Curriculum Laws and Demand for Public High School: Evidence from 1920s Anti-Evolution Policies

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September 2, 2026

Abstract

Government control over the curriculum taught in public schools is an intensely debated political topic in the United States, yet little evidence exists on the effect of curriculum interventions on demand for public education. We examine the impact of one of the earliest state-level curriculum laws in the United States on citizens' willingness to enroll in and allocate resources for public schools. Specifically, we study the American South during a wave of religiously motivated anti-evolution legislation in the 1920s. Using a difference-in-differences strategy and linked US Census Data, we investigate the effects of anti-evolution curriculum. We find that passage of criminal anti-evolution laws decreases the probability of enrolling in high school conditional on completing 8th grade. We also find, however, those who do attend high school after the policy is in place are more likely to complete. We also document declines in local tax receipts, school expenditures, and average daily attendance. The results suggest state-level curriculum mandates make citizens broadly less supportive of the public education system, while illuminating the relationship between religious affiliation and public-school enrollment during the American high school movement.

1 Introduction

The provision of public education is one of the most resource-intensive functions of state and local governments. The United States spends $1 trillion annually on the provision of public K-12 education, with around 43% of expenditures provided at the local level, 47% provided by the state government, and 10% by the federal government. Public education finance largely depends on citizens' willingness to tax themselves to support the public school system, and that willingness itself depends on citizens' perceived effectiveness of public schools.

Public education in the United States has long been a battleground for cultural and political issues. Often this conflict manifests itself in the form of movements to introduce state-level control over local curricula. Far from remaining in the realm of political discourse, political division over educational policy generates substantial differences in public school curriculum across districts and states. For example, 34 states require abstinence to be a key feature of sex education curriculum (Diep, Castillo Sanchez and Salganicoff (2025)) and at least 18 states have passed laws aimed at restricting Critical Race Theory and related concepts from being discussed in public schools (Sawchuk, 2024). Similar public debate has abounded over climate science, vaccination, American history textbooks, and the biological theory of evolution.

Perhaps it is unsurprising that curriculum mandates remain controversial topics in the public discourse, as prior work in economics has established that curriculum mandates affect students' future attitudes toward the relevant issues. For example, Arold (2024) finds that reformed science standards which allowed greater teaching of evolution in the 2000s led students to express greater belief in evolution as adults and have a higher likelihood of participation in life-science careers. The adoption of a pro-government curriculum caused students to hold more favorable views of the Chinese government (Cantoni et al., 2017).

Language laws to introduce (or ban) instruction in native tongues strengthen students' cultural identity (Clots-Figueras and Masella (2013), Fouka (2020)).

While prior work has demonstrated the effectiveness of curriculum mandates on shaping student attitudes, little is known about how implementing culturally motivated curriculum policies impacts sorting into public high school and support for public education. In this study, we provide novel evidence that state-level curriculum mandates negatively affect local support for the public education system. We study this question in the historical context of anti-evolution laws passed in the American South by three states: Tennessee (1925), Mississippi (1926), and Arkansas (1928). Support for these laws was driven by religiously motivated backlash to the increasingly widespread awareness of Darwin's theory of evolution in the context of a broader debate between fundamentalists and modernists in American Protestantism.

These curriculum mandates came into effect during the American High School Movement, a period of rapidly expanding enrollment in secondary education. In Arkansas in 1940, just 15.1% of adults had completed a high school education U.S. Census Bureau (2006). Thus, the decision to enroll in high school is an important margin by which people could express their belief in the value of public education. We leverage the staggered rollout of anti-evolution laws in a border-pairs difference-in-differences framework. We compare cohorts living in counties along state borders between an anti-evolution law state and a non-adopting state before and after passage of the law. Controlling for border pair fixed effects, we find the curriculum mandate causes high school enrollment to decline by 1.9 percentage points. We also find that, conditional on high school enrollment, the probability of finishing grade 12 increases by 2.5 percentage points. We do not find an effect of anti-evolution laws on college enrollment.

Having documented a decline in high school enrollment following the im-

plementation of state-level curriculum mandates, we next examine how those mandates affect financial support for public education. We utilize county-level panel data on Southern school resources from Carruthers and Wanamaker (2019) and supplement this with additional archival data collection on teacher licenses. We find that passage of the anti-evolution laws are associated with a broad decrease in the allocation of resources to public schools. Local tax revenues, total school expenditures, enrollment across K-12, and number of teachers per capita all decline. We find inconsistent effects on average teacher salary which could be driven by changes in the composition of teachers across anti-evolution states.

One reason state-level curriculum mandates are so controversial in the present day is that they are often imposed on school districts whose constituents hold opposing views. It is quite reasonable to expect that the impacts of mandates are heterogeneous based on the underlying support in local communities. One advantage of our historical setting is that we can explore how supportive vs opposed communities respond to the anti-evolution laws. Specifically, we leverage the fact that Arkansas's 1928 law was passed by ballot initiative. We first collect the county-level share of the vote in favor of the anti-evolution law. We then use a machine learning approach to predict the vote margin on the basis of observable economic and religious characteristics. Interestingly, we find that a county's religious composition is far more predictive of its support than its economic structure. We then use this model to predict the vote share on a hypothetical referenda across all counties in the American South. This allows us to construct a control group of counties which look similar to Arkansas counties on the basis of their predicted baseline anti-evolution sentiment.

While we are the first to link state-level curriculum mandates with decreases in local school enrollments and resource allocation, our findings are consistent with a broad literature in the economics of education going back to Lankford

(1985) which shows that voters are more willing to support increased taxes for school funding when they believe their tax dollars are being spent efficiently. For example, people are less likely to vote in favor of a tax increase (Kogan, Lavertu and Peskowitz, 2016)) or contribute private donations (Figlio and Kenny, 2009) to schools when they receive signals that schools are performing poorly; citizens are more likely to vote in favor when trust in public schools is high (Brunner et al., 2024). We hypothesize that willingness to allocate resources declines more strongly in places which are less supportive of the mandate, which would align with the view that public trust is a necessary precondition for strong support for public education.

2 Data

2.1 1920 to 1940 Census Linking

Around two dozen states considered passing anti-evolution laws in the 1920s. The first was Kentucky in 1922, although the proposed legislation failed to pass by a single vote in the Kentucky House (Brake, 2024). Our initial analysis focuses on Tennessee, Mississippi, and Arkansas, the only states that ultimately passed criminal laws restricting evolution instruction in schools that receive public funding. The large number of states which debated legislation along with, at times, narrow margins by which proposed laws were defeated suggests the exact timing of implementation was driven by idiosyncratic institutional factors across states.

Although the timing of law passage is plausibly exogenous, factors such as religiosity or the relative shares of religious denominations living in a particular state may affect both likelihood of law passage and high school attainment. We therefore limit our initial analysis to counties adjacent to state borders in

Tennessee, Mississippi, and Arkansas (treated counties) and those counties along the borders of neighboring states which did not implement anti-evolution laws (control counties). Figure 1 displays the treated and control counties.

We build our initial dataset by first identifying males in the 1920 census living in a county on the border of Tennessee, Mississippi, or Arkansas. In order to construct clear comparison groups, we make two initial data restrictions. First, we drop individuals who were children in treatment counties bordering another treatment state. Second, we drop those who were children in a control county bordering more than one treatment state.

Of these individuals, we retain those who were between 10 and 22 during at the passage of the corresponding anti-evolution policy. We link this data to the 1940 census, the first year educational outcomes are reported, in order to identify the educational attainment. The final dataset is comprised of 177,582 individuals residing in border counties.

2.2 School Resource Data

We use data on school resources in the American South from Carruthers and Wanamaker (2019). Carruthers and Wanamaker (2019) represents the most comprehensive dataset on school resources in the 1910-1940 period. This data consists of a county-level panel of information on school enrollment, number of teachers, teacher salaries, expenditures on public education, local tax rates, and much more.[1] This data is primarily compiled from biannual reports to each state's department of education. Because the historical archives do not exist uniformly for all states, the panel is unbalanced. Appendix A discusses data coverage for the counties in our sample period.

[1] see: https://www.icpsr.umich.edu/sites/icpsr/view/studies/109625/versions/V1

2.3 Arkansas Voting Data

Arkansas was the only state of the three to pass its criminal anti-evolution law by ballot initiative. Although the initiative passed with 63% of the vote, there is substantial heterogeneity across the state in terms of support. We obtain county-level vote shares on the initiative from historical newspaper archives of the *Arkansas Gazette* which we use as a measure of local support for the law.

3 Empirical Strategy

We aim to measure the impact of state-level anti-evolution curriculum laws on high school enrollment and financial support for public education. Our main identification strategy is a difference-in-differences approach based on plausibly random variation in the timing and implementation of anti-evolution laws. Specifically, the design is based on comparing children below high-school-age to cohorts too old to be in school at the passage of the law. This strategy gets at the impact of the policy on children who have not yet enrolled in high school and whose decision to enroll may be determined by the policy. However, because the anti-evolution policy feasibly impacts both those on the margin of entering high school and those already of high school age, we also estimate a specification comparing high school age cohorts to those too old to enroll in high school at the time of the policy. The main specification is the following:

$$\begin{aligned} y_{isc} = \beta_0 + \beta_1 TreatedCohort_i \times TreatedState_s \\ + TreatedState_s + X_i + \delta_c + \epsilon_{isb} \end{aligned} \tag{1}$$

Where y_{isc} is the educational or economic outcome of interest for individual i from birth county c, in birth state s measured in the 1940 census. We are primarily interested in high school enrollment and high school completion, al-

though we also test for the effect of anti-evolution laws on economic outcomes such as wages and the probability of working in a professional occupation. We first define $TreatedCohort_i$ as an indicator equal to one if the individual belongs to a birth cohort which was younger than age 14 at the time of law implementation. We choose 14 as the cutoff because it is the age needed to obtain a work permit during this time period. For these specifications, $TreatedCohort_i$ is equal to zero for individuals aged 19 through 22 at the passage of the policy, and children aged 14 though 18 are omitted from the sample. For specifications measuring the impact of the policy on those currently of high school age, we set $TreatedCohort_i$ to one for cohorts aged 14 through 18 at implementation and omit cohorts aged 10 to 13. $TreatedState_c$ is an indicator for counties belonging to Tennessee, Mississippi, or Arkansas. X_i is a set of individual-level controls for other determinants of enrollment and economic outcomes. It includes dummies for urban, female, white, and foreign born parents. It also includes age, age squared, parental occupation score in 1920, and dummies for the presence of compulsory schooling laws and laws specifying minimum leave age. δ_c is a border-county pair fixed effect. The inclusion of this fixed effect means we are identifying the effect of mandates on enrollment within small geographic areas on either side of state border; these county-pairs are predominantly small and rural.

Figures 2a and 2b depict the raw time series of educational attainment for cohorts in treated and control states around the time of anti-evolution law implementation. Figure 2a plots years of education and Figure 2b plots the share of individuals who have completed any high school. In both figures, there is a clear parallel trend between the states with and without anti-evolution laws. Although individuals born in states which eventually pass anti-evolution laws have on average fewer completed years of schooling than individuals in states

which never pass a statewide mandate against teaching evolution, individuals in anti-evolution states are also more likely to ever attend high school, which could reflect a higher likelihood of dropping out. Although the gap between treated and control states remains relatively stable for years of education, there is a clear convergence in high school enrollment for cohorts which are exposed to the anti-evolution mandate. The enrollment advantage for treated states disappears, which suggests that the laws negatively impact entry into high school. We confirm this descriptive result holds using the specification outlined in Equation 1.

After examining the educational and labor market impacts of anti-evolution mandates using the border fixed effects design, we next investigate the effect of anti-evolution mandates on public school financing. We use the standard difference-in-differences approach with controls:

$$\begin{aligned} y_{ct} =& \beta_0 + \beta_1 Post_t \times TreatedState_s \\ &+ \delta_c + \gamma_t + \epsilon_{ct} \end{aligned} \tag{2}$$

Where y_{ct} is a set of outcomes in county c and year t related to measures of public support for the public education system: per capita measures of tax rates, expenditures, enrollment, number of teachers, and average teacher salary. β_1 is the coefficient of interest capturing the effect of anti-evolution mandates on school finance outcomes. δ_c is a county fixed effect and γ_t is a year fixed effect.

We are confronted with two potential limitations of the baseline difference-in-differences approach. The first is data availability: although Carruthers and Wanamaker (2019) is the most comprehensive dataset on school resources, data is not consistently reported for all counties in all states in the American South. For example, Mississippi (one of the states with anti-evolution laws) is almost

entirely missing, leaving two treated states represented in the data (Arkansas and Tennessee). These states passed their laws in different years. A staggered treatment rollout within a two-way fixed effects model presents concerns about erroneous comparisons arising from heterogeneous treatment effects, as highlighted by a number of recent papers (de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021; Callaway and Sant'Anna, 2021).

To address these concerns, we restrict the sample as follows. First, we construct control counties using five never-treated states which have county-level data consistently reported in Carruthers and Wanamaker (2019): Alabama, Georgia, Louisiana, North Carolina, and South Carolina. We then run the standard difference-in-difference twice: once with Tennessee as the treated state (dropping Arkansas), and once with Arkansas as the treated state (dropping Tennessee). Thus, we eliminate "forbidden comparisons" in the parlance of Goodman-Bacon (2021) between the two treated states with different treatment years. Robust standard errors are clustered at the county level.

4 Results

4.1 Educational Outcomes

We begin by investigating the impact of state curriculum mandates on high school enrollment, which is an important revealed preference metric of the perceived value of public education during a time period in which baseline high school enrollment is far from universal. Table 1 presents results from Equation 1 using the border-pairs fixed effects design. Column 1 restricts the sample to all individuals born in border counties in the age 10-22 cohorts at time of law implementation who have completed 8th grade. Among these individuals, passage

of an anti-evolution law reduces enrollment into high school by 1.9 percentage points. This effect size is about 3% of the mean ever-enrollment rate of 65%, or about one-tenth the difference between urban and rural enrollment rates.

Column 2 examines the effect of anti-evolution mandates on completing high school conditional on having enrolled: we restrict the sample to individuals born in the border counties who have completed at least one year of high school. We find that anti-evolution mandates increase completion rates by 2.5 percentage points. While there is selection out of high school enrollment, the students who choose to enroll are more likely to graduate. However, Column 3 shows there is no impact on college attendance among individuals who completed grade 12. While it appears that anti-evolution mandates do not affect college readiness, given our sample restriction it is possible that mandates impact college attendance if the students who choose not to pursue high school after the laws are passed would have been more likely to attend college; given the limited information available in the Census, we are not able to investigate the selection effect of the law on student quality.

4.2 Labor Market Outcomes

Having established that anti-evolution laws reduce high school enrollment, we next examine the impact of these laws on students' future success in the labor market. Columns 2 and 3 show there is no effect of an anti-evolution law on individuals' probability of pursuing a professional occupation or on their wages. These null findings suggest that evolution curriculum does not translate to productive skills in the labor market, and stand in contrast to Arold (2024) which finds exposure to evolution increases the likelihood students pursue life science careers in the 2000s. It is not difficult to reconcile these findings given the difference in the quality of evolution education between the time periods-

in the 1920s, instruction in evolution consisted largely of depicting humans as descended from monkeys (thus spurring the "monkey laws" backlash) with limited nuance. Meanwhile, fewer than 3% of individuals in our sample worked in professional occupations in 1940, so there is very limited scope for evolution education to directly inspire or prepare students for jobs in science.

In Column 1, we find that individuals who were exposed to anti-evolution laws are 1.5 percentage points less likely to move states. Although this is only suggestive evidence, this finding comports with Carvalho, Koyama and Williams (2024) and Cohen-Zada (2006) who posit that a primary motivation of supporters of curriculum mandates is to ensure students maintain in-group identity. Although we cannot test in-group identity directly, a higher propensity to remain in one's birth state could be a reflection of greater cultural ties.

4.3 Support for Public Education

We next test for the effect of anti-evolution mandates on measures of citizens' willingness to provide financial support for public education. Table 3 presents results from the standard difference-in-difference approach outlined in Equation 2. Panel A considers only Arkansas as the treated state relative to the five never-treated Southern control states which have extensive representation in the Carruthers and Wanamaker (2019) data. Panel B considers Tennessee as the only treated state (dropping Arkansas). Thus, each panel represents a series of six regressions with different outcome variables wherein a sole treated state is compared to a set of never-treated "clean controls".

With the exception of teacher salary, the results on public finance outcomes are relatively similar in direction and magnitude whether we consider Arkansas or Tennessee as the treated state. Anti-evolution laws are associated with large declines in local tax revenues and school expenditures per capita. Local tax

revenue decreases by slightly less than $1 per person, while expenditures decline by $1-2 per person. These are large declines when the per capita GDP of Arkansas was $224 in 1930. We also measure a decline in average daily attendance across all levels of the public school system, indicating the effect of curriculum mandates goes beyond high school. The two panels differ in the results with respect to teachers: In Arkansas, the number of teachers per capita falls while the average salary increases. In Tennessee, the number of teachers per capita rises slightly while the average teacher salary falls. These findings could be explained by a number of theories which we aim to investigate in future research; for example, the migration of younger teachers who are paid less on average from Arkansas to Tennessee. As a preliminary exercise, we obtain newly collected data on teacher licenses from archives at the University of Michigan. Figure 3 shows a convergence in teacher licenses across counties in Arkansas which were more or less in support of the law. In the early 1920s, areas that were on average more opposed to the law had more qualified teachers; this gap disappears by the end of the decade.

4.4 Heterogeneity by Baseline Support for Anti-Evolution Laws

One reason that state-level curriculum mandates remain controversial in public discourse is that they are often imposed on local school districts whose constituents hold opposing views. A key advantage of studying 1920s-era curriculum mandates is that, at least in Arkansas, the law was passed by ballot initiative. There are a few examples of recent curriculum mandates that have been passed by ballot initiative, but these pertain either to English-language instruction (as in Arizona and Massachusetts in the early 2000s) or more recently to sexual health instruction (Washington, 2020). There are few examples of

specific curriculum laws for which measures of support exist at a more granular level. By studying how the effect of the 1920s anti-evolution law on support for public education varies based on local support for the law, we hope to gain insight into the heterogeneous consequences of these mandates for local school districts.

To do this, we first obtain county-level vote shares on Arkansas's 1928 ballot initiative. Overall, the law was popular- it passed with 63% of the vote. Support was strongest among rural counties. Only four counties were outright opposed, which were primarily more urban counties including Pulaski (Little Rock) and Jefferson (Pine Bluff). Because opposition counties are clearly different than supportive counties on the basis of observable characteristics, we do not want to directly compare these counties to each other. Instead, we aim to construct an appropriate control group for the opposition counties in Arkansas from similar counties in never-treated Southern states.

The construction of this control group is a prediction exercise. Therefore, we employ a supervised, tree-based machine learning algorithm (XGBoost) which predicts vote margin on the based of a wide set of county-level demographic (race, foreign born, literacy), economic (urban-rural status, manufacturing establishments, manufacturing wages), and religious (number of adherents to different denominations per capita) variables. We then generate county-level predicted vote shares on a hypothetical anti-evolution law ballot initiative for all counties in the US.

Figure 4 depicts the predicted vote margin using this approach. It also shows analogously constructed predictions for voter turnout, yea votes as a share of the white population, and nay votes as a share of the white population. Notably, predicted margin is in favor of the policy in nearly all counties. This is to be expected given the vote passed with large margins in all but the most

urban Arkansas counties. Larger pro-policy margins are predicted in the South and West, with lower margins in the Midwest and Northeast. Predicted voter turnout is unsurprisingly lowest in counties with large shares of black population. The yea and nay votes per white population are also heavily influenced by the share of black population in a county.

In Table 4, we present XGBoost's measure of the predictive value of each variable. This measure is known as the gain, and it captures the reduction in prediction error induced when splitting the sample by each variable. Table 4 shows the results. Interestingly, the religious variables (60% gain) are much more predictive of vote margin than economic (6%) or demographic (30%) variables. Given the centrality of the anti-evolution debate to the fundamentalist-modernist split in American Protestantism, it is perhaps unsurprising that religious affiliation is a most prominent predictor of policy support.

5 Conclusion

State-level curriculum mandates are a hotly debated topic in the US public discourse. We study the impact of the first major state-level curriculum mandates on support for public education using the anti-evolution laws passed in the American South in the 1920s. Using variation in policy mandates in small geographies around state borders, we find that anti-evolution laws caused declines in high school attendance; however, students who did attend were more likely to graduate. We find no effect of these laws on college attendance, wages, or likelihood of working in a professional occupation.

We also find these laws cause broad declines in citizens' willingness to financially support public education. Per capita tax revenue and expenditure on education declines, as does average daily attendance across the public school system. This evidence suggests people opposed to the mandates become less

likely to fund a system that does not reflect their values. In future iterations of this paper, we aim to test this hypothesis directly by exploiting geographic variation in support for the Arkansas ballot intiative. This approach will hopefully allow us to document the long-run effects of state curriculum mandates on local public education.

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6 Figures

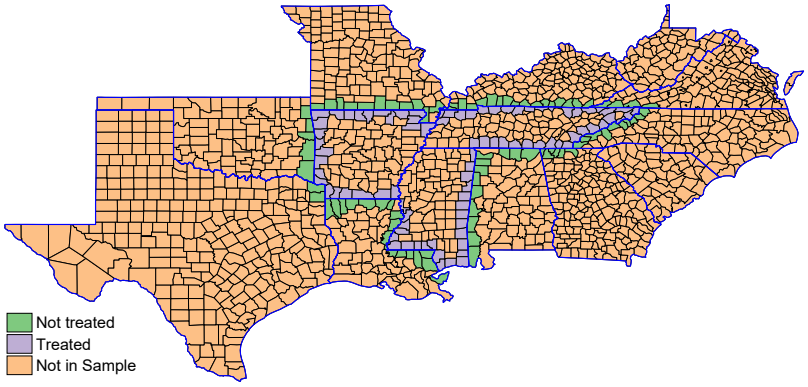
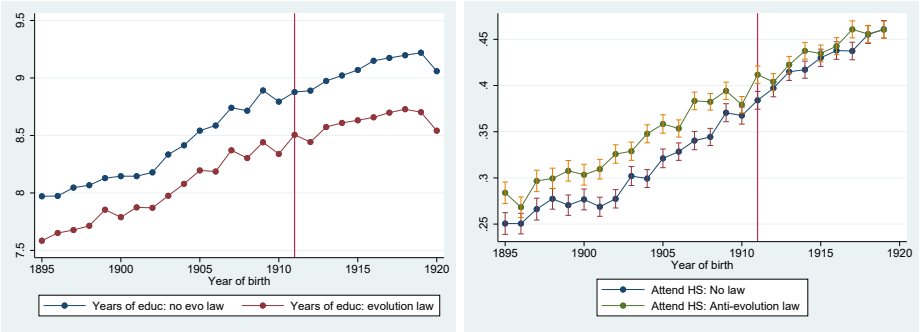


Figure 1: Treated and Untreated Counties in the Border Pairs Analysis



(a) Years of Education (b) Any High School

Figure 2: Descriptive Time Series of Anti-Evolution Policies on Educational Outcomes

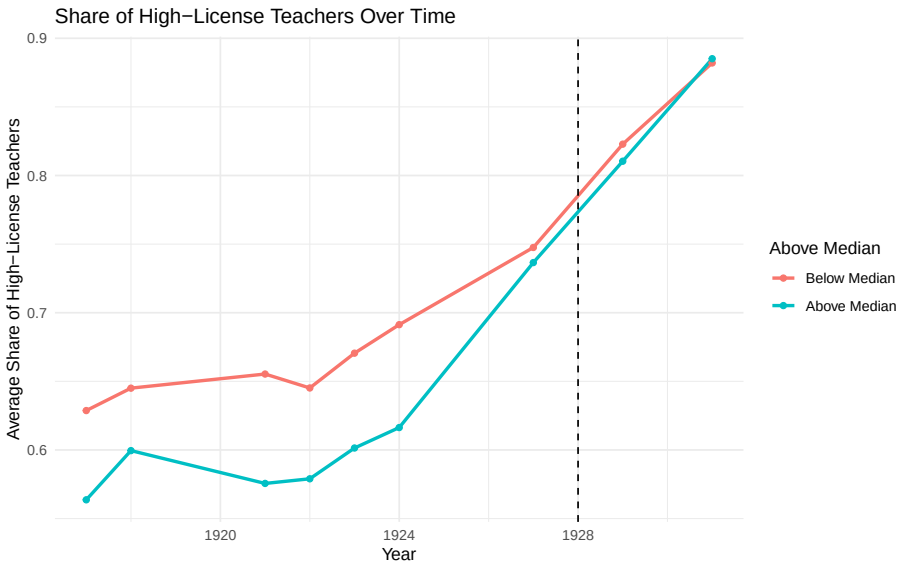
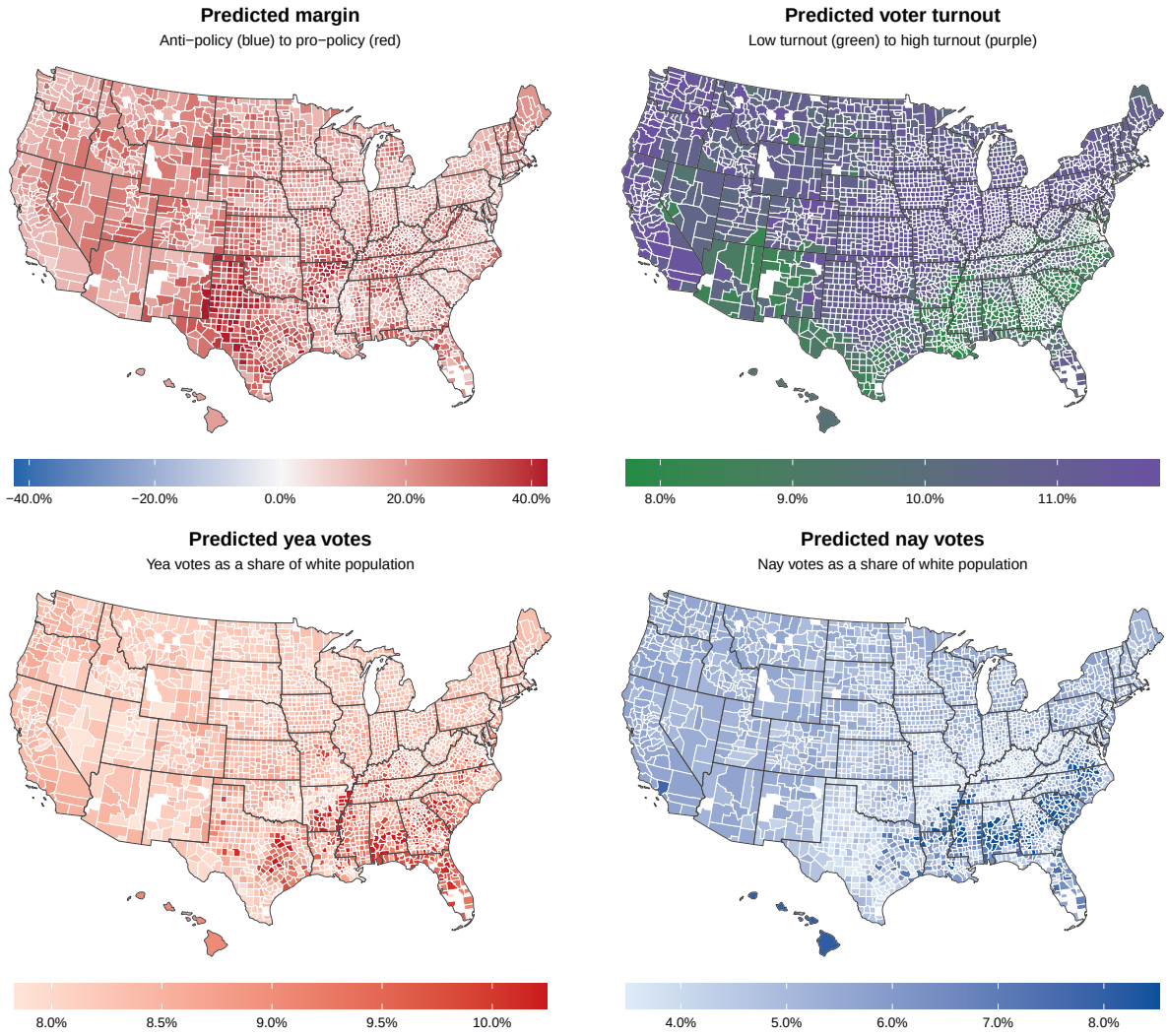


Figure 3: Share of Teacher with Highest Grade License by Referendum Vote Margin



XGBoost predictions trained on 75 Arkansas counties. Display colors are capped at the 2nd and 98th percentiles. County polygons use 2020 county borders and do not capture historical boundary changes.

Figure 4: Predicted Anti-Evolution Voting Across U.S. Counties.

7 Tables

Table 1: Educational Outcomes

VARIABLES	(1) Any High School	(2) Finished Grade 12	(3) Any college
Treated	-0.019**	0.025**	-0.001
	(0.008)	(0.010)	(0.013)
Compulsory school-entry law in effect		0.007	0.024
		(0.013)	(0.020)
Compulsory attendance law in effect		-0.012	0.017
		(0.014)	(0.019)
Child-labor minimum-age law in effect		-0.008	-0.019
		(0.014)	(0.017)
Age			-0.040***
			(0.012)
Age squared	0.000**	0.001***	0.001***
	(0.000)	(0.000)	(0.000)
Urban	0.143***	0.057***	0.080***
	(0.006)	(0.007)	(0.007)
Foreign born parents	0.090*	0.117**	0.075
	(0.047)	(0.055)	(0.061)
White	0.208***	-0.009	0.110***
	(0.013)	(0.012)	(0.013)
Father's Occupation Score	0.006***	0.005***	0.006***
	(0.000)	(0.000)	(0.000)
Observations	67,287	46,446	27,585
R-squared	0.117	0.033	0.065
Mean of dep. variable	0.650	0.594	0.312

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The independent variable Any High School is defined as grade completion above 8th grade. The sample is anyone who completed the 8th grade. Finished Grade 12 is defined as completion any grade equal to or higher to grade 12. The sample is anyone who completed at least one year of high school. Any college is defined as completion of any grade higher than grade 12. The sample is anyone who completed grade 12. Standard errors are clustered at the county and are displayed in parenthesis. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 2: Labor Market Outcomes

VARIABLES	(1) Move state	(2) Professional occupation	(3) Log wages
Treated	-0.015***	0.001	0.004
	(0.005)	(0.002)	(0.015)
Compulsory school-entry law in effect	0.013	0.007**	0.039
	(0.008)	(0.003)	(0.028)
Compulsory attendance law in effect	0.008	-0.002	0.029
	(0.008)	(0.003)	(0.024)
Child-labor minimum-age law in effect	-0.007	0.005	-0.001
	(0.010)	(0.005)	(0.019)
Age squared	-0.000**	-0.000***	-0.004***
	(0.000)	(0.000)	(0.000)
Urban	0.292***	0.013***	0.665***
	(0.011)	(0.001)	(0.016)
Foreign born parents	0.106**	0.015	0.110
	(0.053)	(0.022)	(0.096)
White	-0.000	0.017***	0.612***
	(0.008)	(0.002)	(0.016)
Father's Occupation Score	0.000	0.002***	0.012***
	(0.000)	(0.000)	(0.000)
Observations	108,200	108,200	73,303
R-squared	0.125	0.019	0.234
Mean of dep. variable	0.322	0.0264	6.234

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The sample is anyone who completed the 8th grade. Standard errors are clustered at the county and are displayed in parenthesis. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 3: Impact of Anti-Evolution Policy on School Quality Measures

VARIABLES	(1) Avg Salary	(2) Teachers per Cap.	(3) Enrollment per Cap.	(4) Avg Daily Attendance per Cap.	(5) Total Expenditures per Cap.	(6) Local Tax per Cap.
Panel A: Arkansas vs. Controls (Tennessee dropped)						
Treated x Post	410.687***	-0.001***	-0.040***	-0.037***	-2.075***	-0.713***
	(8.348)	(0.000)	(0.002)	(0.002)	(0.157)	(0.079)
Observations	4,381	8,397	8,910	8,903	8,645	8,460
Within R-squared	0.363	0.025	0.029	0.035	0.013	0.006
Panel B: Tennessee vs. Controls (Arkansas dropped)						
Treated x Post	-23.866*	0.000*	-0.042***	-0.061***	-1.396***	-0.953***
	(13.694)	(0.000)	(0.002)	(0.002)	(0.121)	(0.058)
Observations	4,768	11,452	12,406	12,392	12,714	12,498
Within R-squared	0.001	0.000	0.041	0.113	0.007	0.011

Each column is a separate regression of the indicated outcome on Treated × Post, with year and 1920 county fixed effects absorbed and x‘robust standard errors in parentheses. Panel A compares Arkansas (treated 1928) to the five never-treated control states (Alabama, Georgia, Louisiana, North Carolina, South Carolina), dropping Tennessee; Panel B compares Tennessee (treated 1925) to the same controls, dropping Arkansas. Each panel is restricted to the years its anchor state reports non-missing data for that outcome, which avoids the two-way fixed-effects "forbidden comparison" between the two treated states with different treatment years (Goodman-Bacon 2021). Outcomes other than average salary are expressed per capita. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Predictor Importance in XGBoost Algorithm

Rank	Predictor	Gain	Cover	Frequency
Margin of victory				
1	Church of Christ per capita	15.79%	14.36%	14.19%
2	Colored Methodist per capita	13.28%	11.53%	11.49%
3	Other Races	10.25%	8.91%	8.78%
4	Total Pop. (1920)	10.04%	8.77%	8.78%
5	American Baptists per capita	8.56%	7.97%	8.11%
6	Presby. USA per capita	8.07%	13.07%	12.84%
7	Black Male Population	6.15%	2.69%	2.70%
8	Protestant Episcopal per capita	4.98%	3.83%	4.05%
9	Total Black Church members 1926	4.57%	2.72%	2.70%
10	Manufacturing estab. per capita	4.23%	6.77%	6.76%
11	Cumberland Presby. per capita	2.85%	4.79%	4.73%
12	Negro Baptist per capita	2.35%	0.62%	0.68%
13	# Illiterate	2.05%	1.79%	2.03%
14	Methodist Episcopal per capita	1.41%	1.95%	2.03%
15	# Illiterate black	1.39%	3.42%	3.38%

Table 5: Gain (reduction in prediction error) associated with splitting the sample by each variable. Cover is a measure of how much of the training data is affected by the split along each variable. Frequency is share of all splits along each variable.