the effect of exposure to a 12-year high school requirement on adult outcomes. Column (1) shows no meaningful effect on employment. This suggests that the estimated effects on log income are not driven by changes in selection into employment. Exposure to the reform increases log income by 0.075, corresponding to roughly a 7.5 percent increase in earnings. The reform also increases the occupational income score by 0.604 points. The occupational income score assigns individuals the median income associated with their occupation based on 1950 Census data and therefore reflects the income level of the occupations individuals hold. The increase in income indicates a positive marginal return to the additional year of education. Our estimate aligns with values found by Angrist and Krueger (1991) for 1930s birth cohorts, which is notable given that theirs captures a very different margin, the local average treatment effect among compliers with compulsory schooling laws, whereas we are in a sense considering the ITT on "always takers" for high school completion. The estimate is consistent as well with a broader range of values in the literature on the rate of return to education (Card 1999), and the fact that we only estimate

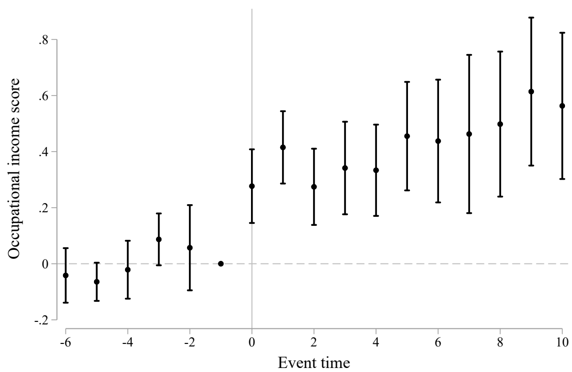
Figure 3: Event-time estimates



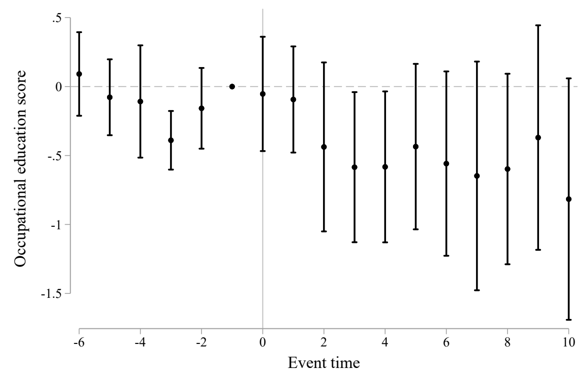
(a) Employed



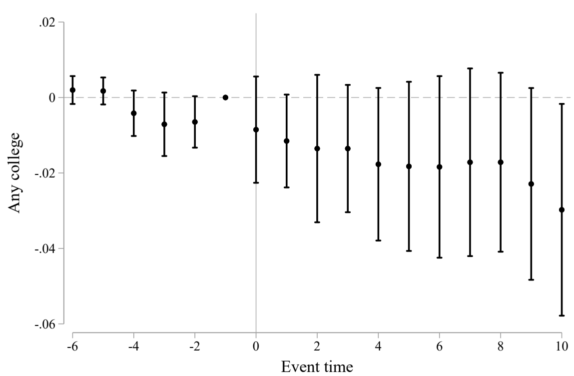
(b) Log income



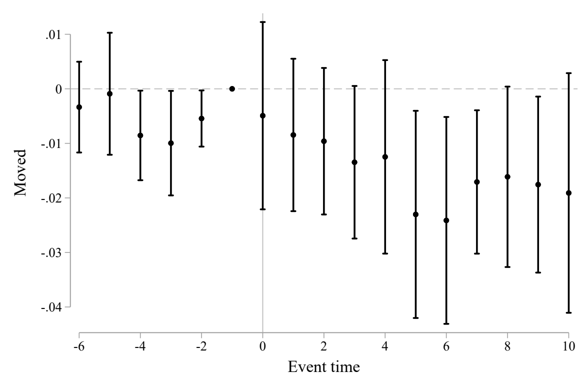
(c) Occupational income score



(d) Occupational education score



(e) Any college



(f) Moved

Notes: The figure reports event-time estimates examining the effect of exposure to a 12-year high school requirement on the outcomes shown in each panel: employment, log income, occupational income score, occupational education score, an indicator for any college attendance, and interstate migration. The sample includes individuals born between 1920 and 1940 and observed at ages 25–59 in the 1950–1990 IPUMS census samples. The estimation sample excludes Maryland, Missouri, Utah, New Hampshire, Alaska, and Hawaii. "Switcher" states are Georgia, Louisiana, North Carolina, South Carolina, Texas, and Virginia; all other states serve as always-12 comparison states. Estimates are obtained using a two-stage difference-in-differences procedure (Gardner 2022). The first stage includes birth-cohort fixed effects, census-year fixed effects, state fixed effects, and individual covariates (sex, race). All regressions are weighted using person weights. The omitted category is event time −1. The plotted window includes event times −6 to 10; the regression includes binned tails outside this window (not shown). Vertical bars denote 95% confidence intervals with standard errors clustered at the state level.

Table 2: Treatment Effects

	Employed	Log income	Occ. income score	Occ. education score	Any college	Moved
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment effect	0.004	0.075***	0.604***	-0.555	-0.022*	-0.021**
	(0.004)	(0.014)	(0.104)	(0.366)	(0.013)	(0.008)
Wild bootstrap p-value	0.425	0.000	0.000	0.265	0.170	0.050
Mean dep. var.	0.7	10.2	26.3	31.9	0.3	0.4
Effect relative to mean (%)	0.6	7.5	2.3	-1.7	-8.7	-5.8
N	5,602,836	3,699,966	4,602,131	4,552,905	5,602,836	5,602,836

Notes: The table reports estimates of the effect of exposure to a 12-year high school requirement on the outcomes listed at the top of each column: employment, log income, occupational income score, occupational education score, an indicator for any college attendance, and interstate migration. The sample includes individuals born between 1920 and 1940 and observed at ages 25–59 in the 1950–1990 IPUMS census samples. The estimation sample excludes Maryland, Missouri, Utah, New Hampshire, Alaska, and Hawaii. "Switcher" states are Georgia, Louisiana, North Carolina, South Carolina, Texas, and Virginia; all other states serve as always-12 comparison states. Estimates are obtained using a two-stage difference-in-differences procedure (Gardner 2022). The first stage includes birth-cohort fixed effects, census-year fixed effects, state fixed effects, and individual covariates (sex, race). All regressions are weighted using person weights. Standard errors, reported in parentheses, are clustered at the state level. Statistical significance stars correspond to these standard errors (not the wild bootstrap p-values). The wild bootstrap p-values, reported in a separate row, are based on 200 replications using Rademacher weights and state-level clustering. * $p<0.10$, ** $p<0.05$, and *** $p<0.01$.

an intention-to-treat effect could explain why it is attenuated modestly compared to the typical finding of about 10-12%. The estimate of 7.5% is somewhat lower than the 13% found by Arteaga (2018) from a change in course unit requirements in college or the 12% found by Gong and Pan (2023) for an extra year in college, though the latter estimate does combine the effect of the extra year with the effect of graduating with honors and represents a local treatment effect on the affected student population as opposed to an ITT.

Having occupational income score as a separate outcome enables a further decomposition of this effect. If treated individuals were simply earning more while doing the same work, we would observe a positive effect on income and a null effect on occupational income score. Alternatively, as we find, occupational income scores increase, which is suggestive of occupational upgrading. Our point estimate of 0.604 compared to the mean of 26.4 represents an increase of about 0.07 standard deviations.

Columns (4) and (5) report estimates for the occupational education score and an indicator for any college attendance. The occupational education score measures the share of workers in an occupation who completed at least one year of college. The estimate for the occupational education score is not statistically different from zero. Thus while the occupational income score results suggest movement into occupations that might require greater human capital, those are not necessarily occupations requiring college education. Considering that less than 10% of the population had a bachelor's degree or higher in 1950,[13] and that the stated objective of implementing the twelve-year program was often unrelated to preparation for college, these results are consistent with each other. In fact, the estimate for the effect of the twelve-year program

[13] See https://nces.ed.gov/programs/digest/d13/tables/dt13_104.20.asp.

on completion of any college is, if anything negative. Visual inspection of the mean plot for college in Figure 2 suggests that the policy did not decrease college attendance for the treated states so much as there was sharper positive upwards trend in college attendance among the controls. Column (6) shows that exposure to the reform reduces the probability of living outside the birth state by about 2 percentage points, though again this appears to be because of an increase in the controls rather than a decrease among the treated states. At the least, the evidence does not support a hypothesis that treatment with an additional year in the curriculum led to greater rates of interstate mobility. Because of these potential differential underlying trends seen in the mean plots for education and mobility, in Section 5.2 we repeat the main analysis while allowing for region-by-year linear trends.

5.1 Life cycle effects

To test for differential effects of the policy over the life cycle, we repeat our main analysis for the specification with log real labor income as the outcome, now interacting treatment status in the second stage with categorical bins for five-year age groups. The point estimates and implied treatment effects by age group are reported in Table 3. For the youngest exposed individuals under age 30, the estimated effect is actually negative. This could reflect the fact that their entry into the labor market is delayed by one year, leading to reduced cumulative experience or on-the-job learning. The estimated effect increases with age, from 1.5% for the 30-34 bin to 10.9% at ages 35-39. It peaks at 16.8% for ages 45-49 then declines for older age bins. The delay in observable effects until the late thirties is consistent with a human capital interpretation of the additional year of schooling. At younger ages, the effect having less experience dominates, while at higher ages, the returns to greater productivity are realized.

Table 3: Treatment Effects Over the Life Cycle

	25–29 (1)	30–34 (2)	35–39 (3)	40–44 (4)	45–49 (5)	50–54 (6)	55–59 (7)
Interaction estimate	-0.085***	0.099***	0.194***	0.211***	0.253***	0.203***	0.144***
	(0.012)	(0.017)	(0.017)	(0.020)	(0.022)	(0.026)	(0.029)
Treatment effect	-0.085	0.015	0.109	0.127	0.168	0.118	0.059

Notes: The table reports estimates of the effect of exposure to a 12-year high school requirement on log real labor income, with treatment status interacted with five-year age bins (25-29 is the omitted group). The sample and estimation procedure are the same as for the main results reported in Table 2. The point estimates on the interaction term are listed in the top row, and the implied treatment effect for each age group is in the bottom row. Standard errors, reported in parentheses, are clustered at the state level. * p<0.10, ** p<0.05, and *** p<0.01.

5.2 Regional Cohort Trends

The treated states in our sample are concentrated in the U.S. South, which experienced economic convergence with the North over the period we study (Barro and Sala-i-Martin 1992), attributed in part to improvements in school quality (Card and Krueger 1992). Our sample cohorts also overlap with the Great Migration, during which interstate migration patterns among Black Southerners shifted substantially (Collins 2021). To evaluate the potential influence of these regional trends, Appendix Table A2 examines the robustness of our results to the inclusion of birth region-by-census year interactions. These controls allow for flexible, region-specific changes over time, capturing shocks that affect individuals from the same birth region differently across census years.

After adding these controls, the estimates for any college attendance and interstate migration become small and statistically insignificant, suggesting that the baseline effects on those outcomes reflected broader regional dynamics rather than the schooling reform, which are now absorbed by the birth region-by-year trends. In contrast, the estimate for log income remains positive and statistically significant, though smaller in magnitude (0.046). We interpret the lower point estimate of approximately 5% as the plausible return to the additional year of schooling net of potential underlying trends in regional convergence.

5.3 Veteran Status

The birth cohorts in our sample turned 18 in years overlapping with the World War II and Korean War drafts. If the additional year of schooling altered the timing of graduation and thereby affected draft eligibility, military service could be endogenous to the reform. Column (1) of Appendix Table A3 addresses this by estimating the effect of the switch to the twelve-year program on veteran status itself. The coefficient is small and negative, indicating that the policy may have reduced the likelihood of military service by 0.9 percentage points. As veterans tend to have higher earnings than the non-veteran population, this would attenuate our estimated treatment effect for the impact of the switch to the twelve-year system. Columns (2) through (5) show that controlling for veteran status, which is not strictly valid given the result in Column 1 that it may be affected by the reform, either as a single indicator or separately by war, actually has minimal impact on the estimated treatment effects for log income and occupational income score. Sample sizes decrease because war-specific status is missing for some observations. This could also partly explain the reduction in the magnitude of the point estimates in Columns (3) and (5).

5.4 Heterogeneity by Gender and Race

The educational system in the United States during this historical period was racially segregated. Within a school district there were separate schools for White and Black children, all enrollment and education statistics were tracked and reported separately by race, and there is extensive evidence of racial disparities in educational quality and funding (Margo 1990; Card et al. 2025). There were also sharp disparities in access to post-secondary educational and labor market opportunities. Although the twelve-year program in each state and district was implemented for all schools regardless of race, the school systems remained separate, and these prevailing disparities could mean that the curricular change produced different effects by race.[14] We test that hypothesis here.

The same period also exhibited significant gender differences in educational and labor market opportunities. Women were excluded from certain fields of study in school and then afterwards faced occupational segregation (Blau and Hendricks 1979). This period was also characterized by rapid changes in the degree and nature of female labor supply (Goldin 2006). We therefore test for heterogeneity in the impact of the twelve-year program by gender as well. Table 4 reports estimates separately by gender and race. Panel A shows results for men, while Panel B reports results for women. Panels C and D present estimates for White and non-White individuals, respectively.[15]

Across gender, the positive effects on log income and occupational income score are present for both men and women, although they are larger in magnitude for women. The increase in log income is approximately 11.7% for women and 4.0% for men. One possible explanation is mechanical: in our sample, for the 1950 census, mean income for working females is $1,457 compared to $2,507 for males, in which case a larger percent increase for females could still correspond to a similar, or even smaller, absolute increase in labor income. Alternatively, it has been argued that high school was more valuable for girls than for boys, because boys would have many other opportunities afterwards to acquire additional skills whereas for girls it was their only chance to build job-related skills (Carter and Prus 1982). There is also evidence that the returns to a college education in the mid-twentieth century were higher for women than for men (DiPrete and Buchmann 2006).

The larger effect on occupational income score for women indicates more of a potential shift in employment opportunities available. Notably, despite large differences in labor force participation by gender, our estimates for the extensive margin of employment are close to zero for both groups. The policy appears to have altered which jobs people take, but not whether they take a job. The negative effect on occupational education score

[14] In Louisiana, the State Department of Education Bulletin No. 557 describing the change to the twelve-year system applied to all schools, but it was distributed separately to "Negro Principals" in Circular No. 2340 not by the State Superintendent of Education but by a separate State Supervisor of Negro Education (State of Louisiana Department of Education 1945).

[15] In our sample, "non-White" is primarily individuals whose race is recorded as "Black/African American" (92%).

is driven by men, while the estimate for women is negative but not statistically significant. The negative estimates for interstate migration are present for both groups, although somewhat larger in magnitude for men.

Despite the separate institutional environments facing students of different races, our estimates show that the positive effects on log income and occupational income score are observed for both White and non-White individuals. The increase in log income is somewhat larger for non-White individuals, while the estimates for occupational income score are of similar magnitude across groups. The estimate for employment is small for both groups, though positive and statistically significant for White individuals. The negative effect on interstate migration is much larger and statistically precise for White individuals. As noted, our sample period coincides with the Great Migration, where Black individuals left Southern states, and evidence suggests that those who did leave were positively selected (Collins and Wanamaker 2014).

5.5 Margins of Adjustment

Figures 4 and 5 provide insight into the margins through which the effect of the change to the twelve-year program operated. First, we see clear evidence of the switchover in Figure 4, which plots the annual number of public high school graduates in the treated states. In states with rapid implementation, such as Louisiana, South Carolina, and North Carolina, the number of graduates declines sharply in the years when the additional grade was introduced. This pattern is consistent with the insertion of an additional year into the curriculum: cohorts that would have otherwise graduated in the dip years were required to remain in school for an additional year. The temporary reduction in graduates, therefore, provides validation of the timing and substance of the policy change.

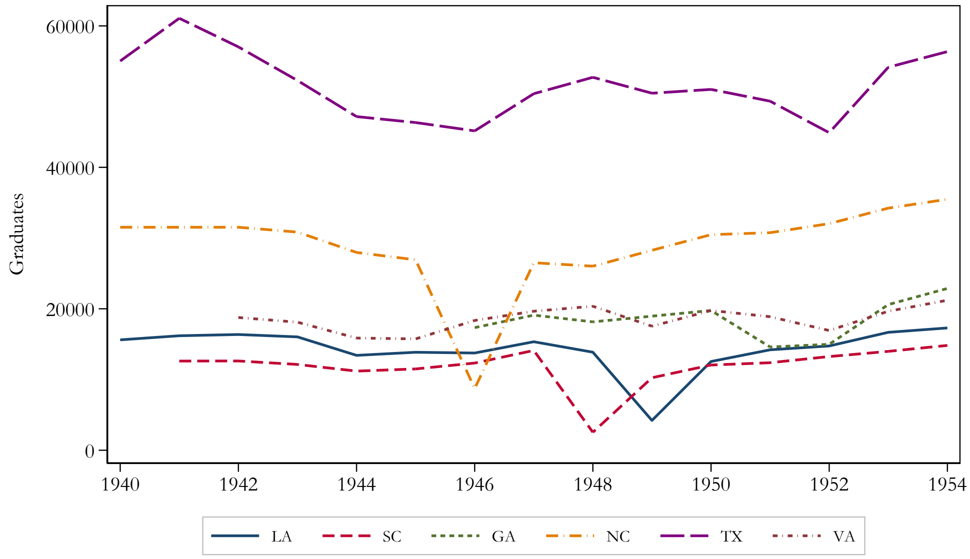
Although this might raise questions of whether the treatment effect we estimate partly reflects the impact of a temporary reduction in labor supply in these years, there are several reasons why this is unlikely. First, our sample also includes several states such as Virginia that were gradual adopters, and the estimates are the same there as with the more rapid adopter states. Second, our sample includes birth cohorts from up to a decade after the temporary labor supply shock. This also suggests that equilibrium effects are not a first-order concern here. Even when we include cohorts up to 20 years after implementation in Section 5.6.3, the point estimate varies little. Third, we observe in our life cycle analysis in Section 5.1 that the treatment effect becomes more pronounced after age 35. For the 25-29 year-old sample, we estimate a negative effect, which we attributed to a possible gap in accumulated work experience. A negative labor supply shock in the year before the treated cohorts began graduating could also help explain this, if it meant temporarily higher wages for the last untreated cohorts.

Table 4: Heterogeneity

	Employed	Log income	Occ. income score	Occ. education score	Any college	Moved
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Men						
Treatment effect	0.002	0.040***	0.338***	-0.911**	-0.024	-0.025***
	(0.004)	(0.011)	(0.089)	(0.387)	(0.015)	(0.009)
N	2,714,971	2,206,119	2,584,554	2,536,686	2,714,971	2,714,971
Panel B: Women						
Treatment effect	0.004	0.117***	0.894***	-0.222	-0.020*	-0.018**
	(0.007)	(0.018)	(0.134)	(0.402)	(0.011)	(0.009)
N	2,887,865	1,493,847	2,017,577	2,016,219	2,887,865	2,887,865
Panel C: White						
Treatment effect	0.010**	0.042***	0.273***	-0.520	-0.018	-0.026***
	(0.004)	(0.009)	(0.105)	(0.372)	(0.013)	(0.007)
N	4,972,271	3,280,817	4,100,855	4,056,437	4,972,271	4,972,271
Panel D: Non-white						
Treatment effect	-0.000	0.042*	0.515***	-0.206	-0.012	-0.010
	(0.005)	(0.023)	(0.164)	(0.363)	(0.009)	(0.019)
N	630,565	419,149	501,276	496,468	630,565	630,565

Notes: The table reports estimates of the effect of exposure to a 12-year high school requirement on the outcomes listed at the top of each column: employment, log income, occupational income score, occupational education score, an indicator for any college attendance, and interstate migration. Panels A–D report estimates separately for men, women, White individuals, and non-White individuals, respectively. The sample includes individuals born between 1920 and 1940 and observed at ages 25–59 in the 1950–1990 IPUMS census samples. The estimation sample excludes Maryland, Missouri, Utah, New Hampshire, Alaska, and Hawaii. "Switcher" states are Georgia, Louisiana, North Carolina, South Carolina, Texas, and Virginia; all other states serve as always-12 comparison states. Estimates are obtained using a two-stage difference-in-differences procedure (Gardner 2022). The first stage includes birth-cohort fixed effects, census-year fixed effects, state fixed effects, and individual covariates (sex, race). In Panels A and B, covariates include race; in Panels C and D, covariates include sex. All regressions are weighted using person weights. Standard errors, reported in parentheses, are clustered at the state level. * p<0.10, ** p<0.05, and *** p<0.01.

Figure 4: Number of high school graduates by year



Notes: Number of reported public high school graduates by state by year. Data drawn from annual/biennial state department of education reports where available.

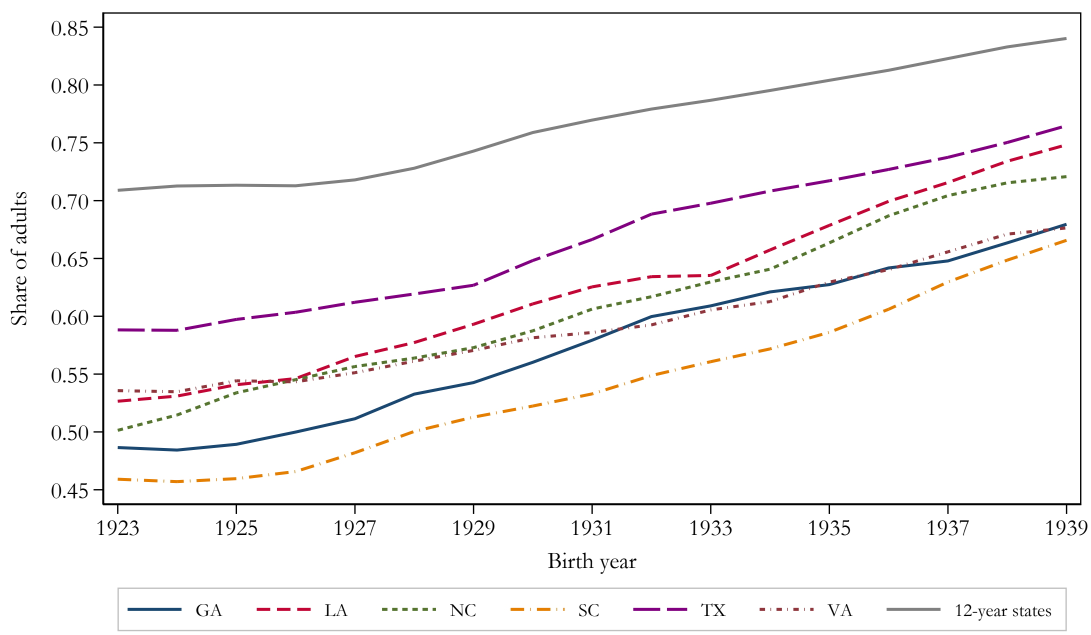
A second question is whether the change from an eleven-year to a twelve-year program altered the propensity to graduate from high school. Under a strict model where agents evaluate the marginal rate of return to education against their idiosyncratic discount rate to derive a stopping rule, those whose optimal number of years of education was 11 would become less likely to graduate from high school. An argument in the other direction is that better training in elementary school, which the insertion of an additional year before secondary school may have provided, could have prepared students better for high school, making graduation more likely. Research into skill formation asserts that earlier skill acquisition may have positive dynamic effects that cause later investments in human capital to be more effective (Cunha and Heckman 2007).

Figure 5 examines whether the reform affected high school graduation. The share of adults reporting at least a high school diploma evolves smoothly across birth cohorts, with no visible break at the cohorts exposed to the reform. This suggests that the transition to a twelve-year system did not generate a sharp change in overall high school completion. Because census measures of educational attainment do not consistently distinguish between completion under eleven- and twelve-year systems, we do not estimate this margin directly using the full sample. Instead, Figure 6 uses the 1990 census, where enumerators first asked about degree completion instead of just years of education (Jaeger 1997), to provide descriptive evidence. We also estimate our main empirical specification using this measure of high school graduation as the outcome, obtaining a point estimate of -0.0096 that is statistically indistinguishable from zero.

While these cohort profiles do not rule out more subtle changes in the composition of graduates, they do not indicate a discontinuous change in completion at the reform threshold, and there was no obvious relative upward trajectory in high school graduation rates for the six treated states relative to the set of control states that always had a twelve-year system.

These patterns suggest that the reform primarily altered the amount of schooling required to obtain a high school diploma, rather than the probability of obtaining the diploma itself. A narrative more consistent with the data than a year-by-year evaluation of the returns to schooling is a degree-by-degree evaluation, whereby students target a specific diploma to obtain, such as middle school, high school, or college. Because estimated employment does not change meaningfully following the reform, the impact of the twelve-year system is unlikely to have been large enough to alter major life choices such as school completion on the margin. The positive effects on log income reflect higher earnings among those employed, rather than changes in selection into the labor force. Overall, these results are most consistent with an increase in the human capital embodied in the high school diploma, rather than changes in educational attainment or labor market participation.

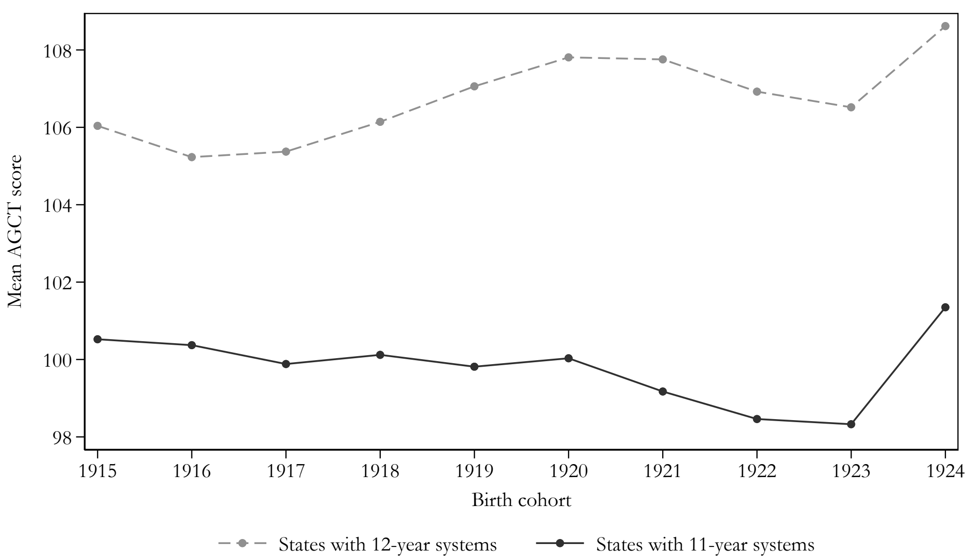
Figure 5: Share of adults with a high school diploma, by birth cohort



Notes: Fraction of adults born in each state reporting having a high school diploma, by birth year (3-year rolling average). Data drawn from the 1990 decennial census. Running our estimator on this subsample, with an indicator for graduating from high school as the outcome, yields a point estimate on treatment of -0.0096 ($SE = 0.0141$).

The interpretation of the twelve-year system as increasing the human capital content of a high school education assumes that the human capital content of eleven-year graduates was lower initially, and that it was otherwise on a similar trajectory as in states with a twelve-year system. We present suggestive evidence in Figure 6. Using World War II enlistment records with Army General Classification Test (AGCT) scores, we compare high school graduate enlistees from states with eleven-year systems to those from states with twelve-year systems in cohorts prior to the reform.[16] AGCT scores are consistently lower for graduates from eleven-year systems, with differences of about 5 to 6 points. A regression of AGCT scores on an indicator for eleven-year systems, controlling for race and birth year fixed effects, yields an estimated difference of -5.38 points ($p = 0.000$), or approximately 0.25 standard deviations. Because these data are only available for cohorts that completed high school before any state in our sample transitioned to a twelve-year system, we cannot directly examine how human capital changed following the reform. Instead, we interpret these differences as suggestive evidence that baseline levels of human capital were lower under the eleven-year system. Moreover, our estimated baseline gap of about 0.25 standard deviations directly aligns with the estimated 0.3 standard deviation *increase* in AFQT scores found for cohorts treated with an additional year of education (Cascio and Lewis 2006).

Figure 6: Mean AGCT scores among Army recruits with at least a high school education.



Notes: Means are rolling three-year averages comparing AGCT scores by birth cohort for states that still had an 11-year system but would soon transition against those that already had a 12-year system in place. Scores are available for enlistees from March 1943 onwards (Goldstein et al. 2023).

[16]The World War II enlistment records of Army recruits have been cleaned and compiled by Goldstein et al. (2023), and starting in May of 1943 include measured AGCT scores. The Army General Classification Test (AGCT) was designed to measure general mental acuity and has been used as a proxy for intelligence (Ferrie et al. 2012).

5.6 Robustness to Specification

5.6.1 Leave-One-Out Analysis

To assess whether the results are driven by any single switching state, we re-estimate the baseline specification repeatedly, each time omitting one switcher state from the sample. This exercise ensures that the estimated effects are not disproportionately influenced by the timing or magnitude of the reform in a particular state.

Appendix Figure A3 reports the resulting estimates across outcomes. For each panel, the red marker labeled "All" corresponds to the baseline estimate using the full set of switcher states, while the remaining markers report estimates obtained when excluding one switcher state at a time. Across outcomes, the estimates remain broadly stable in magnitude and statistical precision. In particular, the positive effects on log income and occupational income score persist across all specifications. The estimates for employment, occupational education score, any college attendance, and interstate migration also show limited sensitivity to the exclusion of individual states, although some variation in precision is observed.

Overall, these results indicate that the main findings are not driven by any single switching state and reflect patterns common across the set of treated states.

5.6.2 Randomization Inference

While wild cluster bootstrap p-values (reported in Table 2) yield similar conclusions to conventional inference, we further assess the statistical significance of the estimates using a randomization inference procedure tailored to the structure of the reform. This approach is particularly useful in our setting given the limited number of switching states, which raises concerns about conventional inference based on asymptotic approximations.

To implement this exercise, we generate placebo assignments of the reform by reassigning the full treatment path—defined as the state-by-birth-cohort exposure profile observed in the switching states—to sets of non-switcher states. In each iteration, we randomly select a group of control states and assign them the treatment paths of the switchers, preserving both the timing and intensity of exposure. We then re-estimate the baseline two-stage difference-in-differences specification using these placebo assignments. Repeating this procedure 100 times yields a distribution of placebo estimates for each outcome.

Appendix Figure A4 reports these distributions. In each panel, the histogram shows the distribution of placebo estimates, while the red dashed line denotes the estimate obtained under the true treatment assignment. For log income and occupational income score, the actual estimates lie in the upper tail of the placebo distributions, indicating that the observed effects are unlikely to arise by chance under random assignment of the reform. In contrast, the estimates for employment, occupational education score, any college attendance, and interstate migration fall well within the range of placebo estimates, suggesting that these effects are not statistically distinguishable from zero under this procedure.

Overall, the randomization inference results reinforce the evidence of positive effects on earnings and occupational income, while indicating weaker and less robust effects for the remaining outcomes.

5.6.3 Width of Analysis Window

To evaluate the sensitivity of our results to the range of birth cohorts analyzed, we next re-estimate the model across several alternatives. Figure A1 in the Appendix plots the estimated coefficients and a 95% confidence interval for cohort widths ranging from 2 years (1928 to 1932) up to 20 years around the 1930 cohort (1910 to 1950). As treatment intensity in our sample first increases in 1925 (North Carolina) and last increases in 1940 (Virginia), the narrower ranges discard information, and the point estimates decrease in magnitude. Yet the point estimates all lie within the same confidence band for windows as narrow as 1922 to 1938 and as wide as 1910 to 1940, with estimates ranging from 0.06 at the end point to a peak of about 0.09 for the 1916 to 1944 range of birth cohorts.

5.6.4 Stricter Definition of Treatment Status

Because the estimation approach required for two-stage difference-in-differences requires observations of each treated unit when its treatment intensity is zero, our preferred main specification classifies states with small positive intensities of twelve-year programs in the early years as still fully untreated. This choice lets us include four additional states in the analysis and aligns with the goal of estimating the impact of the statewide policy implementation of the twelve-year program, and should attenuate our estimates since we are classifying several partially treated cohorts as untreated in the model.

The strict implementation of our estimator requires dropping Georgia, North Carolina, Texas, and Virginia—leaving Louisiana and South Carolina as the two states with true 0% implementation rates for the 1920 birth cohorts. We present the results from estimating this restricted version of the model in Appendix Table A4. Point estimates for labor income and occupational income score are very close to those found using our preferred specification. The coefficient on employment as an outcome reduces from 0.004 to -0.010 and appears to be precisely estimated, and there is a similar reduction in the estimated magnitude on completion of any college; however, with only two treated units the precision of these clustered standard errors is likely overstated (Cameron et al. 2008), and we cannot establish that the confidence intervals are not overlapping with those from the main specification in Table 2.

5.6.5 Linearity of Treatment Effect

In our main specification, the continuous treatment variable enters linearly, so that the reported coefficient represents the effect of moving from no exposure to the twelve-year system to full exposure within a state. To assess whether this functional form obscures nonlinearity in the dose-response relationship, we bin the continuous treatment variable into terciles of adoption intensity and include each bin as a separate indicator in the second stage. Appendix Table A5 reports the results. For log income, the point estimates increase roughly proportionally across bins, from 0.021 for low intensity to 0.042 for medium and 0.072 for high, supporting the linear specification as a reasonable approximation. For other outcomes, particularly occupational income score, college attendance, and interstate migration, the estimates across bins are less stable. Because most states transitioned rapidly from low to high adoption, relatively few state-cohort cells fall in the medium-intensity range, limiting what we can conclude from these intermediate estimates. We take the proportional pattern in earnings as the primary evidence that the linear specification adequately captures the underlying dose-response relationship.

6 Conclusion

Exploiting the staggered timing of adoption of the now-standard twelve-year system of elementary and secondary education among the last six states to make the change, we provide a direct estimate of the human capital content of a year of schooling in a setting where the obtained credentials do not change but the underlying amount of education does. We find that this year of schooling increases earnings by about 5–8%, which is directly in line with the estimates from Angrist and Krueger (1991) studying a similar range of birth cohorts. This pattern is difficult to reconcile with a pure signaling model, under which identical credentials would produce identical outcomes regardless of the underlying education. The estimated effect of the additional year is near zero or negative right after graduation and grows over the life cycle, suggesting that employers gradually observe the higher productivity of workers whose education included an additional year of schooling. Because treated and untreated workers hold the same credential, this additional human capital is likely revealed over time through on-the-job performance, eventually overcoming the lower level of job experience for twelve-year students whose labor market entry is delayed by one year.

The ability to examine longer-term outcomes across the life cycle is a distinct advantage of our setting relative to a broader literature on instructional time. We observe not only an increase in wages but also a shift in occupations towards those with higher median earnings, providing evidence that the returns to the additional year of schooling operate in part through occupational upgrading. This is consistent with the additional year providing skills that enabled workers to access higher-quality jobs. Whereas this policy change expanded the length of time spent in formal education by one year, equivalent to about 1,000 hours of instructional time, other reforms studied in the literature added time to the school day (Figlio et al. 2018; Padilla-Romo 2022), added a day to the school week (Thompson 2021), or added days to one school year (Parinduri 2014). Because the human capital impact of these shifts in education is typically measured in near-term gains in test scores rather than on long-run labor market outcomes, a direct comparison of returns per unit of instructional time is not possible. A future comparison might focus on when diminishing returns to instructional time set in, as the marginal value of additional hours at the end of a long day may be distinct from that of an additional day in the week or one more year at an older age.

The policy change we study here extended the time that treated students spent in school from approximately age 17 to age 18. Current policy debates center on expanding publicly funded preschool, which adds structured educational time at the opposite end of the educational trajectory. While we cannot speak directly to those interventions, in light of the growing evidence on "critical periods" in investment in human capital in early child development (Heckman 2006; Cunha and Heckman 2007), additional structured educational time at younger ages might yield larger long-run returns than we estimate. The evidence on the human capital impact of these interventions is, however, mixed, ranging from large increases (Bailey et al. 2021) to effects that fade out (Currie and Thomas 2000). One key difference may be the dimension of human capital affected, with preschool seen as having a greater impact on so-called "noncognitive" skills that lead to greater patience or reduced juvenile crime (Gray-Lobe et al. 2023; Heckman and Kautz 2012). The addition of time into the elementary and secondary curriculum, on the other hand, created room for more elective courses or further credits in math—investments that plausibly more directly target the cognitive skills with a clear labor market return. Taken together, our results suggest that expansions of structured schooling time at the secondary level can generate durable earnings returns of a magnitude consistent with canonical estimates of the return to education, and that these gains operate through both higher wages and access to better-paying occupations rather than through credential effects alone.

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Appendix A - Tables

Table A1: Summary Statistics

	Always-12 states (1)	Switcher states, earlier cohorts (2)	Switcher states, later cohorts (3)	(2)-(1) (4)	(3)-(1) (5)
Female	0.512	0.527	0.522	0.015***	0.010***
	(0.000)	(0.001)	(0.001)		
Nonwhite	0.076	0.298	0.262	0.222***	0.186***
	(0.000)	(0.001)	(0.001)		
Age	41.459	41.350	41.378	-0.109	-0.081
	(0.006)	(0.023)	(0.013)		
Veteran	0.266	0.232	0.217	-0.034***	-0.048***
	(0.000)	(0.001)	(0.001)		
Urban	0.730	0.685	0.661	-0.045	-0.069
	(0.000)	(0.002)	(0.001)		
Living on a farm	0.051	0.073	0.042	0.022**	-0.009
	(0.000)	(0.001)	(0.000)		
Homeowner	0.757	0.701	0.708	-0.056***	-0.049***
	(0.000)	(0.001)	(0.001)		
Family size	3.659	3.763	3.735	0.104**	0.077**
	(0.001)	(0.004)	(0.003)		
Birth year	1929	1924	1933	-5***	4***
	(0.004)	(0.006)	(0.007)		
Census year	1971	1966	1975	-6***	4***
	(0.007)	(0.026)	(0.015)		
N	4,551,101	372,020	679,801		
Mean exposure			0.814		

Notes: The table reports weighted summary statistics for individuals born between 1920 and 1940 and observed at ages 25–59 in the 1940–1990 IPUMS census samples. The estimation sample excludes Maryland, Missouri, Utah, New Hampshire, Alaska, and Hawaii. States are grouped as follows: (1) "Always-12 controls" are states that required 12 years of schooling throughout the period; (2) "Switcher pre-change" are individuals in states that later adopted the 12-year requirement but belong to cohorts not exposed to the change; and (3) "Switcher post-change" are exposed cohorts in those same switcher states. All means are computed using person weights. Standard deviations are reported in parentheses beneath the means. Columns (4) and (5) report differences relative to column (1), estimated from regressions of each characteristic on group indicators, * p<0.10, ** p<0.05, and *** p<0.01. Standard errors are clustered at the state level. The row "Mean exposure (switcher post)" reports the weighted average exposure among individuals in switcher states who were affected by the change.

Table A2: Robustness to Birth-Region Linear Trends

	Log income (1)	Any college (2)	Moved (3)
Treatment effect	0.046***	-0.007	-0.002
	(0.014)	(0.011)	(0.008)
Birth-region linear trends	✓	✓	✓
N	3,699,966	5,602,836	5,602,836

Notes: The table reports estimates of the effect of exposure to a 12-year high school requirement on the outcomes listed at the top of each column: an indicator for interstate migration and an indicator for any college attendance. The sample includes individuals born between 1920 and 1940 and observed at ages 25–59 in the 1950–1990 IPUMS census samples. The estimation sample excludes Maryland, Missouri, Utah, New Hampshire, Alaska, and Hawaii. "Switcher" states are Georgia, Louisiana, North Carolina, South Carolina, Texas, and Virginia; all other states serve as always-12 comparison states. Estimates are obtained using a two-stage difference-in-differences procedure (Gardner 2022). The first stage includes birth-cohort fixed effects, census-year fixed effects, state fixed effects, individual covariates (sex and race), and birth-region linear time trends. All regressions are weighted using person weights. Standard errors, reported in parentheses, are clustered at the state level. * p<0.10, ** p<0.05, and *** p<0.01.

Table A3: Treatment Effects and Veteran Status

	Veteran	Log income		Occ. income score	
	(1)	(2)	(3)	(4)	(5)
Treatment effect	-0.009**	0.075***	0.070***	0.618***	0.405***
	(0.004)	(0.013)	(0.012)	(0.098)	(0.055)
Veteran status control		X		X	
War-specific veteran controls			X		X
N	5,602,836	3,699,966	3,386,061	4,602,131	4,156,375

Notes: The table reports estimates of the effect of exposure to a 12-year high school requirement on veteran status (Column 1), and then adds controls for veteran status in Columns (2) to (5) with log income or occupational income score as the outcome. Estimates are obtained using a two-stage difference-in-differences procedure (Gardner 2022). The first stage includes birth-cohort fixed effects, census-year fixed effects, state fixed effects, and individual covariates (sex, race). All regressions are weighted using person weights. Standard errors, reported in parentheses, are clustered at the state level. * p<0.10, ** p<0.05, and *** p<0.01.

Table A4: Treatment Effects for Louisiana and South Carolina Only

	Employed (1)	Log income (2)	Occ. income score (3)	Occ. education score (4)	Any college (5)	Moved (6)
Treatment effect	-0.010***	0.080***	0.696***	-0.934	-0.038**	-0.019***
	(0.002)	(0.015)	(0.103)	(0.688)	(0.018)	(0.007)
N	4,778,494	3,155,393	3,928,139	3,887,679	4,778,494	4,778,494

Notes: The table reports estimates of the effect of exposure to a 12-year high school requirement on the outcomes listed at the top of each column: employment, log income, occupational income score, occupational education score, an indicator for any college attendance, and interstate migration. The sample includes individuals born between 1920 and 1940 and observed at ages 25–59 in the 1950–1990 IPUMS census samples. The treated units in the estimation sample include only Louisiana and South Carolina. Estimates are obtained using a two-stage difference-in-differences procedure (Gardner 2022). The first stage includes birth-cohort fixed effects, census-year fixed effects, state fixed effects, and individual covariates (sex, race). All regressions are weighted using person weights. Standard errors, reported in parentheses, are clustered at the state level. * p<0.10, ** p<0.05, and *** p<0.01.

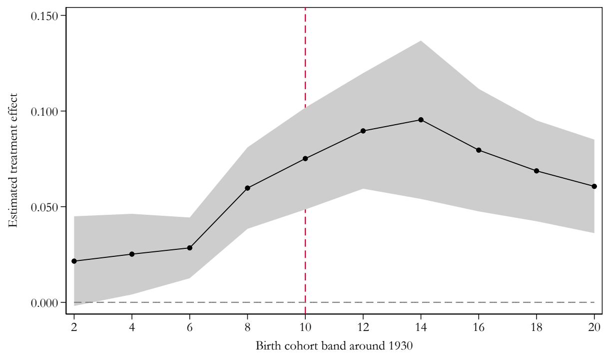
Table A5: Treatment Effects Allowing for Non-linear Dosage Response

	Employed	Log income	Occ. income score	Occ. education score	Any college	Moved
	(1)	(2)	(3)	(4)	(5)	(6)
Low intensity $(0, \frac{1}{3}]$	0.002	0.021***	0.181	-0.242	-0.008	-0.001
	(0.004)	(0.006)	(0.138)	(0.235)	(0.005)	(0.007)
Medium intensity $(\frac{1}{3}, \frac{2}{3}]$	0.000	0.042***	0.014	-1.625***	-0.049***	-0.029***
	(0.001)	(0.008)	(0.056)	(0.261)	(0.007)	(0.006)
High intensity $(\frac{2}{3}, 1]$	0.004	0.072***	0.579***	-0.501	-0.020*	-0.020***
	(0.004)	(0.013)	(0.100)	(0.335)	(0.012)	(0.008)
N	5,602,836	3,699,966	4,602,131	4,552,905	5,602,836	5,602,836

Notes: The table reports estimates of the effect of exposure to a 12-year high school requirement on the outcomes listed at the top of each column: employment, log income, occupational income score, occupational education score, an indicator for any college attendance, and interstate migration. Estimates are obtained using a two-stage difference-in-differences procedure (Gardner 2022), where treatment with the twelve-year program is measured in intensity categories instead of as a continuous variable. The first stage includes birth-cohort fixed effects, census-year fixed effects, state fixed effects, and individual covariates (sex, race). All regressions are weighted using person weights. Standard errors, reported in parentheses, are clustered at the state level. * p<0.10, ** p<0.05, and *** p<0.01.

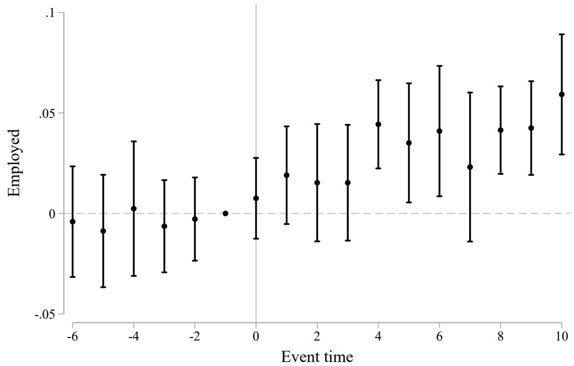
Appendix A - Figures

Figure A1: Estimated treatment effect for varying birth cohort windows around 1930

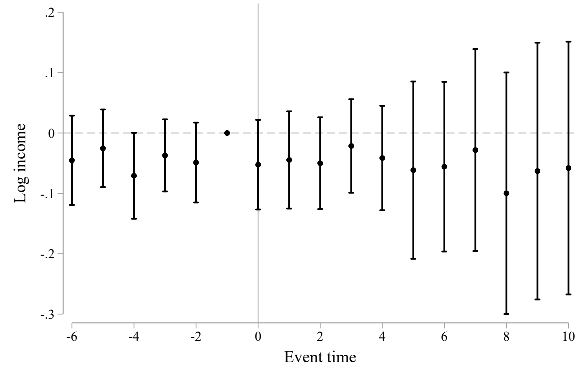


Notes: The x-axis reports the width of the estimation window in each direction: 2 indicates a birth cohort range of 1928-1932. Our primary specification, indicated by the vertical dashed line, is a 10-year window.

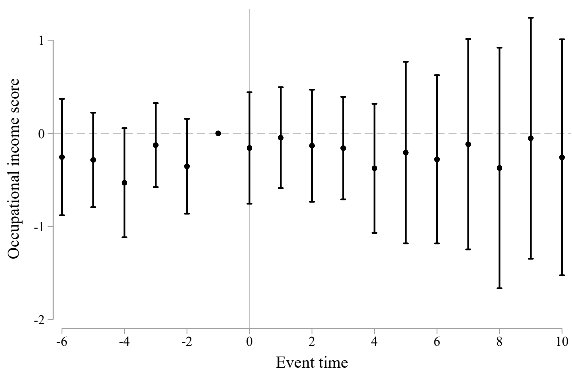
Figure A2: Event-time estimates



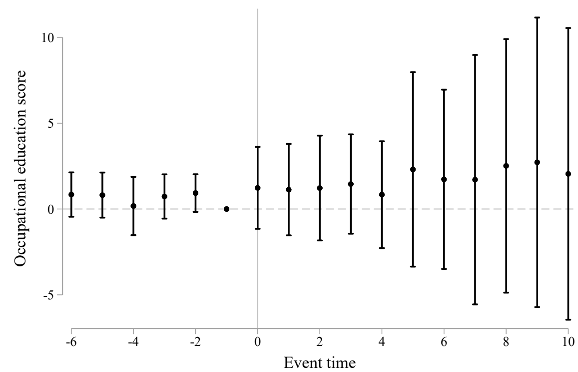
(a) Employed



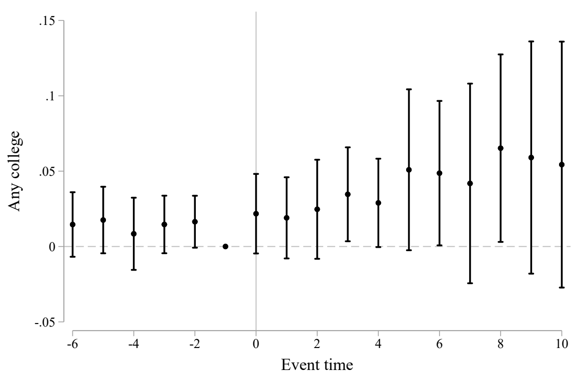
(b) Log income



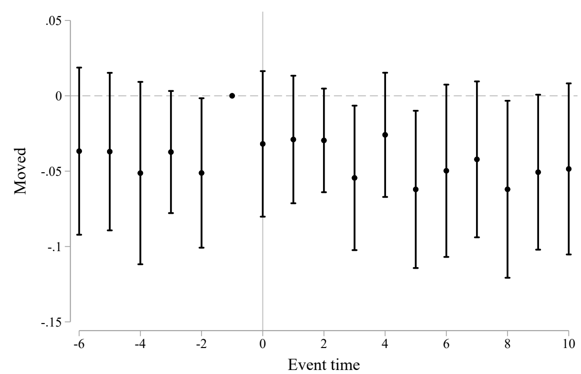
(c) Occupational income score



(d) Occupational education score



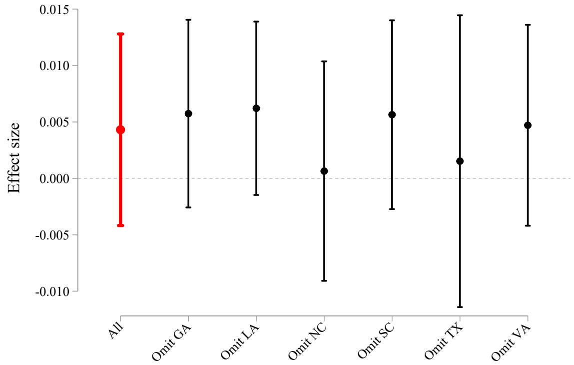
(e) Any college



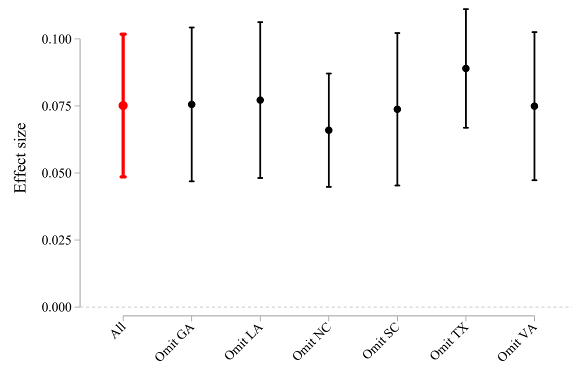
(f) Moved

Notes: The figure reports event-time estimates examining the effect of exposure to a 12-year high school requirement on the outcomes shown in each panel. The sample includes individuals born between 1920 and 1940 and observed at ages 25–59 in the 1950–1990 IPUMS census samples. The estimation sample excludes Maryland, Missouri, Utah, New Hampshire, Alaska, and Hawaii. Treated states are Georgia, Louisiana, North Carolina, South Carolina, Texas, and Virginia; all other states serve as always-12 comparison states. Estimates are obtained using a modified two-stage difference-in-differences procedure following Section 2.6 of Gardner 2022. The first stage is estimated using all observations from never-treated states and only the last pre-treatment cohort from eventually treated states. The first stage includes birth-cohort fixed effects, census-year fixed effects, state fixed effects, and individual covariates (sex, and race). All regressions are weighted using person weights. The omitted category is event time −1. The plotted window includes event times −6 to 10; the regression includes binned tails outside this window (not shown). Vertical bars denote 95% confidence intervals based on a cluster bootstrap with 100 replications, resampling at the state level, and re-estimating both stages in each replication.

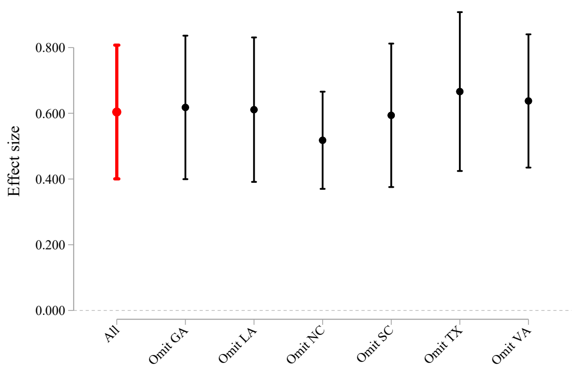
Figure A3: Leave-one-switcher-out Estimates



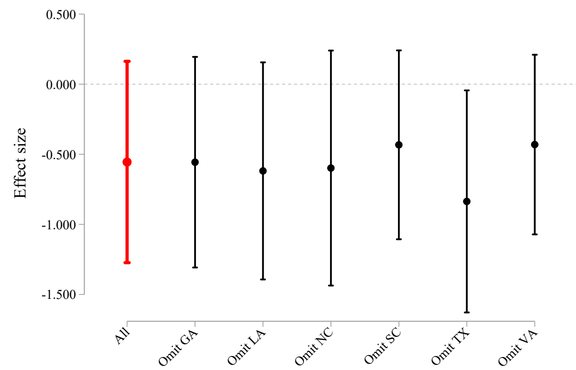
(a) Employed



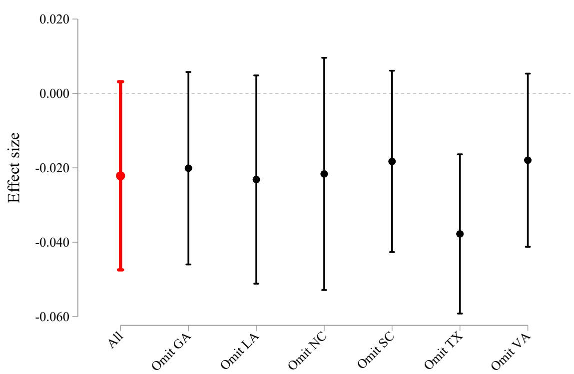
(b) Log income



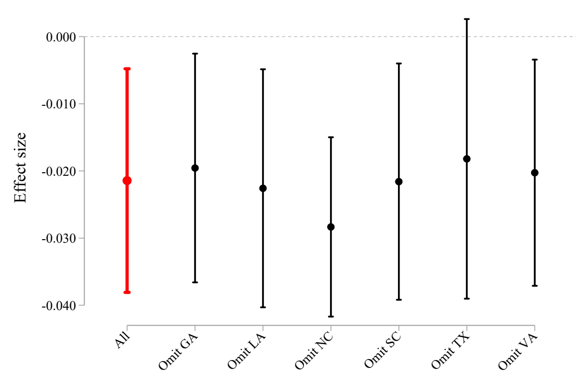
(c) Occupational income score



(d) Occupational education score



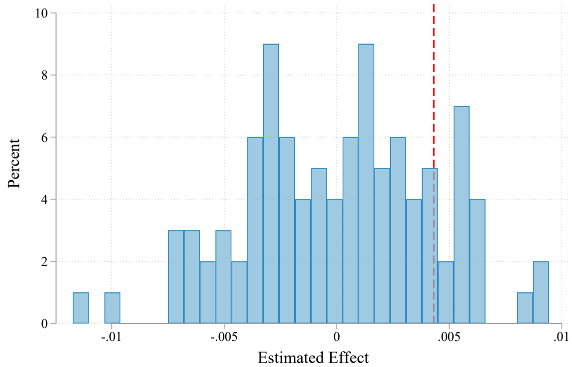
(e) Any college



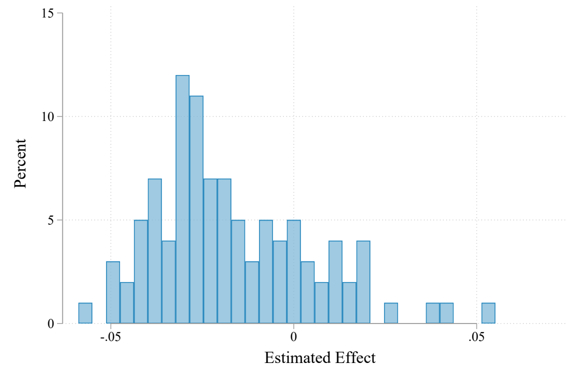
(f) Moved

Notes: The figure reports estimates of the effect of exposure to a 12-year high school requirement on the outcomes shown in each panel: employment, log income, occupational income score, occupational education score, an indicator for any college attendance, and interstate migration. The sample includes individuals born between 1920 and 1940 and observed at ages 25–59 in the 1950–1990 IPUMS census samples. The estimation sample excludes Maryland, Missouri, Utah, New Hampshire, Alaska, and Hawaii. Treated states are Georgia, Louisiana, North Carolina, South Carolina, Texas, and Virginia; all other states serve as always-12 comparison states. In each panel, the red marker labeled "All" reports the estimate obtained using the full set of switcher states. The remaining markers report estimates obtained by re-estimating the same specification while omitting one switcher state at a time. Estimates are obtained using a two-stage difference-in-differences procedure (Gardner 2022). The first stage includes birth-cohort fixed effects, census-year fixed effects, state fixed effects, and individual covariates (sex, race). All regressions are weighted using person weights. Vertical bars denote 95% confidence intervals with standard errors clustered at the state level.

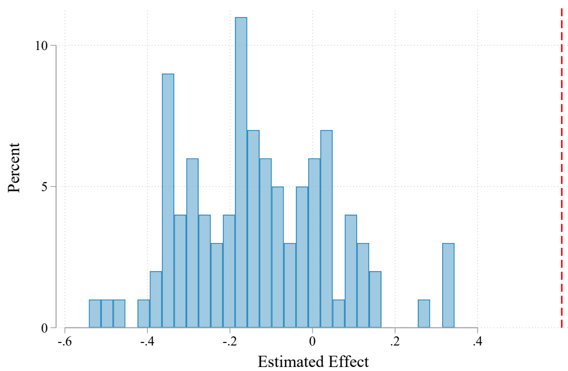
Figure A4: Randomized Inference Distributions



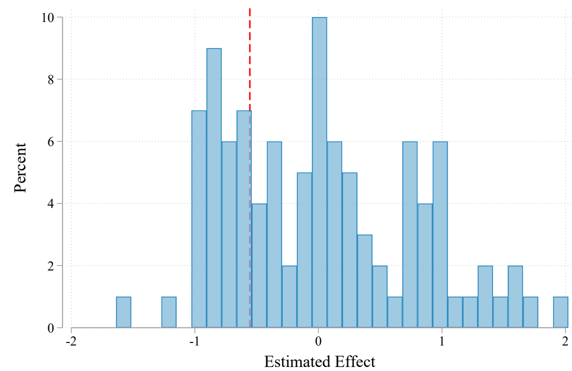
(a) Employed



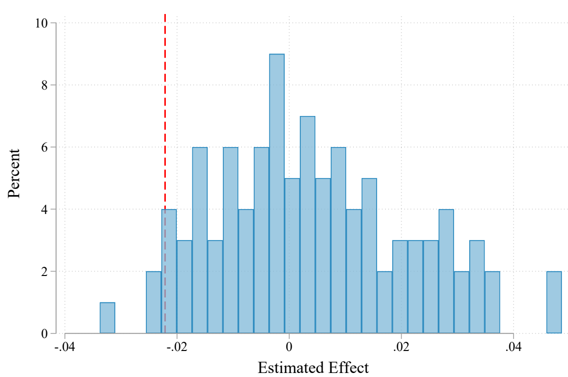
(b) Log income



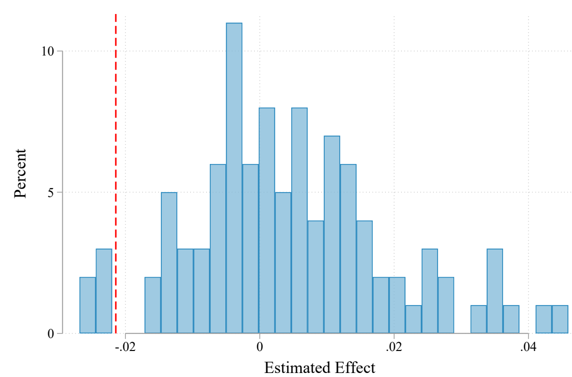
(c) Occupational income score



(d) Occupational education score



(e) Any college



(f) Moved

Notes: The figure reports randomization inference distributions for the estimated effect of exposure to a 12-year high school requirement on the outcomes shown in each panel. The sample includes individuals born between 1920 and 1940 and observed at ages 25–59 in the 1950–1990 IPUMS census samples. The estimation sample excludes Maryland, Missouri, Utah, New Hampshire, Alaska, and Hawaii. Treated states are Georgia, Louisiana, North Carolina, South Carolina, Texas, and Virginia; all other states serve as always-12 comparison states. Each histogram displays the distribution of estimated treatment effects obtained from 100 placebo assignments of the reform. In each iteration, the full treatment path (state-by-cohort exposure profile) of the switching states is reassigned to a set of control states, and the same estimation procedure is applied. The red dashed line in each panel denotes the estimate obtained using the true treatment assignment in the data. Estimates are obtained using two-stage difference-in-differences (Gardner 2022). The first stage includes birth-cohort fixed effects, census-year fixed effects, state fixed effects, and individual covariates (sex, race). All regressions are weighted using person weights and standard errors are clustered at the state level.

B Appendix: Reform Implementation Details by State

The most common method of incorporating an additional year in the curriculum was to place it after the seventh grade and before the first year of high school. An example of such a transition plan proposed for Louisiana appears in Figure B1. We list our full set of calculated exposure intensities by birth cohort for each of the six treated states in Table B1, which is based on enrollment or graduation data reported in the individual archival annual reports for each state. Further details regarding the motivation, implementation, and timeline for each of the six states studied in the main text follows.

Figure B1: Proposed Five-Year Implementation Plan for the Twelve-Grade System in Louisiana, 1943–1949.

The following diagram shows that the installation of an eighth grade in the elementary schools beginning in 1943 (to give a year to make adjustments in the curriculum) would involve no extra cost until 1947.

Grades	1	2	3	4	5	6	7	8	I	II	III	IV
1942-3	X	X	X	X	X	X	X	0	X	X	X	X
1943-4	X	X	X	X	X	X	X	X	0	X	X	X
1944-5	X	X	X	X	X	X	X	X	X	0	X	X
1945-6	X	X	X	X	X	X	X	X	X	X	0	X
1946-7	X	X	X	X	X	X	X	X	X	X	X	0
1947-8	X	X	X	X	X	X	X	X	X	X	X	X

As will be seen from the above diagram, the total number of children to be taught each year remains the same until 1947. In 1943 there are eight years of elementary school. But there is no first year class in high school, since the previous year's seventh graders are now in eighth grade elementary. Enough high school teachers are thereby freed to teach the eighth grade elementary school children.

Notes: Plan showing the phased addition of an eighth grade beginning in 1944. The actual eventual plan of adoption was analogous, only starting one year later. Source: Louisiana Educational Survey Commission (1942).

B.1 Georgia

The State Superintendent of Schools recommended that Georgia should have twelve public schools instead of eleven in a letter introducing the biennial report dated January 1, 1945 (State Department of Education, State of Georgia (1944) p. 12). At that point, only the Atlanta, Griffin, and Thomasville city school systems and those in Bibb, Chatham, Glynn, and Polk counties had twelve grades, representing 14.9% of total state enrollment. In the 1946 report, the recommendation was upgraded to requesting the state legislature to enact laws providing for a twelfth grade (State Department of Education, State of Georgia (1946) p. 49). The General Assembly passed such legislation in 1947, allowing schools at least four years to make the transition. About 60% of school systems enrolled their first twelfth grade class in the fall of 1950 and most the remaining 40% did so in the fall of 1951, except for six school systems that need another year or two

Table B1: Share of each birth cohort exposed to a twelve-year program for each treated state (%)

	GA	**LA**	**NC**	**SC**	**TX**	**VA**
1920	18	0	5	0	0	14
1921	18	0	7	0	0	14
1922	18	0	8	0	2	14
1923	18	0	9	0	2	14
1924	18	0	15	0	5	15
1925	18	0	27	0	8	16
1926	18	0	28	0	20	17
1927	18	0	30	0	71	21
1928	18	0	86	1	82	23
1929	18	0	98	2	85	31
1930	18	0	99	31	90	32
1931	18	20	100	96	100	52
1932	18	100	100	99	100	57
1933	18	100	100	100	100	64
1934	78	100	100	100	100	71
1935	100	100	100	100	100	75
1936	100	100	100	100	100	81
1937	100	100	100	100	100	85
1938	100	100	100	100	100	89
1939	100	100	100	100	100	92
1940	100	100	100	100	100	93

Authors' calculations based on data reported in each state's historical annual education reports.

(State Department of Education, State of Georgia (1950) p. 9, State Department of Education, State of Georgia (1952) p. 26). It was left up to individual districts how to implement the change, though guidance was to start with a year between seventh grade and the first year of high school rather than tacking on a year at the end of the current high school curriculum (Green 1947). In a school beginning the transition in 1947 using the recommended approach, the first class among the treated districts to graduate high school having completed twelve grades would be in 1951.

B.2 Louisiana

The Louisiana State Legislature set up an Educational Survey Commission in 1940 to study "the conditions and needs of the public schools of Louisiana" (Louisiana Educational Survey Commission 1942). One of the commission's central conclusions was that the public education system needed an additional year: "All but six of the forty-eight states in the Union have found it necessary, in order to give an adequate elementary and secondary education, to provide twelve years of schooling. Louisiana is one of the six." This recommendation was supported by a survey on whether to add an eighth grade to elementary school, followed by four years of high school. Eighty-nine percent of citizens, 81% of teachers, 76% of principals, 81% of school boards. . . and 0% of school superintendents agreed (Louisiana Educational Survey Commission 1942). As in other states, proponents emphasized that seventeen year-old high school graduates were less mature (and in some cases not even old enough to work full-time), that more electives could be offered over a twelve-year curriculum, that graduates of twelve-year systems had higher test scores, that only a minority of students went to college, that students who did go to college were less well-prepared, and that Louisiana was behind most other states that had twelve-year systems (Morning Advocate 1940). Gubernatorial candidate Jimmie Davis said, "An added year of education would give the boy or girl that additional advantage which would be useful to him in his future life. Most other states give their children twelve years of schooling and so our children have that competitive disadvantage" (The Times-Picayune 1944c). Some students surveyed at the time expressed disappointment that they would have to wait an extra year to enter the world or continue on to post-secondary education (The Times-Picayune 1944a).

The Louisiana State Board of Education resolved to implement a 12-year school program by the start of the 1944-1945 school year (State of Louisiana Department of Education 1944). Parochial schools, at least within the Archdiocese of New Orleans, appear to have adopted the program as well (Times-Picayune 1944). A Committee on the Twelve Year Program formed to determine how to implement the transition proposed a phased transition over five years, which was a common approach for school systems that transitioned. Under this plan, children completing seventh grade in spring 1944 advanced directly to high school (ninth grade) that fall. In contrast, students finishing seventh grade in 1945 entered the newly created eighth grade. Orleans Parish and Caddo Parish started one year earlier, implementing the program in the 1944-1945 school year (Times-Picayune 1949). Although several news articles from the time suggested that Louisiana, or at least New Orleans, did at one point have a twelve-year system that it eliminated in the 1920s (Morning Advocate 1940; The Times-Picayune 1944b; New Orleans States 1944), we were unable to corroborate this in a search of historical news articles from the 1920s or the archived annual reports of the department of education. Implementation was evident in the fall of 1949, when Louisiana State University reported enrolling 800 fewer freshmen than in the previous year (Morning Advocate 1949).

The commission on the twelve-year program estimated in 1942 that implementation would increase annual costs by $1 million (Morning Advocate 1940), although a benefit of gradual implementation was that no additional teachers would be needed for the first five years of the transition (Morning Advocate 1942). One immediate cost was the need for new textbooks, estimated at $54,000 for New Orleans (New Orleans States 1944).

B.3 North Carolina

The North Carolina General Assembly passed a law replacing the eleven-year system with a twelve-year system on March 13, 1941, and a committee organized to study its implementation recommended that the additional year should be inserted between elementary school and high school (The Enterprise 1942). The timing of when the first twelve-year graduates was spread out over more years than in Louisiana, South Carolina, or Georgia, though many began around 1947. There were only three high school graduates from the Angier School in 1946 owing to the gap of one class with each year from the insertion of an extra grade before high school, with the first full twelve-year cohort graduating in 1947 (Harnett County News 1946). Likewise, in Waynesville, there was no commencement in 1946 because of the transition, though a small number of students who had transferred in from twelve-year systems still graduated (The Waynesville Mountaineer 1945). The president of Pembroke college reported in June 1949 that they had lower enrollment in their current sophomore and junior classes owing to the transition to the twelve-year system (Wellons 1949). In Murphy, students already enrolled in high school in 1941 when that system's transition began in 1941 were offered the option to stay in school for an extra year or graduate after eleven, with the first required twelve grade graduates finishing high school in 1945 (Bueck 1941).

As in several other states, athletics became a primary issue for opposition. In 1945, students enrolled in high schools with a twelve-year system were deemed ineligible to compete in athletics sponsored by the Western North Carolina High School Activities Association (The Red and White 1945), and the Albermarle Athletic Conference made a similar rule in 1947 (The Roanoke Beacon 1947). If the switch was controversial at the time for other reasons, that was not picked up in the local newspapers; however, in 1983, a North Carolina legislative committee studied whether to eliminate 12th grade, citing concerns that some students were not benefiting from it and that the state was spending over $55 million annually on a year many used to "slide" through with minimal coursework (North Carolina. General Assembly. Legislative Research Commission 1983).

B.4 South Carolina

South Carolina's State Board of Education adopted the twelve-grade plan on January 20, 1944 (South Carolina State Superintendent of Education (1944) p. 81). Advocacy for adopting the twelve-grade system mixed complaints that South Carolina was behind other states with calls for students needing further education, with one columnist calling it "the only college which will be attended" by most local children (The Camden Chronicle 1944b). Implementation occurred primarily between the seventh grade and the first year of high school, such that in a district making the switch in the fall 1944 the first twelve-year graduating class was in the spring of 1949 (The Camden Chronicle 1944a). The vast majority of affected graduating classes were in the spring of 1948 and 1949. One way in which this was apparent was that the incoming freshman class at the University of South Carolina had 550 fewer students in 1948, and that was after admitting more out-of-state students than usual (The Gamecock 1948).

In order to maintain enrollment in state universities during the transition year, South Carolina permitted eleventh grade students in the top 25% of their class who wanted to go to college to graduate rather than stay in high school for the new twelfth grade (South Carolina State Superintendent of Education (1949) p. 17). Take-up of this policy would attenuate our estimated effects, as we are assuming those students graduated from a twelve-year and not an eleven-year system. South Carolina also allowed anyone finishing eleventh grade starting in the spring of 1944 to stay in high school for an optional twelfth grade if desired, which would further attenuate our results if commonplace. However, it was not; in 1946 there were 118 twelve-year graduates state-wide, compared with 12,331 eleven-year graduates (South Carolina State Superintendent of Education (1946) p. 110).

The State Superintendent of Education's report in 1944 emphasized that the additional year was not being "pyramided" on to the high school; instead, pupils were receiving additional preparatory training at the elementary level and throughout the curriculum (South Carolina State Superintendent of Education (1944) p. 71, South Carolina State Superintendent of Education (1945)). Touted benefits included putting students on par with those in other states; students graduating at age 18 and therefore being able to work immediately; and giving students more time to mature so as to better adjust to college or employment. Testing had shown that eleven-year graduates from South Carolina had lower achievement scores compared to graduates of twelve-year systems, and that even in college South Carolina graduates in their sophomore years were on par with freshmen from twelve-year systems (South Carolina State Superintendent of Education (1945) pp. 96-97).

There were calls in state government to cut appropriations for the twelfth grade almost as soon as it was implemented (The Clinton Chronicle 1948), with some claiming the twelfth grade was a duplication of the eleventh grade (The Camden Chronicle 1952), though much of the immediate concern in local areas involved whether twelfth graders would be allowed to compete in high school sports (The Skipper 1946).

B.5 Texas

The timeline, motivation, and decision-making process surrounding the transition to a twelve-year program in Texas is documented in a dissertation on that topic (Watlington 2014). Port Arthur was the first school system to switch over, making the decision to add a grade at end of elementary curriculum in 1926. Data reviewing early results from Port Arthur showed that 13.5% of graduates from the eleven-year system failed out of college compared to 11% of those graduating from the twelve-year system (Watlington 2012). By 1938, when the State Department of Education began to get involved, still only thirteen systems had made the change. Early on, it simply suggested or recommended action towards adoption. As with other states, motivation came partly from the concern that Texas students were behind those in other states. The twelve-year system was formally endorsed by the state superintendent in 1941, and by 1943 all high schools had to switch to the twelve-year system in order to remain accredited. By that point, reported adoption was at 90%, and "nearly all" had complied by 1946, which would make the last graduating class with any eleven-year graduates to be around 1950 (Watlington 2012).

As in the other states, a primary complaint among the schools maintaining an eleven-year system was that schools with a twelve-year system would have a competitive advantage in athletics (The Daily Sun 1937). Teachers in Port Arthur, nonetheless reported favorably on it for providing the chance to cover more material, and for students to be older and more experienced when graduating, making it easier to adapt to college and life in the community (The Daily Sun 1937).

B.6 Virginia

Although Georgia officials claimed that it was the last state to adopt the twelve-year system (State Department of Education, State of Georgia (1950) p. 9), it was in fact Virginia. Virginia superintendents were meeting to discuss the twelve-year program as early as 1941 (Richmond News Leader 1941), and 75% of Virginia schools reportedly had a twelve-year system by 1946 (Tidewater Review 1946), again placing the additional year before high school (Herald-Progress 1951). Yet while the 1956 annual report from the superintendent of public instruction noting that adoption was still progressing, and that four hundred and two high schools in eighty-five counties and thirty cities had either transitioned or were in the process (Commonwealth of Virginia, State Board of Education (1956) p. 34), 41 Virginia schools were still switching over as of 1958 (News Progress (1958), Commonwealth of Virginia, State Board of Education (1958) p. 33), and 20 were still on the eleven-year system in 1959 (Commonwealth of Virginia, State Board of Education (1959) p. 39). Part of the argument in favor of changing over was that their seniors were at a disadvantage in athletic competition against other schools (News Progress 1957a), though news editorials also emphasized that local children were at an educational disadvantage as well (News Progress 1957b). The primary opposition related to cost, with one editorial stating: "Getting back to the twelve-year system, there is no question in the minds of the educators and other well-informed people of the desirability of the system. The question is 'Who's going to pay for it?"' (Clinch Valley News 1957).