Divine Policy: The Impact of Religion in Government

Supplemental Appendix

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A Examples

A.1 A sample of arguments behind the faith-based initiatives

The main arguments by the proponents of the initiatives was a) that faith-based organizations provide better for the needy than the state and b) the initiatives were seen as a way to secure religious freedom (Carlson-Thies, 1999; Chaves, 1999; Cnaan and Boddie, 2002; Formicola, Segers and Weber, 2003; Monsma, 2000; Sager, 2010; Sherwood, 2000). This section lists examples of these main arguments by leading figures behind the initiatives. We asked chatgpt 4 (November 2023) to list the fifteen main persons behind the charitable choice and the faith-based initiatives, including their role and main arguments. We then had an RA check whether she could find online support for the particular role and arguments. For three persons, the RA could not find support for the arguments listed by chatgpt, as they were mainly political or based on compassion.[1] Below we list the remaining thirteen persons and their arguments. All arguments fall within the two main groups.

President George W. Bush: President Bush was instrumental in advocating for and establishing the White House Office of Faith-Based and Community Initiatives, which oversaw the expansion of faith-based programs. **Main argument**: Bush argued that faith-based organizations could deliver social services more effectively than government due to their closeness to the community, compassion, and ability to inspire volunteerism (https://georgewbush-whitehouse.archives.gov/government/fbci/message.html).

John Ashcroft: As Attorney General, Ashcroft was responsible for enforcing the Charitable Choice laws and was a vocal supporter of expanding faith-based programs. **Main Argument**: Ashcroft maintained that faith-based organizations should not be excluded from competing for federal funds simply because of their religious character, advocating for equal treatment in federal funding (https://www.justice.gov/archive/ag/speeches/2003/whfaithbasedconference.htm).

Jim Towey: As the director of the Office of Faith-Based and Community Initiatives, Towey worked to implement the president's faith-based agenda. **Main Argument**: Towey believed that faith-based organizations have a unique ability to heal and transform lives, which is essential for effective social services (https://georgewbush-whitehouse.archives.gov/government/fbci/message.html).

Rick Santorum: As a Senator, Santorum was a leading advocate for the Charitable Choice provision of the 1996 Welfare Reform Act. **Main Argument**: Santorum promoted faith-based initiatives as a way to support communities in helping themselves, believing that local organizations could address needs more directly than distant government agencies (https://www.baptistpress.com/resource-library/news/coalition-to-seek-agreement-on-faith-based-initiatives/).

Stephen Goldsmith: As an advisor to President Bush and the chairman of the Corporation for National and Community Service, he was involved in promoting faith-based solutions to social problems. **Main Argument**: Goldsmith focused on the potential for faith-based and community initiatives to innovate and provide solutions to social problems more efficiently than government bureaucracies (https://www.pewresearch.org/religion/2008/09/23/stephen-goldsmith-previews-how-faith-based-initiatives-would-change-if-john-mccain-is-elected-president/).

[1] The fact that support was not found for these types of arguments is interesting in itself, as it might illustrate that the political and compassionate arguments are often more private and may differ from the public arguments. We have not found a way to disentangle these.

Jay Hein: Served as the director of the Office of Faith-Based and Community Initiatives from 2006 to 2008. **Main Argument**: Hein believed in the value of partnerships between government and faith-based organizations to tackle social issues, emphasizing their unique capabilities in community engagement and support. (https://www.presidency.ucsb.edu/documents/press-briefing-teleconference-with-jay-hein-director-the-faith-based-and-community).

Senator John DiIulio: DiIulio was the first director of the White House Office of Faith-Based and Community Initiatives. **Main Argument**: DiIulio saw faith-based organizations as having a comparative advantage in delivering certain types of social services due to their motivation, community presence, and trust they engender (https://georgewbush-whitehouse.archives.gov/news/releases/2001/03/20010307-11.html).

Tommy Thompson: As Secretary of Health and Human Services, Thompson implemented faith-based initiatives within his department. **Main Argument**: Thompson saw these initiatives as a way to innovate in the provision of social services and to bring new players into the field who could operate on a model of compassion and effectiveness (https://www.emory.edu/EMORY_REPORT/erarchive/2002/November/erNov.4/11_4_02grant.html).

Marvin Olasky: A former advisor to President Bush, Olasky is often credited with influencing Bush's approach to compassionate conservatism and support for faith-based initiatives. **Main Argument**: Olasky believed in the transformative power of faith-based charity, advocating for initiatives that could enable religious organizations to play a greater role in welfare provision (https://s3.us-east-1.amazonaws.com/world-website-storage/wng-prod/Tragedy-of-American-Compassion.pdf).

Don Eberly: He was involved in advising on the development of the faith-based initiatives as a civil society expert. **Main Argument**: Eberly's argument focused on strengthening civil society, including faith-based organizations, to improve social welfare and reduce dependency on government programs (https://politicalresearch.org/2002/07/01/tilting-faith-based-windmills).

Tony Evans: While not a political figure, as a prominent evangelical pastor, Evans has been influential in discussions on the role of the church in social services. **Main Argument**: As a pastor, Evans has spoken about the church's role in social change and the potential for faith-based organizations to provide comprehensive care that addresses both material and spiritual needs (https://georgewbush-whitehouse.archives.gov/news/releases/2003/10/text/20031029-10.html).

Rev. Luis Cortés Jr.: As the founder and CEO of Esperanza USA, the largest Hispanic faith-based evangelical network in the United States, Cortés has been a proponent of faith-based initiatives. **Main Argument**: Cortés advocated for these initiatives as a means to enable minority and faith-based organizations to contribute more effectively to community development and social welfare (https://www.esperanza.us/reverend-luis-cortes-jr/).

B Data Appendix

B.1 The faith-based initiatives

To measure the extent and spread of the faith-based initiatives, we use data from LexisNexis, collected by sociologist Sager (2010). Sager collected data on faith-based legislation passed during the period 1996-2009, which included key words "faith-based" or "Charitable Choice" (Sager (2010), p. 24). From this, Sager coded legislative acts by category and year of passage. We received our

version in April 2017. These are our main data on the treatment.

B.2 Alternative measures of the faith-based initiatives

B.2.1 Data on the faith-based institutions

The information on faith-based liaisons (FBL) and their budgets is based on interviews of officials in all states performed by Rebecca Sager in 2006 revealing whether the state had an FBL, an OFBCI, and what their budgets were at the time of interview and details on their operations (Sager, 2010). Furthermore, the interviews provided information on the timing of establishment of the FBL. The majority of states had an FBL and an OFBCI at the time of interview, but the timing of their implementation varies across states and a few did not have one yet. We exclude data without information on the year of establishment in these analyses. We further have data on the particular activities conducted by the faith-based liaisons, based on the interviews by (Sager, 2010, Appendix C).

B.2.2 Budgets and grants

The Charitable Choice provision initially encompassed the Temporary Assistance for Needy Families (TANF), the main federal welfare money which the state can spend on a variety of services. In 2000, Charitable Choice was included in the Substance Abuse and Mental Health Services Administration's (SAMHSA) block grant. Eventually, the provision was expanded to other programmes and block grants, like Welfare-to-Work and the Community Services Block Grant (CSBG) (Carlson-Thies, 2001). The Department of Health and Human Services was established in 2001 offering funding specifically to small faith- and community-based organizations through its Compassion Capital Fund (CCF) established in 2002 with an annual budget of $30 million in 2002, increasing to $57.8 million in 2007 (Kramer et al., 2005; Chaves and Wineburg, 2010). The CCF has awarded hundred of mini-grants (up to $50,000) directly to local faith-based and community organizations. The time-varying data on appropriations was gathered by Sager (2010) from the LexisNexis database. The dates are the dates of passage, not necessarily the dates of funding. Sager identified 16 states that were granted a total of 42 grants over the period 1998-2007, summing to $70 million. These data are used in columns (3) and (4) of Table 3.

B.2.3 Data on specific grants in 2006

We retrieved various measures of grant sizes of the different programs within the faith-based initiatives from the White House website `https://georgewbush-whitehouse.archives.gov/government/fbci/qr6.html` (thanks to an anonymous referee for directing our attention to this website). These data contain information on so-called faith-based and community initiative activities (aggregate numbers for the period 2005-2006). The website sometimes terms the latter "community-based organizations" and sometimes "secular nonprofits." We use "community-based organizations" throughout, as this is the most used term in the literature and since these organizations can be both secular and religious. At the end of the day, we are not sure they can be distinguished from each other, since most of the "community-based" organizations are likely to be religious organizations or at least work closely with religious organizations. For instance, Chaves and Wineburg (2010)[p 345] notes: "Although "faith-based and community organization" was the official rubric for the kind of organization targeted by the faith-based initiative, the activities outlined above make clear that initiative advocates, activists, and administrators envisioned congregations as a key type of faith-based and community organization. Indeed, one of the faithbased initiative's central, if

unsupported, assumptions was that there is untapped energy, creativity, and human resources lying dormant in congregations (and other community organizations) but available for mobilization by this initiative."

The White House website includes the total grants given to these faith-based and community activities, which encompass federal grant awards to faith-based and community-based organizations (aggregate numbers for the period 2002-2006) and presidential initiatives, which encompass primarily the Compassion Capital Fund and the Mentoring Children of Prisoners (aggregate numbers for the period 2003-2007).[2] For instance, as of 2006, the Mentoring Children of Prisoners had matched more than 70,000 children with parents behind bars with caring mentors (White House, 2008). These data include information for all 50 states, but only for one point in time for each state.

B.3 The GSS variables

The variables from the GSS used in the main analyses are presented below. The GSS variables used for the appendix tables are described in the respective table notes. When a variable is used as dependent variable, we restrict to a sample for which at least 10 persons answered the given question in one state and year.

Afterlife: GSS variable: postlife. Question: "Do you believe there is a life after death?" Answers: no, yes. We construct an indicator variable equal to one if the answer is yes, zero otherwise.

Against abortion: GSS variables: abdefect, abnomore, abhlth, abpoor, abrape, absingle, and abany. Question: "Please tell me whether or not you think it should be possible for a pregnant woman to obtain a legal abortion if the woman wants it for any of the following reasons: the child is likely to have a serious defect, the woman wants no more children, has serious health issues, is too poor to take care of the child, was raped, is unmarried, or for any other reason?" Answers: no, yes. We constructed a dummy variable equal to zero if yes, one if no.

Against homo: GSS variable: homosex. Question: "What about sexual relations between two adults of the same sex - do you think it is always wrong, almost always wrong, wrong only sometimes wrong, or not wrong at all?" We converted this into a categorical variable equal to one if the answer is always wrong, 0.66 if the answer is almost always wrong, 0.33 if the answer is wrong only sometimes, and zero if the answer is not wrong at all.

Against science: GSS variable: consci. Question: "I am going to name some institutions in this country. As far as the people running these institutions are concerned, would you say you have a great deal of confidence, only some confidence, or hardly any confidence at all in them? Scientific Community" We coded a dummy variable equal to one if the answer is hardly any confidence, zero otherwise.

Against women. Measures social views against working women. GSS variables: fework, fefam, fepol, and fepres. Question, fework: "Do you approve or disapprove of a married woman earning money in business or industry if she has a husband capable of supporting her?" Answers: disapprove, approve. Question, fefam: "It is much better for everyone involved if the man is the achiever outside the home and the woman takes care of the home and family". Answers: strongly agree, agree, disagree, disagree. Question, fepol: "Tell me if you agree or disagree with this statement: Most men are better suited emotionally for politics than are most women." Answers: disagree, agree. Question, fepres: "If your party nominated a woman for President, would you vote for her if she were qualified for the job?" Answers: yes, no. We coded a dummy variable equal to one if the respondent's answer disapproved of women for at least two of the questions,

[2]For a few states, the website also has information on the Prisoner Reentry and Access to Recovery Initiatives. Due to lack of coverage, we did not include these data in our analysis.

zero otherwise. The variable is missing if less than two of the questions was answered by the respondent.

Against science. GSS variable: consci, K. Question: "I am going to name some institutions in this country. As far as the people running these institutions are concerned, would you say you have a great deal of confidence, only some confidence, or hardly any confidence at all in them?" Option K: Scientific Community. Answers: "a great deal", "only some", "hardly any". We code a variable equal to one if the respondent answers "only some" or "hardly any", zero otherwise.[3]

Bible: GSS variable: bible. Question: "Which of these statements comes closest to describing your feelings about the Bible?" Answers: "The Bible is the actual word of God and is to be taken literally, word for word", "The Bible is the inspired word of God but not everything in it should be taken literally, word for word", "The Bible is an ancient book of fables, legends, history, and moral precepts recorded by men". From this, we construct two indicators: One is equal to one if respondents believe the Bible to be the literal or inspired word of God, zero otherwise, another is equal to one if respondents believe the Bible to be the literal word of God, zero otherwise.

Bible prayer in public schools: GSS variable: prayer. Question: "The United States Supreme Court has ruled that no state or local government may require the reading of the Lord's Prayer or Bible verses in public schools. What are your views on this–do you approve or disapprove of the court ruling?" We code a dummy equal to one if the respondent approves, zero if he/she disapproves.

Conservative: GSS variable: polviews. Question: "We hear a lot of talk these days about liberals and conservatives. I'm going to show you a seven-point scale on which the political views people might hold are arranged from extremely liberal–point 1–to extremely conservative–point 7. Where would you place yourself on this scale?" We code a dummy variable equal to one if answer is conservative or extremely conservative, zero otherwise.

Education: GSS variable: educ. Categorical variable based on the following range of questions: What is the highest grade in elementary school or high school that you finished and got credit for? If finished 9th-12th grade: Did you ever get a high school diploma or a GED certificate? Did you complete one or more years of college for credit–not including schooling such as business college, technical or vocational school? IF YES: How many years did you complete? Do you have any college degrees? (IF YES: What degree or degrees?) Answer: Integers between 0 - 20.

Evangelical: The same source as the Protestant categories.

Income: GSS variable: realinc. Family income in constant dollars (base = 1986).

Income $>$ 25%: GSS variable: realinc. We code a dummy equal to one if the respondent's family income equals or exceeds 11781 US$ (the 25th percentile in the US wide distribution of income).

Pray: GSS variable: pray. Question: "How often do you pray?" Answers: several times a day, once a day, several times a week, once a week, less than once a week, never. We reverse the GSS variable, so that higher values means more frequent prayer. We also recode it to make it take values between 0 and 1.

Protestant: GSS categories updated by Steensland et al. (2000).

Religious denomination: GSS variable: relig. Question: "What is your religious preference?" Main answers: Protestant, Catholic, Jewish, Some other religion, No religion.

Religious attendance: GSS variable: attend. Question: "How often do you attend religious services?" Respondents can answer never, less than once per year, about once or twice per year, several times a year, about once a month, two to three times a month, nearly every week, every

[3]We find it infeasible to rank the two response categories "hardly any" and "only some" and we chose to aggregate them into one category. The results are unchanged if we keep the ranking from the survey, categorizing "hardly any" confidence as having less confidence in science, compared to "only some".

week, or several times a week. The original variable assumes values between 0 and 8, which we recode to values between 0 and 1. Thus, the variable takes on values 0, 0.125, 0.25, ..., 1.

Strength of religious affiliation: GSS variable: reliten. Question: "Would you call yourself a strong (PREFERENCE NAMED IN RELIG) or a not very strong (PREFERENCE NAMED IN RELIG)" Respondents can answer somewhat strong, not very strong, somewhat strong, or no religion. We code a variable equal to one if the respondent answers that his/her religious affiliation is strong, 0.5 if it is not very strong or somewhat strong, and zero for respondents that answer "no religion".

Republican Variable name: partyid. Question: "Generally speaking, do you usually think of yourself as a Republican, Democrat, or Independent?" (converted to a dummy equal to one if Republican).

Voted Republican: Variable name: presXX. Question: "Did you vote for YY, WW, or ZZ?" We coded a dummy equal to one if the answer is the Republican candidate in each of the elections, zero otherwise.

B.4 Stacking the GSS data

In Section 3.2 we introduced the stacked difference-in-differences design we exploit to identifying the main results. The stacking process always requires to make a decision about a) the nature of the comparison group and b) the fixed time window of analysis. The "clean" comparison group can consist of a) units in states that are never-treated throughout the time window, b) units that are in states that are not-yet treated throughout the time window, but that can be treated after the last event time, and c) a mixture between those. The event study specifications throughout the analysis use stacked data where we compare treated and never-treated units considering a event time window of ten years before (or seven, in case of faith-based organizations) and seven years after the implementation of the faith-based initiatives within all treatment cohorts in the baseline (with a few exceptions related to robustness and data restrictions). We make this decision pondering on the limitations of the GSS data in a staggered treatment timing design and to avoid contamination of potential faith-based initiatives introduced in the control group but unobserved in the later event time periods. In this appendix, we also provide robustness of the main results also checking estimates comparing treated and not-yet treated units (Figure A11) - which are available for a limited time window - or a both never-treated and not-yet treated units (Figure A10). In this section, we want to further explain the empirical challenges posed by the General Social Survey (GSS) data in a staggered treatment adoption design. While we consider the above mentioned fixed time window, the shape of the event periods can be different across treatment cohorts when utilizing the waves from the GSS, due to the change in the surveying strategy of the GSS from yearly waves to even years waves starting from 1994. In Figure A2, we illustrate the sample composition from the GSS when considering the first introduction of the faith-based initiatives by treatment cohort, highlightling the states in each treatment cohort and the GSS waves covered with and without the data gaps. For instance, consider the first treatment cohort, which consists individuals in states receiving faith-based initiatives in 1997. Observing the GSS waves around the treatment timing, we are able to discover data for three years before treatment (in 1994), one year before treatment (in 1996), one year after treatment (in 1998), three years after treatment (in 2000), and so on, spotting only odd event time periods over the calendar period when the waves are observable only for even years in case of treatment timing in a odd year. On the other hand, if we focus on the second treatment cohort, shaped by individuals in states receiving faith-based initiatives in 1998, we find data for four years before treatment (in 1994), two years before treatment (in 1996), in the year of treatment, two years after treatment (in 2000)

etc., capturing only even time periods over the calendar period in case of treatment timing in a even year. To overcome this issue, we proceed as follows. After creating the stacked data over the fixed time window, we create bins of two event time periods, such that the set of events will be $T = \{\{-10,-9\},\ldots,\{-4,-3\},\{-2,-1\},\{0,1\},\{2,3\},\ldots,\{6,7\}\}$. Within each event time bin, time periods are mutually exclusive after 1994, such that $\tau = \{-2,-1\}$ in Equation (1) will include either $\tau = -1$ for the odd treatment cohorts or $\tau = -2$ for the even treatment cohorts, and the reference period will be stable within a treatment cohort. In this way, β_j in Equation (1) identifies the average of treatment effects across each treatment cohort within the event time bin j, while keeping the reference period within the treatment cohorts fixed as the latest period before the occurrence of the treatment for the treated group.[4] The final stacked structure using never-treated as comparison group is illustrated in Figure A3 for the dataset with church attendance as outcome variable, and additional covariates. From the tiles, it is possible to understand how within each treatment cohort the reference period is always the latest before the introduction of the faith-based initiatives, and that our event time bins are equivalent to the event time periods within a treatment cohort.

Concerning the stacked difference-in-differences design in Equation (1), we would like to a short note on the weighting scheme suggested by Wing, Freedman and Hollingsworth (2024) to identify the correct aggregate average treatment effect on the treated across treatment cohorts. While Goodman-Bacon (2021) and Gardner et al. (2024) discuss the robustness of stacked difference-in-differences to heterogeneity across groups and periods in the case of staggered treatment timing, which is equivalent to other novel alternative approaches (Callaway and Sant'Anna, 2021; Sant'Anna and Zhao, 2020; De Chaisemartin and d'Haultfoeuille, 2020; Sun and Abraham, 2021; Borusyak, Jaravel and Spiess, 2021; Wooldridge, 2023; Gardner et al., 2024), Wing, Freedman and Hollingsworth (2024) show that, decomposing the stacked fixed effect coefficient, it corresponds to a convex combination of sub-experiment specific group-time ATTs only in the case of constant treatment share over time, failing to identify a coherent aggregation of the underlying casual effect among all treatment cohorts. The we apply the proposed weights in Wing, Freedman and Hollingsworth (2024) to address this specific issue. However, the stacked event study regression proposed in Wing, Freedman and Hollingsworth (2024) is slightly different from the specification we employ, which is more similar to those used in Jeffers (2024). Wing, Freedman and Hollingsworth (2024) argue that the complexity of our fixed effects specification compared to their "event study" specification is not always desiderable, as it incorporates high dimensional fixed effects which can be more dependent on modelling assumptions and in a "too conservative" manner. We prefer the fixed effect specification over their "event study" specification to model our heterogeneity analysis with religious denominations. In Table A2, we compare estimates for the main results between using the "event study" specification proposed by Wing, Freedman and Hollingsworth (2024) and our main specification using fixed effects both for the difference-in-differences and the triple differences specifications, showing that the difference between estimates is negligible.

Extending to the triple difference specification in Equation (3), Strezhnev (2023) shows that it is possible to address a potential bias similar to the one shown in Callaway and Sant'Anna (2021) in triple difference designs with a staggered treatment timing using direct imputation of the counterfactual *à la* Borusyak, Jaravel and Spiess (2021), where the estimated counterfactual is given a set of unit-stratum-time period fixed effects. We differ from this method by estimating

[4]Within a treatment cohort, event time bins might include two time periods in pre-treatment bins identified before 1994, and in that case fixed effects for the event time bins by treatment cohort do not perfectly overlap with calendar years fixed effects by treatment cohort. In unreported results, we also absorb for the latter, with negligible changes in the coefficients of interest. Weights proposed by Wing, Freedman and Hollingsworth (2024) are calculated based on the event time bins when it comes to the GSS sample.

triple difference within the single sub-experiments using a similar set of fixed effects but exploiting the data structure described above.

B.5 Congregations and membership

The state level data on religious congregations and memberships are provided by the Association of Religion Data Archives (ARDA). We use the longitudinal data set covering the years 1980, 1990, 2000, and 2010, constructed by Grammich et al. (2019). The data covers 302 religious groups, and includes information on total population, religious tradition, number of adherents, and number of congregations. We use the reltrad specification to attach groups to religious denominations.

B.6 Nonprofit organizations

The data on nonprofit organizations is from the National Center for Charitable Statistics (NCCS). The dataset includes digitized information filed to the Internal Revenue Service (IRS) by tax-exempt nonprofit organizations. We use the NCCS Core files, which are based on the Internal Revenue Service's annual Return Transaction Files (RTF).

The dataset contains the almost-universe of non profit organizations in the US, except those that are not required to report receipts to keep the tax-exemption status, which can be small congregations, extremely small organizations with less than 5,000 USD of revenues, and organizations with less than 25,000 USD in gross receipts (a limit increased to 50,000 USD in 2010) which are not asked to fill the Form 990 or it's simplified version, but rather notify their existence (now through replying to an electronic postcard) to maintain their tax-exempted status.[5] The latter organizations are instead included in the IRS Business Master Files, which are updated with an irregular frequency throughout the years tracking the stock of active nonprofit organizations, regardless their filing obligations. NCSS also excludes a small number of other organizations, such as foreign organizations or those that are generally considered part of the government. We geocoded almost all organizations using a combination of their address (for the most part), ZIP code and previously noted county codes by the NCCS. The precision of the geocoding procedure might vary, *e.g.* some of the addresses are P.O. Boxes, but in any case we are able to locate nonprofit organizations to a state and, in most of the cases, coordinates level. We regard an organization as "alive" or in a year t if we have a date of filing for that year from the IRS. The fiscal year and the filing year do not always coincide, and we keep the latter for the reference of our panel of nonprofits. It is possible that some organizations are missing in some year, due to falling out of filing requirements, delays in reporting to the IRS, etc.: we are able to fill the gaps in most of the cases, but we use as baseline sample only organizations for which we have full information in the core data, and for the main sample we restrict them to 501(c)(3) operating public charities, which means excluding private foundations and other types of nonprofit that can play a supporting role for a foundation. Specifically, we focus on nonprofits with LEVEL1 equal to "PC" (public charity) and LEVEL" equal to "O" (operating public charity).

The NCCS uses the National Taxonomy of Exempt Entities (NTEE) system to classify the nonprofit organizations on a scale of 26 major categoriees from A to Z, which includes categories such as "Arts, Culture, and Humanities", "Education", and "Religion-Related". The NCCS tries to keep the latest information available of the NTEE code for each organization in a string variable called `nteefinal`, and the information barely changes over time, especially when it comes to the macro groups. We identify religious organizations if they have the code of the"Religion-Related"

[5] As opposed to the BMF files, this criteria of a minimum revenue in the Core files reduces the risk of including organizations that no longer exist, cf. "Guide to Using NCCS Data", downloaded Nov 2022.

macro group. These codes are mutually exclusive, and sometimes do not fully reflect the core activities of an organization. For this reason, we also categorize an organization as faith-based if its' name contains religiously associated words. We identify these words from the excess frequency of the words in the names of organizations categorized as religious based on the NTEE categorization.

We categorize an organization as faith-based if its' name contains religiously associated words. We identify these words from the excess frequency of the words in the names of organizations categorized as religious based on the NTEE categorization. For instance, 27% of the organizations classified as religious according to the NTEE classification contain the words "ministry" or " ministries", while only 1.1% of the organizations that are not classified as religious according to the NTEE classification contain such words. The words "ministry" or "ministries" therefore obtain an excess frequency of 25.9%. We therefore define these words as being religiously associated. We evaluate the words with excess frequencies down to 0.01. When defining whether the organization is faith-based from its name, we first pre-process and tokenize the names, and then we check using the dictionary of religious words found with the excess frequency. For instance, the most frequent words in organizations that we categorize as religious are Christian, Ministry, Saint, and Church (cf. Figure 6). The list of top-50 words categorized as religious are provided in Figure A16. The results are robust to excluding the ten most frequent religious terms one at a time and altogether.

B.7 Additional state level variables

Public spending per capita: Covers direct welfare expenditure per capita at the state level. Source: US Census Bureau, Annual Survey of State Government Finances and Census of Governments.

Gay marriage laws: In the period from 1998 to 2009 29 states changed their constitutions in order to ban gay marriages. In 2015 the U.S. supreme court ruled all the state bans unconstitutional. Before the constitutional bans several states had statutes defining marriage as between a man and a woman. The variable on restricting gay marriages is a dummy equal to one in the year the state implements a constitutional ban on gay marriage and thereafter, zero otherwise. Data downloaded from https://www.pewforum.org/2009/07/09/state-policies-on-same-sex-marriage/.

Gender gaps: Data from IPUMS (https://cps.ipums.org/cps/). We computed gender gaps in education as the difference between the share of men and women who obtained 4 years of college. Higher scores indicate a larger educated share among men, compared to women. We compute gender gaps in wages as the difference in average wages between men and women. Higher scores indicate larger average wages among men, compared to women. For robustness, we show these gaps as percent of the value for men.

C Additional Tables and Figures

Figure A1: Year of the first faith-based initiative

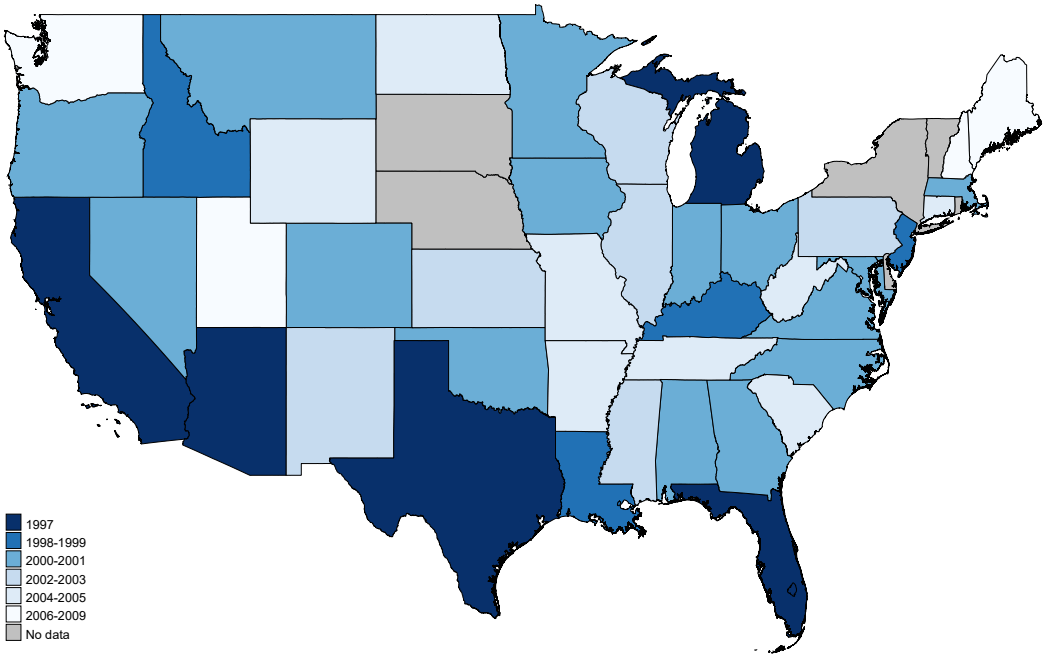
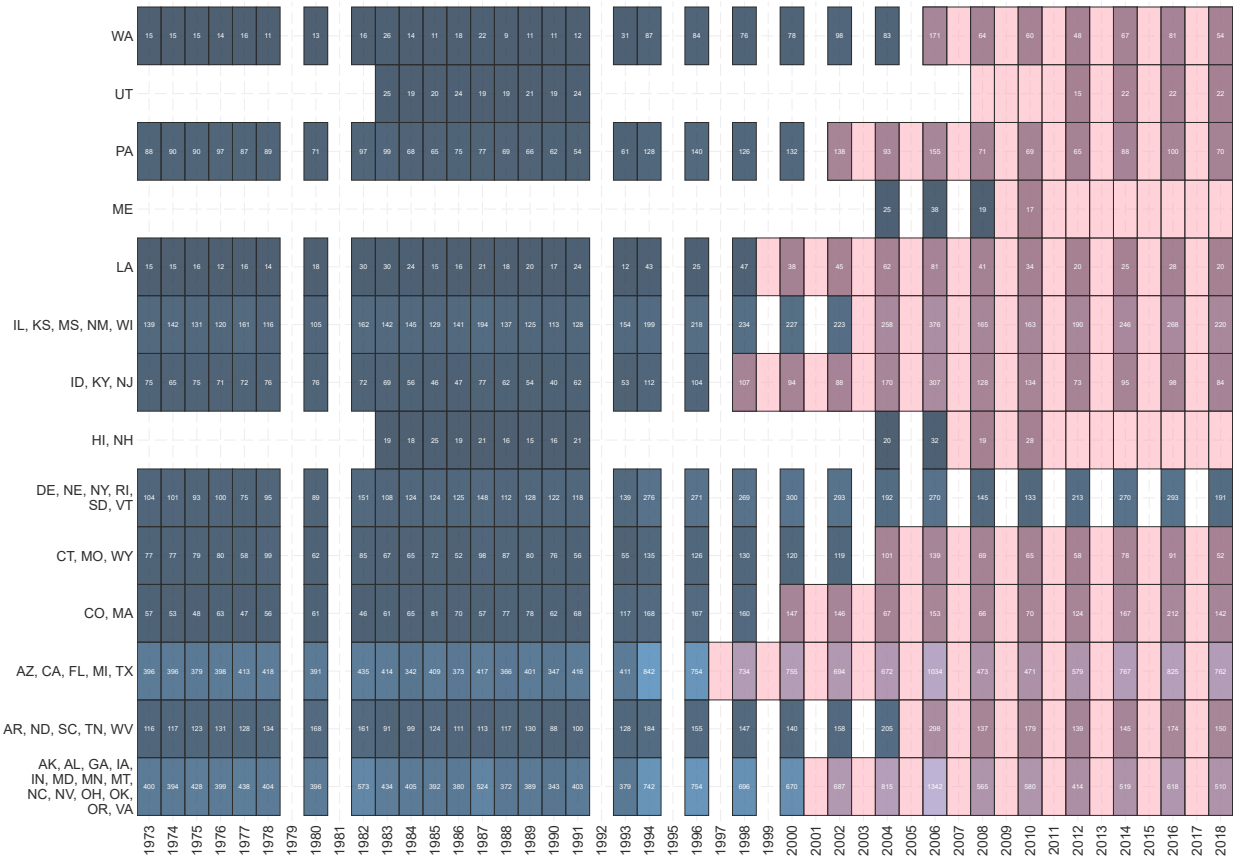
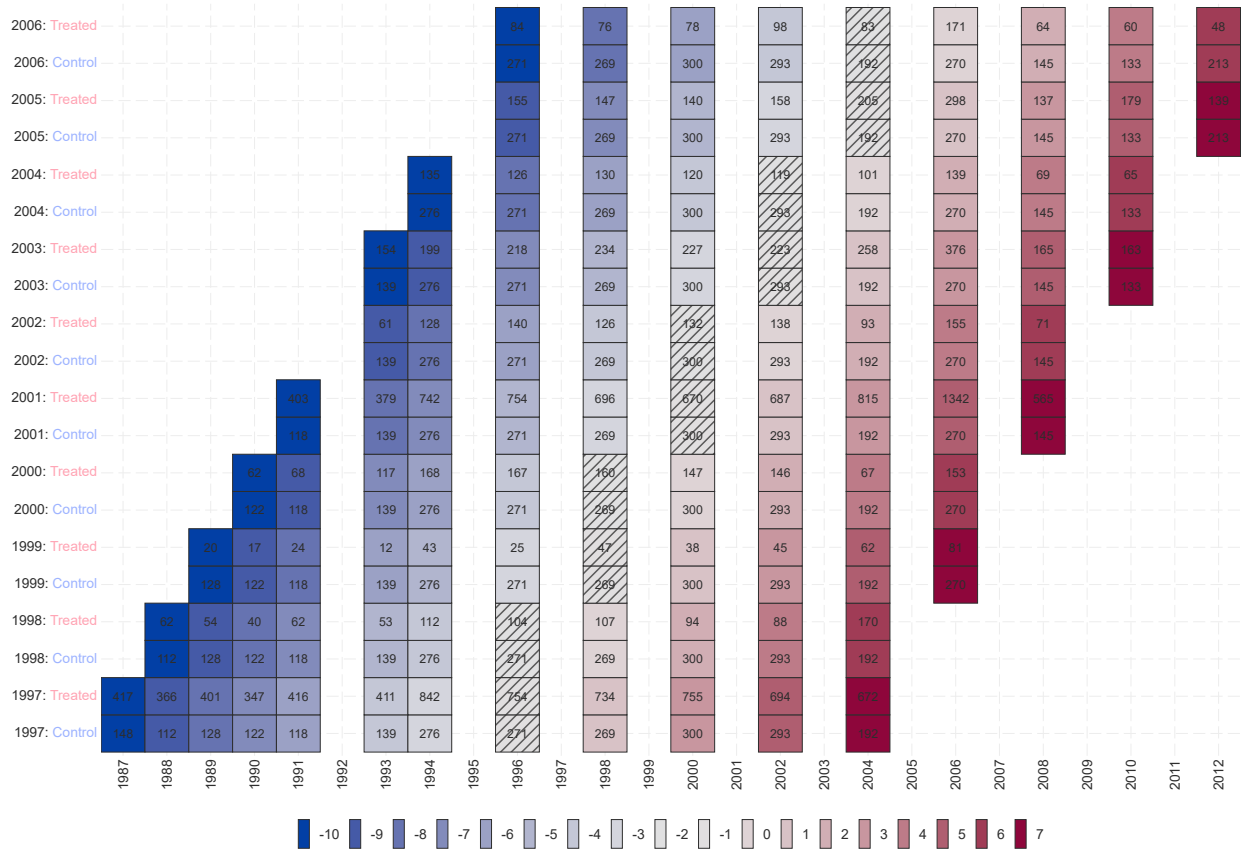


Figure A2: Sample composition of the General Social Survey (GSS): any type of faith-based initiatives



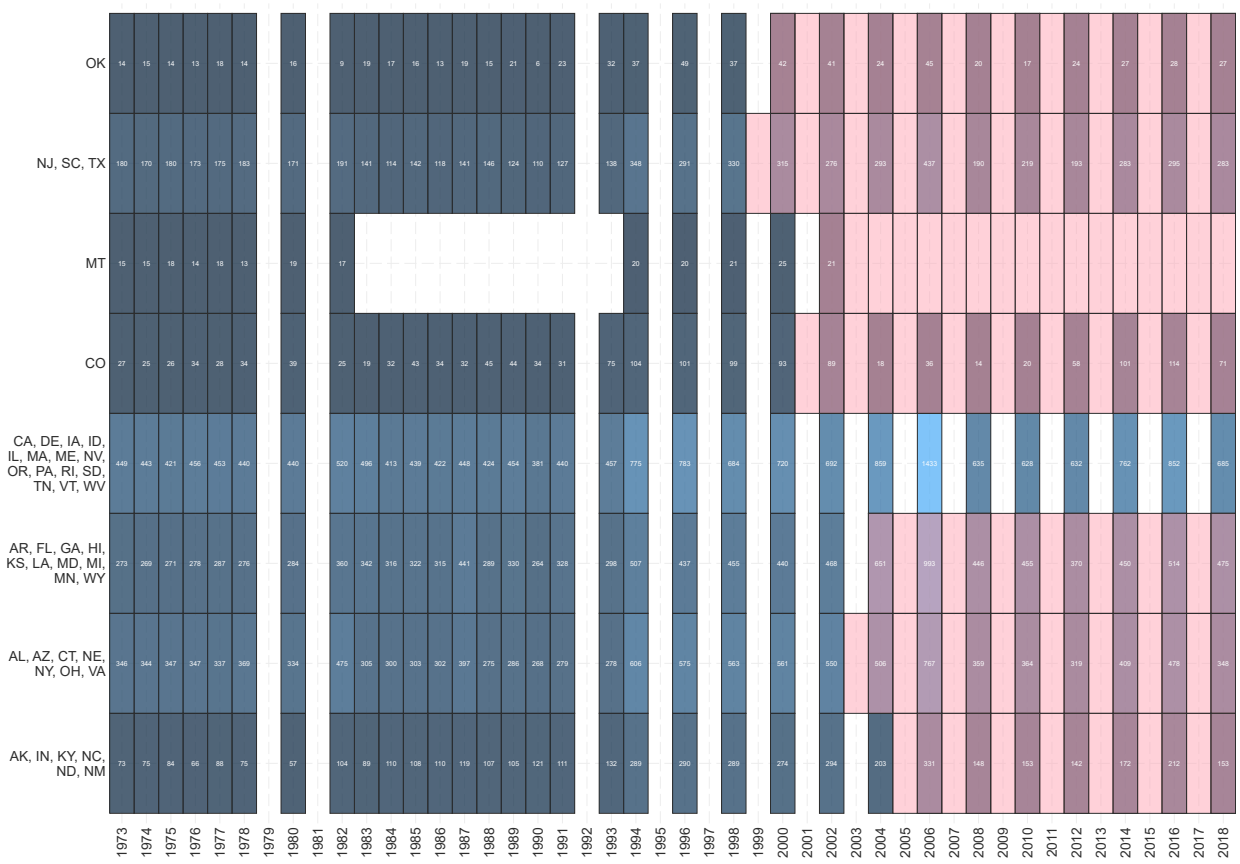
Notes: This figure illustrates the composition of the baseline sample of the General Social Survey (GSS) from the earliest wave available (1973) to the latest (2018), matched with the faith-based initiatives in Sager (2010). Each horizontal group defines a treatment cohort of the indicated states (or never treated), and each tile represents a GSS wave for that treatment cohort, with the frequency of the total number of individuals within that wave indicated in the tile. The shaded pink area indicates whether the year for that treatment cohort is treated or not. There are two main points worth discussing. First, from 1994 onward, the GSS waves switch from a yearly frequency to a two-year frequency, together with some gaps in 1979, 1981 and 1992, which poses a challenge to the identification of the treatment effects with staggered treatment of the faith-based initiatives. Second, some treatment cohorts have a limited coverage in terms of respondents in the GSS waves, both before and after treatment, which limits the scope of our analysis in the states included in those treatment cohorts.

Figure A3: Illustration of the baseline stacked data for the General Social Survey (GSS)



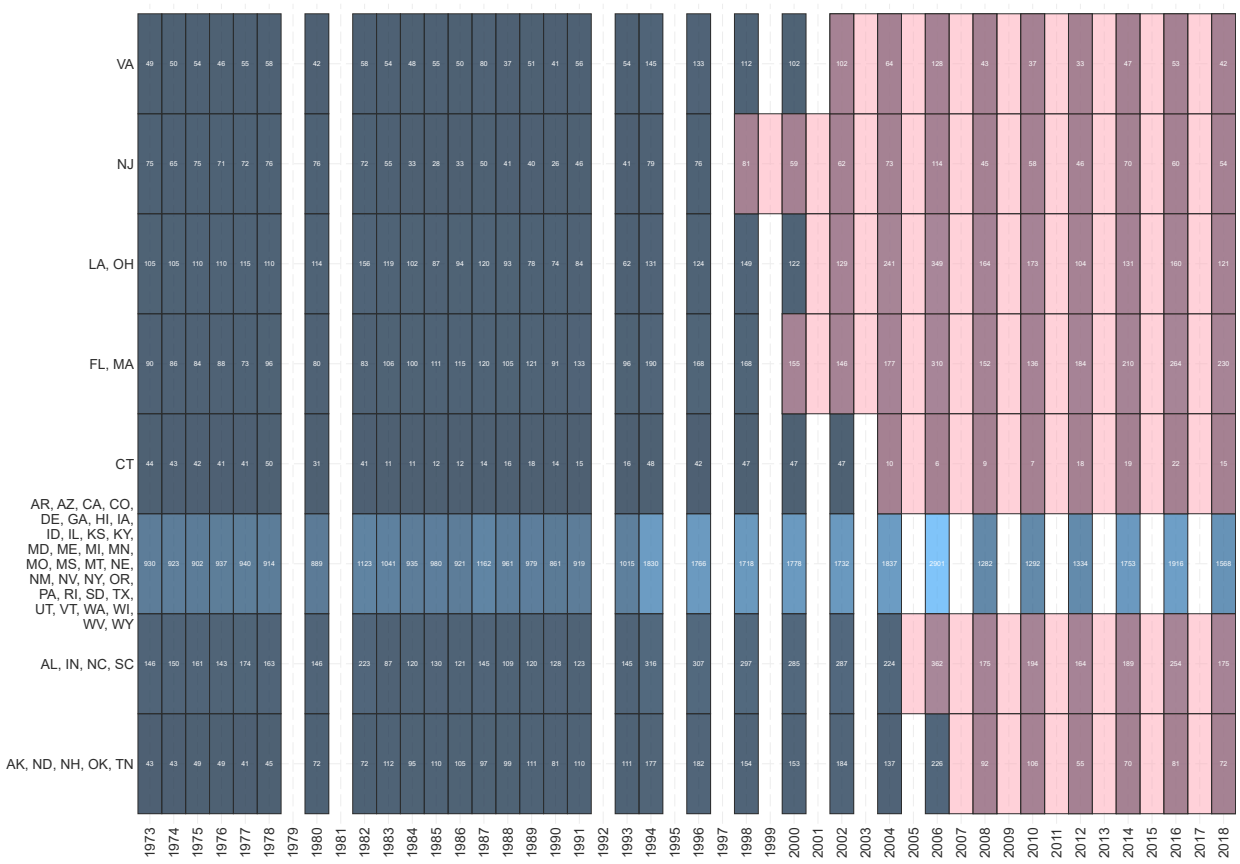
Notes: This figure illustrates the structure of the General Social Survey (GSS) data for measuring church attendance after the stacking procedure described in Section 3.2, which is our baseline sample trimming the data on a fixed time window of ten years before and seven years after the introduction of faith-based initiatives. On the y-axis, we indicate the treatment cohort with its year of treatment, and we differentiate between individuals in treated states within each cohort (pink text) from individuals in states that are never-treated throughout the event time window (blue text). In the legend, we indicate the corresponding event time to each calendar year, which comes in handy to understand the aggregation of event time periods described in Section 3.4, which is mutually exclusive within a treatment cohort. Hence, each tile represents the number of respondents paired with its event time, calendar year, treatment cohort and treatmnet status.

Figure A4: Sample composition of the General Social Survey (GSS): faith-based liaisons as treatment



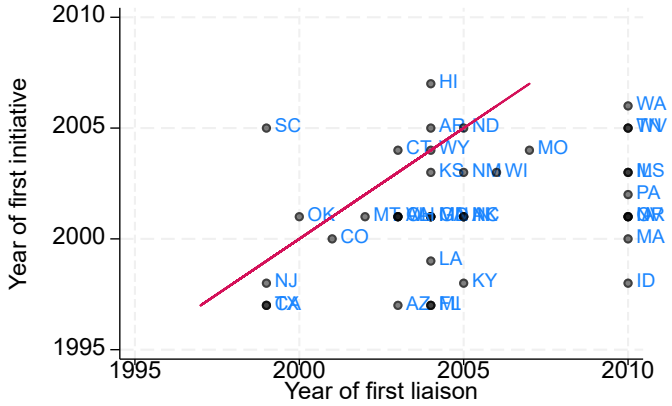
Notes: This figure illustrates the composition of the baseline sample of the General Social Survey (GSS) from the earliest wave available (1973) to the latest (2018), matched with the faith-based liaisons described in Section 2.3. Each horizontal group defines a treatment cohort of the indicated states (or never treated), which means that those states were implementing a faith-based liaison at that indicated same time, and each tile represents a GSS wave for that treatment cohort, with the frequency of the total number of individuals within that wave indicated in the tile. The shaded pink area indicates whether the year for that treatment cohort is treated or not. We remind at the discussion in Section 3.4 and Figure A2 to highlight the data limitations. We removed all the potential liaisons established after the interviews in Sager (2010).

Figure A5: Sample composition of the General Social Survey (GSS): federal appropriation bills



Notes: In this figure, we illustrate the composition of the baseline sample of the General Social Survey (GSS) from the earliest wave available (1973) to the latest (2018), matched with the timing of the federal appropriation bills. Each horizontal group defines a treatment cohort of the indicated states (or never treated), which means that those states were implementing a faith-based liaison at that indicated same time, and each tile represents a GSS wave for that treatment cohort, with the frequency of the total number of individuals within that wave indicated in the tile. The shaded pink area indicates whether the year for that treatment cohort is treated or not. We remind at the discussion in Section 3.4 and Figure A2 to highlight the data limitations. We do not know about federal appropriation bills approved after 2007 as in Sager (2010).

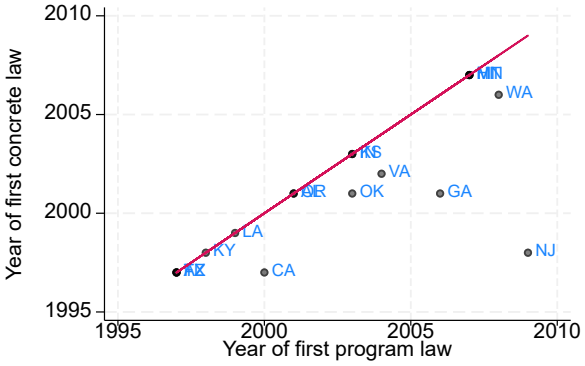
Figure A6: Timing of faith-based initiatives versus timing of liaisons



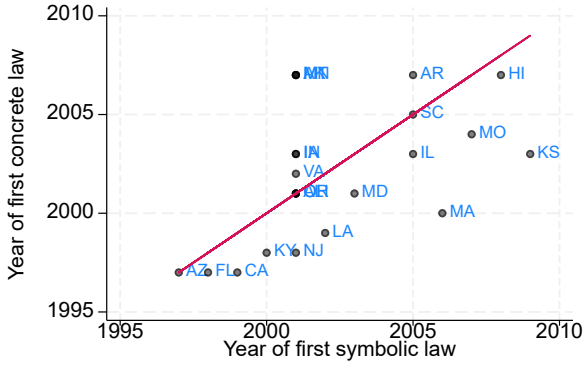
Notes: The year of the implementation of the first faith-based initiative (our baseline measure) and the timing of implementing the first liaison in a state. The line is the 45 degree line. The liaison data stops in 2006. The states that implemented a faith-based initiative in 2006 or before, but who had not implemented a liaison by 2006, are listed to the right in the figure as if they implemented a liaison in 2010. This is simply to illustrate the timing differences and is not used in the analysis.
Result: The vast majority of states implemented a faith-based initiative before they had a faith-based liaison.

Figure A7: The timing of the three types of initiatives

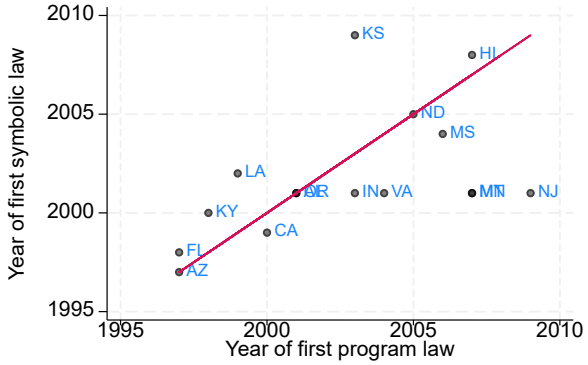
(a) Concrete laws vs. program laws



(b) Concrete laws vs. symbolic laws



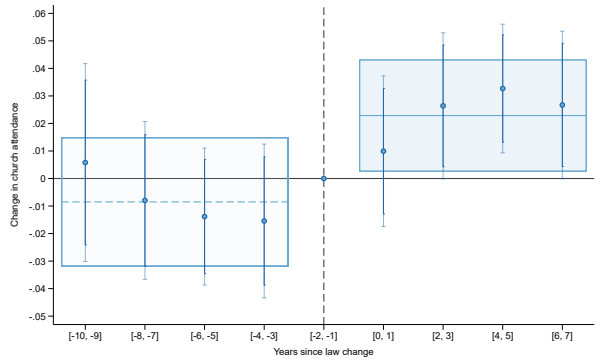
(c) Symbolic laws vs. program laws



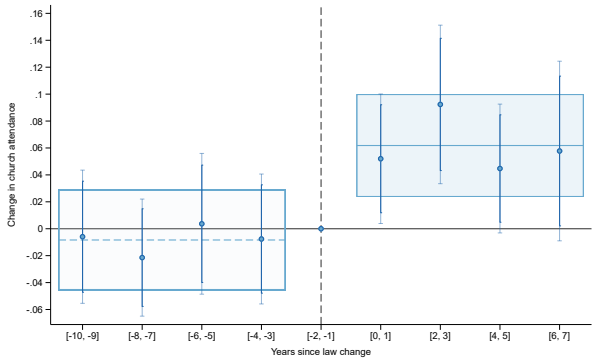
Notes: The correlations between the year of first law of each of the types: concrete, program, or symbolic. The figures include the 45-degree line of equality between the years.
Result: All program laws and most symbolic laws are implemented after a concrete law. There is no pattern in implementation timing between symbolic and program laws.

Figure A8: The impact of the faith-based initiatives on church attendance: estimates without Wing, Freedman and Hollingsworth (2024) weights

(a) DD estimates with only GSS sampling weights



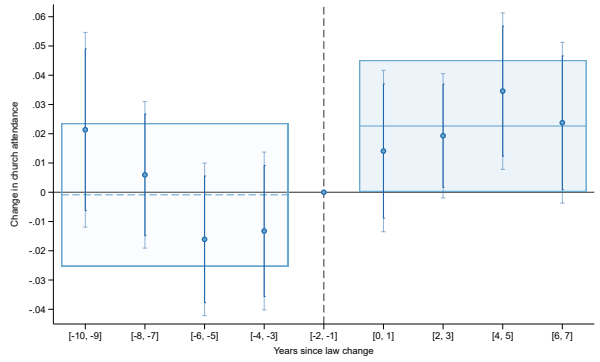
(b) DDD estimates with only GSS sampling weights



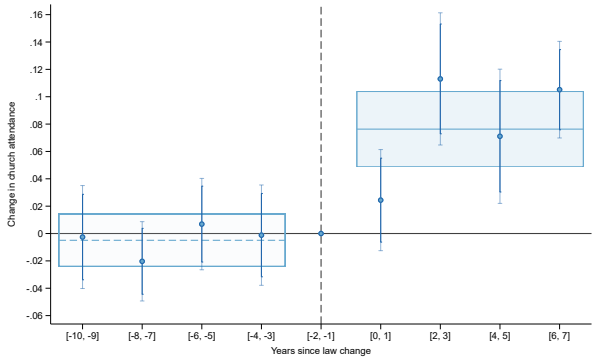
Notes: This figure shows estimates for the same specification and sample of Figure 4, with the single difference that in this case we do not apply the weighting scheme described in Wing, Freedman and Hollingsworth (2024). Regressions are still estimated using weighted least squares, but only with the individual sampling weights from the GSS. In panel (a), we estimate the difference-in-differences in Equation (1) and its version with aggregate interactions for the pre-treatment and post-treatment periods, relative to the last year before the introduction of the faith-based initiatives. Aggregate post-treatment effect: 0.023 (*std. err.* = 0.010, t = 2.282). In panel (b), we present estimates of the triple difference in Equation (1) and its version with aggregate interactions for the pre-treatment and post-treatment periods, relative to the last year before the introduction of the faith-based initiatives. Aggregate post-treatment effect: 0.062 (*std. err.* = 0.019, t = 3.278).

Figure A9: The impact of the faith-based initiatives on church attendance: excluding incomplete waves

(a) DD estimates



(b) DDD estimates

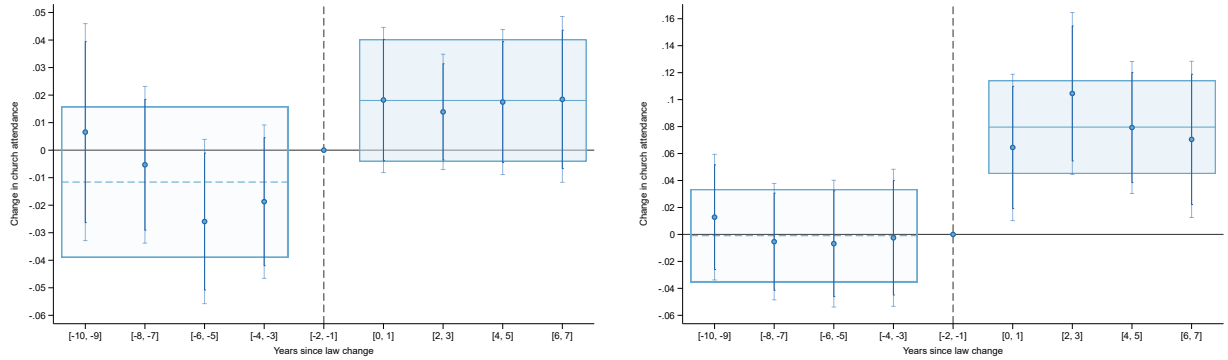


Notes: In this figure, we present estimates analogous to Figure 4 with the difference that, building the stacked sample, we exclude respondents in states with "incomplete" waves – that we cannot fully follow for the entire pre-treatment and post-treatment period within a treatment cohort – or with less than 10 respondents, for a total of 39,299 individual-by-state-by-stack observations. Average church attendance: 0.423; Average share of protestants: 0.417; Change in church attendance post faith-based initiatives: -0.018. Aggregate post-treatment effect: 0.076 (*std. err.* = 0.013, t = 5.664).

Figure A10: The impact of the faith-based initiatives on church attendance: never-treated and not-yet treated comparison groups

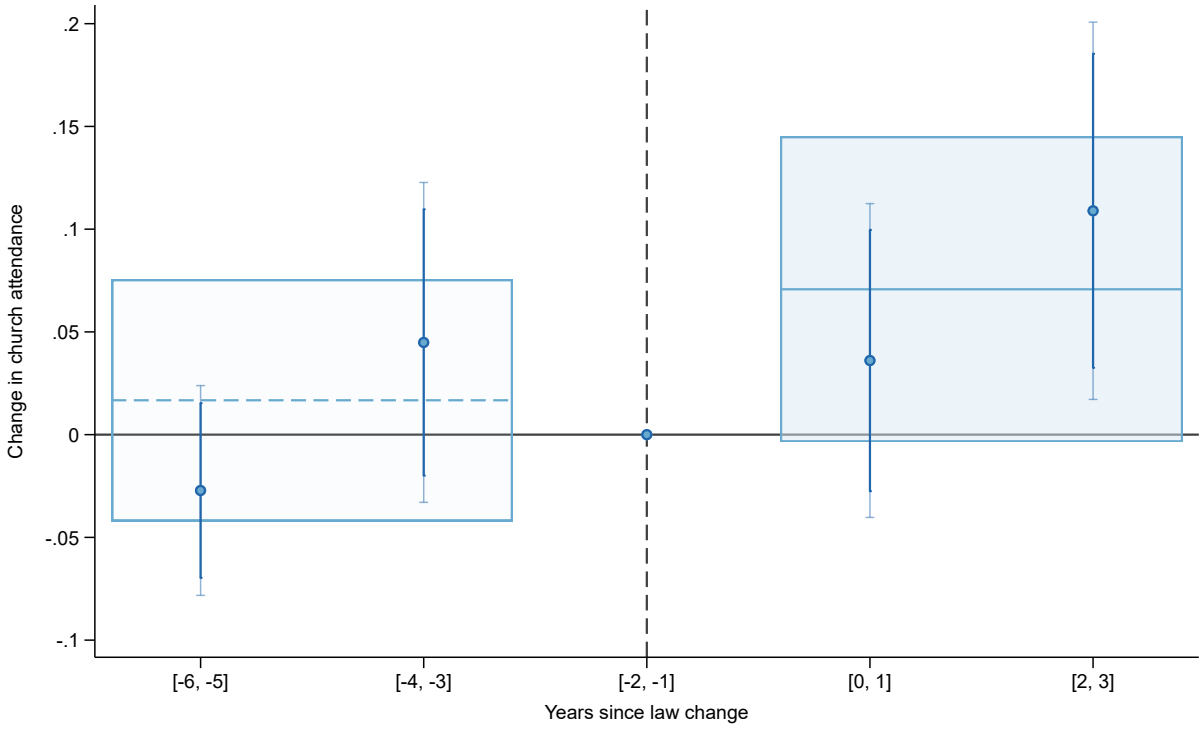
(a) DD estimates with mixed control group

(b) DDD estimates with mixed control group



Notes: In this figure, we show estimates analogous to Figure 4 but changing the comparison group. We build the sample of interest in the same way as described in Section 3.2, but in this case we consider as comparison group respondents that are both in states that do not introduce faith-based initiatives at all and states that have not introduced the faith-based initiatives, but they will in the future. For example, adopting the same time window used in the never-treated scenario, we compare individuals in states treated in 1997 with individuals in states that are never-treated and individuals in states that will adopt faith-based initiatives but only after 2004 — that is, respondents in states with faith-based initiatives introduced in 2005 or 2006. Not-yet treated units for a post-treatment period of seven years are available only for respondents that are treated in 1997 or 1998 due to data limitations in the GSS, as visible in Figure A2. Observations: 47,554; Average church attendance: 0.429; Average share of protestants: 0.445; Change in church attendance post faith-based initiatives: -0.020. Aggregate post-treatment effect: 0.080 (*std. err.* = 0.017, t = 4.659).

Figure A11: The impact of the faith-based initiatives on church attendance: DDD estimates using not-yet treated respondents as comparison group

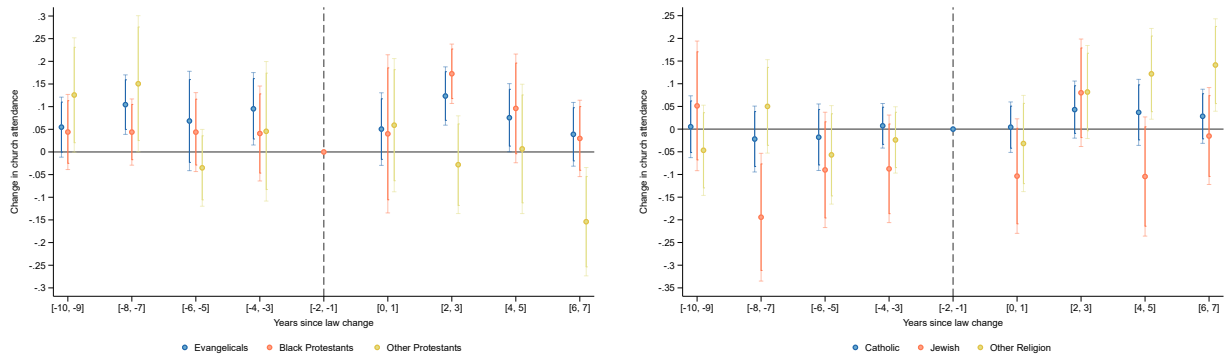


Notes: This figure is analogous to Figure 4b with a different sample composition. For estimating Equation (3) and (4) here we compare respondents identified as protestants in states adopting faith-based initiatives with those in states that will adopt faith-based initiatives four years after, relative to the same average change for non-protestants. Considering a maximum of three years after the introduction of the faith-based initiatives in the treated states, we are able to have a not-yet treated comparison group for each of the treatment cohorts. For example, church attendance of respondents in states receiving the faith-based initiatives in 1997 will be compared to the church attendance of respondents in states receiving the faith-based initiatives from 2001 onward, and so on. Concerning the change in the data structure in the GSS highlighted in Figure A2, a treatment cohort is formed by respondents in treated states treated in odd (even) years compared to respondents in not-yet treated states that will be also treated in a odd (even) yea: in this way, we are able to obtain the same reference event time before the faith-based initiatives within a cohort. Every other detail applies from Figure 4b. Observations: 21,240; Average church attendance: 0.447; Average share of protestants: 0.564; Change in church attendance post faith-based initiatives: 0.001. Aggregate post-treatment effect: 0.071 (*std. err.* = 0.037, t = 1.937).

Figure A12: The impact of the faith-based initiatives on church attendance: within-Protestant and within non-Protestant comparisons

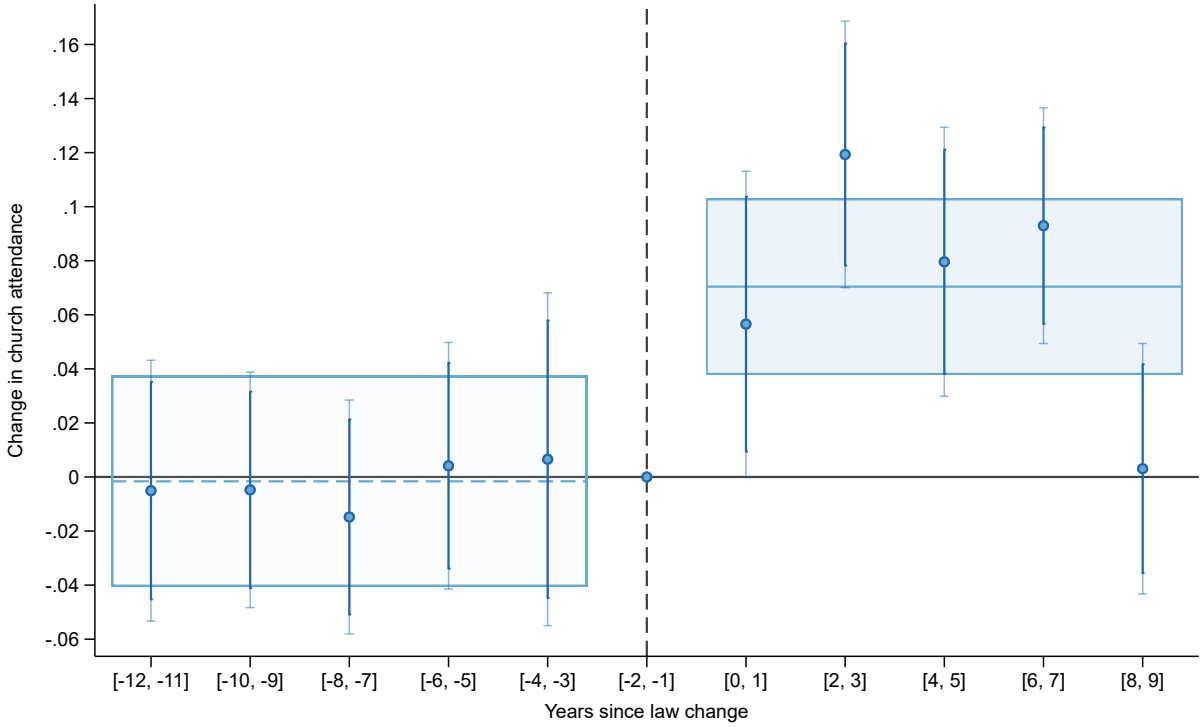
(a) Within Protestants

(b) Within non-Protestants



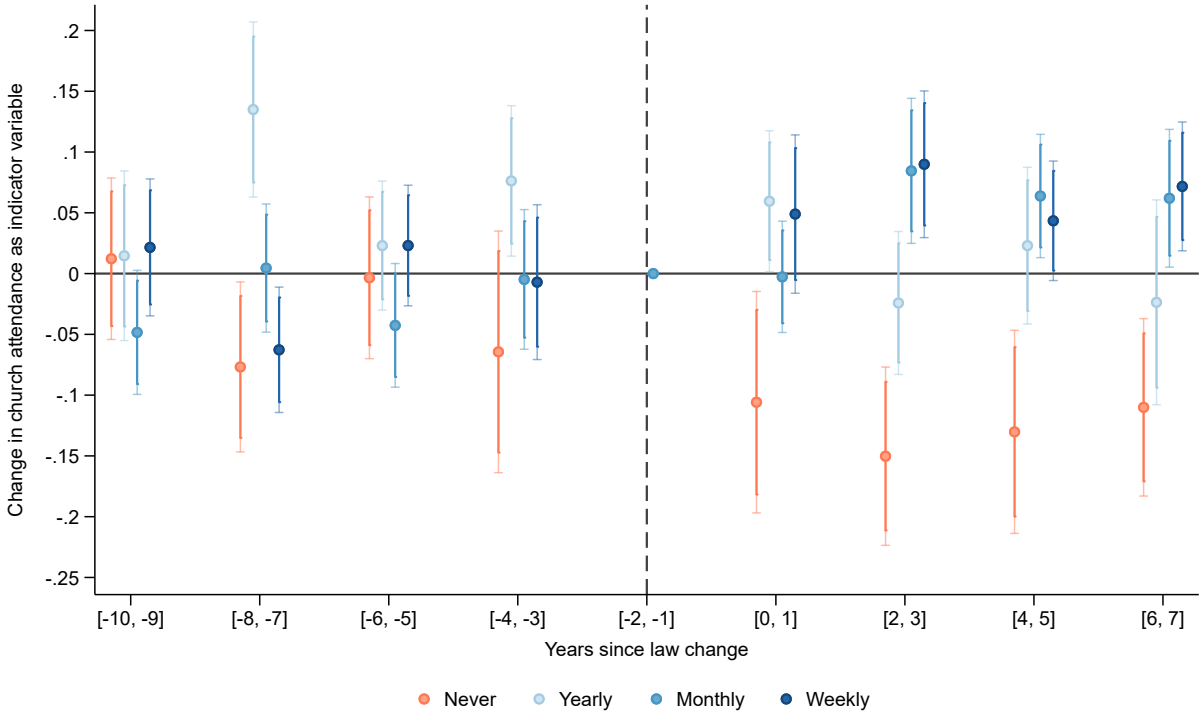
Notes: This figure shows results of a version of the triple difference specification in Equation (3) where we estimate separate coefficients for each religious denomination with respect to a baseline group, using the same sample of Figure 4b. In panel (a), we restrict the sample to non-Protestants respondents, where $r = \{Mainline, Evangelical, Black\ Protestant, Other\ Protestant\}$, and estimates are compared to the differential change in church attendance for mainline protestants in treated states versus never-treated states, relative to the year before the introduction of the faith-based initiatives. Likewise, in panel (b) we restrict the sample to non-Protestants respondents, where $r = \{Catholic, Jewish, Other\ Religion, No\ Denomination\}$, and we compare estimates for the interaction of each denomination to the differential change in church attendance for non-religious. Coefficients estimates on the event time interactions are plotted as dots with their 90% (95%) confidence intervals drawn as thick (thin) vertical lines. Regressions are estimated using weighted least squares with sampling weights and the weighting scheme described in Wing, Freedman and Hollingsworth (2024), and standard errors are clustered at state level.
Result: The rise in church attendance for Protestants is driven by the Evangelicals and Black Protestants. The absence of effects for non-Protestants is rather homogeneous across groups.

Figure A13: The impact of the faith-based initiatives on church attendance: extended event time window



Notes: This figure depicts the same estimates of Figure 4b, with the only difference of using an extended time window. We find estimates of Equation 3 and the aggregate interaction in Equation 4 on individual-level data built using a fixed time window of twelve years before and nine years after the implementation of the faith-based initiatives, and individuals from never-treated states as comparison group. It means that the earliest calendar year in the sample is 1985 for the first treatment cohort and the latest calendar year is 2014 for the last treatment cohort. Observations: 54,916; Average church attendance: 0.423; Average share of protestants: 0.431; Change in church attendance post faith-based initiatives: -0.023. Aggregate post-treatment effect: 0.070 (*std. err.* = 0.016, t = 4.378).

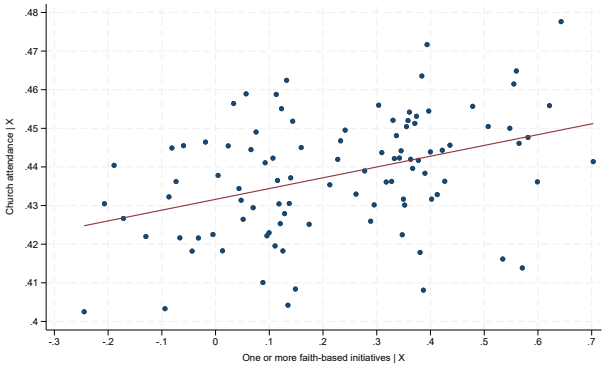
Figure A14: DDD event study church attendance in dummies



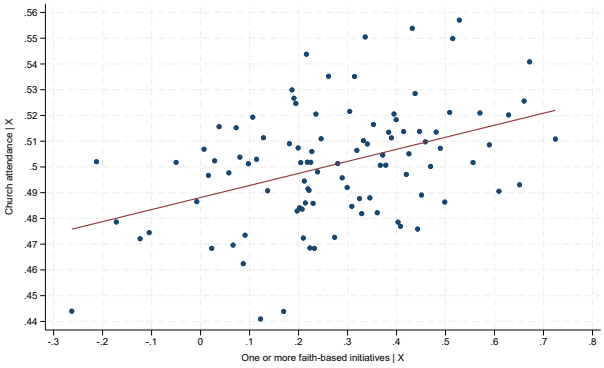
Notes: The figure describes results from Equation (3), with the baseline sample applied in Figure 4b. Every coefficients with the same colour are estimates from a separate regression where we use as outcome an indicator variable equal to one if the individual goes to religious services weekly (in blue), monthly (in light blue), once per year (in lighter blue), or not at all (orange), and zero otherwise. Aggregate post treatment effects for protestants: *Never attending church*: -0.124^{***} (*std. err.* = 0.031, $t = -3.99$); *Attending church yearly*: 0.009 (*std. err.* = 0.028, $t = 0.31$); *Attending church monthly*: 0.052^{**} (*std. err.* = 0.019, $t = 2.69$); *Attending church weekly*: 0.063^{***} (*std. err.* = 0.019, $t = 3.35$). Coefficients estimates on the event time interactions are plotted as dots with their 90% (95%) confidence intervals drawn as thick (thin) vertical lines. Regressions are estimated using weighted least squares with sampling weights and the weighting scheme described in Wing, Freedman and Hollingsworth (2024), and standard errors are clustered at state level.

Figure A15: Binned added-variable plots of the impact of the faith-based initiatives on church attendance

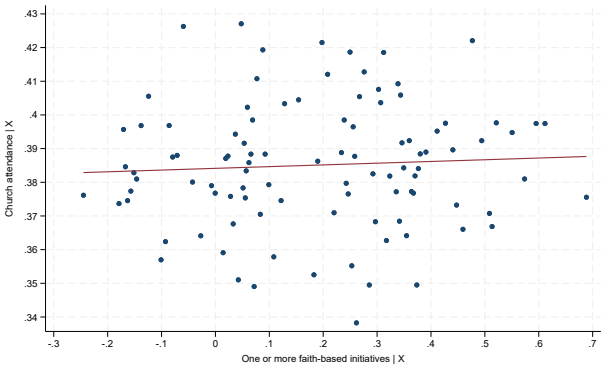
(a) Overall sample



(b) Protestants

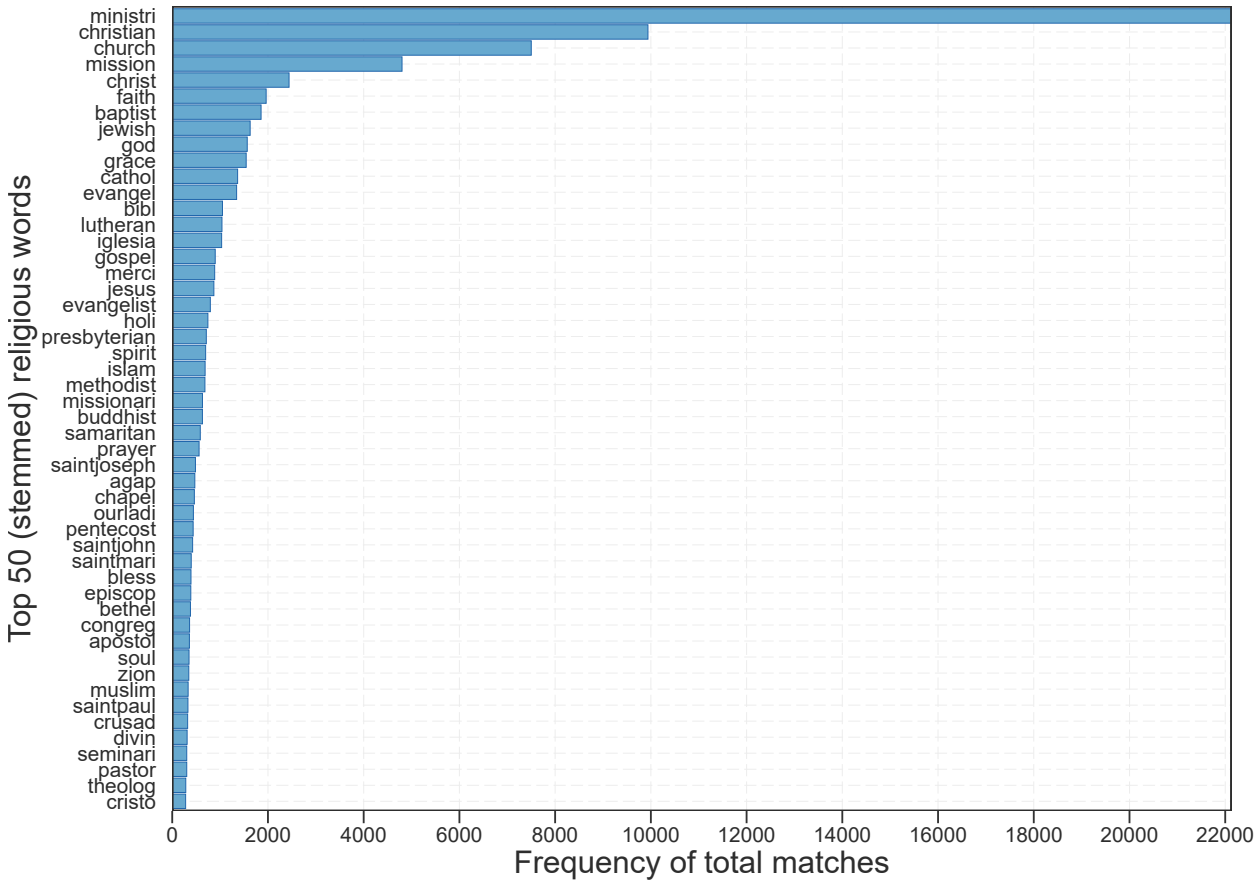


(c) Non-protestants



Notes: Added variables plots of the main results where the observations are binned into 100 equally sized bins. The dependent variable is church attendance. The treatment is an indicator variable if the individual is located in a state that adopted the faith-based initiatives, and zero otherwise, interacted with an indicator variable equal to one in the time periods from when the first faith-based initiative takes place. Included controls are state by cohort and event by cohort fixed effects, and controls for age, gender, marital status and an indicator variable for protestant denomination in the case of the overall sample, mimicking the specification in Equation (1). Panel (a) shows the composition for the overall sample, whereas panel (b) and (c) show estimates for protestants and non-protestants, respectively. The sample is the same used for the estimates in Figure 4.
Result: Estimates seem to generalize to the full sample and are not driven by groups of observations.

Figure A16: Top 50 religious terms in the names of nonprofit organizations

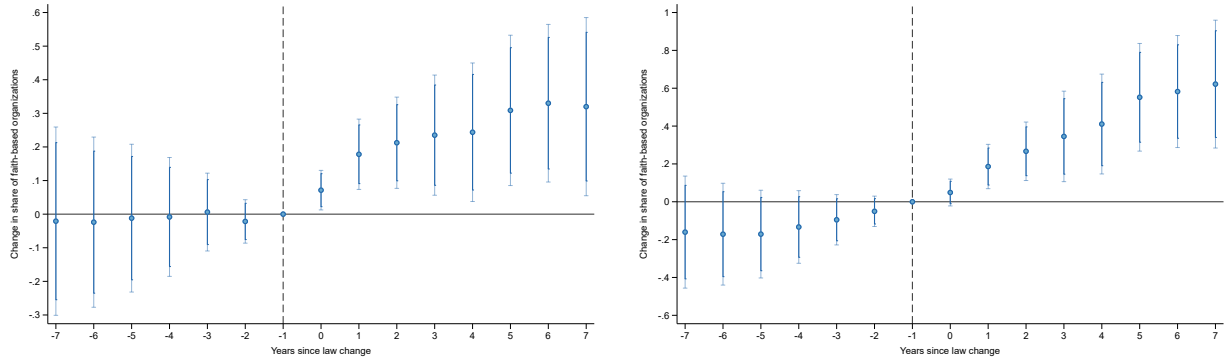


Notes: This bar graph shows the ranked frequencies (the number of times a word appears in the corpus) of the top fifty religious terms in the unique names of the nonprofit organizations in the collected sample after pre-processing, tokenization and stemming of the strings, similar to Figure 6.

Figure A17: The impact of faith-based initiatives on the presence of faith-based organizations: separate religiosity outcomes based on NTEE codes or names with religious words

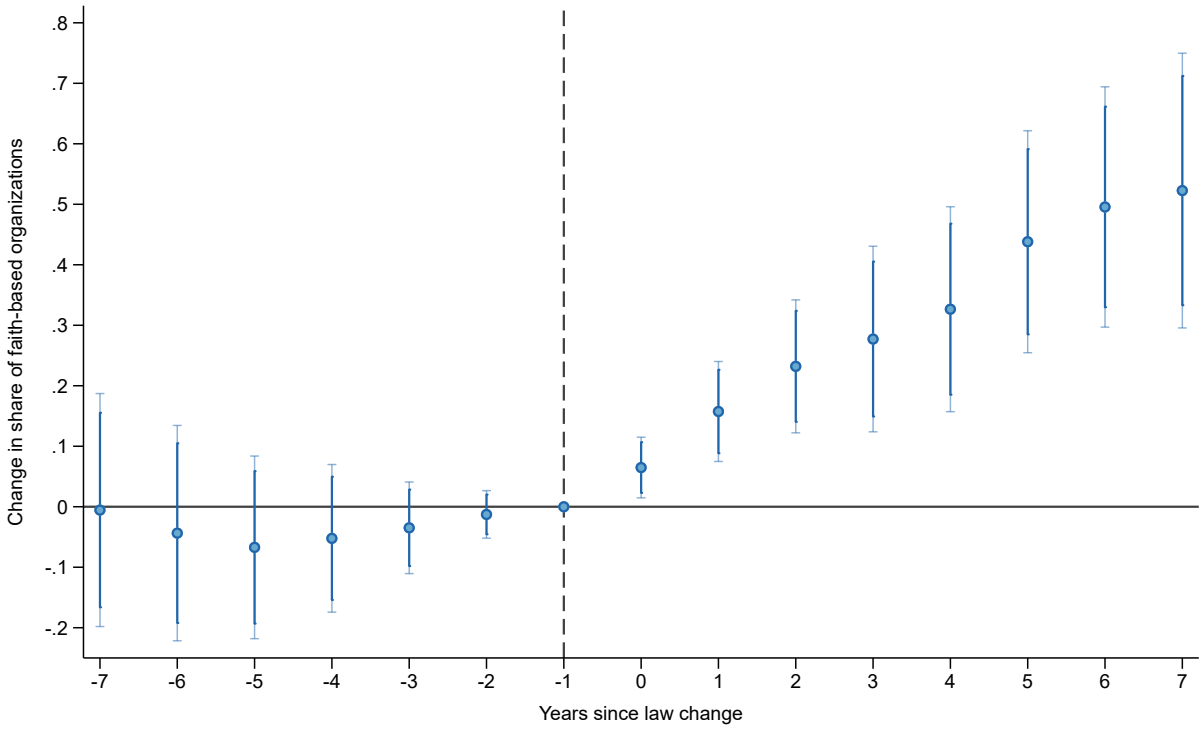
(a) Religion-related NTEE categories

(b) Religious words in name



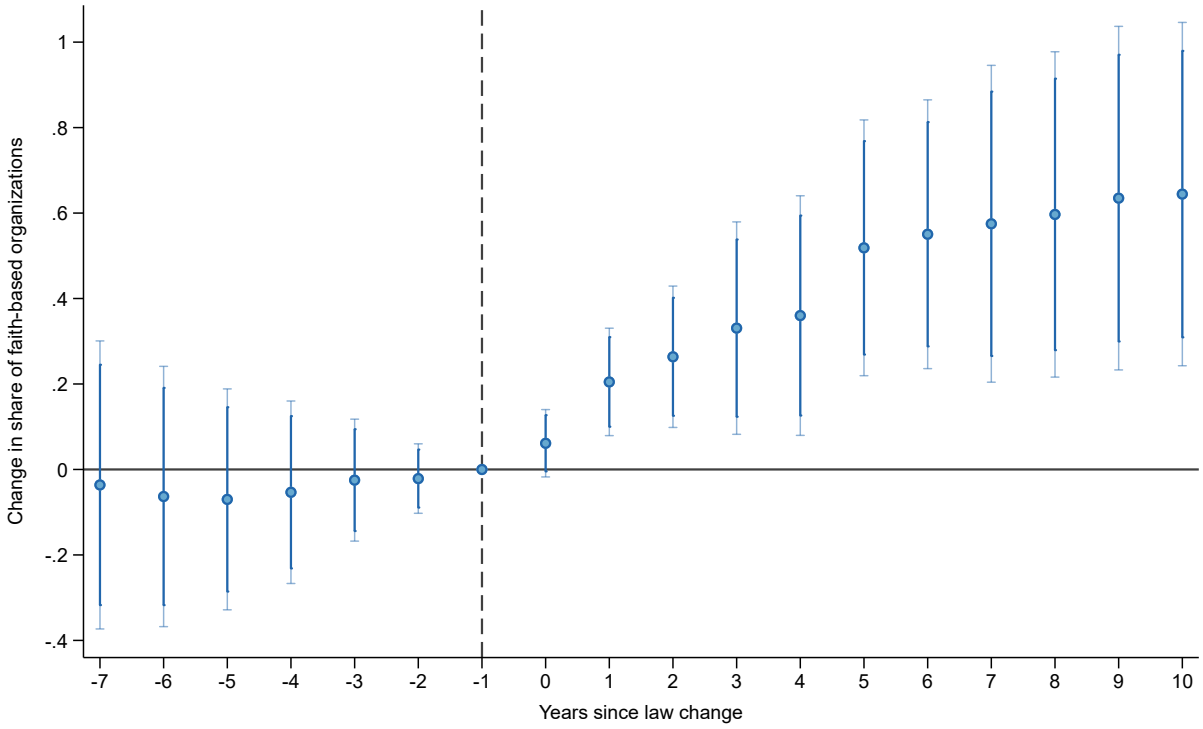
Notes: This figure is analogous to Figure 7, with the only difference that we decompose the previously shown outcome in two separate outcomes for labelling a nonprofit organization as religious. Panel (a) presents the difference-in-differences estimates using as outcome an indicator variable equal to one hundred if the nonprofit organization is a faith-based organization based on its religion-related activities indicated by the NTEE nomenclature, and zero otherwise. Average share of faith-based organizations: 5.660%. Change in the share of faith-based organizations post faith-based initiatives: 1.787%. Aggregate post-treatment effect: 0.237 (*std. err.* = 0.084, t = 2.835). Instead, panel (b) shows the difference-in-differences estimates using as outcome an indicator variable equal to one hundred if the nonprofit organization contains religious words in its name, and zero otherwise. We include fixed effects for the number of tokens in the organization's name by treatment cohort. Average share of faith-based organizations: 8.925. Change in the share of faith-based organizations post faith-based initiatives: 0.655. Aggregate post-treatment effect: 0.377 (*std. err.* = 0.105, t = 3.573).

Figure A18: The impact of faith-based initiatives on the presence of faith-based organizations: Full sample of nonprofits



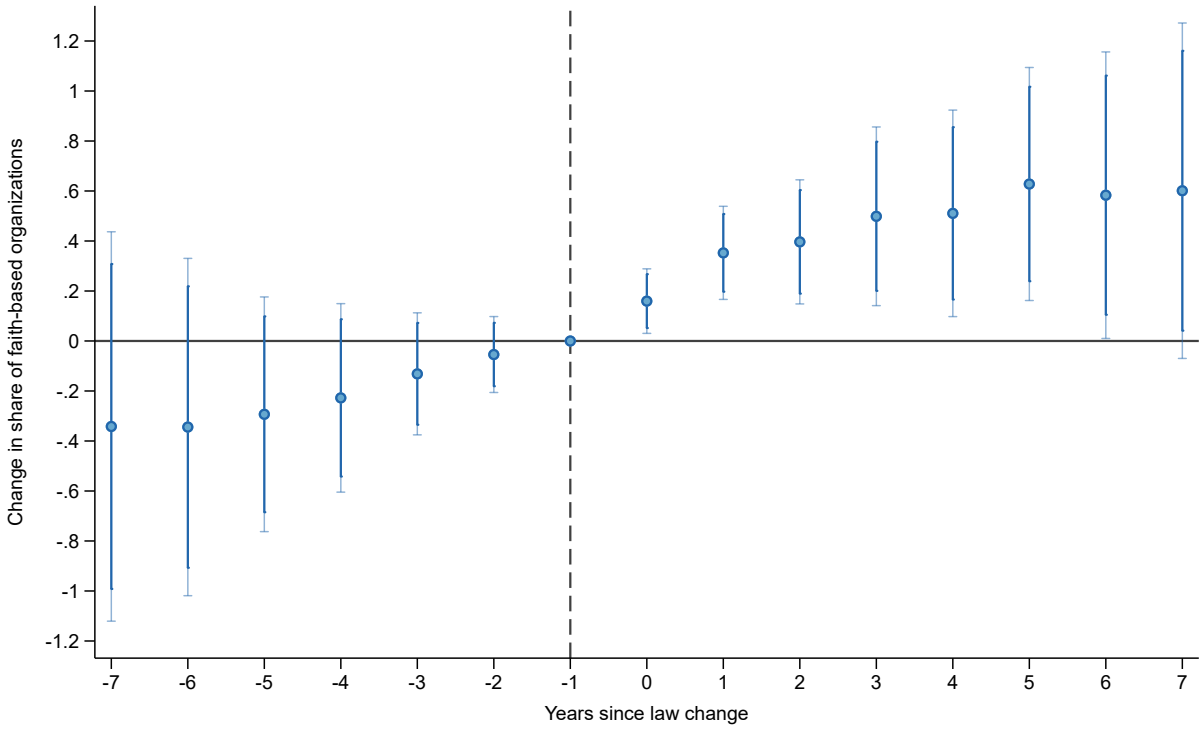
Notes: This figure presents similar estimates as in Figure 7, but considering the full unbalanced sample of nonprofit organizations instead of solely 501(c)(3) operating public charities. We include fixed effects at nonprofit organization level for the IRS subsection by treatment cohort, and the nature of the organization (public charity, private foundation, or other) combined with the reporting charity group (mutual profit, operating, or supporting), also by treatment cohort. Average share of faith-based organizations: 6.899. Change in the share of faith-based organizations post faith-based initiatives: 1.046. Aggregate post-treatment effect: 0.314 (*std. err.* = 0.070, t = 4.491).

Figure A19: The impact of faith-based initiatives on the presence of faith-based organizations: longer post-treatment time period



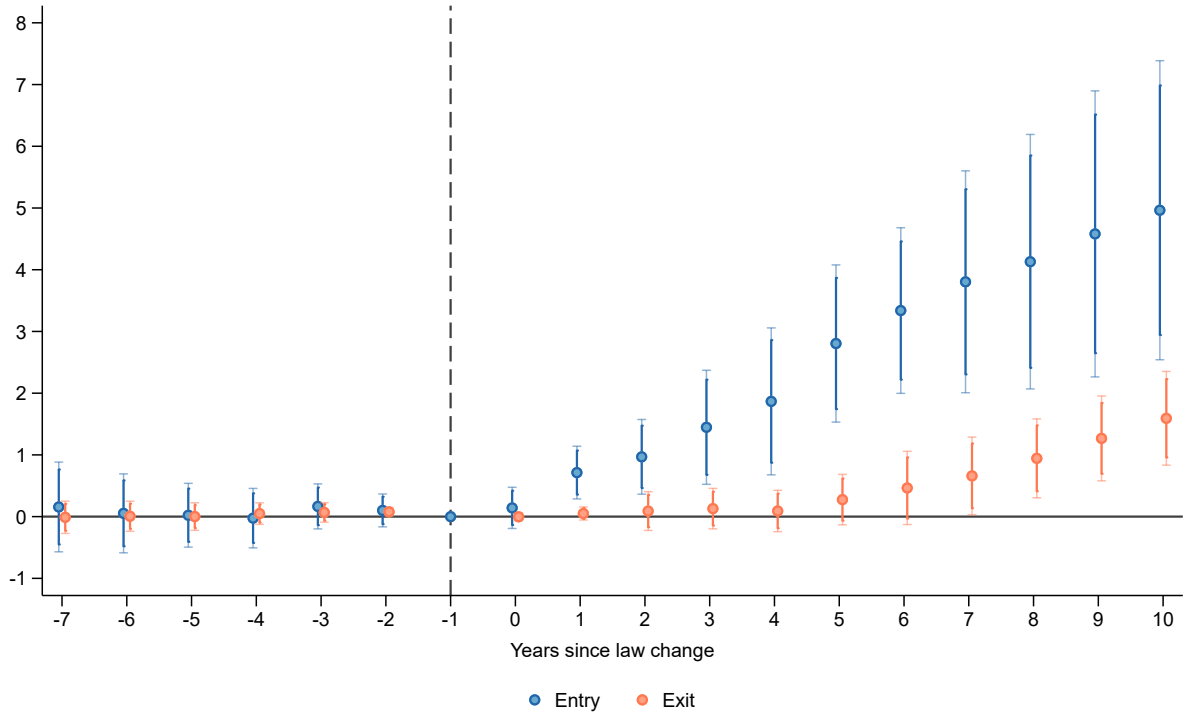
Notes: This figure shows similar estimates as in Figure 7, with the difference that the sample is stacked using a fixed time window up to ten years after the occurence of a faith-based initiative – with 2019 as the ending calendar year in the latest treatment cohort – for a total of 9,190,147 organization-by-stack-by-year observations. Average share of faith-based organizations: 11.191. Change in the share of faith-based organizations post faith-based initiatives: 1.373. Aggregate post-treatment effect: 0.431 (*std. err.* = 0.133, t = 3.245).

Figure A20: The impact of the faith-based initiatives on faith-based organizations: state-year aggregates



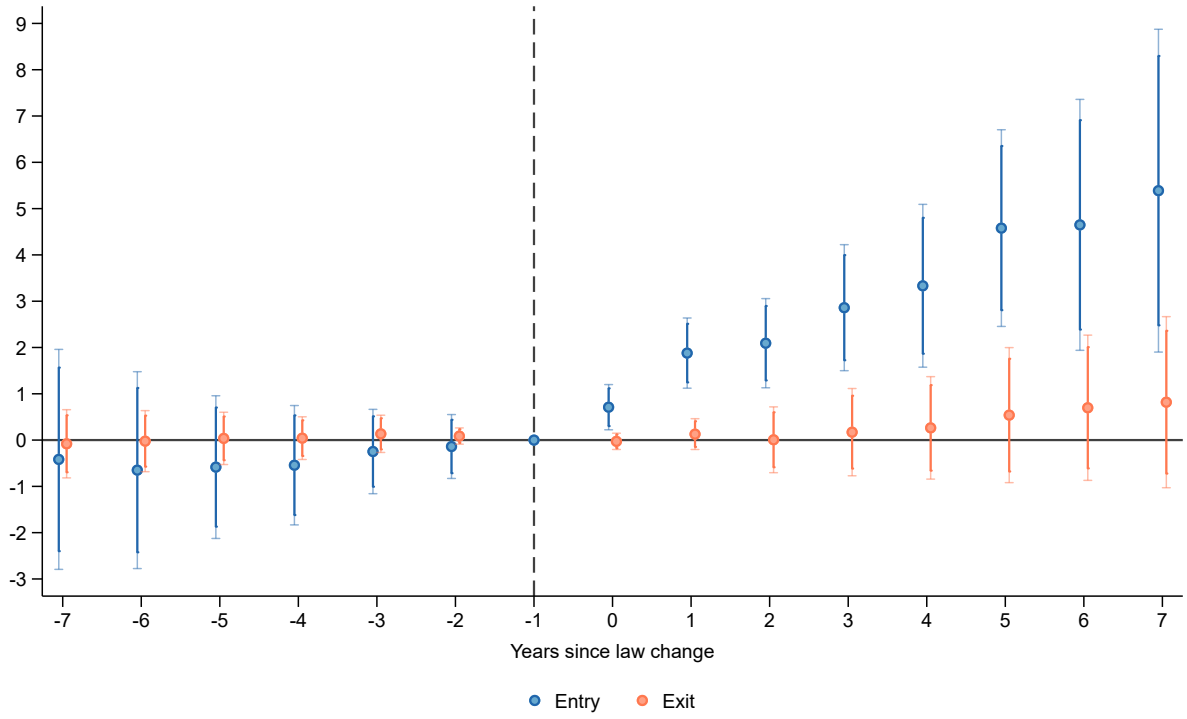
Notes: Regression estimates of the effect of faith-based initiatives on the presence of faith-based organizations. We present results of the difference-in-differences estimates of a version of Equation 1 at state level where the outcome is the (percentage) share of faith-based organizations at a given time period, where the share is calculated as the number of nonprofit organizations with a religion-related NTEE code or by the presence of religious words in their name, over the total number of nonprofit organizations (*i.e* collapsing the indicator variable in Figure 7 by treatment cohort, state and event time). The sample is a balanced panel at state-year level trimmed on a fixed time window of seven years before and seven years after the occurrence of a faith-based initiative, consistent with the analysis in Figure 7, and never-treated states as comparison group, for a total of 1,830 state-by-stack-by-event time observations. The coefficients of interest represent the change in share of faith-based organizations in states that implement a faith-based initiative, relative to states that are not implementing them. See Figure 7 for additional details. Average share of faith-based organizations: 10.540. Change in the share of faith-based organizations post faith-based initiatives: 0.871. Aggregate post-treatment effect: 0.466 (*std. err.* = 0.184, t = 2.530).

Figure A21: The impact of faith-based initiatives on faith-based organizations' dynamics: Longer post-treatment time period



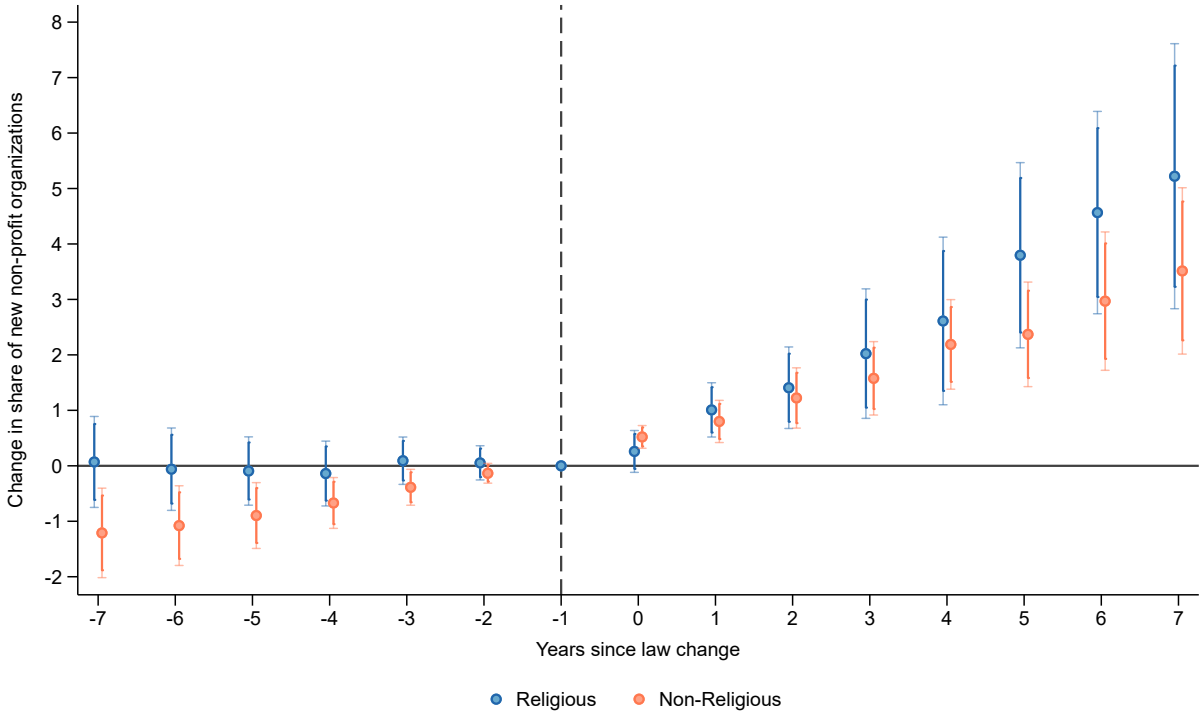
Notes: This figure is equivalent to Figure 8, with the only difference that for these estimates we use a fixed time window of three additional time periods, up to ten years after the occurrence of a faith-based initiative, for a total of 1,771,146. **Entry** – Average share of faith-based organizations starting up 37.507. Change in the share of new faith-based organizations post faith-based initiatives: 38.060. Aggregate post-treatment effect: 2.615 (*std. err.* = 0.619, t = 4.224). **Exit** – Average share of faith-based organizations starting up 37.507. Change in the share of new faith-based organizations post faith-based initiatives: 38.060. Aggregate post-treatment effect: 2.615 (*std. err.* = 0.619, t = 4.224).

Figure A22: The impact of faith-based initiatives on faith-based organizations' dynamics: State-year aggregates



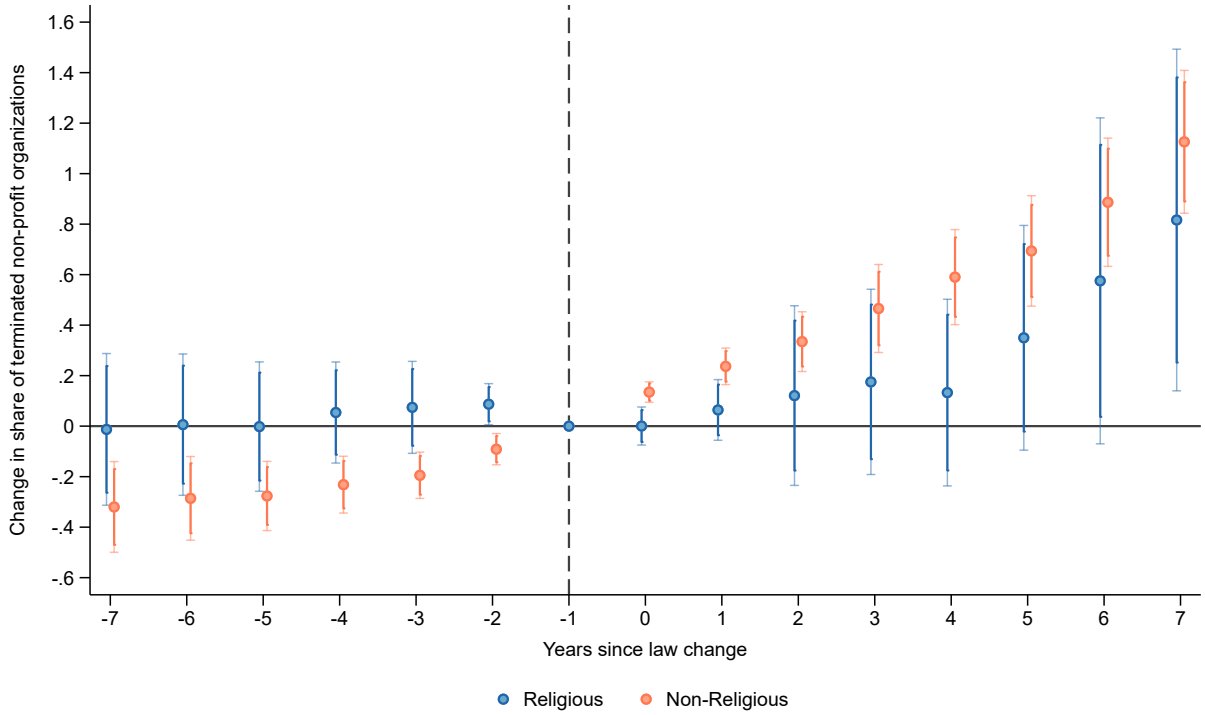
Notes: Regression estimates of the effect of faith-based initiatives on the dynamics of faith-based organizations. We show results of the difference-in-differences of a version of Equation 1 at state level where the outcome is the (cumulative) share of entering (or terminating) faith-based organizations at a given time period over the total number of faith-based organizations after re-balancing the panel such that we count each organization throughout the event time window, for a total of 1,830 state-by-stack-by-event time observations. The coefficients of interest are interactions between an indicator variable flagging the occurrence of faith-based initiatives in the state of the nonprofit organization and event time dummies, relative to the omitted interaction in the event time before the introduction of the faith-based initiative within a treatment cohort, and they describe the change in share of entering (or terminating) faith-based organizations in states with a faith-based initiative with respect to states that do not experience any. **Entry** – Average share of faith-based organizations starting up 38.687. Change in the share of new faith-based organizations post faith-based initiatives: 34.302. Aggregate post-treatment effect: 3.186 (*std. err.* = 0.794, t = 4.013). **Exit** – Average share of terminated faith-based organizations 3.042. Change in the share of terminated faith-based organizations post faith-based initiatives: 4.220. Aggregate post-treatment effect: 0.325 (*std. err.* = 0.492, t = 0.660). Coefficients estimates on the event time interactions are indicated as dots with their 90% (95%) confidence intervals drawn as thick (thin) vertical lines. Standard errors are clustered at the state level.

Figure A23: The impact of faith-based initiatives on religious and non-religious nonprofit organizations' entry



Notes: Regression estimates of the effect of faith-based initiatives on the nonprofit organizations' entry. We show results of the difference-in-differences estimates of a version of Equation 1 at nonprofit organization level, where the outcome variable is equal to one (rescaled to one hundred) from the time period in which the organization starts up, and zero otherwise or if the organization is incumbent throughout the period. We estimate results separately for non-religious and faith-based nonprofit organizations, the latter identified among all nonprofit organizations by its NTEE category or by the presence of religious words in its name as described in Section 4.1. Each sample is a re-balanced panel of organizations by their type, repsectively, such that each organization is present throughout the event time window, using a fixed time window of seven years before and seven years after the presence of a faith-based initiative, and using organizations in never-treated states as comparison group for a total of 1,272,000 faith-based organization-by-stack-by-year observations and 9,506,160 non-religious organization-by-stack-by-year observations, respectively. The coefficients of interest are interactions between an indicator variable signaling the presence of faith-based initiatives in the state of the organization and event event time dummies, relative to the omitted interaction in the event time before the introduction of the faith-based initiative within a treatment cohort, and they describe the differential probability (in percentage points) that faith-based organizations have to start their activities, relative to faith-based organizations in states that do not have faith-based initiatives. **Religious** – Average share of faith-based organizations starting up 35.940. Change in the share of new faith-based organizations post faith-based initiatives: 35.925. Aggregate post-treatment effect: 2.612 (*std. err.* = 0.589, t = 4.436). **Non-Religious** – Average share of non-religious organizations starting up 35.399. Change in the share of new faith-based organizations post faith-based initiatives: 31.748. Aggregate post-treatment effect: 1.895 (*std. err.* = 0.375, t = 5.049). Coefficients estimates on the event time interactions are indicated as dots with their 90% (95%) confidence intervals drawn as thick (thin) vertical lines. Standard errors are clustered at the state level.

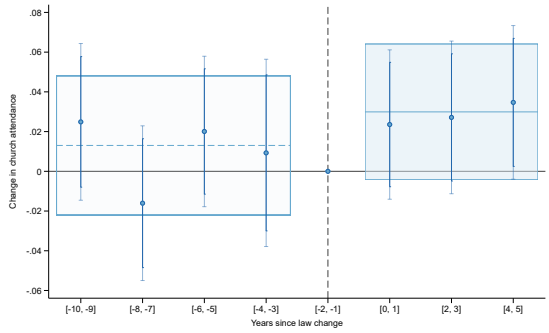
Figure A24: The impact of faith-based initiatives on religious and non-religious nonprofit organizations' exit



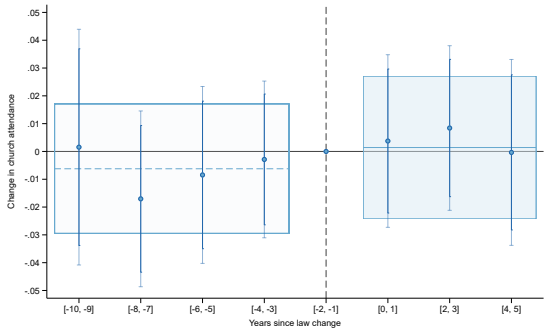
Notes: Regression estimates of the effect of faith-based initiatives on the nonprofit organizations' survival. We show results of the difference-in-differences estimates of a version of Equation 1 at nonprofit organization level, where the outcome variable is equal to one (rescaled to one hundred) from the time period in which the organization ceases to exist, and zero otherwise or if the organization is incumbent throughout the period. We estimate results separately for non-religious and faith-based nonprofit organizations, the latter identified among all nonprofit organizations by its NTEE category or by the presence of religious words in its name as described in Section 4.1. Each sample is a re-balanced panel of organizations by their type, repsectively, such that each organization is present throughout the event time window, using a fixed time window of seven years before and seven years after the presence of a faith-based initiative, and using organizations in never-treated states as comparison group for a total of 1,272,000 faith-based organization-by-stack-by-year observations and 9,506,160 non-religious organization-by-stack-by-year observations, respectively. The coefficients of interest are interactions between an indicator variable highlighting the presence of faith-based initiatives in the state of the organization and event event time dummies, relative to the omitted interaction in the event time before the introduction of the faith-based initiative within a treatment cohort, and they describe the differential probability (in percentage points) that faith-based organizations cease to exist, relative to faith-based organizations in states that do not have faith-based initiatives. **Religious** – Average share of faith-based organizations starting up 2.580. Change in the share of new faith-based organizations post faith-based initiatives: 3.525. Aggregate post-treatment effect: 0.279 (*std. err.* = 0.183, t = 1.527). **Non-Religious** – Average share of non-religious organizations starting up 3.041. Change in the share of new faith-based organizations post faith-based initiatives: 4.113. Aggregate post-treatment effect: 0.559 (*std. err.* = 0.080, t = 6.972). Coefficients estimates on the event time interactions are indicated as dots with their 90% (95%) confidence intervals drawn as thick (thin) vertical lines. Standard errors are clustered at the state level.

Figure A25: The impact of faith-based liaisons and grants on church attendance

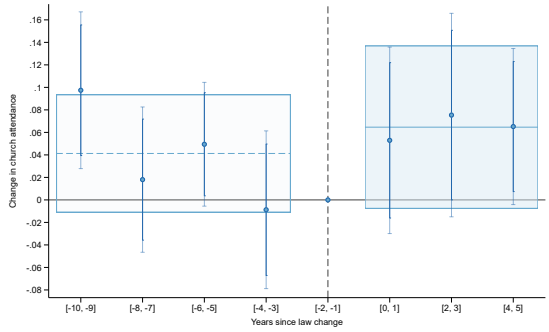
(a) Liaisons, DD estimates



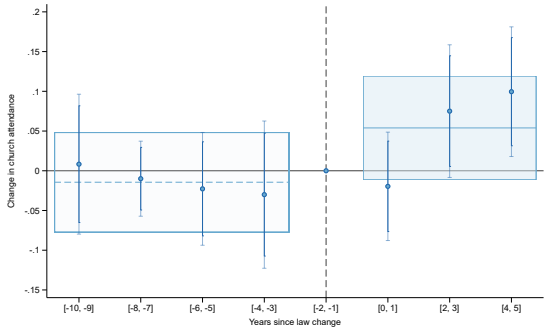
(b) Grants, DD estimates



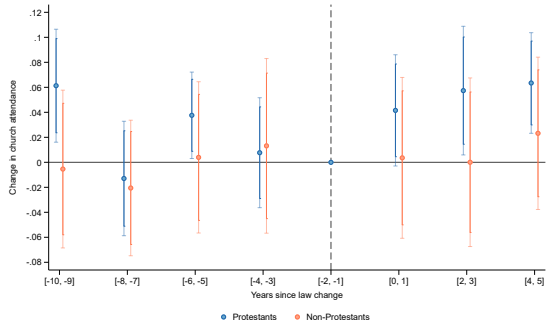
(c) Liaisons, DDD estimates



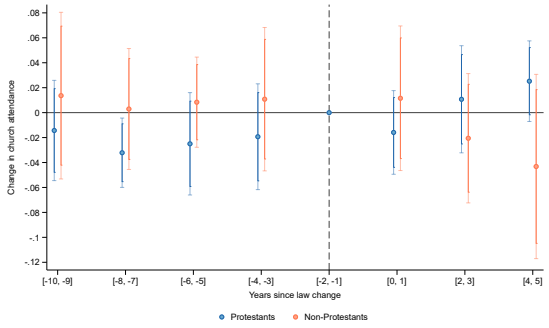
(d) Grants, DDD estimates



(e) Liaisons, DD with split sample



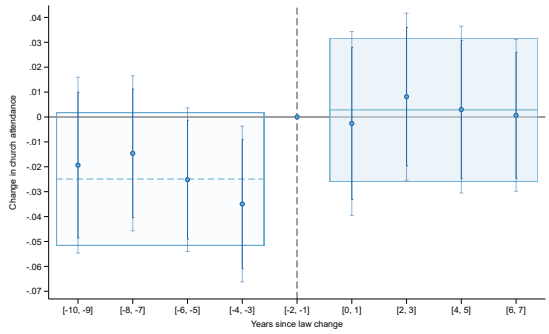
(f) Grants, DD with split sample



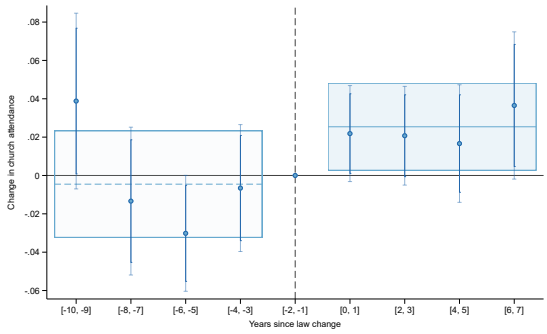
Notes: Regression estimates on the effect of faith-based liaisons and federal grants on church attendance. The dependent variable is church attendance. The sample is GSS respondents stacked as described in Section 3.2, using a fixed time window of ten years before and five years after treatment, as the data on faith-based liaisons and appropriations stop in 2005. Treatment is an indicator variable equal to one if a given state created a faith-based liaison (on the left) or a grant (on the right), interacted with event time and relative to the latest time period before treatment within a treatment cohort. In panel (a) and (b) we present estimates of a version of the Equation (1) and its aggregated counterpart in Equation (2), where the treatment is now the presence of faith-based liaisons or grants, and in panel (e) and (f) we show estimates separately for protestants and non-protestants. In panel (c) and (d) we present triple difference estimates comparing the differential change for protestants with the change for non-protestants, relative to the latest year before treatment. Coefficients estimates on the event time interactions are plotted as dots with their 90% (95%) confidence intervals drawn as thick (thin) vertical lines. Coefficients estimates on the pre-treatment period and post-treatment period aggregated interactions are indicated as lines, with their 95% confidence intervals shown as boxes. Standard errors are clustered at the state level.
Result: For both liaisons and appropriations, the pretrends are insignificant, but for the liaisons, the pretrends are nearly as large as the post effects. These near-positive pretrends are expected if the faith-based initiatives came before the implementation of the faith-based liaisons. The lack of pretrends for the appropriations is consistent with the critique that the faith-based initiatives came with little funding. For both liaisons and appropriations, church attendance seems to be rising more for the Protestants, consistent with the main analysis with the faith-based initiatives as treatment.

Figure A26: Type of first laws DD event study

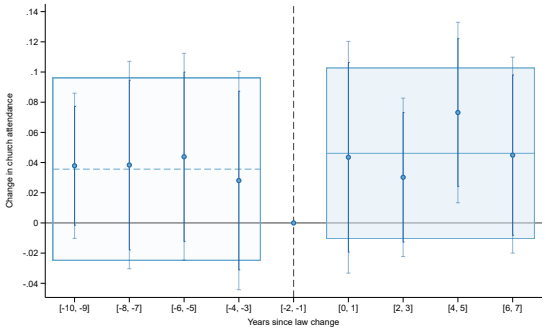
(a) Concrete Laws



(b) Symbolic Laws



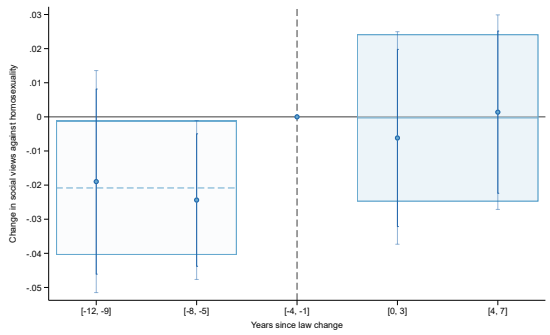
(c) Program Laws



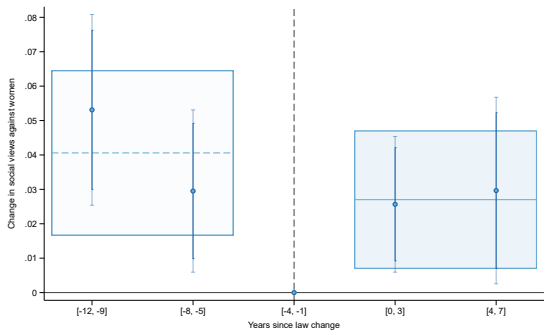
Notes: Regression estimates on the effect of different types of faith-based initiatives on church attendance. Each panel shows difference-in-differences estimates for a version of Equation (1) and the aggregate interactions in Equation (2), but a different treatment and sample composition. The treatment consists on the introduction as the indicated type of law as first faith-based initiative, and zero otherwise. For stacking the sample, we exclude respondents from treated states that are not having the indicated law as first law to avoid contamination in the post-treatment period. Coefficients estimates on the event time interactions are plotted as dots with their 90% (95%) confidence intervals drawn as thick (thin) vertical lines. Coefficients estimates on the pre-treatment period and post-treatment period aggregated interactions are indicated as lines, with their 95% confidence intervals shown as boxes. Standard errors are clustered at the state level.

Figure A27: The impact of the faith-based initiatives on conservative-religious social views: DD estimates

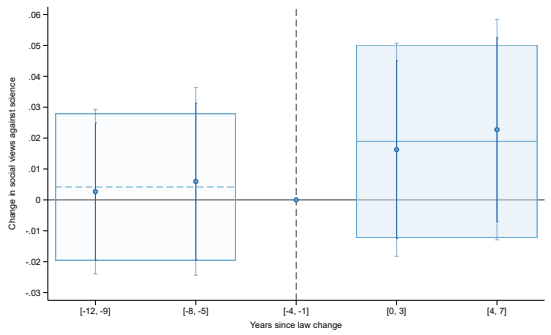
(a) Social views against homosexuals



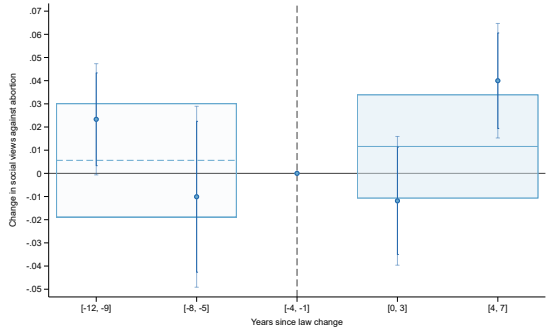
(b) Social views against women



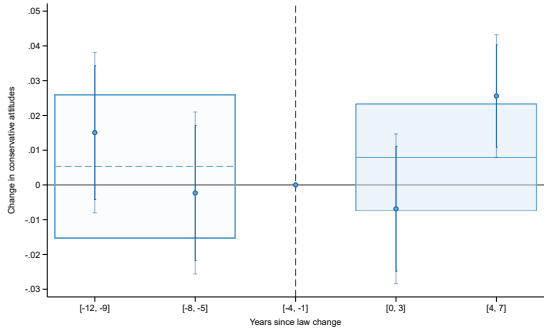
(c) Social views against science



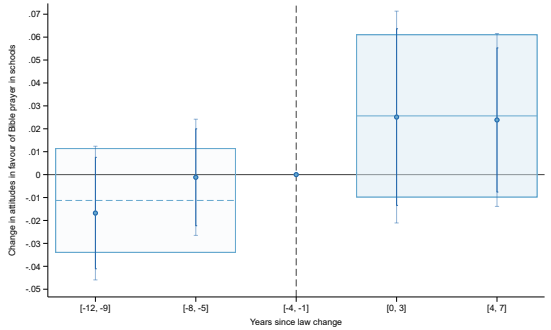
(d) Social views against abortion



(e) Conservative views



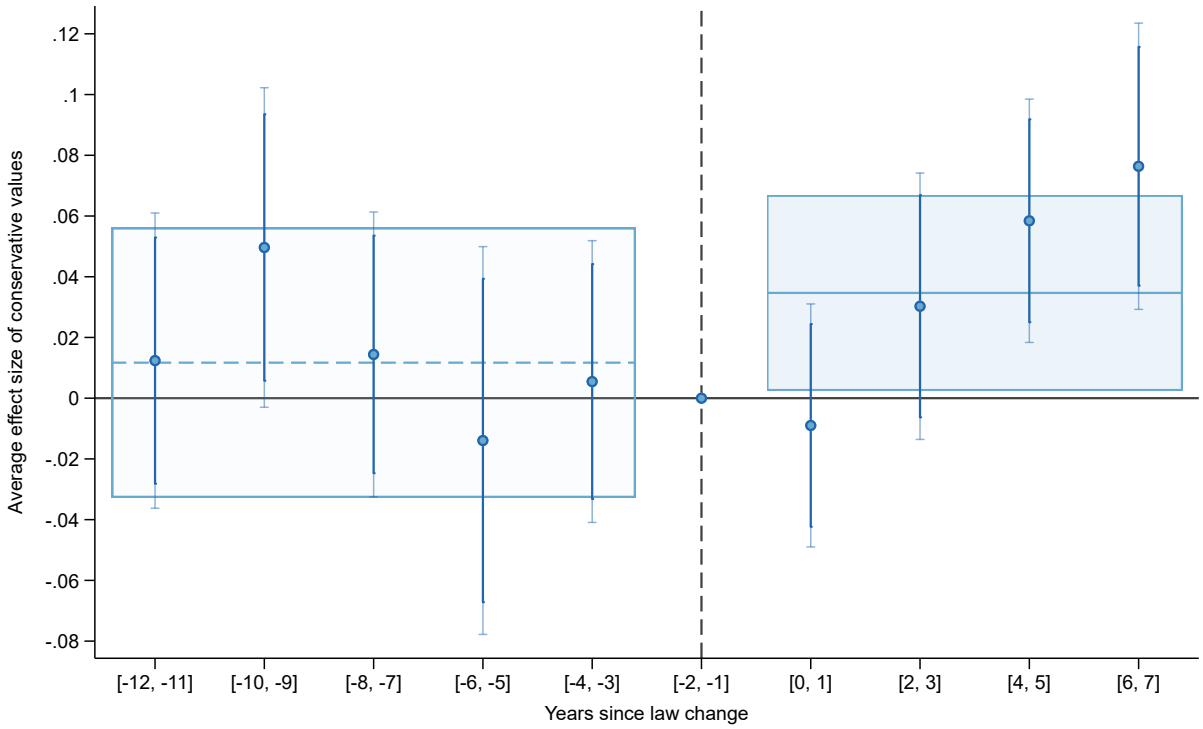
(f) In favor of prayers in schools



Notes: Regression estimates on the marginal effect of the faith-based initiatives on different social values. Each panel presents results for the difference-in-differences coefficients in Equation (1) and the aggregate interactions in Equation (2), using a stacked panel of a fixed time window of ten years before and seven years after the introduction of the faith-based initiatives, as in Figure 10. While event time is still in two event time bins we aggregate estimates of Equation (1) in two event time bins (or four time periods), relative to the average change between one and four years before the faith-based initiatives. Regressions are estimated using weighted least squares with sampling weights and the weighting scheme proposed in Wing, Freedman and Hollingsworth (2024). Panel (a): **Social views against homosexuals** – Observations: 29,961; Mean dep. var.: 0.620; Average share of Protestants: 0.451; Change in dep. var. post faith-based initiatives: -0.111. Aggregate post-treatment effect: -0.000 (*std. err.* = 0.012, t = -0.025). Panel (b): **Social views against women** – Observations: 28,348; Mean dep. var.: 0.189; Average share of Protestants: 0.434; Change in dep. var. post faith-based initiatives: -0.062. Aggregate post-treatment effect: 0.027 (*std. err.* = 0.010, t = 2.724). Panel (c): **Social views against science** – Observations: 27,446; Mean dep. var.: 0.551; Average share of Protestants: 0.436; Change in dep. var. post faith-based initiatives: -0.006. Aggregate post-treatment effect: 0.019 (*std. err.* = 0.015, t = 1.225). Panel (d): **Social views against abortion**: Observations: 33,651; Mean dep. var.: 0.608; Average share of Protestants: 0.443; Change in dep. var. post faith-based initiatives: 0.024. Aggregate post-treatment effect: 0.012 (*std. err.* = 0.011, t = 1.050). Panel (e): **Conservative views** – Observations: 43,862; Mean dep. var.: 0.165; Average share of Protestants: 0.439; Change in dep. var. post faith-based initiatives: 0.002. Aggregate post-treatment effect: 0.008 (*std. err.* = 0.008, t = 1.035). Panel (f): **Views in favor of prayers in schools** – Observations: 28,215; Mean dep. var.: 0.533; Average share of Protestants: 0.442; Change in dep. var. post faith-based initiatives: -0.018. Aggregate post-treatment effect: 0.026 (*std. err.* = 0.018, t = 1.458). Coefficients estimates on the event time interactions are plotted as dots with their 90% (95%) confidence intervals drawn as thick (thin) vertical lines. Boxes: Coefficients estimates on the pre-treatment period and post-treatment period aggregate interactions are indicated as horizontal lines, with their 95% confidence intervals shown as boxes. Standard errors are clustered at the state level.

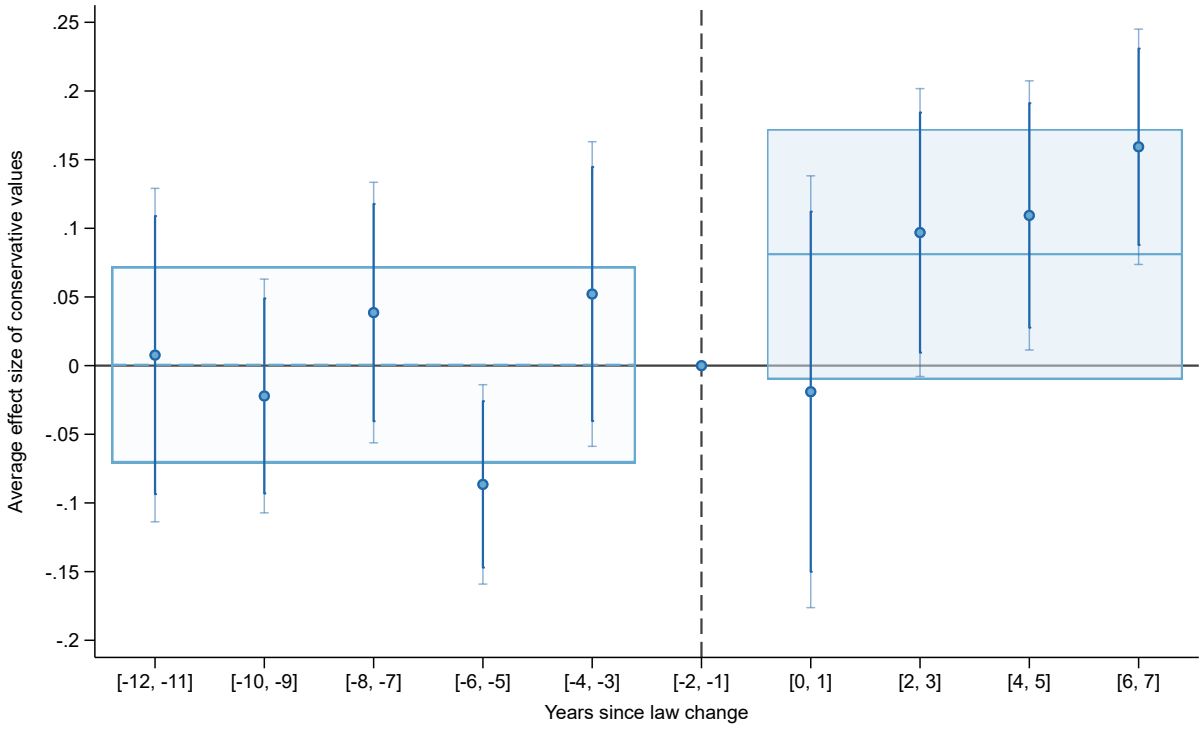
Result: Except for social views against abortion and conservative views, which we know from Figure 10 that increase and the effect is driven by Protestants, there is no trend break for respondents.

Figure A28: Average effect size of conservative-religious social views: DD estimates



Notes: Regression estimates of the average effect size (AES) of the estimates in Figure A27, following the method by Clingingsmith, Khwaja and Kremer (2009). The AES is estimated in a similar way as the difference-in-differences estimates of Equation (1) for the outcomes in Figure A27 but with the individual outcomes stacked in one dataset and standardized relative to the standard deviation in the event period before treatment. The specification additionally includes fixed effects for each outcome. Coefficient estimates on the event time interactions are plotted as dots with their 90% (95%) confidence intervals drawn as thick (thin) vertical lines. Boxes: Coefficients estimates on the pre-treatment period and post-treatment period aggregate interactions are indicated as horizontal lines, with their 95% confidence intervals shown as boxes. Standard errors are clustered at the state level. We obtain similar results normalizing the outcome at the stack level independently of the control baseline.
Result: The faith-based initiatives strengthened average conservative-religious views from 4 years after treatment and on-wards.

Figure A29: Average effect size of conservative-religious social views: DDD estimates

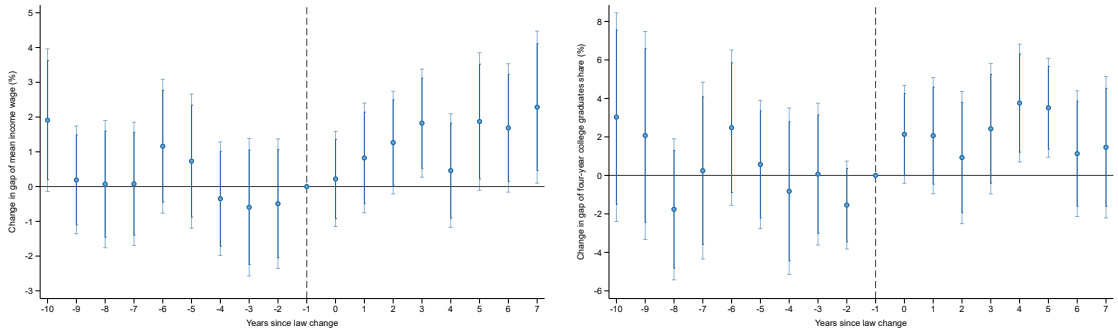


Notes: Regression estimates of the average effect size (AES) of the estimates in Figure 10, following the method by Clingingsmith, Khwaja and Kremer (2009). The AES is estimated in a similar way as the triple difference estimates of Equation (3) for the outcomes in Figure 10 but with the individual outcomes stacked in one dataset and standardized relative to the standard deviation in the event period before treatment. The specification additionally includes fixed effects for each outcome. Coefficient estimates on the event time interactions are plotted as dots with their 90% (95%) confidence intervals drawn as thick (thin) vertical lines. Boxes: Coefficients estimates on the pre-treatment period and post-treatment period aggregate interactions are indicated as horizontal lines, with their 95% confidence intervals shown as boxes. Standard errors are clustered at the state level.
Result: The faith-based initiatives strengthened average conservative-religious views significantly for Protestants from 4 years after treatment and on-wards.

Figure A30: The impact of the faith-based initiatives on gender gaps: state-level outcomes expressed in percentage

(a) Gender wage gap pct

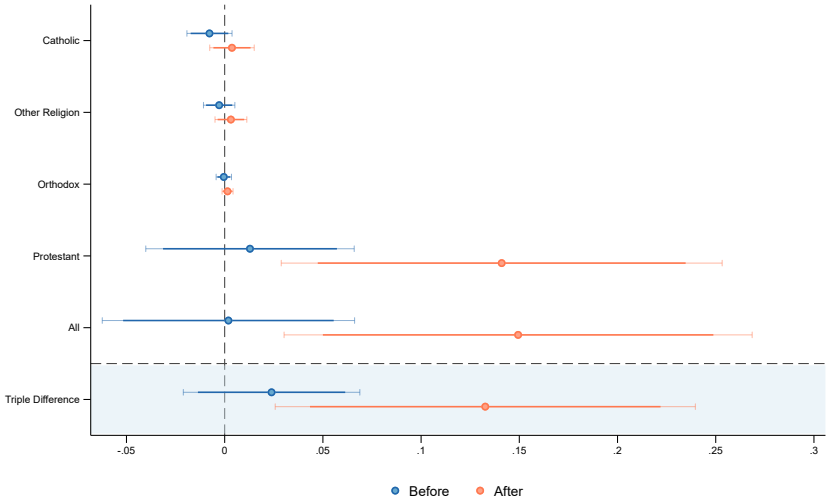
(b) Gender education gap pct



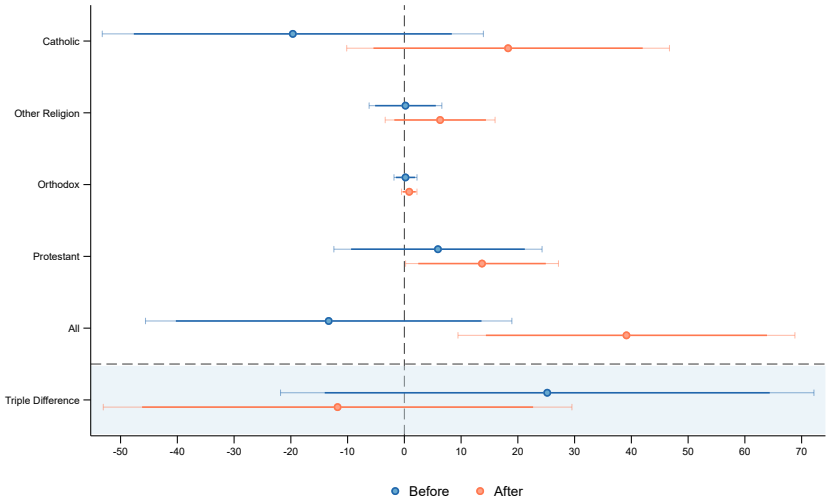
Notes: This figure replicates panels (b) and (c) of Figure 11 using the same sample at state year level. However, in this case we express gender gap outcomes as percentage change of male vs. female in a given state on a given year, *i.e..*, $wage_{male} - wage_{female}/wage_{male}$, for instance.

Figure A31: The impact of faith-based initiatives on the number of congregations and adherents

(a) Number of congregations per 1,000 inhabitants



(b) Number of adherents per 1,000 inhabitants



Notes: Regression estimates of the effect of faith-based initiatives on the number of congregations (panel a) and the number of adherents (panel b) of a religious group per 1,000 inhabitants at state-year level. The first five categories are difference-in-differences estimates of the effect of faith-based initiatives on the outcome variable at state-year level for the indicated religious group, where estimates before treatment are calculated for event time $t \in [-20, -10)$, and estimates after treatment are calculated for event time $t \in [0, 10)$, both with respect to event time $\tau \in [-10, -1]$ (in order to have one estimate for each treatment cohort-state in each event time estimate). Hence, the time window of state-year data goes from twenty years before the introduction of faith-based initiatives to nine years after the event. The last category are triple difference estimates of the effect of faith-based initiatives on the outcome variable for Protestants with respect to the effect on non-Protestants. Data are available for 1980, 1990, 2000 and 2010. **Average number of congregations per 1,000 inhabitants**: 1.251 (*protestants*: 0.998; *christian catholics*: 0.155; *christian orthodox*: 0.006; *other religions*: 0.092). **Average number of adherents per 1,000 inhabitants**: 536.985 (*protestants*: 247.435; *christian catholics*: 250.914; *christian orthodox*: 2.554; *other religions*: 36.081).
Result: The number of Protestant congregations rise significantly after implementation of the faith-based initiatives. Also relative to the remaining denominations. The number of Protestant adherents also rise, but not relative to other denominations.

Table A1: Validity checks of law measure based on budgets

Law measure	#Years		#Laws	
	(1)	(2)	(3)	(4)
Dependent variable:				
Grants number	66.2*	80.3**	26.0***	30.0***
	(34.259)	(33.011)	(9.464)	(9.966)
Grants USD	55.6**	61.3**	24.0***	24.2***
	(24.375)	(23.644)	(7.746)	(7.640)
Community-based organizations USD	50.7**	55.5**	20.6***	20.7***
	(22.333)	(21.950)	(7.092)	(7.149)
Faith-based organizations USD	6.58*	7.57**	4.18**	4.34**
	(3.704)	(3.293)	(1.754)	(1.678)
Compassion Capital Fund USD	0.42***	0.52***	0.13*	0.15*
	(0.154)	(0.193)	(0.074)	(0.083)
Mentoring Children of Prisoners USD	0.48**	0.51***	0.29***	0.28***
	(0.188)	(0.187)	(0.072)	(0.073)
Region FE:	N	Y	N	Y

Notes: OLS estimates across 50 US states in 2006. Each estimate is the result of one regression, where the explanatory variable is the number of years that the state had at least one faith-based initiative implemented by 2010 in columns (1) and (2) and the number of faith-based initiatives implemented by 2010 in columns (3) and (4). The dependent variable varies across rows. Columns (1) and (3) are simple correlations, while columns (2) and (4) include fixed effects for the four large regions: Northeast, Midwest, West, and South. Robust standard errors in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level.
Result: The intensity of the faith-based initiatives (measured in number years or number laws implemented) is positively correlated with specific contents of the faith-based initiatives: grants to faith-based and community organizations (to both secular and faith-based nonprofits), grants through the Compassion Capital Fund and Mentoring Children of Prisoners, and the amount of volunteering.

Table A2: The impact of faith-based initiatives on church attendance: comparison between the specification in Wing, Freedman and Hollingsworth (2024)

Dependent variable: Church attendance				
	ES DID	ES DDD	FE DID	FE DDD
	(1)	(2)	(3)	(4)
$FBI_{sc} \times Post_{ct}$	0.023***	-0.012	0.026***	-0.013
	(0.006)	(0.011)	(0.006)	(0.011)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Protestant\right)_{isct}$		0.074***		0.079***
		(0.016)		(0.014)
Composite Effect for Protestants		0.062***		0.066***
		(0.011)		(0.009)
Observations	45,315	45,315	45,315	45,315
Mean Dep. Var.	0.422	0.422	0.422	0.422
Share of Protestants	0.427	0.427	0.427	0.427
Event Time FE	✓	✓		
State-Cohort FE			✓	
Event Time-Cohort FE			✓	
State-Protestant-Cohort FE				✓
epcFE				✓
Individual Controls	✓	✓	✓	✓

Notes: In this table, we want to compare estimates of our preferred "fixed effects" specification with the event study specification proposed in Wing, Freedman and Hollingsworth (2024), applying the weighting scheme explained by the authors (after the sampling weights in our survey). The sample is assembled as we show in Section 3.2 with a fixed time window of ten years before the faith-based initiatives and seven years after. Column 1 exhibits estimates of the difference-in-differences specification on the overall sample using a version of the event study specification shown in Wing, Freedman and Hollingsworth (2024):

$$y_{isct} = \alpha_0 + \alpha_1 FBI_{sc} + \lambda_t + \beta^{post} FBI_{sc} \times Post_{ct} + \boldsymbol{X}'_{isct}\gamma + \varepsilon_{isct} \tag{C.1}$$

where the only difference in (C.1) is basically that we add covariates. We compare the estimate of column 1 with the estimates in column 3, where we estimate a version of Equation (2). In column 2, we show estimates of a version of Equation (C.1) where we allow for triple differences

$$\begin{aligned} y_{irsct} = {} & \alpha_0 + \alpha_1 FBI_{sc} + \alpha_2 \mathbf{1}\left(r = Protestant\right)_{isct} \\ & + \alpha_3 FBI_{sc} \times \mathbf{1}\left(r = Protestant\right)_{isct} + \alpha_4 \mathbf{1}\left(r = Protestant\right)_{isct} \times Post_{ct} + \lambda_t \\ & + \beta_0 FBI_{sc} \times Post_{ct} + \beta_1 FBI_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Protestant\right)_{isct} \\ & + \boldsymbol{X}'_{irsct}\gamma + \varepsilon_{irsct} \end{aligned} \tag{C.2}$$

and we compare it with estimates in column 4 for a version of Equation (4) without β_{pre} and α_{srt} to allow for both the coefficient for the interaction of treatment with the post-treatment period dummy, and the triple interaction for the indicator variable of Protestants, all relative to the pre-treatment average of the never-treated. The composite effect for protestants is the t-test of the two coefficients for the triple differences. Standard errors are clustered at the state level. *, **, and *** indicate significance at the 10%, 5%, and 1% level.

Table A3: The impact of faith-based initiatives on church attendance: additional controls

Dependent variable: Church attendance	No Covariates		Main Covariates		Different confounders											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
$FBI_{sc} \times Post_{ct}$	0.025***	-0.012	0.026***	-0.013	0.023***	-0.020*	0.026***	-0.011	0.027***	-0.010	0.022***	-0.014	0.035***	-0.005	0.026**	-0.013
	(0.007)	(0.012)	(0.006)	(0.011)	(0.006)	(0.011)	(0.006)	(0.011)	(0.006)	(0.010)	(0.006)	(0.011)	(0.009)	(0.012)	(0.011)	(0.012)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct}$		0.082***		0.079***		0.086***		0.078***		0.074***		0.072***		0.078***		0.074***
		(0.015)		(0.014)		(0.013)		(0.014)		(0.014)		(0.014)		(0.014)		(0.013)
Real Household Income					0.017	0.016									-0.015**	-0.015**
					(0.010)	(0.011)									(0.007)	(0.007)
Educational Level							0.008***	0.008***							0.010***	0.010***
							(0.002)	(0.002)							(0.001)	(0.001)
Republican									0.071***	0.071***					0.081***	0.082***
									(0.011)	(0.011)					(0.012)	(0.012)
African-American											0.075***	0.080***			0.119***	0.122***
											(0.009)	(0.010)			(0.007)	(0.008)
Public spending per capita (lagged)													0.000	0.000	0.000	0.000
													(0.000)	(0.000)	(0.000)	(0.000)
Composite Effect for Protestants		0.070***		0.066***		0.066***		0.067***		0.064***		0.059***		0.074***		0.061***
		(0.008)		(0.009)		(0.007)		(0.009)		(0.009)		(0.008)		(0.010)		(0.012)
Observations	45,449	45,449	45,315	45,315	39,111	39,111	45,189	45,189	45,035	45,035	45,315	45,315	45,315	45,315	38,914	38,914
Mean Dep. Var.	0.422	0.422	0.422	0.422	0.421	0.421	0.422	0.422	0.422	0.422	0.422	0.422	0.422	0.422	0.421	0.421
Share of Protestants	0.428	0.428	0.427	0.427	0.440	0.440	0.427	0.427	0.428	0.428	0.427	0.427	0.427	0.427	0.440	0.440
ctrls			✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Regression estimates of the effect of the faith-based initiatives on church attendance, allowing for different sets of confounders. The sample is the same of the estimates in Figure 4, with the only difference that we exclude individuals for which we do not have informations in the indicated covariates. In the odd columns, we show results of the difference-in-difference estimates in Equation (2) without including β^{pre} in the specification and expressing β^{post} relative to the pre-treatment average. In even columns, we show estimates of a modified version of Equation (4) where we exclude β^{pre} and omit α_{srt} to allow the estimation of both the difference between treated and never-treated groups post-treatment and the triple difference with protestants (losing some degrees of precision). Throughout the columns, we show robustness checks for the impact of the faith-based initiatives with no covariates, individual controls (age, gender and marital status), and leaving-in and out relevant variables indicated in the table. Regressions are estimated using weighted least squares with sampling weights and the weighting scheme proposed in Wing, Freedman and Hollingsworth (2024). The composite effect for Protestants is the t-test of the two coefficients for the triple differences. Standard errors are clustered at the state level. *, **, and *** indicate significance at the 10%, 5%, and 1% level.

Table A4: The impact of faith-based initiatives on church attendance: effect by denominations

Dependent variable: Church attendance			
	Sample Group		
	Only Protestants (Reference: Mainline)	Only Non-Protestants (Reference: No Religion)	All Religions (Reference: Protestants)
	(1)	(2)	(3)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Evangelical)_{isct}$	0.073***		
	(0.027)		
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Black\ Protestant)_{isct}$	0.086*		
	(0.048)		
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Other\ Protestant)_{isct}$	-0.016		
	(0.058)		
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Catholic)_{isct}$		0.027	-0.065***
		(0.026)	(0.017)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Jewish)_{isct}$		-0.035	-0.123***
		(0.044)	(0.039)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Other\ Religion)_{isct}$		0.075*	0.001
		(0.040)	(0.037)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = No\ Denomination)_{isct}$			-0.083***
			(0.019)
Observations	20,653	24,612	45,267
Mean Dep. Var.	0.482	0.374	0.422
Mean Dep. Var. Reference Group	0.413	0.105	0.486
State-Religion-Cohort FE	✓	✓	✓
Event-Religion-Cohort FE	✓	✓	✓
State-Event-Cohort FE	✓	✓	✓
Individual Controls	✓	✓	✓

Notes: In this table, we show estimates of a version of Equation (4) for different religious groups and denominations, based on the same sample used for the results in Figure 4. Each column is a separate regression for individuals belonging to a protestant denomination, individuals belonging to a non-protestant denonination and the full sample of all individuals. In column 1, we estimate the effect of faith-based initiatives for non-Mainline protestants relative to Mainline protestants and the latest period before the introduction of the faith-based initiatives. In column 2, we estimate the effect of faith-based initiatives for non-protestants relative to protestants and the latest period before the introduction of the faith-based initiatives. In column 3, we estimate the effect of faith-based initiatives all religious groups relative to (all) Protestants, relative to the latest period before the introduction of the faith based initiatives. Regressions are estimated using weighted least squares with sampling weights and the weighting scheme proposed in Wing, Freedman and Hollingsworth (2024). Standard errors are clustered at state level.

Table A5: The impact of faith-based initiatives on religiosity: Church attendance as indicator variables

Dependent variable:	Indicator variable if respondent attends church...			
	Never (1)	Annually (2)	Monthly (3)	Weekly (4)
Panel A. Linear Regressions				
$FBI_{sc} \times Post_{ct}$	-0.010	0.044**	-0.019	-0.015
	(0.015)	(0.020)	(0.013)	(0.010)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct}$	-0.085***	-0.038**	0.057***	0.067***
	(0.019)	(0.018)	(0.016)	(0.017)
Composite Effect for Protestants	-0.095***	0.006	0.038***	0.051***
	(0.016)	(0.017)	(0.011)	(0.013)
Observations	45,315	45,315	45,315	45,315
Mean Dep. Var.	0.264	0.256	0.196	0.235
Share of Protestants	0.427	0.427	0.427	0.427
State-Protestant-Cohort FE	✓	✓	✓	✓
Event Time-Protestant-Cohort FE	✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓
Panel B. Binary Response Model (probit)				
$FBI_{sc} \times Post_{ct}$	-0.013	0.034**	-0.027**	0.005
	(0.011)	(0.016)	(0.012)	(0.013)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct}$	-0.059***	-0.030**	0.045***	0.036**
	(0.017)	(0.014)	(0.016)	(0.016)
Composite Effect for Protestants	-0.072***	0.004	0.018***	0.041***
	0.013	0.011	0.009	0.014
Observations	45,315	45,309	45,297	45,305
Mean Dep. Var.	0.264	0.256	0.196	0.235
Share of Protestants	0.427	0.427	0.427	0.427
State-Protestant-Cohort FE	✓	✓	✓	✓
Event Time-Protestant-Cohort FE	✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓

Notes: Regression estimates for the effect of the faith-based initiatives on church attendance. Each column expresses church attendance as an indicator variable equal to one if the respondent goes to church (1) never, (2) annually, (3) monthly or (4) weekly. Panel (A) presents estimates of a modified version of Equation (4) where we omit β^{pre} and α_{srt} to estimate in the same regression both coefficients for the overall effect and the marginal effect for protestants. Panel (a) shows estimates obtained using weighted least squares with sampling weights and the weighting scheme proposed in Wing, Freedman and Hollingsworth (2024). Panel (b) shows estimates obtained using a probit model using the sampling weights and "absorbing" the analytical weights in Wing, Freedman and Hollingsworth (2024). In unreported results, probit results are not different from using only the sampling weights or the analytical weights.
Result: The faith-based initiatives pushed never- or yearly attenders into attending monthly or weekly.

Table A6: The impact of faith-based initiatives on church attendance: Ordinal logit with full categories of church attendance

Dependent variable:	Never	Less than once a year	Once a year	Several times a year	Once a month	Two-three times per month	Nearly every week	Every week	More than once per week
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$FBI_{sc} \times Post_{ct}$	-0.004	-0.001	-0.001	-0.000	0.000	0.001	0.001	0.003	0.002
	(0.010)	(0.002)	(0.002)	(0.000)	(0.001)	(0.002)	(0.001)	(0.007)	(0.004)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Protestant\right)_{isct}$	-0.035***	-0.009***	-0.008***	-0.001**	0.003***	0.006***	0.005***	0.025***	0.014***
	(0.013)	(0.003)	(0.003)	(0.000)	(0.001)	(0.002)	(0.002)	(0.009)	(0.005)
Composite Effect for Protestants	-0.039***	-0.010***	-0.009***	-0.001***	0.003***	0.006***	0.005***	0.029***	0.015***
	0.009	0.002	0.002	0.000	0.001	0.002	0.001	0.007	0.004
Observations	45,315	45,315	45,315	45,315	45,315	45,315	45,315	45,315	45,315
Mean Dep. Var.	0.199	0.082	0.135	0.133	0.072	0.084	0.049	0.180	0.065
Share of Protestants	0.443	0.443	0.443	0.443	0.443	0.443	0.443	0.443	0.443
State-Protestant-Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Event Time-Protestant-Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: This table shows marginal effects of church attendance after the introduction of faith-based initiatives from estimates of a version of Equation (4) as in Table A3 by using an ordinal logit with sampling weights.
Result: The faith-based initiatives pushed never- or yearly attenders into attending monthly or weekly.

Table A7: The impact of faith-based initiatives on church attendance: additional heterogeneity checks

Dependent variable: Church attendance								
Subgroup of interest	Republican		Income		Education		Foreign Born	
	All	Protestant	All	Protestant	All	Protestant	All	Protestant
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$FBI_{sc} \times Post_{ct}$	0.022***	0.065***	-0.010	0.046**	0.035**	0.061**	0.029***	0.068***
	(0.006)	(0.010)	(0.015)	(0.021)	(0.014)	(0.023)	(0.008)	(0.009)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Republican)_{isct}$	0.001	-0.012						
	(0.011)	(0.016)						
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Income \geq 25^{th})_{isct}$			0.041**	0.026				
			(0.016)	(0.025)				
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Education \geq 25^{th})_{isct}$					-0.012	0.004		
					(0.012)	(0.023)		
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Foreign)_{isct}$							-0.018	-0.019
							(0.022)	(0.039)
Composite Effect	0.024*	0.053***	0.031***	0.072***	0.022***	0.065***	0.011	0.049
	(0.012)	(0.012)	(0.008)	(0.010)	(0.006)	(0.009)	(0.020)	(0.038)
Observations	38,913	18,132	38,912	18,132	38,912	18,129	43,546	19,842
Mean Dep. Var.	0.421	0.484	0.421	0.484	0.421	0.484	0.422	0.485
Share of Subgroup	0.253	0.311	0.760	0.754	0.799	0.801	0.125	0.065
State-Subgroup-Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓
Event-Subgroup-Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓	✓	✓	✓	✓

Notes: This table shows results of a version of Equation (4) where instead of having protestants as strata we use different types of variables for placebo, as indicated in the column eathers. Each subgroup has two different triple differences on the full sample and the sample isolating protestants, such that we look at the robustness of the results within the protestant group for each of the subgroups variables. The composite effect is the t-test of the sum of the two coefficients. **Result:** The faith-based initiatives did not influence attendance more for Republicans or the educated, but they did raise attendance more for those with higher incomes. However, this does not explain the heterogeneity with respect to being Protestant. Last, attendance was not influenced differentially for the foreign borns.

Table A8: Pairwise correlation between the GSS measures of religiosity

Variables	Church attendance	Strength of affiliation	Believe in afterlife	Bible word of God	Daily prayer
Church attendance	1.000				
Strength of affiliation	0.618	1.000			
Believe in afterlife	0.232	0.247	1.000		
Bible word of God	0.301	0.302	0.107	1.000	
Prayer	0.526	0.526	0.326	0.330	1.000

Notes: All correlation coefficients are significant at the 1% level.

Table A9: The impact of faith-based initiatives on church attendance: Heterogeneity across macro-regions

Dependent variable: Church attendance	Northeast		Midwest		West		South		Rust Belt	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$FBI_{sc} \times Post_{ct}$	0.049**		-0.037**		0.044**		0.017		-0.023	
	(0.018)		(0.013)		(0.018)		(0.011)		(0.017)	
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct}$		0.091		0.126***		0.061*		0.067**		0.058
		(0.056)		(0.020)		(0.033)		(0.031)		(0.035)
Observations	11,280	11,278	16,417	16,414	22,088	22,086	15,784	15,780	15,186	15,181
Mean Dep. Var.	0.412	0.412	0.425	0.425	0.428	0.428	0.388	0.388	0.423	0.423
Share of Protestants	0.326	0.326	0.399	0.399	0.457	0.457	0.338	0.338	0.393	0.393
State-Cohort FE	✓		✓		✓		✓		✓	
Event Time-Cohort FE	✓		✓		✓		✓		✓	
State-Protestant-Cohort FE		✓		✓		✓		✓		✓
Event Time-Protestant-Cohort FE		✓		✓		✓		✓		✓
State-Event Time-Cohort FE		✓		✓		✓		✓		✓
Individual Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The table shows estimates of Equation 2 and 4 separately for each indicated macro-region, creating stacked datasets where the never treated units are from any regions to keep a constant comparison group independenly of the sub-sample. Regressions are estimated using weighted least squares with sampling weights and the weighted scheme proposed in Wing, Freedman and Hollingsworth (2024). Standard errors are clustered at state level.
Result: Church attendance rises more for Protestants in all macro-regions, significantly for three of five regions.

Table A10: The impact of faith-based initiatives on church attendance: heterogeneity across neighbouring states

Dependent variable: Church attendance	All	Protestant	Non-Protestant
	(1)	(2)	(3)
$FBI_{sc} \times Post_{ct}$	0.033***	0.085***	-0.005
	(0.010)	(0.013)	(0.013)
$FBI_{sc} \times Post_{ct} \times \#Years_{sc}$	-0.003	-0.006*	-0.003
	(0.002)	(0.003)	(0.003)
Observations	45,708	20,672	24,643
Mean Dep. Var.	0.422	0.483	0.374
Share of Protestants	0.429	—	—
State-Cohort FE	✓	✓	✓
Event Time-Cohort FE	✓	✓	✓
Individual Controls	✓	✓	✓

Notes: Difference-in-differences estimates for the same sample of Figure 4, where we interact the treatment with a discrete variable counting the number of years from the earliest treated neighbour state. The variable can take the value zero in case the earliest state is treated at the same time of the treated state, if the unit belongs to a state that is never treated, or in case of no treated neighbours. In column (1) we show results for the overall sample, whereas in column (2) and (3) we split the sample for protestants and non-protestants, respectively.
Result: The impact on church attendance is not larger when the neighbouring state got treated earlier. This indicates that effects are not driven by spillover effects.

Table A11: The impact of faith-based initiatives on church attendance: Heterogeneity across total number of laws

Dependent variable: Church attendance			
	Median	Cutoffs	Dummies
	(1)	(2)	(3)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct} \times \mathbf{1}(\#Laws \leq 50^{th})_{sc}$	0.079***		
	(0.020)		
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct} \times \mathbf{1}(\#Laws > 50^{th})_{sc}$	0.089***		
	(0.023)		
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct} \times \mathbf{1}(\#Laws \leq 10^{th})_{sc}$		0.091**	
		(0.040)	
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct} \times \mathbf{1}(10^{th} < \#Laws \leq 25^{th})_{sc}$		0.087***	
		(0.021)	
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct} \times \mathbf{1}(25^{th} < \#Laws \leq 50^{th})_{sc}$		0.040	
		(0.034)	
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct} \times \mathbf{1}(50^{th} < \#Laws \leq 75^{th})_{sc}$		0.126***	
		(0.030)	
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct} \times \mathbf{1}(75^{th} < \#Laws \leq 90^{th})_{sc}$		0.034	
		(0.021)	
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct} \times \mathbf{1}(\#Laws > 90^{th})_{sc}$		0.119***	
		(0.021)	
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct} \times \mathbf{1}(\#Laws = 1)_{sc}$			0.048
			(0.039)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct} \times \mathbf{1}(\#Laws = 2)_{sc}$			0.164***
			(0.028)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct} \times \mathbf{1}(\#Laws = 4)_{sc}$			0.157***
			(0.028)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct} \times \mathbf{1}(\#Laws = 5)_{sc}$			0.072***
			(0.021)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct} \times \mathbf{1}(\#Laws = 6)_{sc}$			0.034
			(0.031)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct} \times \mathbf{1}(\#Laws = 8)_{sc}$			0.154***
			(0.041)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct} \times \mathbf{1}(\#Laws = 9)_{sc}$			0.112***
			(0.033)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct} \times \mathbf{1}(\#Laws = 10)_{sc}$			0.011
			(0.018)
$\mathbf{1}(r = Protestant)_{isct} \times \mathbf{1}(\#Laws > 10)_{sc}$			0.096***
			(0.026)
Observations	45,315	45,315	45,315
Mean Dep. Var.	0.422	0.422	0.422
Share of Protestants	0.443	0.443	0.443
State-Protestant-Cohort FE	✓	✓	✓
Event Time-Protestant-Cohort FE	✓	✓	✓
State-Event Time-Cohort FE	✓	✓	✓
Individual Controls	✓	✓	✓

Notes: In this table, we present three separate regressions for a version of Equation (4) where we additionally saturate the interactions in the post-treatment period for indicator variables of the number of laws introduced until the end of the period (seven years after the first faith-based initiative). In column 1, we interact the triple difference for two indicator variables of having laws in a state up to or above the median number of laws for the post-treatment period after the first faith-based initiative. in column 2, we use different cutoffs for the distribution of the number of laws in the post treatment period, as indicated in the labels. In column 3, we allow for all the non-linearities with dummies for each number of laws introduced in the treatment period, until a number of ten laws. Regressions are estimated using weighted least squares with sampling weights and the weighting scheme proposed in Wing, Freedman and Hollingsworth (2024). Standard errors are clustered at state level.
Result: The rise in church attendance is not necessarily larger when more laws are implemented.

Table A12: The impact of faith-based initiatives on church attendance: Type of initiatives

Dependent variable: Church attendance	Effect by FBI Type			Heterogeneity
	Concrete	Symbolic	Program	
	(1)	(2)	(3)	(4)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct}$	0.114***	0.064**	0.109**	
	(0.023)	(0.025)	(0.041)	
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct} \times \mathbf{1}(Concrete\ Law)_{sc}$				0.114***
				(0.019)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct} \times \mathbf{1}(Symbolic\ Law)_{sc}$				0.026
				(0.024)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct} \times \mathbf{1}(Program\ Law)_{sc}$				0.076**
				(0.033)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct} \times \mathbf{1}(Concrete + Symbolic)_{sc}$				0.132***
				(0.032)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct} \times \mathbf{1}(Symbolic + Program)_{sc}$				0.136***
				(0.040)
Observations	31,328	21,118	16,405	45,309
Mean Dep. Var.	0.415	0.423	0.402	0.422
Share of Protestants	0.383	0.464	0.404	0.443
State-Protestant-Cohort FE				
Event Time-Protestant-Cohort FE				
State-Event Time-Cohort FE				
Individual Controls				

Notes: This table tests for the heterogeneity of faith-based initiatives in terms of their type. Columns 1 to 3 are equivalent to the estimates in Figure 9, where we estimate aggregated coefficients in Equation (3) for the post-treatment period, and the treatment is having a faith-based initiative of the specified law as first initiative, compared to not having any initiative. Instead, in column 4, we use the full baseline sample of respondents and we saturate the post-treatment periods of Equation (3) by aggregating the post-treatment period interacted with protestants and all different combinations possible of first laws introducing faith-based initiatives. $\mathbf{1}(Concrete + Program)_{sc}$ is omitted because there are no cases in the observed cohorts and states. Regressions are estimated using weighted least squares with sampling weights and the weighting scheme proposed by Wing, Freedman and Hollingsworth (2024). Standard errors are clustered at the state level. *, **, and *** indicate significance at the 10%, 5%, and 1% level.
Result: Concrete laws and program laws (especially if paired with other types of laws), are those that matter the most in the role of faith-based initiatives on fueling religiosity.

Table A13: The impact of alternative treatments on church attendance

Dependent variable: Church attendance				
Complementary Treatment:	Liaison		Grant	
	(1)	(2)	(3)	(4)
$Treatment_{sc} \times Post_{ct}$	0.028		0.004	
	(0.017)		(0.012)	
$Treatment_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Protestant\right)_{isct}$		0.065*		0.054
		(0.036)		(0.032)
Observations	53,365	53,362	107,712	107,709
Mean Dep. Var.	0.421	0.421	0.432	0.432
Share of Protestants	0.489	0.489	0.499	0.499
State-Cohort FE	✓		✓	
Event Time-Cohort FE	✓		✓	
State-Protestant-Cohort FE		✓		✓
Event Time-Protestant-Cohort FE		✓		✓
State-Event Time-Cohort FE		✓		✓
Individual Controls	✓	✓	✓	✓

Notes: In this table, we show results of the impact of faith-based liaisons and federal appropriation bills on church attendance. As in Figure A25, we assemble a different sample for liaisons and grants in a fixed time window of ten years before and five years after the occurrence of the treatment, comparing to respondents in never treated states. Column 1 and 3 show estimates of a version of Equation (1) aggregated for the post period coefficients with the indicated alternative treatment. Column 2 and 4 show results from coefficients of Equation (3) aggregated for the post period coefficients with the indicated alternative treatment. Regressions are estimated using weighted least squares with sampling weights and the weighting scheme proposed by Wing, Freedman and Hollingsworth (2024). Standard errors are clustered at state level. **Result**: The alternative treatment, liaisons and grants, increase church attendance more for Protestants, but only (marginally) significantly for the liaisons. This is consistent with the critique that the initiatives did not provide the promised amount of funding.

Table A14: The impact of faith-based liaison activities on church attendance

Dependent variable: Church attendance												
	Liaison's Activities								Contacts with WH		FBL Position	
Initiative:	Conferences for FBOs	Website or e-mail listserv	Tech assistance seminars for FBOs	Grant writing programs for FBOs	Recruitment of groups for programs	Advisory board to focus on FBOs	Startup funds for FBOs	Network with state agencies and FBOs	Connected	Frequent	Governor's Office	State Agency
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Difference-in-differences with initiative heterogeneity												
$FBL_{sc} \times (Post \mid Initiative = 0)_{sct}$	0.024	0.035	0.034	0.036*	0.024	0.040**	0.028	-0.009	0.029	0.029	0.025	0.028
	(0.022)	(0.022)	(0.022)	(0.020)	(0.017)	(0.019)	(0.017)	(0.029)	(0.020)	(0.018)	(0.016)	(0.018)
$FBL_{sc} \times (Post \mid Initiative = 1)_{sct}$	0.030*	0.026	0.022	0.018	0.045	0.021	0.026	0.030*	0.027	0.025	0.029	0.028
	(0.017)	(0.018)	(0.018)	(0.020)	(0.034)	(0.019)	(0.024)	(0.017)	(0.018)	(0.021)	(0.019)	(0.018)
State-Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Event Time-Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Panel B. Difference-in-difference-in-differences with initiative heterogeneity												
$FBL_{sc} \times (Post \mid Initiative = 0)_{sct} \times \mathbf{1}(r = Protestant)_{isct}$	0.045	0.052	0.058*	0.052*	0.039	0.084***	0.043	0.118**	0.060*	0.032	0.060**	0.064
	(0.035)	(0.038)	(0.032)	(0.029)	(0.034)	(0.028)	(0.037)	(0.054)	(0.032)	(0.037)	(0.027)	(0.038)
$FBL_{sc} \times (Post \mid Initiative = 1)_{sct} \times \mathbf{1}(r = Protestant)_{isct}$	0.077*	0.067*	0.071	0.081	0.176***	0.056	0.123**	0.062*	0.066	0.130***	0.067	0.071**
	(0.045)	(0.038)	(0.050)	(0.057)	(0.038)	(0.044)	(0.046)	(0.036)	(0.041)	(0.043)	(0.041)	(0.030)
State-Protestant-Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Event Time-Protestant-Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State-Event Time-Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	53,362	53,362	53,362	53,362	53,362	53,362	53,362	53,362	53,362	53,362	53,362	53,362
my	0.421	0.421	0.421	0.421	0.421	0.421	0.421	0.421	0.421	0.421	0.421	0.421
Share of Protestants	0.489	0.489	0.489	0.489	0.489	0.489	0.489	0.489	0.489	0.489	0.489	0.489
Share of Initiative | FBL	0.631	0.823	0.552	0.496	0.209	0.653	0.290	0.946	0.741	0.342	0.734	0.170

Notes: Regression estimates of the impact of faith-based liaisons on church attendance conditional on the presence of a specific activity within the operating liaison. The sample is assembled on a fixed time window between ten years before and five years after, as illustrated in Figure A4 and descibed in Section 3.2. In panel (a) we estimate difference-in-differences separately for the post-treatment period between individuals in states that implemented a faith-based liaison and that either had a particular initiative or had not with the following saturated specification

$$y_{isct} = \beta^{i0} FBL_{sc} \times (Post \mid Initiative = 0)_{sct} + \beta^{i1} FBL_{sc} \times (Post \mid Initiative = 1)_{sct} + \sum_{j<\tau} \beta_j FBL_{sc} \times \mathbf{1}(t = j) + \delta_{sc} + \lambda_{ct} + \boldsymbol{X}'_{isct}\gamma + \varepsilon_{isct} \tag{C.3}$$

which is equivalent to Equation (2), but we are following both trajectories of having and not having the activity when treated, relative to not being treated in the latest year before the implementation of the faith-based liaison ($\tau = \{-2, -1\}$). Similarly, in panel (b) we estimate the triple difference version of the previous equation:

$$\begin{aligned} y_{irsct} = {} & \beta^{i0} FBL_{sc} \times (Post \mid Initiative = 0)_{sct} \times \mathbf{1}(r = Protestant)_{isct} + \beta^{i1} FBL_{sc} \times (Post \mid Initiative = 1)_{sct} \times \mathbf{1}(r = Protestant)_{isct} \\ & + \sum_{j<\tau} \beta_j FBL_{sc} \times \mathbf{1}(t = j) \times \mathbf{1}(r = Protestant)_{isct} + \delta_{src} + \lambda_{crt} + \alpha_{srt} + \boldsymbol{X}'_{irsct}\gamma + \varepsilon_{isct} \end{aligned} \tag{C.4}$$

Regressions are still estimated using weighted least squares with sampling weights and the weights proposed by Wing, Freedman and Hollingsworth (2024). Standard errors are clustered at the state level. *, **, and *** indicate significance at the 10%, 5%, and 1% level.

Result: The rise of church attendance is larger in states where the faith-based liaison engaged in recruitment groups for programs, had frequent contact with the White House, and engaged in startup funds for FBOs.

Table A15: Protestants' and Evangelicals' social views

Dependent variable:	Attitudes against				Conservative views	Bible prayer in schools
	Homosex	Women	Science	Abortion		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Panel A	Homosex	Women	Science	Abortion	servative	in schools
evan	0.15***	0.075***	0.069***	0.12***	0.088***	0.14***
	(0.010)	(0.008)	(0.011)	(0.011)	(0.006)	(0.011)
R-squared	0.22	0.13	0.031	0.061	0.040	0.11
Observations	39450	36651	40295	46962	54659	34036
Panel B						
Panel B						
Protestant	0.14***	0.023***	0.059***	0.040***	0.068***	0.14***
	(0.008)	(0.005)	(0.010)	(0.010)	(0.005)	(0.011)
R-squared	0.22	0.12	0.031	0.052	0.038	0.11
Observations	39450	36651	40295	46962	54659	34036

Notes: OLS estimates across individuals in the GSS. All regressions include year of survey and state fixed effects, state-specific trends, as well as individual controls for gender, marital status, and age. The independent variable is equal to one if the respondent adheres to affiliations defined as evangelical in panel A and to Protestant denominations more broadly in panel B. Robust standard errors clustered at the state level in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% level.
Result: Evangelicals in the sample are on average more likely than others to be skeptical towards homosexuals, working women, science, and abortion, have conservative political views, and to have preferences for Bible prayer in public schools and helping others. The broader group of Protestants in general are similar, except that they tend to have slightly weaker preferences for helping others.

Table A16: The impact of the faith-based initiatives on social views

	Social views against				Conservative views	Bible prayer in schools
	Homosex	Women	Science	Abortion		
	(1)	(2)	(3)	(4)	(5)	(6)
$FBI_{sc} \times Post_{ct}$	-0.005	0.027**	-0.018	-0.035**	-0.019**	0.021
	(0.016)	(0.013)	(0.018)	(0.014)	(0.009)	(0.026)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}(r = Protestant)_{isct}$	0.019	-0.054*	0.076***	0.097***	0.049***	0.013
	(0.020)	(0.027)	(0.026)	(0.018)	(0.018)	(0.033)
Composite Effect for Protestants	0.014	-0.027	0.058**	0.063***	0.030*	0.034*
	(0.017)	(0.022)	(0.024)	(0.017)	(0.015)	(0.018)
Observations	29,961	28,348	27,446	33,651	43,862	28,215
Mean Dep. Var.	0.620	0.189	0.551	0.608	0.165	0.533
Share of Protestants	0.451	0.434	0.436	0.443	0.439	0.442
State-Protestant-Cohort FE	✓	✓	✓	✓	✓	✓
Event Time-Protestant-Cohort FE	✓	✓	✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓	✓	✓

Notes: Regressions estimates of the impact of the faith-based initiatives on social views. This is a companion table for Figure A27 and Figure 10, where we show event study plots for the results on social views. Similarly, for each column we estimate a version of Equation (4) where we omit β^{pre} and the state by event time by cohort fixed effects to differentially estimate the impact of faith-based initiatives on the outcome overall and the marginal effect on Protestants compared to the non-Protestants baseline. The sample of respondents is assembled as described in Section 3.2 on a fixed time window of ten years before treatment and seven years after treatment, in line with the baseline sample in Figure 4. The composite effect for protestants is the t-test of the two coefficients for the triple differences. Regressions are estimated using weighted least squares with sampling weights and the weighting scheme proposed by Wing, Freedman and Hollingsworth (2024). Standard errors are clustered at state level.

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