Can Schools Change Religious Attitudes? Evidence from German State Reforms of Compulsory Religious Education*

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Abstract

We study whether compulsory religious education in schools affects students' religiosity as adults. We exploit the staggered termination of compulsory religious education across German states in models with state and cohort fixed effects. Using three different datasets, we find that a reform abolishing compulsory religious education significantly reduced the religiosity of affected students in adulthood. It also reduced the religious actions of personal prayer, church-going, and church membership. Beyond religious attitudes, and consistent with a shift towards worldly norms and economic activities, the reform led to higher labor-market participation and earnings. By contrast, the reform did not affect ethical and political values or non-religious school outcomes.

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I. Introduction

Religious attitudes are an important component of people's personalities and values. In the World Values Survey, 82 percent of participants belong to a religious denomination, 71 percent say that religion is important in their life, and 57 percent pray several times a week.[1] People's religiosity has important consequences for their personal preferences, interpersonal interactions, and economic prosperity (e.g., Iannaccone 1998; McCleary and Barro 2019; Becker, Rubin, and Woessmann 2024). Rigorous research on the emergence and determinants of religious attitudes, though, faces a challenging task as they are often deeply rooted in humans' personality and socialization. But can religious attitudes be taught in school? As public school curricula intervene in individuals' life course, this question addresses a core aspect of the interplay of churches and the state. In this paper, we study whether being exposed to compulsory religious education in school affects religiosity in adulthood. As churches tend to convey specific worldly norms, we also study effects beyond the religious sphere on labor-market outcomes.

We exploit the unique German setting where staggered reforms abolished compulsory religious education across states since the 1970s. The 1949 Constitution of West Germany had formally enshrined religious education as the only subject that is institutionalized as a regular subject in public schools, so that religious education was a compulsory subject in state curricula. Religious education was very intense: High-school graduates were exposed to roughly 1,000 hours of religious education over their school career – more than four times the hours of physics classes, for example (Havers 1972). In reforms enacted at different points in time between 1972 and 2004, the different states replaced the obligation to attend religious education with the option

[1] Figures refer to the average across the 60 countries participating in the World Values Survey in 2010-2014 (Inglehart et al. 2014). In Germany, the shares are 69, 37, and 33 percent, respectively.

to choose between denominational religious education and “ethics” as a non-denominational subject. A particularly interesting feature of the reforms is that the introduced choice was not one between religious instruction and *no* value-oriented instruction, but rather between denominational and *non-denominational* value-oriented instruction. As a consequence, the reforms allow us to identify the impact of the religious part of instruction, holding the overall exposure to value-oriented instruction constant.

Making use of the staggered adoption of the reform, our empirical model uses the variation in the abolishment of compulsory religious education across states and over time to study reform effects on outcomes in adulthood in two-way fixed effects models. Accounting for fixed effects for each state and birth year, the series of reforms provides plausibly exogenous variation in individuals’ exposure to compulsory religious education that can be exploited in an extended difference-in-differences setting. Effects are identified from differences in adult outcomes between cohorts within the same state that were and were not subject to compulsory religious education, relative to the differences between the same cohorts in other states that did not have reform events at the same time. As we do not observe whether an individual attended religious or ethics classes, we can only identify whether an individual was subject to the reform or not. Therefore, we follow an intention-to-treat (ITT) interpretation of the reform that replaced the compulsory nature of religious education in schools by an option to choose between religious and ethics education. Our analysis captures not only the direct consequences of attending ethics instead of religious education but also potential effects of modernized religious curricula (which followed the introduction of the “competitor subject” ethics) and potential reform-driven cohort-specific changes in social norms surrounding religious education.

We use three datasets, each of which allows us to link religious, educational, and labor-market outcomes of adults to their state and time of schooling in childhood. Our merged dataset combines up to 58,000 observations of adults who entered primary school between 1950 and 2004 from the National Educational Panel Study (NEPS), the German General Social Survey (ALLBUS), and the German Socio-Economic Panel (SOEP).

Our results indicate that schools can indeed affect religious outcomes later in life. We find that the reform that abolished compulsory religious education significantly decreased self-reported religiosity of affected students in adulthood. Conditional on state and birth-year fixed effects as well as individual-level control variables, religiosity of students who were not subject to compulsory religious education is 7 percent of a standard deviation lower on average compared to students who were subject to compulsory religious education. Event-study graphs show that reforming states do not have significantly different trends in religiosity in the years prior to reform compared to non-reforming states.

We find similar reductions in three measures capturing specific religious actions: the personal act of prayer, the public act of going to church, and the formal (and costly) act of church membership. Estimation of time-varying treatment effects indicates that effects on religiosity and personal prayer phase in gradually over time, whereas the effect on church membership is closer to a one-time shift. In a subsample that allows to merge regional information, effects are mostly restricted to predominantly Catholic (rather than Protestant) counties.

Beyond the religious sphere, the reforms also influenced labor-market outcomes and fertility. Economic behavior could be affected because decreased religiosity may promote materialistic orientation, time use may shift from religious to economic activities, and terminated payment of church taxes on labor income may increase incentives to work. Our results show that the reforms indeed led to increases in labor-market participation, employment, working hours, and earnings. Consistent with an emphasis of Christian churches on promoting fertility, the reforms also let to a reduction in the number of children. By contrast, there is no evidence of effects on ethical-value outcomes such as reciprocity, trust, volunteering, and life satisfaction, nor on political-value outcomes such as political interest and leaning, voting, and satisfaction with democracy. Consistent with the counterfactual of alternative value-oriented instruction, the reform-induced decline in religiosity thus did not go hand in hand with a reduction in the measured ethical and political values and behaviors.

Several specification and robustness tests support the baseline result. The reforms are not related to placebo outcomes such as years of schooling, type of school degree, or age of first employment, indicating that the identifying variation is unlikely to capture alternative sources such as other contemporaneous educational reforms. Relatedly, results do not change when conditioning on a range of other educational reforms. Results are robust when restricting the sample to individuals who attend school in counties neighboring each other across state borders and including county-pair fixed effects, so that the identifying variation stems from close geographic areas. Results are also confirmed in a series of additional robustness tests and diagnostic tools of the two-way fixed effects estimator (de Chaisemartin and D'Haultfœuille 2020; Callaway and Sant’Anna 2021; Goodman-Bacon 2021). While generally robust, event study results become less precise in specifications that draw on variation with reduced sample sizes either within studied bins or in smaller state samples.

Our study contributes to four strands of literature. First, studies in the economics of religion have shown the importance of religion and religiosity for economic development and personal outcomes (see Barro and McCleary (2003) and McCleary and Barro (2006, 2019) for a cross-country setting, Becker, Rubin, and Woessmann (2021, 2024) for historical and growth contexts, and Becker and Woessmann (2009, 2018) for the German context). Recent analyses of the determinants of religiosity and the demand for religious services investigate, among others, effects of secular competition (Gruber and Hungerman 2008), economic deprivation (Becker and Woessmann 2013), printing technology (Rubin 2014), the performance of pastors (Engelberg et al. 2016), coping with natural disasters (Bentzen 2019), and an adult religious-value intervention (Bryan, Choi, and Karlan 2021). Several papers study the interrelationship between education systems and religion in different contexts (Brown and Taylor 2007; Glaeser and Sacerdote 2008; Chaudhary and Rubin 2011; Hungerman 2014; Franck and Iannaccone 2014; Meyersson 2014; Becker, Nagler, and Woessmann 2017). To the extent that they analyze effects of education on religion, these papers focus on effects of the level of education in general. Here, we focus on a different aspect – the effect of religious education in the school curriculum – as a more direct means by which schools may affect religiosity.

Second, the political economy of state schooling studies why states take over control of school curricula, modeling aspects such as totalitarian indoctrination (Lott 1999), social cohesion (Gradstein and Justman 2002), and socialization (Pritchett and Viarengo 2015). When state-sponsored non-denominational education systems emerged in most Western school systems during the 19th century, churches fiercely resisted this development (Ramirez and Boli 1987; West and Woessmann 2010).[2] Our results suggest that this resistance was rational in the sense that forfeiting the opportunity to instill religious attitudes in public schools did undermine churches' follower base in the long run.

[2] Bazzi, Hilmy, and Marx (2020) show that a backlash of Islamic schools against mass secular education increased religiosity in Indonesia in the 1970s.

Third, a broad literature in the economics of education studies the impact of different school reforms (e.g., Hanushek 1986; Woessmann 2016). While this literature has traditionally looked at students' academic achievement and later labor-market success, more recent contributions also focus on non-academic outcomes such as personality traits (e.g., Almlund et al. 2011) and soft skills (e.g., Koch, Nafziger, and Nielsen 2015).

Fourth, our study contributes to the literature on long-term effects of school curricula on attitudes and norms and, through this channel, on labor-market behavior. Cantoni et al. (2017) investigate how curriculum changes (textbook reforms) in China promoted political ideologies with long-term impacts on societal values and economic attitudes (e.g., skepticism toward free markets). Hara and Rodríguez-Planas (2025) provide evidence from Japan showing the long-term labor-market consequences of desegregating industrial arts and home economics classes in school, highlighting the impact of gender role education. Arold (2024) demonstrates that changes in the coverage of evolution theory in school curricula can influence students' belief in evolution in adulthood, which, in turn, increases the likelihood of working in life sciences. We contribute to this literature by studying how school curricula reforms can affect religious attitudes in the long run. Furthermore, the reform's effects on labor-market decisions, potentially influenced by a more materialistic mindset and less time spent on religious activities like attending church or praying, demonstrate that schools can influence students' economic behavior later in life.

In the following, Section II provides institutional background on the studied reforms. Section III describes the empirical model and Section IV the data. Sections V and VI present our results on reform effects on religious outcomes and on labor-market outcomes and fertility, respectively. Section VII reports specification and robustness tests, and Section VIII concludes.

II. Institutional Background: Reforms Abolishing Compulsory Religious Education in Germany

With the staggered abolishment of compulsory religious education across states and over time, Germany provides a unique setting to study the effects of compulsory religious education. While this setting is specific, it allows us to derive more general lessons about the role of schools in shaping students' values and beliefs.

Historically, most Western school systems have their roots in religious education, with churches playing a central role. During the 19th century, the emergence of state-sponsored secular education systems often met with fierce resistance from churches (Ramirez and Boli 1987; West and Woessmann 2010). This historical context has led to wide variation in how different countries incorporate religious education into their school systems. For instance, countries such as the United States and China maintain a strict separation of church and state, forbidding religious education in public schools. In contrast, other countries such as Italy and the Netherlands offer religious education as an elective subject, whereas others such as Saudi Arabia and Pakistan offer it as a compulsory subject. In Germany, religious education was a compulsory part of the curriculum in nearly all states after World War II. Between 1972 and 2004, state reforms gradually introduced the option for students to choose between religious education and a non-denominational "ethics" course, leading to significant changes in the landscape of religious instruction.

Historical background. There are a couple of historical milestones that led to the profound role of religious education in the German school system. The Prussian School Supervision Act of 1872 was at the center of the *Kulturkampf* ("culture battle") between the Prussian Empire under Bismarck and the Catholic Church during the 1870s. This legislation abolished the churches' control of the Prussian primary school system, putting the state in charge of school organization and curricula with the aim to provide a value-neutral education. However, religious education remained a regular school subject. During the Weimar Republic (1918-1933), there was some debate about whether religious education should be offered in schools at all, but in the end the supporters of religious education prevailed.

In Nazi Germany, the role of religious education was formally strengthened by the *Reichskonkordat* (Concordat between the Holy See and the German Reich) closed between Hitler and the Pope. It assigned Catholic religious education the role of a regular school subject. In reality, however, the Nazi regime did not adhere to these rules. A prominent example is the so-called *Kreuzkampf* ("cross battle") in the region of Oldenburg Münsterland in 1936, where the regional minister for education and church gave the order to take away all crosses, pictures, and other religious symbols from schools (*Kreuzerlass*). After protests by civil society that were famously supported by Bishop Clemens August Graf von Galen, the order was partly taken back, and crosses were again allowed to be placed in schools in this region. Referring back to Bishop Wilhelm Emmanuel von Ketteler during the *Kulturkampf*, Bishop von Galen strongly emphasized the crucial role of the church's (rather than the state's) grip of schools for the children's socialization and thus for church followership in the long run.

Post-war situation. Against the backdrop of the Nazi takeover of schools and in close agreement with the Allied forces, the Constitution (*Grundgesetz*) of the Federal Republic of Germany, enacted in 1949, establishes in Article 7 that religious education is a regular subject in schools.[3] This makes it compulsory that schools provide religious education, which is explicitly

[3] Article 141 states that this clause does not apply to states that had had a different state law on the issue in place on January 1, 1949, which effectively granted an exemption to the two city states of Berlin and Bremen.

to be taught in accordance with the principles of the respective religious community. Before reforms that started in the 1970s, enrollment in religious education classes was the default for all students from first to final grade. Parents (and adolescents aged at least 14)[4] could formally request non-participation if the child was not baptized, but this was a rare exception (Havers 1972).

Religious education is taught by confession (Catholic or Protestant).[5] Based on contracts between the states – who are responsible for education policy – and the churches, the content is not restricted to "religious studies" but is based on dogmatic elements bound to the respective denomination and its doctrinal theology (Lott 2005). Religious-education teachers are paid by the states and work as state employees but must be chosen and certified by the respective church (receiving the Catholic *Missio canonica* or the Protestant *Vocatio*). The importance given to the subject in Germany's school curricula is illustrated by the fact that during their school careers, high-school graduates were exposed to 1,000 hours of religious education – compared, e.g., to 240 hours of physics education (Havers (1972) based on the Baden-Wuerttemberg curriculum).

The reforms. From the 1970s onwards, eight of the eleven West German states terminated the compulsory nature of religious education (Helbig and Nikolai 2015). Parents could now choose between religious education and a newly introduced subject, usually called "ethics",[6]

[4] In Bavaria and the Saarland, students had to get parental permission until age 18.

[5] Parents can choose the denomination, which in practice is very uniform in most schools because of the strong regional concentration of confessions in Germany. As soon as the number of students from the respective minority denomination exceeds a very low threshold (which varies slightly across states, e.g., five students in Bavaria), schools are required to provide religious education in both denominations. Thus, even in mainly Catholic areas or schools, Protestant students attend Protestant religious education, and vice versa.

[6] Depending on the state, the alternative subject is called "ethics", "philosophy", "values and norms", or "humanistic life skills".

which provides an alternative form of value-oriented instruction that was non-denominational. Importantly, the reform was applicable to all schools, both public and private. As indicated in Table 1, Bavaria was the first state to enact the reform in 1972 and Hamburg and North Rhine-Westphalia were the last in 2004 (see also map in Online Appendix Figure A1). Relying on state-wide variation in Germany, our empirical analysis thus draws on variation across a limited number of eleven states, eight of which changed treatment status. We account for the small number of states when computing standard errors.

Two reasons are generally put forward for the reform introduction, one on the initiative of the churches and the other of the schools (Lott 2005; Havers 1972). First, in 1968 the student movement at German universities started to challenge tradition and conservatism of the parental generation. When an increasing number of high-school students in urban areas decided to opt out of religious education to enjoy free time, the churches reacted by pushing for a compulsory alternative subject that students are obliged to attend instead, to make opt-out less attractive.[7] Consistent with the initiation by the churches, Bavaria – which in many dimensions is generally viewed as the most conservative among the West German states – was the first to enact the reform. Second, schools also welcomed the reform, as rising opt-out meant that they were increasingly faced with organizational challenges to comply with their supervisory duty for students during school hours.

Interestingly, the rollout of the reform across states was orthogonal to the political leaning of and changes in the state government. As is obvious from column 4 of Table 1, four reforms were

[7] To ensure that results are not driven by reactive reforms to early opt-out during the student movement, in robustness tests we show that results are robust to leaving out early reforming states (see Section VII) and to restricting the sample to rural areas (see Section V).

implemented by a right-of-center Christian Democrat (CDU/CSU) government and four by a left-of-center Social Democrat (SPD) government. The time pattern is literally alternating between the two camps. Furthermore, for each single reform, the party that was in power in the legislative period of the reform had already been in power in the prior legislative period, implying that no reform was implemented after a change in government (column 5). Similarly, the reform rollout was not driven by the size of a state, as the two largest states (Bavaria and North Rhine-Westphalia) were the first and last to implement the reform, respectively. These patterns make it unlikely that the reforms were due to political trends or shocks.[8]

Reforms were also not due to specific religious trends in the reforming states. Administrative data show that there was no specific trend in the memberships in the Catholic and Protestant Churches in the years before and after the reforms were introduced in the respective states (see Online Appendix Figure A2).[9]

Typically, the state parliaments (*Landtag*) would pass the resolution that institutes the reform and announce it within the twelve months preceding the next school year. Thus, the time lag between the announcement and the implementation of the new policies tends to happen during the preceding school year, which makes anticipation effects unlikely.

Consequences of the reform. There are three main consequences of the reform that might give rise to overall long-term reform effects. First, individual students could now attend ethics instead of religious education. Unfortunately, there is no administrative data on how many

[8] The result that we do not find reform effects on political outcomes (Section V) also speaks against the existence of political shocks coinciding with the timing of the reforms.

[9] Unfortunately, administrative church-membership data are not available by members' year of school entry or year of birth, so they cannot be used in our difference-in-differences model with cohort fixed effects. They only allow to show that there were no specific overall religious trends in states before they implemented the reforms.

students chose ethics in the years right after the reform implementation. Initially, the number was potentially small, particularly in rural areas. Reports dating back to the reform years suggest that in some places, schools could not find staff to teach ethics classes (Lott 2005). Selective data in later years point towards a modest decline in the number of students attending religious education. For example, data from North Rhine-Westphalia, which implemented the reform in 2004, reveal that it took 17 years for the share of students opting for ethics to increase from 6 to 21 percent.[10] This suggests that the full implementation of the reform, including the recruitment and training of teachers, and changes in social norms regarding the choice of subjects took a considerable amount of time. Current data indicate that 73 percent of students in West German public schools attend religious education and 20 percent ethics or related substitute subjects (Kultusministerkonferenz 2021).[11] Thus, only about one fifth of students are affected in the sense that they themselves attend non-denominational ethics rather than denominational religious education.

Second, the subject ethics acted as a newly emerged competitor to religious education, putting religious education curricula under modernizing pressure. Studying curricula before and after the reform, we find that religious education curricula tended to change after the reform. As one example, Online Appendix Table A1 provides an overview of curricula in Bavaria. The 1967 pre-reform curriculum of Catholic religious education never even mentions non-Christian

[10] The data are available at https://www.schulministerium.nrw/amtliche-schuldaten (accessed July 21, 2024).

[11] The number for religious education includes all religions (including Islam and Judaism) as well as denomination-overarching religious education; 33 percent of West German students attend Catholic and 34 percent Protestant religious education. 7 percent of students attend neither religious education nor ethics, which mostly refers to primary schools in North Rhine-Westphalia, where ethics is not yet ubiquitously implemented in all schools, and to secondary schools in Schleswig-Holstein, where religious education/ethics classes of consecutive grades can be offered combinedly in one grade so that students in the other grade currently do not attend it.

religions. By contrast, the 1979 post-reform curriculum has a whole section in grade 9 designated to learning about other religions. The pre-reform curriculum puts more focus on guiding students towards Christianity, whereas the post-reform curriculum emphasizes guiding students towards responsible and informed behavior defined by Christian values.[12] As an example of a late reformer, the 1999 pre-reform syllabus in North Rhine-Westphalia focuses on religious values to guide students, whereas the 2014 post-reform syllabus emphasizes helping students develop their own values based on religion and faith. Overall, the comparison of curricula points to a decrease in the practice of prayers and literal interpretation of the bible after the compulsory nature of religious education was abolished.

Third, the reform may have changed perceived social norms since it was now officially approved that alternatives to religious education exist, indicating an apparent acceptance in society not to be religious. This could have changed religious views even for students who still attended religious education classes. To the extent that these effects are specific to the affected student cohorts rather than to the population overall, they would be captured by our empirical approach. For example, the norm that it is generally accepted by society to opt out of religious activities may be much more salient for students in cohorts where peers opt out of religious education than for older cohorts who have already left school at the time of the reform and thus do not have peers who opted out.

[12] In the syllabus of the new subject ethics in Bavaria, religion of any kind is completely absent (except for one reference to Christian values). The focus is on enabling students to work out answers to ethical questions by themselves in open discussions based on real-life situations. After the curricular changes in religious education, ethics and religious education have a lot of common topics and focus both on conveying values; the major difference is the final justification of values taught in class (Schwoerbel 1985).

Any identified long-term reform effects are therefore likely to stem from a combination of declining attendance in religious education, adapting the content of religious education classes to the new competitor subject's content, and changing social norms. We therefore expect that the reform does not only affect students who chose to attend ethics classes, but also students who continued attending religious education classes. In addition, the description makes clear that several elements of the enactment of the reform were gradual rather than abrupt, leading to an expectation that reform effects may phase in rather than happen discontinuously.

III. Empirical Model

To estimate the effect of the abolishment of compulsory religious education on adult outcomes, we make use of the different timing of reform events across German states. The staggered adoption allows us to estimate reform effects in a generalized difference-in-differences setting with varying timing of treatment. The key idea is that states without a reform in a certain year act as counterfactuals for states with a reform in that year, after accounting for time-invariant differences between states and national differences between years. Our baseline two-way fixed effects model with state and cohort fixed effects models reform effects as immediate and permanent shifts in outcomes:

$$R_{i,s,t} = 1(t_{i,s} \geq t_s^*)\beta_{Reform} + \boldsymbol{X_i}\beta_{Controls} + \mu_s + \lambda_t + \varepsilon_{i,s,t} \qquad (1)$$

The adulthood outcome (e.g., religiosity) $R_{i,s,t}$ of individual *i* who started primary school in state *s* and year *t* is a function of an indicator term $1(t_{i,s} \geq t_s^*)$ that equals one if the primary school entry year $t_{i,s}$ of individual *i* in state *s* is larger than or equal to the year of reform t_s^* in state s.[13] Apart from state and cohort fixed effects (μ_s and λ_t, respectively), a vector of individual-level controls $\boldsymbol{X_i}$ and an error term $\varepsilon_{i,s,t}$ complete the model. Throughout the paper, standard errors are clustered at the state level. We report *p*-values for two clustering methods. The first one is the standard clustering approach which accounts for potential correlation of error terms across years within states and provides conservative inference if reform timing is random (Athey and Imbens 2022; Abadie et al. 2023). The second one is the wild cluster bootstrap approach suggested by Roodman et al. (2019) which provides asymptotic refinement by accounting for the limited number of clusters given by the West German states.[14]

The parameter of interest, β_{Reform}, depicts the intention to treat (ITT) effect that captures the overall effect of the reform, that is, the effect of being offered the choice between attending religious education or ethics. The treatment effect is identified from changes in adult outcomes across cohorts within the same state that were and were not affected by the reform, relative to the same changes in other states without reform events at the same time.

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[13] Coding individuals as treated only if the reform had been implemented at their primary school entry is our preferred categorization because it starts with the first cohort that could have avoided religious education completely by choosing the non-denominational alternative from the first grade onwards. The fact that students who were already beyond primary school entry in the year of reform introduction are categorized as exposed to compulsory religious education even if they received some exposure to the reformed curriculum might mean that our baseline specification underestimates the true effect. In robustness analyses, we confirm results in a dosage specification where treatment is defined as the share of compulsory school years that an individual spent in the reformed system, as well as in a specification that defines treatment by entry into secondary school (see Section VII).

[14] We use Webb weights and 9999 replications. The approach is more conservative than the Cameron, Gelbach, and Miller (2008) approach to wild cluster bootstrapping which tends to yield substantially lower *p*-values throughout (not shown).

The variation in the timing of reforms across states provides us with plausibly exogenous variation in individuals' exposure to compulsory religious education. The main identifying assumption is that the exact timing of the reform is as good as random (e.g., Athey and Imbens 2022; Borusyak, Jaravel, and Spiess 2024). This seems plausible given the idiosyncrasies of the reform processes in the German federal political system described above. For example, the reform rollout did not indicate any political trend, with implementations alternating between right-wing and left-wing governments and no reform enacted in the first legislative period after a change in government (see Table 1).

One way in which the identifying assumption could be violated is the existence of other school reforms that happened simultaneously. However, the timing of the religious-education reform is very peculiar, and we are not aware of other reforms with even vaguely similar patterns of timing across states. In fact, results are robust in specifications that control for a range of other education reforms (see Section VII). An additional way to test this concern is to estimate reform effects on non-religious school outcomes such as degree completion or years of schooling. The religious-education reform did not affect any other subjects and did not substitute religious education by classes prone to enhance achievement in other curricular subjects. As we thus do not expect any first-order effects of the religious-education curriculum on other school outcomes, such analysis can be interpreted as a placebo test that, if it failed, would indicate the possibility of simultaneous school reforms.

In a further specification that aims to compare observations that are as similar as possible in the absence of treatment, we restrict the sample to individuals living in counties that are directly at the border to a different state. In this specification, we can additionally include fixed effects for each pair of counties that are next to each other on either side of a state border, thereby further reducing geographic heterogeneity in the identifying variation.[15]

In addition, it is an attractive feature of the event-study approach that including a trend variable relative to the reform $(t_{i,s} - t_s^*)$ constitutes a falsification test of the identifying assumption of randomness in reform timing (keeping the assumption of time-invariant treatment effects for now):

$$R_{i,s,t} = 1(t_{i,s} \geq t_s^*)\beta_{Reform} + (t_{i,s} - t_s^*)\beta_{Trend} + \boldsymbol{X_i}\beta_{Controls} + \mu_s + \lambda_t + \varepsilon_{i,s,t} \qquad (2)$$

The parameter β_{Trend} captures how the average outcomes change in reforming states relative to non-reforming states. Rejecting the null hypothesis that $\beta_{Trend} = 0$ would indicate that the timing of the reform may not be as good as random.

While specifications (1) and (2) model the reform as an immediate and permanent shock, the discussion in Section II suggests that reform implementation may have been gradual rather than abrupt. As a result, the ITT effect may be expected to set in gradually over cohorts. To disentangle reform effects that affect all cohorts equally from those that increase for subsequent cohorts, we extend specification (2) by an interaction of the reform indicator $(t_{\mathrm{i},s} \geq t_s^*)$ with the trend term $(t_{\mathrm{i},s} - t_s^*)$:

$$\begin{aligned} R_{i,s,t} = 1(t_{i,s} \geq t_s^*)\beta_{Reform} + (t_{i,s} - t_s^*)\beta_{Trend} + 1(t_{\mathrm{i},s} \geq t_s^*)(t_{\mathrm{i},s} - t_s^*)\beta_{Reform*Trend} \\ + \boldsymbol{X_i}\beta_{Controls} + \mu_s + \lambda_t + \varepsilon_{i,s,t} \qquad (3) \end{aligned}$$

[15] Counties (*Landkreise* and *kreisfreie Städte*) in Germany are substantially smaller than in the US. There are 325 counties in West Germany with a mean population of about 200,000 inhabitants (median about 150,000).

In this specification, the parameter on the interaction term, $\beta_{Reform*Trend}$, captures the average annual change in reforming states after the reform, relative to the change in the same states prior to the reform (and relative to non-reforming states). The parameters β_{Reform} and $\beta_{Reform*Trend}$ reveal whether the reform affects outcomes as immediate permanent shocks or gradually over time, respectively (Lafortune, Rothstein, and Schanzenbach 2018). The parameter β_{Trend} now captures differential pre-trends between treatment and control states.

To lift the assumption of linearity in pre- and post-trends of the parametric specifications and allow for flexible reform effects over time, we also estimate non-parametric models of the effects of a reform in year t_s^* on outcomes k years before and after the reform:

$$R_{i,s,t} = \sum_{k=-19}^{20} 1\left(t_{i,s} = t_s^* + k\right)\beta_k + \boldsymbol{X_i}\beta_{Controls} + \mu_s + \lambda_t + \varepsilon_{i,s,t} \tag{4}$$

Effects, captured by the parameter vector β_k, are estimated relative to the excluded category $k = 0$. To smooth the numbers of observations in the sample across years, we group observations together in bins of five years each. We visualize the results of this non-parametric specification in an event-study graph.

The two-way fixed effects model assumes homogeneity in treatment effects. We implement the estimators suggested by Sun and Abraham (2021) and Callaway and Sant'Anna (2021) and use the diagnostic tools suggested by de Chaisemartin and D'Haultfœuille (2020) and Goodman-Bacon (2021) to show that our results are not contaminated by this assumption.[16]

[16] Furthermore, excluding covariates does not change our qualitative results, indicating that cohorts with different covariates are unlikely to react differently to the reform (see Section VII).

IV. Data

Our treatment variable indicates whether a given German state has abolished compulsory religious education at a given point in time. The coding of reform events, indicated in Table 1, is taken from Helbig and Nikolai (2015). We define an individual as treated if the reform that replaced compulsory religious education by the choice between ethics and religious education had been enacted in the year that the individual entered primary school.

To estimate reform effects on individuals' adult outcomes, we assemble three individual-level datasets that provide a broad picture of religiosity in Germany and are each drawn to be representative for the German adult population (see Online Appendix A for details): the adult cohort of the National Education Panel Study (NEPS), the German General Social Survey (ALLBUS), and the German Socio-Economic Panel (SOEP). NEPS is focused on the educational sciences and provides a panel of over 12,000 adults observed between 2007 and 2016. ALLBUS is focused on the social sciences and provides repeated cross-sections of over 15,000 adults observed between 1980 and 2016. SOEP is focused on economics and the social sciences and provides a panel of over 30,000 adults observed between 1984 and 2017. To study a range of religious (and other) outcomes in adulthood and maximize statistical power, in our main analysis we use all three datasets and merge them together. Depending on the outcome under study, our combined estimation sample includes up to 58,000 observations. While the datasets are representative, they are small relative to the adult population of the applicable birth cohorts in Germany so that the probability that an individual appears in more than one dataset is minimal.[17]

[17] Given the sample sizes of NEPS (12,281 individuals), ALLBUS (15,924 individuals), and SOEP (30,498 individuals) and the German population of the primary-school entry cohorts between 1950 and 2004 (approximately

All three datasets allow us to observe individuals' state and year of primary school entry, which is the basic data requirement of our evaluation approach. That is, each dataset allows us to link the religiosity of individuals in adulthood to their state of schooling in childhood, even if they migrated to other states in-between.[18] Our sample consists of all individuals who entered primary school in West Germany between 1950 and 2004. We exclude individuals who entered primary school before 1950 because they did not have their entire schooling career in the Federal Republic of Germany (founded in 1949). Primary school entry by 2004 ensures that individuals have turned at least 18 years old by 2016/17.

Our main outcome of interest is self-reported religiosity, which we interpret as a comprehensive measure describing an individual both believing in religious content and showing religious belonging by living a religious life in public (McCleary and Barro 2019). The three other religious outcome measures capture different ways in which individuals articulate their religiosity in specific actions: the personal act of prayer, the public act of going to church, and the formal act of church membership. The latter act is also directly economically relevant, as church membership in Germany is automatically related to paying church taxes, which are levied as a surcharge on income tax and are collected for the churches by the tax authority as part of general income taxation. Paying these church taxes is thus not voluntary and can only be avoided by leaving the church.

60 million, measured in 2010), the sum of the probabilities that an individual appears in two or three surveys is approximately 0.0000293 percent: P(two or three surveys) = P(NEPS)×P(ALLBUS) + P(NEPS)×P(SOEP) + P(ALLBUS)×P(SOEP) + P(NEPS)×P(ALLBUS)×P(SOEP) = 2.93×10^{-7}.

[18] If available directly, we use information on the year and state of primary school entry. If not, we use the year and state of birth and assume that individuals enter primary school six years later in the same state.

As the religious outcome variables are elicited with varying numbers of answer categories in the different datasets (see Online Appendix Table A2 for details), we standardize the religious measures within each dataset before merging the three surveys together and include dataset fixed effects throughout.[19] If a measure is observed multiple times per individual in a panel dataset, we use the most recent available observation on any given variable and include survey-year fixed effects (stored separately for each question for each individual) throughout.

The three datasets also provide batteries of measures of labor-market outcomes, as well as of ethical-value, political-value, and educational outcomes. Control variables include gender, migration status, and mothers' and fathers' education. Table 2 provides descriptive statistics for the merged dataset. Roughly one third of observations are treated by the reform, i.e., they entered primary school after compulsory religious education had been abolished. Column (7) provides information on respondents' age for each variable. The specific age range varies slightly depending on the outcome variable due to the combination of different surveys. For religiosity, our main outcome variable, respondents' ages range from 18 to 70 years.

The final column of Table 2 shows results of a regression of an outcome-specific indicator variable on the reform indicator and basic control variables. Item availability seems mostly unrelated to our analysis. While significant reform coefficients on item availability are found for nine of the 27 outcomes, their small magnitude and the lack of correlation between item availability and main outcome effects suggest that sample selection does not drive our findings.

Finally, Online Appendix Table A3 shows descriptive statistics of background characteristics by treatment status. By construction, the individuals exposed to the reform are

[19] To document that results are not driven by the standardized merging, robustness checks also show results for each of the three datasets separately (see Section VII).

from later birth cohorts, implying that they are also more likely to display characteristics that have become more common over time in the German society, such as a higher probability of migration background and more educated parents. In the empirical analyses, we account for trends in the German society by including year fixed effects and conditioning on these characteristics. As year fixed effects are not accounted for in the raw comparisons, individuals exposed to the reform are more likely to have parents with migration background and higher education. Other, less time-variant variables such as the gender ratio hardly differ between groups.

V. The Effect of Abolishing Compulsory Religious Education on Religiosity

This section reports our baseline results on effects of the studied reform on religious outcomes. Section VI turns to effects on non-religious outcomes, and Section VII provides results of specification and robustness tests.

Our results show that the abolishment of compulsory religious education decreased the religiosity of affected students in adulthood. The event-study graph of Figure 1 indicates that individuals who entered school after the reform report significantly lower levels of religiosity.[20] Visual inspection suggests that reform effects increase for subsequent cohorts, consistent with a phase-in of effects due to gradual reform implementation. An omnibus hypothesis test that the post-event effects are jointly zero is rejected at the 1 percent level. By contrast, the test does not

[20] Online Appendix Table A4 provides the non-parametric regression results underlying this figure.

reject that the pre-event effects are jointly zero, indicating that reforming states had not been on different trends from non-reforming states prior to the reform.[21]

The parametric estimation in the first column of Table 3 indicates that reform exposure while being in school decreases religiosity in adulthood by 7 percent of a standard deviation on average. For a straightforward indication of the magnitude of this effect, we can express religiosity as a dummy variable. The reform reduces the likelihood that a person is (rather or very) religious by 2.9 percentage points (independent of whether estimated by linear probability or probit model; see Online Appendix Table A5), compared to an average incidence of 52.4 percent in our dataset. The incidence of being very religious is reduced by 2.2 percentage points (average incidence 10.9 percent).

An alternative way to illustrate the magnitude of the reform effect are persuasion rates (DellaVigna and Gentzkow 2010), i.e., the share of religious people who lose their religiosity due to the reform. We follow Cantoni et al. (2017) in calculating conditional persuasion rates by predicting the fraction of individuals who would be religious in the absence of the reform from our model. The resulting persuasion rates amount to 6 percent for (at least rather) religious people and 20 percent for very religious people, which is in the range of estimates found on various attitudinal outcomes for a Chinese curricular reform in Cantoni et al. (2017).

The reform also led to significant reductions in the three measures of specific religious actions (columns 2-4 of Table 3). The standardized effects are of a similar magnitude to overall religiosity. The reform reduces the personal act of prayer by 5 percent of a standard deviation

[21] In the graph, the apparent small insignificant downward trend to the left of the event may reflect that these cohorts were partly exposed to the reform in later grades. Consistently, a dosage specification (see Section VII) that takes this into account yields slightly larger estimates than the baseline specification.

(marginally significant), the public act of going to church by 7 percent, and the formal act of church membership by 8 percent. The respective event-study graphs are shown in Panel A of Figure 2.

To test whether reforming states are on a general time trend that is different from non-reforming states, the odd columns of Table 4 add a linear trend relative to the respective reform event to the model. There is no significant differential trend for religiosity or any of the religious-action outcomes, in line with the assumption that the timing of reform events is as good as random.

The even columns of Table 4 report results of the rather demanding specification with time-varying treatment effects that allows for both a shift term of the reform, a relative trend, and an interaction between the two. Confirming the graphical depiction, results indicate that the reform effect on religiosity phases in gradually over time: religiosity decreases by 0.013 standard deviations on average per year in reforming states after the reform, relative to the average change in the same state prior to the reform. A similar gradual treatment effect emerges for personal prayer. By contrast, the effect on affiliation with a religious community is mostly captured by a one-time shift. This may be related to the fact that church membership in Germany implies the requirement to pay church taxes: Individuals who were exposed to the reform even in the early years after a state's implementation do react by leaving their church as adults to avoid paying church taxes, whereas their subjective religiosity and prayer are not yet as strongly impacted. For church-going, the separate estimates in this specification are too imprecise to distinguish between a one-time shift and gradual phasing-in.

Treatment effects on religiosity are very similar for women and men (Panel A of Table 5). The same is true for church affiliation. By contrast, treatment effects on prayer materialize only for women but not men, whereas treatment effects on church-going are larger for men. Results in Panel B show no strongly differential pattern for individuals who went to schools in rural and urban areas (available for a limited number of observations in RemoteNEPS). The effect is somewhat larger (although less precisely estimated) in urban areas for religiosity, larger in rural areas for prayer, and similar for affiliation. When distinguishing individuals' school county by the majority confession (Panel C), results are driven by Catholic areas, where religiosity tended to be more deeply engrained. In another subset of observations and outcomes (available in ALLBUS and SOEP) where we can link individuals to the denomination of their parents (Panel D), the effect on church-going also appears to be restricted to individuals with all-Catholic parents. By contrast, while estimates are somewhat imprecise, the effect on religious affiliation is in fact larger for individuals whose both parents were Protestant.[22]

In contrast to the effects on religiosity and religious actions, we do not find evidence that the reform affected various value outcomes. In particular, there are no significant treatment effects on a series of measures of ethical-value outcomes including reciprocity, trust, risk preference, volunteering, and life satisfaction (Panel A of Table 6). The absence of treatment effects on these ethical outcomes is consistent with the fact that the post-reform counterfactual to compulsory religious education in our setting is not the option to opt out of value-oriented classes, but rather a choice between two types of value-oriented classes that are either denominational or not. Apparently, attending the non-denominational subject ethics does not lead to lower levels of the different ethical-value outcomes compared to the subject religious education. Similarly, there is

[22] Unfortunately, the information on county-level identifiers that allow to merge administrative data on religious denominations in the county (available in the restricted RemoteNEPS environment) and the information on the religious denominations of parents (available in ALLBUS and SOEP) come from different data environments that cannot be merged in one analysis.

no evidence of effects on political-value outcomes such as political interest, satisfaction with democracy, or left-right voting patterns (Panel B).

VI. Effects on Labor-Market Outcomes and Fertility

Beyond the religious sphere, the reform may have affected economic behavior and outcomes for at least three reasons. First, according to Christian values, the decrease in religiosity may have promoted materialistic orientation. For example, the bible quotes Jesus as saying, "It is easier for a camel to go through the eye of a needle than for someone who is rich to enter the kingdom of God." (Mark 10:24-27, Luke 18:24-27). Second, the reduction in time used for various religious actions may have induced a substitution effect towards economic activities (Barro and McCleary 2003; Gruber and Hungerman 2008). Third, together with income taxes, church taxes are automatically deducted from employees' salaries and forwarded to the churches in Germany. Therefore, leaving the church means not paying church taxes anymore in Germany, resulting in a reduction of the tax rate on labor income and thus an increase in work incentives.[23]

Results show that the reform indeed had positive effects on labor-market outcomes. The probability to participate in the labor market increases by 1.5 percentage points (column 1 of Table 7), compared to a mean of 82 percent, and the probability to be employed by 2.3 percentage points (column 2; mean 78 percent). Among those employed, earnings increase by 5.3 percent (column 3). While this is a relatively large effect, it is relatively imprecisely estimated, suggesting that the magnitude should be interpreted with caution. Working hours rise

[23] For instance, exemplary calculations indicate that for an average gross income of €37,000, the saved tax would amount to €449 per year. See, Handelsblatt, 'Church tax: what are the pros and cons of leaving the church?', https://www.handelsblatt.com/arts_und_style/kirchensteuer-was-sind-die-vor-und-nachteile-eines-kirchenaustritts-2024/25300246.html (accessed July 21, 2024).

by 0.6 hours per week (column 4), compared to a mean of 35.6 hours. Conversely, the probability of engaging in part time work falls by 2.0 percentage points (column 5, mean 20 percent).

To put the relatively large estimates of labor-market effects into perspective, we can consider the time one could save by no longer participating in religious activities. Consider an active religious person who goes to church once per week and prays every day. If we assumed that attending mass (including commuting) takes about 90 minutes and a daily prayer about 5 minutes, this person would experience time savings of about two hours per week by terminating these activities. If this time was fully substituted by working in the labor market, this could fully account for the 5-percent increase in earnings (2/40 weekly hours = 5 percent). However, based on observed religious activity, the actual time savings for a religious person are likely substantially smaller on average, at about 30 minutes per week.[24] While this is in line with the increase in weekly work hours of 0.6, it suggests that time savings are not the only channel that leads to the earnings increase.

The reform also affected fertility. Increased work time and reduced part-time work may have reduced time to take care of children. More generally, reduced religiosity may also have had direct effects on fertility, particularly because the biblical proclamation to "be fruitful and multiply" (Genesis 1:28) has been strongly promoted by Christian churches. Our results show

[24] In our data, persons who consider themselves as being "religious" or "very religious" on average report to go to church between "1-3 times per month" and "several times a year". They on average report to pray "once per week". Together, this would lead to a rough estimate of 30 minutes of weekly religious activity.

that the reform indeed decreased the number of children by 0.09 children per respondent (column 6, mean 1.4 children).[25]

Panel B of Figure 2 shows that the results on labor-market outcomes and fertility also hold in event study specifications. Importantly, the different labor-market/fertility effects do not differ by gender (see Online Appendix Figures A3 and A4).

Consistent with the effect heterogeneities on religious outcomes, the effects on some of the labor-market outcomes are more pronounced in predominantly Catholic areas (Online Appendix Table A6). While separate sample sizes for Protestant and Catholic counties are relatively small, introducing some imprecision, effects on employment and earnings are stronger in – and statistical significance is restricted to – Catholic regions. These denominational differences are consistent with likely larger time savings due to reduced religious activities in Catholic areas.

Overall, the results for labor-market outcomes and fertility suggest that the reform impacted people's lives beyond the religious sphere.

VII. Specification and Robustness Tests

This section reports tests of challenges to our identification strategy, of the robustness of our results, and of properties of the two-way fixed effects estimator.

Effects on non-religious school outcomes. For our identification strategy to hold, the abolishment of religious education should not be accompanied by other educational reforms or other state-cohort-specific events with the same timing structure. As meaningful other school reforms should leave traces in general educational outcomes, one way to test this is to estimate

[25] In a previous version, we also reported results on several attitudes towards gender and family roles, as well as marriage, but results are too imprecise to warrant definitive conclusions (see Panel A of Table 7 in Arold, Woessmann, and Zierow (2022)).

treatment effects on non-religious educational outcomes. Results show that the reform is not significantly related to the non-religious educational outcomes in our datasets, namely years of schooling, the type of school degree, or the age of first employment (Table 8). As the studied reform did not lead to a change in schooling hours and or in the structure or content of the non-religious subjects, we interpret this as a placebo test that is in line with our identifying assumption. This interpretation is also consistent with the non-existence of effects on ethical-value and political-value outcomes (see Section V above).

Border specification with county-pair fixed effects. To reduce the possible incidence of unobserved differences, we can restrict the analysis to individuals from geographically close and thus arguably highly similar counties. For a subset of individuals in the NEPS data, we observe individuals' county of schooling. This allows us to restrict the sample to pairs of counties separated by a state border (see Online Appendix Figure A5). Additionally, in this specification we can include county-pair fixed effects for each pair of neighboring counties that is divided by a state border (Dube, Lester, and Reich 2010; Bentzen, Pizzigolotto, and Sperling 2025). The identifying variation is thus restricted to a comparison of pairs of counties on either side of the respective state border. In this smaller sample, the treatment effect on religiosity remains highly significant and increases in size to 0.16 standard deviations (Table 9). The same is true for prayer, whereas the effect on affiliation does not hold in this specification.

To rule out bias from sorting of students across states in response to the reform, we can also estimate the border specification by assigning the treatment status based on the state of birth rather than the state of schooling. The correlation coefficient between state of birth and state of schooling in our sample is 0.88. Results are very consistent (Online Appendix Table A7), with negative effects on religiosity and (marginally significantly) on prayer and no significant effect on religious affiliation. The absolute values of the point estimates for religiosity and prayer are numerically smaller compared to the border specification based on the state of schooling, but larger compared to the main specification. These findings may reflect some degree of endogenous sorting of students across states, or just attenuation when using state of birth rather than state of schooling. In any case, they do not significantly alter our main conclusions.

Additional robustness analyses. A series of additional tests confirm the robustness of our findings to variations in control variables, treatment specifications, outcome measures, inference, and estimation samples.

To ensure that the estimated reform effects do not pick up effects of other education reforms, we include controls for a range of other reforms. These include reforms of the length of compulsory schooling (e.g., Pischke and von Wachter 2008; Cygan-Rehm 2022), of the duration of the highest-track school (“G8/G9 reform”, e.g., Andrietti and Su 2019; Marcus and Zambre 2019), and of whether philosophy, sexual education, and political education, respectively, are taught in school (see Helbig and Nikolai 2015). Results are robust to controlling for these other education reforms (Online Appendix Table A8).

Similarly, results hardly change in specifications that exclude all covariates (Online Appendix Table A9). This insensitivity to consideration of demographic and family-background characteristics is consistent with the assumption of homogeneous treatment effects and reduces concerns of remaining bias from non-measured factors.

A couple of robustness checks relate to the coding of treatment. First, we replace the dummy variable indicating reform exposure by a dosage variable measuring the share of school years out of the total compulsory school years in which individuals were exposed to the reform. Results are robust and point estimates become larger for each of the religious outcomes (Online

Appendix Table A10), as expected if the conservative baseline indicator coding produces an underestimate of the true effect.

Second, we alternatively define a student to be exposed if the reform was in place at the time of entry into secondary (rather than primary) school. Secondary school starts at fifth grade in Germany, when students are ten years old, i.e., after four years of primary school. Results are very similar (Online Appendix Table A11). This could reflect that lessons in primary school may not be the main drivers of religious attitudes and that the primary reform impact stems from experiences during secondary school. Alternatively, however, the relatively short time span of four years between primary and secondary school entry, as well as the difference's relevance for our estimation only during the transition period, may not be sufficient to show discernible differences between the two groups.

To ensure that the results for our separate outcome measures are not spurious, we create indices of outcome groups (Anderson 2008). We combine the measures in each of our five groups of outcomes as presented in Tables 3 and 6-8 – religious outcomes, labor-market and fertility outcomes, ethical values, political values, and educational outcomes – into one index, respectively. Each index is standardized, constructed as the equally weighted average of the standardized values of its underlying measures (Kling, Liebman, and Katz 2007). Results on the indices strongly confirm our baseline results: The reform significantly affects religious outcomes and labor-market and fertility outcomes, but not ethical-value, political-value, and educational outcomes (Online Appendix Table A12). The effects (in absolute terms) are 8.7 percent of a standard deviation on the index of religious outcomes and 5.8 percent on labor-market and fertility outcomes. Consistent with measurement error in the separate measures, inference gets considerably more precise with the indices; estimated effects on religious and labor-market/fertility outcomes are each significant at the 1 percent level.

This high level of precision also implies that inference remains significant when adjusting for multiple hypothesis testing across the five outcome indices. In fact, even with the highly conservative Bonferroni family-wise error-rate adjustment of *p*-values for the number of tested outcomes, effects on religious and labor-market/fertility outcomes are statistically significant at conventional levels (Table A12).

As another alternative way of inference, we randomly reshuffle the reform years across states. Online Appendix Figure A6 shows the distribution of the placebo coefficients on religiosity based on 1,000 permutations (randomization using actual reform years without replacement). The median placebo "reform" effect on religiosity equals -0.001. Our main estimate based on the accurate reform timing (-0.071) is larger (in absolute terms) than the 10th percentile of the placebo distribution (-0.067), underscoring that it is unlikely to be spurious.

A potential concern in our setting is that the effects might be related to the student movement in the late 1960s and early 1970s. To test this, we exclude all early reforming states from the sample and keep only those states which reformed since the 1980s. Results are largely unaffected in this smaller sample (Online Appendix Table A13).

While our baseline analysis merges the NEPS, ALLBUS, and SOEP datasets to maximize statistical power, we also estimate the models separately for the three datasets to ensure that results are not driven by any specific dataset or by the merging. Results indicate that the effects tend to materialize in each of the separate datasets, although obviously at lower levels of statistical precision (Online Appendix Table A14). Furthermore, we construct a core sample of individuals who have no missing information on the core variables. Moving the analysis to this core sample causes a substantial reduction in sample size compared to the main analysis. For example, it cuts the sample size by almost half for religiosity and by about 80 percent for religious affiliation and most labor-market outcomes. Despite these reductions, results in Online Appendix Table A15 show that the effect sign remains the same for all nine outcomes. Effects remain significant (at the 10 percent level with standard clustering) for six outcomes. Although the substantial reduction in sample size reduces precision, these results suggest that our baseline findings are not primarily driven by underlying differences in sample size.

In our main analysis, the outcome variables are measured by the last available response of each individual to observe the ultimate outcome and maximize the temporal distance between exposure to treatment and measurement of outcome. This conservative approach aims to capture lasting effects of education. Robustness checks using the first available response or the average of the first and last observations to mitigate potential measurement errors are shown in Online Appendix Tables A16 and A17.

We also test the robustness of our event study graphs in several alternative specifications. We reduce the number of years that enter one bin (Online Appendix Figure A7), keep only states that reform between 1972 and 1983 and non-reforming states in the sample (Online Appendix Figure A8), and group outcomes into indices (Online Appendix Figure A9). While in general our results are robust to these variations, results become less clean when pushing on the limited sample size. Specifically, the coefficients are less stable when using two-year instead of five-year bins (reducing the sample size per bin) and when using the smaller sample of reforming states (reducing the overall sample size). In all cases, effects point in the same direction as the main event study figures for all ten outcomes. In six of the ten analyses in Online Appendix Figure A8, there is at least one significant post-reform coefficient. In five of them, the omnibus hypothesis test of zero post-event effects can be rejected. Reassuringly, pre-reform coefficients are insignificant, and we can never reject the omnibus hypothesis test of zero pre-event effects. Disregarding church-going, the outcome indices in Online Appendix Figure A9 indicate no significant overall pre-trends for religious and labor-market/fertility outcomes, and post-event effects are significant at the 1 percent level.

Finally, we test whether the reform had an impact on students' parents. Online Appendix Figure A10 shows that the religiosity of neither the mothers nor the fathers of the respondents were affected by the reform. This indicates that the reform genuinely affected students, rather than any indirect effects of attitudes transmitted from their parents. Reassuringly, there are also no pre-trends in the religiosity of mothers and fathers.

Tests of the two-way fixed effects estimator. Our setting generalizes the classic two-group/two-period difference-in-differences setting in that there are eleven states among whom eight change their treatment status in different years over an extended time horizon. To ensure that our estimates are not driven by two-by-two reform estimates with negative weights, we implement the estimator suggested by Callaway and Sant'Anna (2021) that is immune to bias from negative weighting. The procedure uses only not-yet treated units and never-treated units as controls. Already-treated units, which could potentially cause negative weighting, are omitted from the analysis. Reassuringly, the aggregated estimates of the average treatment effect on the treated (ATT) for the four religious outcomes are very similar to our baseline two-way fixed effects estimates (see Online Appendix Table A18). In fact, the ATT estimates are larger (in absolute terms) than the corresponding baseline estimates, although sometimes at lower levels of statistical significance. We also use other event study estimators that are robust to time-varying treatment effects, namely Sun and Abraham (2021), Callaway-Sant'Anna (2021), Gardner

(2021), and Cengiz et al. (2019) (see Online Appendix Figure A11). Overall, our results are robust to these variations.

Online Appendix B reports additional diagnostic tests suggested by de Chaisemartin and D'Haultfœuille (2020) and by Goodman-Bacon (2021) which further corroborate our baseline results and indicate that our findings are not driven by a setting that would give rise to negative weights.

VIII. Conclusions

Our study investigates whether compulsory religious education affects people's religiosity in the long run. We argue that the different timing of reforms that abandoned compulsory religious education across German states provides plausibly exogenous variation in individuals' exposure to compulsory religious education. Students could now choose to attend non-denominational ethics classes rather than religious education, which likely also changed overall social norms towards religion and, by competitive pressures, the content of religious classes. We find that, conditional on state and birth-year fixed effects, the reform that terminated compulsory religious education led to a significant reduction in the religiosity of affected students in adulthood. The reform reduced the share of people reporting to be religious by about 3 percentage points (compared to an average incidence of 52 percent) and of those reporting to be very religious by 2 percentage points (average 11 percent), which corresponds to estimated persuasion rates – religious people who lose their religiosity due to the reform – of 6 and 20 percent, respectively. We also find reductions in three measures of religious actions – prayer, church-going, and religious affiliation.

We do not find that the reform affected the measured ethical values and behaviors such as reciprocity, trust, volunteering, and life satisfaction, nor the measured political values and behaviors such as interest in politics, satisfaction with democracy, or voting. It appears that the counterfactual of attending non-denominational ethics classes was equivalent to attending religious-education classes in terms of these outcomes, speaking against concerns in the policy debate at the time that abolishing compulsory religious education may deteriorate students' ethical orientation.

Beyond the religious sphere, the reform also had economically relevant consequences, affecting employment, earnings, and fertility. Overall, our results indicate that religious indoctrination in school can indeed exert a life-time influence on students.

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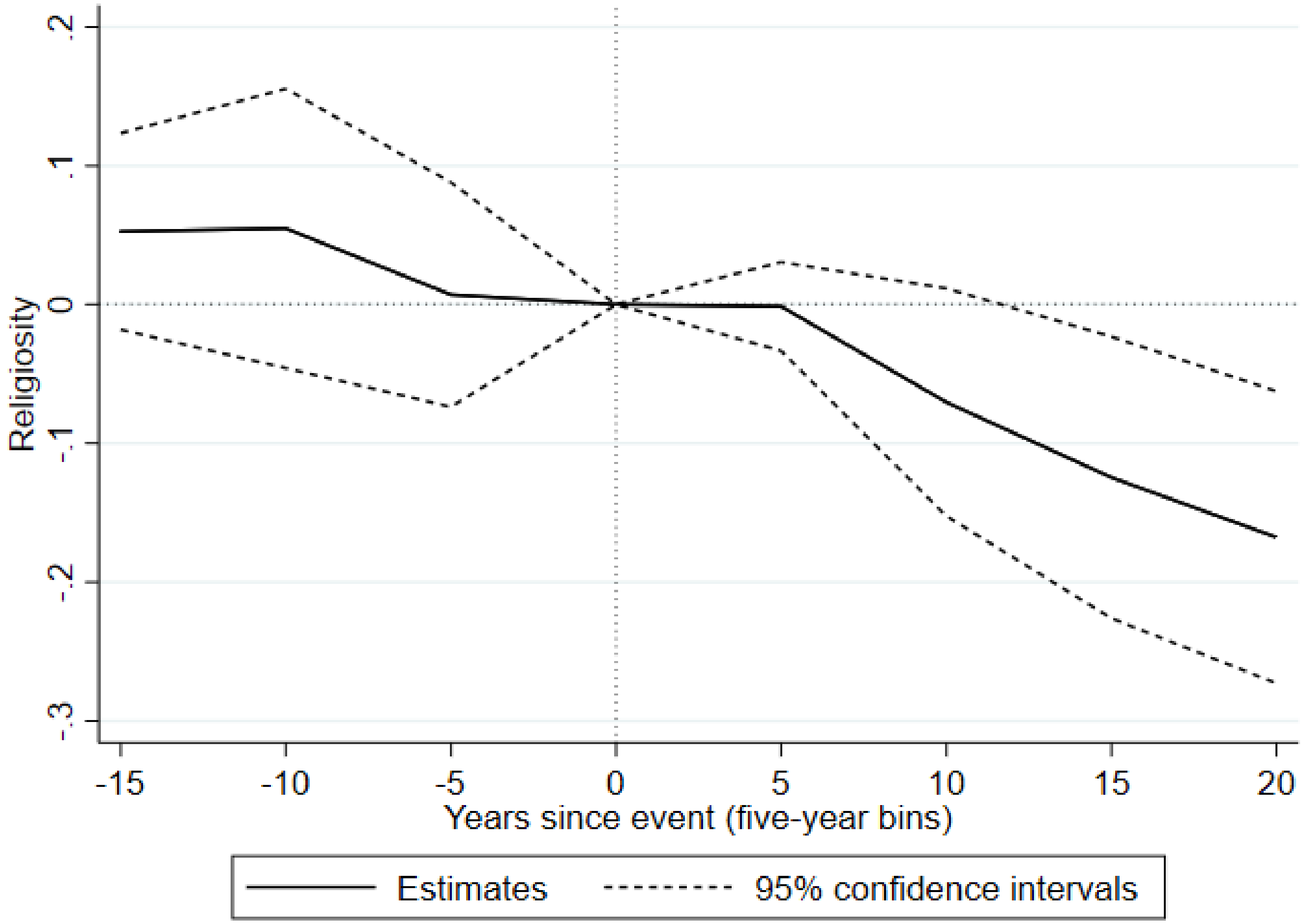
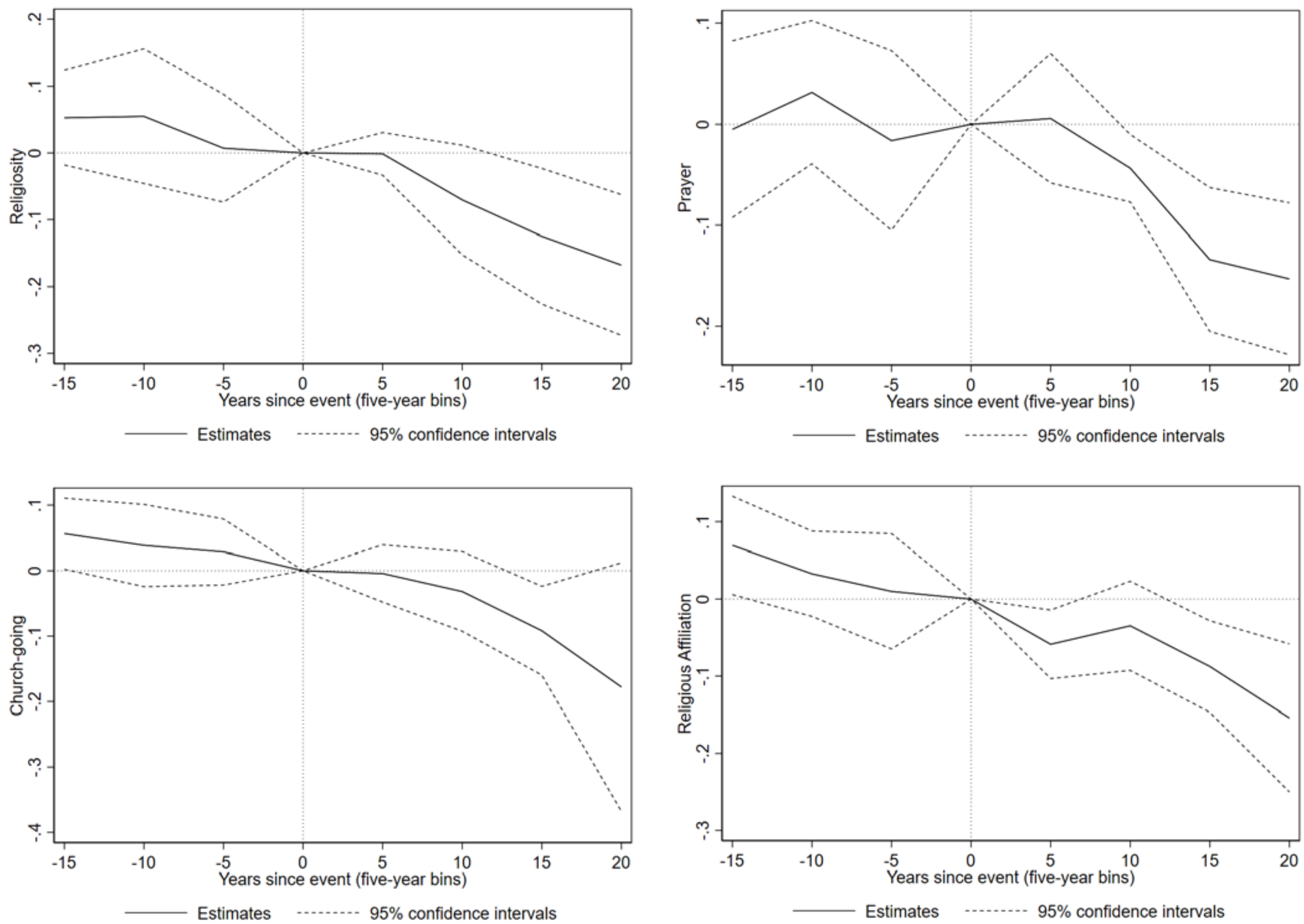
Figure 1: The effect of abolishing compulsory religious education on religiosity: Non-parametric event-study estimates



Notes: Coefficients from non-parametric event-study regressions and their 95 percent confidence intervals. Dependent variable: religiosity (standardized, based on 4-point-scale NEPS question “How religious are you?” and 10-point-scale ALLBUS question “Would you say that you are rather religious or rather not?”). Numbers on horizontal axis refer to final year of respective five-year bins; i.e., 0 = last five years prior to treatment (excluded category), 5 = first five years of treatment. Inference: Standard clustering at state level. The *p*-values of omnibus hypothesis tests of zero pre- and post-event effects are 0.343 and 0.008, respectively. Data sources: National Education Panel Study (NEPS) Cohort 6 (religiosity); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity).

Figure 2: The effect of abolishing compulsory religious education on religiosity and labor-market outcomes: Non-parametric event-study estimates

Panel A: Religious Outcomes



Panel B: Labor-Market Outcomes and Fertility

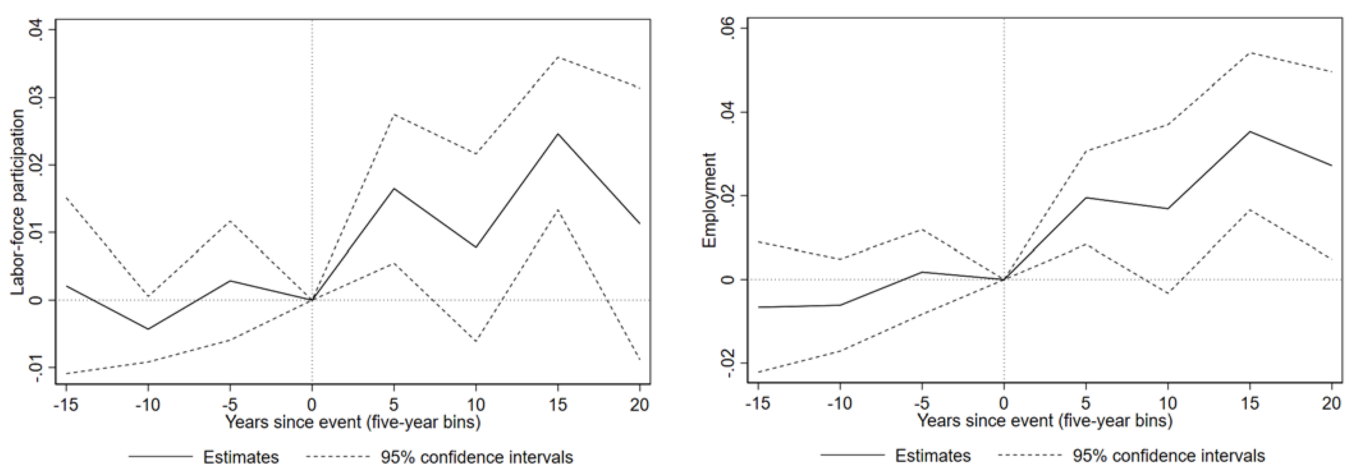
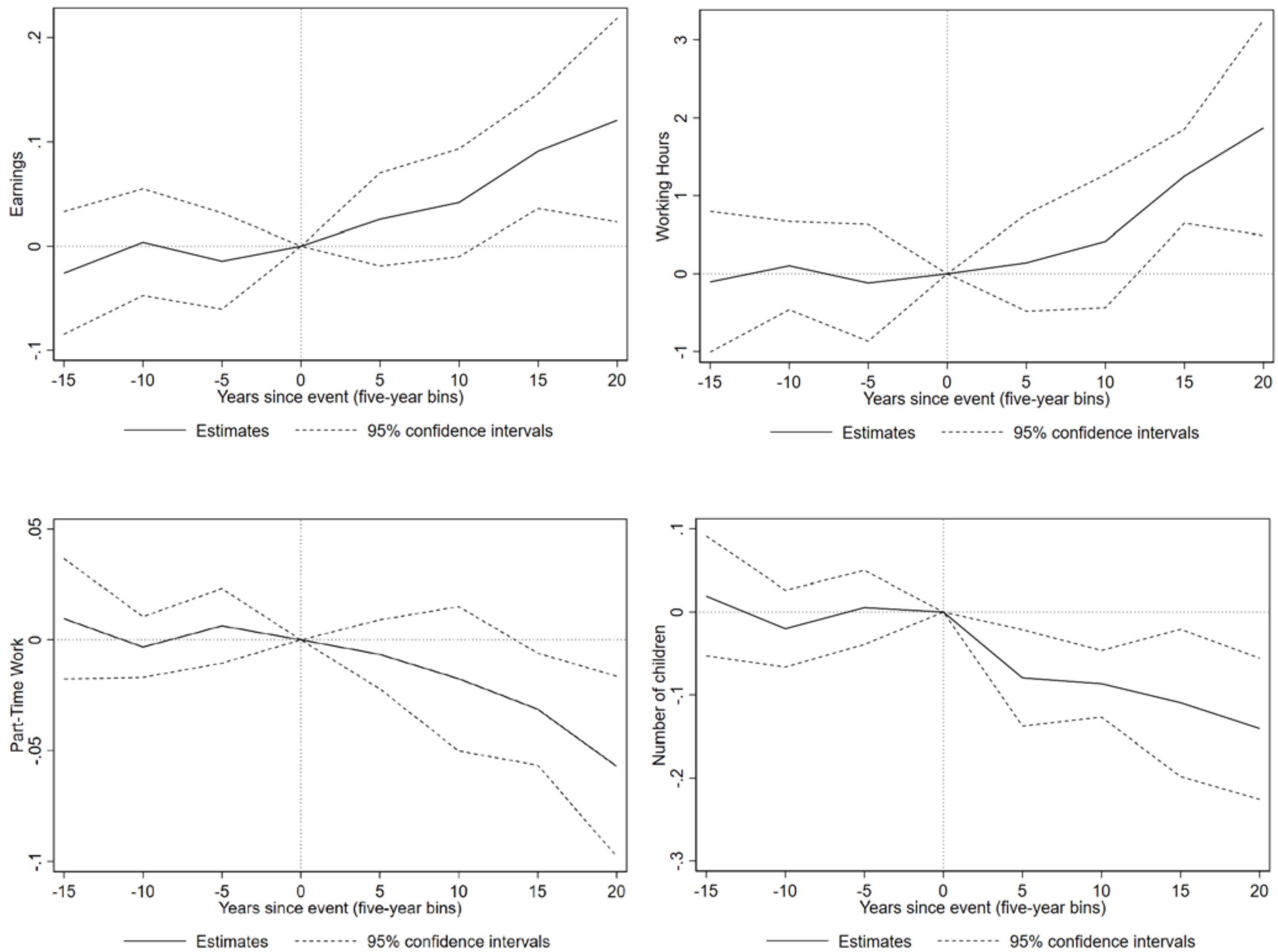


Figure 2 (continued)



Notes: Coefficients from non-parametric event-study regressions and their 95 percent confidence intervals. Dependent variable indicated on the vertical axis (see Table A2 for details). Numbers on horizontal axis refer to final year of respective five-year bins; i.e., 0 = last five years prior to treatment (excluded category), 5 = first five years of treatment. Inference: Standard clustering at state level. The p-values of omnibus hypothesis tests of zero pre- and post-event effects are 0.343 and 0.008 for religiosity, 0.588 and 0.003 for prayer, 0.139 and 0.088 for church-going, 0.052 and 0.020 for religious affiliation, 0.271 and 0.002 for labor-force participation, 0.469 and 0.003 for employment, 0.201 and 0.000 for earnings, 0.745 and 0.000 for working hours, 0.207 and 0.029 for part-time work, and 0.315 and 0.004 for the number of children, respectively. Data sources (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, affiliation, labor-force-participation, employment, earnings, working hours, part-time work, number of children); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity, prayer, church-going, affiliation, labor-force-participation, employment, earnings, working hours, part-time work, number of children); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (church-going, affiliation, labor-force-participation, employment, earnings, working hours, part-time work, number of children).

Table 1: The rollout of abolishing compulsory religious education: Timing of treatment and governing parties

			Governing parties in legislation period	
Reform year	State	Time in treatment	before the reform	of the reform
(1)	(2)	(3)	(4)	(5)
Before 1949	Berlin	1		
Before 1949	Bremen	1		
1972	Bavaria	0.60	CSU (1966-1970)	CSU (1970-1974)
1974	Lower Saxony	0.56	SPD (1970-1974)	SPD, FDP (1974-1976)
1977	Rhineland-Palatinate	0.51	CDU (1971-1975)	CDU (1975-1979)
1977	Hesse	0.51	SPD, FDP (1970-1974)	SPD, FDP (1974-1978)
1983	Baden-Württemberg	0.40	CDU (1976-1980)	CDU (1980-1984)
1992	Schleswig-Holstein	0.24	SPD (1988-1992)	SPD (1992-1996)
2004	Hamburg	0.02	CDU, PRO, FDP (2001-2004)	CDU (2004-2008)
2004	North Rhine-Westphalia	0.02	SPD, Grüne (1995-2000)	SPD, Grüne (2000-2005)
No reform	Saarland	0		

Notes: The table lists the dates of reforms abolishing compulsory religious education for the respective states (from Helbig and Nikolai 2015), the share of years each state spends treated in the estimation sample from 1950-2004, and the governing parties before and during the reform.

Table 2: Descriptive statistics

	Mean	Std. dev.	Min.	Max.	Obs.	Source	Age (min; mean; max)	Sample
Reform (treatment indicator)	0.32	0.47	0.00	1.00	58,703			
Religious outcomes								
Religiosity	0.00	1.00	-1.69	1.77	15,688	NEPS, ALLBUS	(18;45;70)	0.009 (0.017) [0.027]
Prayer	0.00	1.00	-1.26	2.44	13,276	NEPS, ALLBUS	(18;45;70)	0.004 (0.116) [0.196]
Church-going	0.00	1.00	-1.16	3.07	42,776	ALLBUS, SOEP	(17;43;72)	0.014 (0.007) [0.028]
Affiliation	0.00	1.00	-2.22	0.57	45,925	NEPS, ALLBUS, SOEP	(17;42;73)	-0.004 (0.700) [0.783]
Ethical-value outcomes								
Reciprocity	0.00	1.00	-5.11	0.97	21,150	ALLBUS, SOEP	(17;44;72)	-0.015 (0.235) [0.406]
Trust	0.00	1.00	-2.71	2.01	37,070	NEPS, ALLBUS, SOEP	(17;45;72)	0.004 (0.663) [0.713]
Risk-taking	0.00	1.00	-3.00	2.64	35,556	NEPS, SOEP	(17;45;73)	0.007 (0.099) [0.122]
Volunteering	0.43	0.49	0.00	1.00	37,971	NEPS, ALLBUS, SOEP	(17;44;73)	0.023 (0.000) [0.006]
Life satisfaction	0.00	1.00	-4.85	1.56	48,177	NEPS, ALLBUS, SOEP	(17;45;73)	-0.002 (0.753) [0.790]
Political-value outcomes								
Interest in politics	0.00	1.00	-2.47	2.00	52,970	NEPS, ALLBUS, SOEP	(17;43;73)	0.009 (0.034) [0.149]
Politics too complicated	0.00	1.00	-1.95	2.25	9,160	NEPS, ALLBUS	(19;48;69)	0.008 (0.052) [0.131]
Satisfaction with democracy	0.00	1.00	-2.86	1.90	14,519	ALLBUS, SOEP	(16;40;70)	0.006 (0.345) [0.529]
Political spectrum: right	0.00	1.00	-3.02	3.37	40,161	NEPS, ALLBUS, SOEP	(17;43;72)	-0.002 (0.857) [0.822]
Vote in election	0.87	0.34	0.00	1.00	32,133	NEPS, ALLBUS, SOEP	(18;44;72)	0.016 (0.029) [0.123]
Vote left	0.57	0.49	0.00	1.00	27,088	NEPS, ALLBUS, SOEP	(18;45;72)	0.005 (0.402) [0.508]
Vote extreme	0.07	0.25	0.00	1.00	27,100	NEPS, ALLBUS, SOEP	(18;45;72)	0.005 (0.429) [0.522]
Labor-market outcomes and fertility								
Labor-force participation	0.82	0.38	0.00	1.00	58,168	NEPS, ALLBUS, SOEP	(17;44;73)	-0.000 (0.922) [0.917]
Employment	0.78	0.41	0.00	1.00	58,168	NEPS, ALLBUS, SOEP	(17;44;73)	-0.000 (0.922) [0.917]
Earnings	7.14	0.90	0.00	11.61	44,935	NEPS, ALLBUS, SOEP	(16;43;73)	0.023 (0.008) [0.048]
Working hours	35.56	14.89	0.00	120.0	45,781	NEPS, ALLBUS, SOEP	(16;43;73)	0.021 (0.003) [0.014]
Part-time work	0.20	0.40	0.00	1.00	45,781	NEPS, ALLBUS, SOEP	(16;43;73)	0.021 (0.003) [0.014]

(continued on next page)

Table 2 (continued)

	Mean	Std. dev.	Min.	Max.	Obs.	Source	Age (min; mean; max)	Sample
Number of children	1.38	1.25	0.00	12.00	52,668	NEPS, ALLBUS, SOEP	(15;43;73)	0.016 (0.002) [0.021]
Educational outcomes								
Years of education	12.96	2.83	6.00	25.00	42,772	NEPS, ALLBUS, SOEP	(17;45;73)	0.011 (0.027) [0.025]
Abitur	0.38	0.49	0.00	1.00	52,283	NEPS, ALLBUS, SOEP	(16;41;73)	0.014 (0.002) [0.005]
Age of first employment	21.11	3.88	1.33	65.25	38,985	NEPS, SOEP	(15;43;73)	0.008 (0.120) [0.148]
Parental outcomes								
Mother's religiosity	0	1	-5.48	0.31	24,223	ALLBUS, SOEP	(15;38;73)	-0.014 (0.083) [0.234]
Father's religiosity	0	1	-4.47	0.37	23,868	ALLBUS, SOEP	(15;38;73)	-0.011 (0.086) [0.234]
Controls								
Female	0.51	0.50	0.00	1.00	58,703			
Migration status	0.05	0.22	0.00	1.00	58,703			
Mother's education								
Basic (*Hauptschulabschluss* or less)	0.61	0.49	0.00	1.00	58,703			
Medium (*Realschulabschluss*)	0.18	0.39	0.00	1.00	58,703			
High (*Abitur* or more)	0.09	0.29	0.00	1.00	58,703			
Father's education								
Basic (*Hauptschulabschluss* or less)	0.57	0.50	0.00	1.00	58,703			
Medium (*Realschulabschluss*)	0.13	0.34	0.00	1.00	58,703			
High (*Abitur* or more)	0.16	0.36	0.00	1.00	58,703			
NEPS	0.21	0.41	0.00	1.00	58,703			
ALLBUS	0.27	0.44	0.00	1.00	58,703			
SOEP	0.52	0.50	0.00	1.00	58,703			

Notes: Descriptive statistics. Column (6): outcome-specific data sources. Column 7: minimum, mean, and maximum of outcome-specific age at time of survey. Column 8: coefficient and *p*-values (standard clustering at state level in parentheses; wild cluster bootstrap by Roodman et al. (2019) in brackets) of indicator for item availability on reform indicator, basic controls, state fixed effects, and birth year fixed effects. Sums of category means of mother's and father's education, respectively, do not add up to one because missing values are set to zero, defining a separate binary explanatory variable that accounts for the missing values. Data sources: National Education Panel Study (NEPS) Cohort 6; German General Social Survey (ALLBUS) Cumulation 1980-2016; German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34).

Table 3: Effects of abolishing compulsory religious education on religiosity and religious actions

	Religiosity	Prayer	Church-going	Affiliation
	(1)	(2)	(3)	(4)
Reform	-0.071	-0.046	-0.066	-0.081
	(0.018)	*(0.101)*	*(0.020)*	*(0.009)*
	[0.061]	*[0.136]*	*[0.022]*	*[0.066]*
State fixed effects	Yes	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	15,688	13,276	42,776	45,925

Notes: Dependent variables indicated in column headers. All dependent variables are standardized (see Online Appendix Table A2 for details). Controls: gender, migration status, mother's education, father's education, survey and survey-year fixed effects. Inference: *p*-values with clustering at the state level; parentheses: standard clustering at state level; brackets: wild cluster bootstrap by Roodman et al. (2019). Data sources (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, affiliation); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity, prayer, church-going, affiliation); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (church-going, affiliation).

Table 4: Time-varying treatment effects on religious outcomes

	Religiosity		Prayer		Church-going		Affiliation	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reform	-0.072	0.017	-0.045	0.037	-0.049	0.005	-0.087	-0.054
	(0.031)	*(0.593)*	*(0.129)*	*(0.159)*	*(0.063)*	*(0.906)*	*(0.005)*	*(0.034)*
	[0.149]	*[0.733]*	*[0.214]*	*[0.209]*	*[0.075]*	*[0.925]*	*[0.052]*	*[0.068]*
Years relative to reform	0.000	0.002	-0.000	0.001	-0.007	-0.006	0.003	0.003
	(0.942)	*(0.611)*	*(0.821)*	*(0.660)*	*(0.007)*	*(0.015)*	*(0.135)*	*(0.051)*
	[0.941]	*[0.731]*	*[0.822]*	*[0.715]*	*[0.284]*	*[0.328]*	*[0.231]*	*[0.149]*
Reform x		-0.013		-0.012		-0.007		-0.004
Years relative to reform		*(0.001)*		*(0.001)*		*(0.161)*		*(0.129)*
		[0.105]		*[0.035]*		*[0.480]*		*[0.288]*
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15,688	15,688	13,276	13,276	42,776	42,776	45,925	45,925

Notes: Dependent variables indicated in column headers. All dependent variables are standardized (see Online Appendix Table A2 for details). Controls: gender, migration status, mother's education, father's education, survey and survey-year fixed effects. Inference: *p*-values with clustering at the state level; parentheses: standard clustering at state level; brackets: wild cluster bootstrap by Roodman et al. (2019). Data sources (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, affiliation); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity, prayer, church-going, affiliation); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (church-going, affiliation).

Table 5: Heterogeneous treatment effects on religious outcomes

	Religiosity		Prayer		Church-going		Affiliation	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Gender	Females	Males	Females	Males	Females	Males	Females	Males
Reform	-0.067	-0.073	-0.085	-0.007	-0.039	-0.097	-0.075	-0.085
	(0.024)	*(0.060)*	*(0.057)*	*(0.835)*	*(0.251)*	*(0.009)*	*(0.012)*	*(0.025)*
	[0.033]	*[0.237]*	*[0.114]*	*[0.841]*	*[0.179]*	*[0.037]*	*[0.112]*	*[0.094]*
Panel B: Area	Rural	Urban	Rural	Urban	Rural	Urban	Rural	Urban
Reform	-0.067	-0.123	-0.100	-0.024	–	–	-0.064	-0.040
	(0.038)	*(0.071)*	*(0.037)*	*(0.615)*			*(0.131)*	*(0.670)*
	[0.007]	*[0.102]*	*[0.034]*	*[0.572]*			*[0.196]*	*[0.700]*
Panel C: Area	Catholic	Protestant	Catholic	Protestant	Catholic	Protestant	Catholic	Protestant
Reform	-0.157	-0.016	-0.124	-0.041	–	–	-0.211	0.064
	(0.009)	*(0.687)*	*(0.004)*	*(0.482)*			*(0.001)*	*(0.285)*
	[0.021]	*[0.655]*	*[0.015]*	*[0.468]*			*[0.017]*	*[0.317]*
Panel D: Parents	Catholic	Protestant	Catholic	Protestant	Catholic	Protestant	Catholic	Protestant
Reform	–	–	–	–	-0.071	0.004	-0.077	-0.113
					(0.199)	*(0.904)*	*(0.044)*	*(0.047)*
					[0.324]	*[0.903]*	*[0.199]*	*[0.120]*

Notes: Each cell reports the coefficient on reform treatment from a separate regression. All regressions include state and birth-year fixed effects and controls. Dependent variables indicated in column headers. All dependent variables are standardized (see Online Appendix Table A2 for details). Controls: gender, migration status, mother's education, father's education, survey and survey-year fixed effects. Urban area if county has more than 100,000 inhabitants; rural otherwise (available only for RemoteNEPS). Catholic area if number of Catholics over sum of Protestants and Catholics in county is larger than 0.5; Protestant area otherwise (available only for RemoteNEPS). Catholic/Protestant parents if both parents are Catholic/Protestant (available only for ALLBUS and SOEP). Inference: *p*-values with clustering at the state level; parentheses: standard clustering at state level; brackets: wild cluster bootstrap by Roodman et al. (2019). Data sources (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, religious affiliation); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity, prayer, church-going, religious affiliation); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (church-going, religious affiliation).

Table 6: Effects on ethical-value and political-value outcomes

Panel A: Ethical-value outcomes

	Reciprocity (1)	Trust (2)	Risk-taking (3)	Volunteering (4)	Life satisfaction (5)
Reform	0.006	0.007	0.008	0.007	-0.014
	(0.734)	*(0.780)*	*(0.636)*	*(0.681)*	*(0.478)*
	[0.748]	*[0.816]*	*[0.748]*	*[0.792]*	*[0.682]*
State and birth-year fixed effects, controls	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	0	0	0	0.43	0
Observations	21,150	37,070	35,556	37,971	48,177

Panel B: Political-value outcomes

	Interest in politics (1)	Politics too complicated (2)	Satisfaction with democracy (3)	Political spectrum: right (4)	Vote in election (5)	Vote left (6)	Vote extreme (7)
Reform	0.010	0.017	0.001	-0.021	0.011	-0.016	-0.004
	(0.530)	*(0.675)*	*(0.980)*	*(0.195)*	*(0.070)*	*(0.245)*	*(0.477)*
	[0.603]	*[0.718]*	*[0.992]*	*[0.249]*	*[0.128]*	*[0.404]*	*[0.485]*
State and birth-year fixed effects, controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. variable	0	0	0	0	0.87	0.57	0.07
Observations	52,970	9,160	14,519	40,161	32,133	27,088	27,100

Notes: Dependent variables indicated in column headers. Dependent variables (see Online Appendix Table A2 for details): panel A: columns (1) – (3), (5): standardized; column (4): indicator variable; panel B: columns (1) – (4): standardized; columns (5) – (7): indicator variable. Controls: gender, migration status, mother's education, father's education, survey and survey-year fixed effects. Inference: *p*-values with clustering at the state level; parentheses: standard clustering at state level; brackets: wild cluster bootstrap by Roodman et al. (2019). Data sources: National Education Panel Study (NEPS) Cohort 6 (trust, risk-taking, volunteering, life satisfaction, interest in politics, politics too complicated, political spectrum: right, vote in election, vote left, vote extreme); German General Social Survey (ALLBUS) Cumulation 1980-2016 (reciprocity, trust, volunteering, life satisfaction, interest in politics, politics too complicated, satisfaction with democracy, political spectrum: right, vote in election, vote left, vote extreme); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (reciprocity, trust, risk-taking, volunteering, life satisfaction, interest in politics, satisfaction with democracy, political spectrum: right, vote in election, vote left, vote extreme).

Table 7: Effects on labor-market outcomes and fertility

	Labor-force participation	Employment	Earnings	Working hours	Part-time work	Number of children
	(1)	(2)	(3)	(4)	(5)	(6)
Reform	0.015	0.023	0.053	0.590	-0.020	-0.088
	(0.002)	*(0.000)*	*(0.032)*	*(0.095)*	*(0.067)*	*(0.006)*
	[0.036]	*[0.002]*	*[0.057]*	*[0.168]*	*[0.137]*	*[0.031]*
State and birth-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	0.82	0.78	7.14	35.56	0.20	1.38
Std. dev. of dependent variable	0.38	0.41	0.90	14.89	0.40	1.25
Observations	58,168	58,168	44,935	45,781	45,781	52,668

Notes: Dependent variables indicated in column headers. Dependent variables (see Online Appendix Table A2 for details): Columns (1), (2), (5): indicator variable; columns (4), (6): numbers; column (3): log earnings. Controls: gender, migration status, mother's education, father's education, survey and survey-year fixed effects. Inference: *p*-values with clustering at the state level; parentheses: standard clustering at state level; brackets: wild cluster bootstrap by Roodman et al. (2019). Data sources: National Education Panel Study (NEPS) Cohort 6 (labor-force participation, employment, earnings, working hours, part-time work, number of children); German General Social Survey (ALLBUS) Cumulation 1980-2016 (labor-force participation, employment, earnings, working hours, part-time work, number of children); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (labor-force participation, employment, earnings, working hours, part-time work, number of children).

Table 8: Effects on educational outcomes

	Years of education	*Abitur*	Age at first employment
	(1)	(2)	(3)
Reform	0.032	-0.023	0.018
	(0.670)	*(0.075)*	*(0.866)*
	[0.730]	*[0.226]*	*[0.899]*
State fixed effects	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Mean of dependent variable	12.96	0.38	21.11
Std. dev. of dependent variable	2.83	0.49	3.88
Observations	42,772	52,283	38,985

Notes: Dependent variables indicated in column headers. Dependent variables (see Online Appendix Table A2 for details): column (1), (3): number; column (2): indicator variable. Controls: gender, migration status, mother's education, father's education, survey and survey-year fixed effects. Inference: *p*-values with clustering at the state level; parentheses: standard clustering at state level; brackets: wild cluster bootstrap by Roodman et al. (2019). Data sources: National Education Panel Study (NEPS) Cohort 6 (years of education, abitur, age of first employment); German General Social Survey (ALLBUS) Cumulation 1980-2016 (years of education, abitur); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (years of education, abitur, age of first employment).

Table 9: Effects on religious outcomes: Border specification with county-pair fixed effects

	Religiosity	Prayer	Affiliation
	(1)	(2)	(3)
Reform	-0.162	-0.168	0.004
	(0.022)	*(0.063)*	*(0.909)*
	[0.007]	*[0.036]*	*[0.903]*
State fixed effects	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	3,070	2,648	3,072

Notes: Dependent variables indicated in column headers (church-going not covered in NEPS data). All dependent variables are standardized (see Online Appendix Table A2 for details). Controls: gender, migration status, mother's education, father's education, survey, survey-year fixed effects, and bordering-county-pair fixed effects. Inference: *p*-values with clustering at the state level; parentheses: standard clustering at state level; brackets: wild cluster bootstrap by Roodman et al. (2019). Data sources: National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, affiliation).