

C A G E

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

CAGE working paper no. 732

November 2024

Benjamin W. Arold,
Ludger Woessmann,
Larissa Zierow

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

Benjamin W. Arold, Ludger Woessmann, and Larissa Zierow[†]

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 higher labor-market participation and earnings. The reform did not affect ethical and political values or non-religious school outcomes.

Keywords: religious education, religiosity, school reforms

JEL classification: Z12, I28, H75

September 11, 2024

* For helpful comments and discussion, we would like to thank the editor Aline Bütikofer, four anonymous referees, Robert Barro, Samuel Bazzi, Jeanet Bentzen, Christina Gathmann, Larry Iannaccone, Rachel McCleary, Jared Rubin, Mikko Silliman, and seminar participants at Harvard, Chapman, Bonn, the SIEPR Workshop in the Economics of Religion at Stanford, the CEPR Workshop on the Economics of Religion in Venice, the American Economic Association in San Diego, the European Association of Labour Economists in Uppsala, the European Society for Population Economics in Bath, the German Economic Association in Leipzig, the Spring Meeting of Young Economists in Brussels, the CESifo Area Conference in the Economics of Education in Munich, the Workshop on Economics of Education in Lund, the International Workshop on Applied Economics of Education in Catanzaro, the EffEE conference in Munich, the Section on Economics and Empirical Social Sciences of the German Academy of Sciences at Max Planck in Munich, the Evidence-Based Economics Summer Meeting in Herrsching, the EDGE Jamboree in Munich, and the Economics of Education Committee of the German Economic Association. Financial support by the Leibniz Competition (SAW 2019) is gratefully acknowledged. The contribution by Woessmann is part of the German Science Foundation project CRC TRR 190. Among others, this paper uses data from the National Educational Panel Study (NEPS), Starting Cohort Adults, doi:10.5157/NEPS:SC6:8.0.0, collected with funding from the German Federal Ministry of Education and Research until 2013 and carried out by the Leibniz Institute for Educational Trajectories (LIfBi) in cooperation with a nationwide network since 2014.

[†] Arold: University of Cambridge; ba475@cam.ac.uk. Woessmann: University of Munich, ifo Institute; Hoover Institution, Stanford University; CESifo, IZA; woessmann@ifo.de. Zierow: Reutlingen University, CESifo; larissa.zierow@reutlingen-university.de.

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); McCleary and Barro (2019); Becker, Rubin, and Woessmann (2024)). Rigorous research on the emergence and determinants of religious attitudes, though, faces a challenging task as they are often deeply rooted in humans' personality and socialization. But can religious attitudes be taught in school? As public school curricula intervene in individuals' life course, this question addresses a core aspect of the interplay of churches and the state. In this paper, we study whether being exposed to compulsory religious education in school affects religiosity in adulthood. As churches tend to convey specific worldly norms, we also study effects beyond the religious sphere on labor-market outcomes.

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

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

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

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

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

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

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

Beyond the religious sphere, the reforms also affected labor-market outcomes. They led to increases in labor-market participation, employment, working hours, and earnings. The reforms also led to a reduction in the number of children. By contrast, there is no evidence of effects on ethical-value outcomes such as reciprocity, trust, volunteering, and life satisfaction, nor on

political-value outcomes such as political interest and leaning, voting, and satisfaction with democracy. Consistent with the counterfactual of alternative value-oriented instruction, the reform-induced decline in religiosity thus did not go hand in hand with a reduction in the measured ethical and political values and behaviors.

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

Our study contributes to four strands of literature. First, studies in the economics of religion have shown the importance of religion and religiosity for economic development and personal outcomes (see Barro and McCleary (2003) and McCleary and Barro (2006, 2019) for a cross-country setting, Becker, Rubin, and Woessmann (2021, 2024) for historical and growth contexts, and Becker and Woessmann (2009, 2018) for the German context). Recent analyses of the determinants of religiosity and the demand for religious services investigate, among others, effects of secular competition (Gruber and Hungerman (2008)), economic deprivation (Becker

and Woessmann (2013)), printing technology (Rubin (2014)), the performance of pastors (Engelberg et al. (2016)), coping with natural disasters (Bentzen (2019)), and an adult religious-value intervention (Bryan, Choi, and Karlan (2021)). Several papers study the interrelationship between education systems and religion in different contexts (Brown and Taylor (2007); Glaeser and Sacerdote (2008); Chaudhary and Rubin (2011); Hungerman (2014); Franck and Iannaccone (2014); Meyersson (2014); Becker, Nagler, and Woessmann (2017)). To the extent that they analyze effects of education on religion, these papers focus on effects of the level of education in general. Here, we focus on a different aspect – the effect of religious education in the school curriculum – as a more direct means by which schools may affect religiosity.

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

Third, a broad literature in the economics of education studies the impact of different school reforms (e.g., Hanushek (1986); Woessmann (2016)). While this literature has traditionally looked at students' academic achievement and later labor-market success, more recent

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

contributions also focus on non-academic outcomes such as personality traits (e.g., Almlund et al. (2011)) and soft skills (e.g., Koch, Nafziger, and Nielsen (2015)).

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

In the following, section 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 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.

While this setting is specific, it allows us to derive more general lessons about the role of schools in shaping students' values and beliefs.

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

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

debate about whether religious education should be offered in schools at all, but in the end the supporters of religious education prevailed.

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

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

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

students from first to final grade. Parents (and adolescents aged at least 14)⁴ could formally request non-participation if the child was not baptized, but this was a rare exception (Havers (1972)).

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

The reforms. From the 1970s onwards, eight of the eleven West German states terminated the compulsory nature of religious education (Helbig and Nikolai (2015)). Parents could now choose between religious education and a newly introduced subject, usually called “ethics”,⁶ which provides an alternative form of value-oriented instruction that was non-denominational. Importantly, the reform was applicable to all schools, both public and private. As indicated in

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

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

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

Table 1, Bavaria was the first state to enact the reform in 1972 and Hamburg and North Rhine-Westphalia were the last in 2004 (see also map in Appendix Figure A1). Relying on state-wide variation in Germany, our empirical analysis thus draws on variation across a limited number of eleven states, eight of which changed treatment status. We account for the small number of states when computing standard errors.

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

Interestingly, the rollout of the reform across states was orthogonal to the political leaning of and changes in the state government. As is obvious from column 4 of Table 1, four reforms were implemented by a right-of-center Christian Democrat (CDU/CSU) government and four by a left-of-center Social Democrat (SPD) government. The time pattern is literally alternating

⁷ To ensure that results are not driven by reactive reforms to early opt-out during the student movement, in robustness tests we show that results are robust to leaving out early reforming states (see section 7) and to restricting the sample to rural areas (see section 5).

between the two camps. Furthermore, for each single reform, the party that was in power in the legislative period of the reform had already been in power in the prior legislative period, implying that no reform was implemented after a change in government (column 5). Similarly, the reform rollout was not driven by the size of a state, as the two largest states (Bavaria and North Rhine-Westphalia) were the first and last to implement the reform, respectively. These patterns make it unlikely that the reforms were due to political trends or shocks.⁸

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

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

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

⁸ The result that we do not find reform effects on political outcomes (section 5) also speaks against the existence of political shocks coinciding with the timing of the reforms.

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

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

Second, the subject ethics acted as a newly emerged competitor to religious education, putting religious education curricula under modernizing pressure. Studying curricula before and after the reform, we find that religious education curricula tended to change after the reform. As one example, Appendix Table A1 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 about other religions. The pre-reform curriculum puts more focus on guiding students

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

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

towards Christianity, whereas the post-reform curriculum emphasizes guiding students towards responsible and informed behavior defined by Christian values.¹² As an example of a late reformer, the 1999 pre-reform syllabus in North Rhine-Westphalia focuses on religious values to guide students, whereas the 2014 post-reform syllabus emphasizes helping students develop their own values based on religion and faith. Overall, the comparison of curricula points to a decrease in the practice of prayers and literal interpretation of the bible after the compulsory nature of religious education was abolished.

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

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

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

reform does not only affect students who chose to attend ethics classes, but also students who continued attending religious education classes. In addition, the description makes clear that several elements of the enactment of the reform were gradual rather than abrupt, leading to an expectation that reform effects may phase in rather than happen discontinuously.

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:

$$R_{i,s,t} = 1(t_{i,s} \geq t_s^*)\beta_{Reform} + \mathbf{X}_i\beta_{Controls} + \mu_s + \lambda_t + \varepsilon_{i,s,t} \quad (1)$$

The adulthood outcome (e.g., religiosity) $R_{i,s,t}$ of individual i who started primary school in state s and year t is a function of an indicator term $1(t_{i,s} \geq t_s^*)$ that equals one if the primary school entry year $t_{i,s}$ of individual i in state s is larger than or equal to the year of reform t_s^* in state s .¹³

¹³ Coding individuals as treated only if the reform had been implemented at their primary school entry is our preferred categorization because it starts with the first cohort that could have avoided religious education completely by choosing the non-denominational alternative from the first grade onwards. The fact that students who were already beyond primary school entry in the year of reform introduction are categorized as exposed to compulsory religious education even if they received some exposure to the reformed curriculum might mean that our baseline

Apart from state and cohort fixed effects (μ_s and λ_t , respectively), a vector of individual-level controls \mathbf{X}_i and an error term $\varepsilon_{i,s,t}$ complete the model. Throughout the paper, standard errors are clustered at the state level. We report p -values for two clustering methods. The first one is the standard clustering approach which accounts for potential correlation of error terms across years within states and provides conservative inference if reform timing is random (Athey and Imbens (2022); Abadie et al. (2023)). The second one is the wild cluster bootstrap approach suggested by Roodman et al. (2019) which provides asymptotic refinement by accounting for the limited number of clusters given by the West German states.¹⁴

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

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. For example, the

specification underestimates the true effect. In robustness analyses, we confirm results in a dosage specification where treatment is defined as the share of compulsory school years that an individual spent in the reformed system, as well as in a specification that defines treatment by entry into secondary school (see section 7).

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

reform rollout did not indicate any political trend, with implementations alternating between right-wing and left-wing governments and no reform enacted in the first legislative period after a change in government (see Table 1).

One way in which the identifying assumption could be violated is the existence of other school reforms that happened simultaneously. However, the timing of the religious-education reform is very peculiar, and we are not aware of other reforms with even vaguely similar patterns of timing across states. In fact, results are robust in specifications that control for a range of other education reforms (see section 7). An additional way to test this concern is to estimate reform effects on non-religious school outcomes such as degree completion or years of schooling. The religious-education reform did not affect any other subjects and did not substitute religious education by classes prone to enhance achievement in other curricular subjects. As we thus do not expect any first-order effects of the religious-education curriculum on other school outcomes, such analysis can be interpreted as a placebo test that, if it failed, would indicate the possibility of simultaneous school reforms.

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

In addition, it is an attractive feature of the event-study approach that including a trend variable relative to the reform ($t_{i,s} - t_s^*$) constitutes a falsification test of the identifying

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).

assumption of randomness in reform timing (keeping the assumption of time-invariant treatment effects for now):

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

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 ($t_{i,s} - t_s^*$):

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

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:

$$R_{i,s,t} = \sum_{k=-19}^{20} 1(t_{i,s} = t_s^* + k)\beta_k + \mathbf{X}_i\beta_{Controls} + \mu_s + \lambda_t + \varepsilon_{i,s,t} \quad (4)$$

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

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

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

¹⁶ Furthermore, excluding covariates does not change our qualitative results, indicating that cohorts with different covariates are unlikely to react differently to the reform (see section 7).

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

All three datasets allow us to observe individuals' state and year of primary school entry, which is the basic data requirement of our evaluation approach. That is, each dataset allows us to link the religiosity of individuals in adulthood to their state of schooling in childhood, even if they migrated to other states in-between.¹⁸ Our sample consists of all individuals who entered primary school in West Germany between 1950 and 2004. We exclude individuals who entered primary school before 1950 because they did not have their entire schooling career in the Federal

¹⁷ Given the sample sizes of NEPS (12,281 individuals), ALLBUS (15,924 individuals), and SOEP (30,498 individuals) and the German population of the primary-school entry cohorts between 1950 and 2004 (approximately 60 million, measured in 2010), the sum of the probabilities that an individual appears in two or three surveys is approximately 0.0000293 percent: $P(\text{two or three surveys}) = P(\text{NEPS}) \times P(\text{ALLBUS}) + P(\text{NEPS}) \times P(\text{SOEP}) + P(\text{ALLBUS}) \times P(\text{SOEP}) + P(\text{NEPS}) \times P(\text{ALLBUS}) \times P(\text{SOEP}) = 2.93 \times 10^{-7}$.

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

Republic of Germany (founded in 1949). Primary school entry by 2004 ensures that individuals have turned at least 18 years old by 2016/17.

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

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

The three datasets also provide batteries of measures of labor-market outcomes, as well as of ethical-value, political-value, and educational outcomes. Control variables include gender, migration status, and mothers' and fathers' education. Table 2 provides descriptive statistics for

¹⁹ To document that results are not driven by the standardized merging, robustness checks also show results for each of the three datasets separately (see section 7).

the merged dataset. Roughly one third of observations are treated by the reform, i.e., they entered primary school after compulsory religious education had been abolished. Column (7) provides information on respondents' age for each variable. The specific age range varies slightly depending on the outcome variable due to the combination of different surveys. For religiosity, our main outcome variable, respondents' ages range from 18 to 70 years.

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

Finally, Appendix Table A3 shows descriptive statistics of background characteristics by treatment status. By construction, the individuals exposed to the reform are from later birth cohorts, implying that they are also more likely to display characteristics that have become more common over time in the German society, such as a higher probability of migration background and more educated parents. In the empirical analyses, we account for trends in the German society by including year fixed effects and conditioning on these characteristics. As year fixed effects are not accounted for in the raw comparisons, individuals exposed to the reform are more likely to have parents with migration background and higher education. Other, less time-variant variables such as the gender ratio hardly differ between groups.

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 results of specification and robustness tests.

Our results show that the abolishment of compulsory religious education decreased the religiosity of affected students in adulthood. The event-study graph of Figure 1 indicates that individuals who entered school after the reform report significantly lower levels of religiosity.²⁰ Visual inspection suggests that reform effects increase for subsequent cohorts, consistent with a phase-in of effects due to gradual reform implementation. An omnibus hypothesis test that the post-event effects are jointly zero is rejected at the 1 percent level. By contrast, the test does not reject that the pre-event effects are jointly zero, indicating that reforming states had not been on different trends from non-reforming states prior to the reform.²¹

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

An alternative way to illustrate the magnitude of the reform effect are persuasion rates (DellaVigna and Gentzkow (2010)), i.e., the share of religious people who lose their religiosity due to the reform. We follow Cantoni et al. (2017) in calculating conditional persuasion rates by predicting the fraction of individuals who would be religious in the absence of the reform from

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

²¹ In the graph, the apparent small insignificant downward trend to the left of the event may reflect that these cohorts were partly exposed to the reform in later grades. Consistently, a dosage specification (see section 7) that takes this into account yields slightly larger estimates than the baseline specification.

our model. The resulting persuasion rates amount to 6 percent for (at least rather) religious people and 20 percent for very religious people, which is in the range of estimates found on various attitudinal outcomes for a Chinese curricular reform in Cantoni et al. (2017).

The reform also led to significant reductions in the three measures of specific religious actions (columns 2-4 of Table 3). The standardized effects are of a similar magnitude to overall religiosity. The reform reduces the personal act of prayer by 5 percent of a standard deviation (marginally significant), the public act of going to church by 7 percent, and the formal act of church membership by 8 percent. The respective event-study graphs are shown in Panel A of Figure 2.

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

The even columns of Table 4 report results of the rather demanding specification with time-varying treatment effects that allows for both a shift term of the reform, a relative trend, and an interaction between the two. Confirming the graphical depiction, results indicate that the reform effect on religiosity phases in gradually over time: religiosity decreases by 0.013 standard deviations on average per year in reforming states after the reform, relative to the average change in the same state prior to the reform. A similar gradual treatment effect emerges for personal prayer. By contrast, the effect on affiliation with a religious community is mostly captured by a one-time shift. This may be related to the fact that church membership in Germany implies the requirement to pay church taxes: Individuals who were exposed to the reform even in the early

years after a state's implementation do react by leaving their church as adults to avoid paying church taxes, whereas their subjective religiosity and prayer are not yet as strongly impacted. For church-going, the separate estimates in this specification are too imprecise to distinguish between a one-time shift and gradual phasing-in.

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

In contrast to the effects on religiosity and religious actions, we do not find evidence that the reform affected various value outcomes. In particular, there are no significant treatment effects on a series of measures of ethical-value outcomes including reciprocity, trust, risk preference,

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

volunteering, and life satisfaction (Panel A of Table 6). The absence of treatment effects on these ethical outcomes is consistent with the fact that the post-reform counterfactual to compulsory religious education in our setting is not the option to opt out of value-oriented classes, but rather a choice between two types of value-oriented classes that are either denominational or not. Apparently, attending the non-denominational subject ethics does not lead to lower levels of the different ethical-value outcomes compared to the subject religious education. Similarly, there is 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 Labor-Market Outcomes

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

²³ For instance, exemplary calculations indicate that for an average gross income of €37,000, the saved tax would amount to €49 per year. See, Handelsblatt, ‘Church tax: what are the pros and cons of leaving the church?’,

Results show that the reform indeed had positive effects on labor-market outcomes. The probability to participate in the labor market increases by 1.5 percentage points (column 1 of Table 7), compared to a mean of 82 percent, and the probability to be employed by 2.3 percentage points (column 2; mean 78 percent). Among those employed, earnings increase by 5.3 percent (column 3). While this is a relatively large effect, it is relatively imprecisely estimated, suggesting that the magnitude should be interpreted with caution. Working hours rise by 0.6 hours per week (column 4), compared to a mean of 35.6 hours. Conversely, the probability of engaging in part time work falls by 2.0 percentage points (column 5, mean 20 percent). Possibly as a consequence of increased work time and reduced part-time work and thus reduced time to take care of children, the reform also decreased the number of children by 0.09 children per respondent (column 6, mean 1.4 children).

Panel B of Figure 2 shows that the results on labor-market outcomes also hold in event study specifications. Importantly, the different labor-market effects do not differ by gender (see Figures A3 and A4 in the Appendix).²⁴

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

https://www.handelsblatt.com/arts_und_style/kirchensteuer-was-sind-die-vor-und-nachteile-eines-kirchenaustritts-2024/25300246.html (accessed July 21, 2024).

²⁴ In a previous version, we also reported results on several attitudes towards gender and family roles, as well as marriage, but results are too imprecise to warrant definitive conclusions (see Panel A of Table 7 in Arold, Woessmann, and Zierow (2022)).

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

7. Specification and Robustness Tests

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

Effects on non-religious school outcomes. For our identification strategy to hold, the abolishment of religious education should not be accompanied by other educational reforms or other state-cohort-specific events with the same timing structure. As meaningful other school reforms should leave traces in general educational outcomes, one way to test this is to estimate treatment effects on non-religious educational outcomes. Results show that the reform is not significantly related to the non-religious educational outcomes in our datasets, namely years of schooling, the type of school degree, or the age of first employment (Table 8). As the studied reform did not lead to a change in schooling hours and or in the structure or content of the non-religious subjects, we interpret this as a placebo test that is in line with our identifying assumption. This interpretation is also consistent with the non-existence of effects on ethical-value and political-value outcomes (see section 5 above).

Border specification with county-pair fixed effects. To reduce the possible incidence of unobserved differences, we can restrict the analysis to individuals from geographically close and thus arguably highly similar counties. For a subset of individuals in the NEPS data, we observe individuals' county of schooling. This allows us to restrict the sample to pairs of counties separated by a state border (see Appendix Figure A5). Additionally, in this specification we can include county-pair fixed effects for each pair of neighboring counties that is divided by a state border (Dube, Lester, and Reich (2010); Bentzen 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 (Table 9). The same is true for prayer, whereas the effect on affiliation does not hold in this specification.

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

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

To ensure that the estimated reform effects do not pick up effects of other education reforms, we include controls for a range of other reforms. These include reforms of the length of compulsory schooling (e.g., Pischke and von Wachter (2008); Cygan-Rehm (2022)), of the duration of the highest-track school (“G8/G9 reform”, e.g., Andrietti and Su (2019); Marcus and Zambre (2019)), and of whether philosophy, sexual education, and political education,

respectively, are taught in school (see Helbig and Nikolai (2015)). Results are robust to controlling for these other education reforms (Appendix Table A8).

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

A couple of robustness checks relate to the coding of treatment. First, we replace the dummy variable indicating reform exposure by a dosage variable measuring the share of school years out of the total compulsory school years in which individuals were exposed to the reform. Results are robust and point estimates become larger for each of the religious outcomes (Appendix Table A10), as expected if the conservative baseline indicator coding produces an underestimate of the true effect.

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

To ensure that the results for our separate outcome measures are not spurious, we create indices of outcome groups (Anderson (2008)). We combine the measures in each of our five

groups of outcomes as presented in Tables 3 and 6-8 – religious outcomes, labor-market 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 and labor-market outcomes, but not ethical-value, political-value, and educational outcomes (Appendix Table A12). The effects (in absolute terms) are 8.7 percent of a standard deviation on the index of religious outcomes and 5.8 percent on labor-market outcomes. Consistent with measurement error in the separate measures, inference gets considerably more precise with the indices; estimated effects on religious and labor-market outcomes are each significant at the 1 percent level.

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

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

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

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

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

We also test the robustness of our event study graphs in several alternative specifications. We reduce the number of years that enter one bin (Appendix Figure A7), keep only states that reform between 1972 and 1983 and non-reforming states in the sample (Appendix Figure A8), and group outcomes into indices (Appendix Figure A9). While in general our results are robust to these variations, results become less clean when pushing on the limited sample size. Specifically, the coefficients are less stable when using two-year instead of five-year bins (reducing the sample size per bin) and when using the smaller sample of reforming states (reducing the overall sample size). In all cases, effects point in the same direction as the main event study figures for all ten outcomes. In six of the ten analyses in Appendix Figure A8, there is at least one significant post-reform coefficient. In five of them, the omnibus hypothesis test of zero post-event effects can be rejected. Reassuringly, pre-reform coefficients are insignificant, and we can never reject the omnibus hypothesis test of zero pre-event effects. Disregarding religious affiliation (the only one of the ten outcomes in Figure 2 for which the omnibus hypothesis test of zero pre-event effects is significant at the ten percent level), the outcome indices in Appendix Figure A9 indicate no significant overall pre-trends for religious and labor-market outcomes, and post-event effects are significant at the 1 percent level.

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

Tests of the two-way fixed effects estimator. Our setting generalizes the classic two-group/two-period difference-in-differences setting in that there are eleven states among whom

eight change their treatment status in different years over an extended time horizon. To ensure that our estimates are not driven by two-by-two reform estimates with negative weights, we implement the estimator suggested by Callaway and Sant'Anna (2021) that is immune to bias from negative weighting. The procedure uses only not-yet treated units and never-treated units as controls. Already-treated units, which could potentially cause negative weighting, are omitted from the analysis. Reassuringly, the aggregated estimates of the average treatment effect on the treated (ATT) for the four religious outcomes are very similar to our baseline two-way fixed effects estimates (see Appendix Table A18). In fact, the ATT estimates are larger (in absolute terms) than the corresponding baseline estimates, although sometimes at lower levels of statistical significance. We also use other event study estimators that are robust to time-varying treatment effects, namely Sun and Abraham (2021), Callaway-Sant'Anna (2021), Gardner (2021), and Cengiz et al. (2019) (see Appendix Figure A11). Overall, our results are robust to these variations.

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

8. Conclusions

Our study investigates whether compulsory religious education affects people's religiosity in the long run. We argue that the different timing of reforms that abandoned compulsory religious education across German states provides plausibly exogenous variation in individuals' exposure to compulsory religious education. Students could now choose to attend non-denominational ethics classes rather than religious education, which likely also changed overall social norms

towards religion and, by competitive pressures, the content of religious classes. We find that, conditional on state and birth-year fixed effects, the 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 about 3 percentage points (compared to an average incidence of 52 percent) and of those reporting to be very religious by 2 percentage points (average 11 percent), which corresponds to estimated persuasion rates – religious people who lose their religiosity due to the reform – of 6 and 20 percent, respectively. We also find reductions in three measures of religious actions – prayer, church-going, and religious affiliation.

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

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

References

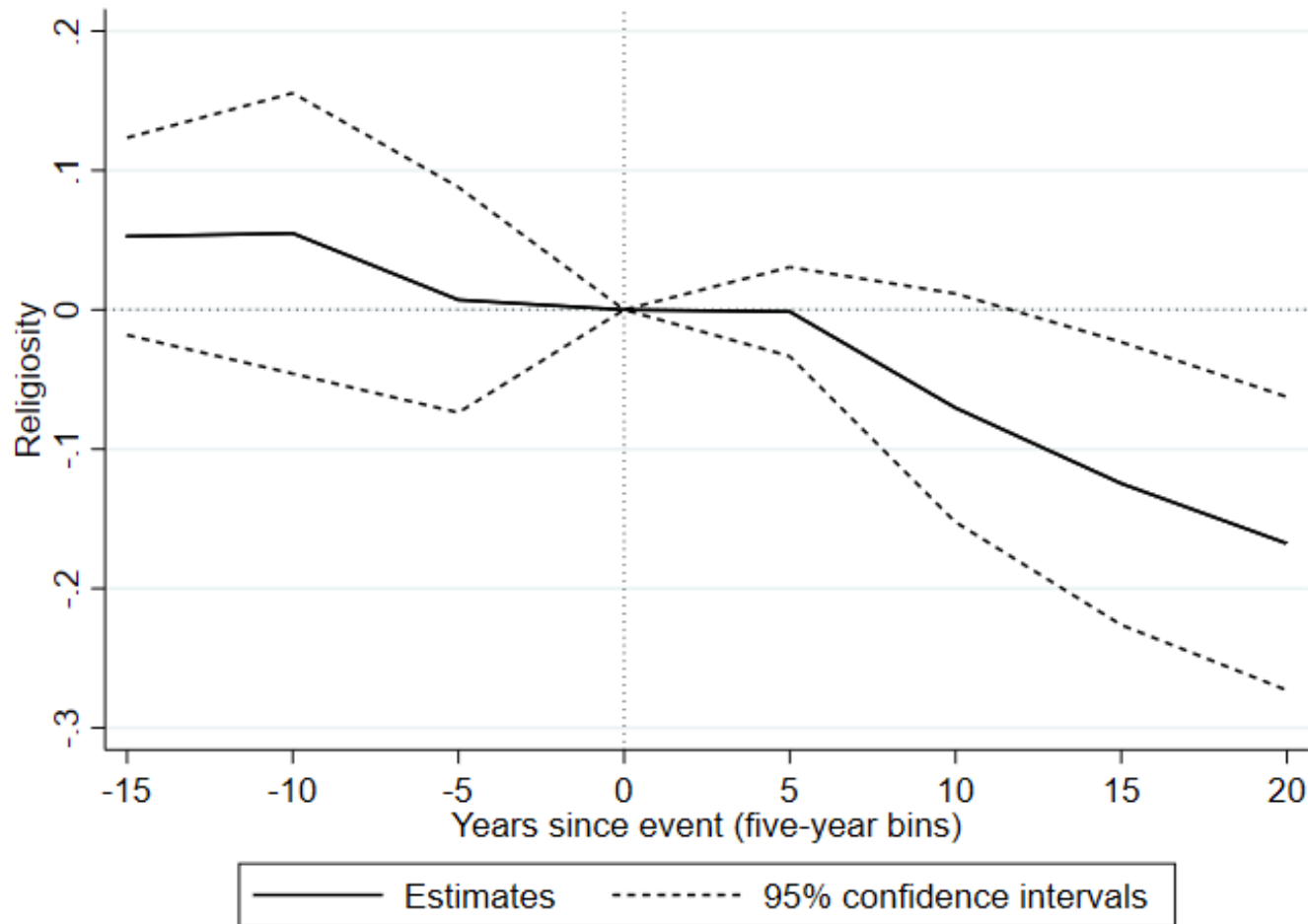
- Abadie, Alberto, Susan Athey, Guido W. Imbens, and Jeffrey M. Wooldridge. 2023. "When should you adjust standard errors for clustering?" *Quarterly Journal of Economics* 138 (1): 1-35.
- Almlund, Mathilde, Angela L. Duckworth, James Heckman, and Tim Kautz. 2011. "Personality psychology and economics." In *Handbook of the Economics of Education, Vol. 4*, edited by Eric A. Hanushek, Stephen Machin, and Ludger Woessmann, 1-181. Amsterdam: North Holland.
- Anderson, Michael L. 2008. "Multiple inference and gender differences in the effects of early intervention: A reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects." *Journal of the American Statistical Association* 103 (484): 1481-1495.
- Andrietti, Vincenzo, and Xuejuan Su. 2019. "The impact of schooling intensity on student learning: Evidence from a quasi-experiment." *Education Finance and Policy* 14 (4): 679-701.
- Arold, Benjamin W. 2024. "Evolution vs. Creationism in the Classroom: The Lasting Effects of Science Education." *Quarterly Journal of Economics*, forthcoming.
- Arold, Benjamin W., Ludger Woessmann, and Larissa Zierow. 2022. "Can Schools Change Religious Attitudes? Evidence from German State Reforms of Compulsory Religious Education." IZA Discussion Paper 14989. Bonn: IZA Institute of Labor Economics.
- Athey, Susan, and Guido W. Imbens. 2022. "Design-based analysis in difference-in-differences settings with staggered adoption." *Journal of Econometrics* 226 (1): 62-79.
- Barro, Robert J., and Rachel M. McCleary. 2003. "Religion and economic growth across countries." *American Sociological Review* 68 (5): 760-781.
- Bazzi, Samuel, Masyhur Hilmy, and Benjamin Marx. 2020. "Islam and the state: Religious education in the age of mass schooling." NBER Working Paper 27073. Cambridge, MA: National Bureau of Economic Research.
- Becker, Sascha O., Markus Nagler, and Ludger Woessmann. 2017. "Education and religious participation: City-level evidence from Germany's secularization period 1890-1930." *Journal of Economic Growth* 22 (3): 273-311.
- Becker, Sascha O., Jared Rubin, and Ludger Woessmann. 2021. "Religion in economic history: A survey." In *The Handbook of Historical Economics*, edited by Alberto Bisin and Giovanni Federico, 585-639. London: Academic Press.
- Becker, Sascha O., Jared Rubin, and Ludger Woessmann. 2024. "Religion and growth." *Journal of Economic Literature* 62 (3).
- Becker, Sascha O., and Ludger Woessmann. 2009. "Was Weber wrong? A human capital theory of Protestant economic history." *Quarterly Journal of Economics* 124 (2): 531-596.
- Becker, Sascha O., and Ludger Woessmann. 2013. "Not the opium of the people: Income and secularization in a panel of Prussian counties." *American Economic Review, Papers and Proceedings* 103 (3): 539-544.
- Becker, Sascha O., and Ludger Woessmann. 2018. "Social cohesion, religious beliefs, and the effect of Protestantism on suicide." *Review of Economics and Statistics* 100 (3): 377-391.

- Bentzen, Jeanet Sinding. 2019. "Acts of God? Religiosity and natural disasters across subnational world districts." *Economic Journal* 129 (622): 2295-2321.
- Bentzen, Jeanet Sinding, and Lena Lindbjerg Sperling. 2020. "God politics." CEPR Discussion Paper 14380. London: Centre for Economic Policy Research.
- Blossfeld, Hans-Peter, Hans-Günther Roßbach, and Jutta von Maurice. 2011. "Education as a lifelong process: The German National Educational Panel Study (NEPS)." *Zeitschrift für Erziehungswissenschaft* 14 [Special Issue].
- Borusyak, Kirill, Xavier Jaravel, and Jann Spiess. 2021. "Revisiting event study designs: Robust and efficient estimation." Mimeo. London: University College London.
- Brown, Sarah, and Karl Taylor. 2007. "Religion and education: Evidence from the National Child Development Study." *Journal of Economic Behavior and Organization* 63 (3): 439-460.
- Bryan, Gharad, James J. Choi, and Dean Karlan. 2021. "Randomizing religion: The impact of Protestant evangelism on economic outcomes." *Quarterly Journal of Economics* 136 (1): 293-380.
- Callaway, Brantly, and Pedro H.C. Sant'Anna. 2021. "Difference-in-differences with multiple time periods." *Journal of Econometrics* 225 (2): 200-230.
- Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller. 2008. "Bootstrap-based improvements for inference with clustered errors." *Review of Economics and Statistics* 90 (3): 414-427.
- Cantoni, Davide, Yuyu Chen, David Y. Yang, Noam Yuchtman, and Y. Jane Zhang. 2017. "Curriculum and ideology." *Journal of Political Economy* 125 (2): 338-392.
- Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer. 2019. "The Effect of Minimum Wages on Low-Wage Jobs." *Quarterly Journal of Economics* 134 (3): 1405-1454.
- Chaudhary, Latika, and Jared Rubin. 2011. "Reading, writing, and religion: Institutions and human capital formation." *Journal of Comparative Economics* 39 (1): 17-33.
- Cygan-Rehm, Kamila. 2021. "Are there no wage returns to compulsory schooling in Germany? A reassessment." *Journal of Applied Econometrics* 37 (1): 218-223.
- de Chaisemartin, Clément, and Xavier D'Haultfœuille. 2020. "Two-way fixed effects estimators with heterogeneous treatment effects." *American Economic Review* 110 (9): 2964-2996.
- DellaVigna, Stefano, and Matthew Gentzkow. 2010. "Persuasion: Empirical evidence." *Annual Review of Economics* 2 (1): 643-669.
- Dube, Arindrajit, T. William Lester, and Michael Reich. 2010. "Minimum wage effects across state borders: Estimates using contiguous counties." *Review of Economics and Statistics* 92 (4): 945-964.
- Engelberg, Joseph, Raymond Fisman, Jay C. Hartzell, and Christopher A. Parsons. 2016. "Human capital and the supply of religion." *Review of Economics and Statistics* 98 (3): 415-427.
- Franck, Raphaël, and Laurence R. Iannaccone. 2014. "Religious decline in the 20th century West: Testing alternative explanations." *Public Choice* 159 (3-4): 385-414.
- Gardner, John. 2021. "Two-stage differences in differences." Mimeo.

- GESIS. 2019. *German General Social Survey (ALLBUS) – Cumulation 1980-2016. GESIS Data Archive ZA4588 Data file version 1.0.0*. Cologne: GESIS - Leibniz Institute for the Social Sciences.
- Glaeser, Edward L., and Bruce I. Sacerdote. 2008. "Education and religion." *Journal of Human Capital* 2 (2): 188-215.
- Goebel, Jan, Markus M. Grabka, Stefan Liebig, Martin Kroh, David Richter, Carsten Schröder, and Jürgen Schupp. 2019. "The German Socio-Economic Panel (SOEP)." *Journal of Economics and Statistics* 239 (2): 345-360.
- Goodman-Bacon, Andrew. 2021. "Difference-in-differences with variation in treatment timing." *Journal of Econometrics* 225 (2): 254-277.
- Gradstein, Mark, and Moshe Justman. 2002. "Education, social cohesion, and economic growth." *American Economic Review* 92 (4): 1192-1204.
- Gruber, Jonathan, and Daniel M. Hungerman. 2008. "The Church vs. the mall: What happens when religion faces increased secular competition." *Quarterly Journal of Economics* 123 (2): 831-862.
- Hanushek, Eric A. 1986. "The economics of schooling: Production and efficiency in public schools." *Journal of Economic Literature* 24 (3): 1141-1177.
- Hara, Hiromi, and Nuria Rodriguez-Planas. 2024. "Long-Term Consequences of Teaching Gender Roles: Evidence from Desegregating Industrial Arts and Home Economics in Japan." *Journal of Labor Economics*, forthcoming.
- Havers, Norbert. 1972. *Der Religionsunterricht – Analyse eines unbeliebten Fachs*. München: Kösel-Verlag.
- Helbig, Marcel, and Rita Nikolai. 2015. *Die Unvergleichbaren: Der Wandel der Schulsysteme in den deutschen Bundesländern seit 1949*. Bad Heilbrunn: Klinkhardt.
- Hungerman, Daniel M. 2014. "The effect of education on religion: Evidence from compulsory schooling laws." *Journal of Economic Behavior and Organization* 104: 52-63.
- Iannaccone, Laurence R. 1998. "Introduction to the economics of religion." *Journal of Economic Literature* 36 (3): 1465-1496.
- Inglehart, Ronald, Christian Haerpfer, Alejandro Moreno, Christian Welzel, Kseniya Kizilova, Jaime Diez-Medrano, Marta Lagos, Pippa Norris, Eduard Ponarin, Bi Puranen, et al. 2014. *World Values Survey: All rounds - Country-pooled datafile version*: <https://www.worldvaluessurvey.org/WVSDocumentationWVL.jsp>. Madrid: JD Systems Institute.
- Kling, Jeffrey R., Jeffrey B. Liebman, and Lawrence F. Katz. 2007. "Experimental analysis of neighborhood effects." *Econometrica* 75 (1): 83-119.
- Koch, Alexander, Julia Nafziger, and Helena Skyt Nielsen. 2015. "Behavioral economics of education." *Journal of Economic Behavior & Organization* 115: 3-17.
- Kultusministerkonferenz. 2021. *Auswertung Religionsunterricht Schuljahr 2019/20*. Berlin: Sekretariat der Ständigen Konferenz der Kultusminister der Länder in der Bundesrepublik Deutschland.

- Lafortune, Julien, Jesse Rothstein, and Diane Whitmore Schanzenbach. 2018. "School finance reform and the distribution of student achievement." *American Economic Journal: Applied Economics* 10 (2): 1-26.
- Lott, John R. Jr. 1999. "Public schooling, indoctrination, and totalitarianism." *Journal of Political Economy* 107 (S6): S127-S157.
- Lott, Jürgen. 2005. "Religionsunterricht in Deutschland." *Jahrbuch für Pädagogik* 2005: 143-162.
- Marcus, Jan, and Vaishali Zambre. 2019. "The effect of increasing education efficiency on university enrollment: Evidence from administrative data and an unusual schooling reform in Germany." *Journal of Human Resources* 54 (2): 468-502.
- McCleary, Rachel M., and Robert J. Barro. 2006. "Religion and economy." *Journal of Economic Perspectives* 20 (2): 49-72.
- McCleary, Rachel M., and Robert J. Barro. 2019. *The wealth of religions: The political economy of believing and belonging*. Princeton, NJ: Princeton University Press.
- Meyersson, Erik. 2014. "Islamic rule and the empowerment of the poor and pious." *Econometrica* 82 (1): 229-269.
- Pischke, Jörn-Steffen, and Till von Wachter. 2008. "Zero returns to compulsory schooling in Germany: Evidence and interpretation." *Review of Economics and Statistics* 90 (3): 592-598.
- Pritchett, Lant, and Martina Viarengo. 2015. "The state, socialisation, and private schooling: When will governments support alternative producers?" *Journal of Development Studies* 51 (7): 784-807.
- Ramirez, Francisco O., and John Boli. 1987. "The political construction of mass schooling: European origins and worldwide institutionalization." *Sociology of Education* 60 (1): 2-17.
- Rios-Avila, Fernando, Pedro H.C. Sant'Anna, Brantly Callaway, and Asjad Naqvi. 2021. "csdid and drdid: Doubly robust differences-in-differences with multiple time periods." Mimeo.
- Roodman, David, James G. MacKinnon, Morten Ø. Nielsen, and Matthew D. Webb. 2019. "Fast and wild: Bootstrap inference in Stata using boottest." *Stata Journal* 19 (1): 4-60.
- Rubin, Jared. 2014. "Printing and Protestants: An empirical test of the role of printing in the Reformation." *Review of Economics and Statistics* 96 (2): 270-286.
- Schwoerbel, Wolfgang. 1985. "Das neue Unterrichtsfach Ethik." *Lehren und Lernen* (2): 9-38.
- Sun, Liyang, and Sarah Abraham. 2021. "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects." *Journal of Econometrics* 225 (2): 175-199.
- West, Martin R., and Ludger Woessmann. 2010. "'Every Catholic child in a Catholic school': Historical resistance to state schooling, contemporary private competition and student achievement across countries." *Economic Journal* 120 (546): F229-F255.
- Woessmann, Ludger. 2016. "The importance of school systems: Evidence from international differences in student achievement." *Journal of Economic Perspectives* 30 (3): 3-32.

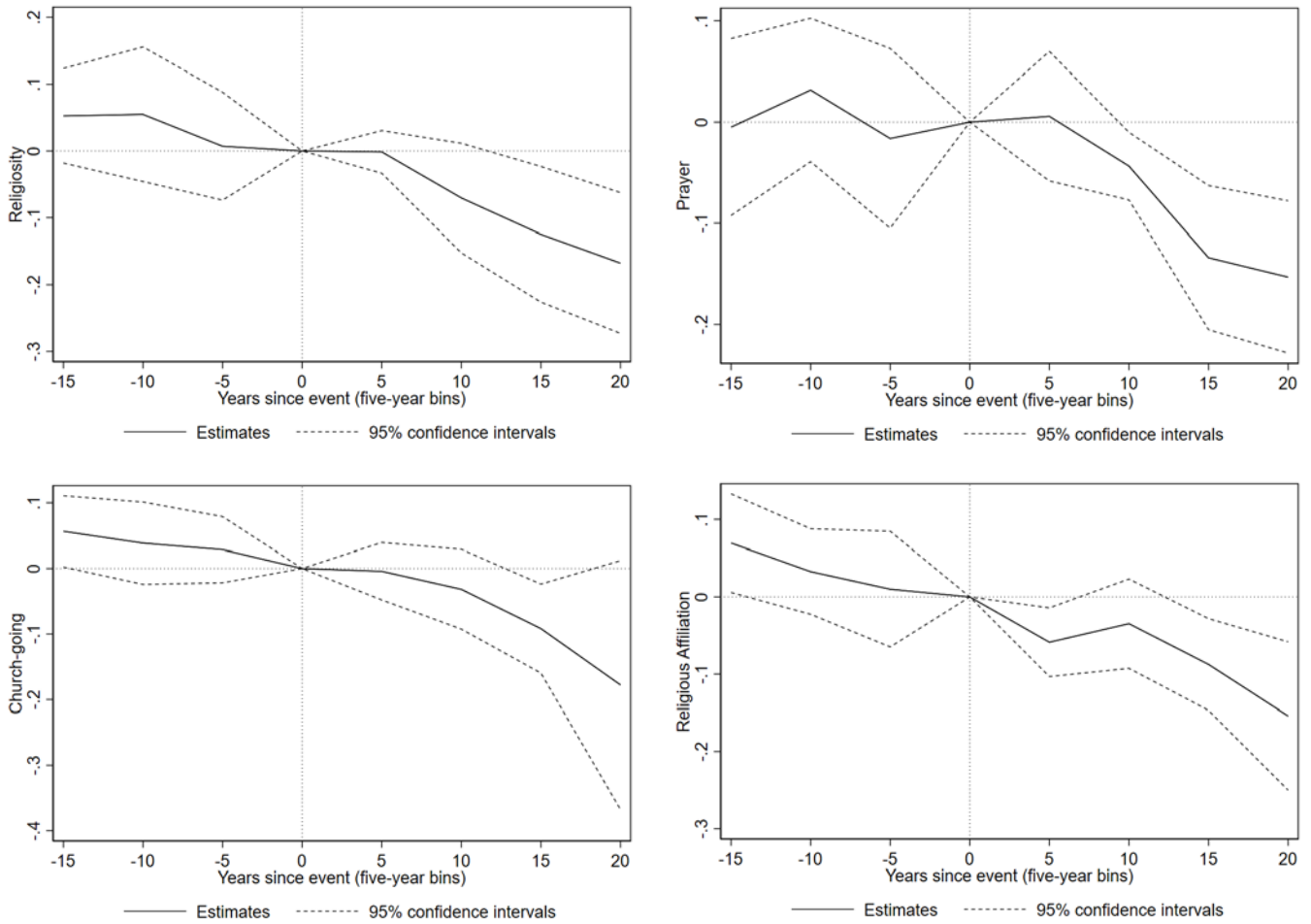
Figure 1: The effect of abolishing compulsory religious education on religiosity: Non-parametric event-study estimates



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

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

Panel A: Religious Outcomes



Panel B: Labor-Market Outcomes

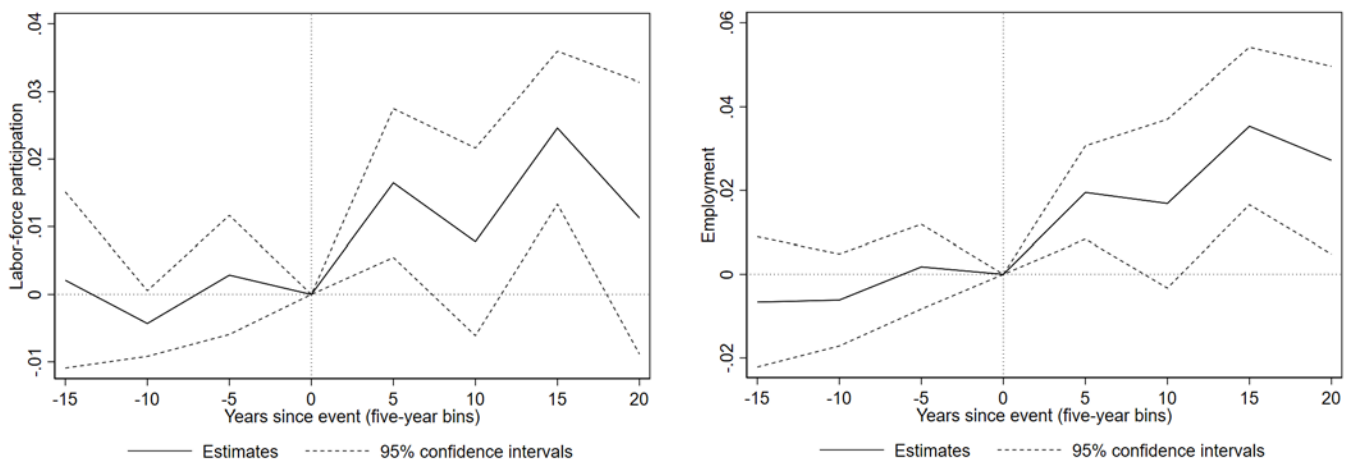
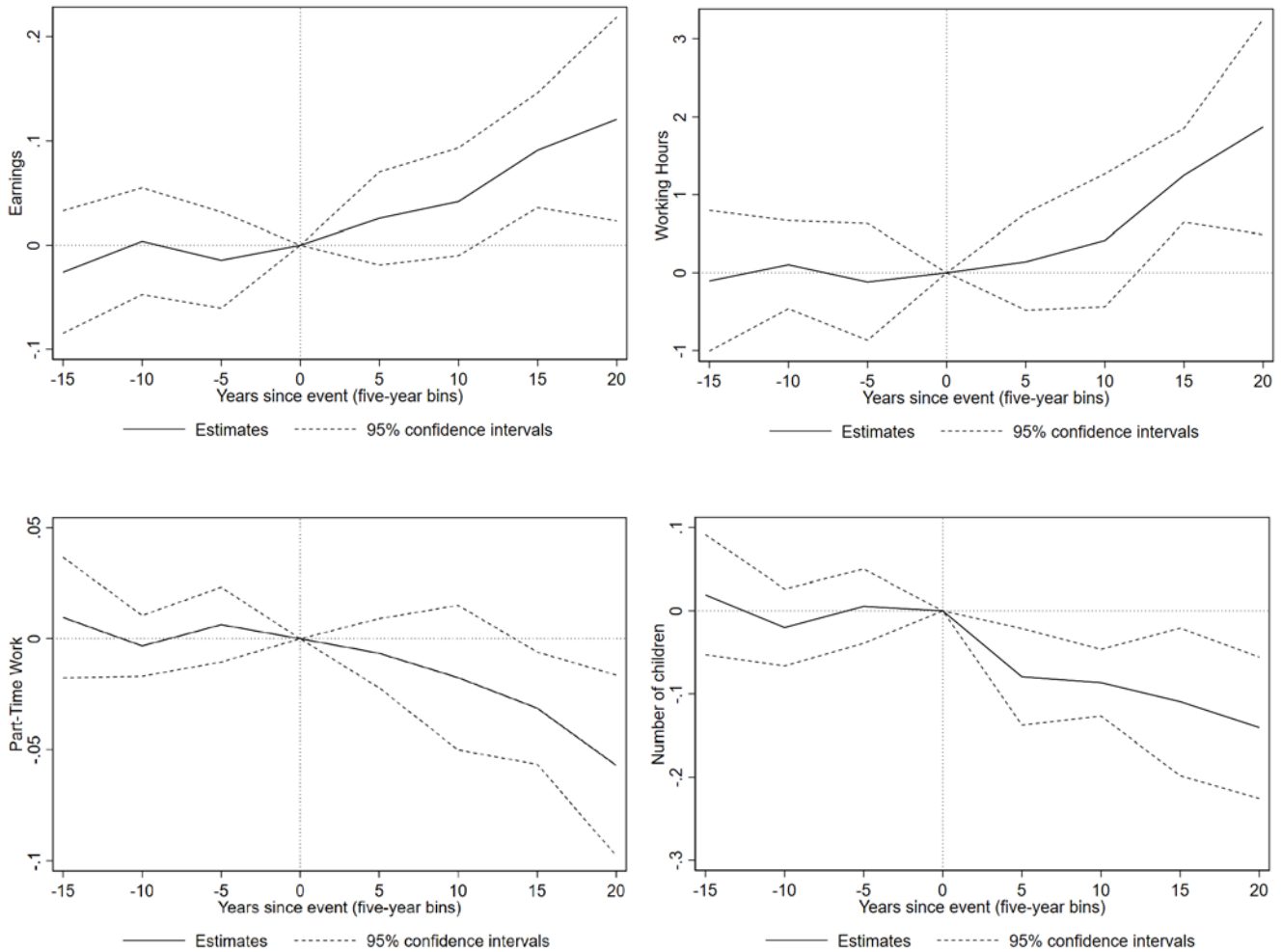


Figure 2 (continued)



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

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

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

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

Table 2: Descriptive statistics

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

(continued on next page)

Table 2 (continued)

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

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

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

	Religiosity (1)	Prayer (2)	Church-going (3)	Affiliation (4)
Reform	-0.071 (0.018) [0.061]	-0.046 (0.101) [0.136]	-0.066 (0.020) [0.022]	-0.081 (0.009) [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 (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, affiliation); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity, prayer, church-going, affiliation); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (church-going, affiliation).

Table 4: Time-varying treatment effects on religious outcomes

	Religiosity		Prayer		Church-going		Affiliation	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reform	-0.072 (0.031) [0.149]	0.017 (0.593) [0.733]	-0.045 (0.129) [0.214]	0.037 (0.159) [0.209]	-0.049 (0.063) [0.075]	0.005 (0.906) [0.925]	-0.087 (0.005) [0.052]	-0.054 (0.034) [0.068]
Years relative to reform	0.000 (0.942) [0.941]	0.002 (0.611) [0.731]	-0.000 (0.821) [0.822]	0.001 (0.660) [0.715]	-0.007 (0.007) [0.284]	-0.006 (0.015) [0.328]	0.003 (0.135) [0.231]	0.003 (0.051) [0.149]
Reform x Years relative to reform		-0.013 (0.001) [0.105]		-0.012 (0.001) [0.035]		-0.007 (0.161) [0.480]		-0.004 (0.129) [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 (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, affiliation); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity, prayer, church-going, affiliation); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (church-going, affiliation).

Table 5: Heterogeneous treatment effects on religious outcomes

	Religiosity		Prayer		Church-going		Affiliation	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Gender	Females	Males	Females	Males	Females	Males	Females	Males
Reform	-0.067 (0.024) [0.033]	-0.073 (0.060) [0.237]	-0.085 (0.057) [0.114]	-0.007 (0.835) [0.841]	-0.039 (0.251) [0.179]	-0.097 (0.009) [0.037]	-0.075 (0.012) [0.112]	-0.085 (0.025) [0.094]
Panel B: Area	Rural	Urban	Rural	Urban	Rural	Urban	Rural	Urban
Reform	-0.067 (0.038) [0.007]	-0.123 (0.071) [0.102]	-0.100 (0.037) [0.034]	-0.024 (0.615) [0.572]	–	–	-0.064 (0.131) [0.196]	-0.040 (0.670) [0.700]
Panel C: Area	Catholic	Protestant	Catholic	Protestant	Catholic	Protestant	Catholic	Protestant
Reform	-0.157 (0.009) [0.021]	-0.016 (0.687) [0.655]	-0.124 (0.004) [0.015]	-0.041 (0.482) [0.468]	–	–	-0.211 (0.001) [0.017]	0.064 (0.285) [0.317]
Panel D: Parents	Catholic	Protestant	Catholic	Protestant	Catholic	Protestant	Catholic	Protestant
Reform	–	–	–	–	-0.071 (0.199) [0.324]	0.004 (0.904) [0.903]	-0.077 (0.044) [0.199]	-0.113 (0.047) [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 (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, religious affiliation); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity, prayer, church-going, religious affiliation); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (church-going, religious affiliation).

Table 6: Effects on ethical-value and political-value outcomes**Panel A: Ethical-value outcomes**

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

Panel B: Political-value outcomes

	Interest in politics (1)	Politics too complicated (2)	Satisfaction with democracy (3)	Political spectrum: right (4)	Vote in election (5)	Vote left (6)	Vote extreme (7)
Reform	0.010 (0.530) [0.603]	0.017 (0.675) [0.718]	0.001 (0.980) [0.992]	-0.021 (0.195) [0.249]	0.011 (0.070) [0.128]	-0.016 (0.245) [0.404]	-0.004 (0.477) [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 (trust, risk-taking, volunteering, life satisfaction, interest in politics, politics too complicated, political spectrum: right, vote in election, vote left, vote extreme); German General Social Survey (ALLBUS) Cumulation 1980-2016 (reciprocity, trust, volunteering, life satisfaction, interest in politics, politics too complicated, satisfaction with democracy, political spectrum: right, vote in election, vote left, vote extreme); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (reciprocity, trust, risk-taking, volunteering, life satisfaction, interest in politics, satisfaction with democracy, political spectrum: right, vote in election, vote left, vote extreme).

Table 7: Effects on labor-market outcomes

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

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

Table 8: Effects on educational outcomes

	Years of education (1)	<i>Abitur</i> (2)	Age at first employment (3)
Reform	0.032 (0.670) [0.730]	-0.023 (0.075) [0.226]	0.018 (0.866) [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 (years of education, abitur, age of first employment); German General Social Survey (ALLBUS) Cumulation 1980-2016 (years of education, abitur); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (years of education, abitur, age of first employment).

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

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

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

Appendix A: Data

This appendix provides additional detail on the three individual-level datasets and their preparation and describes how we merge them for our analysis.

A.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.²⁵

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

²⁵ One “starting cohort” contains many birth cohorts. The Starting Cohort 6, which we use in our analysis, includes birth cohorts from 1944 to 1988.

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.

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.²⁶ 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).

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

²⁶ 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)).

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 individuals going to school in counties neighboring each other across state borders (and including county-pair fixed effects).²⁷

A.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 2016.²⁸ 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.

²⁷ 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.

²⁸ Beyond the biennial survey pattern, there was one additional wave administered in 1991.

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 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. 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.

A.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). 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.

A.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.

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.

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.

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

This appendix reports two diagnostic tests of the two-way fixed effects specification that complement the results of the Callaway and Sant'Anna (2021) estimator reported in the main text.

B.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 (see Appendix Table A19).

B.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

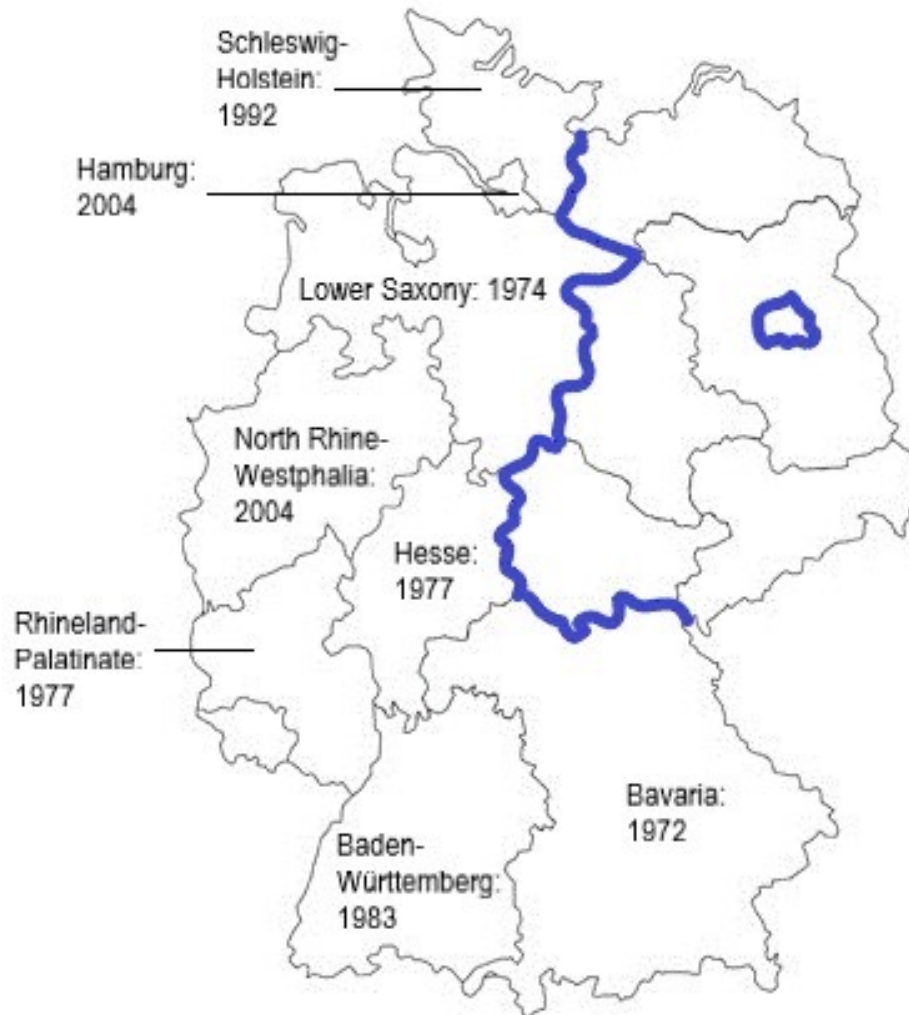
components that is immune to biases from negative weighting. To implement the analysis, we collapse data to means of state-cohort cells. To create a balanced panel, we drop observations in cohorts before 1949 or after 1991, which implies deletion of 21 percent of all state-cohort cells.

The graphs contained in Appendix Tables A20-A23 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 Appendix Tables A20-A23, 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 3). 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.

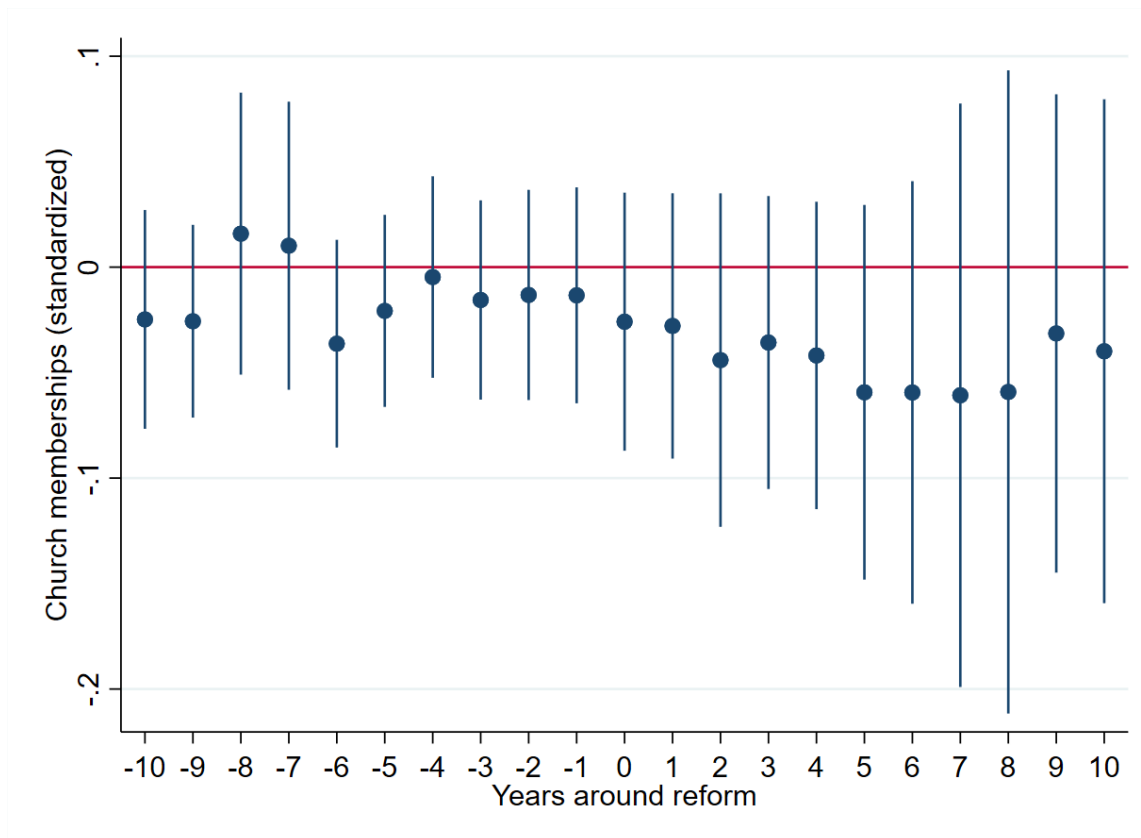
Appendix Figures and Tables

Figure A1: Religious education reforms in West German states



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

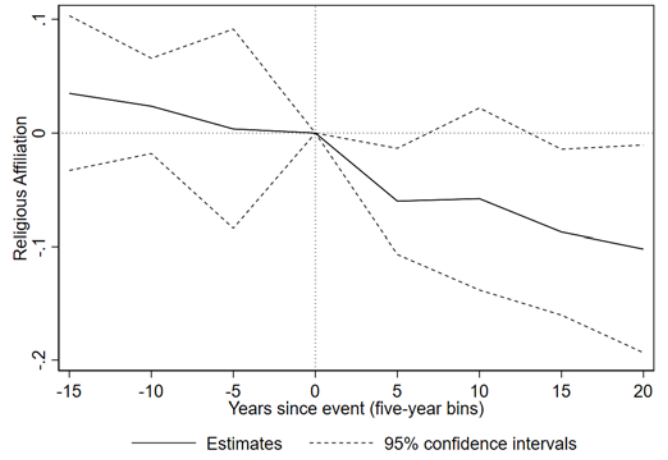
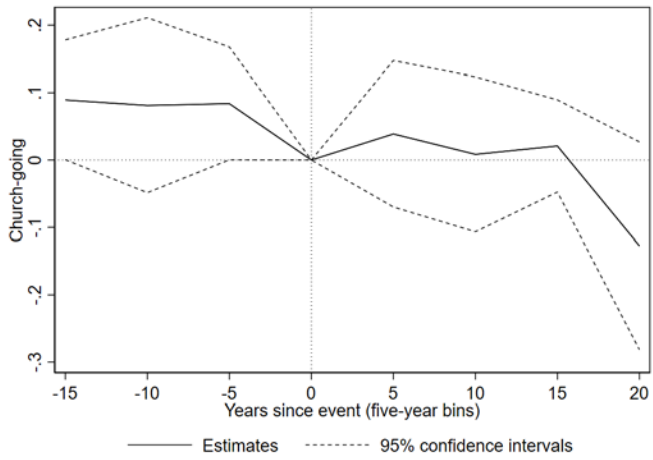
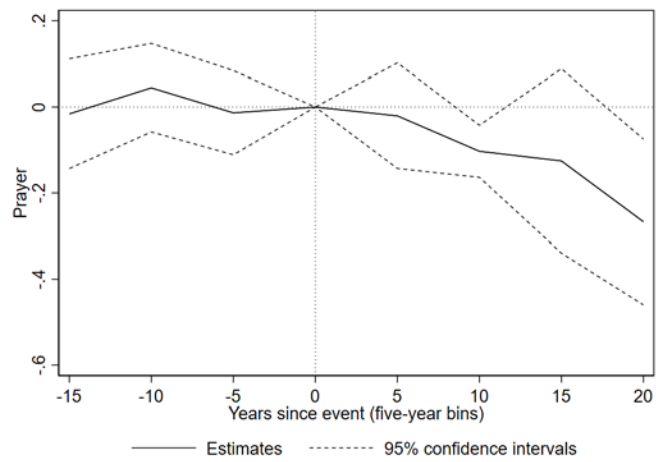
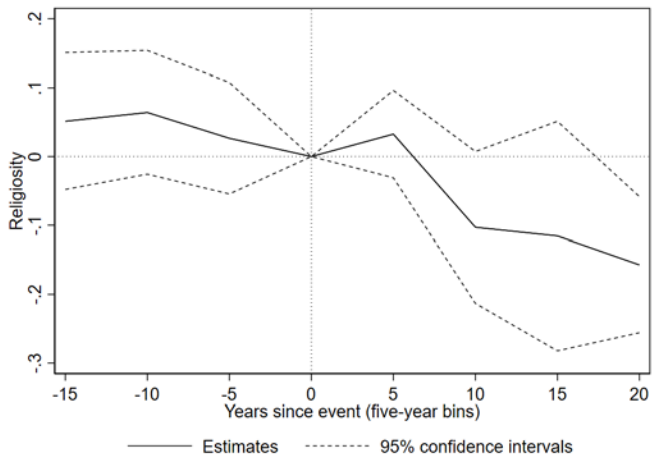
Figure A2: State-wide church membership before and after the reforms



Notes: Coefficients from a regression of church memberships on the leads and lags 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: Effects on religiosity and labor-market outcomes of females

Panel A: Religious Outcomes



Panel B: Labor-Market Outcomes

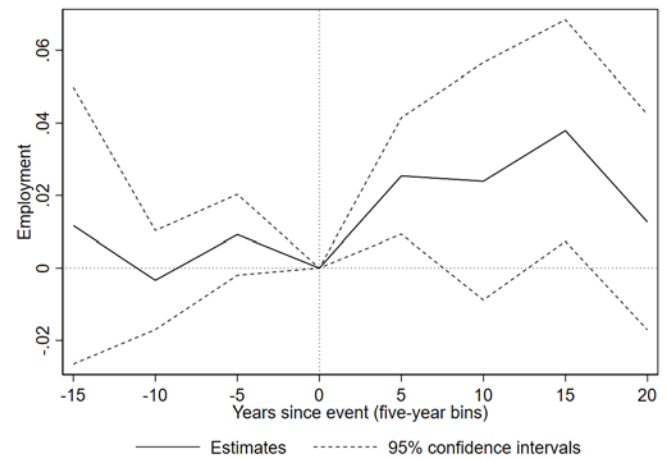
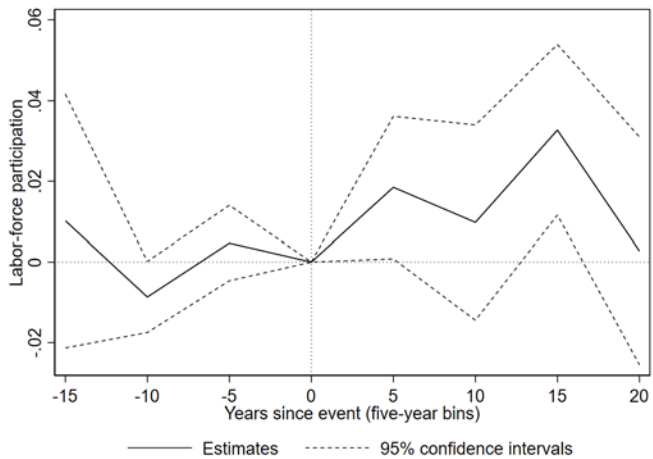
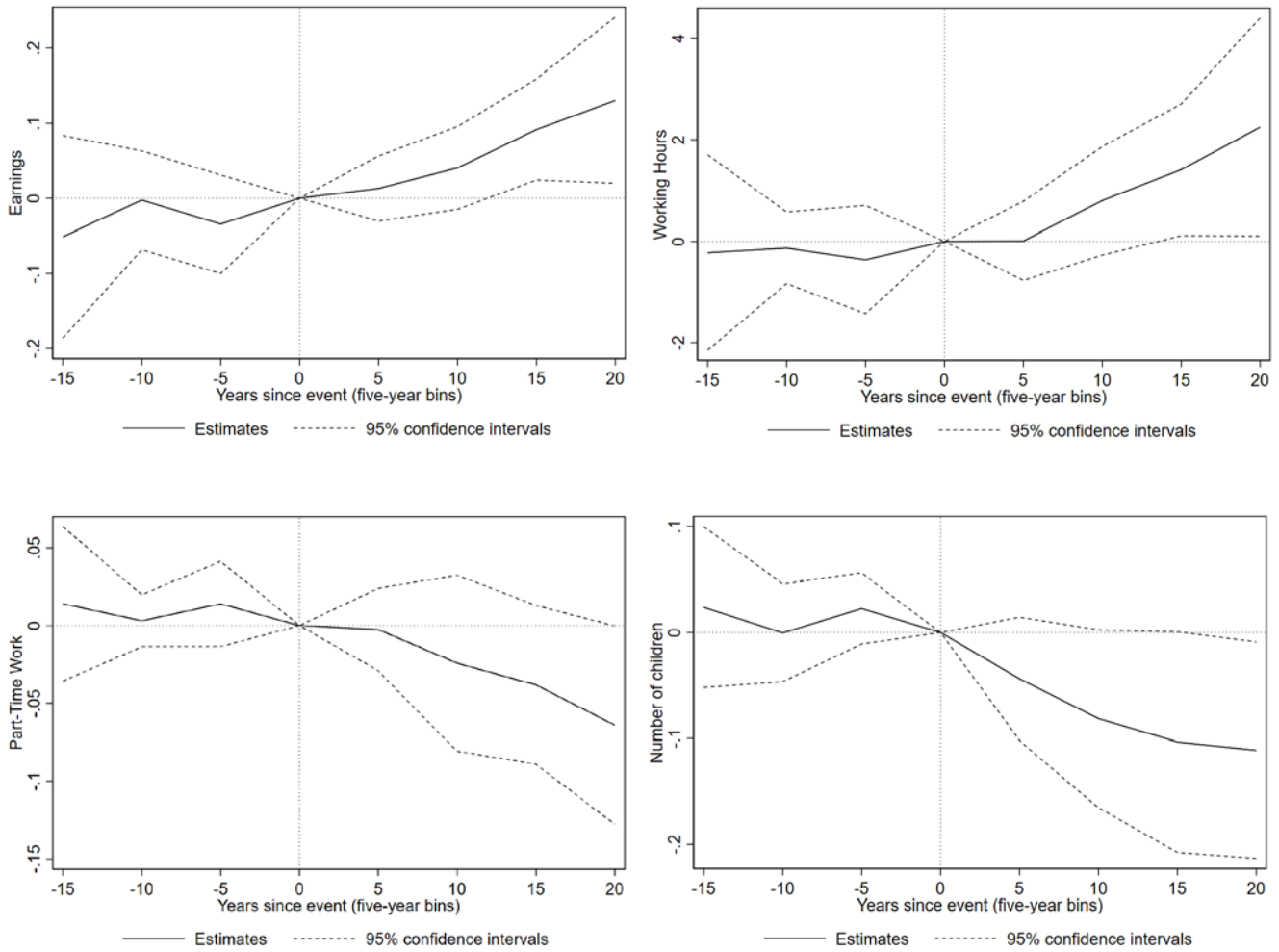


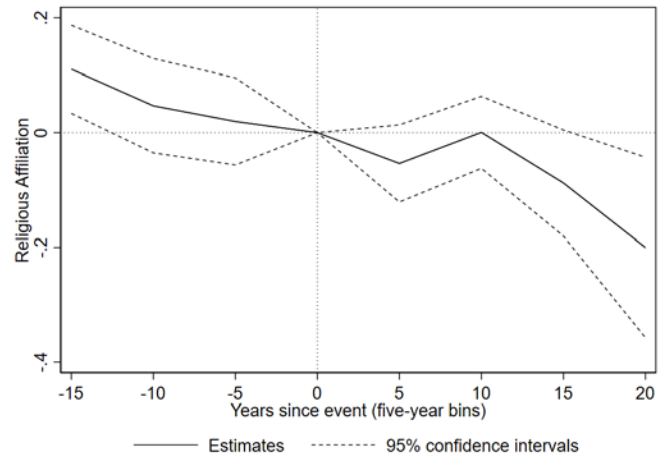
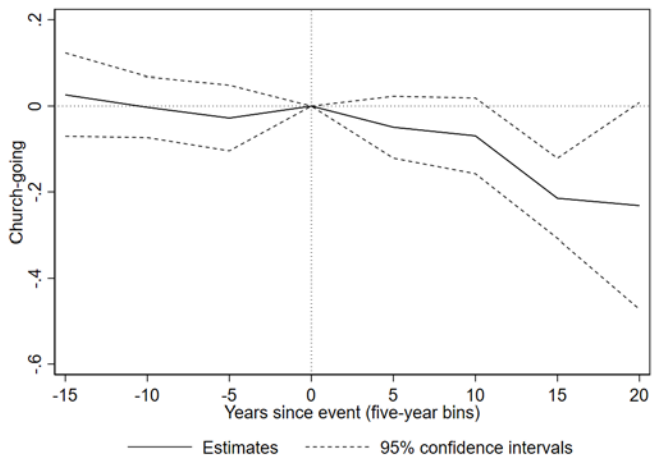
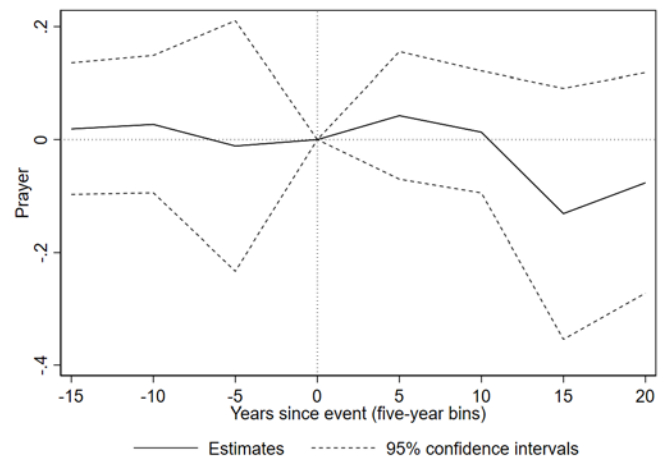
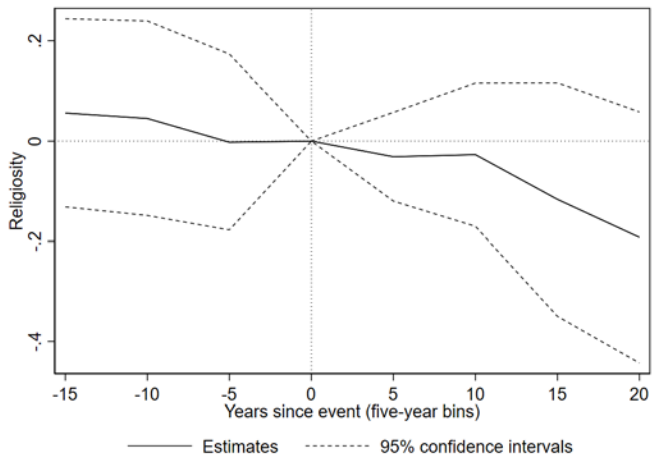
Figure A3 (continued)



Notes: Coefficients from non-parametric event-study regressions and their 95 percent confidence intervals, only females. Dependent variable indicated on the vertical axis (see Table A2 for details). Numbers on horizontal axis refer to final year of respective five-year bins; i.e., 0 = last five years prior to treatment (excluded category), 5 = first five years of treatment. Inference: Standard clustering at state level. The p-values of omnibus hypothesis tests of zero pre- and post-event effects are 0.350 and 0.000 for religiosity, 0.100 and 0.000 for prayer, 0.173 and 0.022 for church-going, 0.539 and 0.080 for religious affiliation, 0.126 and 0.009 for labor-force participation, 0.001 and 0.044 for employment, 0.193 and 0.005 for earnings, 0.794 and 0.024 for working hours, 0.625 and 0.083 for part-time work, and 0.470 and 0.003 for the number of children, respectively. Data sources (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, affiliation, labor-force-participation, employment, earnings, working hours, part-time work, number of children); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity, prayer, church-going, affiliation, labor-force-participation, employment, earnings, working hours, part-time work, number of children); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (church-going, affiliation, labor-force-participation, employment, earnings, working hours, part-time work, number of children).

Figure A4: Effects on religiosity and labor-market outcomes of males

Panel A: Religious Outcomes



Panel B: Labor-Market Outcomes

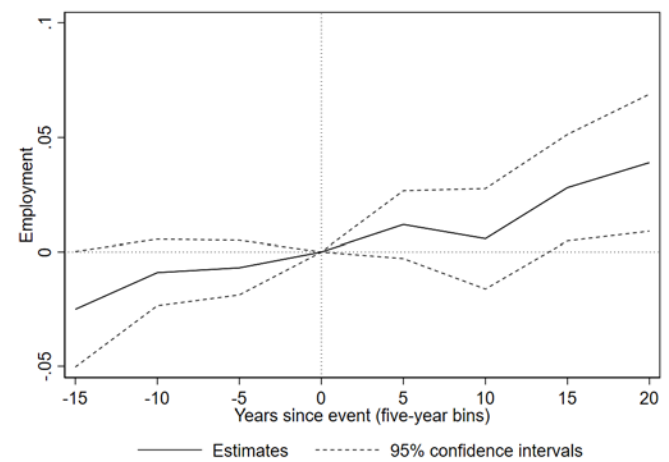
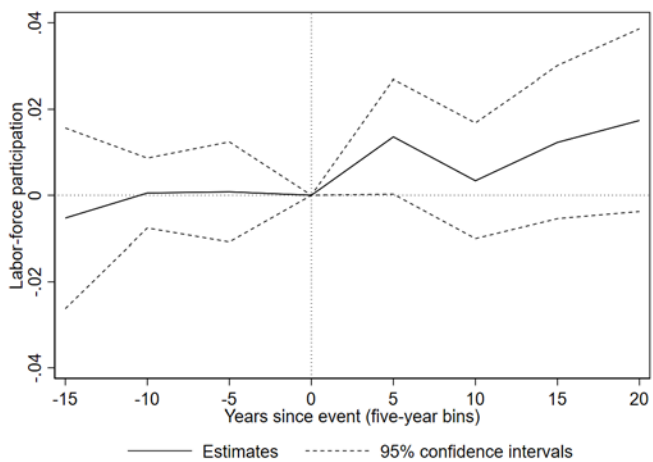
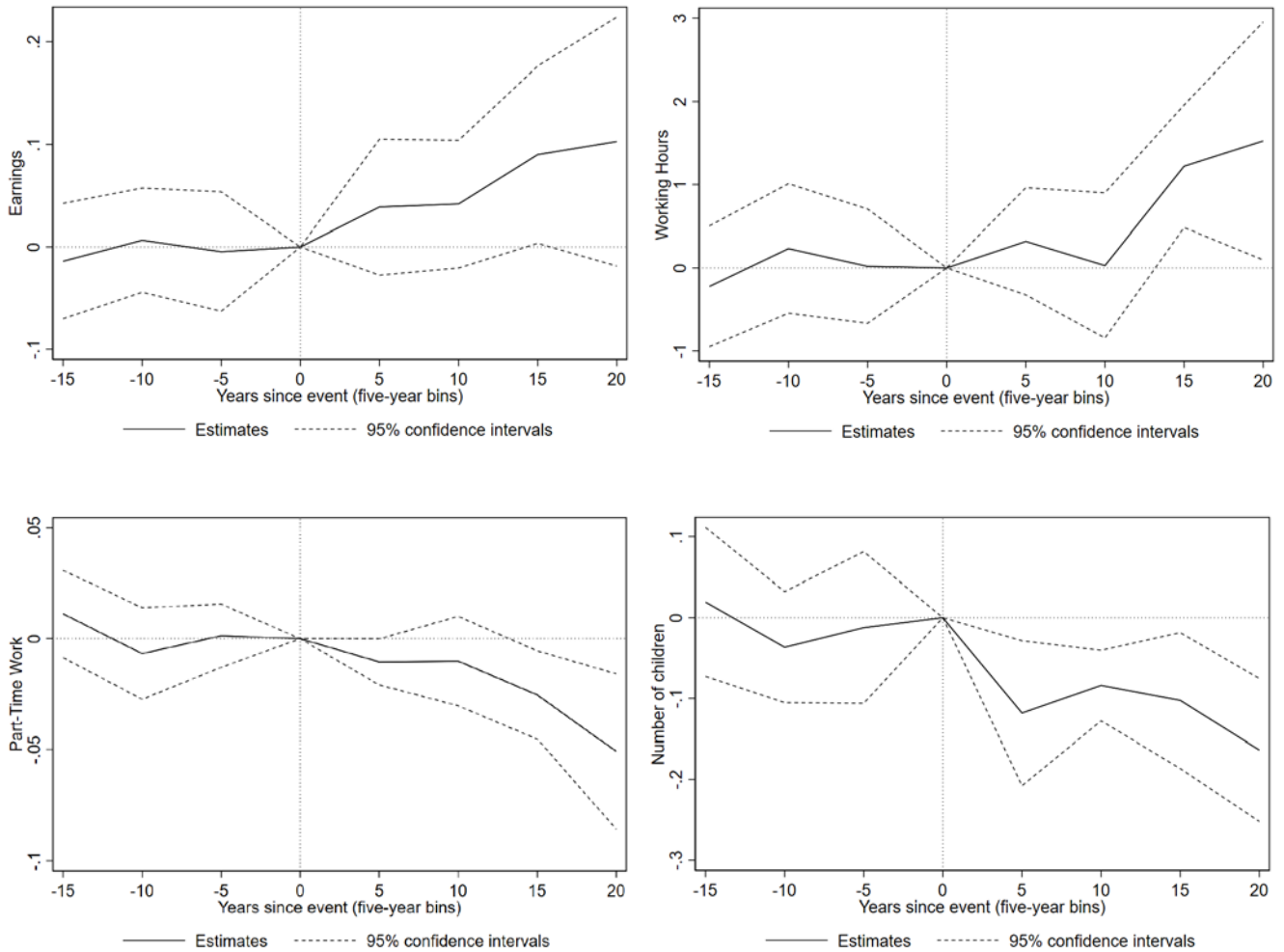
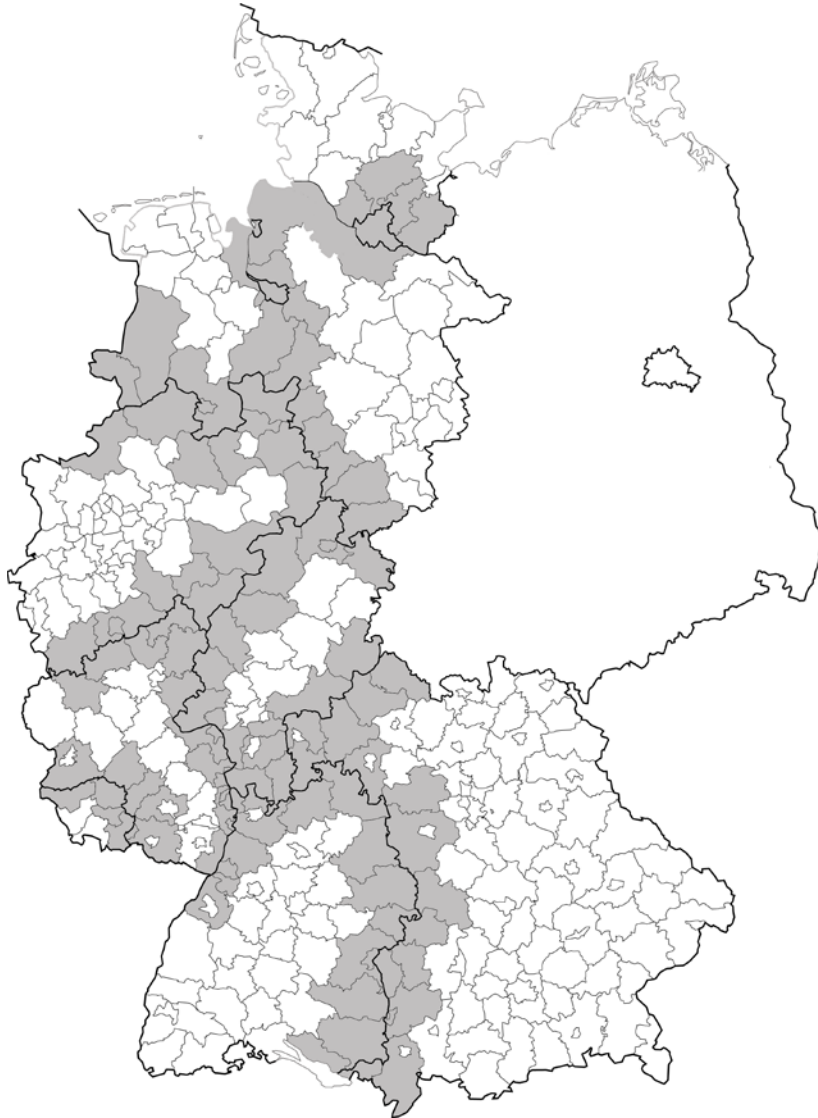


Figure A4 (continued)



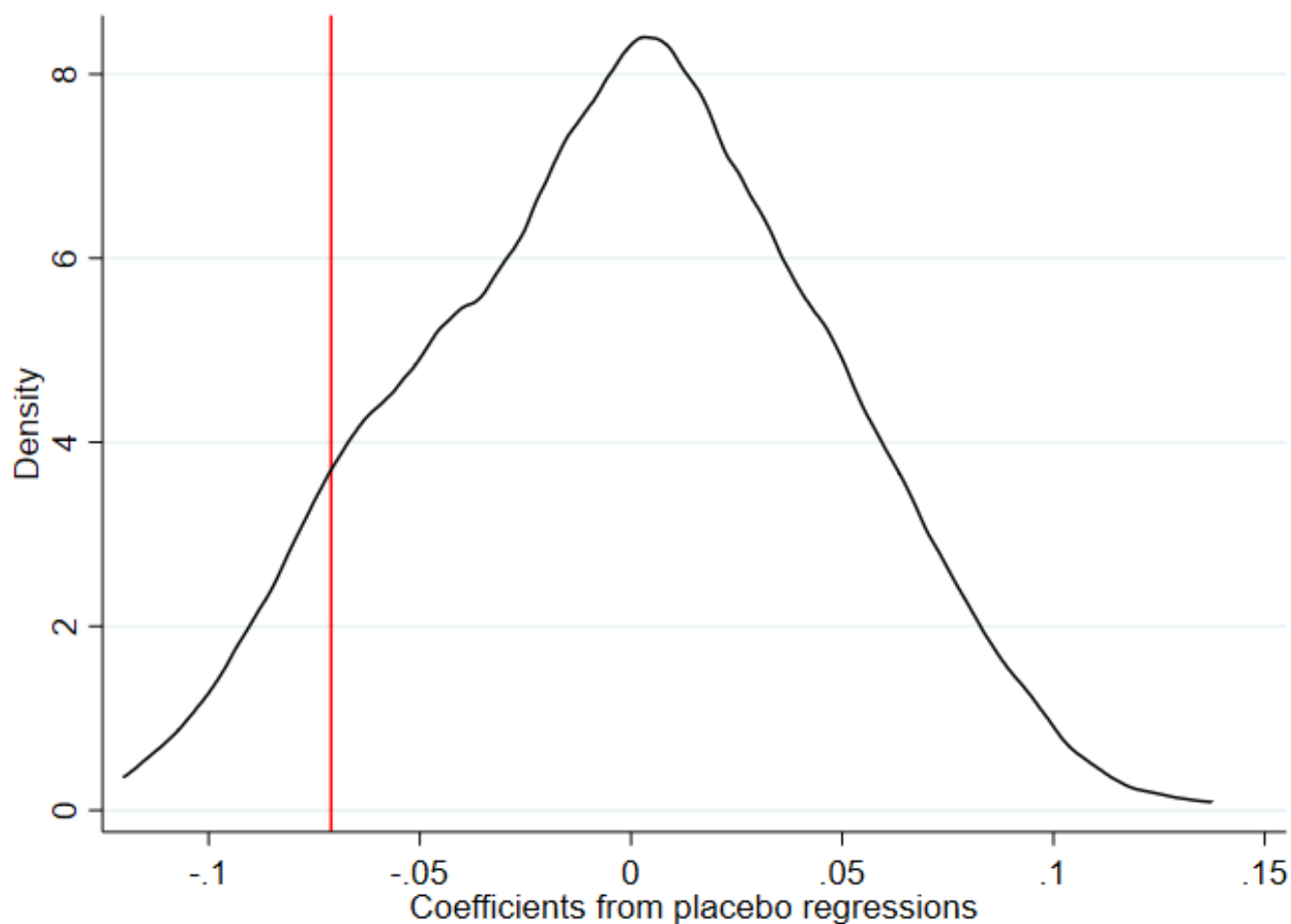
Notes: Coefficients from non-parametric event-study regressions and their 95 percent confidence intervals, only males. Dependent variable indicated on the vertical axis (see Table A2 for details). Numbers on horizontal axis refer to final year of respective five-year bins; i.e., 0 = last five years prior to treatment (excluded category), 5 = first five years of treatment. Inference: Standard clustering at state level. The p-values of omnibus hypothesis tests of zero pre- and post-event effects are 0.564 and 0.424 for religiosity, 0.893 and 0.022 for prayer, 0.393 and 0.002 for church-going, 0.002 and 0.116 for religious affiliation, 0.812 and 0.186 for labor-force participation, 0.211 and 0.022 for employment, 0.896 and 0.146 for earnings, 0.806 and 0.010 for working hours, 0.186 and 0.010 for part-time work, and 0.063 and 0.000 for the number of children, respectively. Data sources (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, affiliation, labor-force-participation, employment, earnings, working hours, part-time work, number of children); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity, prayer, church-going, affiliation, labor-force-participation, employment, earnings, working hours, part-time work, number of children); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (church-going, affiliation, labor-force-participation, employment, earnings, working hours, part-time work, number of children).

Figure A5: 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.

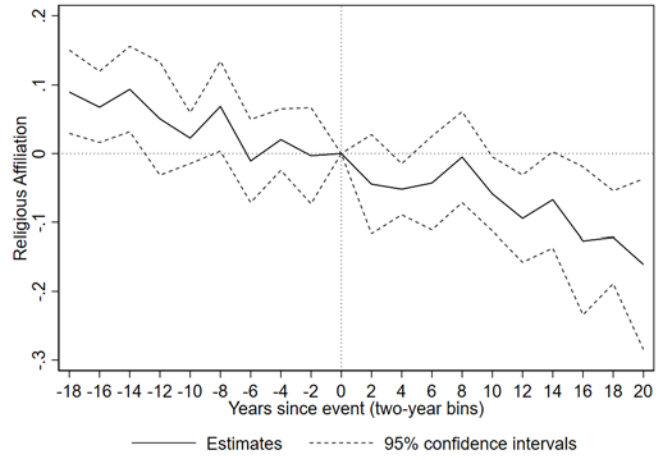
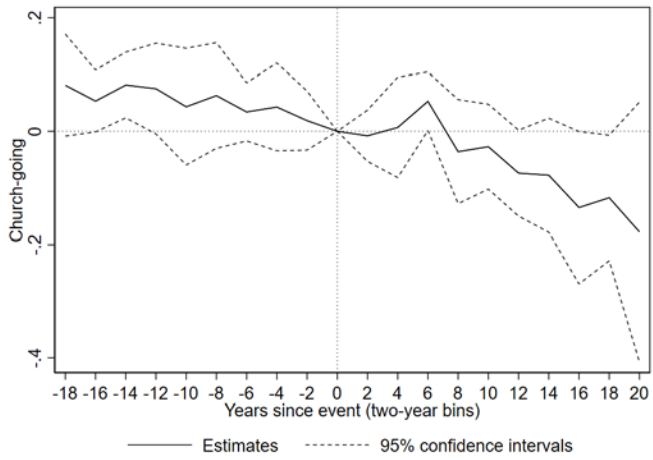
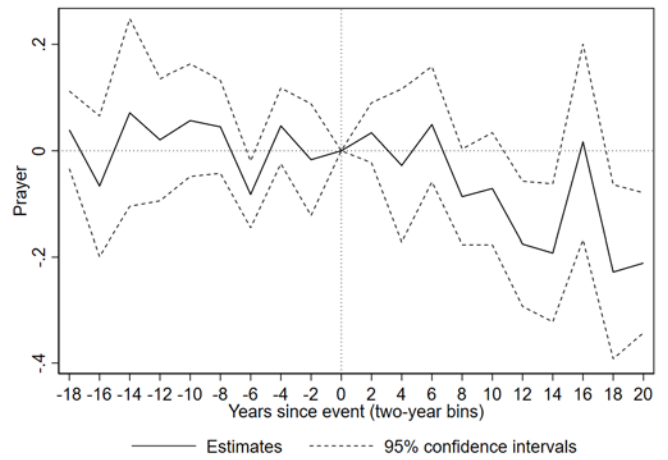
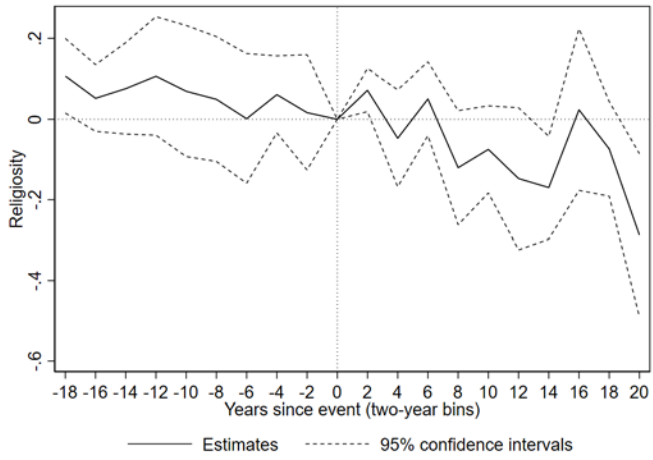
Figure A6: Distribution of placebo estimates of reform effect on religiosity



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, birth-year, 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 (religiosity); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity).

Figure A7: Event-study estimates with two-year-bins

Panel A: Religious Outcomes



Panel B: Labor-Market Outcomes

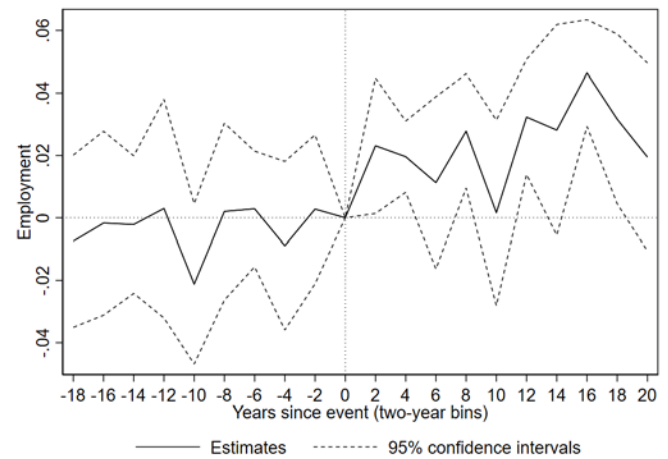
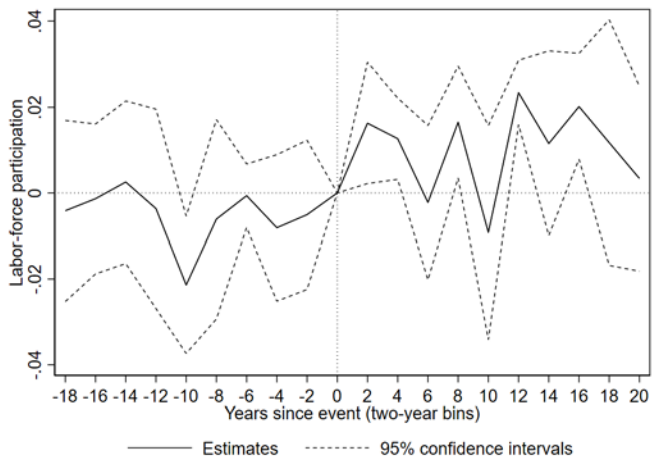
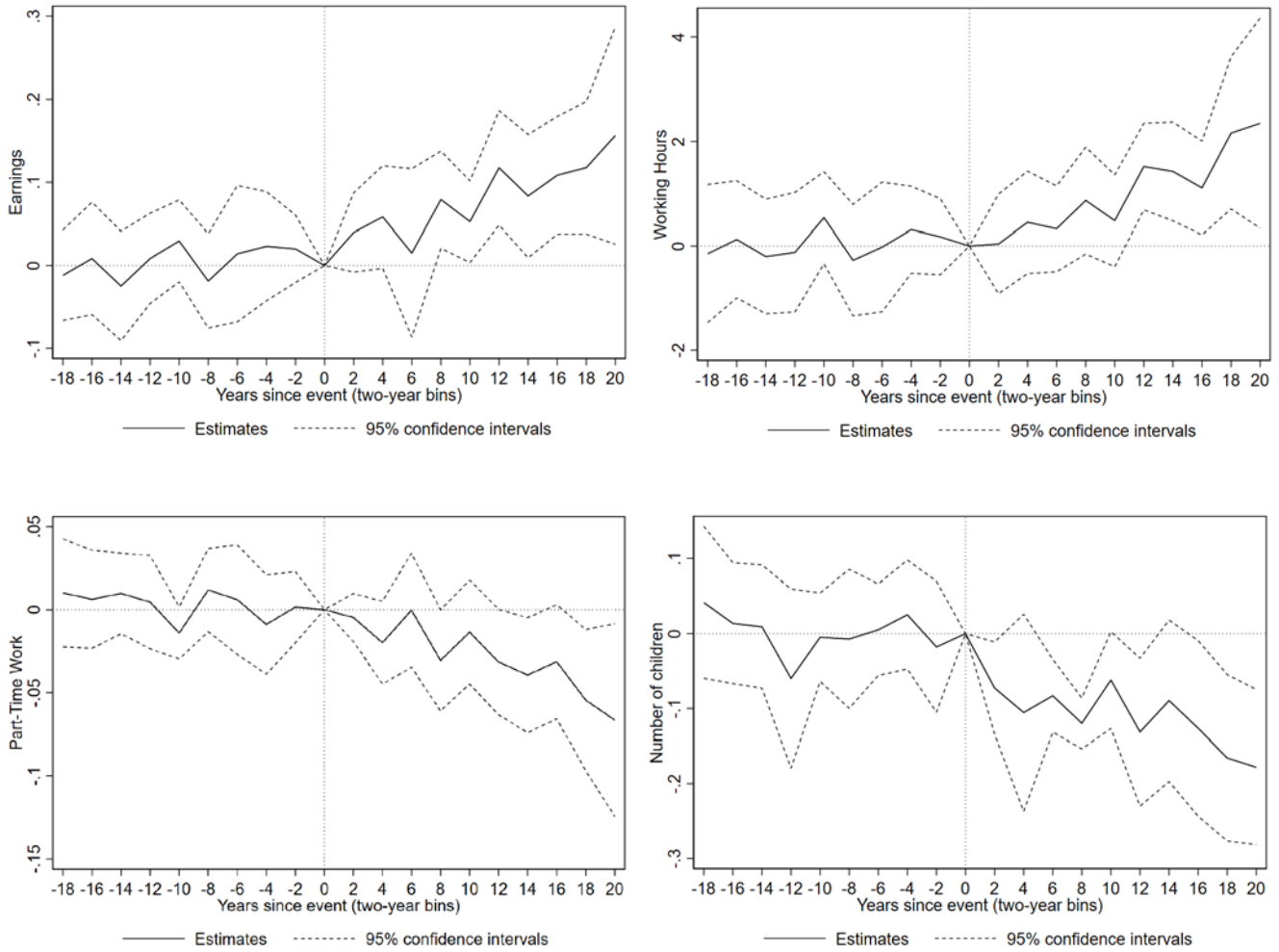


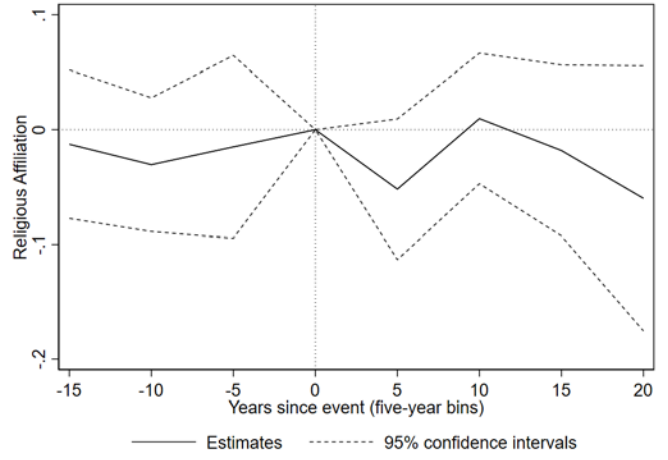
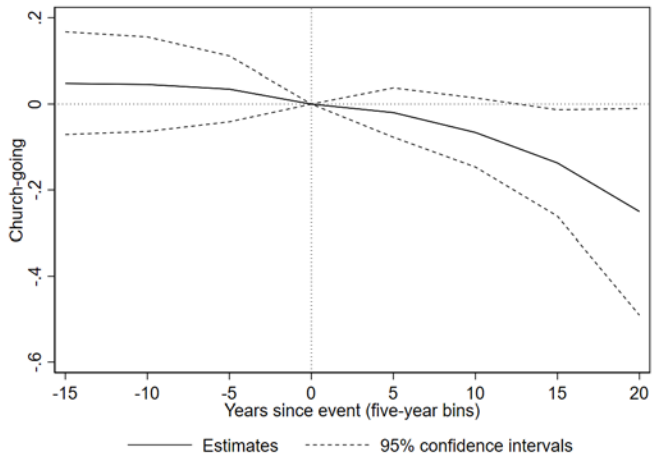
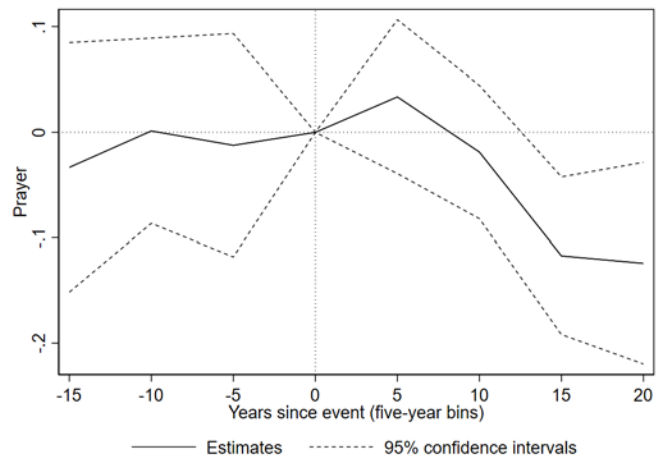
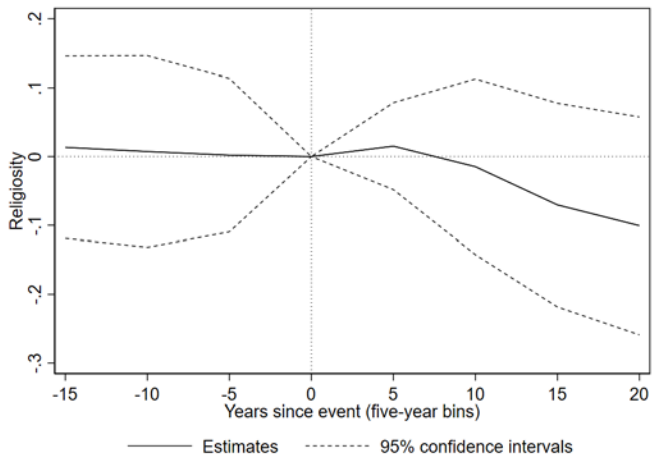
Figure A7 (continued)



Notes: Coefficients from non-parametric event-study regressions and their 95 percent confidence intervals. Dependent variable indicated on the vertical axis (see Table A2 for details). Numbers on horizontal axis refer to final year of respective two-year bins; i.e., 0 = last two years prior to treatment (excluded category), 2 = first two years of treatment. Inference: Standard clustering at state level. The p-values of omnibus hypothesis tests of zero pre- and post-event effects are 0.007 and 0.000 for religiosity, 0.000 and 0.000 for prayer, 0.000 and 0.000 for church-going, 0.000 and 0.000 for religious affiliation, 0.000 and 0.000 for labor-force participation, 0.036 and 0.000 for employment, 0.000 and 0.000 for earnings, 0.000 and 0.000 for working hours, 0.001 and 0.000 for part-time work, and 0.000 and 0.000 for the number of children, respectively. Data sources (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, affiliation, labor-force-participation, employment, earnings, working hours, part-time work, number of children); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity, prayer, church-going, affiliation, labor-force-participation, employment, earnings, working hours, part-time work, number of children); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (church-going, affiliation, labor-force-participation, employment, earnings, working hours, part-time work, number of children).

Figure A8: Sample of states that reform between 1972 and 1983

Panel A: Religious Outcomes



Panel B: Labor-Market Outcomes

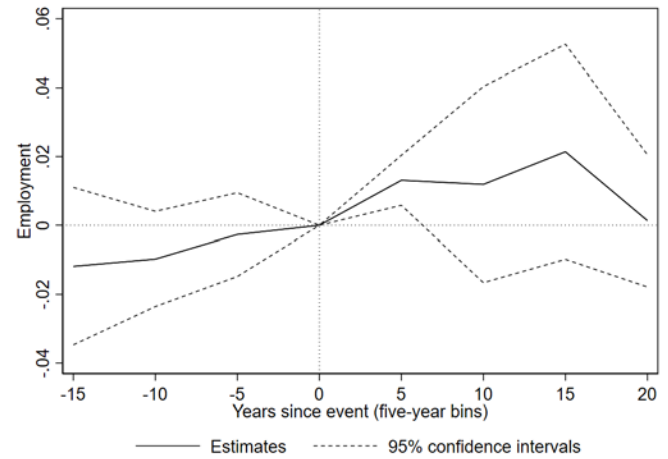
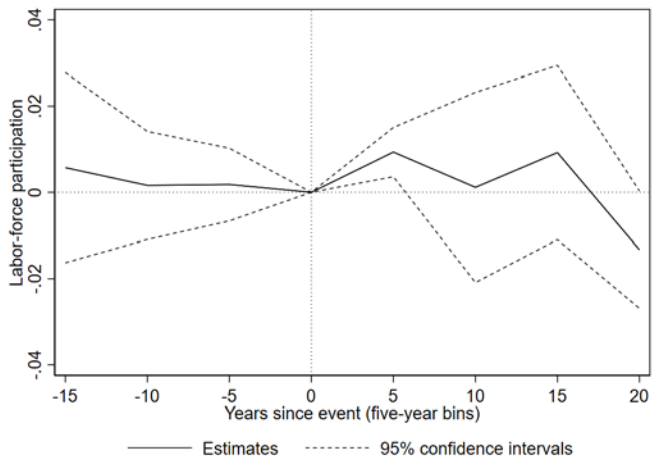
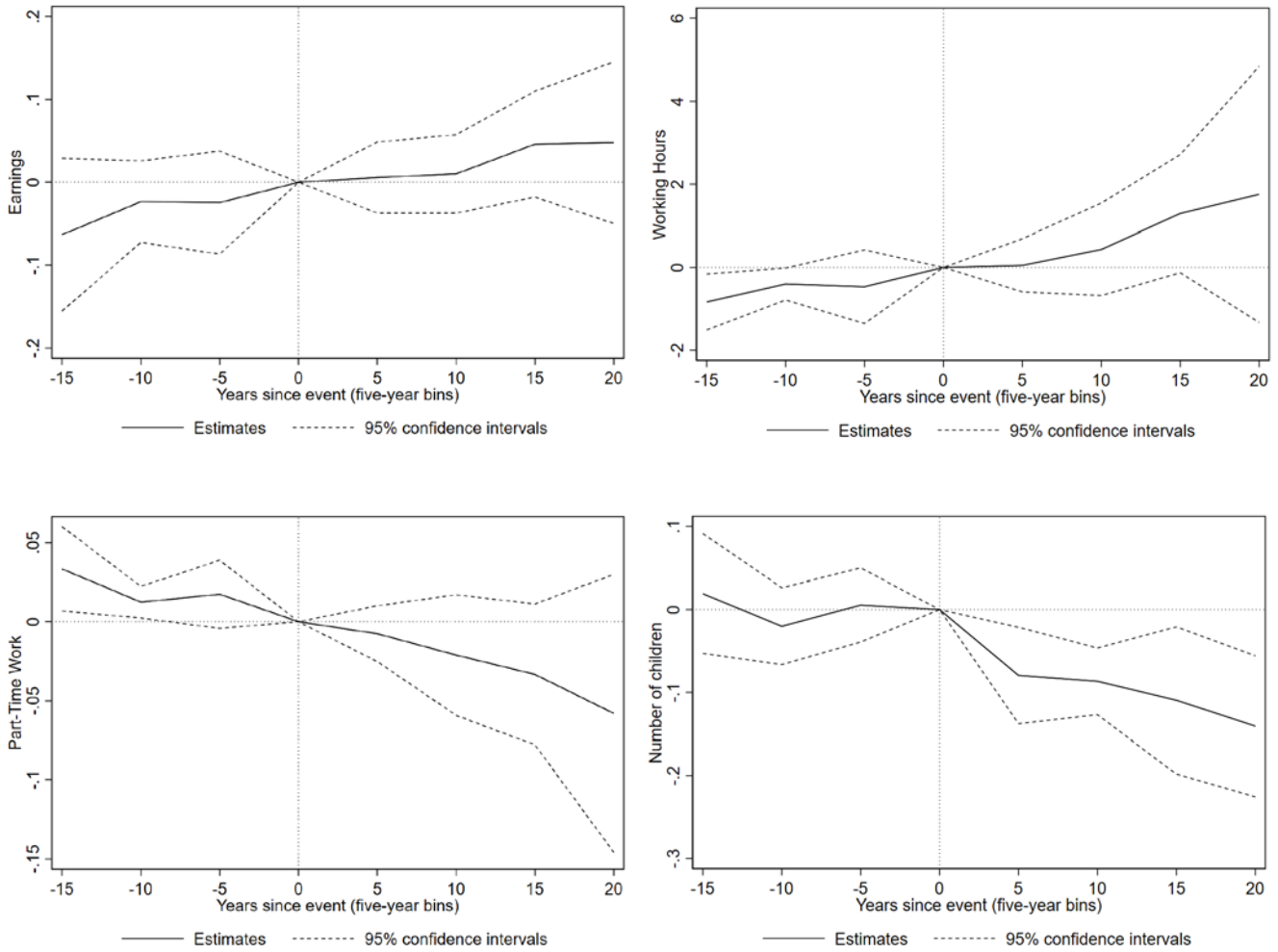
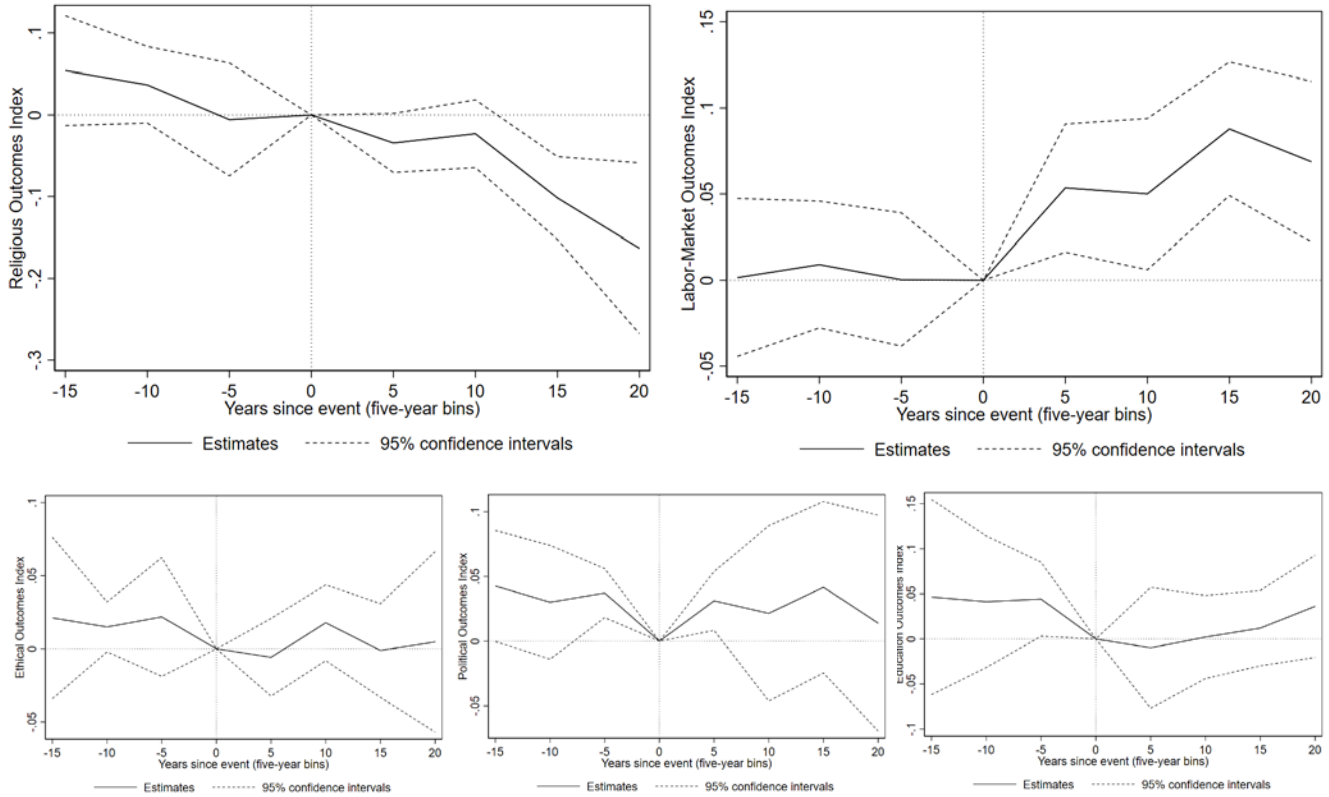


Figure A8 (continued)



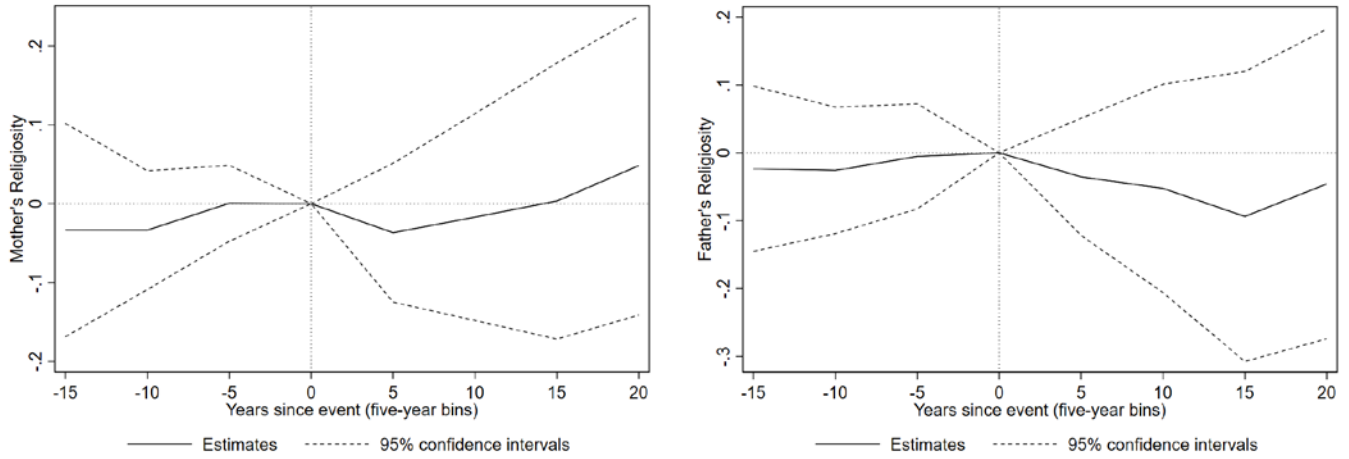
Notes: Coefficients from non-parametric event-study regressions and their 95 percent confidence intervals, keep only states that reform between 1972 and 1983 (and non-reforming states). Dependent variable indicated on the vertical axis (see Table A2 for details). Numbers on horizontal axis refer to final year of respective five-year bins; i.e., 0 = last five years prior to treatment (excluded category), 5 = first five years of treatment. Inference: Standard clustering at state level. The p-values of omnibus hypothesis tests of zero pre- and post-event effects are 0.995 and 0.110 for religiosity, 0.847 and 0.016 for prayer, 0.704 and 0.189 for church-going, 0.495 and 0.252 for religious affiliation, 0.862 and 0.004 for labor-force participation, 0.206 and 0.014 for employment, 0.237 and 0.188 for earnings, 0.069 and 0.001 for working hours, 0.077 and 0.325 for part-time work, and 0.454 and 0.013 for the number of children, respectively. Data sources (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, affiliation, labor-force-participation, employment, earnings, working hours, part-time work, number of children); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity, prayer, church-going, affiliation, labor-force-participation, employment, earnings, working hours, part-time work, number of children); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (church-going, affiliation, labor-force-participation, employment, earnings, working hours, part-time work, number of children).

Figure A9: Event-study estimates for outcome indices



Notes: Coefficients from non-parametric event-study regressions and their 95 percent confidence intervals. Dependent variable indicated on the vertical axis. 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 3; (2) Table 7 (“part-time work” and “number of children” inverted); (3) Panel A of Table 6; (4) Panel B of Table 6 (“politics too complicated” inverted); (5) Table 8). 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 0.158 and 0.008 for religious outcomes index, 0.925 and 0.004 for labor-market outcomes index, 0.248 and 0.109 for ethical outcomes index, 0.009 and 0.013 for political outcomes index, and 0.149 and 0.371 for education outcomes index, respectively. Data sources (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religious outcomes, labor-market outcomes, ethical-value outcomes, political-value outcomes, educational outcomes); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religious outcomes, labor-market outcomes, ethical-value outcomes, political-value outcomes, educational outcomes); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (religious outcomes, labor-market outcomes, ethical-value outcomes, political-value outcomes, educational outcomes).

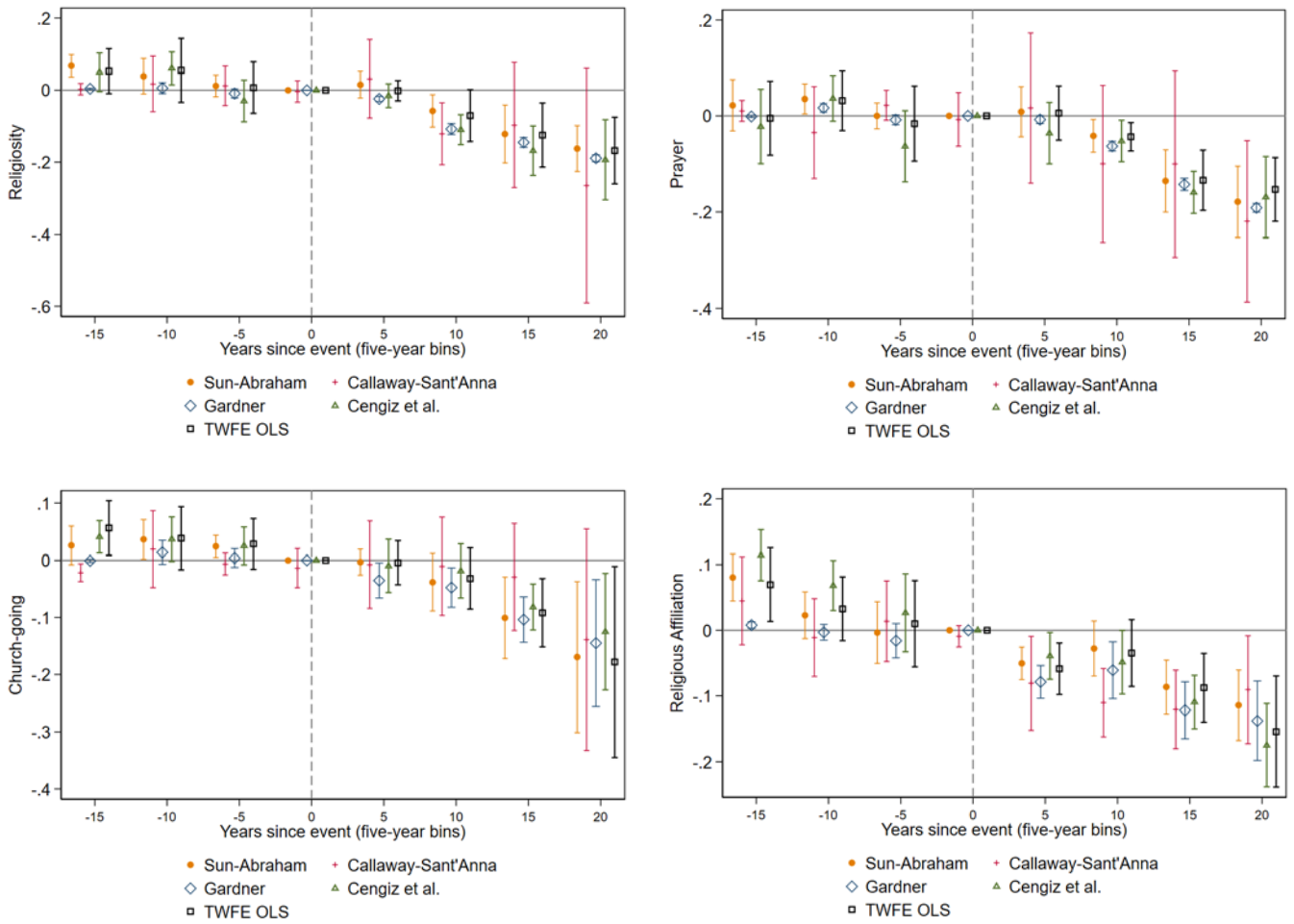
Figure A10: Effects on mothers' and fathers' religiosity



Notes: Coefficients from non-parametric event-study regressions and their 95 percent confidence intervals. Dependent variable indicated on the vertical axis (see Table A2 for details). Numbers on horizontal axis refer to final year of respective five-year bins; i.e., 0 = last five years prior to treatment (excluded category), 5 = first five years of treatment. Inference: Standard clustering at state level. The p-values of omnibus hypothesis tests of zero pre- and post-event effects are 0.536 and 0.192 for mother's religiosity, and 0.775 and 0.338 for father's religiosity, respectively. Data sources (by outcomes): German General Social Survey (ALLBUS) Cumulation 1980-2016 (mother's religiosity, father's religiosity); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (mother's religiosity, father's religiosity).

Figure A11: Alternative estimators robust to time-varying treatment effects

Panel A: Religious Outcomes



Panel B: Labor-Market Outcomes

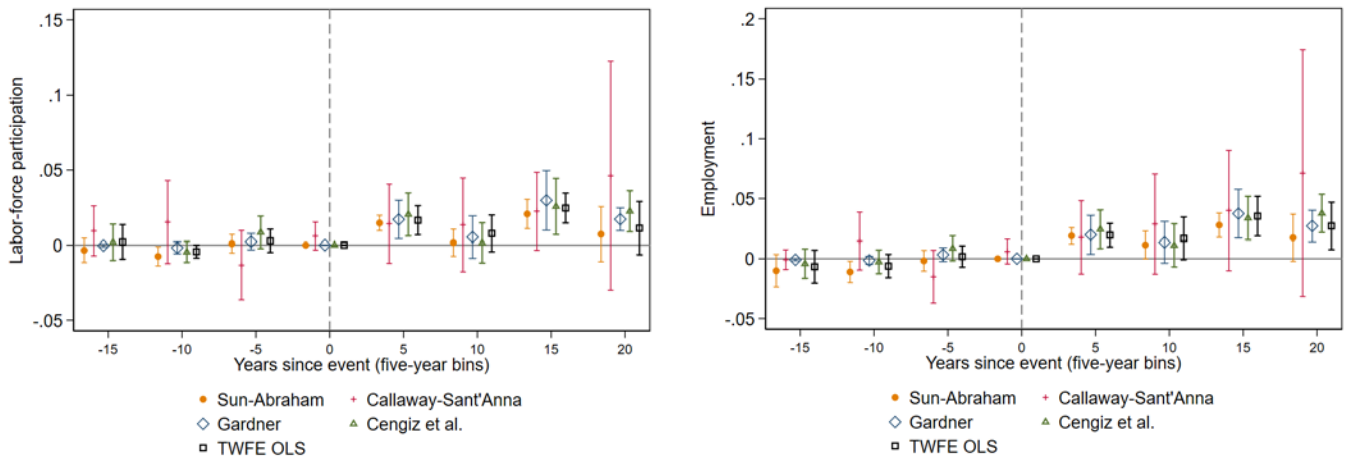
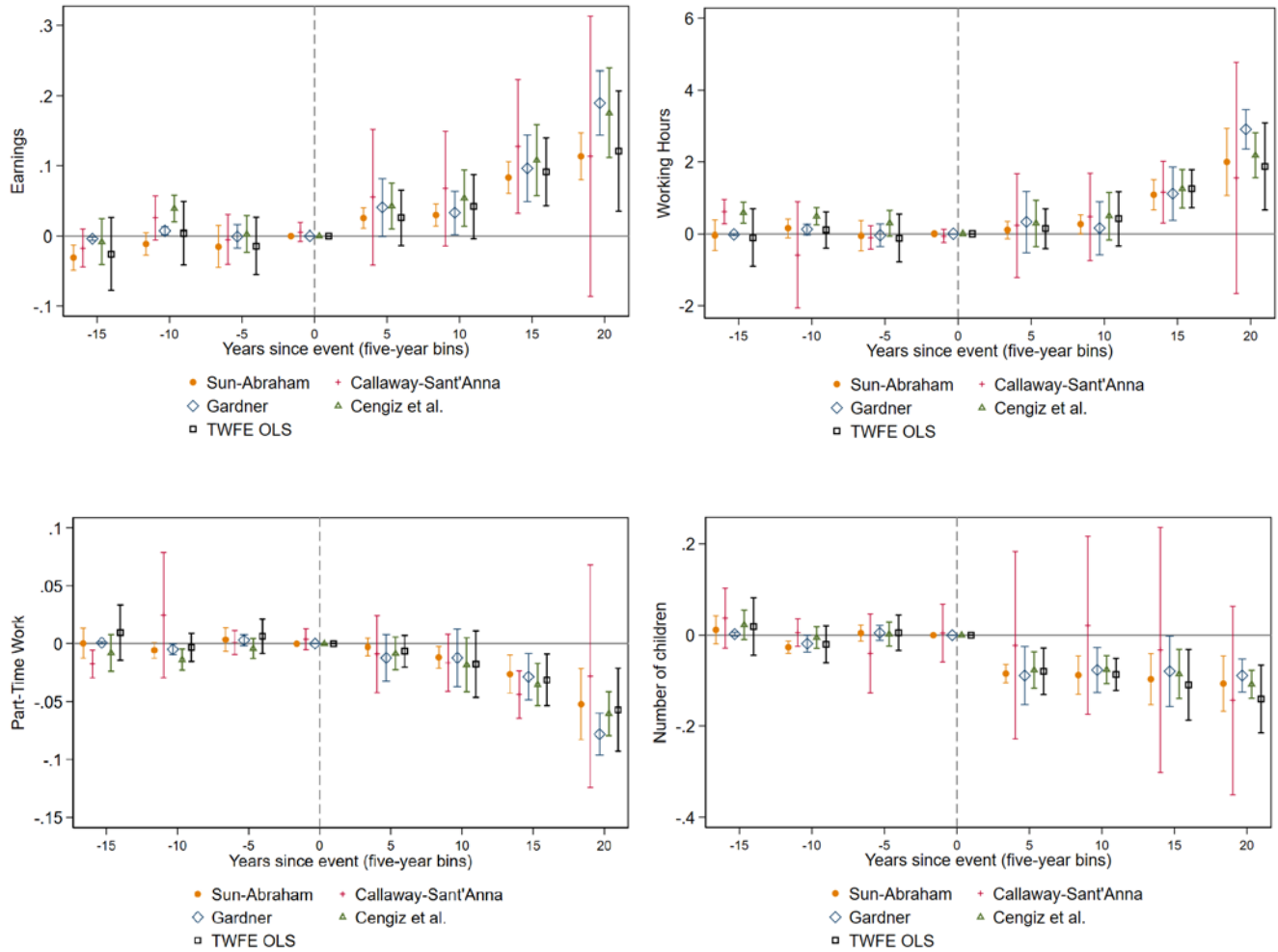


Figure A11 (continued)



Notes: Coefficients from non-parametric event-study regressions and their 95 percent confidence intervals. Abraham and Sun (2021) estimator depicted in orange with circle markers, using last-treated unit as control cohort. Callaway and Sant'Anna (2021) estimator depicted in red with plus markers, using doubly robust inverse probability weighting and not-yet-treated and never-treated observations as controls. Gardner (2021) estimator depicted in blue with diamond markers, using not-yet-treated and never-treated observations as controls. Cengiz et al. (2019) estimator depicted in green with triangle markers, using last-treated unit as control cohort. TWFE OLS depicted in black with square markers. Controls: Indicator variables for gender, races/ethnicities, subsidized lunch status, home possessions (separate indicator variables for computer and books), birth month, test session, and fixed effects for state, cohort, and test year. Dependent variable indicated on the vertical axis (see Table A2 for details). Numbers on horizontal axis refer to final year of respective five-year bins; i.e., 0 = last five years prior to treatment (excluded category), 5 = first five years of treatment. Inference: Standard clustering at state level. Data sources (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, affiliation, labor-force-participation, employment, earnings, working hours, part-time work, number of children); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity, prayer, church-going, affiliation, labor-force-participation, employment, earnings, working hours, part-time work, number of children); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (church-going, affiliation, labor-force-participation, employment, earnings, working hours, part-time work, number of children).

Table A1: 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
<p>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.)</p> <p>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)</p>	<p>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.)</p>	<p>The syllabus discusses ethical questions that are important in life in our time and covers them more and more in depth in every grade.</p> <p>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.</p>
<p>Religious education is to be:</p> <ul style="list-style-type: none"> - 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) 	<p>Religious education is to be:</p> <ul style="list-style-type: none"> - 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) 	

(continued on next page)

Table A1 (continued)

Syllabus of Catholic religious education, 1967	Syllabus of Catholic religious education, 1979	Syllabus of Ethics, 1986
<p>Main goals of religious education:</p> <ul style="list-style-type: none"> - 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) 	<p>Main goals of religious education:</p> <ul style="list-style-type: none"> - 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) 	<p>Main goals of ethics:</p> <ul style="list-style-type: none"> - 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
<p>Syllabus in grades 5-10:</p> <ul style="list-style-type: none"> - 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) 	<p>Syllabus in grades 5-10:</p> <ul style="list-style-type: none"> - 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) 	<p>Syllabus in grades 5-10:</p> <ul style="list-style-type: none"> - 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, ...
<p>Syllabus is structured by giving one or two topics per grade. Topics are given without specific guidance on how to teach these topics.</p>	<p>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.</p>	<p>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.</p>

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

Table A2: Outcome measures derived from the three datasets

	NEPS (1)	ALLBUS (2)	SOEP (3)
<i>Religious outcomes</i>			
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)
<i>Ethical-value outcomes</i>			
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)

(continued on next page)

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)
<i>Political-value outcomes</i>			
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 <i>Bundestag</i> election? (2)	Did you vote in last federal election? (2)	Attendance <i>Bundestag</i> election 2013 (2)
Vote left (d)	If <i>Bundestag</i> 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 (<i>Bundestagswahl</i>) on September 22, 2013? Which party did you vote for? (9)
Vote extreme (d)	If <i>Bundestag</i> 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 (<i>Bundestagswahl</i>) on September 22, 2013? Which party did you vote for? (9)
<i>Labor-market outcomes</i>			
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)
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?
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?
Part-time work (d)	Derived from: Working hours (=1, if number of working hours <= 20; =0 if number of working hours > 20)	Derived from: Working hours (=1, if number of working hours <= 20; =0 if number of working hours > 20)	Derived from: Working hours (=1, if number of working hours <= 20; =0 if number of working hours > 20)

(continued on next page)

Table A2 (continued)

	NEPS (1)	ALLBUS (2)	SOEP (3)
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?
<i>Educational outcomes</i>			
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
<i>Abitur</i> (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?
<i>Parental outcomes</i>			
Mother's religiosity (s)	-	When you were a child, what religion did your mother belong to at that time? (6)	Does / did your mother belong to a church or religious group? (6)
Father's religiosity (s)	-	When you were a child, what religion did your father belong to at that time? (6)	Does / did your father belong to a church or religious group? (6)

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. 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: Background characteristics by treatment status

	Exposed to reform		Not exposed to reform		Δ Mean (5)
	Mean (1)	Std. dev. (2)	Mean (3)	Std. dev. (4)	
Female	0.523	0.500	0.511	0.500	0.012
Migration status	0.077	0.267	0.042	0.200	0.035
Education mother					
Basic	0.444	0.497	0.683	0.465	-0.239
Medium	0.256	0.437	0.147	0.354	0.109
High	0.133	0.340	0.072	0.258	0.061
Education father					
Basic	0.437	0.496	0.631	0.483	-0.194
Medium	0.170	0.376	0.111	0.314	0.059
High	0.193	0.395	0.140	0.347	0.053
NEPS	0.153	0.360	0.236	0.424	-0.083
ALLBUS	0.213	0.409	0.299	0.458	-0.086
SOEP	0.634	0.482	0.466	0.499	0.168
State (<i>Bundesland</i>)	7.166	2.575	5.796	2.234	1.370
Survey Year	2010.197	5.468	2009.056	6.882	1.141
Age	35.054	10.464	48.536	11.447	-13.482

Notes: Mean, standard deviation, and mean difference of covariates of treated (“Exposed to reform”) and untreated (“Not exposed to reform”) respondents. Sample size of treated (untreated) respondents equals 18,778 (39,925). All mean differences are significant at the 1 percent level. 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. Survey year and age refer to reporting time of religiosity (survey year and age are outcome-specific). Note that survey year and age jointly define birth year. 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: Effects on religious outcomes: Non-parametric estimation

	Religiosity (1)	Prayer (2)	Church-going (3)	Affiliation (4)
Pre 15+	0.053 (0.128) [0.133]	-0.005 (0.901) [0.898]	0.056 (0.043) [0.014]	0.069 (0.035) [0.135]
Pre 10-14	0.055 (0.253) [0.281]	0.032 (0.343) [0.325]	0.038 (0.202) [0.386]	0.033 (0.216) [0.402]
Pre 5-9	0.007 (0.849) [0.881]	-0.016 (0.693) [0.782]	0.029 (0.235) [0.334]	0.010 (0.773) [0.812]
Post 1-5	-0.001 (0.925) [0.917]	0.006 (0.845) [0.833]	-0.004 (0.833) [0.836]	-0.059 (0.015) [0.078]
Post 6-10	-0.070 (0.085) [0.185]	-0.044 (0.016) [0.008]	-0.032 (0.272) [0.342]	-0.035 (0.211) [0.234]
Post 11-15	-0.125 (0.021) [0.076]	-0.134 (0.002) [0.003]	-0.092 (0.013) [0.004]	-0.088 (0.008) [0.026]
Post 16+	-0.168 (0.005) [0.068]	-0.153 (0.001) [0.007]	-0.178 (0.063) [0.113]	-0.154 (0.005) [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 (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, religious affiliation); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity, prayer, church-going, religious affiliation); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (church-going, religious affiliation).

Table A5: Effect on religiosity: Dummy coding of religiosity

	(Rather or very) religious		Very religious	
	Linear probability model (1)	Probit model (2)	Linear probability model (3)	Probit model (4)
Reform	-0.029 (0.066) [0.124]	-0.029 (0.039) [0.073]	-0.022 (0.007) [0.064]	-0.021 (0.001) [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 (by outcome): National Education Panel Study (NEPS) Cohort 6 (religiosity); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity).

Table A6: Heterogeneous treatment effects on labor-market outcomes by Catholic and Protestant areas

	Labor-force participation (1)	Employment (2)	Earnings (3)	Working hours (4)	Part-time work (5)	Number of children (6)
<i>Panel A: Catholic areas</i>						
Reform	0.012 (0.175) [0.199]	0.037 (0.002) [0.026]	0.052 (0.030) [0.089]	1.050 (0.155) [0.157]	-0.025 (0.219) [0.244]	-0.119 (0.022) [0.208]
<i>Panel B: Protestant areas</i>						
Reform	0.010 (0.515) [0.514]	0.021 (0.244) [0.276]	0.002 (0.962) [0.964]	1.236 (0.083) [0.116]	-0.030 (0.211) [0.234]	-0.080 (0.019) [0.050]
State and birth-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

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 (columns (1), (2), (5): indicator variable; columns (4), (6): numbers; column (3): log earnings; 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 A7: Border specification with county-pair fixed effects and treatment assignment based on state of birth

	Religiosity (1)	Prayer (2)	Affiliation (3)
Reform	-0.094 (0.064) [0.031]	-0.100 (0.111) [0.072]	-0.020 (0.648) [0.613]
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). Treatment assigned using state of birth instead of state of schooling. Controls: gender, migration status, mother's education, father's education, survey, survey-year fixed effects, and bordering-county-pair fixed effects. Inference: p -values with clustering at the state level; parentheses: standard clustering at state level; brackets: wild cluster bootstrap by Roodman et al. (2019). Data sources: National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, affiliation).

Table A8: Effect on religious outcomes: Controls for other school reforms

	Compulsory schooling (1)	G8/G9 (2)	Philosophy (3)	Sexual education (4)	Political education (5)	All other school reforms (6)
Outcome: Religiosity	-0.073 (0.032) [0.047]	-0.073 (0.013) [0.040]	-0.087 (0.006) [0.027]	-0.075 (0.016) [0.057]	-0.066 (0.031) [0.113]	-0.079 (0.015) [0.042]
Observations	15,386	15,386	15,386	15,386	15,386	15,386
Outcome: Prayer	-0.052 (0.176) [0.200]	-0.047 (0.116) [0.173]	-0.053 (0.127) [0.193]	-0.048 (0.093) [0.123]	-0.032 (0.210) [0.267]	-0.058 (0.099) [0.103]
Observations	13,006	13,006	13,006	13,006	13,006	13,006
Outcome: Church-going	-0.065 (0.047) [0.035]	-0.061 (0.034) [0.032]	-0.091 (0.007) [0.018]	-0.055 (0.068) [0.087]	-0.050 (0.095) [0.010]	-0.066 (0.041) [0.032]
Observations	42,304	42,304	42,304	42,304	42,304	42,304
Outcome: Affiliation	-0.077 (0.016) [0.035]	-0.082 (0.009) [0.064]	-0.090 (0.013) [0.065]	-0.084 (0.005) [0.053]	-0.074 (0.006) [0.055]	-0.074 (0.006) [0.022]
Observations	45,286	45,286	45,286	45,286	45,286	45,286

Notes: Each cell reports the coefficient on reform treatment from a separate regression. Dependent variables indicated in table section headers (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. School reform controls are not available for Saarland. All regressions control for state and birth-year fixed effects, as well as gender, migration status, mother's education, father's education, survey and survey-year fixed effects. Inference: *p*-values with clustering at the state level; parentheses: standard clustering at state level; brackets: wild cluster bootstrap by Roodman et al. (2019). Data sources (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, religious affiliation); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity, prayer, church-going, religious affiliation); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (church-going, religious affiliation).

Table A9: Effects on religious outcomes: No control variables

	Religiosity (1)	Prayer (2)	Church-going (3)	Affiliation (4)
Reform	-0.073 (0.011) [0.032]	-0.038 (0.181) [0.217]	-0.068 (0.024) [0.026]	-0.079 (0.015) [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 (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, religious affiliation); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity, prayer, church-going, religious affiliation); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (church-going, religious affiliation).

Table A10: Effects on religious outcomes: Dosage treatment

	Religiosity (1)	Prayer (2)	Church-going (3)	Affiliation (4)
Reform	-0.092 (0.016) [0.053]	-0.047 (0.146) [0.156]	-0.074 (0.032) [0.042]	-0.097 (0.012) [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 (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, religious affiliation); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity, prayer, church-going, religious affiliation); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (church-going, religious affiliation).

Table A11: Effects on religious outcomes: Reform timing coded at secondary school entry

	Religiosity (1)	Prayer (2)	Church-going (3)	Affiliation (4)
Reform	-0.071 (0.040) [0.121]	-0.041 (0.154) [0.128]	-0.058 (0.035) [0.050]	-0.070 (0.035) [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 (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, religious affiliation); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity, prayer, church-going, religious affiliation); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (church-going, religious affiliation).

Table A12: Effects on outcome indices and adjustment for multiple inference

	Religious outcomes (1)	Labor-market outcomes (2)	Ethical- value outcomes (3)	Political-value outcome (4)	Educational outcomes (5)
Reform	-0.087 (0.002) [0.006]	0.058 (0.004) [0.009]	-0.008 (0.586) [0.672]	0.010 (0.637) [0.799]	-0.020 (0.439) [0.577]
<i>Bonferroni-adjusted p-values</i>	(0.010) [0.030]	(0.020) [0.045]	(1.000) [1.000]	(1.000) [1.000]	(1.000) [1.000]
State fixed effects	Yes	Yes	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Observations	53,044	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 3; (2) Table 7 (“part-time work” and “number of children” inverted); (3) Panel A of Table 6; (4) Panel B of Table 6 (“politics too complicated” inverted); (5) Table 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); Bonferroni family-wise error rate adjustment of *p*-values. Data sources (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religious outcomes, labor-market outcomes, ethical-value outcomes, political-value outcomes, educational outcomes); German General Social Stable Survey (ALLBUS) Cumulation 1980-2016 (religious outcomes, labor-market outcomes, ethical-value outcomes, political-value outcomes, educational outcomes); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (religious outcomes, labor-market outcomes, ethical-value outcomes, political-value outcomes, educational outcomes).

Table A13: Effects on religious outcomes: Excluding early reforming states

	Religiosity (1)	Prayer (2)	Church-going (3)	Affiliation (4)
Reform	-0.090 (0.014) [0.086]	-0.023 (0.608) [0.538]	-0.121 (0.062) [0.365]	-0.087 (0.037) [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 (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, religious affiliation); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity, prayer, church-going, religious affiliation); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (church-going, religious affiliation).

Table A14: Effects on religious outcomes: Separately by dataset (NEPS, ALLBUS, SOEP)

	Religiosity (1)	Prayer (2)	Church-going (3)	Affiliation (4)
Only NEPS data				
Reform	-0.090 (0.002) [0.012]	-0.083 (0.023) [0.034]	Not covered in NEPS	-0.071 (0.161) [0.321]
Observations	9,232	7,963		9,237
Only ALLBUS data				
Reform	-0.044 (0.326) [0.438]	0.018 (0.635) [0.677]	-0.062 (0.077) [0.175]	-0.111 (0.001) [0.026]
Observations	6,456	5,313	15,714	15,860
Only SOEP data				
Reform	Not covered in SOEP	Not covered in SOEP	-0.055 (0.065) [0.042]	-0.066 (0.058) [0.139]
Observations			27,062	20,828
State fixed effects	Yes	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

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 sources (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, religious affiliation); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity, prayer, church-going, religious affiliation); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (church-going, religious affiliation).

Table A15: Effects for a constant core sample of observations with data on all outcomes

	Religiosity	Prayer	Affiliation	Labor-force participation	Employment	Net Earnings	Working hours	Part-time work	Number of children
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Reform	-0.007 (0.636) <i>[0.553]</i>	-0.065 (0.034) <i>[0.063]</i>	-0.034 (0.249) <i>[0.479]</i>	0.013 (0.036) <i>[0.017]</i>	0.020 (0.053) <i>[0.033]</i>	0.039 (0.175) <i>[0.191]</i>	1.073 (0.064) <i>[0.138]</i>	-0.032 (0.100) <i>[0.201]</i>	-0.080 (0.083) <i>[0.195]</i>
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,303	8,303	8,303	8,303	8,303	8,303	8,303	8,303	8,303

Notes: Sample: Respondents without missing information on any of the core outcome variables indicated in column headers (see Table A2 for details about outcome variables). Controls: gender, migration status, mother's education, father's education, survey and survey-year fixed effects. Inference: *p*-values with clustering at the state level; parentheses: standard clustering at state level; brackets: wild cluster bootstrap by Roodman et al. (2019). Data sources (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, affiliation, labor-force-participation, employment, earnings, working hours, part-time work, number of children); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity, prayer, affiliation, labor-force-participation, employment, earnings, working hours, part-time work, number of children).

Table A16: Effects using the first available observation of each variable

	Religiosity (1)	Prayer (2)	Church-going (3)	Affiliation (4)
Reform	-0.056 (0.051) [0.130]	-0.019 (0.390) [0.425]	-0.079 (0.014) [0.018]	-0.071 (0.023) [0.121]
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), and coming from the first available wave of the longitudinal datasets (NEPS and SOEP). Controls: gender, migration status, mother's education, father's education, survey and survey-year fixed effects. Inference: *p*-values with clustering at the state level; parentheses: standard clustering at state level; brackets: wild cluster bootstrap by Roodman et al. (2019). Data sources (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, affiliation); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity, prayer, church-going, affiliation); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (church-going, affiliation).

Table A17: Effects using the average of the first and last available observations of each variable

	Religiosity (1)	Prayer (2)	Church-going (3)	Affiliation (4)
Reform	-0.064 (0.027) [0.071]	-0.031 (0.194) [0.259]	-0.070 (0.018) [0.019]	-0.074 (0.012) [0.078]
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) , and coming from the average of the first and last available wave of the longitudinal datasets (NEPS and SOEP). Controls: gender, migration status, mother's education, father's education, survey and survey-year fixed effects. Inference: *p*-values with clustering at the state level; parentheses: standard clustering at state level; brackets: wild cluster bootstrap by Roodman et al. (2019). Data sources (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, affiliation); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity, prayer, church-going, affiliation); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (church-going, affiliation).

Table A18: Effects on religious outcomes: Callaway and Sant'Anna estimator

	Religiosity		Prayer		Church-going		Affiliation	
	Without controls (1)	With controls (2)	Without controls (3)	With controls (4)	Without controls (5)	With controls (6)	Without controls (7)	With controls (8)
Reform	-0.086 <i>(0.061)</i>	-0.114 <i>(0.042)</i>	-0.109 <i>(0.136)</i>	-0.131 <i>(0.052)</i>	-0.097 <i>(0.112)</i>	-0.105 <i>(0.095)</i>	-0.078 <i>(0.000)</i>	-0.098 <i>(0.000)</i>
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 (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, religious affiliation); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity, prayer, church-going, religious affiliation); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (church-going, religious affiliation).

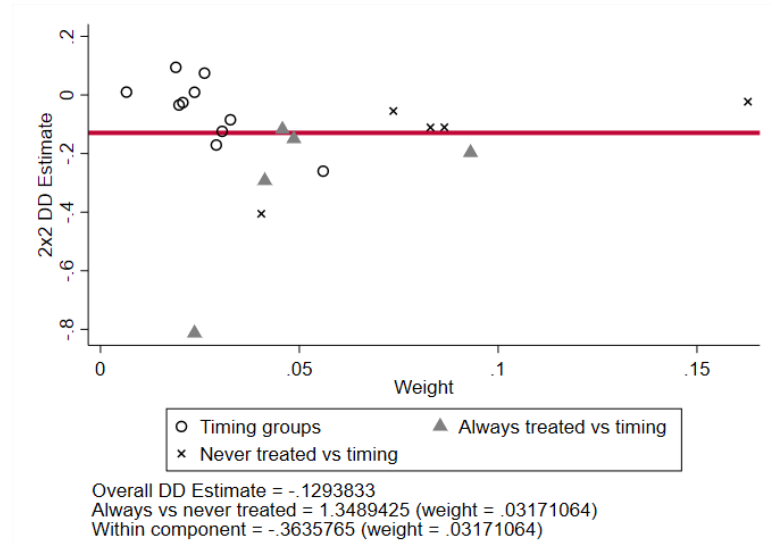
Table A19: Effects on religious outcomes: Excluding Berlin and Bremen

	Religiosity (1)	Prayer (2)	Church-going (3)	Affiliation (4)
Reform	-0.073 (0.028) [0.095]	-0.050 (0.084) [0.097]	-0.052 (0.059) [0.058]	-0.087 (0.007) [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 (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religiosity, prayer, religious affiliation); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity, prayer, church-going, religious affiliation); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (church-going, religious affiliation).

Table A20: Goodman-Bacon decomposition of effect on religiosity

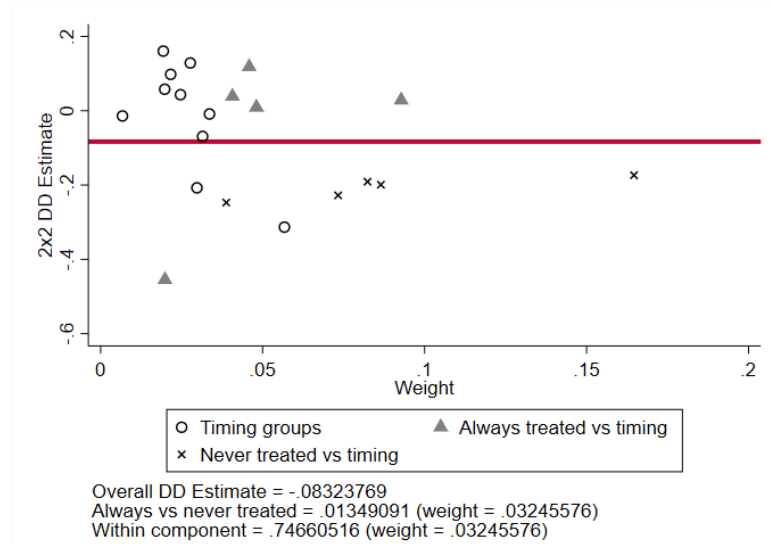
	Beta (1)	Total weight (2)
Overall	-0.129	-
Timing groups	-0.087	0.263
Always vs. timing	-0.247	0.252
Never vs. timing	-0.095	0.446
Always vs. never	1.349	0.006
Within	-0.364	0.032



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 (by outcomes): National Education Panel Study (NEPS) Cohort 6 (religiosity); German General Social Survey (ALLBUS) Cumulation 1980-2016 (religiosity); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (religiosity).

Table A21: Goodman-Bacon decomposition of effect on personal prayer

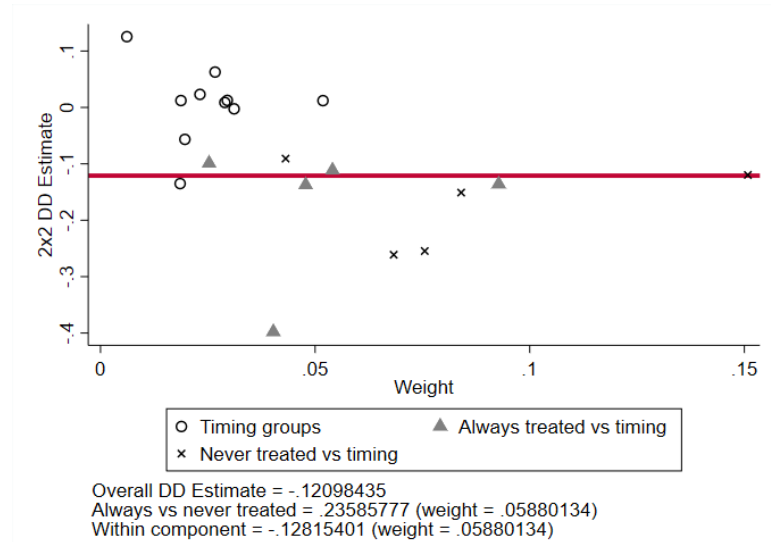
	Beta (1)	Total weight (2)
Overall	-0.083	-
Timing groups	-0.056	0.271
Always vs. timing	0.004	0.247
Never vs. timing	-0.196	0.445
Always vs. never	0.013	0.005
Within	0.747	0.032



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 (by outcomes): National Education Panel Study (NEPS) Cohort 6 (prayer); German General Social Survey (ALLBUS) Cumulation 1980-2016 (prayer); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (prayer).

Table A22: Goodman-Bacon decomposition of effect on church-going

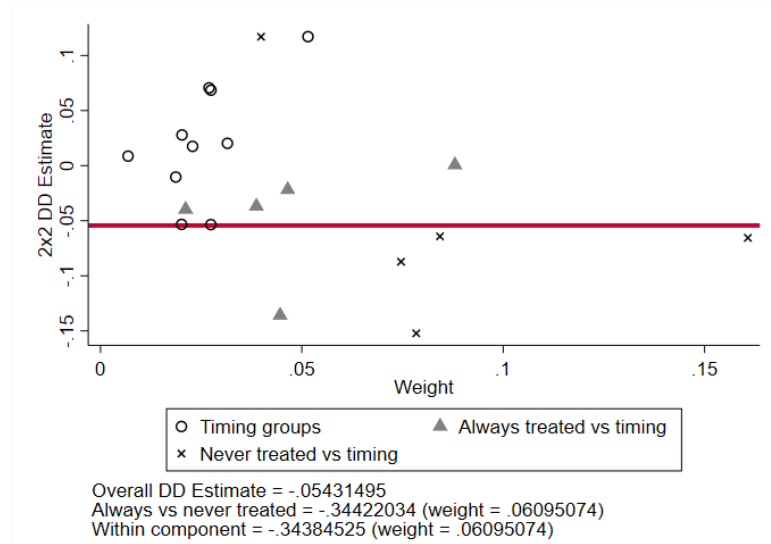
	Beta (1)	Total weight (2)
Overall	-0.121	-
Timing groups	0.003	0.254
Always vs. timing	-0.168	0.260
Never vs. timing	-0.169	0.421
Always vs. never	0.236	0.006
Within	-0.128	0.059



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 (by outcomes): National Education Panel Study (NEPS) Cohort 6 (church-going); German General Social Survey (ALLBUS) Cumulation 1980-2016 (church-going); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (church-going).

Table A23: Goodman-Bacon decomposition of effect on religious affiliation

	Beta (1)	Total weight (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 (by outcomes): National Education Panel Study (NEPS) Cohort 6 (affiliation); German General Social Survey (ALLBUS) Cumulation 1980-2016 (affiliation); German Socio-Economic Panel (SOEP) Core 1984-2017 (v.34) (affiliation).