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 abolishing compulsory religious education significantly reduced religiosity of affected students in adulthood. It also reduced the religious actions of personal prayer, church-going, and church membership. Beyond religious attitudes, the reform led to more equalized gender roles, fewer marriages and children, and higher labor-market participation and earnings. The reform did not affect ethical and political values or non-religious school outcomes.

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1. 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); Rubin (2017)).[2] 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 family and worldly norms, we also study effects beyond the religious sphere on family and 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

[1] Figures refer to 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.

[2] On 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) for a historical context, and Becker and Woessmann (2009, 2018) for the German context.

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 to choose between denominational religious education and "ethics" as a non-denominational subject. A particularly interesting feature of the reforms is that opting out of value-oriented instruction was not feasible. 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.

We use three datasets, each of which allows us to link religious 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 abolishment of compulsory religious education decreased self-reported religiosity of affected students in adulthood by 7 percent of a standard deviation. 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. Effects on religiosity and personal prayer phase in gradually over time, whereas the effect on church membership is immediate. Effects are mostly restricted to predominantly Catholic (rather than Protestant) counties.

Beyond the religious sphere, the reforms also affected family and labor-market outcomes. First, they led to more equitable and less conservative attitudes towards gender roles and family norms. Second, they affected actual family outcomes by reducing the incidence of marriage and the number of children. Third, they increased labor-market participation, employment, working hours, and earnings. 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 come at the detriment of reduced ethical values in general.

Several specification and robustness tests support the baseline result. The reforms are not related to other school outcomes such as degree completion, which may be interpreted as placebo outcomes, and are robust to 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 by recent diagnostic tools of the two-way fixed effects estimator.

Our study contributes to the economics of religion literature that investigates various determinants of religiosity (e.g., Gruber and Hungerman (2008); Becker and Woessmann (2013); Rubin (2014); Engelberg et al. (2016); Bentzen (2019); Bryan, Choi, and Karlan (2021)). Existing studies on the interrelationship between education systems and religion (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)), if interested in effects of education on religion, 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.

We also contribute to the political economy of state schooling which studies why states take over control of school curricula (e.g., Lott (1999); Gradstein and Justman (2002); Pritchett and Viarengo (2015)). Our results suggest that the historical resistance of the churches against the emerging state-sponsored non-denominational education systems (Ramirez and Boli (1987); West and Woessmann (2010)) was rational in the sense that forfeiting the opportunity to instill religious attitudes in public schools undermined churches' follower base in the long run. We also contribute to the economics of education literature that studies the impact of various school reforms on academic (e.g., Hanushek (1986); Woessmann (2016)) and non-academic outcomes (e.g., Almlund et al. (2011); Koch, Nafziger, and Nielsen (2015); Cantoni et al. (2017)) by studying how school curricula reforms affect outcomes beyond traditional achievement measures, namely religious attitudes in the long run.

In the following, section 2 provides institutional background on the studied reforms. Section 3 describes the empirical model and section 4 the data. Sections 5 and 6 present our results on reform effects on religious outcomes and on family and labor-market outcomes, respectively. Section 7 reports specification and robustness tests, and section 8 concludes.

2. 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.[3]

Pre-reform situation. Against the backdrop of the Nazi takeover of schools and in close agreement with the Allied forces (see Appendix A.1 for historical background), the Constitution (*Grundgesetz*) of the Federal Republic of Germany, enacted in 1949, establishes in Article 7 that religious education is a regular subject in public schools.[4] This makes it compulsory that public schools provide religious education, which is explicitly 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 or 18, depending on the state) 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

[3] By contrast, it is hard to imagine exogenous variation in religious education in countries where it is barred from public schools (e.g., the United States with its strict separation of church and state that forbids religious education in public schools) or offered as an elective subject (e.g., Italy or the Netherlands).

[4] 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.

[5] Parents choose the denomination, and as soon as adherents to a denomination exceed a very low threshold, schools are required to provide religious education in the denomination.

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. Between 1972 and 2004, eight of the eleven West German states terminated the compulsory nature of religious education (Table 1, see also map in Appendix Figure A1). Parents could now choose between religious education and a newly introduced subject, usually called "ethics", which provided an alternative form of value-oriented instruction that was non-denominational. The impetus for the reforms came from churches and schools to prevent rising opt-out (see Appendix A.2 for details).

The rollout of the reforms across states was orthogonal to the political leaning of and changes in the state government, making it unlikely that they were due to political trends or shocks. Four reforms each were implemented by right-of-center and left-of-center governments, and the timing alternates between the two camps (column 4 of Table 1). Furthermore, no reform was implemented in a legislative period after a change in government (column 5). The reform rollout was also 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.

Reforms were also not due to specific religious trends in the reforming states. There was no specific trend in church memberships in the years before the reforms were introduced in the respective states (see Appendix Figure A2).[6]

There are three main consequences of the reform that might give rise to overall long-term reform effects (see Appendix A.3 for details). First, individual students could now attend ethics instead of religious education. Unfortunately, there is no administrative data on how many students chose ethics in the years right after the reform implementation, but current data show that only about one fifth of students attend ethics classes and are thus affected in the sense that they themselves do not attend denominational religious education. Second, as a newly emerged competitor, the subject ethics put the curricula of religious education under modernizing pressure, with new coverage of non-Christian religions and a focus on helping students develop their own values rather than practicing prayer and literal bible interpretation. Third, the reform may have changed perceived social norms since it was now officially approved that alternatives to religious education exist. The indicated apparent acceptance in society not to be religious may have changed religious views even for students who still attended religious education classes.

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, as several elements of the reform enactment were gradual rather than abrupt, we expect that reform effects may phase in rather than happen discontinuously.

[6] 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.

3. 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:

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

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 $1\left(t_{i,s} \geq t_s^*\right)$ 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*.[7] 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

[7] Coding treatment at primary school entry is our preferred categorization because it starts with the first cohort that could have avoided religious education completely. This baseline specification underestimates the true effect to the extent that some older cohorts were in fact exposed to the reformed curriculum in higher grades. Results are confirmed in a dosage specification that defines treatment by years spent in the reformed system, as well as in a specification that codes treatment at secondary school entry (see Appendix D).

(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.[8]

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.

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., Borusyak, Jaravel, and Spiess (2021); Athey and Imbens (2022)). This seems plausible given the idiosyncrasies of the reform processes in the German federal political system described above.

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 section 7, we present several specification tests that corroborate this identifying assumption and also restrict the identifying variation to neighboring counties that are plausibly very similar in the absence of treatment.

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

In addition, we can include a trend variable relative to the reform $(t_{i,s} - t_s^*)$ as a falsification test of the identifying assumption of randomness in reform timing, keeping the assumption of time-invariant treatment effects for now:

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

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 2 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 $(t_{i,s} \geq t_s^*)$ with the trend term $(t_{i,s} - t_s^*)$:

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

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.

4. 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 Data Appendix B 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.

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.[9] 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 reached adulthood 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

[9] 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.

general income taxation. Paying these church taxes can only be avoided by leaving the church. For comparability, we standardize all religious measures within each dataset.

The three datasets also provide batteries of measures of attitudes towards gender and family roles and of actual family and labor-market outcomes, as well as of ethical-value, political-value, and educational outcomes. Control variables include gender, migration status, mothers' and fathers' education, and survey and survey-year fixed effects (see Appendix Table A1 for descriptive statistics).

5. 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 6 turns to effects on non-religious outcomes, and section 7 provides 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.[10] 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 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.[11]

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[10] Appendix Table A3 provides the non-parametric regression results underlying this figure.

[11] 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 Appendix D) that takes this into account yields slightly larger estimates than the baseline specification.

The parametric estimation in Table 2 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 (Appendix Table A4), 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 2). 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 (marginally significant), the public act of going to church by 7 percent, and the formal act of church membership by 8 percent.[12]

To test whether reforming states are on a general time trend that is different from non-reforming states, the odd columns of Table 3 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

[12] Appendix Figures A3-A5 show the respective event-study graphs.

religious-action outcomes, in line with the assumption that the timing of reform events is as good as random.

The even columns of Table 3 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, which may have triggered church leaving among individuals exposed to a reform just introduced even if 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 and church affiliation are very similar for women and men (Appendix Table A5). Treatment effects on prayer materialize only for women but not men, whereas treatment effects on church-going are larger for men. Results show no strongly differential pattern for individuals who went to schools in rural and urban areas. When distinguishing individuals' school county by the majority confession, results are driven by Catholic areas, where religiosity tended to be more deeply engrained. In a subsample with information on parental denomination, the effect on church-going also appears to be restricted to individuals with all-Catholic parents, whereas the (somewhat imprecise) estimate on religious affiliation is in fact larger for individuals with Protestant parents.

In contrast to the effects on religiosity and religious actions, we do not find evidence that the reform affected various value outcomes. There are no significant treatment effects on a series of measures of ethical-value outcomes including reciprocity, trust, risk preference, volunteering, and life satisfaction (Appendix Table A6, Panel A). 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 different ethical-value outcomes compared to the subject religious education. Similarly, there is no evidence of effects on political-value outcomes such as political interest, satisfaction with democracy, or left-right voting patterns (Panel B).

6. Effects on Family and Labor-Market Outcomes

Historically, the churches strongly promoted traditional religious family role models, advocating gender-specific roles in families and marriage before cohabitation. Therefore, we also study effects of the termination of compulsory religious education beyond the religious sphere on people's attitudes towards gender and family roles and on subsequent family outcomes.

Results show that the reform led to a decrease in conservative gender and family attitudes. Abolishing compulsory religious education reduced the likelihood to think that men are better suited for certain professions than women by 8 percent of a standard deviation (Panel A of Table 4). The negative effect on views on different gender duties in housework is not statistically significant, but the reform also significantly decreased the likelihood to think that women cannot use technical devices as well as men. Similarly, the reform reduced the view that people should get married if they permanently live with a partner.

The reform also affected actual family outcomes (Panel B of Table 4, columns 1-2). The treatment reduced the probability to be married by 1.5 percentage points, compared to an average marriage rate of 60 percent. The reform also decreased the number of children by 0.09 children per respondent, compared to an average of 1.4 children.

The reform may additionally have affected economic behavior and outcomes. According to Christian values, the decrease in religiosity may have promoted materialistic orientation.[13] The reduction in time used for religious actions may have induced a substitution effect towards economic activities (Barro and McCleary (2003); Gruber and Hungerman (2008)). In addition, leaving the church means a reduction in the tax rate on labor income in Germany, increasing incentives to work.

Results show that the reform indeed had positive effects on labor-market outcomes (Panel B of Table 4, columns 3-6). The probability to participate in the labor market increases by 1.5 percentage points, compared to a mean of 82 percent, and the probability to be employed by 2.3 percentage points (mean 78 percent). Among those employed, working hours rise by 0.6 hours per week (mean 35.6 hours). Earnings increase by 5.3 percent. Overall, the results suggest that the reform impacted people's lives well beyond the religious sphere.[14]

7. Specification and Robustness Tests

Effects on non-religious school outcomes. The religious-education reform did not affect the content, structure, or hours of any other subjects and did not substitute religious education by

[13] 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)

[14] The family and labor-market effects do not differ significantly by gender (not shown). Consistent with the effect heterogeneities on religious outcomes, however, the effects on family and labor-market outcomes are more pronounced in predominantly Catholic areas (Appendix Table A7).

classes prone to enhance achievement in other curricular subjects. Thus, we do not expect any first-order effects of the religious-education curriculum reform on other school outcomes. Indeed, 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 (Appendix Table A8). This can be interpreted as a placebo test in line with our identifying assumption that the abolishment of religious education is not accompanied by other state-cohort-specific events such as educational reforms with the same timing structure (see also Appendix D). This interpretation is also consistent with the non-existence of effects on ethical-value and political-value outcomes (section 5).

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.[15] 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 Appendix Figure A6). 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 and Sperling (2020)). 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 (Appendix Table A9). The same is true for prayer, whereas the effect on affiliation does not hold in this specification.

[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).

Tests of the two-way fixed effects estimator. 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 by using only not-yet treated units and never-treated units as controls. 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 Appendix Table A10). Appendix C reports additional diagnostic tests suggested by de Chaisemartin and D'Haultfœuille (2020) and by Goodman-Bacon (2021) which further corroborate our baseline model.

Additional robustness tests. Further analyses in Appendix D show that results are robust to controlling for a series of other education reforms, a dosage specification, treatment coding at secondary school entry, combining groups of outcome measures in indices, multiple hypothesis testing, inference from random permutations, excluding early reforming states, and separate analyses for the three datasets.

8. Conclusions

We investigate whether compulsory religious education affects people's religiosity in the long run. 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 termination of 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 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 ethical values such as reciprocity, trust, volunteering, and life satisfaction, nor political values 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.

Beyond the religious sphere, the reform also affected family and economic outcomes. Affected students express less conservative gender and family norms later in life. This finding shows that gender norms – important determinants of lifetime outcomes (e.g., Kleven et al. (2019); Jayachandran (2021)) whose causes are not well understood – are malleable in public settings outside the family. The abolishment of compulsory religious education also affected actual family outcomes (lower incidence of marriage and children) and labor-market outcomes (higher employment and earnings). Thus, the reform had economically relevant consequences.

Overall, our results indicate that religious indoctrination in school can indeed exert a life-time influence on students.

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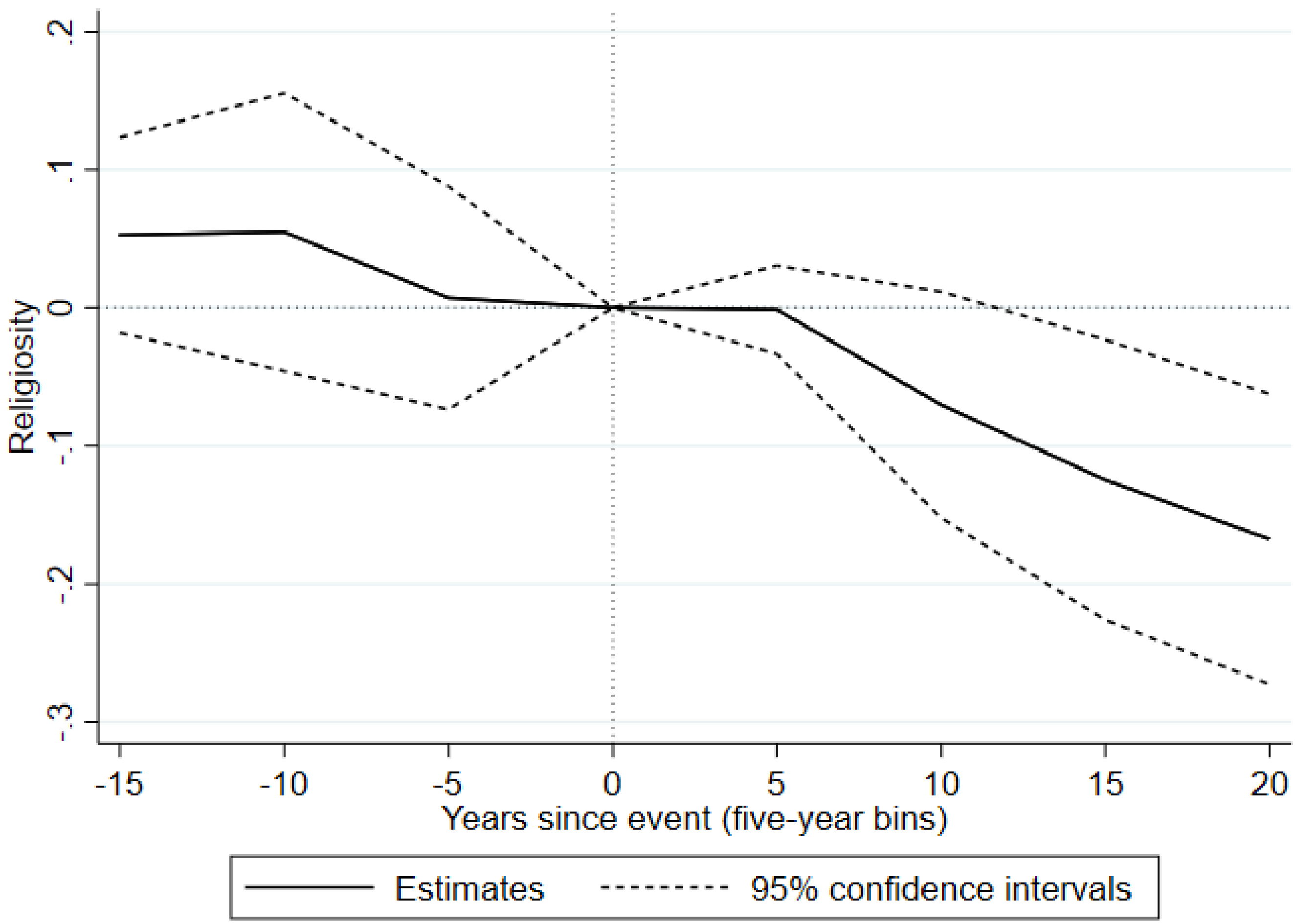
Figure 1: The effect of abolishing compulsory religious education on religiosity: Non-parametric event-study estimates



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; German General Social Survey (ALLBUS) Cumulation 1980-2016.

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: 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 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: 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: 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 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: 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 4: Effects on family and labor-market outcomes

Panel A: Attitudes towards gender and family roles

	Different gender suitability for professions	Different gender duties in the home	Gender use of technical devices	Attitude towards marriage
	(1)	(2)	(3)	(4)
Reform	-0.084	-0.035	-0.061	-0.117
	(0.084)	*(0.371)*	*(0.005)*	*(0.002)*
	[0.183]	*[0.452]*	*[0.044]*	*[0.044]*
State and birth-year fixed effects, controls	Yes	Yes	Yes	Yes
Observations	8,868	18,008	8,859	14,943

Panel B: Family and labor-market outcomes

	Married	Number of children	Labor-force participation	Employment	Working hours	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
Reform	-0.015	-0.088	0.015	0.023	0.590	0.053
	(0.114)	*(0.006)*	*(0.002)*	*(0.000)*	*(0.095)*	*(0.032)*
	[0.074]	*[0.031]*	*[0.036]*	*[0.002]*	*[0.168]*	*[0.057]*
State and birth-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dependent variable	0.60	1.38	0.82	0.78	35.56	7.14
Std. dev. of dependent variable	0.49	1.25	0.38	0.41	14.89	0.90
Observations	56,673	52,668	58,168	58,168	45,781	44,935

Notes: Dependent variables indicated in column headers. Dependent variables (see Appendix Table A2 for details): all dependent variables in panel A are standardized; panel B: columns (1), (3), (4): indicator variable; columns (2), (5): numbers; column (6): 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; German General Social Survey (ALLBUS) Cumulation 1980-2016; German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34).

Appendix A: Additional Background on the Reforms

This appendix provides additional background on the history of compulsory religious education in Germany (section A.1) and discusses the main reasons (section A.2) and consequences (section A.3) that have been put forward for the reforms.

A.1 Historical Background on Compulsory Religious Education in Germany

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.

A.2 Impetus for Reform Introduction

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.[16] 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.

[16] 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 (Appendix D) and to restricting the sample to rural areas (section 5).

A.3 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 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. 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)).[17] 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, Table A11 provides an overview of curricula in Bavaria. The 1967 pre-reform curriculum of Catholic religious education never even mentions non-Christian religions. By contrast, the 1979 post-reform curriculum has a whole section in grade 9 designated to learning

[17] 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.

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.[18] 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.

[18] 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)).

Appendix B: Data

This appendix provides additional detail on the three individual-level datasets and their preparation (sections B.1-B.3) and describes how we merge them for our analysis (section B.4).

B.1 National Education Panel Study (NEPS)

The National Education Panel Study (NEPS) is a large-scale longitudinal survey capturing educational biographies of individuals in Germany (Blossfeld, Roßbach, and von Maurice (2011)). It focuses not only on competencies, educational processes, educational decisions, and returns to education throughout the life span of individuals, but also covers a wide range of other topics including several questions on religiosity. NEPS has six different "starting cohorts", from newborns to adults, which are then followed through their lives.[19]

We use Starting Cohort 6 which covers the educational and professional careers of a representative sample of adults with a special focus on adult education and lifelong learning. The survey was first administered in 2007/2008 with seven follow-up waves until 2015/2016. Whenever a variable of an individual is measured in multiple waves, we use its most recent non-missing value. The data cover detailed retrospective questions on the educational biographies of respondents including the state and year of primary school entry, which we use to link the status of compulsory religious education for this state-year combination. Whenever the state of the primary school location is not available, we use the state of residence of the individual in the primary school entry year instead if available. Whenever the year of primary school entry is not available, we use the year of secondary school entry minus four, if available, given the default duration of primary school equals four years in Germany.

[19] One "starting cohort" can contain many birth cohorts. The Starting Cohort 6, which we use in our analysis, includes birth cohorts from 1944 to 1988.

We keep individuals in the sample who provide information about their state and year of primary school entry, as well as about basic control variables (gender and migration background). We further require that the individuals entered primary school after 1949 and before 2005 in a West German state.[20] The resulting sample consists of 12,281 individuals.

Regarding religious outcome variables, NEPS contains our main outcome variable religiosity as well as information on personal prayer and religious affiliation. Church-going is not included. NEPS also contains most variables from the other outcome variable groups (Table A2). Compared to ALLBUS and SOEP, gender role attitudes are particularly well covered.

Regarding control variables, NEPS contains information on gender, migration status, father's and mother's education, and the survey year. Missing values of father's and mother's education are set to zero, and a separate binary explanatory variable is introduced that accounts for the missing values. Given our approach to use the most recent available information per individual and variable, we store the survey year of an individual separately for each outcome variable and use it accordingly as outcome-specific control variable in the regression analyses. In contrast to ALLBUS and SOEP, information on the religious affiliation of the parents is not available in NEPS.

To access fine-grained geographical information below the state level, we make use of RemoteNEPS, the technology that enables remote data processing of sensitive information. RemoteNEPS provides the county identifier of an individual's primary school location, which we merge to administrative data about the county structure (rural vs. urban, Catholic vs. Protestant). In addition, we can use this information to implement our border specification of

[20] For Baden-Württemberg and Saarland, we only keep individuals in the sample who entered primary school after 1952 and 1956, respectively, as the legal status of religious education was not defined or cannot be retrieved from legal documents for the previous years (Helbig and Nikolai (2015)).

individuals going to school in counties neighboring each other across state borders (and including county-pair fixed effects).[21]

B.2 German General Social Survey (ALLBUS)

The German General Social Survey (ALLBUS) is a biennial cross-sectional survey that monitors societal change by interviewing a nationally representative sample of adults in Germany since 1980 (GESIS (2019)). It provides a picture of the attitudes, behaviors, and social structure of the population in Germany. We use the ALLBUS Cumulation that combines 20 waves from 1980 to 2016.[22] The ALLBUS Cumulation contains all variables from the twenty waves that are elicited in at least two waves. Unlike NEPS and SOEP, the cross-sectional data structure of ALLBUS implies that each individual is observed only once.

The data contain information on the state a respondent lived in during childhood, which we assume is the primary school entry state. If this information is not available, we assume that the respondent entered primary school in her state of birth. Unlike NEPS, ALLBUS does not elicit the year of primary school entry. We assume that respondents entered primary school six years after their birth year, given that most students enter primary school at the age of six in Germany. We then merge the state-level data on compulsory religious education to the thus defined state and year of primary school entry of each individual.

We keep all individuals in the sample who provide the variables to approximate the state and year of primary school entry as well as basic control variables, and who entered primary school after 1949 and before 2005 in a West German state. The overall sample size equals 15,924

[21] SOEP also has a remote feature which would allow to access information on the county of residence, but not the county of schooling. In addition, it would be infeasible to merge other datasets with RemoteNEPS.

[22] Beyond the biennial survey pattern, there was one additional wave administered in 1991.

individuals. However, the number of observations varies substantially between variables, as not all questions were asked in all waves.

ALLBUS is the only dataset that contains all of our four religious outcome variables – religiosity, prayer, church-going, and affiliation. It is also comprehensive with regards to the other outcome variables, with the exception that it only covers two variables on attitudes towards gender and family roles (different gender duties in the home and attitudes towards marriage, see Table A2). ALLBUS contains the same basic control variables as NEPS. In addition, it provides information on the religion of the mother and father for a subset of individuals. We apply the same approach to address missing values described above for NEPS to ALLBUS and SOEP.

B.3 German Socio-Economic Panel (SOEP)

The German Socio-Economic Panel (SOEP) is a representative longitudinal survey of private households and individuals in Germany. It covers many topics including household composition, occupational biographies, employment, earnings, health, and satisfaction. We employ the SOEP Core 1984-2017 (v.34) which follows individuals since 1984 and has been repeatedly supplemented with new samples to account for changes that took place in the German society, such as samples of migrants and refugees (Goebel et al. (2019)). Analogous to NEPS, we use the most recent available non-missing value of a variable for each individual.

To approximate the state and year of primary school entry, we assume that individuals entered primary school in the state of their last school attendance, which is elicited in SOEP for a subset of respondents. For the other respondents, we assume that they entered primary school in their state of birth. As in ALLBUS, we assume that individuals entered primary school six years after their birth and accordingly merge status information on compulsory religious education.

We again keep all individuals in the sample who provide the variables to approximate the state and year of primary school entry as well as basic control variables, and who entered primary school after 1949 and before 2005 in a West German state. The resulting sample size equals 30,498 individuals.

SOEP contains two of the four religious outcome variables (church-going and religious affiliation) and two of the four variables measuring attitudes towards gender and family roles (different gender duties in the home and attitudes towards marriage, see Table A2). However, SOEP provides a comprehensive set of other outcomes, with a special focus on labor-market, educational, and ethical-value outcomes. In terms of control variables, SOEP is comparable to ALLBUS: In addition to the main control variables, it also contains information about the religion(s) of the mother and father for a subset of individuals.

B.4 Merging the three Datasets

NEPS, ALLBUS, and SOEP are collected independently from each other. Hence, their data structure and variables are not aligned. To merge the three datasets, we start by evaluating the questionnaires of the three datasets and select only variables for the merging procedure whose question wordings in the questionnaires are directly comparable.

For each selected variable, we recode the answer categories in each dataset to be directly comparable across datasets. This implies standardization in most cases, but occasionally also requires the recoding of variables to analogous dummy or categorical variables. Table A2 provides a list of the precise wording and number of answer categories for all outcome variables for each of the three datasets. As the religious outcome variables are elicited with varying numbers of answer categories in the different datasets, we standardize the religious measures within each dataset before merging the three surveys together and include dataset fixed effects throughout.[23]

For example, our main outcome variable religiosity in NEPS is phrased, “Faith and religion are part of everyday life for some people. What about you? Regardless of whether you belong to a religious community, how religious would you say you are?” There are four answer categories, “Not at all religious”, “Slightly non-religious”, “Slightly religious”, and “Very religious”. In ALLBUS, the question on religiosity is phrased, “Would you describe yourself as more religious or more not religious? We have a scale for this. Where would you place yourself on this scale?” The ten answer categories range from “not religious” to “religious”. In SOEP, there is no question on religiosity. Because of the different answer categories in NEPS and ALLBUS, both religiosity variables are standardized before being merged together.

Other variables also required re-coding of answer categories before standardization such that an increase in the variable implies a change in the same direction across datasets. For example, an increase in the raw variable on personal prayer in NEPS implies a decrease in the propensity to pray, whereas an increase in the corresponding raw variable in ALLBUS implies an increase in the propensity to pray. Throughout the paper, all answer categories are ordered before standardization such that an increase in the variable implies an increase in religiosity. The same is true for conservative attitudes towards gender and family roles.

Before merging the datasets, we create three dummy variables, one for each dataset, to indicate the respective data source. Finally, we order all variables analogously in the three datasets and then append NEPS with ALLBUS and SOEP.

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

Appendix C: Diagnostics of the Two-way Fixed Effects Specification

This appendix reports two diagnostic tests of the two-way fixed effects specification suggested by de Chaisemartin and D'Haultfœuille (2020) (section C.1) and by Goodman-Bacon (2021) (section C.2) that complement the results of the Callaway and Sant'Anna (2021) estimator reported in section 7 of the main text.[24]

C.1 Diagnostics by de Chaisemartin and D'Haultfœuille (2020)

The diagnostic test by de Chaisemartin and D'Haultfœuille (2020) is based on the observation that the estimate derived from a two-way fixed effects difference-in-differences estimation under the common trend assumption is a weighted sum of the average treatment effect in each group and period. Heterogeneity in treatment effects can lead to negative weights attached to specific group-period estimates. When estimating the weights of the group-period clusters in our setting, 46 of the 216 ATTs receive a negative weight, which sum to -0.070. Investigation indicates that negative weights are particularly frequent in estimates involving the two always-treated states in our setting, Berlin and Bremen, which effectively had adopted the reform by the time our sample starts in 1950.

When conducting the analysis without the two always-treated states, only five of the 125 ATTs receive a negative weight, which sum to only -0.004. Reassuringly, estimates of the treatment effects on all religious outcomes in our main specification are qualitatively unaffected when excluding Berlin and Bremen (Table A12).

[24] The result that excluding covariates does not change our qualitative results (Appendix D) indicates that cohorts with different covariates are unlikely to react differently to the reform, which further corroborates the assumption of homogeneity in treatment effects (e.g., Sun and Abraham (2021)).

C.2 Decomposition by Goodman-Bacon (2021)

In addition, we perform the Goodman-Bacon (2021) decomposition to display potential heterogeneity in the estimated effect components and clarify which relationships and groups matter most. Specifically, we analyze whether our main result holds in a subset of effect components that is immune to biases from negative weighting. To implement the analysis, we collapse data to means of state-cohort cells. To create a balanced panel, we drop observations in cohorts before 1949 or after 1991, which implies deletion of 21 percent of all state-cohort cells.

The graphs contained in Tables A13-A16 show scatterplots of two-by-two difference-in-differences estimates and their associated weights for the four measures of religious outcomes. The figures depict three types of two-group/two-period comparisons that differ by control group: (1) timing groups, i.e., groups whose treatment at different times serves as each other's control groups in two ways: those treated later serve as the control group for an earlier treatment group and those treated earlier serve as the control group for the later group; (2) always treated, where a group treated prior to the start of the analysis serves as the control group; and (3) never treated, where a group which never receives the treatment serves as the control group. In our setting, the two always-treated states are Berlin and Bremen. There is one never-treated state that never adopted the reform (Saarland). All other West German states adopted the reform within our estimation sample from 1950 to 2004.

The difference-in-differences estimators derived from the Goodman-Bacon (2021) decomposition, shown in the first line of Tables A13-A16, are similar to the results of our main specification. The estimator is in fact larger in absolute terms for three of the four religious outcomes, and only slightly smaller for religious affiliation. The overall effect of the reform on religiosity is -0.129 (compared to -0.071 in our main specification of Table 2). Across all four religious outcomes, the never vs. timing comparison receives the largest weight. This comparison is immune to biases from time-varying treatment effects and, reassuringly, displays a negative effect in all four decompositions. Overall, results of the diagnostic tests thus indicate that our findings are not driven by a setting that would give rise to negative weights.

Appendix D: Additional Robustness Analyses

Adding to the robustness analyses shown in section 7 of the main text, this appendix reports several tests that 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 (Tables A17-A20).

Similarly, results hardly change in specifications that exclude all covariates (Table A21). 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 (Table A22), 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. Results are very similar (Table A23).

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 seven groups of outcomes as presented in Tables 2, 4, A6, and A8 – religious outcomes, gender attitudes, family outcomes, labor-market 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, gender-attitude, family, and labor-market outcomes, but not ethical-value, political-value, and educational outcomes (Table A24). The effects (in absolute terms) are 8.7 percent of a standard deviation on the index of religious outcomes, 8.3 percent on gender attitudes, 6.5 percent on family outcomes, and 3.1 percent on labor-market outcomes. Consistent with measurement error in the separate measures, inference gets considerably more precise with the indices; estimated effects on religious, gender-attitude, and family 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 seven 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, gender-attitude, and family outcomes are statistically significant at conventional levels, whereas the effect on the labor-market index is no longer significant with this adjustment (Table A24).

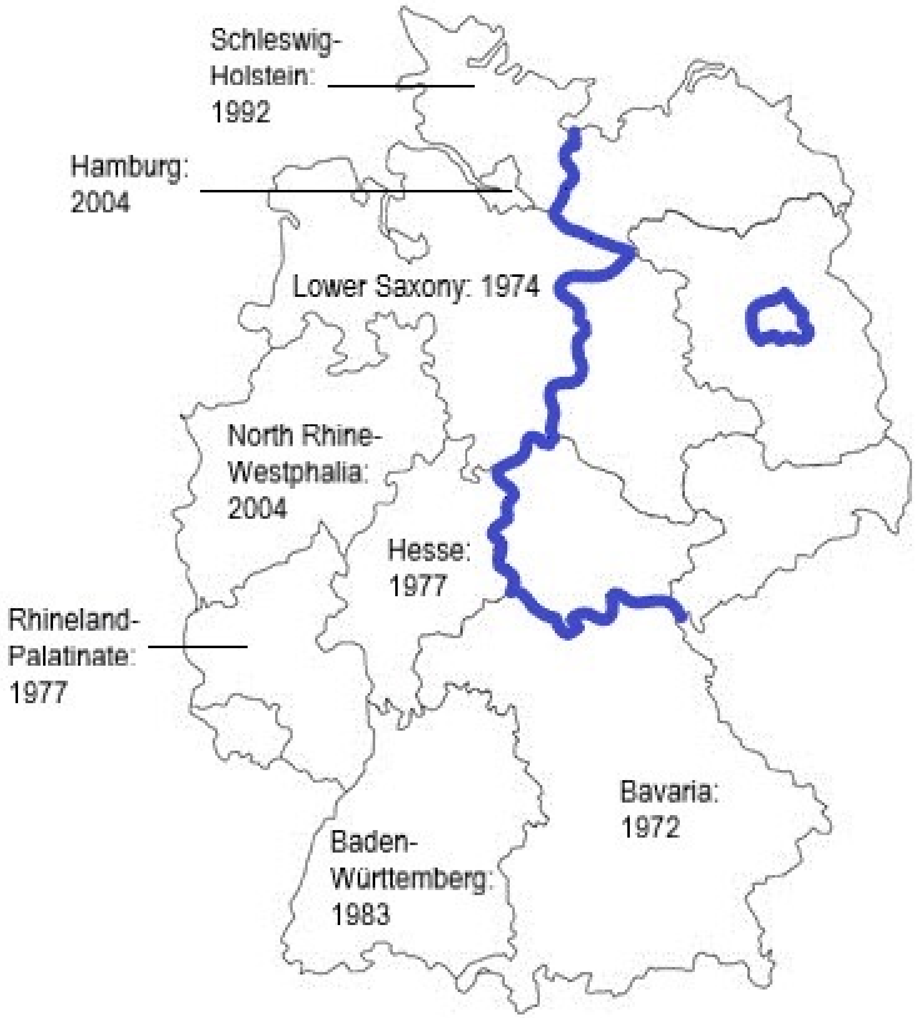
As another alternative way of inference, we randomly reshuffle the reform years across states. Figure A7 shows the distribution of the placebo coefficients on religiosity based on 1,000 permutations (randomization using actual reform years without replacement). The median placebo “reform” effect on religiosity equals -0.001. Our main estimate based on the accurate reform timing (-0.071) is larger (in absolute terms) than the 10^{th} 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 (Table A25).

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 (Tables A26-A28).

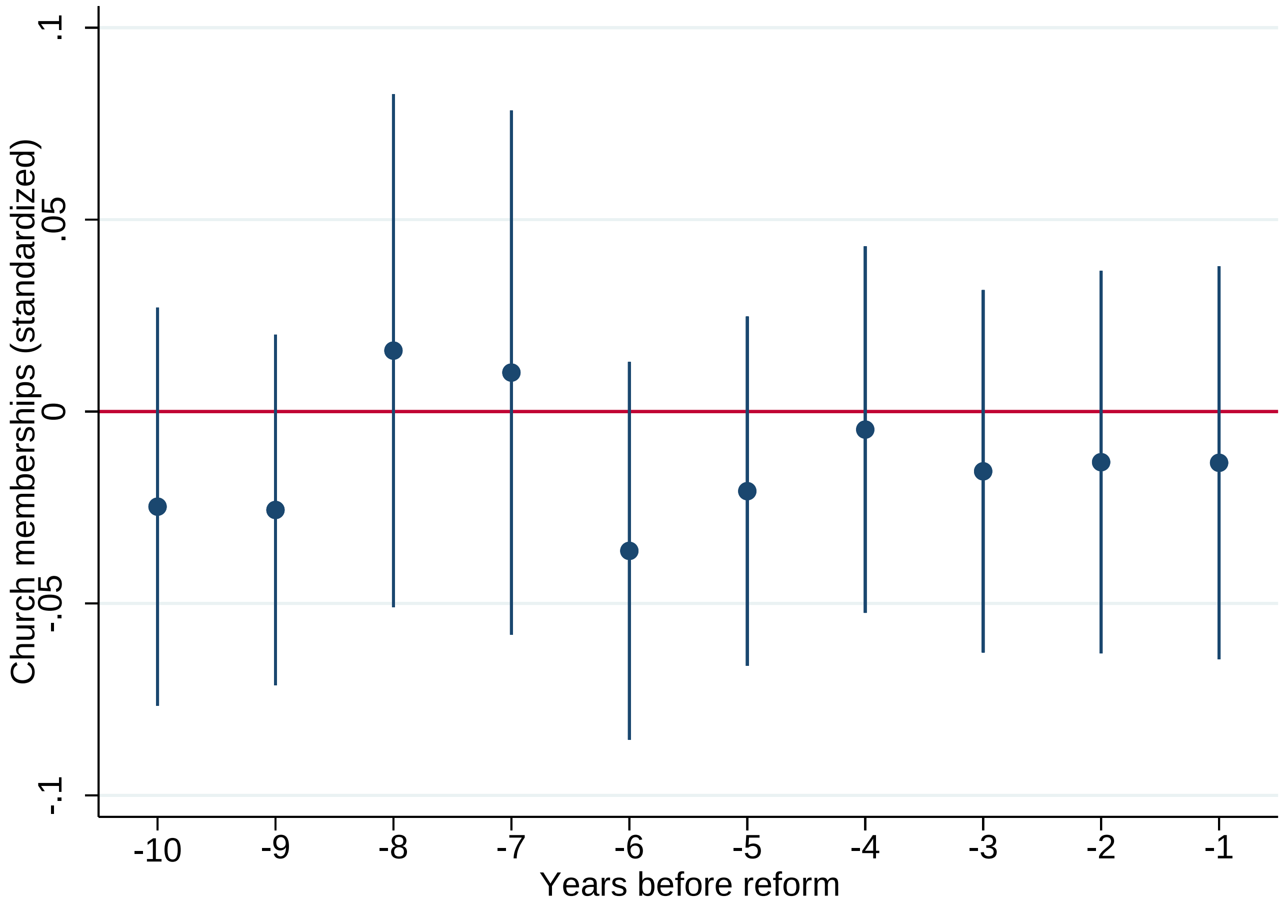
Appendix Figures and Tables

Figure A1: Religious education reforms in West German states



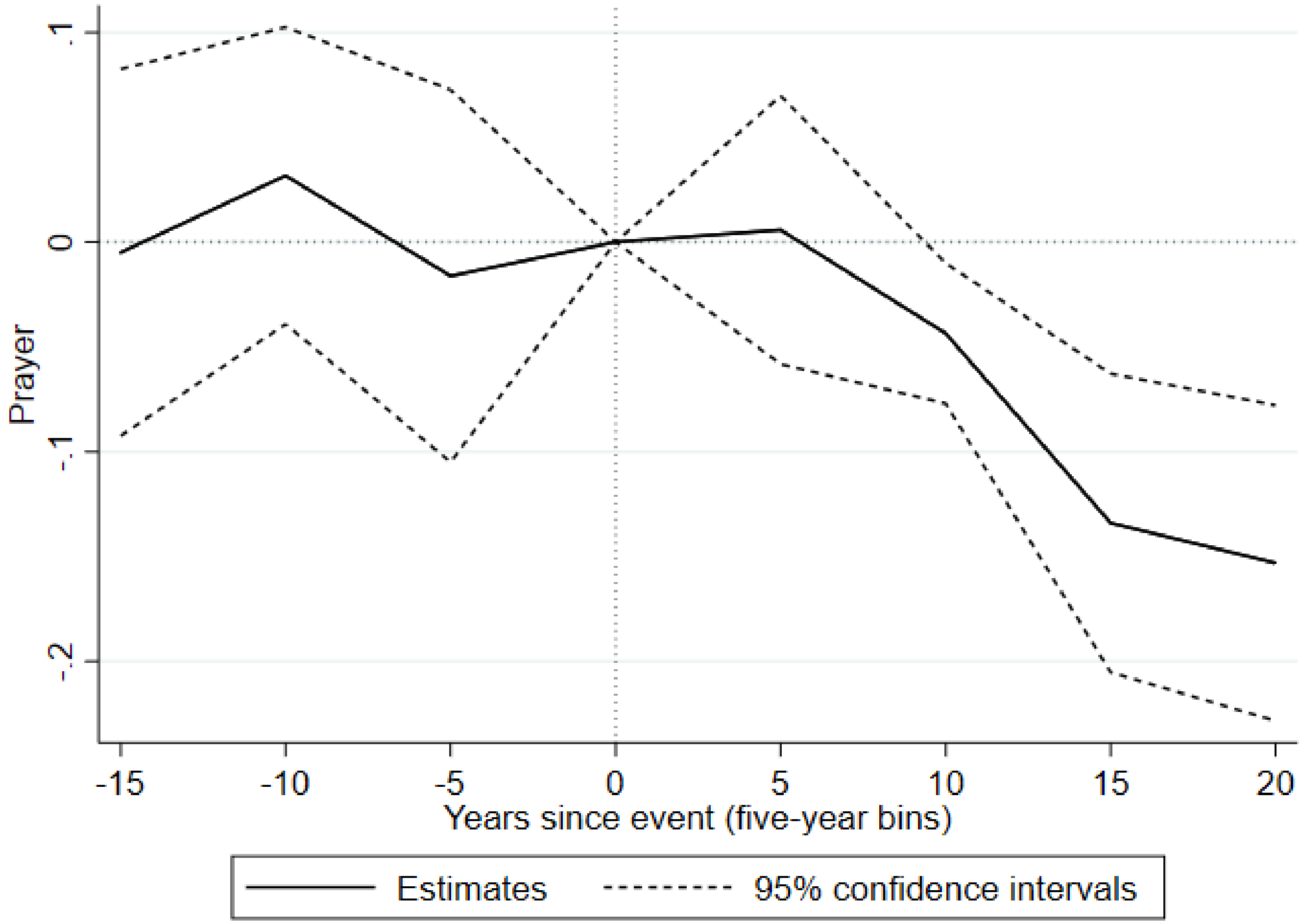
Notes: Map displays years of the abolishment of compulsory religious education of West German states.

Figure A2: State-wide church membership before the reforms



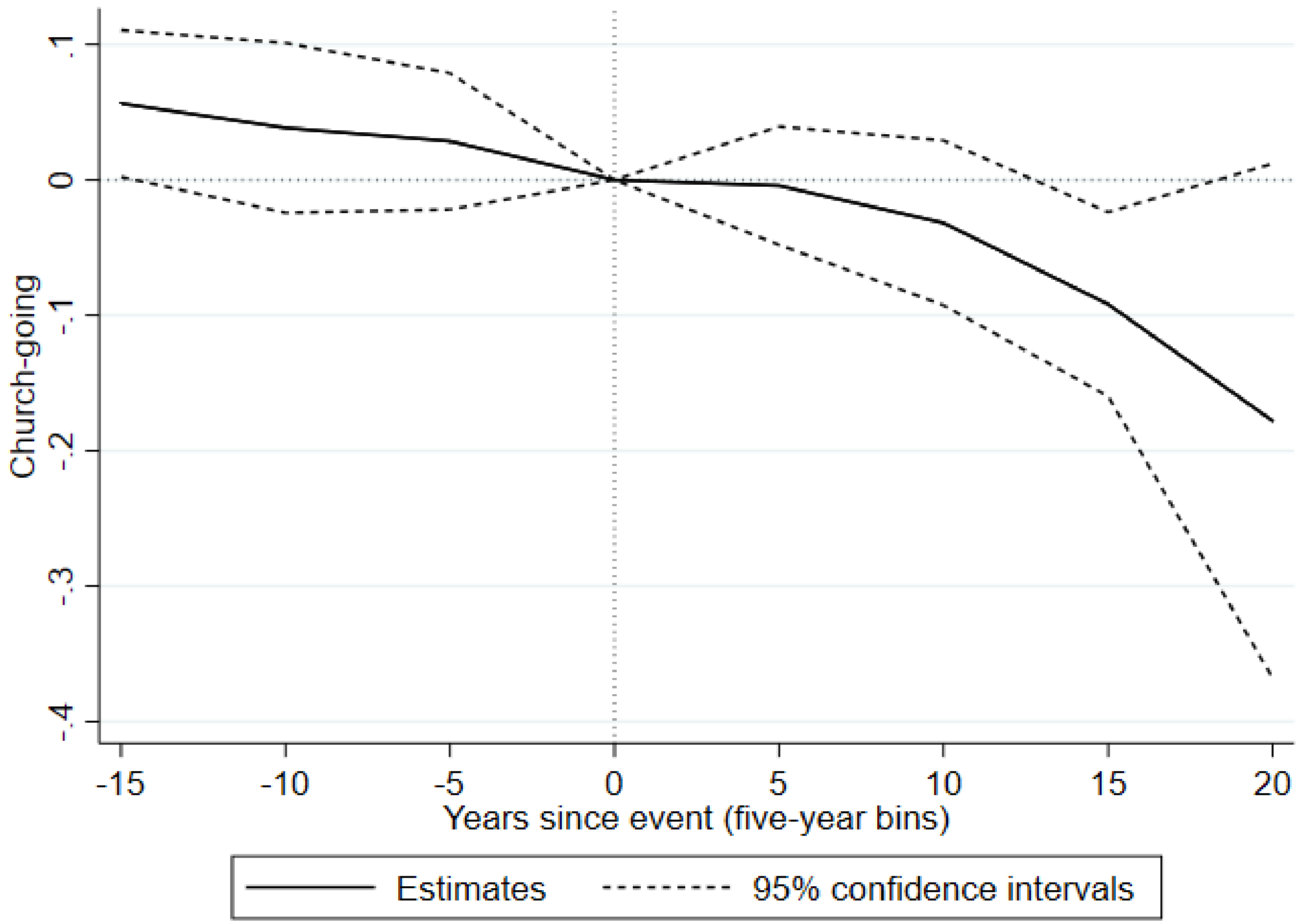
Notes: Coefficients from a regression of church memberships on the leads of reform events (95 percent confidence intervals). Dependent variable: memberships in the Catholic and Protestant Churches between 1965-2016 (standardized). Controls include population, year fixed effects, and state fixed effects. Data sources: Statistical Yearbooks of the Federal Statistical Office.

Figure A3: Non-parametric event-study estimates of effect on personal prayer



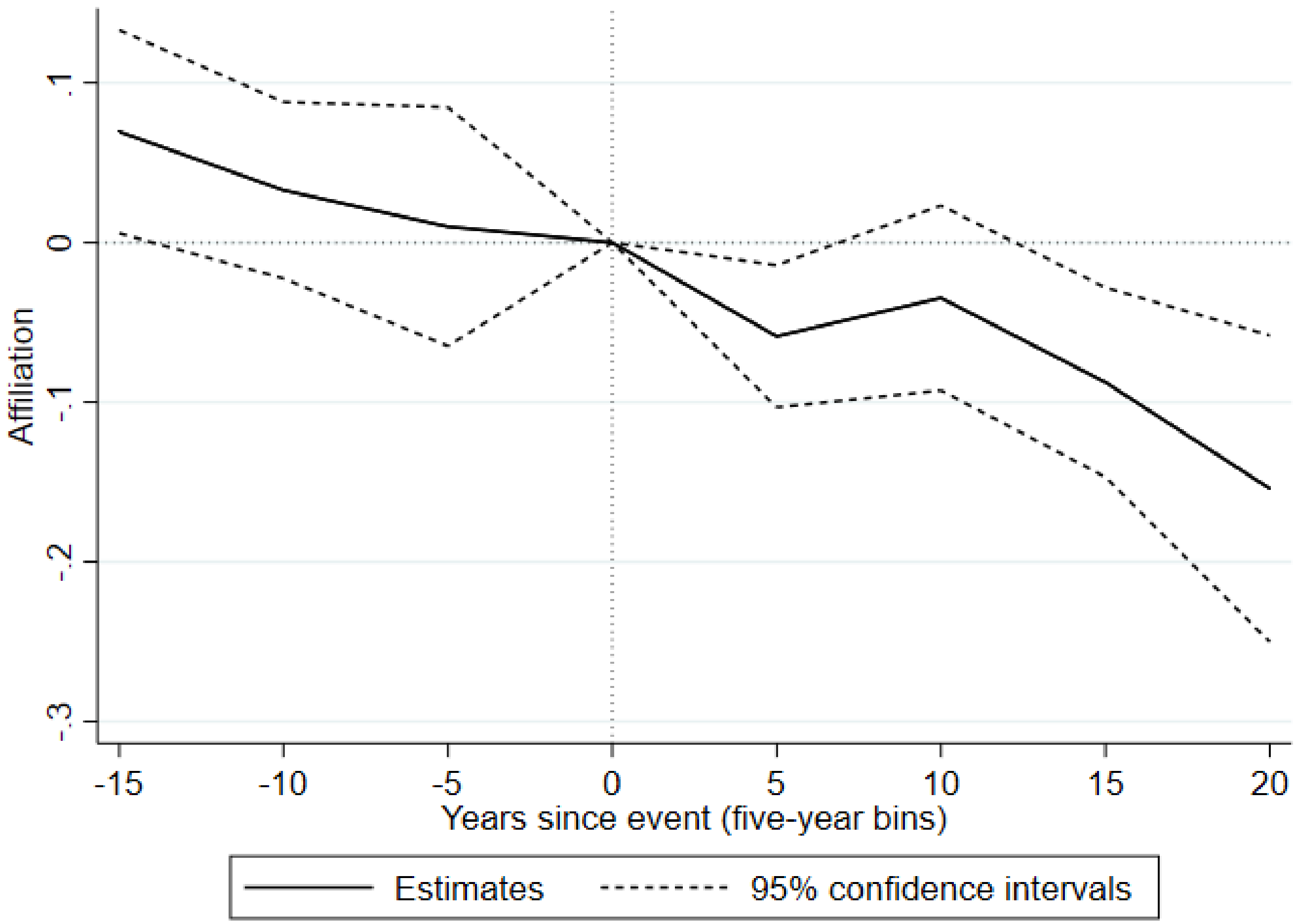
Notes: Coefficients from non-parametric event-study regressions and their 95 percent confidence intervals. Dependent variable: personal prayer (standardized, based on 7-point-scale NEPS question “How often do you pray?” and the same 11-point-scale ALLBUS question). Numbers on horizontal axis refer to 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 0.588 and 0.003, respectively. Data sources: National Education Panel Study (NEPS) Cohort 6; German General Social Survey (ALLBUS) Cumulation 1980-2016.

Figure A4: Non-parametric event-study estimates of effect on church-going



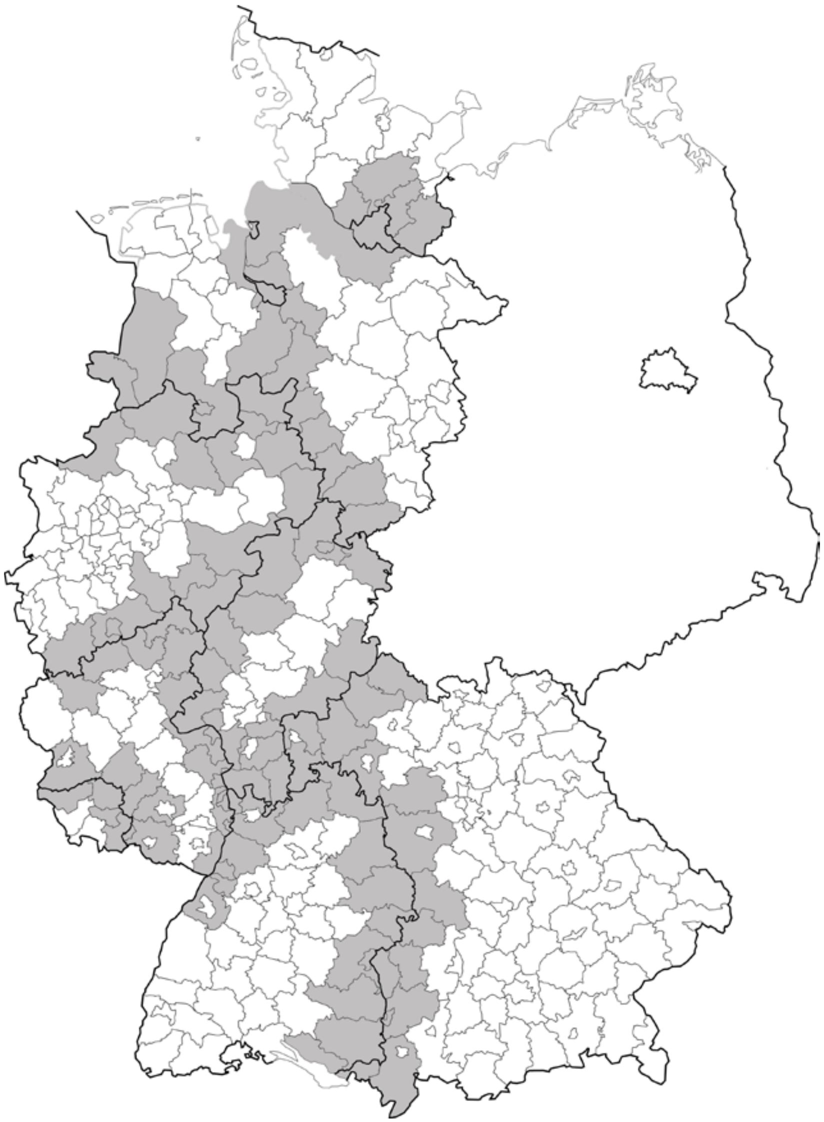
Notes: Coefficients from non-parametric event-study regressions and their 95 percent confidence intervals. Dependent variable: church-going (standardized, based on 6-point-scale ALLBUS question "As a rule, how often do you go to church?" and 4-point-scale SOEP question "Which of the following activities do you take part in during your free time? Attending church, religious events"). 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 0.139 and 0.087, respectively. Data sources: German General Social Survey (ALLBUS) Cumulation 1980-2016; German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34).

Figure A5: Non-parametric event-study estimates of effect on religious affiliation



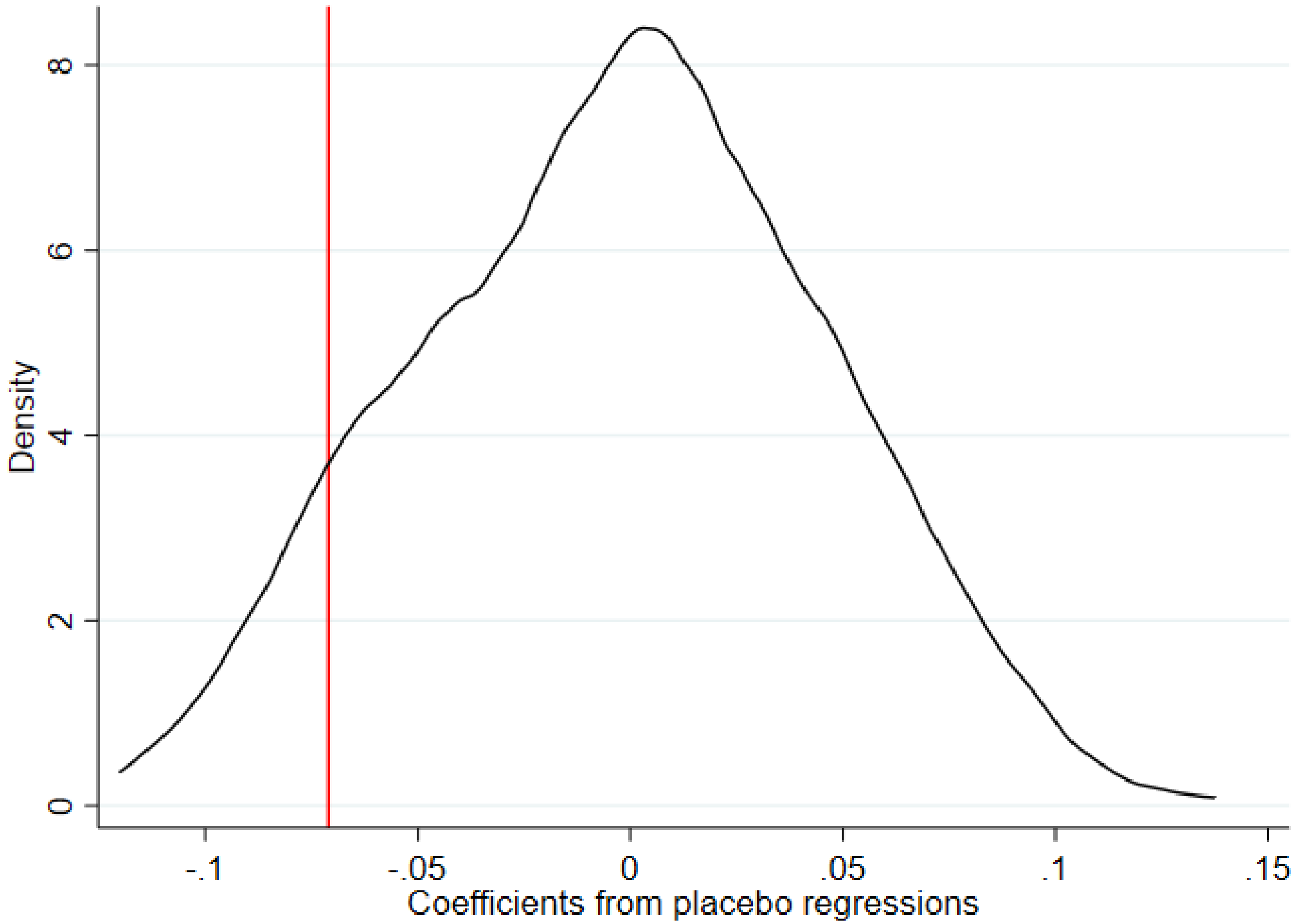
Notes: Coefficients from non-parametric event-study regressions and their 95 percent confidence intervals. Dependent variable: religious affiliation (standardized, based on 6-point-scale ALLBUS question “Which religion do you belong to?” and 11-point scale SOEP question “Do you belong to a church, religious community or faith?”). 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 0.052 and 0.020, respectively. Data sources: German General Social Survey (ALLBUS) Cumulation 1980-2016; German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34).

Figure A6: Counties in the border specification



Notes: Grey shaded counties form the sample of counties in the border specification that are directly at the border to another state. Thick and thin grey lines represent state and county borders, respectively.

Figure A7: Distribution of placebo estimates of reform effect on religious affiliation



Notes: Kernel density plot of coefficients from parametric regressions with randomly reshuffled reform years across reforming states (1,000 permutations). 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?”). Controls: gender, migration status, mother’s education, father’s education, as well as state, birthyear, survey and survey-year fixed effects. Red vertical line indicates coefficient of reform effect from baseline model (-0.071). Data sources: National Education Panel Study (NEPS) Cohort 6; German General Social Survey (ALLBUS) Cumulation 1980-2016.

Table A1: Descriptive statistics

	Mean	Std. dev.	Min.	Max.	Obs.
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
Prayer	0.00	1.00	-1.26	2.44	13,276
Church-going	0.00	1.00	-1.16	3.07	42,776
Affiliation	0.00	1.00	-2.22	0.57	45,925
Ethical-value outcomes					
Reciprocity	0.00	1.00	-5.11	0.97	21,150
Trust	0.00	1.00	-2.71	2.01	37,070
Risk-taking	0.00	1.00	-3.00	2.64	35,556
Volunteering	0.43	0.49	0.00	1.00	37,971
Life satisfaction	0.00	1.00	-4.85	1.56	48,177
Political-value outcomes					
Interest in politics	0.00	1.00	-2.47	2.00	52,970
Politics too complicated	0.00	1.00	-1.95	2.25	9,160
Satisfaction with democracy	0.00	1.00	-2.86	1.90	14,519
Political spectrum: right	0.00	1.00	-3.02	3.37	40,161
Vote in election	0.87	0.34	0.00	1.00	32,133
Vote left	0.57	0.49	0.00	1.00	27,088
Vote extreme	0.07	0.25	0.00	1.00	27,100
Attitudes towards gender and family roles					
Different gender suitability for professions	0.00	1.00	-1.90	1.28	8,868
Different gender duties in the home	0.00	1.00	-1.29	3.55	18,008
Gender use of technical devices	0.00	1.00	-1.06	2.52	8,859
Attitude towards marriage	0.00	1.00	-1.35	1.35	14,943

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Table A1 (continued)

	Mean	Std. dev.	Min.	Max.	Obs.
Family and labor-market outcomes					
Currently married	0.60	0.49	0.00	1.00	56,673
Number of children	1.38	1.25	0.00	12.00	52,668
Labor-force participation	0.82	0.38	0.00	1.00	58,168
Employment	0.78	0.41	0.00	1.00	58,168
Working hours	35.56	14.89	0.00	120.00	45,781
Earnings	7.14	0.90	0.00	11.61	44,935
Educational outcomes					
Years of education	12.96	2.83	6.00	25.00	42,772
Abitur	0.38	0.49	0.00	1.00	52,283
Age of first employment	21.11	3.88	1.33	65.25	38,985
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. The sums of the 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 A2: Outcome measures derived from the three datasets

	NEPS (1)	ALLBUS (2)	SOEP (3)
Religious outcomes			
Religiosity (s)	Regardless of whether you belong to a religious community, how religious would you say are you? (4)	Would you describe yourself as more religious or more not religious? (10)	–
Prayer (s)	How often do you pray? (7)	How often do you pray? (11)	–
Church-going (s)	–	As a rule, how often do you go to church? (6)	Which of the following activities do you take part in during your free time? Attending church, religious events (4)
Affiliation (s)	Do you belong to a faith or religion? (2)	Which religion do you belong to? (6)	Do you belong to a church, religious community or faith? (11)
Ethical-value outcomes			
Reciprocity (s)	–	I go out of my way to help somebody who has helped me before. (7)	I make particular effort to help someone who has previously helped me. (7)
Trust (s)	I trust other people easily, I believe in the goodness in people (5)	Some people think that most people can be trusted. Others think that one can't be careful enough when dealing with other people. What do you think? (4)	On the whole trust people (4)
Risk-taking (s)	How do you assess yourself: Are you generally willing to take risks or do you try to avoid risks? Please respond on a scale from 0 to 10. ‘0’ indicates that you are not willing to take risks while ‘10’ means that you are very much willing to take risks. You can use the numbers in between to stagger your answer. (11)	–	How do you rate yourself personally? In general, are you someone who is ready to take risks or do you try to avoid risks? (11)
Volunteering (d)	Have you ever been actively involved in clubs, organizations, initiatives or self-help groups before? (3)	Please tell me here, too, how often you do the following in your leisure time: Voluntary work in clubs, associations or community services (5)	Which of the following activities do you take part in during your free time? Please check off how often you do each activity: at least once a week, at least once a month, less often, never. Volunteer work in clubs or social services (4)

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Table A2 (continued)

	NEPS (1)	ALLBUS (2)	SOEP (3)
Life satisfaction (s)	I would like to begin by asking you a few questions about your current satisfaction with different aspects of your life. Please answer on a scale of 0 to 10. ‘0’ means that you are entirely unsatisfied, ‘10’ means that you are entirely satisfied. You can grade your assessment using the numbers in between. In general, how satisfied are you currently with your life? (11)	And now another general question. How satisfied are you – all in all – with your life at the moment? (11)	In conclusion, we would like to ask you about your satisfaction with your life in general. How satisfied are you with your life, all things considered? (11)
Political-value outcomes			
Interest in politics (s)	How much are interested in politics? Are you very interested, rather interested, little interested or not interested at all? (4)	How interested in politics are you? Very strongly, strongly, middling, very little, or not at all? (5)	Generally speaking, how interested are you in politics? (4)
Politics too complicated (s)	How often do politics seems so complicated to you that you don’t really understand what it’s all about? (5)	On this list, there are a number of opinions one can hear now and then. For each opinion, please tell me if you: completely agree, tend to agree, tend not to agree, or completely disagree: Politics is so complicated that somebody like me can’t understand what’s going on at all. (4)	–
Satisfaction with democracy (s)	–	Let’s turn to democracy in Germany: Generally speaking, how satisfied are you with democracy as practiced in Germany? (6)	Satisfaction with democracy in Germany (11)
Political spectrum: right (s)	In politics you sometimes talk about ‘left’ and ‘right’. Where on a scale from 0 to 10 would you grade yourself, if 0 is left and 10 is right? (11)	Many people use the terms “left” and “right” to describe differing political views. Here we have a scale that runs from left to right. If you think of your own political views, where would you place them on this scale? (10)	In politics people often talk about “left” and “right” when it comes to characterize different political attitudes. If you think about your own political views: Where would you place yours? (11)

(continued on next page)

Table A2 (continued)

	NEPS (1)	ALLBUS (2)	SOEP (3)
Vote in election (d)	Some people do not vote nowadays for various reasons. What about you? Did you vote during the last *Bundestag* election? (2)	Did you vote in last federal election? (2)	Attendance *Bundestag* election 2013 (2)
Vote left (d)	If *Bundestag* elections were to be held tomorrow, which party would you give your second vote to? (8)	If there was a federal election next Sunday, which party would you vote for with your second vote? (10)	And how was it at the last general election (*Bundestagswahl*) on September 22, 2013? Which party did you vote for? (9)
Vote extreme (d)	If *Bundestag* elections were to be held tomorrow, which party would you give your second vote to? (8)	If there was a federal election next Sunday, which party would you vote for with your second vote? (10)	And how was it at the last general election (*Bundestagswahl*) on September 22, 2013? Which party did you vote for? (9)
Attitudes towards gender and family roles			
Different gender suitability for professions (s)	Men are better suited for certain professions than women. Do you completely disagree, somewhat disagree, somewhat agree or agree completely? (4)	–	–
Different gender duties in the home (s)	Men and women should have the same duties in the home (4)	How do you and your partner share these activities in your household? Who does what? Cleaning the house/flat (6)	Men involved in housework (4)
Gender use of technical devices (s)	Women can use technical devices as well as men. (4)	–	–
Attitude towards marriage (s)	–	Do you think one should get married if one is living with a partner on a permanent basis? (3)	Marriage when living with partner permanent (4)
Family and labor-market outcomes			
Married (d)	Family status (4)	What is your marital status? (9)	What is your family status? (7)
Number of children (n)	Number of children	Do you have any children, and if so, how many?	Do you have or had children? If so, how much?

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Table A2 (continued)

	NEPS (1)	ALLBUS (2)	SOEP (3)
Labor-force participation (d)	Derived from: Currently employed? Currently unemployed? (2)	And now let's continue with employment and your occupation. Which of the categories on the card applies to you? (2)	Labor force status (11)
Employment (d)	Currently employed? (2)	And now let's continue with employment and your occupation. Which of the categories on the card applies to you? (2)	Labor force status (11)
Working hours (n)	How many hours per week do you actually work currently?	How many hours per week do you normally work in your main job, including overtime?	And how many hours do you generally work per week, including any overtime?
Earnings, log (n)	How high were your net earnings in your last month working? Please provide the sum after taxes and social insurance contributions. If you received extra compensation in your last month of working, such as vacation pay or back pay, please do not include this. Do, however, include overtime pay.	How high is your own net monthly income? By this I mean the amount remaining after deductions for tax and social security contributions.	What were your net earnings for the past month, after deductions for taxes and social insurance contributions, including overtime payments?
Educational outcomes			
Years of education (n)	Years of education = f(CASMIN)	Not counting the time you may have spent at a vocational school as part of your vocational training, how many years of schooling did you receive? If you went to university, please include the time you have spent there	Number of years of education
Abitur (d)	Which school-leaving certificate did you acquire? (8)	What general school leaving certificate do you have? (7)	What type of school-leaving certificate did you attain? (6)
Age of first employment (n)	Age at first employment (years)	–	How old were you when you first started working?

Notes: Translations of the original German questions from the official English codebook of the respective dataset. Scale of derived outcomes measure: s = standardized; n = number; d = dummy. For categorical variables, numbers in parentheses refer to the number of categories as presented in the respective dataset before recoding and merging. All gender and family role attitudinal outcomes are recoded such that an increase in the variable implies an increase in conservatism. 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 A3: Effects on religious outcomes: Non-parametric estimation

	Religiosity (1)	Prayer (2)	Church-going (3)	Affiliation (4)
Pre 15+	0.053	-0.005	0.056	0.069
	(0.128)	*(0.901)*	*(0.043)*	*(0.035)*
	[0.133]	*[0.898]*	*[0.014]*	*[0.135]*
Pre 10-14	0.055	0.032	0.038	0.033
	(0.253)	*(0.343)*	*(0.202)*	*(0.216)*
	[0.281]	*[0.325]*	*[0.386]*	*[0.402]*
Pre 5-9	0.007	-0.016	0.029	0.010
	(0.849)	*(0.693)*	*(0.235)*	*(0.773)*
	[0.881]	*[0.782]*	*[0.334]*	*[0.812]*
Post 1-5	-0.001	0.006	-0.004	-0.059
	(0.925)	*(0.845)*	*(0.833)*	*(0.015)*
	[0.917]	*[0.833]*	*[0.836]*	*[0.078]*
Post 6-10	-0.070	-0.044	-0.032	-0.035
	(0.085)	*(0.016)*	*(0.272)*	*(0.211)*
	[0.185]	*[0.008]*	*[0.342]*	*[0.234]*
Post 11-15	-0.125	-0.134	-0.092	-0.088
	(0.021)	*(0.002)*	*(0.013)*	*(0.008)*
	[0.076]	*[0.003]*	*[0.004]*	*[0.026]*
Post 16+	-0.168	-0.153	-0.178	-0.154
	(0.005)	*(0.001)*	*(0.063)*	*(0.005)*
	[0.068]	*[0.007]*	*[0.113]*	*[0.015]*
Observations	15,688	13,276	42,776	45,925

Notes: Dependent variables indicated in column headers. All dependent variables are standardized (see 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: 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 A4: Effect on religiosity: Dummy coding of religiosity

	(Rather or very) religious		Very religious	
	Linear probability model	Probit model	Linear probability model	Probit model
	(1)	(2)	(3)	(4)
Reform	-0.029	-0.029	-0.022	-0.021
	(0.066)	*(0.039)*	*(0.007)*	*(0.001)*
	[0.124]	*[0.073]*	*[0.064]*	*[0.092]*
State fixed effects	Yes	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	15,688	15,688	15,688	15,688

Notes: Columns (1) and (3): OLS; columns (2) and (4): average marginal treatment effect of probit model. Dependent variable: columns (1) and (2): dummy equaling one if respondent is rather religious or very religious, zero otherwise; columns (3) and (4): dummy equaling one if respondent is very religious, zero otherwise. ALLBUS religiosity scale (from 1 to 10) re-scaled as very religious = 9-10 and rather religious = 6-8. 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; German General Social Survey (ALLBUS) Cumulation 1980-2016.

Table A5: 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 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: 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 A6: Effects on ethical-value and political-value outcomes

Panel A: Ethical-value outcomes

	Reciprocity	Trust	Risk-taking	Volunteering	Life satisfaction
	(1)	(2)	(3)	(4)	(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	Politics too complicated	Satisfaction with democracy	Political spectrum: right	Vote in election	Vote left	Vote extreme
	(1)	(2)	(3)	(4)	(5)	(6)	(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 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; German General Social Survey (ALLBUS) Cumulation 1980-2016; German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34).

Table A7: Heterogeneous treatment effects on family and labor-market outcomes by Catholic and Protestant areas

Panel A: Attitudes towards gender and family roles

	Different gender suitability for professions		Different gender duties in the home		Gender use of technical devices		Attitudes towards marriage	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Catholic	Protestant	Catholic	Protestant	Catholic	Protestant	Catholic	Protestant
Reform	-0.118	-0.045	-0.085	-0.043	-0.078	-0.048	–	–
	(0.063)	*(0.251)*	*(0.239)*	*(0.278)*	*(0.102)*	*(0.405)*		
	[0.276]	*[0.285]*	*[0.420]*	*[0.313]*	*[0.134]*	*[0.562]*		

Panel B: Family and labor-market outcomes

	Married		Number of children		Labor-force participation		Employment		Working hours		Earnings	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Cath.	Prot.	Cath.	Prot.	Cath.	Prot.	Cath.	Prot.	Cath.	Prot.	Cath.	Prot.
Reform	-0.020	-0.014	-0.119	-0.080	0.012	0.010	0.037	0.021	1.050	1.236	0.052	0.002
	(0.369)	*(0.290)*	*(0.022)*	*(0.019)*	*(0.175)*	*(0.515)*	*(0.002)*	*(0.244)*	*(0.155)*	*(0.083)*	*(0.030)*	*(0.962)*
	[0.421]	*[0.292]*	*[0.208]*	*[0.050]*	*[0.199]*	*[0.514]*	*[0.026]*	*[0.276]*	*[0.157]*	*[0.116]*	*[0.089]*	*[0.964]*

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 Appendix Table A2 for details). Controls: gender, migration status, mother's education, father's education, survey and survey-year fixed effects. 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). 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; German General Social Survey (ALLBUS) Cumulation 1980-2016; German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34).

Table A8: 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 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; German General Social Survey (ALLBUS) Cumulation 1980-2016; German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34).

Table A9: Effects on religious outcomes: Border specification with county-pair fixed effects

	Religiosity	Prayer	Affiliation
	(1)	(2)	(3)
Reform	-0.162	-0.169	0.006
	(0.022)	*(0.063)*	*(0.883)*
	[0.007]	*[0.036]*	*[0.877]*
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 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.

Table A10: Effects on religious outcomes: Callaway and Sant'Anna estimator

	Religiosity		Prayer		Church-going		Affiliation	
	Without controls	With controls	Without controls	With controls	Without controls	With controls	Without controls	With controls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reform	-0.086	-0.114	-0.109	-0.131	-0.097	-0.105	-0.078	-0.098
	(0.061)	*(0.042)*	*(0.136)*	*(0.052)*	*(0.112)*	*(0.095)*	*(0.000)*	*(0.000)*
Observations	15,066	15,063	12,821	12,821	41,232	41,219	44,187	44,187

Notes: Simple average treatment effects based on Callaway and Sant'Anna (2021), with not yet treated units and never-treated units as controls. Dependent variables indicated in column headers. All dependent variables are standardized (see Table A2 for details). Controls (if included, as indicated in the column header): gender, migration status, mother's education, father's education, and survey fixed effects. Estimator: DR IPW estimator. Inference: *p*-values with clustering at the state level. Implemented using Stata package csdid, version 1.6 (Rios-Avila et al. (2021)). 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 A11: Curricula before and after the reform: The case of Bavaria

Syllabus of Catholic religious education, 1967	Syllabus of Catholic religious education, 1979	Syllabus of Ethics, 1986
Religious education in the classroom is only supposed to be a part of a bigger all-encompassing religious education, the catechesis. (p. 666, Part IV 4.) Catechesis is defined as the instruction and indoctrination of the practices and beliefs of the Catholic Church. (p. 663, Part III 1.) (p. 666, Part IV 5.) Religious education teachers are warned from adopting general pedagogical values that are taught in school since they are based on anthropocentric pedagogic which is not compliant with the nature of catechesis. (p. 667, Part V)	Content-wise religious education is supposed to be seen as relating to catechesis, but the organization and shape of religious education as a school subject underlies the educational mandate of the state. (p. 102, 1.)	The syllabus discusses ethical questions that are important in life in our time and covers them more and more in depth in every grade. Open and focused discussion is important and should be encouraged by the teacher. In discussion students are supposed to formulate their own findings and use them to explore ethical solutions and ways of acting for different situations in life. Discussions of ethical questions are supposed to be based on concrete situations that are drawn from real life.
Religious education is to be: - Centered around reality not some abstract concept - Integrated in school and not separate from all other courses - Focused on Jesus Christ and bringing across the “basic truth” and “central message” of the catholic faith and the bible - Dialogical structure of education as interpersonal process which is based in encounter/connection between god and man - Application of living with faith (pp. 667-668, Part V)	**Religious education is to be:** - Very clearly structured with set topics, content, methods and controls - Learning targets and content of lessons are mandatory and must be covered as laid out in syllabus - Interactive, events and field trips should be used to enhance learning experience and make it more connected to life (pp. 103-106)	

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Table A11 (continued)

Syllabus of Catholic religious education, 1967	Syllabus of Catholic religious education, 1979	Syllabus of Ethics, 1986
Main goals of religious education: - Introduction and instruction of prayer as the central way of self-disclosure to god - Guide to having the church in one's life - Guide to dealing with the unfaith of one's environment - Formation of one's conscience - Gender education must be done with help of parents (pp. 669-672, Part VI)	**Main goals of religious education:** - Religious education is supposed to enable responsible thinking and behavior based on religion and faith - Reflect on and question the purpose of human life and the world - Break up pretended faith and thoughtless unfaith. In doing so helping to prevent a degeneration of pluralism into "passive indifference". - Aid faithful student to be more actively connected to religion, aid the "searching" student in finding the answers of the church to his questions, give the unfaithful student opportunity to become clearer in his viewpoint or change it (pp. 102-103)	**Main goals of ethics:** - Guide students towards responsible actions in their personal life and in society - Show the commonalities of general ethical values and Christian values - Teaching tolerance towards others
Syllabus in grades 5-10: - Grade 5-6: kids' development peaks, they are increasingly able to think critically - Grade 7: puberty causes "a crisis" and kids change their attitude about what's important and who their role models are, also "sexual impulses disturb the young adult" - Grades 8-9 are supposed to cover the current themes in the church from a historical standpoint - Grade 9 is supposed to help young adults answer important questions in their life - Despite its big advantages religious education in the classroom is limited through the compulsory atmosphere in school and should be complemented with "religious community days" (pp. 679-689, Part Lehrpläne)	**Syllabus in grades 5-10:** - Grade 5: students are supposed to recognize faith and religion as something to guide them (pp. 107-114) - Grade 6: As they approach the end of their childhood students are supposed to capture and open themselves up to the guiding power of the Christian faith (pp. 115-123) - Grade 7: in a time of personal insecurity students are supposed to discover how faith can help solve their own problems and difficulties (pp. 123-131) - Grade 8: Amidst puberty students are supposed to experiment with the Christian way of life and consider it a serious possibility in shaping their own life (pp. 132-138) - Grade 9: at the end of the first period in their life students are supposed to perceive faith as life-improving and life as being open for faith (page 139-144)	**Syllabus in grades 5-10:** - The two main topics are "Man and his/her personal life" and "Man in a society with others" - Every grade works on both topics so that both are only fully covered at the end of ninth grade. - Subtopics in "Man and his/her personal life": time-management, good deeds, seeing beauty, independent learning, meaningful free-time activities, making decisions, social impact of work … - Subtopics in "Man in a society with others": being accepted, ending conflict, behavior towards strangers/foreigners, causes for prejudice, respecting freedom of opinion, meaning of authority, meaning of guilt, dealing with guilt, …
Syllabus is structured by giving one or two topics per grade. Topics are given without specific guidance on how to teach these topics.	Syllabus states up to eight topics, for every grade, all of which are explained in how they are to be taught and what they should encompass.	Syllabus is divided into two main topics which are both discussed fifth through ninth/tenth grade. In each grade, different subtopics of the two main topics are discussed without specific guidance how these topics are to be taught.

Notes: Own depiction based on the respective curricula as published in the *Amtsblatt des bayerischen Staatsministeriums für Unterricht und Kultus*.

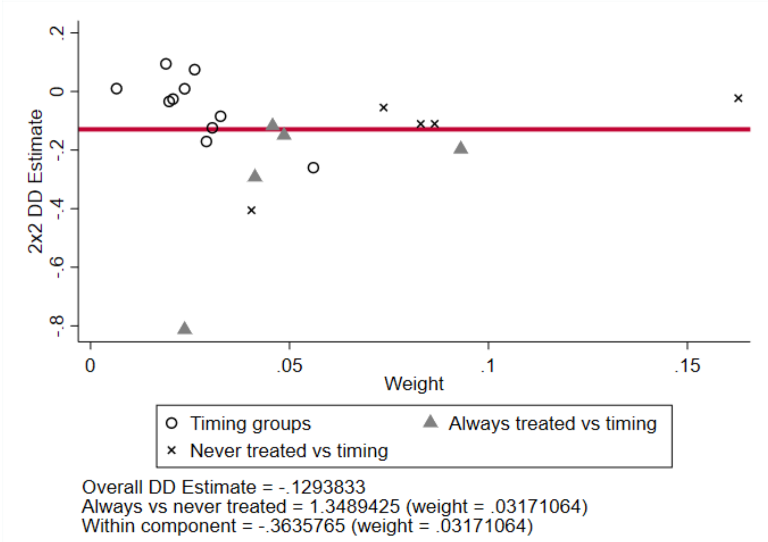
Table A12: Effects on religious outcomes: Excluding Berlin and Bremen

	Religiosity	Prayer	Church-going	Affiliation
	(1)	(2)	(3)	(4)
Reform	-0.073	-0.050	-0.052	-0.087
	(0.028)	*(0.084)*	*(0.059)*	*(0.007)*
	[0.095]	*[0.097]*	*[0.058]*	*[0.051]*
State fixed effects	Yes	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	15,066	12,821	41,232	44,193

Notes: Observations from Berlin and Bremen are excluded from the sample. Dependent variables indicated in column headers. All dependent variables are standardized (see 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: 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 A13: Goodman-Bacon decomposition of effect on religiosity

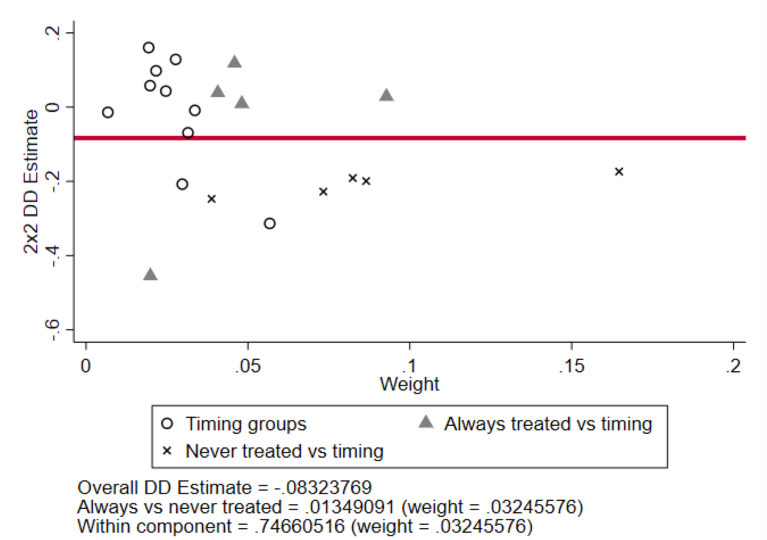
	Beta	Total weight
	(1)	(2)
Overall	-0.129	-
Timing groups	-0.087	0.263
Always vs. timing	-0.247	0.252
Never vs. timing	-0.095	0.446
Always vs. never	1.349	0.006
Within	-0.364	0.032



Notes: Decomposition of difference-in-differences estimator with variation in treatment timing based on Goodman-Bacon (2021). The figure shows a scatterplot of all two-group/two-period difference-in-difference estimates and their associated weights in the two-way fixed effects model. Depicted types differ by control group: (1) timing groups, or groups whose treatment at different times serves as each other's control groups; (2) always treated, where a group treated prior to the start of the analysis serves as the control group; and (3) never treated, where a group which never receives the treatment serves as the control group. Also shown are the component due to variation in controls across always treated and never treated groups, as well as the "within" residual component. Data are collapsed to means of state-cohort cells. Observations with birth year before 1949 or after 1991 are dropped to create a balanced panel. Dependent variable: religiosity (standardized). Controls: gender, migration status, mother's education, father's education, survey and survey-year fixed effects. 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 A14: Goodman-Bacon decomposition of effect on personal prayer

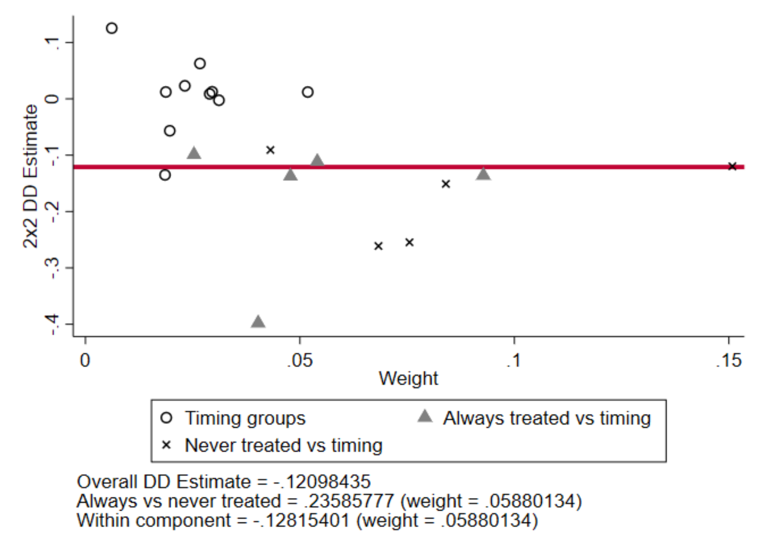
	Beta (1)	Total weight (2)
Overall	-0.083	-
Timing groups	-0.056	0.271
Always vs. timing	0.004	0.247
Never vs. timing	-0.196	0.445
Always vs. never	0.013	0.005
Within	0.747	0.032



Notes: Decomposition of difference-in-differences estimator with variation in treatment timing based on Goodman-Bacon (2021). The figure shows a scatterplot of all two-group/two-period difference-in-difference estimates and their associated weights in the two-way fixed effects model. Depicted types differ by control group: (1) timing groups, or groups whose treatment at different times serves as each other's control groups; (2) always treated, where a group treated prior to the start of the analysis serves as the control group; and (3) never treated, where a group which never receives the treatment serves as the control group. Also shown are the component due to variation in controls across always treated and never treated groups, as well as the "within" residual component. Data are collapsed to means of state-cohort cells. Observations with birth year before 1949 or after 1991 are dropped to create a balanced panel. Dependent variable: prayer (standardized). Controls: gender, migration status, mother's education, father's education, survey and survey-year fixed effects. 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 A15: Goodman-Bacon decomposition of effect on church-going

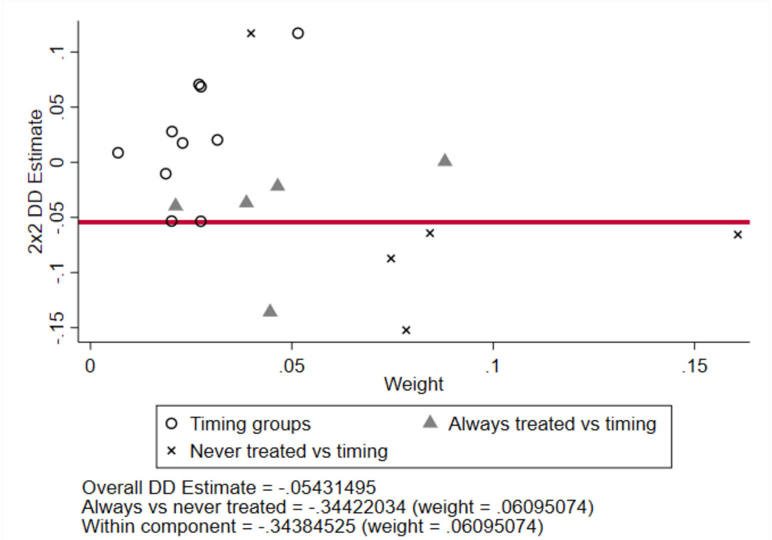
	Beta	Total weight
	(1)	(2)
Overall	-0.121	-
Timing groups	0.003	0.254
Always vs. timing	-0.168	0.260
Never vs. timing	-0.169	0.421
Always vs. never	0.236	0.006
Within	-0.128	0.059



Notes: Decomposition of difference-in-differences estimator with variation in treatment timing based on Goodman-Bacon (2021). The figure shows a scatterplot of all two-group/two-period difference-in-difference estimates and their associated weights in the two-way fixed effects model. Depicted types differ by control group: (1) timing groups, or groups whose treatment at different times serves as each other's control groups; (2) always treated, where a group treated prior to the start of the analysis serves as the control group; and (3) never treated, where a group which never receives the treatment serves as the control group. Also shown are the component due to variation in controls across always treated and never treated groups, as well as the "within" residual component. Data are collapsed to means of state-cohort cells. Observations with birth year before 1949 or after 1991 are dropped to create a balanced panel. Dependent variable: church-going (standardized). Controls: gender, migration status, mother's education, father's education, survey and survey-year fixed effects. 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 A16: Goodman-Bacon decomposition of effect on religious affiliation

	Beta	Total weight
	(1)	(2)
Overall	-0.054	-
Timing groups	0.035	0.253
Always vs. timing	-0.039	0.239
Never vs. timing	-0.068	0.438
Always vs. never	-0.344	0.010
Within	-0.344	0.060



Notes: Decomposition of difference-in-differences estimator with variation in treatment timing based on Goodman-Bacon (2021). The figure shows a scatterplot of all two-group/two-period difference-in-difference estimates and their associated weights in the two-way fixed effects model. Depicted types differ by control group: (1) timing groups, or groups whose treatment at different times serves as each other's control groups; (2) always treated, where a group treated prior to the start of the analysis serves as the control group; and (3) never treated, where a group which never receives the treatment serves as the control group. Also shown are the component due to variation in controls across always treated and never treated groups, as well as the "within" residual component. Data are collapsed to means of state-cohort cells. Observations with birth year before 1949 or after 1991 are dropped to create a balanced panel. Dependent variable: affiliation (standardized). Controls: gender, migration status, mother's education, father's education, survey and survey-year fixed effects. 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 A17: Effect on religiosity: Controls for other school reforms

	Compulsory schooling	G8/G9	Philosophy	Sexual education	Political education	All other school reforms
	(1)	(2)	(3)	(4)	(5)	(6)
Reform	-0.072	-0.073	-0.085	-0.071	-0.065	-0.067
	(0.040)	*(0.017)*	*(0.014)*	*(0.048)*	*(0.041)*	*(0.067)*
	[0.058]	*[0.058]*	*[0.073]*	*[0.236]*	*[0.140]*	*[0.135]*
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15,198	15,198	15,185	15,198	15,198	15,198

Notes: Dependent variable: Standardized religiosity (see Table A2 for details). Regressions include additional controls for school reforms as enacted in the state and year of a respondent's primary school entry, as indicated in the column header: (1) years of compulsory schooling (between 8 and 10); (2) dummy equaling one if duration of *Gymnasium* is 8 years, zero otherwise; (3) dummy equaling one if philosophy is taught in school (above and beyond the school subject "ethics" evaluated in this paper), zero otherwise; (4) dummy equaling one if sexual education is taught in school, zero otherwise; (5) dummy variable equaling one if political education is taught in school, zero otherwise; (6) all five school reforms together. 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; German General Social Survey (ALLBUS) Cumulation 1980-2016.

Table A18: Effect on personal prayer: Controls for other school reforms

	Compulsory schooling	G8/G9	Philosophy	Sexual education	Political education	All other school reforms
	(1)	(2)	(3)	(4)	(5)	(6)
Reform	-0.063	-0.054	-0.061	-0.054	-0.040	-0.068
	(0.098)	*(0.059)*	*(0.077)*	*(0.060)*	*(0.098)*	*(0.063)*
	[0.069]	*[0.072]*	*[0.134]*	*[0.062]*	*[0.084]*	*[0.058]*
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,929	12,929	12,915	12,929	12,929	12,929

Notes: Dependent variable: Standardized personal prayer (see Table A2 for details). Regressions include additional controls for school reforms as enacted in the state and year of a respondent's primary school entry, as indicated in the column header: (1) years of compulsory schooling (between 8 and 10); (2) dummy equaling one if duration of *Gymnasium* is 8 years, zero otherwise; (3) dummy equaling one if philosophy is taught in school (above and beyond the school subject "ethics" evaluated in this paper), zero otherwise; (4) dummy equaling one if sexual education is taught in school, zero otherwise; (5) dummy variable equaling one if political education is taught in school, zero otherwise; (6) all five school reforms together. 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; German General Social Survey (ALLBUS) Cumulation 1980-2016.

Table A19: Effect on church-going: Controls for other school reforms

	Compulsory schooling	G8/G9	Philosophy	Sexual education	Political education	All other school reforms
	(1)	(2)	(3)	(4)	(5)	(6)
Reform	-0.059	-0.054	-0.076	-0.049	-0.042	-0.049
	(0.055)	*(0.041)*	*(0.018)*	*(0.094)*	*(0.103)*	*(0.078)*
	[0.044]	*[0.042]*	*[0.062]*	*[0.144]*	*[0.134]*	*[0.102]*
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	41,559	41,559	41,457	41,559	41,559	41,559

Notes: Dependent variable: Standardized church-going (see Table A2 for details). Regressions include additional controls for school reforms as enacted in the state and year of a respondent's primary school entry, as indicated in the column header: (1) years of compulsory schooling (between 8 and 10); (2) dummy equaling one if duration of *Gymnasium* is 8 years, zero otherwise; (3) dummy equaling one if philosophy is taught in school (above and beyond the school subject "ethics" evaluated in this paper), zero otherwise; (4) dummy equaling one if sexual education is taught in school, zero otherwise; (5) dummy variable equaling one if political education is taught in school, zero otherwise; (6) all five school reforms together. 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; German General Social Survey (ALLBUS) Cumulation 1980-2016.

Table A20: Effect on religious affiliation: Controls for other school reforms

	Compulsory schooling	G8/G9	Philosophy	Sexual education	Political education	All other school reforms
	(1)	(2)	(3)	(4)	(5)	(6)
Reform	-0.081	-0.087	-0.096	-0.088	-0.080	-0.075
	(0.012)	*(0.004)*	*(0.007)*	*(0.003)*	*(0.002)*	*(0.009)*
	[0.025]	*[0.032]*	*[0.053]*	*[0.039]*	*[0.021]*	*[0.252]*
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	44,571	44,571	44,465	44,571	44,571	44,571

Notes: Dependent variable: Standardized religious affiliation (see Table A2 for details). Regressions include additional controls for school reforms as enacted in the state and year of a respondent's primary school entry, as indicated in the column header: (1) years of compulsory schooling (between 8 and 10); (2) dummy equaling one if duration of *Gymnasium* is 8 years, zero otherwise; (3) dummy equaling one if philosophy is taught in school (above and beyond the school subject "ethics" evaluated in this paper), zero otherwise; (4) dummy equaling one if sexual education is taught in school, zero otherwise; (5) dummy variable equaling one if political education is taught in school, zero otherwise; (6) all five school reforms together. 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; German General Social Survey (ALLBUS) Cumulation 1980-2016.

Table A21: Effects on religious outcomes: No control variables

	Religiosity	Prayer	Church-going	Affiliation
	(1)	(2)	(3)	(4)
Reform	-0.073	-0.038	-0.068	-0.079
	(0.011)	*(0.181)*	*(0.024)*	*(0.015)*
	[0.032]	*[0.217]*	*[0.026]*	*[0.087]*
State fixed effects	Yes	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes	Yes
Survey-year fixed effects	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 Table A2 for details). 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; German General Social Survey (ALLBUS) Cumulation 1980-2016; German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34).

Table A22: Effects on religious outcomes: Dosage treatment

	Religiosity	Prayer	Church-going	Affiliation
	(1)	(2)	(3)	(4)
Reform	-0.092	-0.047	-0.074	-0.097
	(0.016)	*(0.146)*	*(0.032)*	*(0.012)*
	[0.053]	*[0.156]*	*[0.042]*	*[0.010]*
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 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: 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 A23: Effects on religious outcomes: Reform timing coded at secondary school entry

	Religiosity	Prayer	Church-going	Affiliation
	(1)	(2)	(3)	(4)
Reform	-0.071	-0.041	-0.058	-0.070
	(0.040)	*(0.154)*	*(0.035)*	*(0.035)*
	[0.121]	*[0.128]*	*[0.050]*	*[0.133]*
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 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: 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 A24: Effects on outcome indices and adjustment for multiple inference

	Religious outcomes	Gender attitude outcomes	Family outcomes	Labor-market outcomes	Ethical-value outcomes	Political-value outcome	Educational outcomes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Reform	-0.087	-0.083	-0.065	0.031	-0.008	0.010	-0.020
	(0.002)	*(0.001)*	*(0.005)*	*(0.032)*	*(0.586)*	*(0.637)*	*(0.439)*
	[0.006]	*[0.017]*	*[0.012]*	*[0.073]*	*[0.672]*	*[0.799]*	*[0.577]*
Bonferroni-adjusted p-values	*(0.014)*	*(0.007)*	*(0.035)*	*(0.224)*	*(1.000)*	*(1.000)*	*(1.000)*
	[0.042]	*[0.119]*	*[0.084]*	*[0.511]*	*[1.000]*	*[1.000]*	*[1.000]*
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	53,044	25,866	57,972	58,178	54,259	52,992	57,311

Notes: Dependent variables indicated in column headers. All dependent variables are indices derived as equally weighted averages of their respective underlying standardized components. The respective index components are shown in the following tables: (1) Table 2; (2) Panel A of Table 4; (3) Panel B of Table 4, columns 1-2; (4) Panel B of Table 4, columns 3-6; (5) Panel A of Table A6; (6) Panel B of Table A6, columns 1-3 and 5 ("politics too complicated" inverted); (7) Table A8. 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); Bonferroni family-wise error rate adjustment of *p*-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 A25: Effects on religious outcomes: Excluding early reforming states

	Religiosity (1)	Prayer (2)	Church-going (3)	Affiliation (4)
Reform	-0.090	-0.023	-0.121	-0.087
	(0.014)	*(0.608)*	*(0.062)*	*(0.037)*
	[0.086]	*[0.538]*	*[0.365]*	*[0.342]*
State fixed effects	Yes	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	8,320	7,046	23,039	24,245

Notes: Observations from Bavaria, Hesse, Lower Saxony, and Rhineland-Palatinate are excluded from the sample. Dependent variables indicated in column headers. All dependent variables are standardized (see 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: 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 A26: Effects on religious outcomes: Only NEPS data

	Religiosity	Prayer	Church-going	Affiliation
	(1)	(2)	(3)	(4)
Reform	-0.090	-0.083	Not covered	-0.071
	(0.002)	*(0.023)*	in NEPS	*(0.161)*
	[0.012]	*[0.034]*		*[0.321]*
State fixed effects	Yes	Yes		Yes
Birth-year fixed effects	Yes	Yes		Yes
Controls	Yes	Yes		Yes
Observations	9,232	7,963		9,237

Notes: Dependent variables indicated in column headers. All dependent variables are standardized (see Table A2 for details). Controls: gender, migration status, mother's education, father's education, 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 source: National Education Panel Study (NEPS) Cohort 6.

Table A27: Effects on religious outcomes: Only ALLBUS data

	Religiosity	Prayer	Church-going	Affiliation
	(1)	(2)	(3)	(4)
Reform	-0.044	0.018	-0.062	-0.111
	(0.326)	*(0.635)*	*(0.077)*	*(0.001)*
	[0.438]	*[0.677]*	*[0.175]*	*[0.026]*
State fixed effects	Yes	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	6,456	5,313	15,714	15,860

Notes: Dependent variables indicated in column headers. All dependent variables are standardized (see Table A2 for details). Controls: gender, migration status, mother's education, father's education, 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 source: German General Social Survey (ALLBUS) Cumulation 1980-2016.

Table A28: Effects on religious outcomes: Only SOEP data

	Religiosity	Prayer	Church-going	Affiliation
	(1)	(2)	(3)	(4)
Reform	Not covered in SOEP	Not covered in SOEP	-0.055	-0.066
			(0.065)	*(0.058)*
			[0.042]	*[0.139]*
State fixed effects			Yes	Yes
Birth-year fixed effects			Yes	Yes
Controls			Yes	Yes
Observations			27,062	20,828

Notes: Dependent variables indicated in column headers. All dependent variables are standardized (see Table A2 for details). Controls: gender, migration status, mother's education, father's education, 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 source: German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34).