

THE LONG TERM EFFECTS OF SCHOOL PRAYER^{*}

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Abstract

This paper studies whether school prayer and Bible reading affect adult beliefs and behavior. We exploit the U.S. Supreme Court's early-1960s school-prayer decisions, which removed these practices only from states where they were still legal. Linking this state-by-cohort variation to adult outcomes in the General Social Survey, we estimate the long-run effects of removing school prayer on religiosity, family formation, labor-market outcomes, gender attitudes, and social views. The abolition of school prayer lowers adult religiosity; reduces marriage and fertility; increases employment; and shifts gender and selected social attitudes in a less traditional direction. The employment effect is driven by women, and may operate partly through decreased full-time homemaking and less traditional gender roles. A decomposition indicates that school prayer bans explain more than half the decline in American religious attendance, and roughly one quarter of the decline in fertility and rise in female labor force participation. The findings show that even brief daily devotional practices in childhood can shape adult beliefs, family life, and economic behavior.

Keywords: School prayer; religious education; religiosity; childhood socialization; family formation; female labor supply.

JEL Codes: Z12, I21, J12, J16.

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1 Introduction

School prayer is one of the most enduring and contested issues in education. Few school practices sit so directly at the intersection of childhood socialization, religious belief, constitutional law, and political conflict. A short daily prayer or Bible reading may appear minor relative to curriculum, teacher quality, or school finance. Yet for many families, religious exercises in public schools symbolized either the moral foundation of education, or for others, they represented state-sponsored religious coercion. The controversy has never fully disappeared. Globally, debates over religion in the classroom remain active in state legislatures, courts, and school boards.¹

Does a routine religious practice experienced during childhood alter beliefs and behavior decades later? This paper studies whether the removal of school-sponsored prayer and devotional Bible reading from public schools had persistent effects on students' adult lives. We examine the long-run consequences of the Supreme Court's school-prayer decisions of the early 1960s, focusing on adult religiosity, labor-market outcomes, gender attitudes, family formation, and broader social and political views.

It is not obvious how, if at all, school prayer should affect economic outcomes. On the one hand, schools are central institutions of learning which may shape adult outcomes and values. Repeated exposure to prayer and Bible reading during childhood could reinforce religious identity, increase participation in religious communities, and transmit norms about family, gender, work, and social behavior. On the other hand, school prayer may have had little lasting effect. Students may have ignored devotional exercises, resisted them, or treated them as perfunctory rituals. Families with strong religious preferences could substitute toward churches, private schools, or home religious instruction. Conversely, compulsory or semi-compulsory religious exercises might even generate back-

¹Despite a ban, several US states have recently reintroduced school religious instruction. For example, Texas recently made Bible stories required reading in school and several states have required display of the Ten Commandments in schools. There are ongoing court cases regarding the legality of such actions in the United States, and school prayer recently was contested in the United Kingdom as well as in European Union members such as France and Italy.

lash among some students. Whether removing school prayer weakened adult religiosity and altered later-life outcomes is thus ultimately an empirical question.

We study how school prayer affects long term outcomes using the abolition of state-sponsored prayer and devotional Bible reading in American public schools following the Supreme Court's landmark 1962 and 1963 decisions in *Engel v. Vitale* and *Abington School District v. Schempp*. Before these decisions, states differed substantially in their treatment of devotional exercises in public schools. In some states, Bible reading or prayer was still required or permitted by law; in others, state courts or statutes had already prohibited the practice. The 1963 Supreme Court of the United States (SCOTUS) ruling therefore generated sharp cross-state variation in exposure to the reform. The ruling directly changed the legal environment in states where devotional exercises were still in place, while leaving already-secularized states largely unaffected. At the same time, the ruling affected only cohorts who were still in school when the ban took effect. Older cohorts had completed their schooling under the pre-ruling regime, while younger cohorts spent an increasing share of their school years under the post-ruling regime.

Our empirical strategy exploits the fact that pupils in different states saw differential exposure to school prayer following the ban. We compare cohorts who were exposed to the post-ruling, prayer-free schooling regime in affected states to nearby cohorts in the same states, relative to the same cohort differences in states where school prayer had already been abolished. The treatment measures the share of a respondent's compulsory school years that occurred after the ruling in a state where school prayer or devotional Bible reading was still legally prescribed or permitted in 1963. We link this exposure measure to adult outcomes in the General Social Survey (GSS) using respondents' birth cohorts and the state in which they lived at age 16. Under the assumption that, absent the ruling, outcomes for affected and unaffected states would have evolved similarly across cohorts, we explore how a school prayer ban affected adult outcomes such as religiosity, labor market participation and social attitudes.

Our results show that removing school prayer had lasting effects. Full exposure to the court ruling banning prayer reduces an index of adult religiosity by 0.021, or roughly 0.07 standard deviations. The effect is concentrated in measures of religious intensity and participation: exposed cohorts are 5.4 percentage points less likely to describe themselves as strong believers and 3.9 percentage points less likely to attend church. These changes in religiosity are accompanied by changes in family formation. Full exposure to the ruling increases the probability of being divorced by 3.3 percentage points, reduces the probability of being currently married by 4.1 percentage points, and lowers fertility by 0.136 children.

We also see effects on labor market outcomes, which are linked to the family formation results. Exposure to the post-ruling regime increases employment by 5.0 percentage points and raises income on the extensive margin, while leaving income conditional on employment essentially unchanged. This pattern suggests that the labor-market effects operate primarily through labor-force attachment rather than higher earnings among workers. These labor market results are entirely driven by women, and are consistent with changes in gender norms and family attitudes. Individuals exposed to the removal of school prayer hold less traditional gender attitudes in adulthood. Full exposure reduces a gender-attitude index by 0.037, about 0.13 standard deviations, and the fraction of women in full-time homemaking by 4.2 percentage points.

We also find effects on some broader social attitudes, but little effect on political affiliation. Exposure reduces science skepticism by 3.0 percentage points and opposition to same-sex marriage by 11.1 percentage points, while having little average effect on conservative self-identification or opposition to abortion. The reform therefore did not simply move respondents uniformly to the political left. Instead, its effects are concentrated in domains closely connected to religiosity, traditional family structure, gender roles, science, and sexuality.

Event-study estimates broadly support the research design. We find little evidence that affected and unaffected states were on differential trends before the ruling. After the rul-

ing, the effects emerge gradually across successive cohorts, consistent with the fact that younger cohorts spent a larger share of their schooling under the post-ruling regime. Dose-response figures similarly show that adult religiosity declines with greater exposure to the reform. Robustness checks using alternative treatment definitions, alternative classifications of ambiguous states, restricted samples, flexible regional trends, nonlinear models, randomization inference, and sensitivity analysis for violations of parallel trends yield similar conclusions.

The estimated effects are economically meaningful and generally consistent across domains. Expressed in standard-deviation units, full exposure to the post-ruling regime reduces religiosity by about 0.07 standard deviations and strong religious identification by 0.11 standard deviations. The corresponding effects on marriage, divorce, and fertility are approximately 0.08–0.09 standard deviations, while employment, extensive-margin income, gender attitudes, and science skepticism change by roughly 0.11–0.14 standard deviations. The estimates are large enough to suggest that they in part drive major aggregate changes in postwar American society. Approximately 77 percent of the U.S. population lived in states where the Supreme Court decisions newly removed prayer or devotional Bible reading. Combining this population exposure with our estimated treatment effects implies that the rulings reduced aggregate church attendance by about 3 percentage points, lowered fertility by approximately 0.11 children per woman, and increased female labor-force participation by roughly 6 percentage points. These implied effects correspond to about 60 percent of the long-run decline in religious attendance and approximately one quarter of the contemporaneous decline in fertility and rise in female labor-force participation.

The findings imply that even brief, routine school practices can have persistent effects on adult beliefs and behavior. Removing school-sponsored prayer did not merely alter the formal boundary between church and state inside public schools. It changed the religious and social development of cohorts educated under the new regime, with consequences for

adult religiosity, family formation, labor-force attachment, gender attitudes, and selected social views. The results show that schools shape more than human capital. They also transmit, reinforce, or weaken social and religious norms, and changes in those norms can have measurable consequences decades later.

This paper contributes to a growing economics literature on the causes and consequences of religious belief, participation, and institutions. Important work in the economics of religion emphasizes that religious participation is not only a matter of private belief, but also an activity shaped by incentives, organizations, social networks, and institutional supply (Iannaccone, 1998; Iyer, 2016; McCleary and Barro, 2019). A related macroeconomic literature studies the relationship between religion and economic development across countries, showing that religious beliefs and religious participation are correlated with growth, institutions, and social behavior (Barro and McCleary, 2003; McCleary and Barro, 2006). While much of this literature documents broad associations between religion and economic outcomes, a central empirical challenge is that religiosity is endogenous: religious beliefs, family background, education, community institutions, and economic conditions are jointly determined.

Recent work has therefore moved toward identifying exogenous sources of variation in religion, religious institutions, and religiously shaped culture. In historical settings, Rubin (2014) shows that the diffusion of printing helped spread Protestantism during the Reformation, while Almelhem et al. (2026) use textual analysis to document the secularization of scientific language and the emergence of progress-oriented ideas before the Industrial Revolution. Chaudhary and Rubin (2011) study how religious and political institutions shaped human-capital formation in colonial India. Other papers exploit contemporary shocks to religious demand or supply. Bentzen (2019) shows that exposure to natural disasters increases religiosity, consistent with religious coping, while Bentzen et al. (2026) find that faith-based initiatives in U.S. states increased religiosity and conservative religious social views. In experimental work, Bryan et al. (2021) randomize exposure to

Protestant evangelism in the Philippines and find short-run increases in religiosity and income. These studies establish that religion can respond to institutional, environmental, and programmatic shocks, and that changes in religiosity can affect economically relevant behavior.

Our paper is also related to work on education as a channel of socialization and preference formation. Schools transmit not only skills, but also norms, identities, and political or religious ideas. For different countries, historic time periods and religions, Glaeser and Sacerdote (2008), Hungerman (2014), Becker et al. (2017), and Cesur and Mocan (2018) show that education contributed to declining religiosity. More broadly, Pritchett and Viarengo (2015) emphasize the role of the state in socialization through schooling, and Cantoni et al. (2017) provide causal evidence that curriculum can shape ideology. Adukia and Harrison (2025) explore textbooks in private versus public schools. The secularization literature further shows that religious decline in the West is a long-run process whose causes remain contested (Franck and Iannaccone, 2014; Squicciarini, 2020). Fruehwirth et al. (2019) show that religion can lower depression. Focusing on religious school curricula, Arold (2024) shows that teaching evolution as opposed to creationism in schools has significant effects on working in the life sciences, and Arold et al. (2025) demonstrate that abolishing compulsory religious education in Germany reduced adulthood religiosity and raised labor market participation.²

Together, these papers suggest that schools are a central setting in which states, communities, and religious organizations may shape beliefs and social attitudes, but they do not isolate the long-run consequences of removing specific religious practices from public schools. This paper fills that gap by studying one of the most important church–state reforms in U.S. education: the abolition of state-sponsored prayer and devotional Bible

²In a broad sense, the paper also contributes to the economics of education by showing that school practices can have persistent effects beyond test scores and human capital, shaping beliefs and social norms decades later. The paper thus links to a large literature in the economics of education, exploring how factors like teacher quality (Chetty et al., 2014; Dinerstein et al., 2022; Rockoff, 2004), school choice (Hoxby, 2000; Campos and Kearns, 2024), books (Adukia et al., 2023) and information (Hoxby, Turner, et al., 2013).

reading in public schools following the Supreme Court’s school-prayer decisions of the early 1960s. To our knowledge, this is the first empirical paper to study the long-run effects of abolishing school prayer in the United States. By linking this variation to adult outcomes, the paper contributes to the economics of religion by estimating how the removal of a routine religious practice from public schools affected later-life religiosity, family formation, labor-market outcomes, gender attitudes, and broader social views. We also show that the measured effects are large enough to explain a significant portion of trends in the United States, ranging from the declines in religiosity and fertility to the rise in female labor force participation. These results are important both in understanding the causes and consequences of long term trends in the United States and globally, as well as of relevance to ongoing policy debates today.

The remainder of this paper is organized as follows. Section 2 discusses the economics of prayer in school, and gives a history of school prayer in the United States. Section 3 discusses our empirical strategy exploiting the abolition of public school prayer following Supreme Court decisions. Section 4 describes our main analysis data while section 5 presents our main results. Section 6 presents robustness analysis and section 7 concludes.

2 Institutional Background

2.1 Prayer in Schools

Prayer has been a part of formal education, and the first Western higher education institution, Plato’s Academy, contained a grove of olive trees sacred to the goddess Athena and practiced pagan observances. European universities such as Oxford, Cambridge and the Sorbonne grew from a clerical tradition and in the United States early universities such as Harvard and Yale were largely founded by religious groups and had direct theological instruction (Wooldridge, 2021). The role of religion in education has decreased as societies secularized and modernized, but many major universities around the world retain a

religious affiliation. Additionally, religious organizations run a large number of schools. Nearly one in twelve American elementary and secondary school students go to a private religious school and in the UK one-quarter of state primary schools are run by the Church of England. Despite the important role that religion has played in education, there is little work studying the long run effects of prayer in school.

Why might prayer in school affect later-life economic outcomes? The most obvious way in which school prayer might affect outcomes is later life religiosity. Students exposed to religion may be more likely to retain religious values later in life. On the other hand, it is not obvious that school prayer would lead to higher levels of religiosity. One could imagine that there could also be a backlash to compelled religious instruction. Many European countries have compulsory religion courses or school prayer, but low levels of religious observance relative to the United States, which bans prayer from public schools.³

The effects on labor market outcomes are likely more nuanced. A large literature beginning at least with Weber and Kalberg (2013) has argued that religion directly affects educational and economic outcomes such as literacy and earnings; see also Becker and Woessmann (2009). Bryan et al. (2021), using a randomized control trial with Protestant missionaries, find that short-run labor market earnings increase. The authors argue that this is likely through a mechanism of increased grit and hard work.⁴ On the other hand, studies such as Arold (2024) suggest that religion might affect career choices and social attitudes. Namely, more conservative social attitudes in the long run could shift women away from the labor market. This could also affect marriage and fertility choices if more religious households are more likely to focus on homemaking rather than the labor market.

³For example, countries like Sweden and the United Kingdom have general religious education courses and countries like Spain require public schools to offer religious (generally Catholic) education. Despite this, these countries have very low levels of church-attendance and reported belief in God.

⁴One possible positive channel is the development of self-discipline and prosocial behavior. Prayer and other structured religious practices may encourage reflection, patience, honesty, and concern for others. If these habits improve classroom behavior, reduce conflict, or strengthen students' willingness to cooperate, they could contribute to better academic performance and stronger workplace skills. Prayer may also create social ties among students, families, and community members. These networks can later provide information about jobs, mentoring, financial assistance, and professional opportunities. In this way, prayer could affect earnings indirectly by increasing social capital.

Religion may also affect social and political attitudes. Jacob et al. (2026) show that many evangelical sermons in the United States contain political messages. Effects of school prayer on religiosity could thus translate into changes in political attitudes. Religion may also shape general social attitudes, lead to stronger social ties or the willingness to include individuals with differing characteristics.

While there are compelling reasons to think that school prayer might affect economic outcomes, it is entirely possible that one might find no effects. For example, religious parents may switch their children from public to private schools. Additionally, pupils may not pay attention during religious lessons, or they may push back or rebel against organized prayer.

2.2 School Prayer in the United States

2.2.1 School Prayer in Early America

During the colonial era and during early American history, school prayer was ubiquitous in schools throughout what would eventually become the United States. The first laws in the American colonies regulating school prayer were the "Old Deluder of Satan" school acts passed between 1642 and 1648 by Puritan Massachusetts. These laws were explicitly aimed at promoting "*knowledge of the Scriptures*" and "*to read & understand the principles of religion*". The primary text in American education was the *New England Primer*, which included texts from the King James Bible accompanied by Puritan Calvinist interpretations.

While education and religious indoctrination were effectively synonymous in the 18th century, this began to change in the 19th century. As the United States transitioned into the 19th century, Horace Mann championed the Common School Movement, which aimed to create a universal, state-funded public education system. To unite a diversifying population, Mann advocated for non-sectarian religious instruction. Religious instruction in schools became an extremely contentious political issue as more Catholic immigrants ar-

rived from Ireland and Germany, as well as Southern and Eastern Europe. This tension sometimes boiled over into violence, for example in the 1844 Philadelphia Prayer Riots more than 20 people were killed after rumors spread that Catholics were planning on removing King James Bibles from public schools (Kathan, 1989).

While the Establishment Clause of the US Constitution, stating that "*Congress shall make no law respecting an establishment of religion*" dates to the passage of the document in 1789, for the first century and a half of American history the clause was only interpreted as applying to the federal government. Many states had established churches, with Massachusetts being the last to disestablish the Congregational Church in 1833. However, growing tensions between Protestant and Catholic groups led to restrictions on state funding of parochial schools. President Ulysses Grant pushed for a failed Blaine Amendment, banning public funding of religious schools. President Grant argued that education "*should be left to families, churches, and private schools devoid of public funds.*" (DeForrest, 2003)

2.2.2 Later Court Decisions

In the wake of failure to pass the Blaine Amendment, many US states passed laws prohibiting state funding of religious schools, and later banning religious instruction in public schools. In a 1947 landmark Supreme Court decision, *Everson v. Board of Education* applied the Establishment Clause to state law. Justice Hugo Black famously invoked Thomas Jefferson's metaphor, declaring that the First Amendment erected a "*wall of separation between church and State.*" While courts gradually barred state support for religious education in the decade after *Everson v. Board of Education*, the issue of prayer in schools remained unresolved nationally (Green, 2018).

During the 1950s, anxiety about the Cold War and Communism led to an increase in public religion, for example adding "*Under God*" to the Pledge of Allegiance in 1954 and adopting "*In God We Trust*" as the national motto in 1956. It was against this backdrop of Cold War piety that the state of New York attempted to standardize moral instruction by

composing a generic, ostensibly ecumenical prayer for public school classrooms. After the New York State Board of Regents recommended a short prayer for use at the beginning of the school day, a legal challenge arose from plaintiffs opposed to school prayer.

This attempt at reintroducing school prayer to New York schools led to the seminal *Engel v. Vitale* case in 1962. The Supreme Court ruled 6–1 that the practice violated the Establishment Clause. Justice Hugo Black’s majority opinion emphasized that the constitutional problem did not depend on whether the prayer was denominational, brief, or nominally voluntary. The following year, the Court addressed Bible reading in the *School District of Abington Township v. Schempp*, which was decided together with a similar Maryland case *Murray v. Curlett*. Prior to the ruling, Pennsylvania law required public schools to begin the day with the reading of at least ten Bible verses. In the Abington schools, the reading was followed by recitation of the Lord’s Prayer. The Court ruled 8–1 that the exercises were unconstitutional, and by the end of 1963 both prayer and Bible reading were banned from public schools (Dierenfield, 2007).

Prior to these cases, states had different rules mandating, allowing or forbidding prayer in school as well as Bible readings. Figure 1 shows the legal status of Bible reading in the U.S. fifty states at the time of the *Abington School District v. Schempp* Supreme Court decision. Following these cases, public school prayer was effectively outlawed in the United States. While prayer remains almost nonexistent in public schools, there remains significant controversy. The decisions banning prayer were initially unpopular, and in 1971 a Gallup poll found that 65% of Americans disapproved of the court ruling that local governments could not compel recitation of the Lord’s Prayer or reading Bible verses in public schools, while 25% approved. In 2014 a similar Gallup poll found that a majority of Americans support prayer in schools. In addition, in the 2020s some US states have attempted to challenge the court ban on prayer and religious reading. For example, the Mississippi House Bill 1310 was proposed, which would require schools to adopt policies establishing a daily, voluntary period of prayer and reading of a religious text. Tennessee and Texas

have also seen proposed legislation requiring public schools to set aside a specific block of time for voluntary prayer and reading the Bible or other religious texts.

3 Empirical Approach

To estimate the effect of removing prayer and devotional Bible reading from public schools on students' outcomes in adulthood, we exploit the 1963 Supreme Court ruling in *Abington School District v. Schempp*, which declared state-sponsored Bible reading and prayer in public schools unconstitutional. The ruling generates plausibly exogenous variation in students' exposure to school prayer along two dimensions. First, it bound only those states in which devotional Bible reading and prayer were still legally prescribed or permitted at the time of the decision; states that had previously declared the practice an unconstitutional sectarian activity were effectively unaffected. Figure 1 maps this cross-state variation in treatment status, and Appendix Table A1 documents the underlying legal status of Bible reading in each state in 1963. Second, within affected states the ruling altered the schooling of cohorts that were still of school age in 1963, while leaving the schooling of older cohorts, who had already left school, untouched; later cohorts spent a larger share of their school career under the post-ruling regime than earlier ones. Combining both dimensions, we compare the adult outcomes of cohorts schooled before and after the ruling in affected states, relative to the same cohort differences in states that were not affected, after accounting for fixed differences across states and birth cohorts.

Baseline model. We implement this comparison in a two-way fixed effects (TWFE) model. The key idea is that, within affected states, cohorts schooled before the ruling act as a counterfactual for cohorts schooled after the ruling, while states that were not affected absorb any contemporaneous changes common to all states. The baseline specification is:

$$Y_{istu} = \beta \text{Exposure to Ruling}_{st} + \mathbf{X}'_i \gamma + \delta_s + \lambda_t + \theta_u + \varepsilon_{istu}. \quad (1)$$

The adulthood outcome Y_{istu} of individual i , who attended school in state s in cohort t and is surveyed in year u , is a function of the treatment variable $\text{Exposure to Ruling}_{st}$. This variable is continuous and measures the intensity with which the ruling affected an individual's schooling: it equals the share of an individual's school years that fell under the post-ruling regime in a state in which prayer and devotional Bible reading were still in place in 1963. The 1963–1964 academic year marks the first full school year under the post-ruling regime. Accordingly, we define 1964 as the effective reform year. Moreover, we rescale the exposure variable to facilitate the interpretation of our coefficients by dividing the total years of post-ruling exposure by 10, the standard duration of compulsory schooling at the time. As a result, it ranges from zero—for individuals who completed their schooling before the ruling and for all individuals in states that had already abolished the practice—to one, for individuals in affected states whose entire school career took place after the ruling.⁵ An estimated effect of β can thus be interpreted as the impact of full exposure to the reform over the entire 10-year compulsory schooling period. The vector $\mathbf{X}_i$ contains individual-level controls—indicators for gender, rural versus urban upbringing, parental nationality, and religious upbringing. State fixed effects δ_s , birth-cohort fixed effects λ_t , survey-year fixed effects θ_u , and an error term ε_{istu} complete the model. Because the General Social Survey records the state in which a respondent lived at age 16, we link individuals to the legal regime they faced as students even if they later migrated to a different state. Throughout, we cluster standard errors at the state level to account for potential correlation of the error terms across cohorts within states (Abadie et al., 2023).

The parameter of interest, β , captures the intention-to-treat (ITT) effect of the ruling: the effect on adult outcomes of moving from no exposure to full exposure to the post-ruling, prayer-free schooling regime. As we do not observe whether an individual was

⁵We code an individual as fully exposed ($\text{Exposure to Ruling} = 1$) if she entered primary school after the ruling, as this is the first cohort that could have spent its entire school career without school prayer. In robustness analyses we replace the continuous dosage measure by a binary indicator for whether an individual was affected by the ruling at all, and we vary the coding of states whose pre-1963 legal status was ambiguous (see Section 6 and Appendix Tables A3–A7).

personally exposed to classroom prayer, but only the legal regime prevailing in her state and cohort, β should be interpreted as an ITT effect that captures the combined consequences of reduced classroom prayer, of any adaptation of school practices following the ruling, and of any cohort-specific change in social norms surrounding religion in school.

Moreover, compliance with the ruling was gradual and incomplete, particularly in the South, so that some students in affected states continued to experience devotional exercises after 1963 (see Section 2). To the extent that legal treatment status overstates the actual change in exposure, β understates the effect of school prayer itself, and our estimates carry a lower-bound interpretation. Figure 4 illustrates the gradual dose-response relationship non-parametrically, plotting outcomes against bins of exposure intensity both unconditionally and after partialling out the controls and fixed effects. Figure 5 presents a coarser version of this relationship, splitting the group into terciles.

Identifying assumptions. Identification rests on a parallel-trends assumption: absent the ruling, outcomes of students schooled in affected and unaffected states would have evolved along common trends across cohorts. Because the ruling is a single, sharply dated event with a never-treated comparison group, our setting avoids the complications of staggered-adoption designs.⁶ The continuous exposure measure, however, distinguishes two versions of the assumption (Callaway and Sant’Anna, 2021): comparisons of exposed cohorts against the zero-exposure group identify cohort-specific intention-to-treat effects under the standard assumption, while summarizing them in a single dose-response coefficient additionally requires that the effect of a given exposure intensity not vary systematically across cohorts (“strong” parallel trends). This stronger condition is less demanding here than in settings with self-selected doses, since exposure intensity is a determinis-

⁶Most states that had prohibited classroom Bible reading before the 1963 SCOTUS ruling had done so many decades earlier. Because our main analysis compares cohorts around the 1963 ruling, we therefore treat the setting as non-staggered. As robustness checks, we exclude the small number of states that abolished classroom Bible reading after the 1910s and separately estimate a staggered-exposure design for states in which Bible reading was ruled to be a sectarian practice prohibited under the state constitution between 1950 and the 1963 ruling.

tic function of birth cohort and the ruling date. Consistent with it, the binary-treatment estimates and the event-study results corroborate the dose-response interpretation (Section 6).

The TWFE structure addresses several natural threats to the parallel-trends assumption. First, affected states differ systematically from unaffected states. For instance, they are on average somewhat more religious (Table 1), but the state fixed effects absorb all time-invariant differences across states. Second, the United States experienced a pronounced secularization over the studied cohorts; the cohort fixed effects absorb such national trends that are common across states. Survey-year fixed effects additionally net out differences in the timing and composition of the surveys. The effect is therefore identified only from variation that affects adjacent cohorts within a state differentially. Because the treatment stems from a single, sharply dated national event, confounding would require a contemporaneous shock with the same treated-state-by-cohort pattern, which seems unlikely; and selective sorting of families across states in response to the ruling would have to align with the same pattern while requiring costly relocation, when families could instead opt for private schools or homeschooling.

Two further features of the setting support the design. First, anticipation effects are unlikely: the ruling originated in litigation whose outcome and timing were beyond the control of states, schools, and families, and treatment status is assigned mechanically by birth cohort, so already-schooled cohorts could not retroactively change their exposure.⁷ Second, the ruling may have shifted norms surrounding religion in affected communities at large, touching even cohorts that had already left school. Such spillovers onto nominally untreated cohorts within affected states would compress the contrast between exposed and unexposed cohorts and thus bias the estimates toward zero, again rendering our estimates conservative.

⁷The Court's decision in *Engel v. Vitale* (1962), which struck down state-composed prayer one year earlier, may have led some schools to curtail devotional exercises slightly before *Schempp*. Any such early compliance would render cohorts just before the ruling partially treated and attenuate our estimates.

Event study. While specification (1) summarizes the effect in a single coefficient, the exposure of successive cohorts phased in gradually. To test the parallel-trends assumption and to trace out the dynamics of the effect, we estimate a non-parametric event-study version of the model that replaces the treatment variable by a full set of cohort indicators relative to the ruling:

$$Y_{istu} = \sum_{\substack{k \in \mathcal{K} \\ k \neq -1}} \beta_k [D_s \times \mathbf{1}\{t - t^* = k\}] + \mathbf{X}_i' \gamma + \delta_s + \lambda_t + \theta_u + \varepsilon_{istu}, \quad (2)$$

where D_s is an indicator equal to one if state s was affected by the ruling, t^* denotes the common effective reform year, and $\mathcal{K}$ is the set of event-time periods. We omit $k = -1$ as the reference period. The coefficients β_k compare changes across cohorts in affected states with the corresponding changes in unaffected states, relative to the omitted pre-ruling period. To smooth variation in the number of observations across cohorts, we group event time into five-year bins and label each bin by its final year; thus, $k = -1$ denotes the omitted bin covering the five years immediately preceding the ruling. Coefficients on earlier pre-ruling bins provide a falsification test of the identifying assumption: estimates indistinguishable from zero indicate that affected and unaffected states were not on differential trends before the ruling. Coefficients on post-ruling bins reveal whether the effect emerges immediately or phases in gradually, consistent with the gradual build-up of exposure across successive cohorts.

4 Data

Our analysis combines two data inputs. The first is a record of the legal status of devotional Bible reading and prayer in each state before and after the *SCOTUS* ruling, from which we construct the cross-state variation in treatment and the continuous exposure measure used in Section 3. The second is individual-level data from the General Social Survey (GSS),

which provide the adult outcomes and link each respondent to the state and cohort in which she was schooled.

4.1 State-level data: The legal status of school prayer

Treatment status is defined by the legal status of state-sponsored devotional Bible reading and prayer in each state at the time of the 1963 SCOTUS decision. We classify states into two groups. In the first group, devotional Bible reading and prayer were not abolished in public schools in 1963; these states were therefore bound by the ruling and form our affected, or treated, group.⁸ In the second group, devotional Bible reading and prayer had already been abolished before 1963; these states were effectively unaffected by the decision and serve as the comparison group.

We code the 1963 legal status from contemporaneous legal compilations and surveys of state Bible-reading and school-prayer statutes and case law (U.S. Department of the Interior, Office of Education, 1930; Conway, 1956; Doak, 1963; Reich, 1966; LaMorte and Dorminy, 1974)). Figure 1 maps the resulting cross-state variation in treatment status, and Appendix Table A1 lists, for each state, its classification and the underlying statute or ruling. Figure 1 shows that treatment status is well dispersed geographically rather than confined to any one region: the unaffected states that had already abolished the practice range from the Pacific Coast (e.g., California, Washington) and the Mountain West (e.g., Utah, Wyoming) through the Plains and Midwest (e.g., Nebraska, Illinois, Wisconsin) to the Deep South (Louisiana), while the affected states span every census region. The historical record makes clear that compliance with the resulting ban was gradual and incomplete, especially in the South, so that legal treatment status overstates the actual change in classroom exposure and our estimates carry a lower-bound interpretation (Birkby, 1966;

⁸This group includes states in which Bible reading and prayer were required by statute, authorized, or subject to no clear legal provision in 1963. In Appendix Table A8, Columns (2)–(5), we test robustness to alternative classifications based on pre-1963 legal provisions, including dropping ambiguous states from the sample, and coding ambiguous states as controls rather than treatment.

Way, 1968; Dolbeare and Hammond, 1971; Dills and Norton, 2022).

Figure 2 provides suggestive first-stage evidence that the rulings were followed by a substantial decline in school-sponsored religious exercises. In 1962, 55 percent of Gallup respondents reported that prayer, Bible reading, or similar religious observances took place in their local public schools, compared with less than 20 percent reporting regular school-led prayer or meditation in 1984. Although the survey questions are not identical and the comparison should therefore be interpreted cautiously, the approximately 35-percentage-point decline indicates a marked change in school practices following the rulings. At the same time, the persistence of reported prayer in 1984 is consistent with incomplete compliance and reinforces the lower-bound interpretation of our intention-to-treat estimates.⁹

From this legal coding we construct the continuous treatment variable $\text{Exposure to Ruling}_{st}$ used throughout the paper. For individuals schooled in affected states, the variable measures the share of an individual's compulsory school years that fell under the post-ruling regime. We date the first full school year under which the ruling could have any effect to the 1963–1964 academic year and accordingly treat 1964 as the effective event year.¹⁰ We rescale the measure by dividing the number of post-ruling school years by ten (the standard duration of compulsory schooling at the time). The variable equals one for cohorts in affected states who entered primary school in or after 1964, the first to spend their entire school career exposed to the ruling. Cohorts in affected states who were still in school in 1964 take intermediate values, equal to the share of their compulsory school years that fell under the post-ruling regime. Everyone else is coded zero. We assign each respondent to a state-and-cohort regime using the state in which the GSS records her residence at age 16 and her birth year, so that individuals are linked to the legal regime they faced as students even if they later migrated to a different state.

⁹Students may have continued prayer in schools by transferring to private schools. Additionally, it is possible that in some areas public schools did not comply with court ruling and there were not legal challenges.

¹⁰The Court's earlier decision in *Engel v. Vitale* (1962), which struck down state-composed prayer one year before *Schempp*, may have led some schools to curtail devotional exercises slightly earlier. Coding the post-ruling regime from 1964 treats any such early compliance as partial pre-ruling treatment of the immediately preceding cohorts, which works against finding an effect.

4.2 Individual-level data: The General Social Survey

The adult outcomes and individual characteristics come from the GSS, a repeated cross-sectional survey that has interviewed a nationally representative sample of U.S. adults, biennially, since 1972. The GSS is well suited to a design with state and cohort fixed effects: it is standardized and comparable across states and survey waves, it records each respondent's birth year and the state in which she lived at age 16, and it asks a broad battery of questions on religious, family, economic, and social outcomes. We assign respondents to the schooling regime they faced using the state-at-age-16 and birth-year information, and we retain respondents for whom we can construct the exposure measure and the control variables. Because individual outcome questions are asked of different, and sometimes only intermittent, subsets of respondents and survey waves, we restrict attention to variables from the GSS replicating core. This set of permanent questionnaire items is explicitly maintained across waves to enable reliable longitudinal comparisons, thereby ensuring consistency over time. The main estimation sample contains about 38,000 individuals.

We group the outcome variables into five domains. The first domain captures *religiosity*. Our summary measure is an index of religious outcomes, constructed as an equally weighted average of the underlying binary religious indicators, each coded so that higher values denote greater religiosity: identifying as a strong believer, praying frequently, attending church, and believing in the Bible, in God, and in an afterlife, together with reporting a religious affiliation and a fundamentalist self-identification. Appendix Table A2 lists the component questions from which the index is built and reports each separately. The second domain, *family formation*, comprises indicators for being divorced and married and the number of children. The third domain, *labor market* outcomes, comprises an indicator for being employed and two income measures: an intensive-margin measure, defined as log real income conditional on positive earnings, and an extensive-margin measure, defined as log real income over the full sample with non-earners assigned zero income, so that the latter reflects movements in labor-force attachment as well as in earnings. The

fourth domain captures *gender attitudes*: an index summarizing views on women’s roles and abortion. The index is constructed analogously to the religiosity index, as an equally weighted average of binary components coded so that higher values denote more traditional views. The components capture attitudes toward full-time homemaking, abortion in cases of genetic conditions in the fetus, women in politics, and women in the labor force. The fifth domain captures broader *social and attitudinal outcomes*: indicators for skepticism toward science and toward education, homophobic attitudes, opposition to same-sex marriage, and being pro-euthanasia, each coded so that higher values denote more socially conservative or skeptical positions. Appendix Table A10 provides for each outcome the exact survey question and answer options.

The vector of individual-level controls X_i consists of variables describing the childhood environment, which are predetermined with respect to the ruling: an indicator for gender; an indicator for a rural versus urban upbringing; indicators for parental nationality (both parents born in the United States, born abroad, or missing); and indicators for religious upbringing (raised non-religious, mainline Protestant, evangelical Protestant, Catholic, Jewish, or other).

Table 1 reports descriptive statistics for the outcomes and controls, for the full sample and separately for individuals schooled in affected (treated) and unaffected (control) states. Consistent with the institutional history, affected states which abolished prayer in schools later are on average somewhat more religious than unaffected states, consistent with the deep historical links between religion and the organization of schooling (West and Woessmann, 2010). For example, respondents from affected states are more likely to describe themselves as strong believers and were more often raised Protestant or evangelical, and they are somewhat more rural. In our regression analyses, time-invariant level differences are absorbed by the state fixed effects, but we nonetheless report them here and probe the parallel-trends assumption directly in our empirical analyses.

5 Results

5.1 Main Results

5.1.1 Effects on Religiosity

We begin our empirical analysis by examining the long-term impact of the 1963 SCOTUS ruling on adult religiosity and downstream social attitudes. Table 2 reports our baseline estimates for religious outcomes. Full exposure to the post-SCOTUS schooling regime reduces the index of religious outcomes by 0.021, an effect that is statistically significant at the 1 percent level.¹¹ Relative to the dependent-variable standard deviation of 0.28, this corresponds to a decline of approximately 0.07 standard deviations. The effect is therefore economically meaningful, and results indicate that removing school-sponsored prayer and devotional Bible reading produced a persistent reduction in adult religiosity among the cohorts whose schooling was affected by the ruling.

The disaggregated outcomes help identify the dimensions of religiosity underlying this effect. Full exposure reduces the probability of describing oneself as a strong believer by 5.4 percentage points, relative to a sample mean of 46 percent. Church attendance also declines, by 3.9 percentage points, although this estimate is less precise and is only statistically significant at the 10% level. There is also a reduction in prayer, but this effect is statistically insignificant at conventional levels. By contrast, we find little evidence of effects on belief in the Bible, belief in God or an afterlife, religious affiliation, or self-identification as a fundamentalist. The reform thus appears to have affected the intensity with which individuals identify with and participate in religion more than the underlying existence of religious belief.¹²

Figures 3 and 4 provide complementary evidence on the timing and functional form of these effects. The event-study estimates in Figure 3 show no indication that cohorts in af-

¹¹Appendix Table A2 shows additional religious outcomes, from which the index is built.

¹²This pattern is also consistent with the results for the broader religious index and additional measures reported in Appendix Table A2.

affected and unaffected states were on differential trends before the ruling. The coefficients for the religious index and strong-believer indicator are close to zero in the pre-ruling period, and the joint tests fail to reject the null of no pre-treatment effects. After the ruling, the estimates become increasingly negative as successive cohorts receive more of their schooling under the post-ruling regime. The post-ruling coefficients are jointly significant for both the religious index and the strong-believer outcome, with p-values of 0.019 and less than 0.001, respectively. The gradual emergence of the effects is consistent with the institutional design: cohorts immediately following the decision experienced only partial treatment, whereas later cohorts spent a larger portion of their school years without devotional exercises.

The dose-response relationships in Figure 4 lead to the same conclusion. This figure plots TWFE OLS point estimates within exposure bins. The solid line represents the linear fit and the shaded areas denote 95% confidence intervals. The left panels display the unadjusted raw means with no controls while the right panels display the fully specified model with covariates and fixed effects. The figure displays the effect of the ruling on religious outcomes as indicated in the figure headers. Both the raw data and the residualized relationships exhibit a negative association between exposure to the ruling and adult religiosity. The decline is especially pronounced for the strong-believer indicator. Although the bin-level estimates are not perfectly monotonic, the overall pattern supports the linear exposure specification used in the baseline analysis and provides little evidence that the results are driven by a small number of cohorts at a particular exposure level.

5.1.2 Family Formation

We next explore family formation and household structure. Table 3 shows that full exposure increases the probability of being currently divorced by 3.3 percentage points and decreases the probability of being currently married by 4.1 percentage points. Both estimates are statistically significant at the 1 percent level. Exposed individuals also have

0.136 fewer children on average, relative to a dependent-variable mean of 1.85. These estimates suggest that the effects of school prayer extend beyond explicitly religious outcomes and affect family formation.

The results in Table 3 suggest that lower religiosity levels decreased both marriage and childbirth, and increased marital dissolution. Figure 6 reinforces this interpretation. There is no evidence of differential pre-ruling trends for any of the three family outcomes, while the post-ruling coefficients are jointly significant for divorce and fertility and marginally significant for marriage. As with religiosity, the effects generally become more apparent among cohorts with greater exposure to the reform.

5.1.3 Labor Market Outcomes

Table 4 reports results for labor-market outcomes, decomposing these effects between extensive and intensive margin results. Full exposure to the ruling raises employment by 5.0 percentage points, relative to a mean of 82 percent. Consistent with this increase in employment, the extensive-margin income measure rises by 0.528. In contrast, the estimated effect on income conditional on working is essentially zero. Taken together, these findings suggest that the labor market effects are driven by the extensive margin, and indicate that the increase in income is driven primarily by greater labor-force attachment rather than higher earnings among individuals who would have worked in either regime. The event studies in Figure 7 show no evidence of differential pre-trends. The post-ruling estimates for employment and extensive-margin income are generally positive, although the joint post-treatment tests are significant only at approximately the 10 percent level. The dynamic results should therefore be interpreted as supportive but less precise than the corresponding baseline estimates.

Why might employment rise on the extensive margin? One possibility is that more conservative religious attitudes changed family dynamics. Table 5 explores how less prayer in school impacted gender attitudes and whether women became full-time homemakers.

We construct a gender attitude index, comprising questions regarding women's role and abortion attitudes.¹³ In Table 5, full exposure reduces the gender-attitude index by 0.037, or approximately 0.13 standard deviations.

Because higher values of the index capture more traditional views, the estimate implies that individuals educated under the post-ruling regime subsequently express less traditional attitudes regarding gender roles. Support for full-time homemaking declines by 4.2 percentage points. Indeed, Appendix Table A8 further shows that the entirety of the employment effect is driven by women. Figure 8 again shows little evidence of differential pre-ruling trends and a sustained movement toward less traditional attitudes after the decision. The post-ruling coefficients are jointly significant at the 10 percent level for the index and at the 5 percent level for the homemaking outcome. The results strongly suggest that the employment effects are driven by women being less likely to leave the labor force and stay at home.

5.1.4 Social Attitudes

The previous subsection indicated that the court ruling had real labor market effects, and that these were likely driven by changes in social attitudes. What other attitudes may have been affected by less prayer in schools? Table 6 examines broader social and political attitudes. Full exposure reduces science skepticism by 3.0 percentage points and opposition to same-sex marriage by 11.1 percentage points.¹⁴ The latter estimate is particularly large relative to the sample mean of 41 percent and is statistically significant at the 1 percent level. The point estimate for homophobic attitudes is also negative but is not statistically distinguishable from zero. We find a small increase in education skepticism of 2.2 percentage points, significant only at the 10 percent level. There is no detectable average effect on whether respondents identify as conservative or oppose abortion. These results imply

¹³These questions are listed in Appendix Table A10, and pertain to whether women should work, enter politics or stay at home.

¹⁴Note that the sample size is much smaller for same-sex marriage, as this question was only asked intermittently in the GSS.

that the reform did not simply shift respondents uniformly to the political left. Instead, its effects are concentrated in attitudes closely related to religion, science, sexuality, and traditional social norms.

The event studies in Figure 9 support this more differentiated interpretation. Opposition to same-sex marriage falls progressively among post-ruling cohorts, and the post-treatment coefficients are jointly highly significant. Science skepticism also declines after the ruling, though the dynamic estimates are less precise. The estimates for education skepticism, homophobia, and conservative identification show neither systematic post-ruling movements nor evidence of differential pre-trends. Abortion opposition declines for the first exposed cohorts but subsequently returns toward zero, helping reconcile the jointly significant event-study coefficients with the near-zero average effect in Table 6. Thus the broad visual patterns suggest a nuanced view of the effect of school prayer. The policies affected social attitudes and views on education, but not direct political views.¹⁵

5.2 Magnitudes

We next place the estimates on a common scale to compare effects across domains and studies. When placed on a common standardized scale, the affected outcomes move by strikingly similar magnitudes: Full exposure lowers religiosity by 0.07 standard deviations and strong belief by 0.11. Divorce rises by 0.09 standard deviations, while marriage and fertility fall by about 0.08. Employment and extensive-margin income increase by 0.12 and 0.14 standard deviations, while gender traditionalism and science skepticism fall by about 0.12 and 0.11 standard deviations.¹⁶ Overall, the estimates point to a common shift in religious and social norms that propagated into family choices, women’s labor supply,

¹⁵We caution that this may have to do with the time period studied in the paper. Most realized outcomes occur in the 1980s and 1990s, when political polarization in the United States was markedly lower (Kempf and Tsoutsoura, 2021; Kempf and Tsoutsoura, 2024; Colonnelli et al., 2023). Thus many individuals with more conservative social attitudes could be more at place in the traditionally more left-leaning Democratic party relative to the present day.

¹⁶Opposition to same-sex marriage seems to be an outlier at 0.24 standard deviations. However, it should be interpreted with caution as it is estimated on a substantially smaller GSS sample.

and selected attitudes, echoing evidence that education policies can leave sizable imprints on domains they were never designed to touch (Arold and Shakeel, 2026).

These magnitudes are economically meaningful, especially given the low-intensity intervention, the long-run horizon, and the breadth of affected domains. Moreover, they are broadly in line with prior evidence. The religiosity effect is close to the 0.07 standard-deviation decline estimated for the abolition of compulsory religious education in Germany (Arold et al., 2025), and to the roughly 0.10 standard-deviation decline in Protestant church attendance associated with historical increases in schooling (Becker et al., 2017). It also mirrors, with the opposite sign, the 0.08–0.13 standard-deviation increase in religiosity produced by a Protestant evangelism experiment (Bryan et al., 2021). Most of our attitudinal effects are smaller than those of the intensive ideological curriculum reform studied by Cantoni et al. (2017), as one would expect from a brief daily devotional practice rather than a wholesale curriculum change. They also contrast with Arold (2024), where teaching evolution changed scientific beliefs but left religiosity largely unaffected.

The closest comparison, Arold et al. (2025), helps sharpen the mechanism. Their setting removes the compulsory nature of a dedicated confessional subject; ours removes a brief devotional practice from the ordinary school day. The similar religiosity effects suggest that repeated public religious practice can matter even without formal doctrinal instruction. The downstream mechanisms differ, however. In Germany, labor-market effects do not differ by gender and are interpreted as consistent with time-use and church-tax channels. In the United States, the employment response is driven by women and is accompanied by lower support for full-time homemaking and less traditional gender attitudes, pointing instead to a gender-norm channel. The U.S. reform also affects marriage, divorce, gender attitudes, and selected social views, whereas the German reform left measured ethical, gender-related, and political values largely unchanged.

5.3 Long Term Impacts of Supreme Court Ruling

5.3.1 Quantifying the Aggregate Impacts of the Ruling

In this section, we ask what the counterfactual have looked like had the Supreme Court not abolished prayer? As Figure 1 shows, the vast majority of US states allowed school prayer in 1962, prior to the SCOTUS decision. Nearly three-quarters of Americans lived in states that allowed Bible reading and prayer prior to the decisions. Earlier in this section, we showed that the abolition of school prayer had substantial effects on religiosity, fertility and income through female labor force participation. How much of the aggregate US decline in religiosity and fertility, and the increase in female labor force participation can be explained by the court rulings?

The estimates above measure individual-level intention-to-treat effects of moving from zero exposure to full exposure to the post-SCOTUS regime. We next ask how large these effects are in the aggregate. To do so, we implement a simple Oaxaca, 1973 style decomposition that combines the estimated treatment effects with the cross-state population incidence of the reform. Let T_s denote an indicator equal to one for states in which devotional Bible reading or prayer had not already been abolished before the Supreme Court decisions, and therefore where the decisions newly removed school prayer. Let p_s^{1962} denote state population in 1962, and define $w_s = p_s^{1962} / \sum_s p_s^{1962}$. For outcome y , with full-exposure estimate $\hat{\beta}_y$, the aggregate effect under counterfactual exposure vector g_s is

$$\Delta_y(g) = \hat{\beta}_y \sum_s w_s g_s \quad (3)$$

We consider a counterfactual corresponding to the actual legal environment, in which only states that had not already abolished school prayer were newly bound by the decisions. We weight states by their 1962 populations using the Census intercensal state population estimates. The affected states contained 141.98 million residents out of 184.95 million residents in the fifty states in 1962, comprising 76.8% of the total US population.

The decomposition implies that, if all states had still allowed prayer before the Court decisions, the removal of school prayer would have reduced fertility by 0.136 children per adult, and reduced church attendance by 3.9 percentage points. Under the actual legal environment, where only 76.8% of the population lived in states newly affected by the decisions, the corresponding aggregate reductions are 0.104 children for fertility, and 3.0 percentage points for church attendance.

The decomposition suggests that the removal of school prayer can account for a meaningful share of several cross-cohort changes. We compare cohorts fully exposed to the prayer ban, to those that went to school before the ban. Under the actual historical assignment, only residents of affected states receive the treatment, so the implied aggregate effect is $\Delta_Y^{\text{actual}} = \sum_s w_s T_s \hat{\beta}_y$. By contrast, under the counterfactual in which all states still allowed prayer before the Supreme Court decisions, every state would have been newly treated, so

$$\Delta_Y^{\text{all states}} = \sum_s w_s \hat{\beta}_y Y = \hat{\beta} Y. \quad (4)$$

We then compare these implied effects to the observed aggregate change between 1960 and 1980, $\Delta_Y^{1940-1980} = Y_{1980}^{\text{obs}} - Y_{1940}^{\text{obs}}$ and compute the share explained as

$$Y^{SE} = \frac{\Delta_Y^{\text{actual}}}{\Delta_Y^{1940-1980}}. \quad (5)$$

For religious attendance, the estimated full-exposure effect is a 3.9 percentage-point decline, and the population-weighted effect is a 3.0 percentage-point decline. Gallup reports that weekly church attendance dropped from 49% in 1955 to 44% by 1992. Using the roughly 5 percentage-point gap as a benchmark, the population-weighted effect of the Court decisions accounts for about 60.3 percent of the observed difference. Thus, the estimated attendance effect is meaningful, and it explains the slight majority of the long-run decline in religious participation.

For female labor-force attachment, the estimated full-exposure effect is 8.1 percentage points. Weighting by the affected-state population share implies an actual aggregate effect of 6.2 percentage points. The female 25–54 labor-force participation rate rose from about 28% in 1940 to about 51.5% in 1980, a change of 23.5 percentage points. The implied effect of the Court decisions is therefore roughly 26.5% of this cohort-proxy change.

For fertility, the female subsample estimate implies that full exposure reduced completed fertility by 0.138 children. The population-weighted effect of the Court decisions is -0.106 children. As a rough benchmark, the U.S. period total fertility rate fell from 2.23 in 1940 to 1.82 in 1980, a decline of 0.41 births per woman. Relative to this benchmark, the implied effect of school-prayer removal is about 25.8% of the observed decline in fertility, a nontrivial fraction.

Figure 10 summarizes the decomposition. In each panel, the first bar shows the pre-reform outcome, while the second decomposes the observed post-reform outcome into the pre-reform level, the population-weighted change attributable to the SCOTUS ruling, and the remaining change. The estimated effect is scaled by the share of the U.S. population living in states where the ruling newly removed school prayer. The final bar reports the ruling’s estimated contribution as a percentage of the total observed change. The ruling accounts for approximately 60 percent of the decline in religious attendance, 26 percent of the increase in female labor-force participation, and 26 percent of the decline in fertility.

5.3.2 Quantifying the Aggregate Trends

Figure 11 provides a cohort-level counterfactual counterpart to the aggregate decomposition. Let $\hat{\beta}_y$ denote the estimated full-exposure effect and $\bar{E}_c$ the sample mean exposure for birth cohort c . Note that `ExposuretoRulingi` is coded zero for respondents residing at age 16 in states where Bible reading in public school classrooms was declared a sectarian practice forbidden by constitution prior to the SCOTUS ruling. As a result, the cohort mean $\bar{E}_c = \frac{1}{N_c} \sum_{i \in c} E_i$ already incorporates the empirical share of cohort c ’s sample residing in

newly-affected states. Unlike the aggregate decomposition in Figure 10, which applies a single fixed population share (0.768) external to the estimation sample, the cohort-level counterfactual therefore requires no separate population-share adjustment, as this is embedded directly in $\bar{E}_c$. The implied counterfactual outcome absent the SCOTUS ruling is then given by:

$$Y_c^{CF} = Y_c^{obs} - \hat{\beta}_y \bar{E}_c \quad (6)$$

where Y_c^{obs} is the observed cohort mean. Thus, the counterfactual removes the component of the observed outcome attributed to exposure under the maintained assumption that the estimated treatment effect is causal and linear in exposure. In each panel, the solid black line and dashed navy line report the observed and counterfactual cohort profiles, respectively, for religious attendance, female labor force participation, and completed fertility. The shaded region between the two series captures the estimated contribution of the ruling to cross-cohort differences: given a monotonic rise in average exposure ($\bar{E}_c$) across successive cohorts, the divergence between the observed and counterfactual series widens progressively for later cohorts, while remaining small for cohorts whose schooling largely preceded the Supreme Court ruling. Consistent with our aggregate decomposition, this exercise visually underscores the effects of school-prayer decisions on real outcomes.

Figure 11 complements the aggregate decomposition by tracing observed cohort outcomes against the estimated counterfactual trajectories that would have prevailed absent the Supreme Court ruling. Across all three outcomes, the observed and counterfactual series coincide for cohorts educated before the reform and diverge progressively among later cohorts as exposure to the post-ruling regime increases. Religious attendance would have remained systematically higher in the absence of the ruling, while female labor-force participation would have risen more slowly and completed fertility would have declined less sharply. The widening shaded area therefore captures the cumulative contribution of removing school prayer to cohort-level changes in religious participation, women's employment, and fertility. Importantly, the figure indicates that the ruling did not initiate the

broader postwar trends in these outcomes: religious attendance and fertility were already declining, and female labor-force participation was already increasing. Rather, the estimates imply that the reform materially accelerated each of these transformations among the cohorts whose schooling was most affected.

Overall, this cohort-based decomposition suggests that the school-prayer decisions were not merely symbolic and had large effects on real outcomes. Under the paper’s estimates, the reform accounts for approximately one quarter of the share of the movement from the 1940 to the 1980 birth cohort in female labor-force attachment and fertility, while explaining more than half of the portion of the decline in religious attendance. The exercise should be treated with caution as many other forces changed across these cohorts, including the diffusion of contraception, changes in divorce law, rising female education, changing labor-market opportunities, and broader secularization. Nevertheless, the magnitude of the implied effects is large enough to suggest that removing a brief but repeated religious practice from public schools contributed measurably to the transformation of family, work, and religious life across postwar American cohorts.

6 Robustness

We begin our robustness section by complementing our cluster-robust standard errors with a randomization-inference placebo test. We then probe the parallel-trends assumption of the event studies using the sensitivity analysis of Rambachan and Roth (2023). Moreover, we show that the estimates are robust to a broad range of alternative specifications, including alternative treatment definitions, alternative classifications of states whose pre-1963 legal status was ambiguous, a range of sample restrictions, adding additional educational policies as controls, and nonlinear estimators, among others. Across these checks, the results consistently support the baseline conclusions of the main analysis.

Randomization inference. As an alternative way of conducting inference that does not rely on a large number of clusters, we randomly reshuffle treatment across states and re-estimate the model on each of 1,000 permutations, generating a placebo distribution of coefficients against which the true estimate can be compared (Appendix Figures A4–A8). If the estimated effect of the *SCOTUS* ruling reflected some feature of the affected states unrelated to the ruling, we would expect placebo assignments to produce effects of comparable magnitude. Instead, the baseline estimates for the central outcomes fall in the tails of their placebo distributions. For the index of religious outcomes, the baseline coefficient (-0.021) is more extreme than the first percentile of the 1,000 placebo coefficients (-0.018), and the estimate for identifying as a strong believer (-0.054) is likewise more extreme than the first percentile of its placebo distribution (-0.044). While some other estimates, such as the effect on the number of children, are less extreme relative to conventional percentiles of the placebo distribution, the estimates for many other main outcomes such as currently divorced or married and for opposition to same-sex marriage similarly lie in the tails of their respective placebo distributions. These results indicate that the main effects are highly unlikely to be artifacts of the cross-state treatment pattern and do not depend on the asymptotic approximations underlying cluster-robust inference.

Sensitivity to violations of parallel trends. Although the event studies show no systematic differential trends between affected and unaffected states before the ruling, the absence of a significant pre-trend is not by itself proof that the parallel-trends assumption holds, because pre-trends that are estimated imprecisely are less likely to be rejected (Rambachan and Roth, 2023). We therefore assess how sensitive our conclusions are to potential violations of parallel trends using the approach of Rambachan and Roth (2023), which replaces the assumption of exactly parallel trends with the weaker requirement that any post-ruling deviation from a linear trend be no larger than a bound M times the maximal pre-ruling deviation, and reports fixed-length confidence intervals for the treatment

effect as a function of M . These confidence sets correct for the low-power property of conventional pre-trend tests. For the religiosity outcomes and most gender and labor-market outcomes, the robust confidence intervals continue to exclude zero under linear trends ($M = 0$) and remain informative for a range of allowable deviations from linearity, so that the baseline effects are not overturned by smooth violations of parallel trends (Appendix Figures A9–A13). These bounds are conservative, as they impose no restriction on the sign or the shape of the differential trends.

Further robustness. We probe the robustness of our results along several dimensions. First, we consider alternative treatment definitions and codings. The baseline treatment is a continuous measure of exposure intensity, and to show that our conclusions do not hinge on this coding, we re-estimate the model replacing the continuous dosage measure with a binary indicator for whether an individual was affected by the ruling at all (Appendix Tables A3–A7, column 1). The binary-treatment estimates, which rely only on the standard rather than the “strong” parallel-trends assumption, deliver the same conclusions as the dose-response specification, and we cannot reject the equality of the two coefficients. We also probe the classification of states whose pre-1963 legal status was ambiguous, i.e. states in which devotional Bible reading was merely authorized or subject to no clear legal provision. In three variants, we successively drop the authorized and no-clear-provision states from the sample and reassign the no-clear-provision states from treatment to the control group (columns 2–4), and in a fourth, we additionally code states that had abolished the practice before 1963 as treated (column 5). Across all four codings, the estimates are stable and statistically indistinguishable from the baseline, confirming that the results are not driven by the treatment of legally ambiguous states.

Second, the estimates are robust to a range of sample restrictions. Restricting the sample to the twenty most populous states, which contain the bulk of the observations and the largest state clusters, leaves the estimates essentially unchanged (Appendix Tables A3–

A7, column 6). Restricting the sample to individuals whose schooling ended within twenty-five or thirty years of the ruling likewise reproduces the baseline results (columns 7 and 8). Moreover, we drop the small number of states that abolished classroom Bible reading after the 1910s (column 9), and we estimate a staggered-exposure design for states in which Bible reading was ruled to be a sectarian practice prohibited under the state constitution between 1950 and the 1963 ruling (column 10). The stability of the estimates across these subsamples indicates that they are neither driven by a handful of small states nor by long-run comparisons between cohorts far removed from the ruling, and also not by the few states that abolished Bible reading in the decades before the SCOTUS ruling.

Third, results hold when we control for division-specific linear and then linear-and-quadratic time trends, which absorb smooth region-by-cohort changes that affect adjacent cohorts differentially (columns 11 and 12). Finally, because many of our outcomes are binary, we verify that the linear probability model is not driving the results by re-estimating the binary outcomes with probit and logit specifications; the average marginal treatment effects (columns 13 and 14) closely track the baseline OLS estimates. The religiosity results are also robust to broadening the outcome itself: re-estimating the effect on a wider index that adds further GSS religiosity and spirituality items yields the same conclusion (Appendix Table A2).

Moreover, we explicitly control for a range of education policies that vary at the state-by-year level. Among others, we test whether our main effects are robust to controlling for racial desegregation in schools, the banning of Ten Commandments postings in schools, or the banning of mandatory moments of silence. Results are robust throughout, see Appendix Table A9.

Because treatment timing is common across treated units, staggered-adoption biases are not a first-order concern in our setting. Nevertheless, TWFE and alternative DiD estimators may place different weights on units and post-treatment periods, especially when treatment effects are dynamic. We therefore report the estimators by Sun and Abraham

(2021) and Callaway and Sant’Anna (2021) as a robustness check to confirm that our conclusions are not specific to the aggregation scheme implicit in TWFE, see Appendix Figures A14–A18. We also show that our event study estimates are robust to the exclusion of control variables and survey year fixed effects, see Appendix Figures A19–A23.

7 Concluding Remarks

This paper studies the long-run effects of removing state-sponsored prayer and devotional Bible reading from public schools in the United States. The school-prayer decisions of the early 1960s were among the most consequential church–state rulings in American education, with controversy continuing to this day. They also provide a rare opportunity to study whether state-sponsored religious practice experienced in childhood can shape adult beliefs, family choices, labor-market behavior, and social attitudes decades later. Although school prayer was brief and often embedded in the ordinary rhythm of the school day, it was also repeated, public, and administered through one of the most important socializing institutions in society. Whether such a practice had lasting effects is therefore an empirical question.

We exploit variation generated by the Supreme Court’s rulings, which made state-sponsored devotional Bible reading and prayer unconstitutional in public schools. Before the rulings, states differed in whether devotional exercises were required, permitted, or already prohibited. The results show that removing school-sponsored prayer had persistent effects on adult religiosity. We find that exposure to the removal of school prayer changed family formation. Fully exposed cohorts are more likely to be divorced, less likely to be married, and have fewer children. We also find effects on labor-market outcomes, with higher participation for women likely driven by changes in social attitudes. Exposure reduces skepticism toward science and opposition to same-sex marriage, while having little average effect on conservative self-identification or opposition to abortion.

Our study highlights the long reach of institutional change in childhood environments. Removing school-sponsored prayer altered the religious and social development of affected cohorts, with consequences for religious participation, family formation, gender attitudes, employment, and selected social views decades later. These findings underscore the importance of schools as sites of cultural and moral transmission, and show that this value transfer can have lasting effects on social and economic outcomes. The results have broader implications for the economics of religion, education, and socialization. They show that schools transmit more than academic skills. Even practices that take only a few minutes each day can shape beliefs, norms, and behavior well into adulthood when they are repeated across childhood and embedded in public institutions. Public education policy can shape not only human capital, but also the beliefs and norms that structure adult life.

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Tables

Table 1: Descriptive Statistics by Treatment Status

	Full Sample		Control States		Treated States	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
<i>Religious Outcomes</i>						
Index of Religious Outcomes	0.616	0.284	0.576	0.295	0.627	0.280
Strong Believer	0.462	0.499	0.430	0.495	0.471	0.499
<i>Family/Lifestyle Outcomes</i>						
Currently Divorced	0.186	0.389	0.194	0.395	0.183	0.387
Currently Married	0.530	0.499	0.516	0.500	0.534	0.499
Number of Children	1.859	1.595	1.878	1.652	1.854	1.578
<i>Labour Market Outcomes</i>						
Employment	0.825	0.380	0.815	0.388	0.827	0.378
Income, Intensive Margin	9.773	0.960	9.808	0.960	9.763	0.960
Income, Extensive Margin	8.509	3.399	8.518	3.434	8.507	3.389
<i>Gender Attitude Outcomes</i>						
Gender Attitude Index	0.197	0.292	0.185	0.287	0.200	0.293
Full-time Homemaking	0.131	0.338	0.133	0.340	0.131	0.337
Against Abortion	0.199	0.399	0.179	0.384	0.204	0.403
<i>Social/Political Outcomes</i>						
Science Skeptic	0.072	0.259	0.063	0.244	0.075	0.263
Education Skeptic	0.167	0.373	0.179	0.383	0.163	0.370
Homophobia	0.678	0.467	0.614	0.487	0.696	0.460
Against Same-Sex Marriage	0.391	0.488	0.344	0.475	0.406	0.491
Conservative	0.720	0.449	0.697	0.459	0.727	0.446
Pro-Euthanasia	0.712	0.453	0.750	0.433	0.702	0.458
<i>Gender Controls</i>						
Male	0.451	0.498	0.457	0.498	0.449	0.497
Female	0.549	0.498	0.543	0.498	0.551	0.497
<i>Rural vs Urban Controls</i>						
Raised in Urban Area	0.451	0.498	0.543	0.498	0.425	0.494
Raised in Rural Area	0.549	0.498	0.457	0.498	0.575	0.494
<i>Parental Nationality Controls</i>						
Parents Born: US	0.879	0.326	0.851	0.357	0.887	0.317
Parents Born: Abroad	0.095	0.293	0.124	0.330	0.086	0.281
Parents Born: Missing	0.027	0.161	0.025	0.157	0.027	0.162
<i>Religious Upbringing Controls</i>						
Raised as Non-Religious	0.056	0.230	0.075	0.264	0.051	0.220
Raised as Protestant: Mainline	0.467	0.499	0.383	0.486	0.490	0.500
Raised as Protestant: Evangelical	0.105	0.307	0.104	0.305	0.106	0.307
Raised as Catholic	0.294	0.456	0.362	0.481	0.275	0.447
Raised as Jew	0.020	0.141	0.016	0.127	0.021	0.144
Raised as Other	0.017	0.129	0.024	0.154	0.015	0.121
Observations	38,752		8,525		30,227	

Note: This table reports descriptive statistics for outcomes and control variables for the estimation sample used in equation 1, and separately for control and treated states. Treatment status is defined by exposure to the Abington School District v. Schempp Supreme Court decision (1963). Data source: General Social Survey.

Table 2: Effect of the SCOTUS Ruling on Religious Outcomes

	Main Outcome	Religious Outcomes							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Religious Index	Strong Believer	Prayer	Church	Bible	Afterlife	God	Religious	Fundamentalist
Exposure to Ruling	-0.021*** (0.008)	-0.054*** (0.015)	-0.026 (0.019)	-0.039* (0.023)	-0.000 (0.020)	0.012 (0.009)	-0.009 (0.015)	-0.013 (0.013)	-0.015 (0.018)
Mean of Dep. Var.	0.62	0.46	0.69	0.46	0.81	0.74	0.92	0.87	0.32
Std. Dev. of Dep. Var.	0.28	0.50	0.46	0.50	0.40	0.44	0.28	0.34	0.46
Adj. R-squared	0.159	0.050	0.112	0.064	0.065	0.033	0.050	0.122	0.253
Observations	36,150	32,629	24,785	35,900	22,809	26,383	16,954	36,093	34,906

Notes: The table shows TWFE OLS coefficients and standard errors clustered at the state level in parentheses from estimating equation 1. Dependent variables: Main outcome and religious outcomes indicated in the column headers. The composite index of religious outcomes encompasses: self-identification as a ‘strong believer’; measure of prayer frequency; measure of church attendance frequency; belief in the Bible; belief in the afterlife; belief in God; indicator of religious affiliation; and self-identification as a ‘fundamentalist’. All religious outcomes are coded such that an increase in the variable implies an increase in religiosity. Treatment: continuous variable $Exposure_{st}$, representing the share of an individual’s compulsory school years that fell under the post-ruling regime for respondents residing in an affected state at age 16, and zero otherwise. Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Data source: General Social Survey. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 3: Effect of the SCOTUS Ruling on Family/Lifestyle Outcomes

	Main Outcome	Family/Lifestyle Outcomes		
	(1)	(2)	(3)	(4)
	Religious Index	Currently Divorced	Currently Married	Number of Children
Exposure to Ruling	-0.021*** (0.008)	0.033*** (0.011)	-0.041*** (0.013)	-0.136** (0.065)
Mean of Dep. Var.	0.62	0.18	0.53	1.85
Std. Dev. of Dep. Var.	0.28	0.39	0.50	1.59
Adj. R-squared	0.159	0.026	0.025	0.142
Observations	36,150	36,138	36,138	36,080

Notes: The table shows TWFE OLS coefficients and standard errors clustered at the state level in parentheses from estimating equation 1. Dependent variables: Main outcome and family/lifestyle outcomes as indicated in the column headers. Treatment: continuous variable $Exposure_{st}$, representing the share of an individual's compulsory school years that fell under the post-ruling regime for respondents residing in an affected state at age 16, and zero otherwise. Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Data source: General Social Survey. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 4: Effect of the SCOTUS Ruling on Labour Market Outcomes

	Main Outcome	Labour Market Outcomes		
	(1)	(2)	(3)	(4)
	Religious Index	Employed	Income Intensive Margin	Income Extensive Margin
Exposure to Ruling	-0.021*** (0.008)	0.050** (0.020)	0.003 (0.044)	0.528*** (0.178)
Mean of Dep. Var.	0.62	0.82	9.76	8.50
Std. Dev. of Dep. Var.	0.28	0.38	0.96	3.39
Adj. R-squared	0.159	0.066	0.191	0.116
Observations	36,150	30,174	24,809	28,486

Notes: The table shows TWFE OLS coefficients and standard errors clustered at the state level in parentheses from estimating equation 1. Dependent variables: Main outcome and labour market outcomes as indicated in the column headers. Treatment: continuous variable $\text{Exposure to Ruling}_{st}$, representing the share of an individual's compulsory school years that fell under the post-ruling regime for respondents residing in an affected state at age 16, and zero otherwise. Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Data source: General Social Survey. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5: Effect of the SCOTUS Ruling on Gender Attitude Outcomes

	Main Outcome	Gender Attitude Outcomes				
	(1)	(2)	(3)	(4)	(5)	(6)
	Religious Index	Gender Attitude Index	Full-time Homemaker	Against Abortion	Against Women in Politics	Against Working Women
Exposure to Ruling	-0.021*** (0.008)	-0.037** (0.015)	-0.042** (0.017)	-0.004 (0.012)	-0.066*** (0.021)	-0.054** (0.024)
Mean of Dep. Var.	0.62	0.20	0.13	0.20	0.21	0.30
Std. Dev. of Dep. Var.	0.28	0.29	0.34	0.40	0.41	0.46
Adj. R-squared	0.159	0.054	0.101	0.027	0.048	0.062
Observations	36,150	35,509	30,174	22,461	19,219	19,823

Notes: The table shows TWFE OLS coefficients and standard errors clustered at the state level in parentheses from estimating equation 1. Dependent variables: Main outcome and gender attitude outcomes as indicated in the column headers. The composite index of gender attitude outcomes encompasses: opinion on full-time homemaking; opposition to abortion in case of genetic conditions in the fetus; opinion on women in politics; opinion on women in the labour force. All gender attitude outcomes are coded such that an increase in the variable implies a decrease in gender-egalitarian attitudes. Treatment: continuous variable $Exposure_{it}$, representing the share of an individual's compulsory school years that fell under the post-ruling regime for respondents residing in an affected state at age 16, and zero otherwise. Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Data source: General Social Survey. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 6: Effect of the SCOTUS Ruling on Social/Political Outcomes

	Main Outcome	Social/Political Outcomes					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Religious Index	Science Skeptic	Education Skeptic	Homophobia	Against Same-Sex Marriage	Conservative	Pro-Euthanasia
Exposure to Ruling	-0.021*** (0.008)	-0.030*** (0.009)	0.022* (0.012)	-0.021 (0.019)	-0.111*** (0.039)	0.014 (0.012)	0.020 (0.017)
Mean of Dep. Var.	0.62	0.07	0.16	0.68	0.41	0.72	0.71
Std. Dev. of Dep. Var.	0.28	0.26	0.37	0.46	0.49	0.45	0.45
Adj. R-squared	0.159	0.011	0.015	0.157	0.232	0.024	0.042
Observations	36,150	23,131	23,999	21,542	3,939	32,912	20,139

Notes: The table shows TWFE OLS coefficients and standard errors clustered at the state level in parentheses from estimating equation 1. Dependent variables: Main outcome, social attitude outcomes and political outcomes as indicated in the column headers. Treatment: continuous variable $Exposure_{st}$, representing the share of an individual's compulsory school years that fell under the post-ruling regime for respondents residing in an affected state at age 16, and zero otherwise. Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Data source: General Social Survey. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Figures

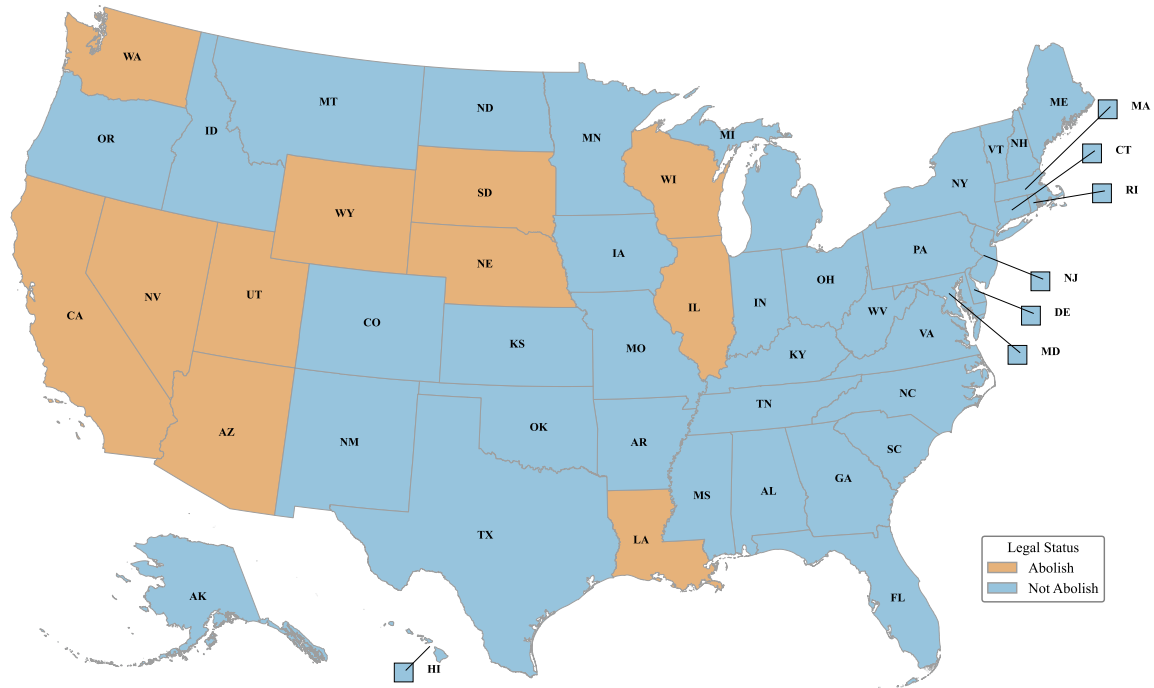


Figure 1: Map of States by Treatment Status

Note: The map depicts the legal status of Bible reading in the U.S. fifty states at the time of the *Abington School District v. Schempp* Supreme Court decision (1963). A detailed list of state treatment status and laws underlying this map is provided in Appendix Table A1. Data sources: (U.S. Department of the Interior, Office of Education, 1930), (Conway, 1956), (Doak, 1963), (Reich, 1966), (LaMorte and Dorminy, 1974).

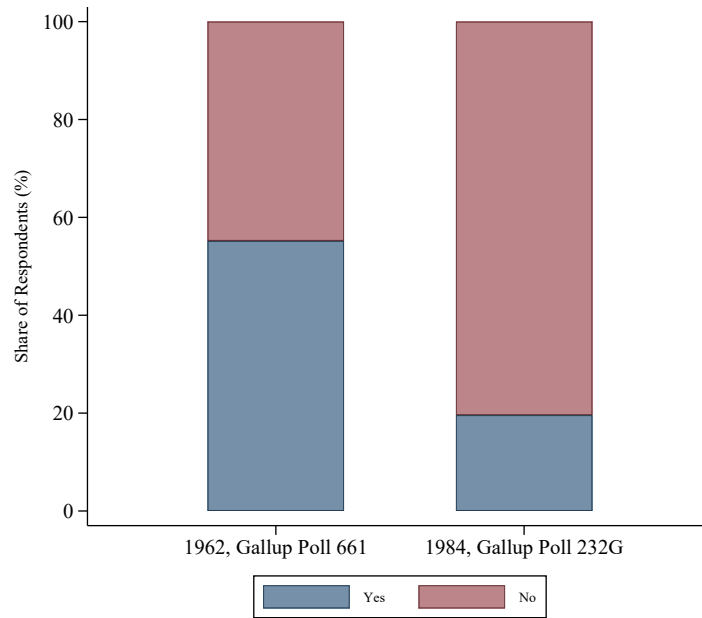


Figure 2: Reported School Prayer Before and After the SCOTUS Ruling

Note: This figure shows the share of Gallup respondents reporting that prayer or other religious observances took place in their local public schools, comparing a pre-ruling wave (1962, Gallup Poll #661, $N = 4,040$) with the closest comparable post-ruling wave (1984, Gallup Poll #232G, $N = 2,638$). The 1962 question asked “Do you happen to know if religious observances – such as prayers, Bible reading, and the like – take place in the public schools in your community?”, with response options Yes/No/Don’t Know. The 1984 question asked respondents to identify, from a card, which of several specific practices (a teacher or school official leading silent prayer or meditation, leading a prayer aloud, or reading a prayer to students) took place in their community’s public schools on a regular basis; the “Yes” category in 1984 combines these three items, and “No” corresponds to the “None of the above” response. Shares are computed among respondents providing a definite response in each wave; respondents answering “Don’t Know,” “No Answer/Not Applicable,” or with missing codes are excluded from the denominator. Data source: Gallup Brain.

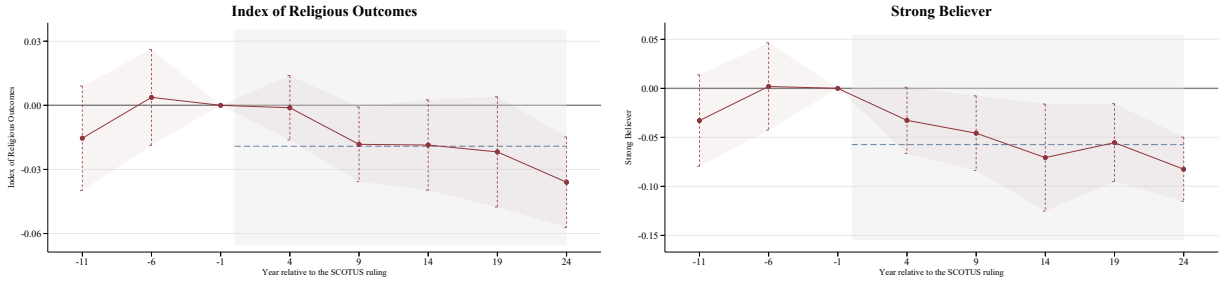


Figure 3: Event-Study Graph for the Effect of the SCOTUS Ruling on Religious Outcomes

Note: The figure shows TWFE OLS point estimates and 95% confidence intervals from estimating equation 2. The figure displays the effect of the ruling on religious outcomes as indicated in the figure headers. The composite index of religious outcomes encompasses: self-identification as a 'strong believer'; measure of prayer frequency; measure of church attendance frequency; belief in the Bible; belief in the afterlife; belief in God; indicator of religious affiliation; and self-identification as a 'fundamentalist'. All religious outcomes are coded such that an increase in the variable implies an increase in religiosity. Treatment: binary indicator for whether the respondent's state of residence at age 16 had already abolished devotional Bible reading and prayer in public schools prior to SCOTUS ruling (comparison group) or not (treated group). Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Numbers on the horizontal axis refer to the final year of respective five-year bins; that is, -1 = last five years prior to treatment. Joint p -values for the null hypothesis of zero effect for the pre-reform periods and the post-reform periods are, respectively: 0.407 and 0.019 for the index of religious outcomes; 0.381 and 0.000 for strong believer. Inference: clustering at the state level. Data source: General Social Survey.

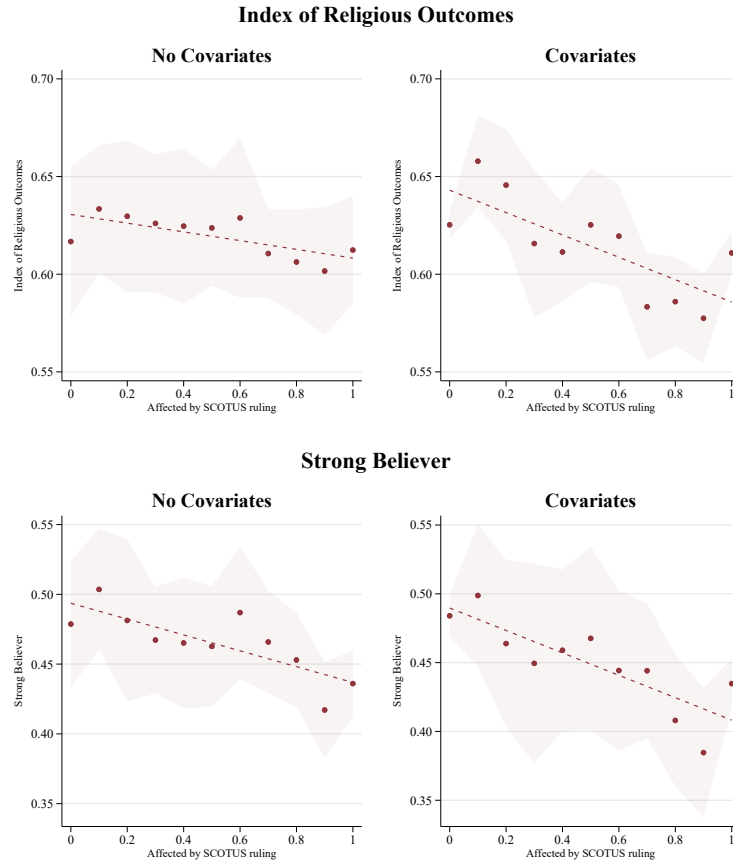


Figure 4: Bin Scatters Displaying Relationships between Reform Exposure and Religious Outcomes

Note: This figure plots TWFE OLS point estimates within exposure bins; the solid line represents the linear fit; and the shaded areas denote 95% confidence intervals. The figure displays the effect of the ruling on religious outcomes as indicated in the figure headers. The composite index of religious outcomes encompasses: self-identification as a 'strong believer'; measure of prayer frequency; measure of church attendance frequency; belief in the Bible; belief in the afterlife; belief in God; indicator of religious affiliation; and self-identification as a 'fundamentalist'. All religious outcomes are coded such that an increase in the variable implies an increase in religiosity. Treatment: continuous variable $Exposure_{st}$, representing the share of an individual's compulsory school years that fell under the post-ruling regime for respondents residing in an affected state at age 16, and zero otherwise. Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Numbers on the horizontal axis refer to the final year of respective five-year bins; that is, -1 = last five years prior to treatment. The left panel displays the unadjusted raw means with no controls; The right panel displays the fully specified model, including covariates and fixed effects. Inference: clustering at the state level. Data source: General Social Survey.

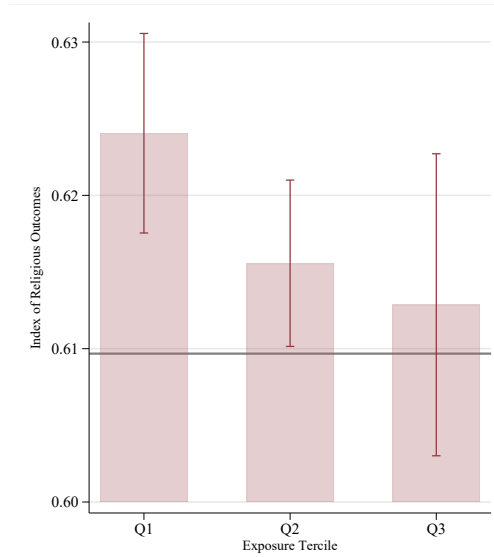


Figure 5: Terciles Displaying Relationships between Reform Exposure and Religious Outcomes

Note: This figure plots TWFE OLS predictive margins of a fully specified model across terciles of reform exposure; the bars represent the estimated means; and the error bars denote 95% confidence intervals. The figure displays the effect of the ruling on a composite index of religious outcomes, which encompasses: self-identification as a ‘strong believer’; measure of prayer frequency; measure of church attendance frequency; belief in the Bible; belief in the afterlife; belief in God; indicator of religious affiliation; and self-identification as a ‘fundamentalist’. All religious outcomes are coded such that an increase in the variable implies an increase in religiosity. Treatment: continuous variable $\text{Exposure to Ruling}_{st}$, representing the share of an individual’s compulsory school years that fell under the post-ruling regime for respondents residing in an affected state at age 16, and zero otherwise. Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Inference: clustering at the state level. Data source: General Social Survey.

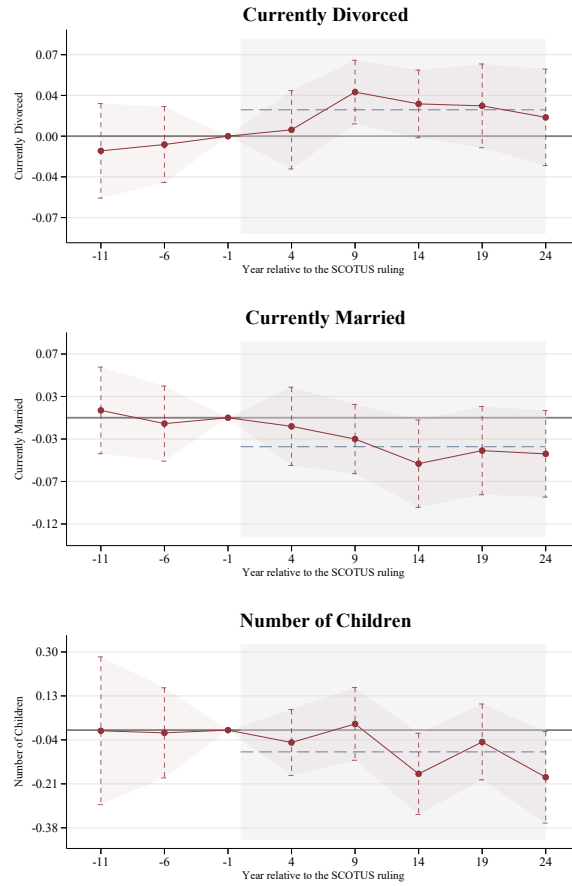


Figure 6: Event-Study Graph for the Effect of the SCOTUS Ruling on Family/Lifestyle Outcomes

Note: The figure shows TWFE OLS point estimates and 95% confidence intervals from estimating equation 2. The figure displays the effect of the ruling on family and lifestyle outcomes as indicated in the figure headers. Treatment: binary indicator for whether the respondent's state of residence at age 16 had already abolished devotional Bible reading and prayer in public schools prior to SCOTUS ruling (comparison group) or not (treated group). Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Numbers on the horizontal axis refer to the final year of respective five-year bins; that is, -1 = last five years prior to treatment. Joint p -values for the null hypothesis of zero effect for the pre-reform periods and the post-reform periods are, respectively: 0.818 and 0.037 for currently divorced; 0.878 and 0.084 for currently married; 0.993 and 0.016 for number of children. Inference: clustering at the state level. Data source: General Social Survey.

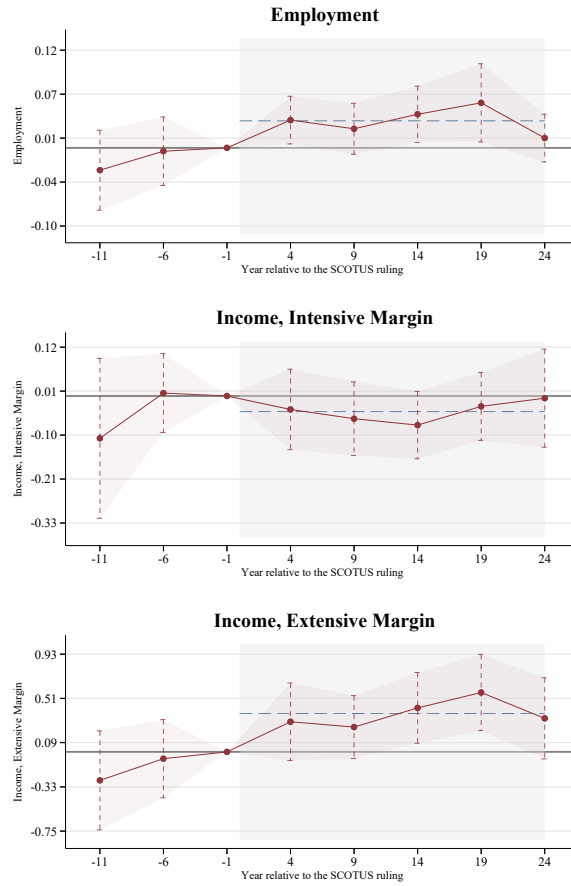


Figure 7: Event-Study Graph for the Effect of the SCOTUS Ruling on Labour Market Outcomes

Note: The figure shows TWFE OLS point estimates and 95% confidence intervals from estimating equation 2. The figure displays the effect of the ruling on labour market outcomes as indicated in the figure headers. Treatment: binary indicator for whether the respondent's state of residence at age 16 had already abolished devotional Bible reading and prayer in public schools prior to SCOTUS ruling (comparison group) or not (treated group). Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Numbers on the horizontal axis refer to the final year of respective five-year bins; that is, -1 = last five years prior to treatment. Joint p -values for the null hypothesis of zero effect for the pre-reform periods and the post-reform periods are, respectively: 0.473 and 0.076 for employment; 0.494 and 0.097 for income, intensive margin; 0.520 and 0.091 for income, extensive margin. Inference: clustering at the state level. Data source: General Social Survey.

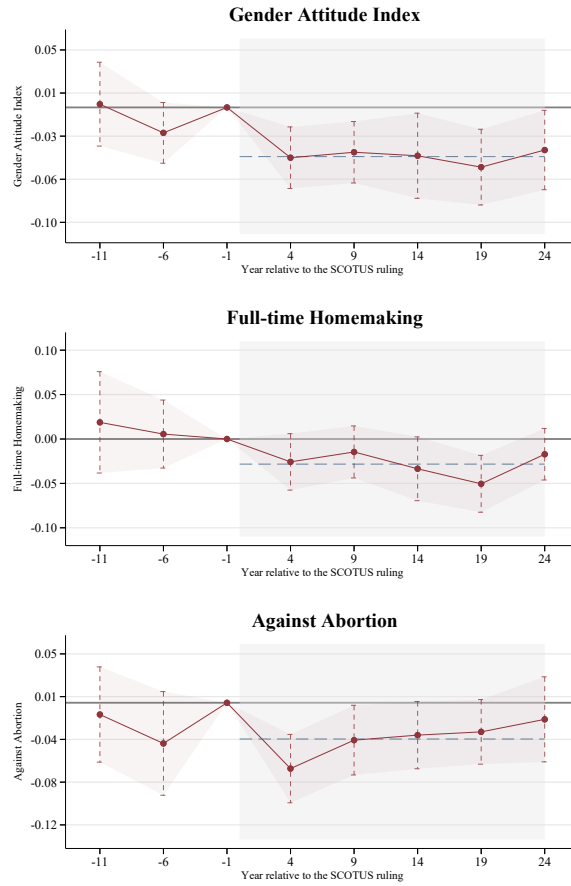


Figure 8: Event-Study Graph for the Effect of the SCOTUS Ruling on Gender Attitude Outcomes

Note: The figure shows TWFE OLS point estimates and 95% confidence intervals from estimating equation 2. The figure displays the effect of the ruling on gender attitude outcomes as indicated in the figure headers. The composite index of gender attitude outcomes encompasses: opinion on full-time homemaking; opposition to abortion in case of genetic conditions in the fetus; opinion on women in politics; opinion on women in the labour force. All gender attitude outcomes are coded such that an increase in the variable implies a decrease in gender-egalitarian attitudes. Treatment: binary indicator for whether the respondent's state of residence at age 16 had already abolished devotional Bible reading and prayer in public schools prior to SCOTUS ruling (comparison group) or not (treated group). Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Numbers on the horizontal axis refer to the final year of respective five-year bins; that is, -1 = last five years prior to treatment. Joint p -values for the null hypothesis of zero effect for the pre-reform periods and the post-reform periods are, respectively: 0.200 and 0.014 for the gender attitude index; 0.805 and 0.031 for full-time homemaking; 0.307 and 0.004 for abortion opposition in case of genetic conditions in the fetus. Inference: clustering at the state level. Data source: General Social Survey.

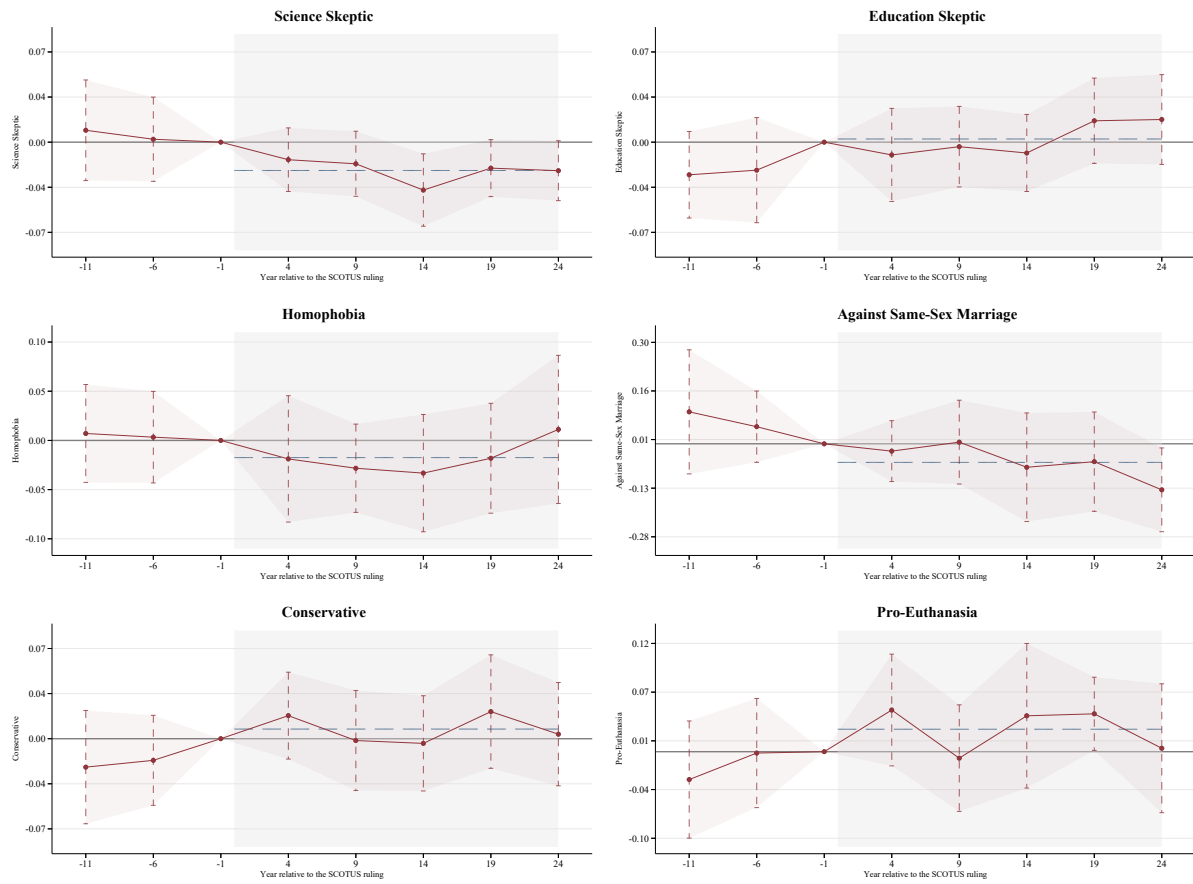


Figure 9: Event-Study Graph for the Effect of the SCOTUS Ruling on Social/Political Outcomes

Note: The figure shows TWFE OLS point estimates and 95% confidence intervals from estimating equation 2. The figure displays the effect of the ruling on social attitude and political outcomes as indicated in the figure headers. Treatment: binary indicator for whether the respondent's state of residence at age 16 had already abolished devotional Bible reading and prayer in public schools prior to SCOTUS ruling (comparison group) or not (treated group). Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Numbers on the horizontal axis refer to the final year of respective five-year bins; that is, -1 = last five years prior to treatment. Joint p -values for the null hypothesis of zero effect for the pre-reform periods and the post-reform periods are, respectively: 0.874 and 0.111 for science skeptic; 0.330 and 0.501 for education skeptic; 0.962 and 0.435 for homophobia; 0.557 and 0.001 for same-sex marriage opposition; 0.527 and 0.611 for conservative; 0.561 and 0.000 for pro-euthanasia. Inference: clustering at the state level. Data source: General Social Survey.

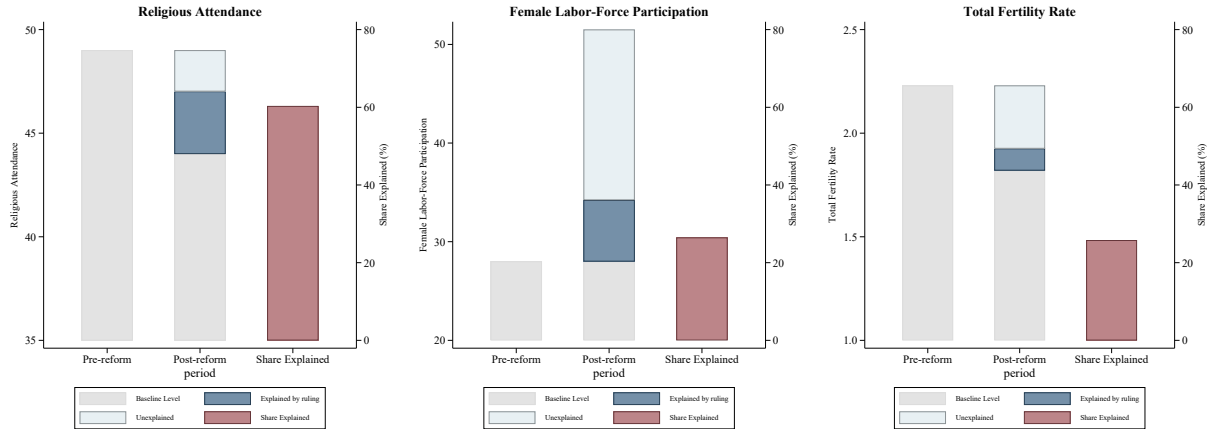


Figure 10: Counterfactual Decomposition of the Effects of the SCOTUS Ruling

Note: This figure shows a counterfactual decomposition of changes between the pre-reform and post-reform periods for religious attendance, female labour-force participation, and the total fertility rate. In each panel, the first bar reports the observed pre-reform average, while the second bar decomposes the post-reform average into the baseline (pre-reform) level, the component explained by the estimated effect of the SCOTUS ruling—TWFE OLS point estimates from estimating equation 1 multiplied by the population share of affected states—and the unexplained residual. The third bar reports the share of the observed post-reform change explained by the estimated effect of the ruling. Female labour-force participation and total fertility rate correspond to the female-subsample effects of employment and the number of children, respectively, as reported in Appendix Table A8. Treatment: continuous variable $Exposure_{st}$, representing the share of an individual's compulsory school years that fell under the post-ruling regime for respondents residing in an affected state at age 16, and zero otherwise. Absolute and relative share explained are, respectively: 3.02 and 60.31% for religious attendance; 6.22 and 26.46% for female labour-force participation; 0.11 and 25.77% for total fertility rate. Inference: clustering at the state level. Data source: General Social Survey and Gallup.

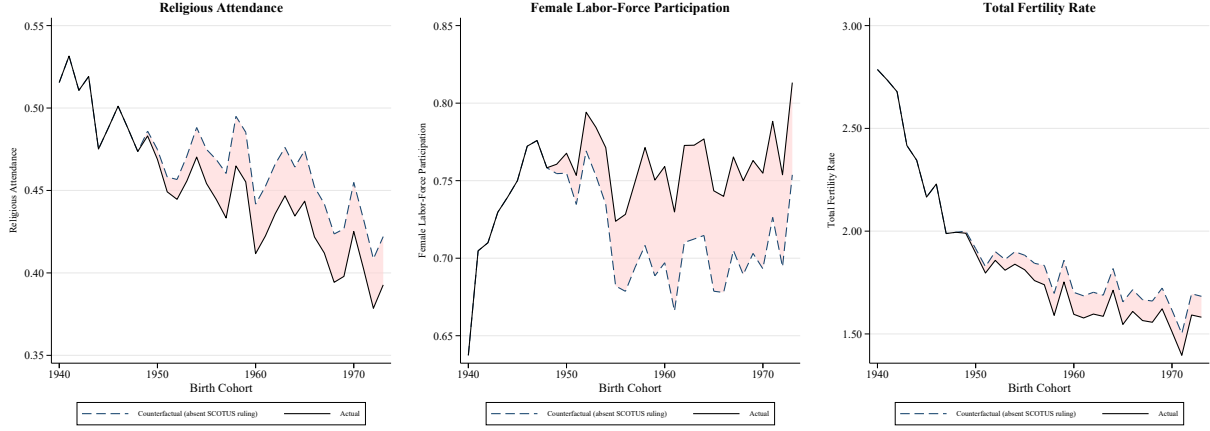


Figure 11: Observed and Counterfactual Cohort Profiles of the Effects of the SCOTUS Ruling

Note: This figure plots the observed birth-cohort mean (solid black line) against a counterfactual cohort-level trajectory absent the SCOTUS ruling (dashed navy line) for religious attendance, female labour-force participation, and completed fertility across birth cohorts. The counterfactual profile for cohort c is calculated as $Y_c^{CF} = Y_c^{obs} - \hat{\beta}_y \bar{E}_c$, where Y_c^{obs} is the observed cohort mean, $\hat{\beta}_y$ is the TWFE OLS point estimate from estimating equation 1, and $\bar{E}_c$ is the sample mean exposure. $\bar{E}_c$ is the cohort- c mean of the individual exposure variable, which is defined to equal zero for respondents residing at age 16 in states where Bible reading in public school classrooms was declared a sectarian practice forbidden by constitution prior to the ruling; $\bar{E}_c$ therefore already embeds the share of cohort c 's estimation sample residing in newly-affected states. The red shaded region between the series represents the estimated cumulative contribution of the school-prayer removal to cohort-level differences. Female labour-force participation and total fertility rate correspond to the female-subsample effects of employment and the number of children, respectively, as reported in Appendix Table A8. Treatment: continuous variable $ExposureToRuling_{st}$, representing the share of an individual's compulsory school years that fell under the post-ruling regime for respondents residing in an affected state at age 16, and zero otherwise. Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Inference: clustering at the state level. Data source: General Social Survey.

Appendix A. Additional Tables and Figures

Appendix Table A1: Legal Status of Bible Reading by State (1963)

State	Ab.	State	Ab.	State	Ab.
Alabama		Louisiana	X	Ohio	
Alaska		Maine		Oklahoma	
Arizona	X	Maryland		Oregon	
Arkansas		Massachusetts		Pennsylvania	
California	X	Michigan		Rhode Island	
Colorado		Minnesota		South Carolina	
Connecticut		Mississippi		South Dakota	X
Delaware		Missouri		Tennessee	
Florida		Montana		Texas	
Georgia		Nebraska	X	Utah	X
Hawaii		Nevada	X	Vermont	
Idaho		New Hampshire		Virginia	
Illinois	X	New Jersey		Washington	X
Indiana		New Mexico		West Virginia	
Iowa		New York		Wisconsin	X
Kansas		North Carolina		Wyoming	X
Kentucky		North Dakota			

Notes: The table depicts the legal status of Bible reading in public schools at the time of the *Abington School District v. Schempp* decision (1963). “X” denotes states that had declared Bible reading a sectarian practice forbidden by constitution prior to 1963, while empty cells indicate states where the practice was either required by statute, authorized, or where there was no clear legal provision regarding Bible reading in public school classrooms. Data sources: (U.S. Department of the Interior, Office of Education, 1930), (Conway, 1956), (Doak, 1963), (Reich, 1966), (LaMorte and Dorminy, 1974).

Appendix Table A2: Effect of the SCOTUS Ruling on Broad Religious Outcomes

	Main Outcome	Alternative Index	Other Religious Outcomes					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Religious Index	Broad Religious Index	Church Activities	Missionary	Spiritual	Religious	Proximity to God	Reborn
Exposure to Ruling	-0.021*** (0.008)	-0.020** (0.008)	-0.017 (0.021)	-0.044** (0.019)	-0.012 (0.022)	-0.038* (0.019)	-0.010 (0.029)	-0.021 (0.022)
Mean of Dep. Var.	0.62	0.59	0.25	0.44	0.70	0.60	0.82	0.40
Std. Dev. of Dep. Var.	0.28	0.28	0.43	0.50	0.46	0.49	0.38	0.49
Adj. R-squared	0.159	0.166	0.047	0.122	0.066	0.094	0.056	0.152
Observations	36,150	36,150	14,333	14,469	13,516	13,523	6,132	15,565

Notes: The table shows TWFE OLS coefficients and standard errors clustered at the state level in parentheses from estimating equation 1. Dependent variables: Main outcome and religious outcomes indicated in the column headers. The composite index of religious outcomes encompasses: self-identification as a 'strong believer'; measure of prayer frequency; measure of church attendance frequency; belief in the Bible; belief in the afterlife; belief in God; indicator of religious affiliation; self-identification as a 'fundamentalist'; measure of church activity attendance frequency; self-identification as a 'missionary'; self-identification as 'spiritual'; self-identification as 'religious'; measure of feeling of proximity to God; self-identified 'Born-Again' experience. All religious outcomes are coded such that an increase in the variable implies an increase in religiosity. Treatment: continuous variable $Exposure_{it}$, representing the share of an individual's compulsory school years that fell under the post-ruling regime for respondents residing in an affected state at age 16, and zero otherwise. Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Data source: General Social Survey. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table A3: Robustness of the Effect of the SCOTUS Ruling on Religious Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Dummy Treatment	Treatment Coding Variation 1	Treatment Coding Variation 2	Treatment Coding Variation 3	Treatment Coding Variation 4	Only Large States	Temporal Sample Restriction 1	Temporal Sample Restriction 2	Excluding Early Reform States	Staggered Reform Timing	Linear Division Specific Time Trends	Division Specific Time Trends	Probit	Logit
Panel A: Outcome: Index of Religious Outcomes														
main														
Exposure to Ruling	-0.014*	-0.021**	-0.021***	-0.019**	-0.020**	-0.028***	-0.017*	-0.018*	-0.017*	-0.017**	-0.016*	-0.016*	n/a	n/a
	(0.007)	(0.008)	(0.008)	(0.008)	(0.009)	(0.006)	(0.009)	(0.010)	(0.009)	(0.008)	(0.009)	(0.009)		
Equality test w/ base. coef. (p-value)	0.492	0.989	0.998	0.844	0.930	0.499	0.715	0.767	0.725	0.707	0.674	0.664		
Panel B: Outcome: Strong Believer														
Exposure to Ruling	-0.048***	-0.039**	-0.053***	-0.046***	-0.052***	-0.060***	-0.046**	-0.041**	-0.043***	-0.041***	-0.040**	-0.039**	-0.052***	-0.052***
	(0.014)	(0.015)	(0.015)	(0.017)	(0.015)	(0.015)	(0.019)	(0.020)	(0.013)	(0.012)	(0.016)	(0.015)	(0.015)	(0.015)
Equality test w/ base. coef. (p-value)	0.787	0.483	0.984	0.716	0.949	0.766	0.744	0.618	0.576	0.489	0.536	0.487	0.948	0.944

Notes: The table shows TWFE OLS coefficients and standard errors clustered at the state level in parentheses from estimating equation 1, for different robustness checks indicated in the column headers as follows. Column (1) continuous treatment variable $Exposure_{st}$ is replaced by a dummy indicator for whether individual's compulsory schooling fell under the post-ruling regime for respondents residing in an affected state at age 16. Column (2) sample excludes respondents residing at age 16 in states where Bible reading was authorized, or where there was no clear legal provision regarding Bible reading in public school classrooms. Column (3) sample excludes respondents residing at age 16 in states where there was no clear legal provision regarding Bible reading in public school classrooms. Column (4) continuous treatment variable $Exposure_{st}$ is coded zero for respondents residing at age 16 in states where there was no clear legal provision regarding Bible reading in public school classrooms. Column (5) continuous treatment variable $Exposure_{st}$ is coded as the share of an individual's compulsory school years that fell under the post-ruling regime for respondents residing at age 16 in states where Bible reading in public school classrooms was declared a sectarian practice forbidden by constitution prior to 1963. Column (6) sample only includes individuals residing at age 16 in the 20 states with the largest population. Column (7) sample only includes individuals whose last year of schooling was within 25 year of the SCOTUS Ruling. Column (8) sample only includes individuals whose last year of schooling was within 30 year of the SCOTUS Ruling. Column (9) sample excludes respondents residing at age 16 in states that abolished Bible reading between the 1920s and 1950s, rather than coding them as unexposed controls. Column (10) continuous treatment variable $Exposure_{st}$ incorporates state-specific pre-SCOTUS reform timing into a staggered exposure design for states where Bible reading in public school classrooms was declared a sectarian practice forbidden by constitution between the 1950s and the SCOTUS ruling. Column (11) regressions include division-specific linear time trends. Column (12) regressions include division-specific linear and quadratic time trends. Column (13) coefficient reports the average marginal treatment effect of a probit specification. Column (14) coefficient reports the average marginal treatment effect of a logit specification. Dependent variables: Outcomes indicated in the panel headers. The composite index of religious outcomes encompasses: self-identification as a 'strong believer'; measure of prayer frequency; measure of church attendance frequency; belief in the Bible; belief in the afterlife; belief in God; indicator of religious affiliation; and self-identification as a 'fundamentalist'. All religious outcomes are coded such that an increase in the variable implies an increase in religiosity. Treatment: continuous variable $Exposure_{st}$, representing the share of an individual's compulsory school years that fell under the post-ruling regime for respondents residing in an affected state at age 16, and zero otherwise. Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Data source: General Social Survey. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table A4: Robustness of the Effect of the SCOTUS Ruling on Family/Lifestyle Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Dummy Treatment	Treatment Coding Variation 1	Treatment Coding Variation 2	Treatment Coding Variation 3	Treatment Coding Variation 4	Only Large States	Temporal Sample Restriction 1	Temporal Sample Restriction 2	Excluding Early Reform States	Staggered Reform Timing	Linear Division Specific Time Trends	Division Specific Time Trends	Probit	Logit
Panel A: Outcome: Currently Divorced														
Exposure to Ruling	0.029*** (0.010)	0.039*** (0.012)	0.034*** (0.010)	0.033*** (0.010)	0.034*** (0.010)	0.043*** (0.008)	0.029** (0.011)	0.026** (0.011)	0.034*** (0.011)	0.026** (0.011)	0.024 (0.014)	0.024 (0.014)	0.030*** (0.010)	0.030*** (0.010)
Equality test w/ base. coef. (p-value)	0.820	0.718	0.920	0.995	0.948	0.436	0.780	0.641	0.960	0.647	0.626	0.616	0.867	0.858
Panel B: Outcome: Currently Married														
Exposure to Ruling	-0.033** (0.014)	-0.016 (0.019)	-0.041*** (0.013)	-0.032** (0.016)	-0.036** (0.015)	-0.047*** (0.016)	-0.030** (0.014)	-0.030** (0.012)	-0.026* (0.014)	-0.033*** (0.012)	-0.032** (0.013)	-0.031** (0.012)	-0.041*** (0.014)	-0.041*** (0.014)
Equality test w/ base. coef. (p-value)	0.665	0.292	0.981	0.673	0.794	0.766	0.568	0.532	0.451	0.632	0.614	0.583	0.996	0.995
Panel C: Outcome: Number of Children main														
Exposure to Ruling	-0.075* (0.043)	-0.024 (0.077)	-0.141** (0.065)	-0.146** (0.062)	-0.175** (0.072)	-0.176** (0.079)	-0.113 (0.075)	-0.110 (0.075)	-0.092 (0.074)	-0.157* (0.092)	-0.192*** (0.071)	-0.196** (0.073)	n/a	n/a
Equality test w/ base. coef. (p-value)	0.434	0.265	0.959	0.912	0.692	0.701	0.814	0.789	0.654	0.852	0.563	0.541		

Notes: The table shows TWFE OLS coefficients and standard errors clustered at the state level in parentheses from estimating equation 1, for different robustness checks indicated in the column headers as follows. Column (1) continuous treatment variable $Exposure_{st}$ is replaced by a dummy indicator for whether individual's compulsory schooling fell under the post-ruling regime for respondents residing in an affected state at age 16. Column (2) sample excludes respondents residing at age 16 in states where Bible reading was authorized, or where there was no clear legal provision regarding Bible reading in public school classrooms. Column (3) sample excludes respondents residing at age 16 in states where there was no clear legal provision regarding Bible reading in public school classrooms. Column (4) continuous treatment variable $Exposure_{st}$ is coded zero for respondents residing at age 16 in states where there was no clear legal provision regarding Bible reading in public school classrooms. Column (5) continuous treatment variable $Exposure_{st}$ is coded as the share of an individual's compulsory school years that fell under the post-ruling regime for respondents residing at age 16 in states where Bible reading in public school classrooms was declared a sectarian practice forbidden by constitution prior to 1963. Column (6) sample only includes individuals residing at age 16 in the 20 states with the largest population. Column (7) sample only includes individuals whose last year of schooling was within 25 year of the SCOTUS Ruling. Column (8) sample only includes individuals whose last year of schooling was within 30 year of the SCOTUS Ruling. Column (9) sample excludes respondents residing at age 16 in states that abolished Bible reading between the 1920s and 1950s, rather than coding them as unexposed controls. Column (10) continuous treatment variable $Exposure_{st}$ incorporates state-specific pre-SCOTUS reform timing into a staggered exposure design for states where Bible reading in public school classrooms was declared a sectarian practice forbidden by constitution between the 1950s and the SCOTUS ruling. Column (11) regressions include division-specific linear time trends. Column (12) regressions include division-specific linear and quadratic time trends. Column (13) coefficient reports the average marginal treatment effect of a probit specification. Column (14) coefficient reports the average marginal treatment effect of a logit specification. Dependent variables: Outcomes indicated in the panel headers. Treatment: continuous variable $Exposure_{st}$, representing the share of an individual's compulsory school years that fell under the post-ruling regime for respondents residing in an affected state at age 16, and zero otherwise. Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Data source: General Social Survey. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table A5: Robustness of the Effect of the SCOTUS Ruling on Labour Market Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Dummy Treatment	Treatment Coding Variation 1	Treatment Coding Variation 2	Treatment Coding Variation 3	Treatment Coding Variation 4	Only Large States	Temporal Sample Restriction 1	Temporal Sample Restriction 2	Excluding Early Reform States	Staggered Reform Timing	Linear Division Specific Time Trends	Division Specific Time Trends	Probit	Logit
Panel A: Outcome: Employment														
Exposure to Ruling	0.042*** (0.015)	0.042* (0.021)	0.050** (0.019)	0.050*** (0.018)	0.064*** (0.019)	0.058*** (0.017)	0.051*** (0.016)	0.046*** (0.017)	0.046 (0.029)	0.044* (0.023)	0.052** (0.026)	0.050* (0.026)	0.048*** (0.018)	0.050*** (0.018)
Equality test w/ base. coef. (p-value)	0.740	0.785	0.995	0.987	0.621	0.769	0.979	0.876	0.907	0.845	0.950	0.998	0.936	0.992
Panel B: Outcome: Income, Intensive Margin main														
Exposure to Ruling	-0.023 (0.037)	0.033 (0.046)	0.003 (0.044)	0.002 (0.042)	0.024 (0.044)	0.056 (0.044)	0.023 (0.046)	0.035 (0.051)	-0.002 (0.064)	-0.005 (0.050)	-0.051 (0.050)	-0.049 (0.051)	n/a	n/a
Equality test w/ base. coef. (p-value)	0.654	0.636	0.996	0.982	0.743	0.395	0.752	0.635	0.951	0.903	0.422	0.444		
Panel C: Outcome: Income, Extensive Margin main														
Exposure to Ruling	0.432** (0.167)	0.524** (0.196)	0.530*** (0.177)	0.536*** (0.170)	0.677*** (0.177)	0.633*** (0.160)	0.531*** (0.165)	0.508*** (0.163)	0.536* (0.276)	0.460** (0.205)	0.517** (0.236)	0.502** (0.236)	n/a	n/a
Equality test w/ base. coef. (p-value)	0.693	0.988	0.995	0.974	0.554	0.661	0.988	0.934	0.981	0.801	0.970	0.929		

Notes: The table shows TWFE OLS coefficients and standard errors clustered at the state level in parentheses from estimating equation 1, for different robustness checks indicated in the column headers as follows. Column (1) continuous treatment variable $Exposure_{it}$ is replaced by a dummy indicator for whether individual's compulsory schooling fell under the post-ruling regime for respondents residing in an affected state at age 16. Column (2) sample excludes respondents residing at age 16 in states where Bible reading was authorized, or where there was no clear legal provision regarding Bible reading in public school classrooms. Column (3) sample excludes respondents residing at age 16 in states where there was no clear legal provision regarding Bible reading in public school classrooms. Column (4) continuous treatment variable $Exposure_{it}$ is coded zero for respondents residing at age 16 in states where there was no clear legal provision regarding Bible reading in public school classrooms. Column (5) continuous treatment variable $Exposure_{it}$ is coded as the share of an individual's compulsory school years that fell under the post-ruling regime for respondents residing at age 16 in states where Bible reading in public school classrooms was declared a sectarian practice forbidden by constitution prior to 1963. Column (6) sample only includes individuals residing at age 16 in the 20 states with the largest population. Column (7) sample only includes individuals whose last year of schooling was within 25 year of the SCOTUS Ruling. Column (8) sample only includes individuals whose last year of schooling was within 30 year of the SCOTUS Ruling. Column (9) sample excludes respondents residing at age 16 in states that abolished Bible reading between the 1920s and 1950s, rather than coding them as unexposed controls. Column (10) continuous treatment variable $Exposure_{it}$ incorporates state-specific pre-SCOTUS reform timing into a staggered exposure design for states where Bible reading in public school classrooms was declared a sectarian practice forbidden by constitution between the 1950s and the SCOTUS ruling. Column (11) regressions include division-specific linear time trends. Column (12) regressions include division-specific linear and quadratic time trends. Column (13) coefficient reports the average marginal treatment effect of a probit specification. Column (14) coefficient reports the average marginal treatment effect of a logit specification. Dependent variables: Outcomes indicated in the panel headers. Treatment: continuous variable $Exposure_{it}$, representing the share of an individual's compulsory school years that fell under the post-ruling regime for respondents residing in an affected state at age 16, and zero otherwise. Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Data source: General Social Survey. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table A6: Robustness of the Effect of the SCOTUS Ruling on Gender Attitude Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Dummy Treatment	Treatment Coding Variation 1	Treatment Coding Variation 2	Treatment Coding Variation 3	Treatment Coding Variation 4	Only Large States	Temporal Sample Restriction 1	Temporal Sample Restriction 2	Excluding Early Reform States	Staggered Reform Timing	Linear Division Specific Time Trends	Division Specific Time Trends	Probit	Logit
Panel A: Outcome: Gender Attitude Index														
main														
Exposure to Ruling	-0.037** (0.015)	-0.040** (0.015)	-0.037** (0.015)	-0.036** (0.014)	-0.043*** (0.015)	-0.052*** (0.015)	-0.035** (0.014)	-0.030** (0.014)	-0.034 (0.021)	-0.030* (0.016)	-0.023 (0.018)	-0.022 (0.018)	n/a	n/a
Equality test w/ base. coef. (p-value)	0.996	0.860	0.992	0.995	0.757	0.456	0.931	0.738	0.908	0.768	0.551	0.530		
Panel B: Outcome: Full-time Homemaking														
Exposure to Ruling	-0.033*** (0.012)	-0.038* (0.019)	-0.042** (0.017)	-0.043** (0.016)	-0.053*** (0.017)	-0.051*** (0.016)	-0.043*** (0.014)	-0.036** (0.014)	-0.040 (0.025)	-0.038* (0.019)	-0.039* (0.022)	-0.037* (0.022)	-0.040** (0.016)	-0.042** (0.016)
Equality test w/ base. coef. (p-value)	0.665	0.851	0.999	0.980	0.672	0.697	0.993	0.789	0.950	0.878	0.891	0.837	0.930	0.991
Panel C: Outcome: Against Abortion														
Exposure to Ruling	-0.025** (0.012)	0.005 (0.017)	-0.003 (0.012)	-0.001 (0.012)	-0.008 (0.011)	-0.009 (0.015)	-0.011 (0.012)	-0.008 (0.011)	-0.010 (0.015)	-0.012 (0.012)	-0.010 (0.014)	-0.009 (0.014)	-0.006 (0.012)	-0.005 (0.012)
Equality test w/ base. coef. (p-value)	0.208	0.671	0.954	0.857	0.777	0.779	0.682	0.806	0.745	0.617	0.743	0.745	0.899	0.941

Notes: The table shows TWFE OLS coefficients and standard errors clustered at the state level in parentheses from estimating equation 1, for different robustness checks indicated in the column headers as follows. Column (1) continuous treatment variable $Exposure_{st}$ is replaced by a dummy indicator for whether individual's compulsory schooling fell under the post-ruling regime for respondents residing in an affected state at age 16. Column (2) sample excludes respondents residing at age 16 in states where Bible reading was authorized, or where there was no clear legal provision regarding Bible reading in public school classrooms. Column (3) sample excludes respondents residing at age 16 in states where there was no clear legal provision regarding Bible reading in public school classrooms. Column (4) continuous treatment variable $Exposure_{st}$ is coded zero for respondents residing at age 16 in states where there was no clear legal provision regarding Bible reading in public school classrooms. Column (5) continuous treatment variable $Exposure_{st}$ is coded as the share of an individual's compulsory school years that fell under the post-ruling regime for respondents residing at age 16 in states where Bible reading in public school classrooms was declared a sectarian practice forbidden by constitution prior to 1963. Column (6) sample only includes individuals residing at age 16 in the 20 states with the largest population. Column (7) sample only includes individuals whose last year of schooling was within 25 year of the SCOTUS Ruling. Column (8) sample only includes individuals whose last year of schooling was within 30 year of the SCOTUS Ruling. Column (9) sample excludes respondents residing at age 16 in states that abolished Bible reading between the 1920s and 1950s, rather than coding them as unexposed controls. Column (10) continuous treatment variable $Exposure_{st}$ incorporates state-specific pre-SCOTUS reform timing into a staggered exposure design for states where Bible reading in public school classrooms was declared a sectarian practice forbidden by constitution between the 1950s and the SCOTUS ruling. Column (11) regressions include division-specific linear time trends. Column (12) regressions include division-specific linear and quadratic time trends. Column (13) coefficient reports the average marginal treatment effect of a probit specification. Column (14) coefficient reports the average marginal treatment effect of a logit specification. Dependent variables: Outcomes indicated in the panel headers. The composite index of gender attitude outcomes encompasses: opinion on full-time homemaking; opposition to abortion in case of genetic conditions in the fetus; opinion on women in politics; opinion on women in the labour force. All gender attitude outcomes are coded such that an increase in the variable implies a decrease in gender-egalitarian attitudes. Treatment: continuous variable $Exposure_{st}$, representing the share of an individual's compulsory school years that fell under the post-ruling regime for respondents residing in an affected state at age 16, and zero otherwise. Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Data source: General Social Survey. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table A7: Robustness of the Effect of the SCOTUS Ruling on Political/Social Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Dummy Treatment	Treatment Coding Variation 1	Treatment Coding Variation 2	Treatment Coding Variation 3	Treatment Coding Variation 4	Only Large States	Temporal Sample Restriction 1	Temporal Sample Restriction 2	Excluding Early Reform States	Staggered Reform Timing	Linear Division Specific Time Trends	Division Specific Time Trends	Probit	Logit
Panel A: Outcome: Science Skeptic														
Exposure to Ruling	-0.026*** (0.009)	-0.032*** (0.011)	-0.029*** (0.009)	-0.025*** (0.008)	-0.032*** (0.010)	-0.025** (0.010)	-0.030*** (0.010)	-0.023*** (0.007)	-0.039*** (0.011)	-0.035*** (0.008)	-0.038*** (0.011)	-0.038*** (0.011)	-0.032*** (0.009)	-0.034*** (0.009)
Equality test w/ base. coef. (p-value)	0.769	0.856	0.915	0.710	0.897	0.686	0.995	0.566	0.504	0.681	0.564	0.556	0.891	0.762
Panel B: Outcome: Education Skeptic														
Exposure to Ruling	0.013 (0.010)	0.021 (0.014)	0.023* (0.012)	0.024** (0.011)	0.023* (0.011)	0.022 (0.015)	0.021 (0.013)	0.021 (0.013)	0.015 (0.016)	0.019 (0.013)	0.007 (0.012)	0.007 (0.012)	0.022** (0.011)	0.022** (0.011)
Equality test w/ base. coef. (p-value)	0.570	0.950	0.984	0.904	0.980	0.991	0.937	0.925	0.723	0.871	0.339	0.363	0.993	0.997
Panel C: Outcome: Homophobia														
Exposure to Ruling	-0.021 (0.026)	-0.004 (0.023)	-0.020 (0.019)	-0.015 (0.019)	-0.026 (0.019)	-0.030 (0.018)	-0.013 (0.020)	-0.012 (0.022)	-0.014 (0.025)	-0.006 (0.020)	-0.034** (0.016)	-0.034** (0.016)	-0.034* (0.018)	-0.037** (0.019)
Equality test w/ base. coef. (p-value)	0.995	0.570	0.965	0.811	0.865	0.744	0.757	0.753	0.809	0.571	0.600	0.598	0.619	0.559
Panel D: Outcome: Against Same-Sex Marriage														
Exposure to Ruling	-0.083* (0.042)	-0.100** (0.040)	-0.109*** (0.039)	-0.096** (0.041)	-0.114*** (0.040)	-0.149*** (0.028)	-0.108*** (0.035)	-0.111*** (0.029)	-0.073 (0.055)	-0.094** (0.043)	-0.088* (0.048)	-0.091* (0.048)	-0.109*** (0.038)	-0.109*** (0.038)
Equality test w/ base. coef. (p-value)	0.634	0.846	0.973	0.801	0.957	0.417	0.966	0.993	0.577	0.783	0.720	0.756	0.982	0.977
Panel E: Outcome: Conservative														
Exposure to Ruling	0.019 (0.012)	0.032** (0.015)	0.015 (0.013)	0.017 (0.013)	0.016 (0.012)	0.004 (0.011)	0.010 (0.013)	0.008 (0.012)	0.015 (0.014)	0.017 (0.014)	0.003 (0.014)	0.002 (0.014)	0.013 (0.013)	0.012 (0.013)
Equality test w/ base. coef. (p-value)	0.787	0.345	0.947	0.857	0.907	0.565	0.846	0.735	0.970	0.883	0.565	0.527	0.966	0.936
Panel F: Outcome: Pro-Euthanasia														
Exposure to Ruling	0.035 (0.022)	0.012 (0.023)	0.019 (0.017)	0.016 (0.017)	0.024 (0.019)	0.019 (0.013)	0.025 (0.017)	0.028 (0.019)	0.012 (0.013)	0.003 (0.013)	-0.004 (0.017)	-0.005 (0.017)	0.014 (0.016)	0.015 (0.016)
Equality test w/ base. coef. (p-value)	0.585	0.787	0.970	0.868	0.849	0.978	0.833	0.740	0.731	0.449	0.312	0.308	0.815	0.837

Appendix Table A7 - Continued

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Dummy Treatment	Treatment Coding Variation 1	Treatment Coding Variation 2	Treatment Coding Variation 3	Treatment Coding Variation 4	Only Large States	Temporal Sample Restriction 1	Temporal Sample Restriction 2	Excluding Early Reform States	Staggered Reform Timing	Linear Division Specific Time Trends	Division Specific Time Trends	Probit	Logit

Notes: The table shows TWFE OLS coefficients and standard errors clustered at the state level in parentheses from estimating equation 1, for different robustness checks indicated in the column headers as follows. Column (1) continuous treatment variable $Exposure_{st}$ is replaced by a dummy indicator for whether individual's compulsory schooling fell under the post-ruling regime for respondents residing in an affected state at age 16. Column (2) sample excludes respondents residing at age 16 in states where Bible reading was authorized, or where there was no clear legal provision regarding Bible reading in public school classrooms. Column (3) sample excludes respondents residing at age 16 in states where there was no clear legal provision regarding Bible reading in public school classrooms. Column (4) continuous treatment variable $Exposure_{st}$ is coded zero for respondents residing at age 16 in states where there was no clear legal provision regarding Bible reading in public school classrooms. Column (5) continuous treatment variable $Exposure_{st}$ is coded as the share of an individual's compulsory school years that fell under the post-ruling regime for respondents residing at age 16 in states where Bible reading in public school classrooms was declared a sectarian practice forbidden by constitution prior to 1963. Column (6) sample only includes individuals residing at age 16 in the 20 states with the largest population. Column (7) sample only includes individuals whose last year of schooling was within 25 year of the SCOTUS Ruling. Column (8) sample only includes individuals whose last year of schooling was within 30 year of the SCOTUS Ruling. Column (9) sample excludes respondents residing at age 16 in states that abolished Bible reading between the 1920s and 1950s, rather than coding them as unexposed controls. Column (10) continuous treatment variable $Exposure_{st}$ incorporates state-specific pre-SCOTUS reform timing into a staggered exposure design for states where Bible reading in public school classrooms was declared a sectarian practice forbidden by constitution between the 1950s and the SCOTUS ruling. Column (11) regressions include division-specific linear time trends. Column (12) regressions include division-specific linear and quadratic time trends. Column (13) coefficient reports the average marginal treatment effect of a probit specification. Column (14) coefficient reports the average marginal treatment effect of a logit specification. Dependent variables: Outcomes indicated in the panel headers. Treatment: continuous variable $Exposure_{st}$, representing the share of an individual's compulsory school years that fell under the post-ruling regime for respondents residing in an affected state at age 16, and zero otherwise. Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Data source: General Social Survey. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table A8: Effect of the SCOTUS Ruling, by Subgroups

	By Gender		By Urban-Rural Divide		By Parental Nationality		By Religious Upbringing			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Male	Female	Urban	Rural	U.S.	Abroad	None	Catholic	Mainline Protestants	Evangelical
Panel A: Religious Outcomes										
Index of Religious Outcomes	-0.026**	-0.018**	-0.029***	-0.008	-0.019**	-0.028	-0.026	-0.023	-0.038***	0.026
	(0.012)	(0.009)	(0.010)	(0.007)	(0.007)	(0.020)	(0.026)	(0.016)	(0.013)	(0.020)
	16,262	19,888	16,268	19,882	32,590	3,467	2,036	10,635	18,064	4,073
Strong Believer	-0.030	-0.072***	-0.056**	-0.046**	-0.049***	-0.085**	-0.036	-0.056*	-0.064**	-0.079**
	(0.020)	(0.021)	(0.022)	(0.020)	(0.015)	(0.035)	(0.066)	(0.031)	(0.028)	(0.036)
	14,643	17,986	14,501	18,128	29,440	3,106	1,756	9,588	16,365	3,776
Panel B: Family/Lifestyle Outcomes										
Currently Divorced	0.036**	0.033**	0.030**	0.022	0.027**	0.076***	-0.134**	0.045***	0.037*	0.070**
	(0.014)	(0.015)	(0.012)	(0.015)	(0.010)	(0.026)	(0.053)	(0.014)	(0.019)	(0.032)
	16,257	19,881	16,259	19,879	32,580	3,465	2,036	10,632	18,058	4,073
Currently Married	-0.055**	-0.033*	-0.035	-0.043***	-0.034**	-0.100***	0.041	-0.062**	-0.048***	0.016
	(0.026)	(0.017)	(0.027)	(0.014)	(0.014)	(0.029)	(0.041)	(0.028)	(0.015)	(0.035)
	16,257	19,881	16,259	19,879	32,580	3,465	2,036	10,632	18,058	4,073
Number of Children	-0.140	-0.138*	-0.095	-0.158**	-0.131**	-0.116	-0.190	-0.214*	-0.246***	0.327
	(0.093)	(0.075)	(0.083)	(0.074)	(0.056)	(0.227)	(0.150)	(0.122)	(0.070)	(0.223)
	16,222	19,858	16,231	19,849	32,528	3,461	2,026	10,614	18,035	4,067
Panel C: Labour Market Outcomes										
Employment	0.011	0.081***	0.039***	0.064**	0.051**	0.039	0.029	0.059**	0.036	0.038
	(0.009)	(0.030)	(0.014)	(0.031)	(0.021)	(0.040)	(0.052)	(0.028)	(0.023)	(0.036)
	13,388	16,786	13,663	16,511	27,258	2,839	1,700	8,953	15,034	3,382
Income, Intensive Margin	-0.043	0.045	0.023	-0.029	-0.015	0.130*	-0.013	0.025	0.001	0.022
	(0.047)	(0.049)	(0.043)	(0.072)	(0.051)	(0.070)	(0.146)	(0.058)	(0.054)	(0.102)
	12,271	12,538	11,242	13,567	22,439	2,319	1,321	7,456	12,386	2,736
Income, Extensive Margin	0.117	0.832***	0.462***	0.580**	0.525***	0.451	0.943**	0.492*	0.382**	0.683*
	(0.096)	(0.268)	(0.157)	(0.261)	(0.185)	(0.408)	(0.408)	(0.272)	(0.177)	(0.372)
	12,697	15,789	12,843	15,643	25,753	2,669	1,561	8,476	14,203	3,219
Panel D: Gender Attitude Outcomes										

Appendix Table A8 - Continued

	By Gender		By Urban-Rural Divide		By Parental Nationality		By Religious Upbringing			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Male	Female	Urban	Rural	U.S.	Abroad	None	Catholic	Mainline Protestants	Evangelical
Gender Attitude Index	-0.017 (0.014)	-0.054*** (0.018)	-0.026* (0.014)	-0.045** (0.019)	-0.033** (0.015)	-0.055** (0.027)	-0.036 (0.033)	-0.039 (0.024)	-0.041** (0.016)	-0.043 (0.026)
	15,968	19,541	16,000	19,509	32,006	3,412	2,005	10,459	17,736	3,994
Full-time Homemaking	-0.003 (0.008)	-0.075*** (0.027)	-0.035*** (0.013)	-0.050* (0.027)	-0.041** (0.017)	-0.059 (0.037)	-0.076* (0.043)	-0.041 (0.026)	-0.037* (0.019)	-0.029 (0.030)
	13,388	16,786	13,663	16,511	27,258	2,839	1,700	8,953	15,034	3,382
Against Abortion	0.003 (0.014)	-0.010 (0.019)	0.009 (0.017)	-0.010 (0.017)	0.003 (0.012)	-0.054 (0.037)	-0.064 (0.045)	-0.031 (0.021)	-0.010 (0.016)	0.034 (0.048)
	10,076	12,385	9,986	12,475	20,297	2,115	1,174	6,566	11,333	2,570
Panel E: Social/Political Outcomes										
Science Skeptic	-0.033** (0.014)	-0.028*** (0.010)	-0.019 (0.014)	-0.038*** (0.010)	-0.030*** (0.008)	-0.038 (0.034)	-0.019 (0.041)	-0.023** (0.011)	-0.029** (0.012)	-0.039 (0.035)
	10,558	12,573	10,455	12,676	20,869	2,204	1,237	6,846	11,655	2,579
Education Skeptic	0.017 (0.015)	0.026 (0.017)	0.041*** (0.012)	0.000 (0.016)	0.020 (0.015)	0.011 (0.038)	-0.034 (0.054)	0.013 (0.014)	0.020 (0.013)	0.062* (0.037)
	10,828	13,171	10,821	13,178	21,640	2,299	1,301	7,037	12,137	2,684
Homophobia	-0.034 (0.024)	-0.010 (0.025)	-0.038 (0.023)	-0.001 (0.021)	-0.014 (0.019)	-0.051 (0.063)	-0.102 (0.068)	-0.033 (0.027)	-0.016 (0.025)	-0.003 (0.042)
	9,836	11,706	9,587	11,955	19,515	1,980	1,191	6,243	10,811	2,492
Against Same-Sex Marriage	-0.164*** (0.053)	-0.058 (0.052)	-0.097** (0.037)	-0.110* (0.055)	-0.109*** (0.037)	-0.034 (0.128)	-0.204* (0.114)	-0.050 (0.063)	-0.128* (0.069)	-0.096 (0.164)
	1,804	2,135	1,849	2,090	3,545	386	283	1,228	1,876	373
Conservative	0.003 (0.018)	0.022 (0.018)	0.017 (0.013)	0.011 (0.014)	0.013 (0.012)	0.009 (0.041)	0.015 (0.046)	-0.012 (0.033)	0.032* (0.016)	0.048 (0.040)
	14,978	17,934	14,860	18,052	29,714	3,119	1,795	9,745	16,487	3,667
Pro-Euthanasia	-0.014 (0.019)	0.043 (0.027)	0.036** (0.015)	0.002 (0.024)	0.015 (0.019)	0.016 (0.036)	-0.061 (0.050)	0.057** (0.024)	0.033 (0.037)	-0.006 (0.036)
	9,065	11,074	9,018	11,121	18,199	1,895	1,135	6,001	10,041	2,244

Appendix Table A8 - Continued

By Gender		By Urban-Rural Divide		By Parental Nationality		By Religious Upbringing			
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Male	Female	Urban	Rural	U.S.	Abroad	None	Catholic	Mainline Protestants	Evangelical

Notes: The table shows TWFE OLS coefficients and standard errors clustered at the state level in parentheses from estimating equation 1, by selected subgroups, as indicated in the column headers. Each entry is from a separate regression model. The outcome is labeled above each panel. The composite index of religious outcomes encompasses: self-identification as a 'strong believer'; measure of prayer frequency; measure of church attendance frequency; belief in the Bible; belief in the afterlife; belief in God; indicator of religious affiliation; and self-identification as a 'fundamentalist'. All religious outcomes are coded such that an increase in the variable implies an increase in religiosity. The composite index of gender attitude outcomes encompasses: opinion on full-time homemaking; opposition to abortion in case of genetic conditions in the fetus; opinion on women in politics; opinion on women in the labour force. All gender attitude outcomes are coded such that an increase in the variable implies a decrease in gender-egalitarian attitudes. Treatment: continuous variable $Exposure_{st}$, representing the share of an individual's compulsory school years that fell under the post-ruling regime for respondents residing in an affected state at age 16, and zero otherwise. Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Standard errors are in parentheses followed by observation count. Data source: General Social Survey. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table A9: Effect of the SCOTUS Ruling, controls for other school reforms

	(1) Racial Segregation	(2) Ten Commandments	(3) Mandatory Moment of Silence	(4) High School Teaching Degree	(5) Elementary Teaching Degree	(6) All Other School Reforms
Panel A: Religious Outcomes						
Index of Religious Outcomes	-0.022*** (0.008) 36,150	-0.021*** (0.008) 36,150	-0.022*** (0.008) 36,150	-0.021** (0.008) 36,150	-0.021** (0.008) 36,150	-0.021** (0.008) 36,150
Strong Believer	-0.054*** (0.015) 32,629	-0.054*** (0.015) 32,629	-0.054*** (0.015) 32,629	-0.055*** (0.015) 32,629	-0.057*** (0.015) 32,629	-0.058*** (0.015) 32,629
Panel B: Family/Lifestyle Outcomes						
Currently Divorced	0.033*** (0.011) 36,138	0.032*** (0.011) 36,138	0.033*** (0.011) 36,138	0.032*** (0.011) 36,138	0.032*** (0.011) 36,138	0.032*** (0.011) 36,138
Currently Married	-0.042*** (0.013) 36,138	-0.040*** (0.013) 36,138	-0.043*** (0.013) 36,138	-0.043*** (0.013) 36,138	-0.043*** (0.014) 36,138	-0.046*** (0.013) 36,138
Number of Children	-0.136** (0.066) 36,080	-0.135** (0.065) 36,080	-0.129* (0.064) 36,080	-0.139** (0.066) 36,080	-0.130** (0.064) 36,080	-0.124* (0.063) 36,080
Panel C: Labour Market Outcomes						
Employment	0.050** (0.020) 30,174	0.050** (0.020) 30,174	0.048** (0.020) 30,174	0.048** (0.020) 30,174	0.050** (0.019) 30,174	0.047** (0.019) 30,174
Income, Intensive Margin	0.003 (0.044) 24,809	0.004 (0.044) 24,809	0.000 (0.044) 24,809	-0.003 (0.044) 24,809	-0.002 (0.043) 24,809	-0.006 (0.044) 24,809
Income, Extensive Margin	0.532*** (0.179) 28,486	0.529*** (0.179) 28,486	0.506*** (0.178) 28,486	0.508*** (0.179) 28,486	0.509*** (0.177) 28,486	0.490*** (0.178) 28,486
Panel D: Gender Attitude Outcomes						

Appendix Table A9 - Continued

	(1) Racial Segregation	(2) Ten Commandments	(3) Mandatory Moment of Silence	(4) High School Teaching Degree	(5) Elementary Teaching Degree	(6) All Other School Reforms
Gender Attitude Index	-0.037** (0.015) 35,509	-0.036** (0.015) 35,509	-0.035** (0.015) 35,509	-0.036** (0.015) 35,509	-0.038** (0.015) 35,509	-0.036** (0.015) 35,509
Full-time Homemaking	-0.043** (0.017) 30,174	-0.042** (0.017) 30,174	-0.040** (0.017) 30,174	-0.041** (0.017) 30,174	-0.042** (0.017) 30,174	-0.040** (0.017) 30,174
Against Abortion	-0.005 (0.012) 22,461	-0.004 (0.012) 22,461	-0.002 (0.012) 22,461	-0.004 (0.012) 22,461	-0.007 (0.012) 22,461	-0.006 (0.012) 22,461
Panel E: Social/Political Outcomes						
Science Skeptic	-0.030*** (0.009) 23,131	-0.030*** (0.009) 23,131	-0.029*** (0.009) 23,131	-0.031*** (0.009) 23,131	-0.032*** (0.009) 23,131	-0.031*** (0.009) 23,131
Education Skeptic	0.023* (0.011) 23,999	0.022* (0.012) 23,999	0.023* (0.012) 23,999	0.021* (0.012) 23,999	0.020* (0.011) 23,999	0.020* (0.011) 23,999
Homophobia	-0.022 (0.019) 21,542	-0.021 (0.019) 21,542	-0.019 (0.019) 21,542	-0.021 (0.019) 21,542	-0.021 (0.019) 21,542	-0.020 (0.019) 21,542
Against Same-Sex Marriage	-0.111*** (0.039) 3,939	-0.111*** (0.039) 3,939	-0.108*** (0.039) 3,939	-0.110*** (0.039) 3,939	-0.113*** (0.040) 3,939	-0.110*** (0.040) 3,939
Conservative	0.013 (0.012) 32,912	0.014 (0.012) 32,912	0.013 (0.012) 32,912	0.015 (0.013) 32,912	0.015 (0.013) 32,912	0.014 (0.013) 32,912
Pro-Euthanasia	0.019 (0.017) 20,139	0.019 (0.017) 20,139	0.018 (0.017) 20,139	0.017 (0.017) 20,139	0.017 (0.016) 20,139	0.014 (0.017) 20,139

Appendix Table A9 - Continued

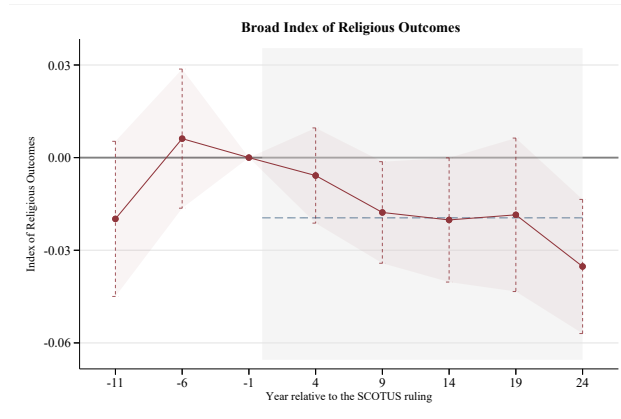
(1)	(2)	(3)	(4)	(5)	(6)
Racial	Ten	Mandatory Moment	High School	Elementary	All Other
Segregation	Commandments	of Silence	Teaching Degree	Teaching Degree	School Reforms

Notes: The table shows TWFE OLS coefficients and standard errors clustered at the state level in parentheses from estimating equation 1, controlling for additional state-by-year binary education policy indicators as reported in the column headers. Column (1) controls for racial desegregation in schools; column (2) controls for the banning of Ten Commandments postings in public schools; column (3) controls for the banning of mandatory moments of silence in public schools; column (4) controls for the introduction teacher certification requirements for high school teachers; column (5) controls for the introduction of teacher certification requirements for elementary school teachers; column (6) includes all five policy controls together. The outcome is labeled above each panel. The composite index of religious outcomes encompasses: self-identification as a 'strong believer'; measure of prayer frequency; measure of church attendance frequency; belief in the Bible; belief in the afterlife; belief in God; indicator of religious affiliation; and self-identification as a 'fundamentalist'. All religious outcomes are coded such that an increase in the variable implies an increase in religiosity. The composite index of gender attitude outcomes encompasses: opinion on full-time homemaking; opposition to abortion in case of genetic conditions in the fetus; opinion on women in politics; opinion on women in the labour force. All gender attitude outcomes are coded such that an increase in the variable implies a decrease in gender-egalitarian attitudes. Treatment: continuous variable $Exposure_{st}$, representing the share of an individual's compulsory school years that fell under the post-ruling regime for respondents residing in an affected state at age 16, and zero otherwise. Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Standard errors are in parentheses followed by observation count. Data source: General Social Survey. Single, double, and triple asterisks indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table A10: GSS Survey Questions and Variable Definitions

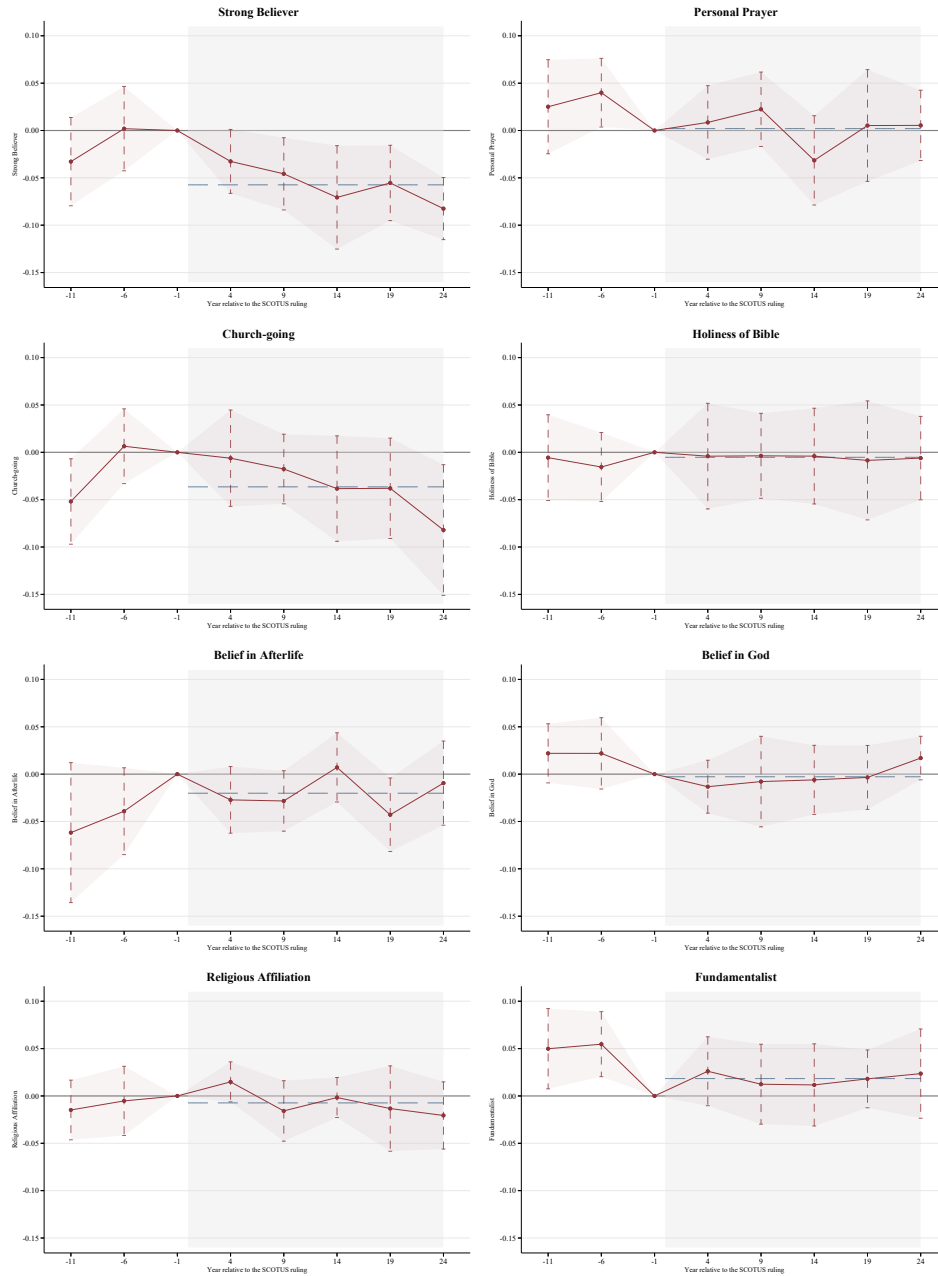
Outcome	Survey Question	Answer Options
<i>Religious Outcomes</i>		
Index of Religious Outcomes	Composite index of the following religious outcomes	
Strong Believer	Would you call yourself a strong believer?	Yes/No
Personal Prayer	About how often do you pray?	Often/Not Often
Church-Going	How often do you attend religious services?	Often/Not Often
Belief in the Bible	Do you feel that the Bible is the word of God?	Yes/No
Belief in the Afterlife	Do you believe there is a life after death?	Yes/No
Belief in God	Are you confident in the existence of God?	Yes/No
Religious Affiliation	Do you have a religious preference?	Yes/No
Fundamentalist	Is your religious belief fundamentalist	Yes/No
<i>Family/Lifestyle Outcomes</i>		
Currently Divorced	Are you currently divorced?	Yes/No
Currently Married	Are you currently married?	Yes/No
Number of Children	How many children have you ever had?	0/1/2/3/4/5/6/7/ 8 or more
<i>Labour Market Outcomes</i>		
Employment	Last week were you working full time, part time, or with a job but not at work?	Yes/No
Income, Intensive Margin	In which of these groups did your earnings for last year fall? (conditional on employment)	Log midpoint of \$1-\$1,000/ \$1,000-\$2,999/.../ \$25,000 or more
Income, Extensive Margin	In which of these groups did your earnings for last year fall? (unconditional)	As above, 0 if not employed
<i>Gender Attitude Outcomes</i>		
Gender Attitude Index	Composite index of the following gender attitude outcomes and an index of abortion stances	
Full-time Homemaking	Last week were you keeping house?	Yes/No
Against Abortion	Are you against a pregnant woman obtaining a legal abortion if there is a strong chance of serious defect in the baby?	Yes/No
Against Women in Politics	Do you believe most men are better suited emotionally for politics than most women?	Yes/No
Against Working Women	Do you believe it is much better if the man is the achiever outside the home and the woman takes care of the home and family?	Yes/No
<i>Social/Political Outcomes</i>		
Science Skeptic	Would you say you have hardly any confidence in the scientific community?	Yes/No
Education Skeptic	Would you say you have hardly any confidence in education?	Yes/No
Homophobia	Do you think sexual relations between two adults of the same sex is wrong?	Yes/No
Against Same-Sex Marriage	Are you against homosexual couples having the right to marry one another?	Yes/No
Conservative	Would you say that you are not a liberal?	Yes/No
Pro-Euthanasia	Do you think doctors should be allowed by law to end a terminally-ill patient's life by some painless means if the patient and his family request it?	Yes/No

Notes: The table displays the original GSS survey prompts and the corresponding answer scales for all outcome variables used in the analysis. Categorical responses have been harmonized into the binary formats shown where applicable. Data source: General Social Survey.



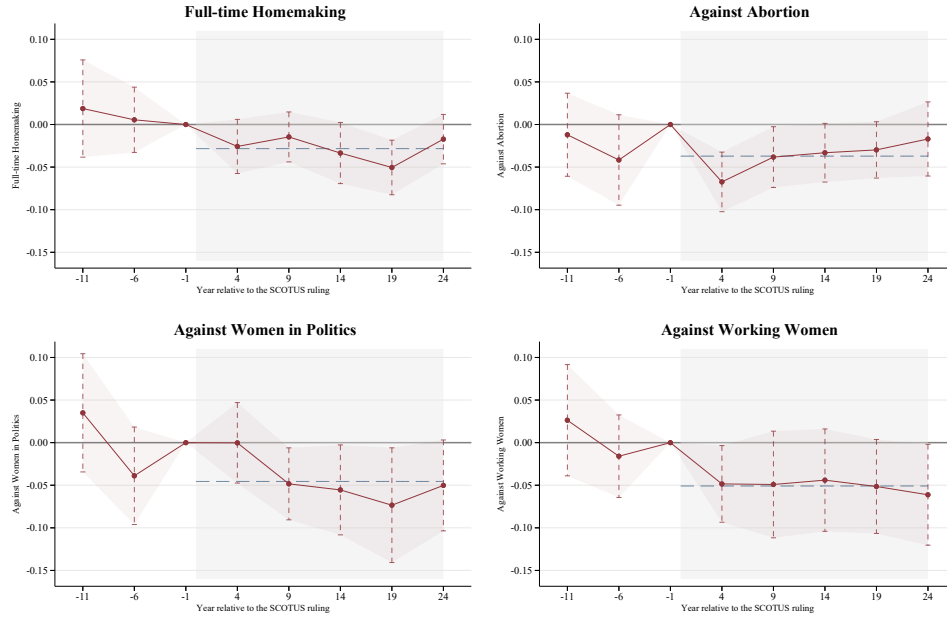
Appendix Figure A1: Event-Study Graph for the Effect of the SCOTUS Ruling on a Broad Index of Religious Outcomes

Note: The figure shows TWFE OLS point estimates and 95% confidence intervals from estimating equation 2. The dependent variable is a broad composite index of religious outcomes, encompassing measures of religiosity and spirituality; prayer, church and church activity attendance frequency; belief in the Bible, the afterlife, religion and God; self-identified religious strength and proximity to God; and fundamentalist, 'born again' and missionary status. Treatment: binary indicator for whether the respondent's state of residence at age 16 had already abolished devotional Bible reading and prayer in public schools prior to SCOTUS ruling (comparison group) or not (treated group). Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Numbers on the horizontal axis refer to the final year of respective five-year bins; that is, -1 = last five years prior to treatment. Joint p -values for the null hypothesis of zero effect are 0.231 for the pre-reform periods and 0.030 for the post-reform periods. Inference: clustering at the state level. Data source: General Social Survey.



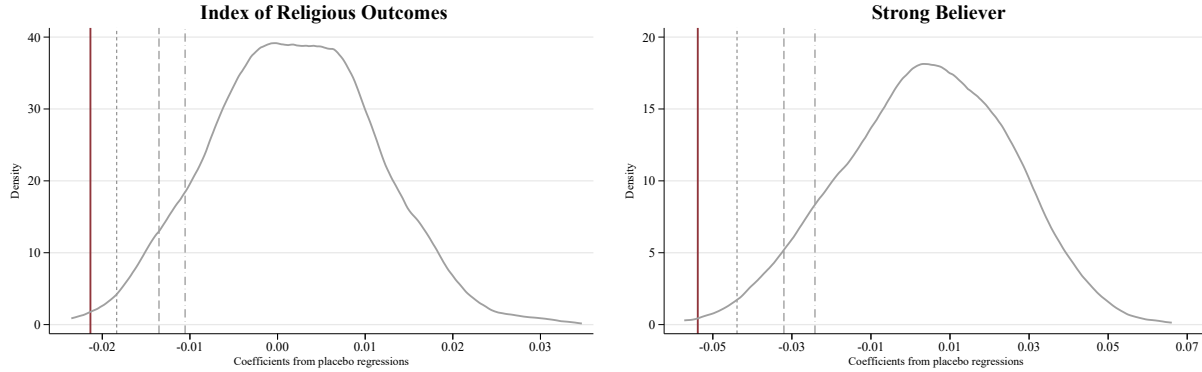
Appendix Figure A2: Event-Study Graph for the Effect of the SCOTUS Ruling on Religious Outcomes

Note: The figure shows TWFE OLS point estimates and 95% confidence intervals from estimating equation 2. The figure displays the effect of the ruling on religious outcomes encompassed by the index of religious outcomes: self-identification as a ‘strong believer’; measure of prayer frequency; measure of church attendance frequency; belief in the Bible; belief in the afterlife; belief in God; indicator of religious affiliation; and self-identification as a ‘fundamentalist’. All religious outcomes are coded such that an increase in the variable implies an increase in religiosity. Treatment: binary indicator for whether the respondent’s state of residence at age 16 had already abolished devotional Bible reading and prayer in public schools prior to SCOTUS ruling (comparison group) or not (treated group). Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Numbers on the horizontal axis refer to the final year of respective five-year bins; that is, -1 = last five years prior to treatment. Joint p -values for the null hypothesis of zero effect for the pre-reform periods and the post-reform periods are, respectively: 0.381 and 0.000 for strong believer; 0.105 and 0.193 for personal prayer; 0.051 and 0.014 for church-going; 0.700 and 1.000 for belief in the bible; 0.191 and 0.040 for belief in the afterlife; 0.280 and 0.110 for belief in God; 0.597 and 0.151 for religious affiliation; 0.002 and 0.402 for fundamentalist. Inference: clustering at the state level. Data source: General Social Survey.



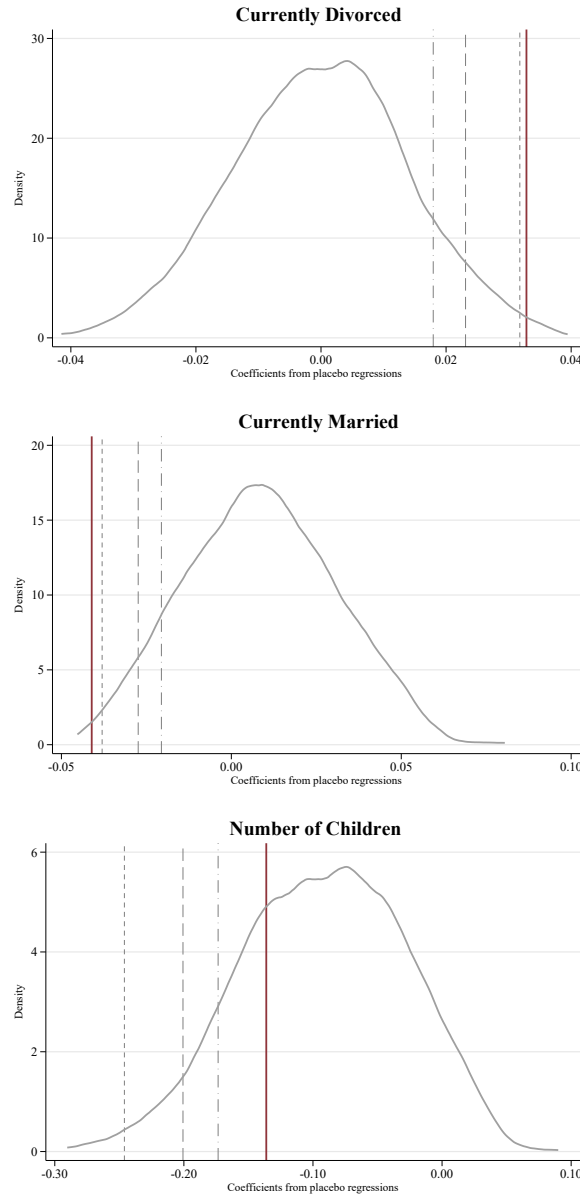
Appendix Figure A3: Event-Study Graph for the Effect of the SCOTUS Ruling on Gender Attitude Outcomes

Note: The figure shows TWFE OLS point estimates and 95% confidence intervals from estimating equation 2. The figure displays the effect of the ruling on gender attitude outcomes encompassed by the index of gender attitude outcomes: opinion on full-time homemaking; opposition to abortion in case of genetic conditions in the fetus; opinion on women in politics; opinion on women in the labour force. All gender attitude outcomes are coded such that an increase in the variable implies a decrease in gender-egalitarian attitudes. Treatment: binary indicator for whether the respondent's state of residence at age 16 had already abolished devotional Bible reading and prayer in public schools prior to SCOTUS ruling (comparison group) or not (treated group). Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Numbers on the horizontal axis refer to the final year of respective five-year bins; that is, -1 = last five years prior to treatment. Joint p -values for the null hypothesis of zero effect for the pre-reform periods and the post-reform periods are, respectively: 0.805 and 0.031 for full-time homemaking; 0.307 and 0.004 for abortion opposition in case of genetic conditions in the fetus; 0.128 and 0.011 for opposition to women in politics; 0.552 and 0.319 for opposition to women in the labour force. Inference: clustering at the state level. Data source: General Social Survey.



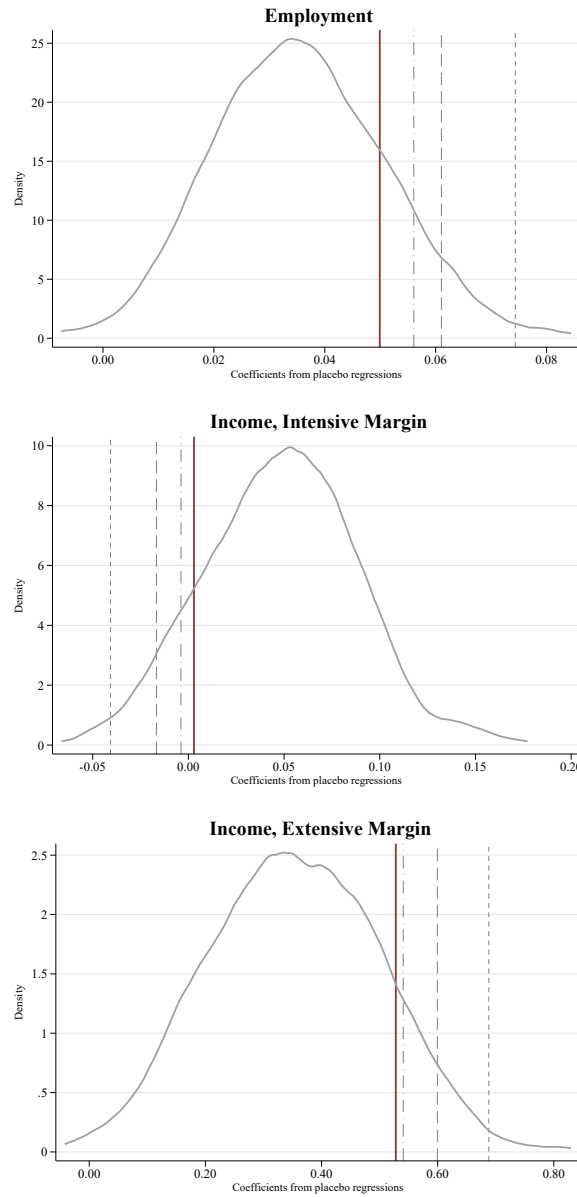
Appendix Figure A4: Density Plot of Placebo Estimates for the Effect of the SCOTUS Ruling on Religious Outcomes, Based on 1,000 Permutations

Note: The figure shows the kernel density plot of TWFE OLS coefficients from randomly reshuffling treatment across states (maintaining the baseline number of treated states) and re-estimating equation 2 on each of 1,000 permutations. The figure displays the effect of the ruling on religious outcomes as indicated in the figure headers. The composite index of religious outcomes encompasses: self-identification as a 'strong believer'; measure of prayer frequency; measure of church attendance frequency; belief in the Bible; belief in the afterlife; belief in God; indicator of religious affiliation; and self-identification as a 'fundamentalist'. All religious outcomes are coded such that an increase in the variable implies an increase in religiosity. Treatment: continuous variable $Exposure_{st}$, representing the share of an individual's compulsory school years that fell under the post-ruling regime for respondents residing in an affected state at age 16, and zero otherwise. Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Red solid vertical line indicates coefficient of reform effect from baseline model (-0.021 for the index of religious outcomes and -0.054 for strong believer). Black dash-dot vertical line indicates 10th percentile of the distribution of the 1000 placebo coefficients (-0.011 for the index of religious outcomes and -0.024 for strong believer). Black dashed vertical line indicates 5th percentile of the distribution of the 1000 placebo coefficients (-0.014 for the index of religious outcomes and -0.032 for strong believer). Black dotted vertical line indicates 1st percentile of the distribution of the 1000 placebo coefficients (-0.018 for the index of religious outcomes and -0.044 for strong believer). Inference: clustering at the state level. Data source: General Social Survey.



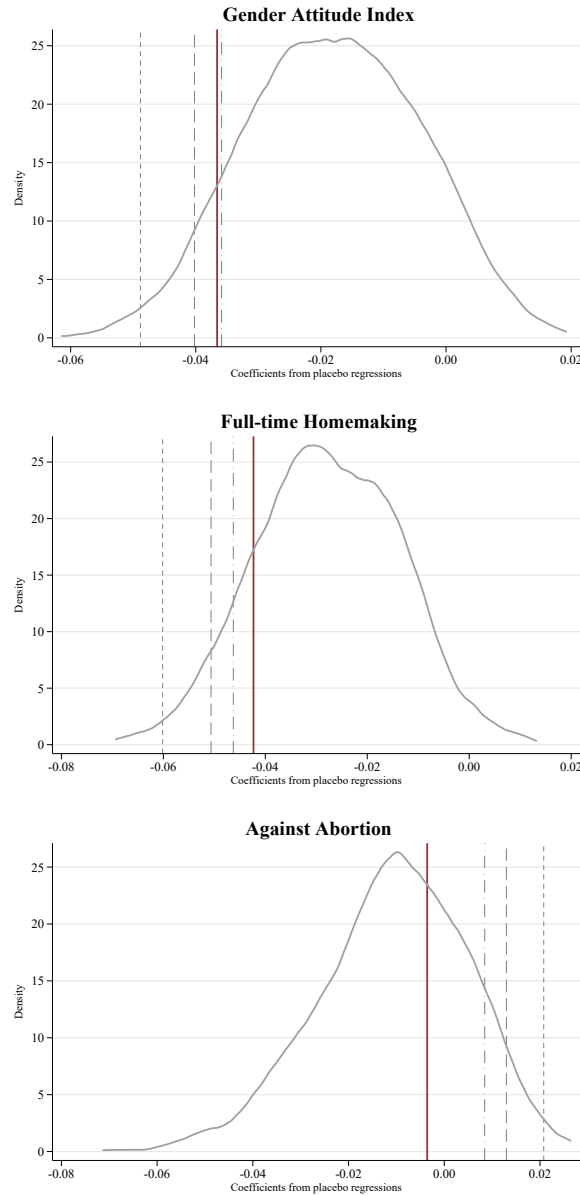
Appendix Figure A5: Density Plot of Placebo Estimates for the Effect of the SCOTUS Ruling on Family/Lifestyle Outcomes, Based on 1,000 Permutations

Note: The figure shows the kernel density plot of TWFE OLS coefficients from randomly reshuffling treatment across states (maintaining the baseline number of treated states) and re-estimating equation 2 on each of 1,000 permutations. The figure displays the effect of the ruling on family and lifestyle outcomes as indicated in the figure headers. Treatment: continuous variable $Exposure_{st}$, representing the share of an individual's compulsory school years that fell under the post-ruling regime for respondents residing in an affected state at age 16, and zero otherwise. Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Red solid vertical line indicates coefficient of reform effect from baseline model (0.033 for currently divorced, -0.041 for currently married and -0.136 for number of children). Black dash-dot vertical line indicates 10th/90th percentile of the distribution of the 1000 placebo coefficients (0.018 for currently divorced, -0.021 for currently married and -0.173 for number of children). Black dashed vertical line indicates 5th/95th percentile of the distribution of the 1000 placebo coefficients (0.023 for currently divorced, -0.027 for currently married and -0.201 for number of children). Black dotted vertical line indicates 1st/99th percentile of the distribution of the 1000 placebo coefficients (0.032 for currently divorced, -0.038 for currently married and -0.246 for number of children). Inference: clustering at the state level. Data source: General Social Survey.



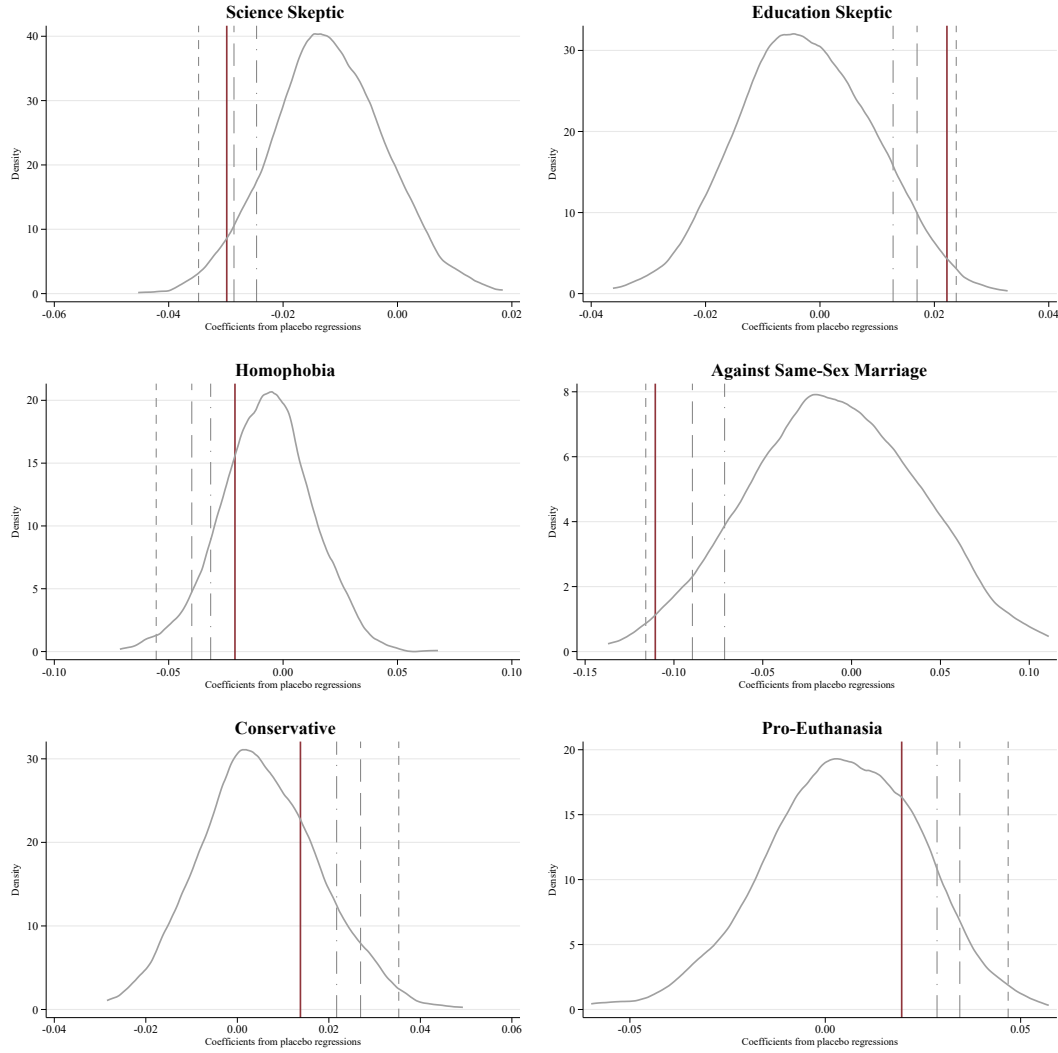
Appendix Figure A6: Density Plot of Placebo Estimates for the Effect of the SCOTUS Ruling on Labour Market Outcomes, Based on 1,000 Permutations

Note: The figure shows the kernel density plot of TWFE OLS coefficients from randomly reshuffling treatment across states (maintaining the baseline number of treated states) and re-estimating equation 2 on each of 1,000 permutations. The figure displays the effect of the ruling on labour market outcomes as indicated in the figure headers. Treatment: continuous variable $Exposure_{st}$, representing the share of an individual's compulsory school years that fell under the post-ruling regime for respondents residing in an affected state at age 16, and zero otherwise. Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Red solid vertical line indicates coefficient of reform effect from baseline model (0.050 for employment, 0.003 for income, intensive margin and 0.528 for income, extensive margin). Black dash-dot vertical line indicates 10th/90th percentile of the distribution of the 1000 placebo coefficients (0.056 for employment, -0.004 for income, intensive margin and 0.541 for income, extensive margin). Black dashed vertical line indicates 5th/95th percentile of the distribution of the 1000 placebo coefficients (0.061 for employment, -0.017 for income, intensive margin and 0.600 for income, extensive margin). Black dotted vertical line indicates 1st/99th percentile of the distribution of the 1000 placebo coefficients (0.074 for employment, -0.041 for income, intensive margin and 0.688 for income, extensive margin). Inference: clustering at the state level. Data source: General Social Survey.



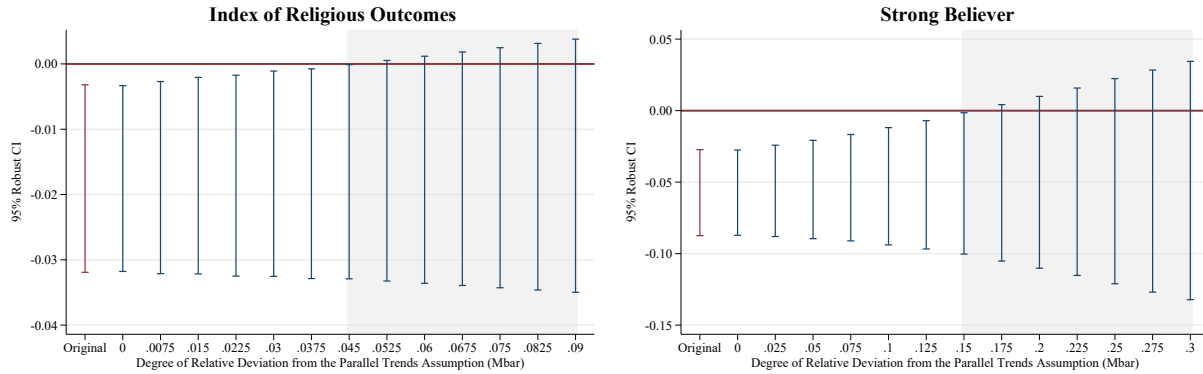
Appendix Figure A7: Density Plot of Placebo Estimates for the Effect of the SCOTUS Ruling on Gender Attitude Outcomes, Based on 1,000 Permutations

Note: The figure shows the kernel density plot of TWFE OLS coefficients from randomly reshuffling treatment across states (maintaining the baseline number of treated states) and re-estimating equation 2 on each of 1,000 permutations. The figure displays the effect of the ruling on gender attitude outcomes as indicated in the figure headers. The composite index of gender attitude outcomes encompasses: opinion on full-time homemaking; opposition to abortion in case of genetic conditions in the fetus; opinion on women in politics; opinion on women in the labour force. All gender attitude outcomes are coded such that an increase in the variable implies a decrease in gender-egalitarian attitudes. Treatment: continuous variable $Exposure_{st}$, representing the share of an individual's compulsory school years that fell under the post-ruling regime for respondents residing in an affected state at age 16, and zero otherwise. Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Red solid vertical line indicates coefficient of reform effect from baseline model (-0.037 for the gender attitude index, -0.042 for full-time homemaking and -0.004 for abortion opposition in case of genetic conditions in the fetus). Black dash-dot vertical line indicates 10th percentile of the distribution of the 1000 placebo coefficients (-0.036 for the gender attitude index, -0.046 for full-time homemaking and 0.008 for abortion opposition in case of genetic conditions in the fetus). Black dashed vertical line indicates 5th percentile of the distribution of the 1000 placebo coefficients (-0.040 for the gender attitude index, -0.051 for full-time homemaking and 0.013 for abortion opposition in case of genetic conditions in the fetus). Black dotted vertical line indicates 1st percentile of the distribution of the 1000 placebo coefficients (-0.049 for the gender attitude index, -0.060 for full-time homemaking and 0.021 for abortion opposition in case of genetic conditions in the fetus). Inference: clustering at the state level. Data source: General Social Survey.



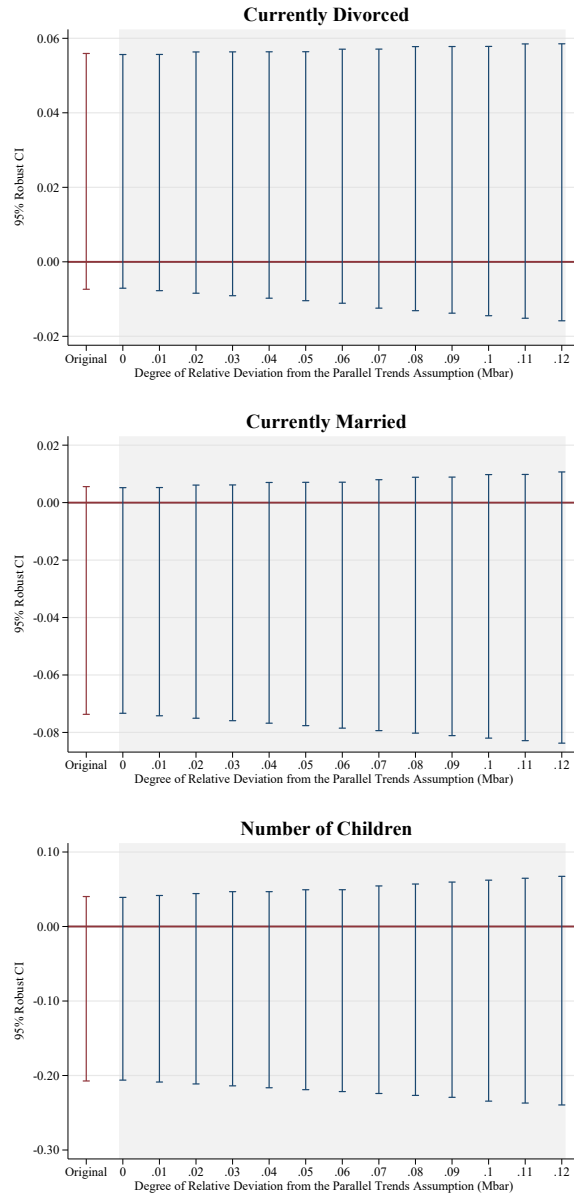
Appendix Figure A8: Density Plot of Placebo Estimates for the Effect of the SCOTUS Ruling on Social/Political Outcomes, Based on 1,000 Permutations

Note: The figure shows the kernel density plot of TWFE OLS coefficients from randomly reshuffling treatment across states (maintaining the baseline number of treated states) and re-estimating equation 2 on each of 1,000 permutations. The figure displays the effect of the ruling on social attitude and political outcomes as indicated in the figure headers. Treatment: continuous variable $Exposure_{st}$, representing the share of an individual's compulsory school years that fell under the post-ruling regime for respondents residing in an affected state at age 16, and zero otherwise. Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Red solid vertical line indicates coefficient of reform effect from baseline model (−0.030 science skeptic, 0.022 for education skeptic, −0.021 for homophobia, −0.111 for same-sex marriage opposition, 0.014 for conservative and 0.020 for pro-euthanasia). Black dash-dot vertical line indicates 10th/90th percentile of the distribution of the 1000 placebo coefficients (−0.025 science skeptic, 0.013 for education skeptic, −0.032 for homophobia, −0.071 for same-sex marriage opposition, 0.022 for conservative and 0.029 for pro-euthanasia). Black dashed vertical line indicates 5th/95th percentile of the distribution of the 1000 placebo coefficients (−0.029 science skeptic, 0.017 for education skeptic, −0.040 for homophobia, −0.090 for same-sex marriage opposition, 0.027 for conservative and 0.034 for pro-euthanasia). Black dotted vertical line indicates 1st/99th percentile of the distribution of the 1000 placebo coefficients (−0.035 science skeptic, 0.024 for education skeptic, −0.055 for homophobia, −0.116 for same-sex marriage opposition, 0.035 for conservative and 0.047 for pro-euthanasia). Inference: clustering at the state level. Data source: General Social Survey.



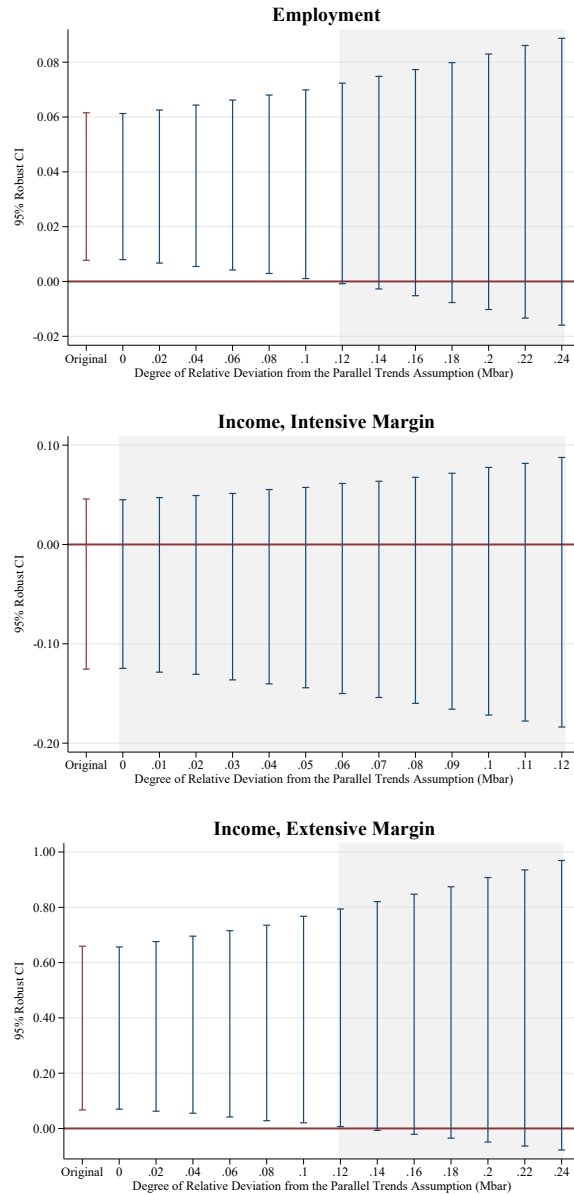
Appendix Figure A9: Rambachan and Roth (2023) Test for Non-Linear Violations of Parallel Trends on Religious Outcomes: Relative Changes

Note: Figure shows sensitivity analysis of estimated effect on religious outcomes as indicated in the figure headers to potential violations of parallel trends assumption per Rambachan and Roth (2023); see also Ang (2020) and Biasi and Sarsons (2021). The composite index of religious outcomes encompasses: self-identification as a 'strong believer'; measure of prayer frequency; measure of church attendance frequency; belief in the Bible; belief in the afterlife; belief in God; indicator of religious affiliation; and self-identification as a 'fundamentalist'. All religious outcomes are coded such that an increase in the variable implies an increase in religiosity. Treatment: binary indicator for whether the respondent's state of residence at age 16 had already abolished devotional Bible reading and prayer in public schools prior to SCOTUS ruling (comparison group) or not (treated group). Red bar represents the average of the 95% confidence interval for the average post-reform effect, computed as an equal-weighted linear combination of the five post-period TWFE coefficients from estimating equation 2 (using the full coefficient covariance matrix from the underlying event-study regression). Blue bars represent corresponding 95% Fixed-Length Confidence Intervals (FLCI) when allowing for per-period violations of parallel trends of up to M . $M = 0$ corresponds to allowing a linear differential trend. $M > 0$ allows for increasingly more varied non-linear trends. Data source: General Social Survey.



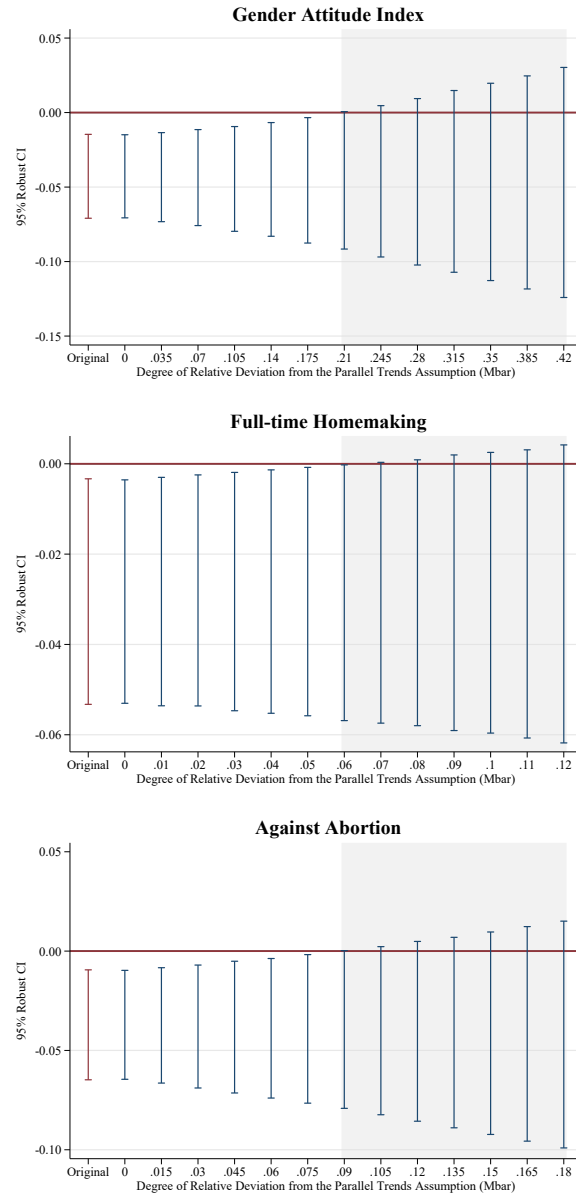
Appendix Figure A10: Rambachan and Roth (2023) Test for Non-Linear Violations of Parallel Trends on Family/Lifestyle Outcomes: Relative Changes

Note: Figure shows sensitivity analysis of estimated effect on family/lifestyle outcomes as indicated in the figure headers to potential violations of parallel trends assumption per Rambachan and Roth (2023); see also Ang (2020) and Biasi and Sarsons (2021). Treatment: binary indicator for whether the respondent's state of residence at age 16 had already abolished devotional Bible reading and prayer in public schools prior to SCOTUS ruling (comparison group) or not (treated group). Red bar represents the average of the 95% confidence interval for the average post-reform effect, computed as an equal-weighted linear combination of the five post-period TWFE coefficients from estimating equation 2 (using the full coefficient covariance matrix from the underlying event-study regression). Blue bars represent corresponding 95% Fixed-Length Confidence Intervals (FLCI) when allowing for per-period violations of parallel trends of up to M . $M = 0$ corresponds to allowing a linear differential trend. $M > 0$ allows for increasingly more varied non-linear trends. Data source: General Social Survey.



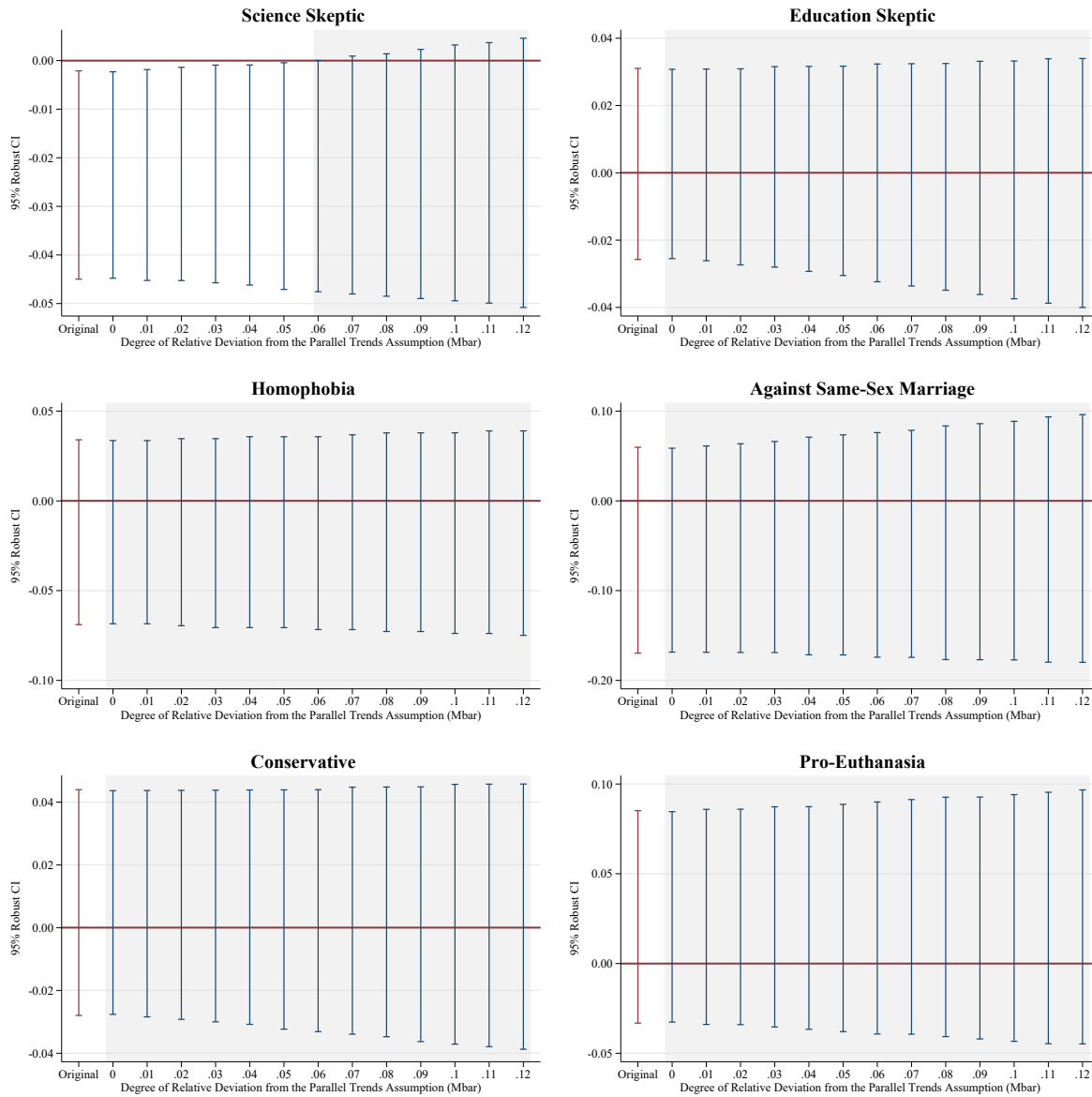
Appendix Figure A11: Rambachan and Roth (2023) Test for Non-Linear Violations of Parallel Trends on Labour Market Outcomes: Relative Changes

Note: Figure shows sensitivity analysis of estimated effect on labour market outcomes as indicated in the figure headers to potential violations of parallel trends assumption per Rambachan and Roth (2023); see also Ang (2020) and Biasi and Sarsons (2021). Treatment: binary indicator for whether the respondent's state of residence at age 16 had already abolished devotional Bible reading and prayer in public schools prior to SCOTUS ruling (comparison group) or not (treated group). Red bar represents the average of the 95% confidence interval for the average post-reform effect, computed as an equal-weighted linear combination of the five post-period TWFE coefficients from estimating equation 2 (using the full coefficient covariance matrix from the underlying event-study regression). Blue bars represent corresponding 95% Fixed-Length Confidence Intervals (FLCI) when allowing for per-period violations of parallel trends of up to M . $M = 0$ corresponds to allowing a linear differential trend. $M > 0$ allows for increasingly more varied non-linear trends. Data source: General Social Survey.



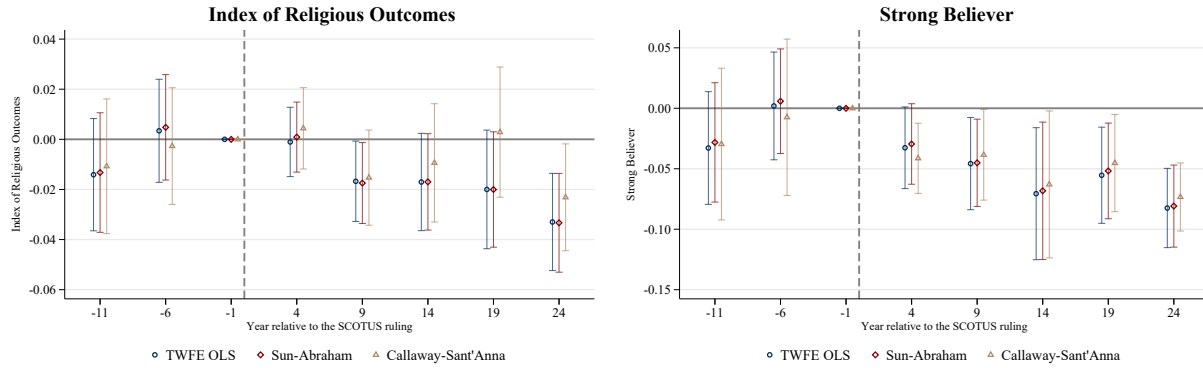
Appendix Figure A12: Rambachan and Roth (2023) Test for Non-Linear Violations of Parallel Trends on Gender Attitude Outcomes: Relative Changes

Note: Figure shows sensitivity analysis of estimated effect on gender attitude outcomes as indicated in the figure headers to potential violations of parallel trends assumption per Rambachan and Roth (2023); see also Ang (2020) and Biasi and Sarsons (2021). The composite index of gender attitude outcomes encompasses: opinion on full-time homemaking; opposition to abortion in case of genetic conditions in the fetus; opinion on women in politics; opinion on women in the labour force. All gender attitude outcomes are coded such that an increase in the variable implies a decrease in gender-egalitarian attitudes. Treatment: binary indicator for whether the respondent's state of residence at age 16 had already abolished devotional Bible reading and prayer in public schools prior to SCOTUS ruling (comparison group) or not (treated group). Red bar represents the average of the 95% confidence interval for the average post-reform effect, computed as an equal-weighted linear combination of the five post-period TWFE coefficients from estimating equation 2 (using the full coefficient covariance matrix from the underlying event-study regression). Blue bars represent corresponding 95% Fixed-Length Confidence Intervals (FLCI) when allowing for per-period violations of parallel trends of up to M . $M = 0$ corresponds to allowing a linear differential trend. $M > 0$ allows for increasingly more varied non-linear trends. Data source: General Social Survey.



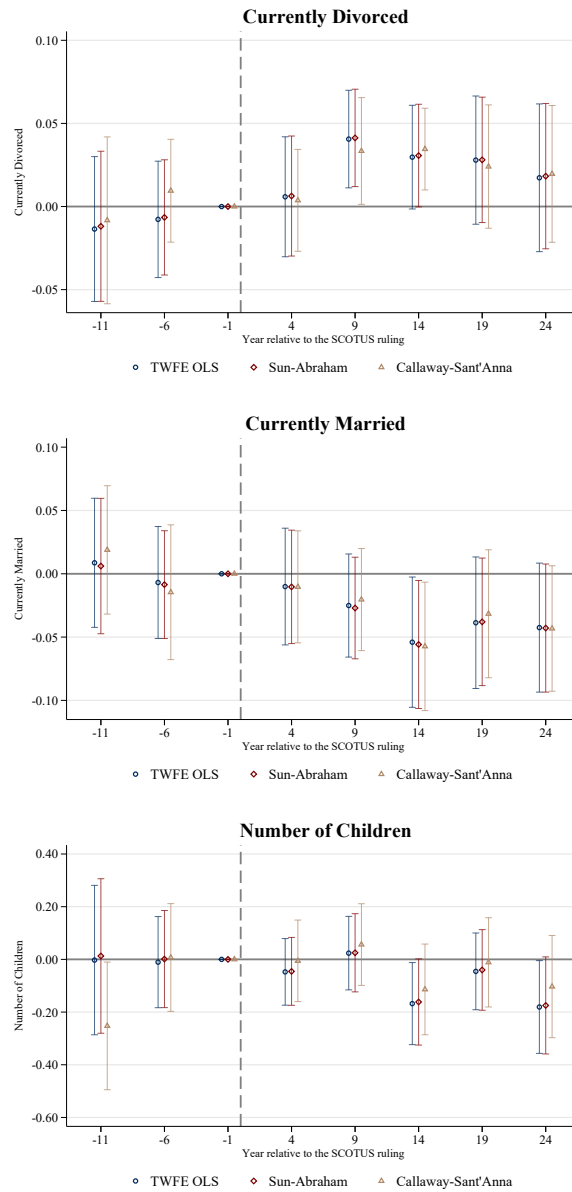
Appendix Figure A13: Rambachan and Roth (2023) Test for Non-Linear Violations of Parallel Trends on Social/Political Outcomes: Relative Changes

Note: Figure shows sensitivity analysis of estimated effect on social/political outcomes as indicated in the figure headers to potential violations of parallel trends assumption per Rambachan and Roth (2023); see also Ang (2020) and Biasi and Sarsons (2021). Treatment: binary indicator for whether the respondent's state of residence at age 16 had already abolished devotional Bible reading and prayer in public schools prior to SCOTUS ruling (comparison group) or not (treated group). Red bar represents the average of the 95% confidence interval for the average post-reform effect, computed as an equal-weighted linear combination of the five post-period TWFE coefficients from estimating equation 2 (using the full coefficient covariance matrix from the underlying event-study regression). Blue bars represent corresponding 95% Fixed-Length Confidence Intervals (FLCI) when allowing for per-period violations of parallel trends of up to M . $M = 0$ corresponds to allowing a linear differential trend. $M > 0$ allows for increasingly more varied non-linear trends. Data source: General Social Survey.



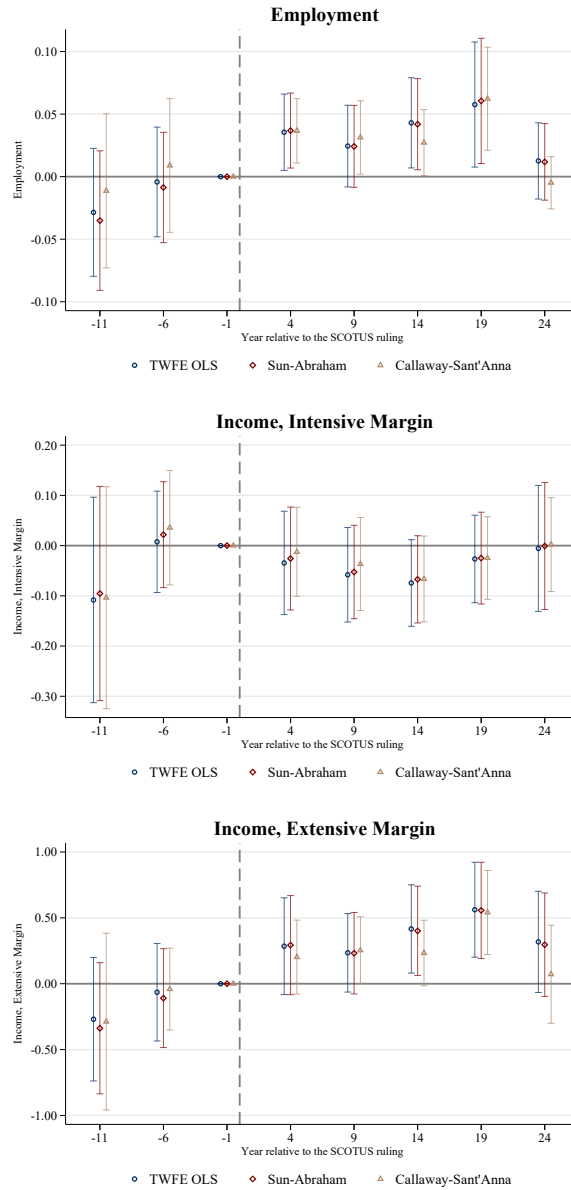
Appendix Figure A14: Alternative Estimators Robust to Time-Varying Treatment Effects on Religious Outcomes

Note: The figure shows coefficients from non-parametric event-study regressions and their 95% confidence intervals on religious outcomes as indicated in the figure headers. The composite index of religious outcomes encompasses: self-identification as a 'strong believer'; measure of prayer frequency; measure of church attendance frequency; belief in the Bible; belief in the afterlife; belief in God; indicator of religious affiliation; and self-identification as a 'fundamentalist'. All religious outcomes are coded such that an increase in the variable implies an increase in religiosity. TWFE OLS depicted in blue with circle markers. (Sun and Abraham, 2021) interaction-weighted estimator is depicted in maroon with diamond markers, using the last-treated units as the control cohort. (Callaway and Sant'Anna, 2021) estimator is depicted in sand-gold with triangle markers, using doubly robust inverse probability weighting and never-treated observations as controls. The composite index of religious outcomes encompasses: self-identification as a 'strong believer'; measure of prayer frequency; measure of church attendance frequency; belief in the Bible; belief in the afterlife; belief in God; indicator of religious affiliation; and self-identification as a 'fundamentalist'. Treatment: binary indicator for whether the respondent's state of residence at age 16 had already abolished devotional Bible reading and prayer in public schools prior to SCOTUS ruling (comparison group) or not (treated group). Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Numbers on the horizontal axis refer to the final year of respective five-year bins; that is, -1 = last five years prior to treatment. Inference: clustering at the state level. Data source: General Social Survey.



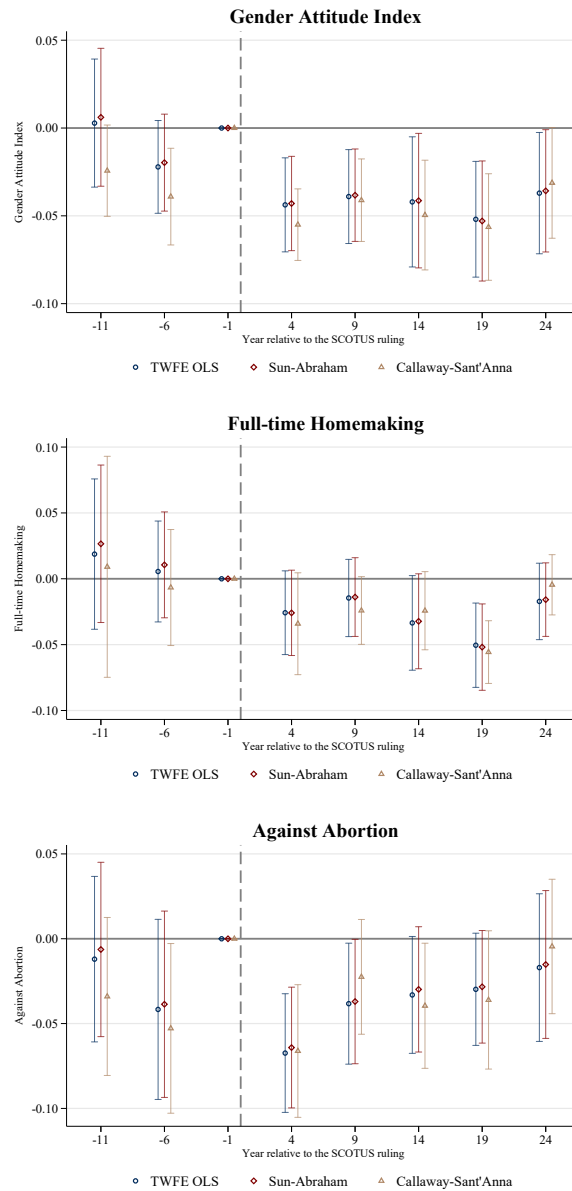
Appendix Figure A15: Alternative Estimators Robust to Time-Varying Treatment Effects on Family/Lifestyle Outcomes

Note: The figure shows coefficients from non-parametric event-study regressions and their 95% confidence intervals on family and lifestyle outcomes as indicated in the figure headers. TWFE OLS depicted in blue with circle markers. (Sun and Abraham, 2021) interaction-weighted estimator is depicted in maroon with diamond markers, using the last-treated units as the control cohort. (Callaway and Sant'Anna, 2021) estimator is depicted in sand-gold with triangle markers, using doubly robust inverse probability weighting and never-treated observations as controls. Treatment: binary indicator for whether the respondent's state of residence at age 16 had already abolished devotional Bible reading and prayer in public schools prior to SCOTUS ruling (comparison group) or not (treated group). Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Numbers on the horizontal axis refer to the final year of respective five-year bins; that is, -1 = last five years prior to treatment. Inference: clustering at the state level. Data source: General Social Survey.



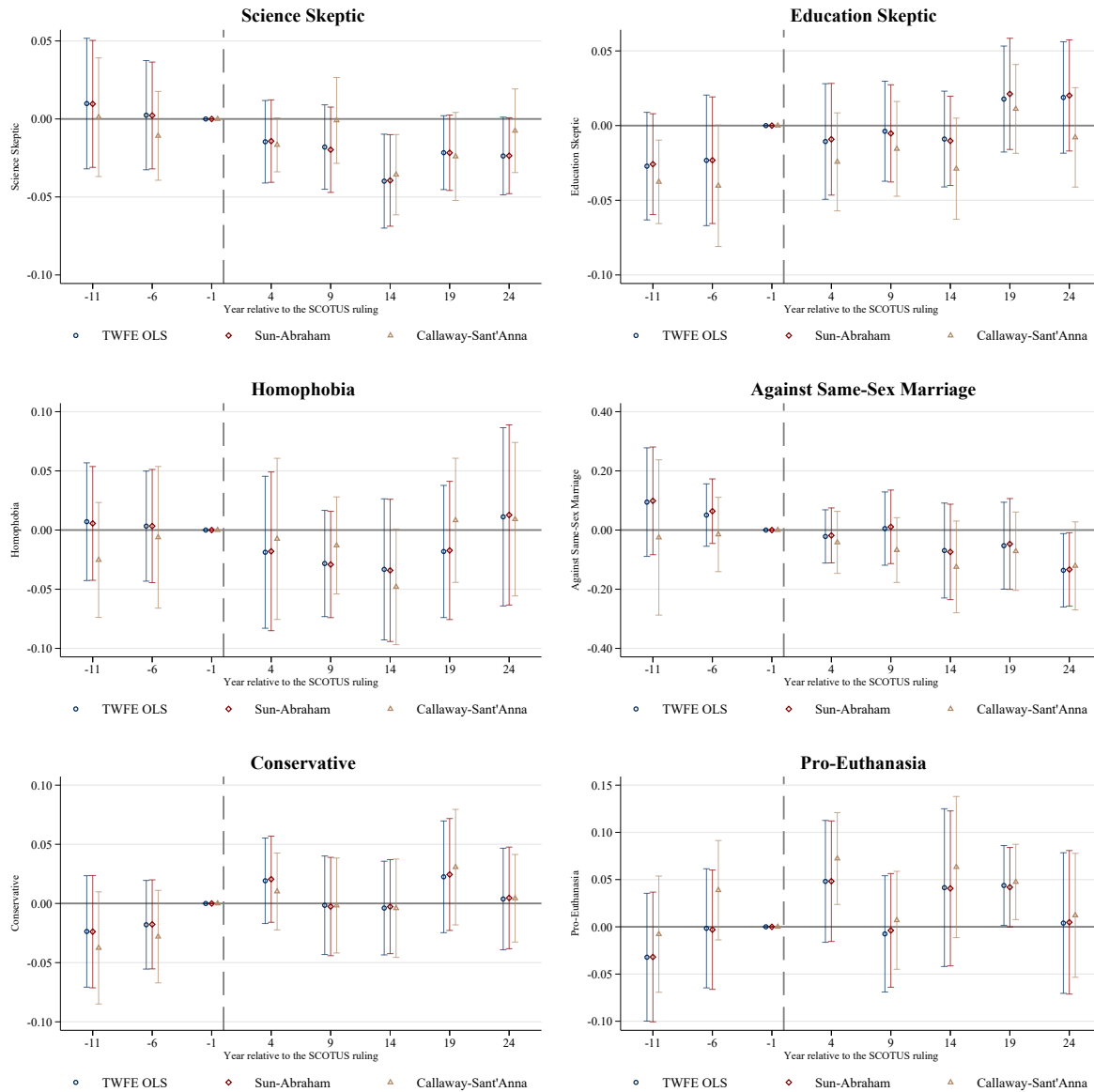
Appendix Figure A16: Alternative Estimators Robust to Time-Varying Treatment Effects on Labour Market Outcomes

Note: The figure shows coefficients from non-parametric event-study regressions and their 95% confidence intervals on labour market outcomes as indicated in the figure headers. TWFE OLS depicted in blue with circle markers. (Sun and Abraham, 2021) interaction-weighted estimator is depicted in maroon with diamond markers, using the last-treated units as the control cohort. (Callaway and Sant'Anna, 2021) estimator is depicted in sand-gold with triangle markers, using doubly robust inverse probability weighting and never-treated observations as controls. Treatment: binary indicator for whether the respondent's state of residence at age 16 had already abolished devotional Bible reading and prayer in public schools prior to SCOTUS ruling (comparison group) or not (treated group). Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Numbers on the horizontal axis refer to the final year of respective five-year bins; that is, -1 = last five years prior to treatment. Inference: clustering at the state level. Data source: General Social Survey.



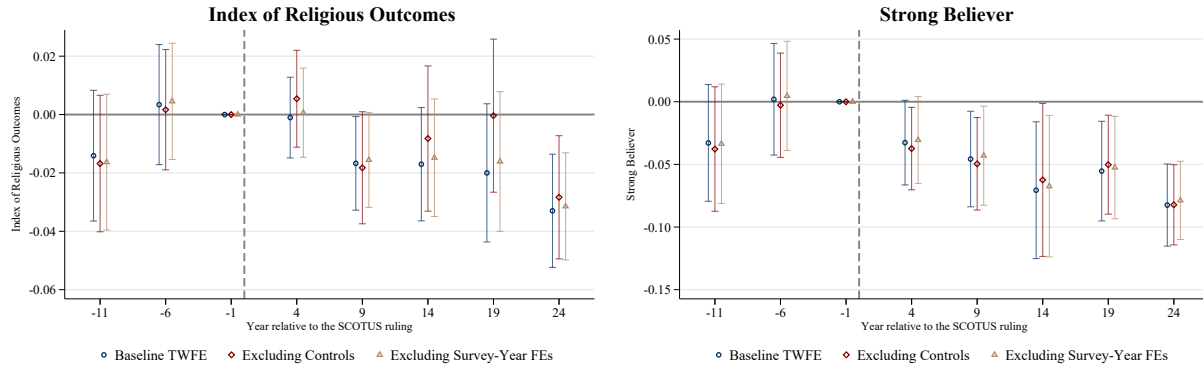
Appendix Figure A17: Alternative Estimators Robust to Time-Varying Treatment Effects on Gender Attitude Outcomes

Note: The figure shows coefficients from non-parametric event-study regressions and their 95% confidence intervals on gender attitude outcomes as indicated in the figure headers. The composite index of gender attitude outcomes encompasses: opinion on full-time homemaking; opposition to abortion in case of genetic conditions in the fetus; opinion on women in politics; opinion on women in the labour force. All gender attitude outcomes are coded such that an increase in the variable implies a decrease in gender-egalitarian attitudes. TWFE OLS depicted in blue with circle markers. (Sun and Abraham, 2021) interaction-weighted estimator is depicted in maroon with diamond markers, using the last-treated units as the control cohort. (Callaway and Sant'Anna, 2021) estimator is depicted in sand-gold with triangle markers, using doubly robust inverse probability weighting and never-treated observations as controls. The composite index of gender attitude outcomes encompasses: opinion on full-time homemaking; opposition to abortion in case of genetic conditions in the fetus; opinion on women in politics; opinion on women in the labour force. Treatment: binary indicator for whether the respondent's state of residence at age 16 had already abolished devotional Bible reading and prayer in public schools prior to SCOTUS ruling (comparison group) or not (treated group). Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Numbers on the horizontal axis refer to the final year of respective five-year bins; that is, -1 = last five years prior to treatment. Inference: clustering at the state level. Data source: General Social Survey.



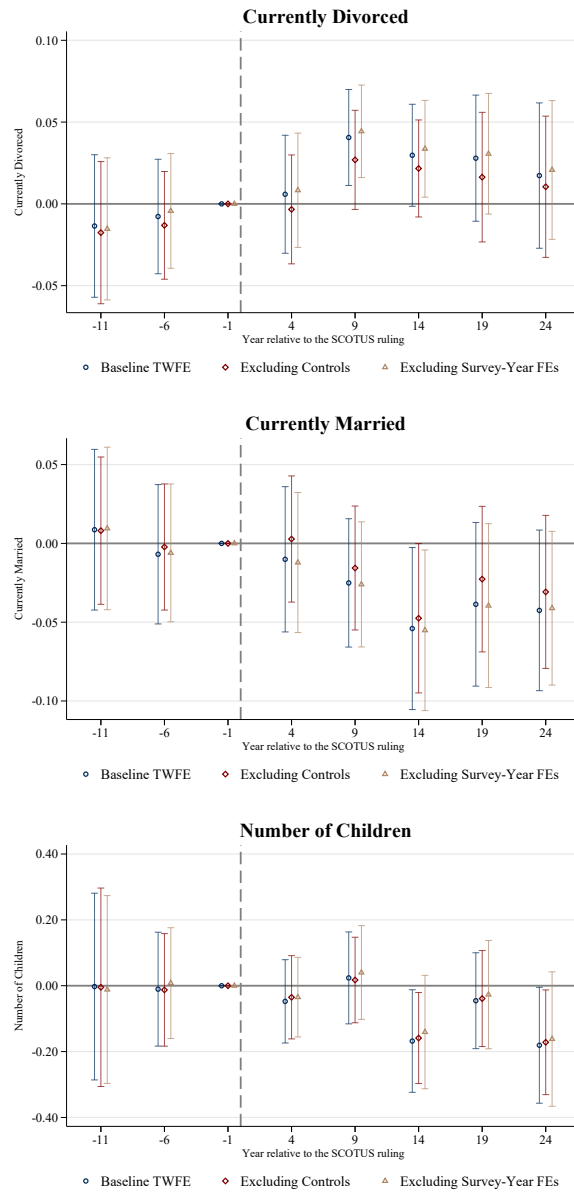
Appendix Figure A18: Alternative Estimators Robust to Time-Varying Treatment Effects on Social/Political Outcomes

Note: The figure shows coefficients from non-parametric event-study regressions and their 95% confidence intervals on social and political outcomes as indicated in the figure headers. TWFE OLS depicted in blue with circle markers. (Sun and Abraham, 2021) interaction-weighted estimator is depicted in maroon with diamond markers, using the last-treated units as the control cohort. (Callaway and Sant'Anna, 2021) estimator is depicted in sand-gold with triangle markers, using doubly robust inverse probability weighting and never-treated observations as controls. Treatment: binary indicator for whether the respondent's state of residence at age 16 had already abolished devotional Bible reading and prayer in public schools prior to SCOTUS ruling (comparison group) or not (treated group). Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Numbers on the horizontal axis refer to the final year of respective five-year bins; that is, -1 = last five years prior to treatment. Inference: clustering at the state level. Data source: General Social Survey.



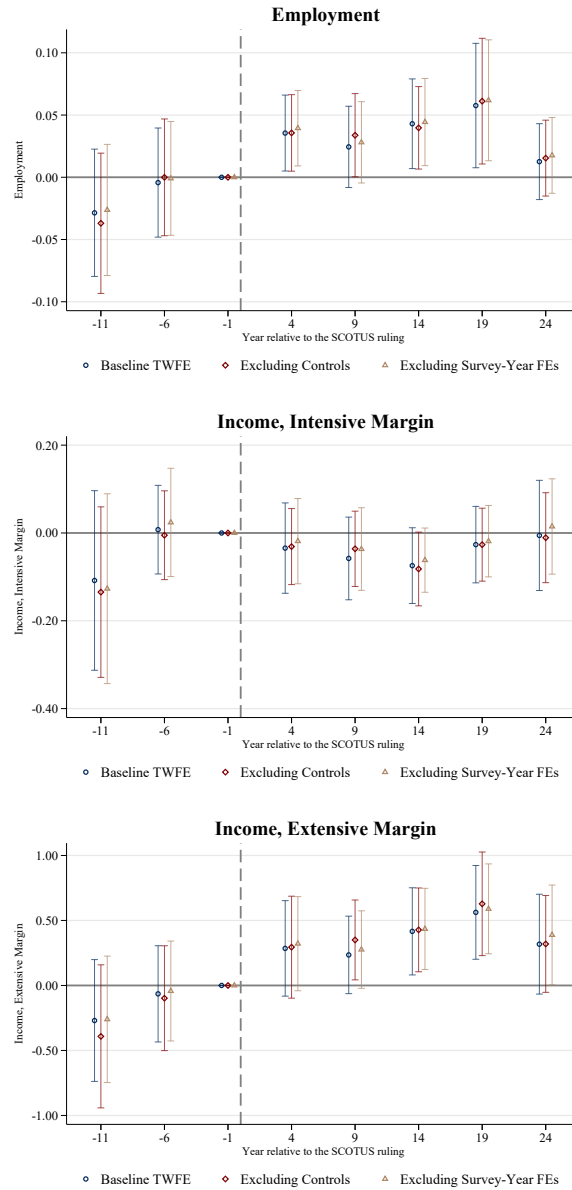
Appendix Figure A19: Sensitivity of Treatment Effects on Religious Outcomes to Specification Choices

Note: The figure shows TWFE OLS point estimates and 95% confidence intervals from estimating equation 2, for different sets of control variables and fixed effects. The baseline TWFE OLS is depicted in blue with circle markers. The specification excluding individual-level controls is depicted in maroon with diamond markers. The specification excluding survey-year fixed effects is depicted in sand-gold with triangle markers. The figure displays the effect of the ruling on religious outcomes as indicated in the figure headers. The composite index of religious outcomes encompasses: self-identification as a 'strong believer'; measure of prayer frequency; measure of church attendance frequency; belief in the Bible; belief in the afterlife; belief in God; indicator of religious affiliation; and self-identification as a 'fundamentalist'. All religious outcomes are coded such that an increase in the variable implies an increase in religiosity. Treatment: binary indicator for whether the respondent's state of residence at age 16 had already abolished devotional Bible reading and prayer in public schools prior to SCOTUS ruling (comparison group) or not (treated group). Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Numbers on the horizontal axis refer to the final year of respective five-year bins; that is, -1 = last five years prior to treatment. Inference: clustering at the state level. Data source: General Social Survey.



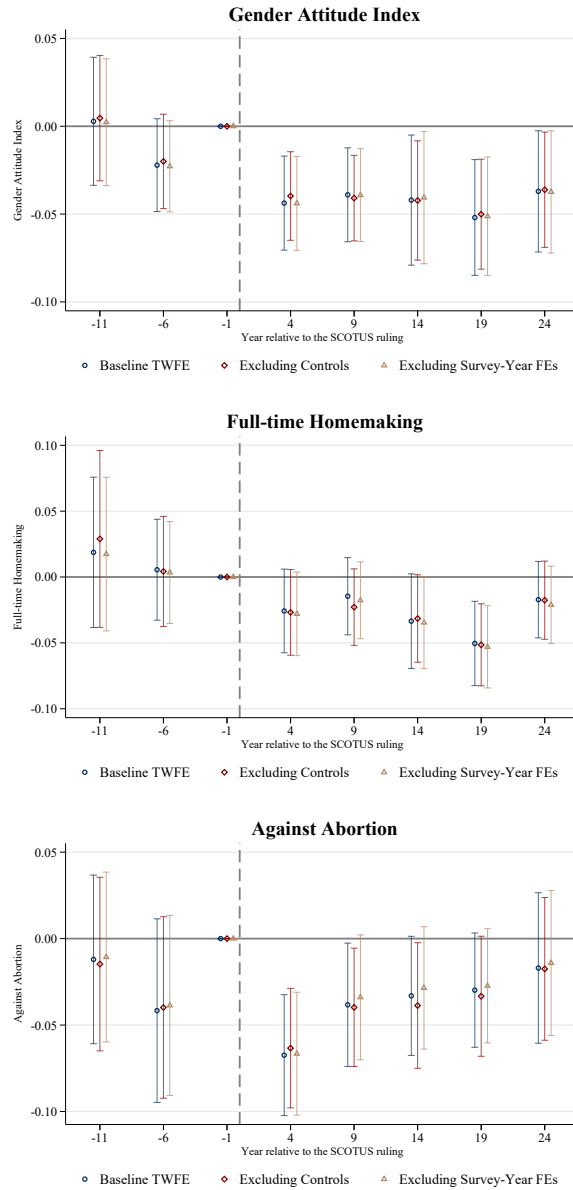
Appendix Figure A20: Sensitivity of Treatment Effects on Family/Lifestyle Outcomes to Specification Choices

Note: The figure shows TWFE OLS point estimates and 95% confidence intervals from estimating equation 2, for different sets of control variables and fixed effects. The baseline TWFE OLS is depicted in blue with circle markers. The specification excluding individual-level controls is depicted in maroon with diamond markers. The specification excluding survey-year fixed effects is depicted in sand-gold with triangle markers. The figure displays the effect of the ruling on family and lifestyle outcomes as indicated in the figure headers. Treatment: binary indicator for whether the respondent's state of residence at age 16 had already abolished devotional Bible reading and prayer in public schools prior to SCOTUS ruling (comparison group) or not (treated group). Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Numbers on the horizontal axis refer to the final year of respective five-year bins; that is, -1 = last five years prior to treatment. Inference: clustering at the state level. Data source: General Social Survey.



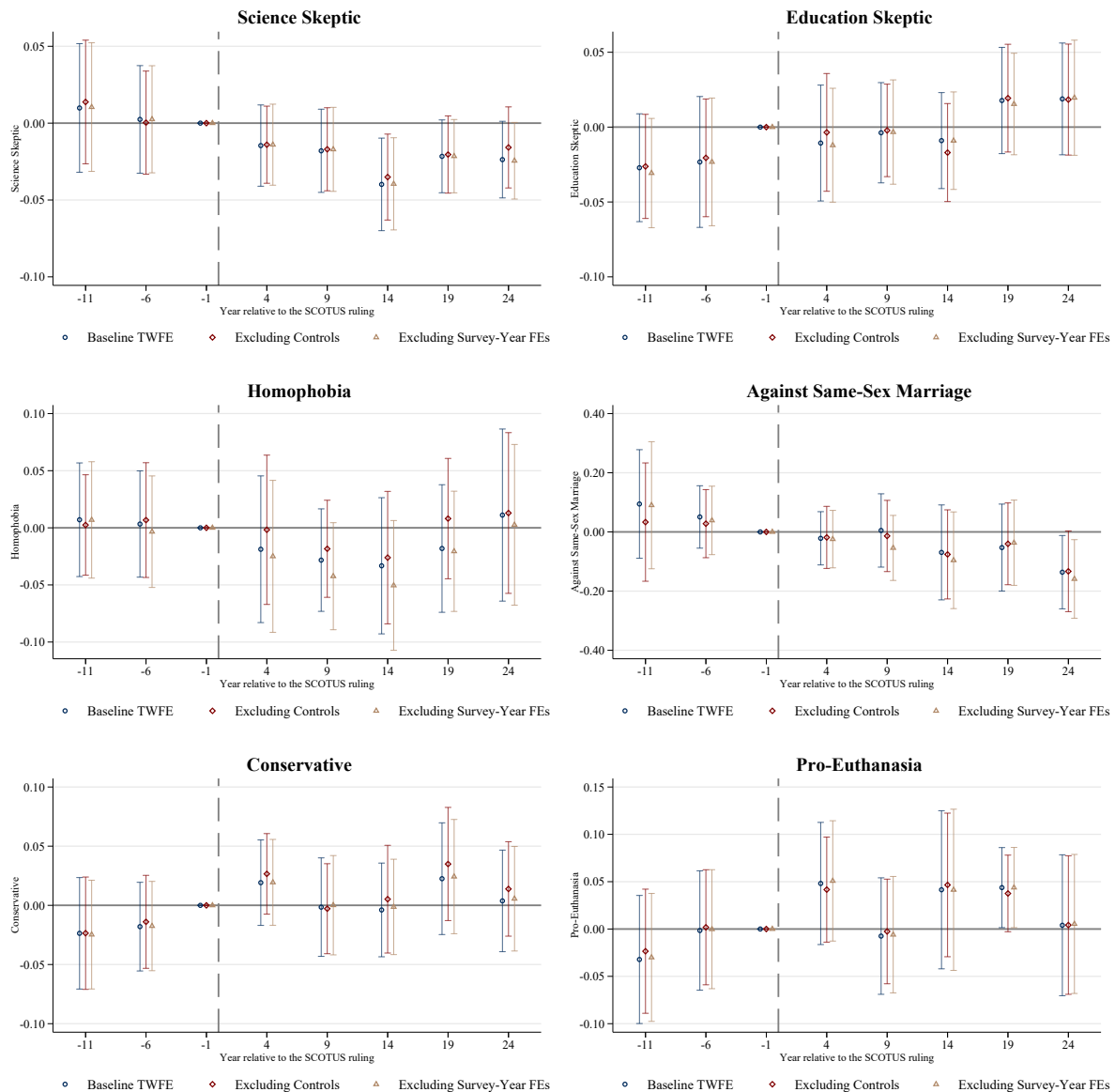
Appendix Figure A21: Sensitivity of Treatment Effects on Labour Market Outcomes to Specification Choices

Note: The figure shows TWFE OLS point estimates and 95% confidence intervals from estimating equation 2, for different sets of control variables and fixed effects. The baseline TWFE OLS is depicted in blue with circle markers. The specification excluding individual-level controls is depicted in maroon with diamond markers. The specification excluding survey-year fixed effects is depicted in sand-gold with triangle markers. The figure displays the effect of the ruling on labour market outcomes as indicated in the figure headers. Treatment: binary indicator for whether the respondent's state of residence at age 16 had already abolished devotional Bible reading and prayer in public schools prior to SCOTUS ruling (comparison group) or not (treated group). Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Numbers on the horizontal axis refer to the final year of respective five-year bins; that is, -1 = last five years prior to treatment. Inference: clustering at the state level. Data source: General Social Survey.



Appendix Figure A22: Sensitivity of Treatment Effects on Gender Attitude Outcomes to Specification Choices

Note: The figure shows TWFE OLS point estimates and 95% confidence intervals from estimating equation 2, for different sets of control variables and fixed effects. The baseline TWFE OLS is depicted in blue with circle markers. The specification excluding individual-level controls is depicted in maroon with diamond markers. The specification excluding survey-year fixed effects is depicted in sand-gold with triangle markers. The figure displays the effect of the ruling on gender attitude outcomes as indicated in the figure headers. The composite index of gender attitude outcomes encompasses: opinion on full-time homemaking; opposition to abortion in case of genetic conditions in the fetus; opinion on women in politics; opinion on women in the labour force. All gender attitude outcomes are coded such that an increase in the variable implies a decrease in gender-egalitarian attitudes. Treatment: binary indicator for whether the respondent's state of residence at age 16 had already abolished devotional Bible reading and prayer in public schools prior to SCOTUS ruling (comparison group) or not (treated group). Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Numbers on the horizontal axis refer to the final year of respective five-year bins; that is, -1 = last five years prior to treatment. Inference: clustering at the state level. Data source: General Social Survey.



Appendix Figure A23: Sensitivity of Treatment Effects on Social/Political Outcomes to Specification Choices

Note: The figure shows TWFE OLS point estimates and 95% confidence intervals from estimating equation 2, for different sets of control variables and fixed effects. The baseline TWFE OLS is depicted in blue with circle markers. The specification excluding individual-level controls is depicted in maroon with diamond markers. The specification excluding survey-year fixed effects is depicted in sand-gold with triangle markers. The figure displays the effect of the ruling on social attitude and political outcomes as indicated in the figure headers. Treatment: binary indicator for whether the respondent's state of residence at age 16 had already abolished devotional Bible reading and prayer in public schools prior to SCOTUS ruling (comparison group) or not (treated group). Controls: indicator variables for gender, rural v. urban upbringing, parental nationality, religious upbringing, and fixed effects for state, cohort, and survey year. Numbers on the horizontal axis refer to the final year of respective five-year bins; that is, -1 = last five years prior to treatment. Data source: General Social Survey.