Divine Policy[*]
The Impact of Religion in Government

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Abstract

How do strengthened church-state relations impact religiosity and social values? To examine, we exploit the staggered introduction of the faith-based initiatives across US states. Introduced by conservative Protestants in the 1990s, these policies aimed to improve conditions for faith-based groups and increase their numbers. Our difference-in-differences analysis reveals that the initiatives increased the number of faith-based nonprofits and strengthened religiosity and conservative-religious social views – such as attitudes against LGBTQ+ and abortion. 9% of Americans who were not regular churchgoers started attending monthly or more. A back-of-the-envelope calculation suggests that the faith-based organizations established as a result of the initiatives may have reached 4.9 million followers yearly. Effects are plausibly causal; we find no systematic differences prior to implementation, evidence is robust to using novel staggered roll-out designs, restricting comparison to contiguous counties, and to estimation based on triple differences exploiting religious group heterogeneity. Effects were only felt by Protestants, while the rest continued to secularize and modernize. Our results contribute to explaining US polarization and highlight consequences of tightened church-state relations.

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Charities and faith-based groups fill needs that no welfare system, no matter how well-designed, can possibly fill. Our nation needs men and women who rescue children from gangs, who tutor children in failed schools, who visit the sick and the dying. In times of personal crisis, people do not need the rules of a bureaucracy; they need the help of a neighbor.

Speech by President George W. Bush, February 26th, 2002.[1]

Grants or contracts with pervasively religious groups do aid, do promote, do foster religion with tax dollars, violating a core principle of the First Amendment. And in the process, those funds promote the theological assumptions, the spiritual message and the biases of the recipients.

Speech by Barry W. Lynn, the executive director of Americans United for Separation of Church and State, March 5th, 2003 (PewResearch, 2003).

1   Introduction

The US is notably religious among Western nations with faith deeply interwoven into the public sphere. At the same time, policies have been implemented that have strengthened relations between religion and government (Sager, 2010; Chaves, 1999; Lindsay, 2007). Among these policies, the faith-based initiatives had an explicit focus to "remove all unwarranted barriers inhibiting government partnerships with faith-based and grassroots charities," as stated in a White House report, "The Quiet Revolution" (2008). We examine whether this intensified presence of religion in government has influenced religiosity and conservative-religious social views.

 We focus on the faith-based initiatives, whose key objective was to expand the number of faith-based nonprofits and to strengthen their links to government. Furthermore, the main initiators – the Protestant evangelical voting bloc – have a particular focus on bringing Christian principles to bear on a range of social issues, such as abortion and same-sex marriage (Lindsay, 2007). Our hypothesis is that increasing the number of faith-based organizations would strengthen religiosity and associated conservative-religious views, both directly through participation in organization-sponsored activities and indirectly through expanding the religious sector. Examining the staggered introduction of faith-based initiatives in a difference-in-differences design, we document three sets of effects: i) religiosity rises, ii) conservative-religious social views and associated outcomes strengthen, and iii) the number of faith-based organizations increases. Only the beliefs and views of

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[1] https://georgewbush-whitehouse.archives.gov/news/releases/2002/02/20020226-11.html.

Protestants were affected, while the views of other denominations and the non-affiliated were unchanged, which would be consistent with social identity theory (individuals are more likely to update their beliefs in response to policies originating from their own in-group) or a disproportionate rise in the number of Protestant organizations.

The faith-based initiatives were introduced in the US from 1996 and on-wards.[2] They involve a series of policies designed to improve conditions for faith-based groups whom were seen as discriminated against in the past and thought to provide better for the needy than the state, illustrated by the opening quote by George W. Bush (Carlson-Thies, 2001; Cnaan and Boddie, 2002; Olasky, 1992; Sager, 2010; Wineburg, 2007). For instance, the initiatives allowed religious groups to deliver social services without having to alter their religious character, as opposed to previously where publicly funded social services could be delivered only in secular surroundings (Chaves, 1999). As a result of the initiatives, many states now have faith-based offices and task forces charged with bridging the gap between government and the faith-based community (Chaves et al., 2004; Sager, 2010). The initiatives raised concerns about proselytizing, illustrated by the second opening quote.[3] Some argued that the "first decades of the twenty-first century have witnessed an increased use of religious liberty as a warrant to justify conservative positions on social issues" (Jelen, Lewis and Djupe, 2018, 43). Others have noted that the initiatives were a cultural change that enhanced religion's visibility and influence in American public life, while strengthening the cooperation between church and state.[4] A particular concern was that the policies would favor conservative Protestants, particularly the Evangelicals, the dominant religious group among the initiators (Chaves, 1999; Sager, 2010). We set out to test these concerns empirically.

The staggered and top-down introduction of the faith-based initiatives provides a source of quasi-experimental variation in religion in government that we can leverage for causal identification. The initiatives were enacted from above by a few dedicated individuals in the political elite as a "quiet revolution" – as was the title of the White House status report from 2008 (Hein, 2014; Sager, 2010; Carlson-Thies, 2001; White House, 2002). In an interview about a bill passed in 1997, a previous advisor to George Bush explained that "it was done very quietly, because we didn't want to draw undue attention to it or spark a bitter church-state separation debate" (Sager, 2010, 42). Our empirical strategy allows us to test whether the initiatives were in fact unaffected by prior confounders.

We rely on the following datasets. To measure the staggered roll-out of the initiatives, we use information on 332 state-level executive orders on faith-based initiatives introduced

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[2]What we term faith-based initiatives include the Charitable Choice and the later faith-based initiatives.

[3]Wineburg (2007), `www.nytimes.com/2009/08/01/us/01beliefs.html`, `www.nytimes.com/2009/03/01/opinion/01jacoby.html?_r=1&pagewanted=all`

[4]Sager (2010, 5), Chaves and Wineburg (2010, 345), Wineburg (2007).

at different points in time across states between 1996 and 2009. These data were collected by Sager (2010) from the World's largest database on legal and public records-related information, LexisNexis. Our main measures of religiosity and attitudes are available for up to 44,758 respondents from the General Social Survey (GSS) during the period 1987-2018. These data also have county identifiers, which enable comparison of pairs of neighboring counties on either side of a state border. To identify mechanisms, we elicit information on 450,072 nonprofit organizations, various state-level information, and details about the types of initiatives and activities of the main initiators that drive results. To measure potential outcomes of changed attitudes, we use data on the timing of laws restricting gay marriage and measures of gender gaps.

We first test the impact on religious beliefs. In a staggered difference-in-differences model, we document that the initiatives increased church attendance and strengthened respondents' religious affiliation and beliefs. The research design allows us to rule out various confounding factors. First, we account for state-specific characteristics fixed in time (e.g., geographic or institutional factors); second, we address differences across time that affect all respondents in a similar way (e.g., certain macroeconomic fluctuations); third, we consider trends in religiosity or conservatism affecting states differently (e.g., states that implemented the initiatives earlier may be on different secularization paths than those implementing later).[5] There are no plausible differential trends in religiosity or the examined social values prior to the initiatives, addressing the concern that the initiatives were a response to rising religiosity (this refers to the examination of pre-trends). Fourth, in addition to state differences, we remove a large part of county differences (e.g., local geographic or institutional factors) by restricting analysis to comparison of pairs of counties on either side of a state border. Fifth, in a triple difference analysis, we exploit that effects are borne by Protestants. This specification leverages within state and year variation, eliminating the reliance on the state-level parallel trends assumption.[6] We address recent critiques of staggered difference-in-differences estimators by using novel "stacked" difference-in-differences designs throughout (Cengiz et al., 2019; Goodman-Bacon, 2021; Sun and Abraham, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Wing, Freedman and Hollingsworth, 2024).

We evaluate the size of the effects relative to the persuasion literature, which allows accounting for the fact that church attendance was already high before the initiatives. Our results suggest that 9% of Americans (who were not already attending church monthly or more often) started attending monthly or more often as a result of the initiatives. This

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[5]The listed confounding factors so far are taken into account with state fixed effects, time fixed effects, and state-by treatment-cohort fixed effects, respectively.

[6]The econometric specification of the latter includes state by event time fixed effects, see Braghieri, Levy and Makarin (2022) for a similar specification.

ranks above the median persuasion rate computed among the best research papers on persuasion by DellaVigna and Gentzkow (2010).

The faith-based initiatives were not an isolated event that affected American society in vacuum. They were part of a broader movement by evangelical Protestant lawyers, scholars, and politicians with influential positions in state and federal governments (Black, 2004; Formicola, Segers and Weber, 2003; Sager, 2010; Chaves et al., 2004). We do not aim to disentangle this bundle, but instead regard the faith-based initiatives as a shock to the presence of religion in government. The 9% rise in church attendance should be seen as a result of this strengthened presence of religion in government, not as an isolated impact of the faith-based initiatives alone. Furthermore, the initiatives were implemented at state *and* federal levels. To estimate causal effects, we focus on state-level variation. We thus consider our results to be conservative estimates of the true impact of the strengthened ties between religion and government.

A central component of the initiatives may help explain results. They aimed at increasing the number of faith-based organizations. When faith-based organizations face reduced regulations, better networking and grant opportunities, and favorable conditions in the competition for social service delivery, we would expect i) an increased number of faith-based organizations and ii) a higher likelihood of fulfillment of their mission. While the missions of these faith-based organizations can contain secular elements, they also include spreading the gospel, deepening knowledge of Jesus, evangelizing communities, and guiding youth to a Christ-centered lifestyle. This may strengthen religiosity and associated values directly through the evangelization, but also through religious market mechanisms: The increased "supply" of religion through a larger number of faith-based organizations would predict strengthened religiosity (Azzi and Ehrenberg, 1975; Iannaccone, 1998; Iyer, 2016). Further, some missions include direct references to conservative-religious values, such as promoting alternatives to abortion and encouraging men to take on leadership roles within the household, in line with the views of Evangelical initiators.

Evidence supports these mechanisms. First, using data on 450,072 US-based nonprofit organizations, we confirm that the initiatives increased the number of faith-based organizations. We define an organization as faith-based if it was categorized as religious by the NCCS or if religious terms entered its' name. To understand the size of the estimates, our back-of-the-envelope calculation shows that effects amount to 2258 additional faith-based organizations, which could have been in contact with 3.5% of American Protestants yearly. Second, we identify a rise in the number of congregations in an alternative dataset from ARDA.com. Third, the rise in religiosity is primarily driven by "concrete laws" that improve conditions for faith-based organizations. An example of these laws were the implementation of faith-based representatives – so-called liaisons – on state advisory

boards. These liaisons would, for instance, have weekly conference calls with faith-based liaisons from other states to facilitate the start-up of new programs. These programs could include reduced regulations for faith-based organizations or the faith-based support of soup kitchens in prisons, for instance. Also, the results are partly caused by "program laws", which involve the provision of government social services through faith-based organizations, such as the soup kitchens. Both concrete and program laws arguably increase the chances that missions of faith-based organizations are spread.

The faith-based initiatives allowed faith-based organizations to keep their religious symbols and messages when delivering government funded welfare services (Chaves, 1999). While this furthers the likelihood that religion is delivered as a by-product of social service delivery, it also gives rise to the concern that results are driven by a narrow segment of the population; the welfare recipients. We do not find support for this. Rather, the results seem to generalize to the full (Protestant) population. For instance, we do not observe larger effects for poorer or less educated individuals. Indeed, the impact of the faith-based initiatives reaches far beyond narrow social service delivery, as we shall see.

We proceed to identify the impact of the initiatives on social views associated with conservative-religious teachings. We examine using three types of data: The social views included in the GSS, data on gay marriage restrictions, and gender gaps computed using IPUMS data. We document that social views on average become more conservative, which is mainly driven by strengthened skepticism towards homosexuals and abortion, two key policy areas expressed as particularly important among Evangelicals (Lindsay, 2007). Furthermore, we detect rising gender gaps and more restrictions on gay marriage as a result of the initiatives.

Our results inform the debate on the interplay between religion and government. Religion and politics were historically intertwined in legal systems, religious persecution, and decisions on religious vs. secular education, for instance. While early thinkers, such as Weber, Marx, and Nietzsche, warned about the use of religion for power (Weber, 1905; Marx, 1844; Nietzsche, 1887), later empirical research confirms the use of divine legitimacy throughout history (Djupe and Calfano, 2013; Hertzke et al., 2018; Jelen, 2006; Bentzen and Gokmen, 2023; Chaney, 2013; Kuran, 2012; Platteau, 2017; Rubin, 2017).[7] As a consequence, some raise alarms over potential state-led proselytization (Finke, 1990; Monsma, 2000). In contrast, Durkheim posited that religion serves as a key pillar in shaping societal norms and ethics (Durkheim, 1912). In particular, religious groups have played a pivotal role in shaping American culture and policy (Putnam and Campbell, 2012; Finke and Stark, 2005). Wald and Calhoun-Brown (2014) argued that religion is a factor that must be considered to

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[7]Others have examined the link between the state and religious freedom in the past (Gill, 2008; Johnson and Koyama, 2019).

fully comprehend American political life.[8] The faith-based initiatives are an example of strengthened ties between religion and government and our results support the arguments that this coalition may strengthen the power of religion in society, influencing the views of followers.

Our analysis further contributes to explaining rising in polarization. Existing research has extensively documented the rising polarization in the United states along political lines (Poole and Rosenthal, 1984, 2001; McCarty, Poole and Rosenthal, 2016; Tucker et al., 2018; Campbell, 2018; Iyengar et al., 2019, among others). We examine polarization along religious lines. In our framework, polarization emerges not from partisan sorting but from the divergence in religious intensity: while some segments in the population embraced the religious messaging associated with the faith-based initiatives, others continued along a secularizing trajectory. This result can be reconciled through two complementary strands of literature. First, social identity theory suggests that Protestant followers would be more likely to update their beliefs in response to policies or signals originating from their own in-group (Turner, 1987; Bonomi, Gennaioli and Tabellini, 2021; Gennaioli and Tabellini, 2023). Second, in-group favoritism would lead Protestant evangelical initiators to disproportionately support faith-based organizations that aligned with their values, accelerating the growth of Protestant-affiliated organizations (Tajfel and Turner, 1979).

Our results also relate to research on the societal impact of religious missions and charities. Research has documented that religious missions benefit human capital accumulation and health in receiving developing nations, e.g. Valencia Caicedo (2019) (see review by Jedwab, Meier zu Selhausen and Moradi, 2022). Our contribution to this research is twofold: We examine the impact of missions in a Western nation and our main outcome is religiosity and conservative-religious views. Research has documented that missions raise religiosity in developing countries (Bryan, Choi and Karlan, 2021; Nunn, 2010). In his history of Christian missions, Robinson (1915) explains how building schools and hospitals was the most effective way for missionaries to convert locals to Christianity. Research has also documented proselytizing in Islamic charities (summarized in Kuran, 2018). Similarly to the faith-based initiatives, Kuran notes that the rhetoric of Islamic charities suggests that they prioritize needs of the poor. Research shows, though, that their beneficiaries belong mostly to the middle class, underscoring that poor relief may not have been the main priority. Confirming this tendency for Protestants, we find no signs that effects are caused by the poor.

We further relate to literature examining the drivers of differences in religiosity. The basic idea is that religiosity rises as the demand for religion or the supply of religion rises (see reviews in Iannaccone, 1998, Iyer, 2016 and Bentzen, 2021). To this research, we

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[8]See also Smidt, Kellstedt and Guth (2017) for an Oxford Handbook on religion and American politics.

add the insight that a government-sponsored higher supply of faith-based organizations strengthens religiosity and conservative-religious views. Perhaps the closest related paper to ours is Gruber and Hungerman (2008), who document falling church attendance across US states after the implementation of blue laws. Gruber and Hungerman interpret this as showing that religiosity falls as churches face increased secular competition (as shops are more likely to be open on Sundays). Our research documents the flip-side; religiosity rises when churches face lower competition in the sense that faith-based organizations face advantages relative to their secular counterparts. McCleary and Barro (2006) found that the presence of an official state religion is associated with higher religiosity in a panel of 68 countries. They argue that subsidies flowing to organized religion increase the supply of religion. We are able to examine these arguments empirically.

More generally, we contribute to a literature on the impact of laws on behavior and values. Gruber and Hungerman (2008) document that not only did the blue laws reduce church attendance, but they also increased drinking and drug use. Mocan and Pogorelova (2017), in their analysis of compulsory schooling laws in Europe during the 1960s and 1970s, identified a decrease in religiosity and superstition as a consequence of increased education. Other studies have revealed cultural backlash after certain laws, such as lower education levels and higher religiosity for Muslim girls after French bans of headscarves (Abdelgadir and Fouka, 2020), backlash after prohibiting German in US schools (Fouka, 2020), and after various US social policies during the past half-century (Wheaton, 2022). We document how certain policies may resonate more strongly with some segments of the population than with others, depending on their religion.

2 Background & Data on the Faith-Based Initiatives

Since the first policy under the faith-based initiatives was implemented in 1996, hundreds of faith-based policies have been implemented in the majority of US states. We will term them all "faith-based initiatives". The initiatives mainly dealt with relations between the government and so-called faith-based nonprofit organizations (FBOs), which include religious organizations (churches, mosques, synagogues, temples) and nonprofit organizations with a religious purpose. Although "faith-based and community organization" was the official rubric for the kind of organization targeted by the faith-based initiatives, initiative advocates, activists, and administrators envisioned congregations as a key type of faith-based and community organization (Chaves and Wineburg, 2010; Sager, 2010).

2.1 Rationale

The rationale behind the faith-based initiatives was two-fold: Faith-based groups i) were thought to be discriminated against in the past and ii) were seen as better at providing for the needy than the state (White House, 2008; Sager, 2010; Chaves, 1999; Formicola, Segers and Weber, 2003; Monsma, 2000). According to proponents, religion had been marginalized during the construction of the American welfare state. The Charitable Choice provision aimed to reverse this trend, ensuring that "religion would not be an alternative to government welfare but rather a supplement and even a partner" (Carlson-Thies, 2001, 110). The initiatives garnered further political support by highlighting the belief that faith-based organizations were better equipped to address the needs of the less fortunate compared to their secular counterparts (Carlson-Thies (1999); Cnaan and Boddie (2002); Sherwood (2000); Appendix A.1 lists examples of these main arguments). The majority of the public believed that closer collaboration between the government and religious groups would lead to better solutions for the social problems facing the United States, according to a public opinion poll from 1999 (U.S. General Accounting Office, 2002; Carlson-Thies, 2001). The main solutions offered by the faith-based initiatives were to increase the number of faith-based and community organizations, to relegate public welfare services through these organizations, and to remove barriers to partnership between them and the government.

2.2 Contents

The two largest groups of faith-based initiatives were the Charitable Choice provision and the Faith-Based and Community Initiatives (FBCI). They were implemented at the federal level and adopted at the state-level through executive orders. Our empirical analysis will exploit state-level variation, but understanding the federal initiatives is crucial for

understanding the initiatives adopted at the state-level.

The Charitable Choice provision – the first faith-based initiative – was part of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) signed into law by President Bill Clinton as part of the 1996 welfare reform. The provision had two main components. Charitable Choice enabled FBOs to receive federal funds without compromising their religious identity (Chaves, 1999). Previously, FBOs were expected to secularize their operations when providing publicly funded social services. In practice, this meant relaxing a series of regulations faced by FBOs. Furthermore, the provision required states that contract with nonprofit organizations for delivery of social services to include religious organizations as eligible contractees (Chaves, 1999).

The Faith-Based and Community Initiatives, implemented in 2001 by George W Bush, also reflected the President's belief that while government can marshal great resources in response to human need, it is best administered through faith-based groups (White House, 2008). The most successful elements of these initiatives, according to the 2008 White House status report "The Quiet Revolution", were:

(i) Executive orders aimed at "leveling the playing field," meaning removing all unwarranted barriers inhibiting government partnerships with faith-based and grassroots charities. These executive orders included creating the White House Office of Faith-Based and Community Initiatives (OFBCI), five initial Centers for Faith-Based and Community Initiatives within federal agencies, and an agenda for the initiatives that "ended discrimination against faith-based and grassroots nonprofit organizations" (White House, 2008, 29). These efforts included removing regulations, rules, and procurement that were seen as discriminating against FBOs.

(ii) Initiatives aimed at "vastly increasing the number of grassroots organizations partnering with government", including giving vouchers, mini-grants, training FBO leaders, and facilitating intermediary grantees.

(iii) President Bush implemented the FBCI in 11 federal departments that administer human service programs. According to the "Quiet Revolution" report, this has resulted in strengthened FBO partnerships relating to issues such as distressed neighborhoods (US Department of Commerce), education (US Department of Education), crime reduction (US Department of Justice), employment (US Department of Labor), homelessness (US Department of Housing and Urban Development), substance abuse (US Department of Health and Human Services), and food security (US Department of Agriculture).

(iv) The FBCI serves as a vehicle for the White House's compassion agenda. Examples include hosting monthly "Compassion in Action" events, the prisoner-reentry program

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where returning offenders are linked with FBOs, or connecting at-risk youth with compassionate adults through FBOs.

(v) Federal funding was sent to the states via direct grants to nonprofits (totaling $14 billion in 2006) and to block grants (totaling more than the nonprofit grants) to local governments. The block grants are used by governors who "recognize faith- and community organizations as key allies in addressing the state's most pressing needs". More than two-thirds of state executives leverage such partnerships through state FBCI offices or liaisons (described in Section 2.3).

(vi) Further initiatives were put in place to strengthen FBO-government relations at the state level. A main component in these initiatives were the faith-based liaisons.

2.3 Faith-based liaisons

The predominant means with which states adopted the federal initiatives was to create state-funded positions called faith-based liaisons (Sager, 2010). For instance, by 2008, 35 governors – 19 Democrat and 16 Republican – had offices or liaisons "dedicated to strengthening faith-based and community organizations" (White House, 2008). The primary policy goal of the liaisons was to link the faith-based organizations to the public social service sector (Chaves et al., 2004). Through interviews with 34 liaisons, Sager (2010) identified a three-part strategic plan of implementation by most liaisons. First, they worked to cut through the red tape between faith- and community leaders and the governor and state officials. Second, they developed clergy leadership advisory committees in which the clergy could meet face-to-face with the governor on a periodic basis to address issues and concerns of their cities. For instance, statewide interfaith steering commissions were formed, resulting in events such as prayer breakfasts. Third, the liaisons worked to pass on information from the federal agency center on faith-based initiatives and grant opportunities. Although the amount of money going to state faith-based appropriations has been increasing some, Sager (2010) found that the vast majority of the interviewed liaisons – 29 of 34 – had no fiscal support outside their salary. Instead, the main task of the liaisons was to encourage government employees to work toward the inclusion of faith-based groups in government-funded programs. The "liaisons became policy brokers and agents of cultural change" (Sager, 2010, 61).

2.4 Reduced regulations

A central component of the initiatives involved reduced regulations for FBOs. A prominent example is the Teen Challenge program, which offers Christian scripture-based drug rehabilitation (Sager, 2010). The Teen Challenge provided treatment to individuals struggling with drug addiction and thus fell under government regulations. In 1995, the Texas Commission on Alcohol and Drug Abuse threatened to shut down the Teen Challenge due to violations of state regulations. Critics argued that this was discrimination against faith. In response, the Teen Challenge Bill was passed in 1997, which granted religious treatment programs exemption from state regulation. These programs no longer had to meet standard criteria, such as employing licensed counselors, performing criminal background checks, adhering to state health and safety standards, or reporting incidents of abuse, neglect, or medication errors as mandated for secular treatment programs (Sager, 2010). Another example included in our dataset is Executive Order 13199, which aimed to eliminate "unnecessary legislative, regulatory, and bureaucratic barriers that hinder effective faith-based and community efforts to address social problems" (Sager, 2010, 32). This deregulation applied to both publicly and privately funded FBOs. In addition, the Charitable Choice provision allowed FBOs to maintain their faith-based hiring criteria (Sager, 2010; Jacobson et al., 2005).

2.5 Funding

The funding aspect of the initiatives has faced significant criticism due to the limited amount of funding that was allocated to faith-based organizations (Kuo, 2005; Chaves and Wineburg, 2010). The primary source of federal funding came from the Compassion Capital Fund (CCF), which had an annual budget of $30 million in 2002, increasing to $57.8 million in 2007 (Chaves and Wineburg, 2010, 344). This funding was distributed in the form of numerous mini-grants to faith-based and community organizations, with each grant amounting to a maximum of $50,000 (Chaves and Wineburg, 2010). While the initiatives may not have provided "the resources to change lives" (Kuo, 2005), these mini-grants may have been sufficient to increase the number of FBOs. Indeed, the majority of nonprofit organizations are smaller organizations, with over one million organizations having annual revenues below $50,000,[9] indicating that the mini-grants could cover start-up costs for a new FBO.

Non-congregational grantees were often encouraged or required to develop partnerships with small, local FBOs. This meant that FBOs could benefit from the initiatives, even without receiving funding themselves. In general, the faith-based initiatives were

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[9] https://www.statista.com/topics/1390/nonprofit-organizations-in-the-us/

designed to tap into the personal and holistic alternative given by congregations and other FBOs by redirecting resources from professional social service organizations (Chaves and Wineburg, 2010; Sager, 2010).

2.6 Evangelicals

The literature highlights the individual-specific nature of implementation. The initiatives were implemented from above by "a few dedicated individuals who strongly believe in the role of religion in social services" (Sager, 2010, 52). They "were pushed forward largely by evangelical Protestant lawyers, scholars, activists, and politicians" (Chaves et al., 2004, 45),(Carlson-Thies, 2001; Chaves, 1999; Sager, 2010). These evangelical activists held influential positions in both state and federal government and pursued the mission of including religion as an essential component of public policy and political life (Black, 2004; Formicola, Segers and Weber, 2003; Sager, 2010; Bartkowski and Regis, 2003; Conger and Green, 2002). Based on interviews with 34 liaisons, Sager (2010) found that these individuals were characterized by their dedication and religious commitment. They self-selected into their positions and had preexisting connections with predominantly conservative Christian religious communities. Additionally, they had personal contacts with the White House and faith-based leaders across various federal sectors. Other scholars have noted the evangelical roots of the initiatives (Lindsay, 2008; Monsma, 2006; Olasky, 1996; Smith and Emerson, 1998; Wright, 2009). For instance, among congregations in Atlanta, Georgia, Owens (2006) found that the attitudes of the clergy towards entanglement with the government and the ethnic composition of congregation members were the key predictors of willingness to seek public funding. Politicians in states that were more likely to implement faith-based policies differed in similar aspects. Some politicians simply found the idea of reintegrating religion into the public sphere appealing (Sager, 2010). In particular, Sager (2010) documents that states with evangelical Republicans among their politicians were more likely to implement faith-based initiatives.

2.7 A broader agenda

Some scholars argue that the faith-based initiatives were more than just an attempt at leveling the playing ground and caring for the needy. Instead, they were likely part of a broader agenda. Generally evangelical Christians have ascended into influential positions in American society, including politics and business (Lindsay, 2007; Sutton, 2014; Wilcox, 2018). Based on 360 interviews with Evangelical leaders in government and corporate America, Lindsay (2007) concludes that these leaders managed to raise in power, despite their low total number. Across a diverse America, these leaders created and expanded

a strong network held together by their common evangelical faith and their vision for how things ought to be in society. The faith-based initiatives may have served as part of their ambitious agenda of bringing "Christian principles to bear on a range of social issues" (Lindsay, 2007, 2). Indeed, the faith-based initiatives have been criticized for being aimed at increasing the presence of a certain brand of Christian religion in government rather than ameliorating the problems of poverty (Kuo, 2005; Kuo and Diiulio, 2008; Sager, 2010). Scholars argue that evangelicals have been the driving force behind debates over abortion and same-sex marriage (Lindsay, 2007; Wilcox, 2018; Fetner, 2008). In particular, opponents of the initiatives express concerns that they may result in discrimination against gay, bisexual, and transgender Americans.[10] We set out to test the actuality of these concerns empirically.

2.8 Heterogeneous treatment

Certain religious groups may have been particularly influenced by the faith-based initiatives. First, since the initiators mainly adhered to the Evangelical religious tradition, their messages may have resonated more with segments of the population adhering to the same tradition (see Turner (1987) on social identity theory, Bonomi, Gennaioli and Tabellini (2021) applied to identity politics). Second, if the initiatives disproportionately increased the number of faith-based organizations adhering to the same tradition as the initiators, this would further amplify the influence on these followers. Third, African American congregations were particularly likely to pursue charitable choice opportunities (Chaves, 1999). Chaves outlines two main reasons for this tendency. The barrier between church and state in African American religion is – culturally and institutionally – lower than in other religious communities in the US (Pattillo-McCoy, 1998). Also, clergy in predominantly black churches enjoy greater power than their counterparts in predominantly white churches to initiate and implement congregational programs of their choosing (Lincoln and Mamiya, 1990). We will test heterogeneity of the impact of the initiatives along these denominational lines.

2.9 Testable predictions

Based on the reviewed literature and the available data, we formulate three testable predictions regarding the impact of the faith-based initiatives, illustrated in Figure 1. We expect impacts of the initiatives on the number of FBOs, religiosity, and conservative-religious views. First, we expect the number of FBOs to rise. Increasing the number

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[10]E.g., https://www.nbcnews.com/feature/nbc-out/civil-rights-groups-wary-trump-s-latest-faith-based-initiative-n872031.

of FBOs was a direct policy goal by President George Bush. Several initiatives may have directly facilitated establishing and maintaining faith-based organizations, such as reduced regulations and distributing mini-grants. More indirect initiatives include representation on government boards and the relegation of social service delivery from secular providers to religious organizations. Inclusion of faith-based organizations on government boards meant that they could further influence decisions on regulations and grants. Also, providing social services through FBOs could potentially offer them additional revenue streams and greater visibility in their neighborhoods, enhancing their ability to establish themselves and sustain their operations.

[FIGURE 1 ABOUT HERE.]

Second, we would expect the faith-based initiatives to increase the likelihood that FBOs achieve their mission. The majority of the faith-based organizations are ministries and churches, whose missions include spreading the word of Jesus and the Bible and to evangelize, for instance. We list examples of these missions in Table 1. These missions are obtained through the Nonprofit Explorer on the ProPublica website, `https://projects.propublica.org/nonprofits/`. We anticipate that this mission will reach a greater share of the population as a consequence of the faith-based initiatives for four key reasons: i) The growing number of faith-based organizations allows them to reach a broader segment of the population ii) the increased social service delivery through FBOs would extend their reach among recipients of these services, iii) permitting FBOs to provide these services while maintaining their religious identity would allow them to promote their mission to the social service recipients, iv) having representatives from FBOs on government boards would enhance their ability to suggest further initiatives to strengthen their position.

In other words, we would expect religiosity to rise. The link from an increased supply of religion (a greater number of FBOs or a better quality of their "product") to strengthened religiosity follows predictions by standard religious market models. Azzi and Ehrenberg (1975) model how individuals allocate their time and resources between religious and secular activities to maximize their current - and afterlife utility (Iannaccone, 1998; Iyer, 2016). A rising number of churches or a better quality of the "product" provided by churches can improve the match between potential followers and religious options by improved access to religious practices and by making the product more attractive. Consequently, this may intensify religious activity among followers and potentially strengthen their conservative-religious views. These predictions are further supported by existing research documenting rising religiosity as a consequence of Christian missions in the Philippines (Bryan, Choi and Karlan, 2021) and historical Africa (Nunn, 2010). We test whether something similar has occurred in a Western society.

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Third, we expect certain social values to be influenced. While many missions of faith-based organizations involve conveying religious messages, another aspect focuses on influencing social values. Particularly promoting and preserving traditional family structures. By now a well-known example is USATransform (Ziklag), whose mission includes "To educate and encourage individuals and organizations to advance positive outcomes in American culture". The organization follows a framework called the Seven Mountain Mandate, whose first two mandates include promoting a Christian worldview and advocating the continuation of traditional family structures. Other examples include the First Choice Center for Women, whose mission includes "To see the word of God birthed in the hearts of men and women and to make abortion unnecessary and undesirable in our region" and the Oaks of Righteousness whose mission includes "Development of male based curriculum for leadership in the home." If these missions are representative, we would expect a strengthening of traditional family values and views against abortion. These latter examples involve the faith-based organizations as a central transmitter of the particular social views. We could also imagine a more direct impact on social views of the evangelicals in government. Much like how symbolic policy spreads messages. In particular, advocates of the initiatives argue that they have been beneficial in reintegrating the faith-based voice into the public sphere (Sager, 2010).

[TABLE 1 ABOUT HERE.]

We set out to test these predictions empirically.

2.10 Data on the faith-based initiatives

State governments were not obliged to adopt policies endorsing the faith-based initiatives unless they explicitly discriminated against faith-based organizations (Sager, 2010). As a result, the implementation varied in terms of timing and extent across different states. For our primary analysis, we rely on data compiled by sociologist Rebecca Sager, which tracks the legislative changes and executive orders related to Charitable Choice and the subsequent faith-based initiatives issued by state governments between 1996 and 2009. This data was sourced from LexisNexis, an electronic database known for its comprehensive collection of legal and public records.[11] For robustness, we use data on the key institutions

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[11]For further details on the data collection, consult Appendix B.1 and Sager (2010). We refrain from attempting to add recent years to the data for two main reasons: First, the extraction from Lexis Nexis includes several choices to be made such as which exact terms to include etc. Using data collected by Sager is a way to secure objectivity. Second, if we were to add more recent years, more states would have adopted the faith-based initiatives, which means that the pool of never treated states shrinks, which again increases potential bias from staggered treatment (De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021). In 2009 when our data ends, 44 out of 50 states had adopted one or more faith-based initiative.

associated with the faith-based initiatives, such as the presence of faith-based liaisons, faith-based offices within state governments, and their grants (Table 3). For identification purposes, we focus on state-level changes, but the faith-based initiatives also involved federal laws, which may have had similar effects. From this perspective, our results can be considered conservative estimates.

Table 2 presents the legislative changes grouped into *Program laws* that involve the provision of government welfare through faith-based organizations, *Concrete laws* that improve overall conditions for faith-based organizations and enhance their access to government in general, and *Symbolic laws* that foster a supportive environment for faith-based organizations. Most of the program laws involve delivery of government welfare through faith-based organizations operating within correctional facilities. The concrete laws allocate positions to faith-based representatives on state advisory boards and distribute appropriations to faith-based organizations. For instance, Florida passed appropriation bills for teenage pregnancy prevention, while Ohio allocated funds for addressing child poverty and reducing out-of-wedlock births. The majority of the symbolic laws encourage government officials to collaborate with faith-based organizations. For instance, in 2004, Wyoming enacted a law stating that "The Department of Family Services shall develop a comprehensive plan to improve the lives and future of all children and families in Wyoming. In developing the plan, the Department shall collaborate with the business councils, state and local agencies, and private groups, services providers and businesses, including FBOs" (Sager, 2010, 99). The different types of laws are described in more detail by Sager (2010, ch.4).

[TABLE 2 ABOUT HERE.]

The first state to implement Charitable Choice policies was Texas in 1997 during the tenure of George W. Bush, driven by his belief in the efficacy of religious groups in addressing societal needs (Sager, 2010, 47). Another front runner state was Florida, governed by Jeb Bush, who shared a similar political inclination towards the initiatives as his brother, George W. Bush.[12] By the year 2009, a total of 44 states had implemented at least one faith-based initiative (panel (a) of Figure 2). Panel (b) of Figure 2 shows the spatial distribution of the total number of faith-based initiatives implemented by 2009. The map reveals that these initiatives potentially mattered everywhere and were not restricted to certain states. Figure A1 depicts instead the first year of implementation, revealing a similar spread across states.

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[12]The implementation experiences in these two states may diverge from those of other states and a concern is that this impacts results. Results are robust to excluding them. The working paper version of this paper (Bentzen and Sperling, 2020) excluded the states from the entire analysis with no change to the conclusions.

[FIGURE 2 ABOUT HERE.]

Regarding the timing of the different types of initiatives, concrete laws were always implemented before or in the same year as the program laws and most often before the implementation of symbolic laws (Figure A7).

3   Empirical Analysis

Our empirical analysis begins with modeling the impact of the faith-based initiatives on religious attendance and beliefs. To do so, we combine the data on faith-based legislative changes with the General Social Survey (GSS). The GSS has asked Americans about their socio-economic characteristics, social views, and practices, including their religious attendance and beliefs since 1972. The data includes state identifiers since 1973. The survey was conducted yearly until 1994 and in even years thereafter. To examine mechanisms, we use yearly information on 450,072 nonprofit organizations. We explain these data in Section 4.

3.1   Main religiosity measure

The GSS measure of religious attendance is based on answers to the question "How often do you attend religious ceremonies?" Respondents can answer never, less than once a year, about once or twice a year, several times a year, about once a month, 2-3 times a month, nearly every week, every week, or several times a week. The original variable assumes values between 0 and 8, which we recode to values (increasing in steps of 0.125) between 0 and 1 to ease readability of results. An attendance score of 0.5 corresponds to attendance once a month, for instance. The GSS holds various measures of the strength of religious beliefs, whereof we choose the four measures available for at least 20,000 respondents (detailed below). All measures are described in detail in Appendix B.

3.2   Staggered treatment

Since treatment (having implemented a faith-based initiative) occurs at different points in time for different states, we would face potential issues with heterogeneous treatment effects if we were to estimate the relation between the faith-based initiatives and religiosity in a simple two-way fixed effects model (Goodman-Bacon, 2021; Sun and Abraham, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021). The problem arises since previously treated states would enter the comparison group for later-treated states. If the impact of the treatment is larger for early-treated states, the two-way fixed

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effects estimates would be biased towards zero, whereas they would be overstated if the impact is larger for later-treated states.

To address this, we employ a design known as *stacked* difference-in-differences. The methodology follows the logic of alternatives, such as Callaway and Sant'Anna (2021), but allows us to examine individual- and organization-level data treated at a more aggregate level, examine individual-level treatment heterogeneity with respect to religious denominations, and to include survey weights at individual level.[13] First seen in Guryan (2004) and gaining attention with Cengiz et al. (2019), the *stacked* difference-in-differences methodology has been exploited in several recent empirical applications (Gormley and Matsa (2011), Deshpande and Li (2019), Fadlon and Nielsen (2021), and Jeffers (2024), among others). The intuition is to assemble a set of sub-experiments – or sub-datasets – with respect to the treatment timing such that, within each treatment cohort, we compare units that are treated in the same year to units in a control group that remain untreated throughout the same event time window. Two states belong to the same treatment cohort if their first faith-based initiative was implemented in the same year. We end up with a sub-dataset for each treatment cohort. Since the faith-based initiatives were implemented every year between 1997 and 2009, we will have a sub-dataset for each of these years.

In each of these sub-datasets, we trim the data to be balanced with respect to the treatment unit (the state) over a fixed event time window, amounting to 10 years before treatment to 7 years after in our baseline specification. Each sub-dataset consists of states that are treated in the particular treatment cohort and the never treated states. Next, we stack the different sub-datasets into one dataset, which consists of all treatment cohorts with their respective comparison group, aligned over the same treatment timing. Since our specification always includes treatment-cohort fixed effects, we are exclusively comparing states treated in the same year with the control group (never-treated states in the baseline). This means that our treatment effects will be conceptually equivalent to treatment effects estimated for each treatment cohort using a linear regression applied to the sub-datasets (Gardner et al., 2024; Jeffers, 2024; Wing, Freedman and Hollingsworth, 2024).

To address the concern raised by Wing, Freedman and Hollingsworth (2024) that the stacked estimators using a fixed effects specification do not identify the target aggregate causal effects of all treatment cohorts, we apply their suggested weighting scheme throughout the analysis. The results are robust to estimation without these weights (Figure A8). Consult Appendix B.4 for details on the stacking and weighting.

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[13]The results are robust to using various alternative estimators robust to staggered treatment by Callaway and Sant'Anna (2021), Borusyak, Jaravel and Spiess (2021), De Chaisemartin and d'Haultfoeuille (2020), and Sun and Abraham (2021), or the simple two way fixed effects estimator (see the working paper version of this paper).

3.3 Sample and period restrictions

Throughout the analysis, we exclude District of Columbia as it is not a state and we have limited information on individuals located in the state from the GSS. The GSS panel is unbalanced in the sense that surveyed individuals are not always representative of all states in all years. We balance the panel ensuring that each state is present before and after treatment in each treatment cohort c.[14] This restriction means that we further loose Hawaii, Maine, New Hampshire and Utah in the GSS analysis. Figure A2 and Figure A3 (described in detail in Appendix B.4) illustrate the sample composition of the GSS baseline data by treatment cohort and the data structure of the final stacked data, respectively. In this sample of states, the last state to be treated in our GSS analysis is Washington, which is treated in 2006, meaning that 2006 is our latest treatment cohort. Since we analyze treatment in event times from 10 years before treatment to seven years after and since the GSS is conducted only in even years after 1994, our latest calendar-year observation is 2012 in the baseline GSS specification. Since the data on faith-based initiatives ends in 2009, results are potentially downward biased in the last three years of the analysis, as some of the never-treated control states potentially became treated in the meantime. When analyzing data for nonprofit organizations, we have data for all years and states, meaning that the last state is treated in 2009 (Maine). In that analysis, 2009 will be the last treatment cohort and 2016 will be the last calendar-year in the baseline analysis.

3.4 Event study DD estimates

To estimate treatment effects, we compare changes in outcomes of individuals located in states adopting the faith-based initiatives before and after their introduction to changes in outcomes of individuals in states that did not implement any faith-based initiatives. We formally estimate the effect of the introduction of faith-based initiatives using the following event-study specification:

$$
y_{isct} = \sum_{\substack{j \in T \\ j \neq \tau}}^{T} \beta_j FBI_{sc} \times \mathbf{1}\left(t = j\right) + \delta_{sc} + \lambda_{ct} + X'_{isct}\gamma + \varepsilon_{isct} \tag{1}
$$

where y_{isct} is the observed outcome for an individual i interviewed in state s at event time t and belonging to the treatment cohort c. FBI_{sc} is an indicator variable equal to one when

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[14]Results are robust to further balancing the sample by excluding 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, Table A9.

a faith-based initiative was implemented in state s in treatment cohort c, zero otherwise.[15] $\mathbf{1}(t = j)$ is an indicator variable equal to one j years after treatment within the treatment cohort. T is the set of event time periods considered for the event study, where we omit τ, the event time just before the introduction of the faith-based initiatives.

The coefficients of interest, β_j, identify the effect of the faith-based initiatives at event time j, relative to the event time just before treatment. The comparison group in the baseline specifications are the never treated states, which include Delaware, Nebraska, New York, Rhode Island, South Dakota, and Vermont in the baseline specifications, cf Figure A2 (the never-treated states are different when we evaluate treatment by liaisons or grants, cf. Figures A5 and A4). Results are robust to including the not-yet-treated states in the comparison group or to restricting the comparison group to the not-yet-treated (Figures A10 and A11).

δ_{sc} are state by treatment cohort fixed effects, meaning that we compare only changes in outcomes for individuals that are treated in the same treatment cohort and state. For instance, we only compare individuals in states who implemented their faith-based initiative in 1997 to the never treated states measured in the same event time window. λ_{ct} are treatment cohort by event time fixed effects, meaning that we compare only changes in outcomes within one treatment cohort and event time. X_{isct} are individual-level controls for respondents' age, marital status, gender and religious denomination, and additional controls for robustness in some cases (*e.g.* education, political preferences, ethnicity, migration status, or public spending at state level, see Tables A3 and A7). Throughout, we cluster standard errors at the state level, the level of our treatment variation (results are robust to clustering at the state by treatment cohort level). We address potential spatial correlation by accounting for the spread of the faith-based initiatives in neighbor states (Table A10). Throughout, we estimate the equation using ordinary least squares with the weights proposed by Wing, Freedman and Hollingsworth (2024) for the identification of the correct aggregated ATTs and sampling weights for the GSS waves. The main results for categorical variables (*e.g.* church attendance) are robust to using a ordered logit (Table A6).

We estimate aggregate treatment effects with the equation:

$$y_{isct} = \beta^{pre} FBI_{sc} \times Pre_{ct} + \beta^{post} FBI_{sc} \times Post_{ct} + \delta_{sc} + \lambda_{ct} + X'_{isct}\gamma + \varepsilon_{isct} \tag{2}$$

where $Post_{ct}$ is an indicator variable equal to one from the year of treatment onwards, zero

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[15]The year of interview is the most detailed timing information available. The choice of a dummy variable instead of the actual number of laws is based on the fact that the individual legislative changes vary greatly in strength and it is not clear whether ten small laws should have a larger impact than one large one. The choice follows Gruber and Hungerman (2008) and Autor (2003). The results are robust to using the number of laws instead (Table A11).

otherwise. Pre_{ct} is an indicator variable equal to one for all time periods before treatment except for the latest time period prior to treatment, zero otherwise. In this specification, we compare effects to event time τ, the latest period before the year of introduction of the faith-based initiatives, mimicking the event study specification in Equation (1). $FBI_{sc} \times Pre_{ct}$ and $FBI_{sc} \times Post_{ct}$ are the interactions between the faith-based initiatives and the Pre- and $Post$-dummies, respectively. β^{post} captures the post effect, relative to event time τ. Note that β^{post} captures the post effect relative to the full pre-period average if we omit the $FBI_{sc} \times Pre_{ct}$ term (as done in some robustness checks).

Specifications in Equation (1) and (2) take account of all state-level counfounders, either constant or varying across treatment cohorts. For instance, general prior state-level religiosity, poverty, public spending, etc. is accounted for with the state fixed effects and changes in these confounders within a state across treatment-cohorts are accounted for with the state by treatment cohort fixed effects.

Since we compare treated states to the never-treated states at the same event time within each of the treatment cohorts, omitted confounders can explain the DD results only if they are caused by events that occur at the same time as the faith-based initiatives. This is indeed plausible since the faith-based initiatives were likely part of a larger package of initiatives with the potential purpose of strengthening the same values as we examine. This is our reason for regarding the faith-based initiatives as a proxy for this movement, not as a precise measure of implementation of the faith-based initiatives. The identifying assumption in the DD specification is thus that the average change in outcome in the post-treatment time period would have been the same in individuals located in states with faith-based initiatives (or the associated bundle of initiatives) and individuals located in states without them, had it not been for the faith-based initiatives (or the associated bundle of initiatives). We validate this assumption in three broad ways. First, we examine whether states differed systematically prior to treatment (examining pre-trends). Second, we examine the differential timing of other associated events such as implementation of faith-based liaisons and the welfare reform (Section 3.6 and Tables A7 and A3). Third, we exploit the heterogeneity of treatment to estimate a triple difference (DDD) model.

3.5 Event study DDD estimates

Drawing on existing literature, we anticipate that the initiatives were more likely to impact Protestants, as this group includes Evangelicals–the primary proponents of the initiatives–and Black Protestants–the earliest religious group to embrace the faith-based initiatives. To allow for such heterogeneous treatment effects, we estimate the following triple difference event study specification

$$y_{irsct} = \sum_{\substack{j \in T \\ j \neq \tau}}^{T} \beta_j \, FBI_{sc} \times \mathbf{1}\,(t = j) \times \mathbf{1}\,(r = Protestant)_{isct}$$

$$+ \, \delta_{src} + \lambda_{crt} + \alpha_{srt} + X'_{irsct}\gamma + \varepsilon_{irsct} \tag{3}$$

and its equivalent aggregate model:

$$
\begin{aligned}
y_{irsct} = \; & \beta^{pre} FBI_{sc} \times Pre_{ct} \times \mathbf{1}\,(r = Protestant)_{isct} \\
& + \beta^{post} FBI_{sc} \times Post_{ct} \times \mathbf{1}\,(r = Protestant)_{isct} \\
& + \delta_{src} + \lambda_{crt} + \alpha_{srt} + X'_{irsct}\gamma + \varepsilon_{irsct}
\end{aligned}
\tag{4}
$$

where y_{irsct} is the outcome for an individual i belonging to the religious strata $r = \{\text{Protestant}, \text{non-Protestant}\}$ interviewed in state s at event time t and belonging to the treatment cohort c. Here, we allow the impact of faith-based initiatives to differ between Protestants and the rest with the indicator variable $\mathbf{1}\,(r = Protestant)_{isct}$ equal to one if the individual is a Protestant, zero otherwise. Protestants take up 56% of the sample, Catholics 24%, the unaffiliated 12%, and remaining groups 8%.

The coefficients of interest, β_j, capture the average change in outcomes for Protestants in states that implemented faith-based initiatives compared to Protestants in states that did not in the event time period j, relative to the average change in outcomes symmetrically for non-Protestants, with respect to the period before treatment. δ_{src} are state by religious strata by treatment cohort fixed effects, where the strata is being Protestant or not, meaning that we compare only changes in outcomes of Protestants in the same state treated with their first faith-based initiative in the same year. λ_{crt} are event time by religious strata by treatment cohort fixed effects, meaning that we compare only changes in outcomes for religious strata within the same treatment cohort and event time. α_{srt} are state by religious strata by event time fixed effects, assuring that we account for all variation across states, time-varying or not, for Protestants and non-Protestants separately. This includes state-level changes in religiosity for Protestants and non-Protestants prior to the initiatives, for instance. Average treatment effects are thus exclusively computed by differential effects of the faith-based initiatives for Protestants versus the rest.

The identifying assumption for the triple difference estimation is that the common trends assumption holds for both religious strata r (Protestants and non-Protestants), meaning that the average change in all post-treatment periods would have been the same between individuals with and without faith-based initiatives, had it not been for the faith-based initiatives. Alternatively, any potential violation of parallel trends should be

constant across strata, meaning they will cancel out with the triple difference (Strezhnev, 2023). We offer plausible evidence of parallel trends for the main results by taking the split sample event studies for both strata and showing pre-treatment coefficients for both groups.

A concern relating to the heterogeneous treatment model in Equation (3) is that the faith-based initiatives potentially influenced the composition of religious denominations. This would affect the interpretation of the results, since part of the impact on outcomes might cover denominational shifts. We do not find support for this when estimating Equation (1) with indicator variables for the different denominations as dependent variable. The faith-based initiatives did not influence the likelihood that respondents identify as Protestant or Catholic, nor did they impact the share of the population who identify with a religious denomination in general (Figure 3). This general absence of a shift is consistent with previous research findings. Other shocks such as earthquakes have been shown to increase religiosity, but even these large shocks also do not persuade nonbelievers, cf. Bentzen (2019). There is a tendency for the share of Protestants to fall and the share of Catholics to rise 6-7 years after treatment. This would be consistent with some Protestants not agreeing with the message by the faith-based initiatives and switching. This also means that we should be careful with the interpretation of the results in the last event time period as we move forward.

[FIGURE 3 ABOUT HERE.]

3.6 Validity checks of the FBI measure

Before we start our analysis, we conduct the following checks of the validity of our main measure of the faith-based initiatives – the executive orders. First, we exploit data across states and years on the main institutions behind the initiatives – the faith-based liaisons (FBL). These data were constructed by Sager (2010) based on interviews with the liaisons in 2006. In addition, Sager (2010) provides information on whether or not the state had a Office of faith-based initiatives (OFBCI) by the time of interview (2006) and the size of the grants held by the particular liaison by the time of interview. From these data, we construct three variables: FBL_{st}, which is an indicator variable equal to one from the year the state had their first faith-based liaison onwards, zero otherwise, $FBL_{st} \times OFBCI_s$, where $OFBCI_s$ is an indicator variable equal to one if the state had an OFBCI by 2006, and $FBL_{st} \times grant_s$, where $grant_s$ is the size of the grants of the liaison by 2006. We find that our measure of the faith-based initiatives predicts the implementation of liaisons, offices, and their grants (Table 3). The specification is equivalent to Equation (2) without β^{pre},

where the unit of observation is instead state-years and where we replace the dependent variable with the institutions and their grants.

Second, we check the timing of our main measure and the faith-based liaisons (Figure A6). The timing of the two is highly correlated. Further, for the vast majority of states (36 of 41 for which we have data on both), a faith-based initiative was implemented before a faith-based liaison, indicating that the faith-based initiative is the relevant treatment indicator.

Third, we use more detailed budget data available for 2006 from the White House official website.[16] Since these data are only available for one year, results are cross-state correlations. We find that states that implemented the faith-based initiatives earlier or implemented more initiatives received more government funding for community- and faith-based organizations and through two of the largest initiatives– the Compassion Capital Fund and the Mentoring Children of Prisoners (Table A1). The results hold when accounting for regional fixed effects as a catch-all for broad state-differences.

Fourth, we document that our main results are robust to using instead the timing of implementation of the liaisons as treatment as part of our examination of mechanisms in Section 4, where we will also exploit additional information on the liaisons to elicit mechanisms.

[TABLE 3 ABOUT HERE.]

We take these results as reassuring in terms of the validity of our main measure of the timing of the faith-based initiatives.

3.7   Main results

To estimate the impact of the faith-based initiatives on church attendance, Figure 4 presents the main event study results of the DD and DDD analyses based on Equations (1) and (3) with church attendance as dependent variable. Baseline controls for gender, age, marital status, Protestant denomination are included throughout as well as state by treatment cohort and cohort by event time fixed effects in the DD estimates and state by religious strata by treatment cohort, cohort by religious strata by event time, and state by religious strata by event time fixed effects in the DDD estimates are included throughout. Panel (a) shows the DD estimates and panel (b) the DDD estimates. Panel (c) shows the DD estimates for Protestants and non-Protestants separately. For the aggregate treatment effects in a table, including controls consecutively, consult Table A3.

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[16]https://georgewbush-whitehouse.archives.gov/government/fbci/qr6.html

Analysis of pre-trends. Crucially for our ability to estimate causal effects, none of the panels show signs of pre-trends, meaning that states do not differ systematically prior to treatment. For any omitted confounders to explain the relation between the faith-based initiatives and church attendance, they would have to correlate with church attendance and the roll-out of the faith-based initiatives, given the control variables and fixed effects. The lack of pre-trends indicates that church attendance does not vary in any systematic way prior to treatment, ruling out significance of such omitted confounders. If anything, states may be on a declining trend prior to treatment, compared to states that are never treated (although this is highly insignificant, panel a). Thus, if anything, our DD estimates can be regarded as lower-bound estimates.

This lack of pre-trends is consistent with the literature on the faith-based initiatives. In particular, there are a priori reasons to believe that the initiatives were not a response to population-level characteristics. First, religiosity of the initiators did not necessarily reflect religiosity of the population. In his description of the faith-based initiatives, Carlson-Thies (2001) observes that "Ironically, this legislative effort was not the result of pressure from the faith communities, nor did it receive much initial support from them." Religiosity of the Congress is also not associated with religiosity of the population. For instance, a 2017 study by PewResearch (2017) found that despite around a quarter of the population reporting religious unaffiliation, only one congressman identified as religiously unaffiliated. Second, the fact that these initiators focused on implementing the initiatives quietly to "not spark a bitter church-state separation debate" (Sager, 2010, 42), illustrates the rather idiosyncratic nature of the initiatives. Third, even if implementation was a response to population level religiosity, it is not obvious whether implementation would be *more* or *less* likely in states with more religious populations. Intuitively, one could expect religious states to be more eager to embrace faith-based initiatives. However, in reality many faith-based organizations were reluctant to seek government funding out of fear of secularization (Chaves, 1999; Sager, 2010). The director of the Washington Office of the Presbyterian Church (USA), for example, wrote that Charitable Choice "will harm religion's historic autonomy from government" and "cause religious institutions to be subject to government oversight and regulation" (Chaves, 1999).

[FIGURE 4 ABOUT HERE.]

We move on to interpret the impact of the initiatives on church attendance. Panel (a) of Figure 4 shows that the initiatives increased the overall level of church attendance only weakly (the aggregate post-effect is significant at the 10%-level). Panels (b) and (c) reveal that this effect is driven exclusively by rising church attendance among Protestants. Church attendance is unaffected for the non-Protestants, consistent with the idea that

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the faith-based initiatives had a strong ideological component, which may not speak to the minds of people of different ideologies. Generally, laws that relate to ideological preferences may influence opponents differently than those who endorse the particular ideology (Abdelgadir and Fouka, 2020; Wheaton, 2022; Fouka, 2020). Further splitting the sample, we find that the rise in church attendance occurs exclusively among Evangelicals and African American Protestants, in line with the arguments of the reviewed literature that Evangelicals were among the initiators and African American churches were early to embrace the faith-based initiatives (Figure A12 and Table A4). The aggregate treatment effect of the faith-based initiatives on church attendance for Protestants is 0.086 (cf notes Figure 4). This means that having implemented one or more faith-based initiatives increased church attendance among Protestants by 8.6 percentage points, compared to the rest. This amounts to 20% of the average state-level church attendance or more than five times the average fall in church attendance during the period 1996-2012. Comparing the standardized betas (not shown), the impact size amounts to 70% of the difference in attendance rates between men and women. The effects are thus both statistically and economically important.

Post-treatment trajectory. Gauging the trajectory of the post effects in Figure 4, Protestants' church attendance rises in the first year after implementation, increases to the highest level 2-3 years after implementation, and stays nearly at this level throughout the period. This is consistent with the fact that the faith-based initiatives involved permanent institutional changes. Attempts to estimate additional post effects are challenged by the fact that the states in our never treated group probably get treated at some point after 2009, which will pull the estimate towards zero. We add two additional event-years in Figure A13, meaning that we estimate effects up to year 2014 for the latest-treated states. The impact on church attendance drops to become insignificant, which may be due to a wearing off of the impact of the initiatives or the fact that our data on the faith-based initiatives ends in 2009.

Non-linearities. The measure of church attendance is a categorical variable that increases in the intensity of church attendance. Estimating instead the impact of the initiatives on the separate church attendance categories reveals meaningful shifts between the categories. The initiatives pushed Protestant never- or annual churchgoers into attending monthly or weekly (Figure A14 and Tables A5-A6). The results are robust to using probit or logit instead of weighted-least squares (Tables A5-A6).

Contiguous county analysis. To further limit the set of potential omitted confounders, we restrict the sample to counties that neighbor a state border and compare counties in pairs on either side (Dube, Lester and Reich, 2010; Spenkuch and Toniatti, 2018). In this exercise, we thus compare respondents in arguably highly similar counties, only separated

by a state border. This is a highly restrictive exercise, since the GSS survey is far from conducted in all years in all counties. To increase the sample size, we add the not-yet-treated states to the comparison group. To estimate treatment effects, we pair respondents in treated counties with respondents in the comparison group (counties in states that never adopted the faith-based initiatives or implemented them after the considered comparison period). This restricts the sample to 28 county-pairs in 11 states, which makes it infeasible to estimate an event study specification as in Equation (1). Instead, we test for differences in church attendance with the following specifications, analogous to Equation (2) and (4) without β^{pre} and in a county-pair design:

$$y_{iknt} = \alpha_k + \delta_{nt} + \beta FBI_{kn} \times Post_{nt} + X'_{iknt}\gamma + \varepsilon_{iknt} \tag{5}$$

$$y_{irknt} = \alpha_{rk} + \lambda_{kt} + \delta_{nrt} + \beta FBI_{kn} \times Post_{nt} \times \mathbf{1}\,(r = Protestant)_{iknt} + X'_{irknt}\gamma + \varepsilon_{irknt} \tag{6}$$

where y_{iknt} is the reported church attendance by a respondent i in county k of a county pair n in the calendar year t, and $r = \{Protestant, non\text{-}Protestant\}$ indicates the religious denomination in which it identifies. Since the cluster is fully nested in a higher level (state), we two-way cluster standard errors for state and county pair (Cameron, Gelbach and Miller, 2011). Table 4 presents the estimates of the two specifications. Even in this highly restricted setup, we confirm that church attendance among Protestants increased significantly more in the county treated with the faith-based initiatives, compared to its close neighbor. The effect sizes are similar to those documented for the full sample above. This rules out all confounders that vary smoothly across state borders.

[TABLE 4 ABOUT HERE.]

Alternative religiosity measures. A potential concern is that the rise in church attendance does not reflect an increase in peoples' *intrinsic* religiosity, ie., the intensity of their religious beliefs. In principle, these individuals may simply have gone to church to obtain social services. Having received the services, they depart without being influenced by the religious component of the visit. We proceed by estimating Equation (1) for the four additional measures of religious intensity available for at least 20,000 individuals in the GSS in Figure 5 (all five measure are highly correlated, cf correlation matrix in Table A8). Since most of these measures are available for much fewer individuals, we show results binned into periods of 4 years instead of 2 (the conclusion is unaltered if keeping the two-year bins). In order to have two pre-treatment and two post-treatment periods, we extend the pre-treatment period so that we regard an event study period encompassing 12 years before treatment to 7 years after treatment.

The measure available for the largest number of individuals (used in panel a), is respondents' self-expressed strength of religious affiliation, based on the question "Would you call yourself a strong [religious affiliation] or not a very strong [religious affiliation]?" Respondents can answer "no religion", "not very strong", "somewhat strong", or "strong". We bundle the answers "not very strong" and "somewhat strong" into one category, as we cannot rank them.[17] We rescale the categorical values so that 0 indicates "no religion", 0.5 indicates "somewhat strong or not very strong", and 1 indicates "strong" religious affiliation. Panel (b) includes a measure based on answers to the question "Do you believe in an Afterlife?" We code yes as one and no as zero. Panel (c) includes a dummy equal to one if the respondent agrees that the Bible is the literal word of God (instead of being the inspired word of God or a book of Fables). Panel (d) includes a categorical variable measuring how often the respondent prays with possible answers ranging from never to several times a day. We rescale so that the categories take increments of 0.2 from 0 (indicating never) to 1 (several times a week). Conclusions are unaltered if using instead each of the individual categories of each measure as dependent variables.

[FIGURE 5 ABOUT HERE.]

Three of the four alternative measures of religiosity rise among Protestants after the implementation of the faith-based initiatives: Strength of religious affiliation, beliefs that the Bible is the word of God, and prayer intensity, whereas beliefs in an Afterlife is not affected. Consistent with previous research, the intensity of beliefs (strength of affiliation) seems to be more easily influenced by the faith-based initiatives, as opposed to whether or not individuals believe in an Afterlife. For instance, Bentzen (2019) documents that natural disasters strengthen the faith for those who already believe, but have no influence on non-believers. Belief in an Afterlife seems a particularly deep-rooted belief. The figures confirm the lack of pre-trends within 8 years before treatment. Afterlife beliefs and partly strength of affiliation may be slightly lower in treated states, indicating that treatment effects may be conservative.

3.8   Additional robustness

Generalizability. Figure A15 presents binned added-variable plots for the aggregate DD specification in Equation (2) for the full sample and the sample split into Protestants and non-Protestants, where observations are binned into 100 equally-sized bins. The observations are scattered smoothly around the regression line, a sign of good model fit.

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[17]If anything, we would have thought that "not very strong" indicated lower religious affiliation than "somewhat strong". However, the numerical category of "not very strong" is higher than that of "somewhat strong". Results are unchanged if we keep the original categories.

This means that the relation between the faith-based initiatives and church attendance is not driven by groups of observations or states. Thus, it is unsurprising that the initiatives increase churchgoing in all four major regions of the USA (Northeast, Midwest, West, South) and the Rust Belt (Table A9), although the impact is insignificant when restricting to the Rust Belt. DD effects are largest for the Northwest and West, whereas DDD effects are largest for the Midwest and South.

Controls. We show robustness of the results to the exclusion or inclusion of control variables (Table A3). In particular, results are not sensitive to the inclusion of the baseline controls for gender, marital status, age, and the Protestant dummy. The estimates on the baseline controls mimic what is found in the literature. For instance, we replicate the well-documented higher religiosity for women (Trzebiatowska and Bruce, 2012). Adding controls for income and education also does not impact results. Interestingly, both are positively associated with church attendance, although income is insignificant. We are not the first to show results contradicting the secularization hypothesis (Stark and Finke, 2000; Glaeser and Sacerdote, 2008; Iannaccone, 1998). Columns (9)-(12) further add controls for whether respondents identify themselves as Republican or African-American. While Republicans and African Americans attend church more often than the average, the impact of faith-based initiatives on church attendance remains unaltered.

Welfare reform. If the faith-based initiatives were implemented to reduce public spending and if changes in public welfare impacts changes in church attendance, this may bias our results. For instance, the first federal-level faith-based initiative (the Charitable Choice) was part of the 1996 Welfare reform. This is a concern for our analysis if all the following simultaneously hold: i) The state-level faith-based initiatives were implemented to cut down on public spending, ii) the initiatives thus coincided with a fall in public spending, and iii) if the rise in church attendance is a consequence of this potential reduction in public spending. We account for this in five distinct ways. First, our analysis includes state by treatment cohort fixed effects throughout, which capture any state-level confounders, constant or varying across treatment cohorts. This means that any confounder would have to change at the same time as the faith-based initiatives in our sample. Second, the DDD analysis additionally removes all state by event time factors, meaning that public spending and any other time-varying state-level confounder is fully accounted for. Third, we add a measure of state-level public spending as control in the DD specifications (columns 13-16 in Table A3). Public spending has no impact on church attendance and its inclusion does not change the impact of the faith-based initiatives on church attendance. Fourth, the impact is not larger for population groups that were potentially affected more than others by the 1996 welfare reform, such as foreign borns or the poor (Table A7). In particular, Hungerman (2005) found that US foreign borns were

more affected by the 1996 welfare reform. These checks show that public spending or the 1996 welfare reform are unlikely to explain results. This is consistent with the critique of the faith-based initiatives for not bringing the promised funding.

Further heterogeneity. Another concern is that Protestant denomination is proxying for another factor, which is the true driver of the differential (DDD) effects. To examine, we allow effects of the faith-based initiatives to vary with voting Republican, a dummy for above the 25th percentile income, and a dummy for above the 25th percentile education in two samples: The full sample and a sample restricted to Protestants (Table A7). We find no differential effects based on Republican voting or education, but interestingly, higher-income individuals are more likely to respond to the initiatives. This is consistent with the observation that Evangelicals from the elite were among the main initiators. Apparently, respondents of similar religion and income levels as the initiators respond more to the initiatives. The income effect vanishes, though, when restricting to Protestants, meaning that income does not explain the differential effect for Protestants.

Neighbor spillovers. Another concern is whether the effects on religious attendance is determined by an influx of more religious individuals from neighboring states instead of increased religiosity among the citizens living in the particular state. An effect driven by migration patterns would raise concerns whether the initiatives influenced the overall level of US religiosity. The testable implication is that religiosity should fall in response to the initiatives in neighboring states since the religious individuals move out of these states. We check in specifications mimicking Equation (2), where we add the interaction term $FBI_{sc} \times Post_{ct} \times Years_{sc}$ in Table A10, where *#Years$_{sc}$* equals the difference between implementation years between state s and their neighbor, $FirstYear_s - FirstYear_n$ if the neighbor implemented before state s, zero otherwise. We find that the interaction term is negative for Protestants, indicating that implementing a faith-based initiative has a smaller effect on Protestant church attendance when the neighboring state had already implemented an initiative. This means that, if anything, the initiatives *increase* religious attendance and beliefs in neighboring states.

4 Testing mechanisms

To test mechanisms, we exploit data on nonprofit organizations, churches and congregations, the types of faith-based initiatives, and the activities by the faith-based liaisons.

4.1 Increased number of faith-based organizations

We start by investigating whether the faith-based initiatives increased the number of faith-based nonprofits, a direct goal set by the initiators. The multiple mini grants and the ease on regulations for faith-based organizations might facilitate opening a faith-based organization and potentially prolong their survival time. This higher supply may explain the strengthened religiosity through either lower access costs to religion or a mechanism more like missionary work: Individuals consuming the service provided by the nonprofit organization are treated with religion as a by-product.

Our main database consists of nonprofit organizations in the US. We collected data on nonprofit organizations based in the US from the National Center for Charitable Statistics (NCCS) for the period 1990-2019. We start our period in 1990 due to a data break in this year. The NCCS provides information for the universe of nonprofit organizations in the US, except congregations or organizations with less than 25,000 USD in gross receipts.[18] Since we are investigating the impact of the faith-based initiatives on faith-based nonprofit organizations, we restrict the sample to public charities. This means that we are considering operating public charities in the 501(c)(3) IRS subsection that are actively reporting receipts to maintain their tax-exempted status (results are robust to using the full sample of nonprofits, cf Figure A18).[19] Organizations enter the NCCS database when they file for nonprofit status. In some instances, the NCCS has gaps in filing years. We fill out these gaps by assuming that an organization exists if it filed in a previous and later year. We end up with an unbalanced panel of 450,072 organizations (results are robust to examining a balanced sample at state-year level of the average number of faith-based organizations instead, cf Figure A20). Since the sample starts in 1990, we have a maximum of seven years in the pre-treatment period. We thus analyze event time windows of 7 pre-treatment years and 7 post-treatment years in the baseline specification. This reduces

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[18]Even if we had data on these small organizations, we would not expect them to drive results. Research shows that most churches and small religious organizations do not have the capacity to receive and administer government grants to carry out social services (Chaves et al., 2004; Green, 2007). Through interviews, Sager (2010) found frustration among liaisons that meetings with small congregations did not yield results in terms of getting new religious groups into the social services fold. Nevertheless, we document effects on congregations at the state-level in Figure A31.

[19]By focusing on the public charities, we remove nearly half of the sample. The excluded organizations are mainly 501(c)(4), 501(c)(5) and 501(c)(6), which are civic leagues, agricultural organizations, and business leagues, for instance.

the sample to 321,563 nonprofit organizations that will be included in our baseline analysis. We prepare the data as described in Section 3.2 in the same way as we did with the GSS data. We are able to allocate nonprofit organizations to their state geocoding coordinates using their address, or in a combination of ZIP codes and county codes to the nearest feature available.

To measure whether or not an organization is faith-based, we combine two pieces of information based on the classification of the organization and the name of the organization. The NCCS uses the National Taxonomy of Exempt Entities (NTEE) system to classify the nonprofit organizations by their activity, which are also followed by the IRS. The NTEE codes include 26 major groups, among which we use the *Religion-Related* category to identify religious organizations. This method classifies 5.7% of the organizations as religious.

The classification-based measure does not capture organizations that are built on religious values, but have another main purpose than religion, such as general fundraising or education. To include such organizations, we exploit information from the name of the organization to predict whether the organization is faith-based. 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, 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. This method categorizes 8.9 percent of the organizations as faith-based.

[FIGURE 6 ABOUT HERE.]

Not surprisingly, the measures based on names and the NCCS categories are highly correlated. For instance, the correlation coefficient between a dummy equal to one if the name contains one or more religious references and the religious categorization from the NTEEs is 0.55. In the sample, of the organizations that do not have a religious name, 94% are also not categorized as having a religious purpose. However, of the organizations that have a religious name, only 50% are categorized with a religious main purpose. This reflects that the names-based measure picks up organizations whose main purpose is not religious, at least not according to the NCCS. An example is the "Youth for Christ USA" (YFC) organization, who teach youth about Jesus in coffee shops and schools, present in 100 countries. Our names-based measure does not capture all religious organizations, though. Of the organizations categorized as religious based on their purpose, only 70%

have a religious name. Our main measure therefore combines the two. We define an organization as religious if it is either categorized as religious according to the NTEE classification or has a name with one or more religious words. According to this measure, 13.6% of the organizations are faith-based.

We cannot distinguish whether organizations are Protestant or not, as rather few of the organizations have names with either distinctly Protestant or non-Protestant words. Even if we had access to this information, it is not clear whether we would expect different effects for Protestant organizations, since the faith-based initiatives did not differentiate across faith.

Our baseline specification is equivalent to the DD specification in Equation (1), where i is a nonprofit organization treated in treatment cohort c and measured in event time $t \in [7;7]$. The dependent variable measures whether or not the particular organization is faith-based by an indicator variable equal to one if the organization is classified as religious according to the NTEE classification or has one or more religious words in their name. The baseline specification includes state by treatment cohort and treatment cohort by event time fixed effects and fixed effects for the length of the name of the organization. The parameter β_j captures the change in the share of religious nonprofit organizations after the implementation of the faith-based initiatives, relative to the change for the never treated organizations. We multiply by 100 to ease readability of the results.

Figure 7 shows the main results. Crucially, there seems to be no pre-trends; organizations do not differ systematically before treatment. However, after a faith-based initiative was implemented, the share of religious organizations rises up to 5 years after treatment, where-after it flattens out. By 7 years after treatment, the share of religious organizations had risen by 0.6 percentage points, amounting to 5% of the average share of faith-based organizations. The average post-treatment effect of 0.37 amounts to a third of the average rise in the share of faith-based organizations between 1996 and 2012.

[FIGURE 7 ABOUT HERE.]

The results are robust to using either of the definitions of a religious organization separately (Figure A17), using the full sample including private foundations, other organizations, and organizations with unknown classification (Figure A18), adding two years to the post-treatment period (Figure A19), or aggregating the data to the state-year level (Figure A20).

The results in Figure 7 may be due to an increase in the number of new faith-based organizations entering, fewer exiting, or a declining number of non-religious organizations. To examine these dynamics, we construct a balanced sample, where each organization is included throughout the period 1990-2012. To examine entry, we construct an indicator

variable equal to one when the organization starts up, zero before. Likewise, exit is an indicator variable equal to one when the organization ceases to exist, zero before. With these variables as dependent variable, we estimate type (1) Equations, where β_j now reflects the impact of the faith-based initiatives on the share of organizations that started up or ceased to exist, respectively.

As a first step, we restrict the sample to the religious organizations in Figure 8. Again, crucially for our ability to estimate causal effects, we find no signs of pre-trends; prior to treatment, the entry and exit rates do not differ systematically. After implementation of the faith-based initiatives, the number of new religious organizations entering rose significantly. In particular, by 7 years after treatment, the share of new religious organizations had risen by 5.3 percentage points as a consequence of the faith-based initiatives, which amounts to 15% of the average entry-rate. This means that the faith-based initiatives can explain 15% of the entry of all new faith-based organizations during the period. We find no effects on exit rates, except from the very end of the sample, which may simply be a mechanical cause of the many new organizations that entered.

[FIGURE 8 ABOUT HERE.]

Examining the non-religious organizations, we find no trend-break around the faith-based initiatives (Figure A23). The secular nonprofit organizations were on a rising trend before the faith-based initiatives and this trend continued after treatment. If anything, the upward trend among the secular nonprofits may be rising slightly after treatment, meaning that we can rule out a declining share of secular organizations as explanation for the rising share of faith-based organizations from Figure 7.

Alternative measure of religious supply. Since the dataset on nonprofit organizations does not include congregations, we use another dataset to check the impact on the supply of congregations. The Association of Religion Data Archives (ARDA) provides decadal data on the number of congregations and adherents by US states. We use the longitudinal dataset covering the years 1980, 1990, 2000, and 2010. The dataset provides information on multiple different congregations, which we aggregate into Catholics, Orthodox, Protestants, and Other religion.

We estimate DD estimations of type (1) and DDD estimations of type (3) with the number of congregations or adherents per capita as dependent variable. 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 compared to event time $t \in [-10, -1]$. Hence, the time window goes from twenty years before the introduction of faith-based initiatives to nine years after. Results are shown in Figure A31. First, there seems to be no pre-trends (the blue dots represent the period before treatment). Next, the faith-based initiatives

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raised the number of adherents and congregations significantly by around 40 and 0.15 per 1,000 capita, respectively. Also, the rise in congregations occurred mainly among Protestants. This may be due to the fact that Protestant congregations take up a larger share of the total number of congregations or because the faith-based initiatives influenced Protestant congregations differentially. For adherents, we do not find statistical differences across Protestants and Catholics, which is consistent with the finding in the GSS data that the initiatives did not result in switching in or out of Protestant denominations.

In summary, our findings support the notion that the supply of religion has increased, as evidenced by the growing number of religious organizations.

4.2 Welfare

An alternative potential mechanism involves the poor going to church for material needs and becoming more religious as a by-product. To test, we note that if this was the main explanation, we would expect the following: First, the program laws – involving public welfare through the churches – should play a central role. Second, the effects should be driven by recipients of social welfare. We analyze each in turn.

We first exploit information on the types of initiatives; program laws, concrete laws, and symbolic laws. In this analysis, we examine how effects depend on the type of initiative implemented by constructing new treatment variables based on the type of initiative that was first implemented. This analysis is complicated by the differential timing of the initiatives with concrete laws preceding program laws, for instance (Figure A7). Thus, analyzing the program laws by a simple dummy for the year of the first program law would be violated by pre-trends caused by the concrete laws. Instead, we compare states whose first initiative was a program law to never-treated states only, excluding states whose first initiative was a concrete or symbolic law. We do so in panel c of Figure 9, showing the DDD estimates of the rise in church attendance among Protestants. Likewise for concrete laws in panel a and symbolic laws in panel b. Again, we find no signs of pre-trends, except for a marginally positive effect in $t \in [-6, -5]$ for the program laws, potentially contaminated by the fact that most program laws occur at the same time as concrete laws. All three types of laws raise church attendance, but most significantly the concrete laws. When analyzing all three types simultaneously, we confirm the stronger effects of the concrete laws, followed by the program laws, and find no significant effects of the symbolic laws (Table A12).

[FIGURE 9 ABOUT HERE.]

Next, if effects were driven by the poor receiving welfare by the churches, we would expect effects to be larger for the poor. We do not find such effects (Table A7). If anything,

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effects are larger for individuals with higher incomes. Thus, results are not driven by the typical welfare recipients.

The lack of explanatory power of the program laws or heterogeneities with respect to income is in line with the critique that the initiatives did not contribute with much additional welfare. These results are also consistent with arguments by other scholars emphasizing that the real impact of the faith-based initiatives was strengthened church-state relations (Sager, 2010; Chaves and Wineburg, 2010; Wineburg, 2007).

4.3 Faith-based liaisons

As a last examination of mechanisms, we exploit information on the activities of the faith-based liaisons – the central persons employed in state governments responsible for implementing the faith-based initiatives. We examine whether church attendance rises after the implementation of a faith-based liaison, the differential effects of the liaisons based on the size of their budgets, and we exploit information on the activities of the liaisons collected by Sager (2010) through interviews. This analysis is complicated by the fact that the faith-based liaisons most often started operating after the implementation of the first faith-based initiative (Figure A6). If the initiatives influenced church attendance independently of the liaisons, this would create pre-trends in an analysis with the faith-based liaisons as independent variable and church attendance as dependent variable. Indeed, we find positive pre-trends in these analyses, particularly for the Protestants (panels a, c, and e of Figure A25). Interestingly, we find no pre-trends for the grants (panels b, d, and f). Generally, we find slightly larger post-effects of the liaisons than when accounting for the size of their grants, again consistent with the complaints of lack of funds by the liaisons.

Next, we exploit information collected by Sager (2010) on the activities of the faith-based liaisons. In interviews with 33 liaisons in 2006, Sager (2010) examined which activities they engaged in. Most liaisons engaged in creating websites to inform the faith-based organizations about grant opportunities (28 of the 33 liaisons) and facilitating networks between state agencies and faith-based organizations (27). In addition, 22 were connected to the White House (12 were in frequent contact), 21 engaged in creating conferences for faith-based organizations, 20 had an office in the state directly under the governor, 18 had created advisory boards to focus on faith-based organizations, 16 had created technical assistance seminars or grant writing programs for faith-based organizations, 11 had engaged in start-up funds for faith-based organizations, 6 had an office in a state agency. In Table A14, we show difference-in-differences and triple difference estimates of the effect of the presence or absence of a specific activity in a faith-based liaison on

church attendance of GSS respondents, compared to individuals in states without the presence of faith-based liaisons and relative to the year before the liaisons introduction. Overall, church attendance particularly rose in states where liaisons recruited volunteers for programs, had frequent contacts with the White House, and installed startup funds for FBOs. These activities all seem beneficial for starting up new faith-based organizations and thus support the finding of a rising number of faith-based start-ups.

To sum up, we find that the faith-based initiatives increased the supply of faith-based organizations and congregations. This is not entirely surprising in light of the stated mission of the initiatives; to increase the number of faith-based nonprofit organizations and generally improve conditions for these organizations. Thus, our results simply provide support for the success of this mission. According to standard models of economics of religion, this higher supply would imply strengthened religiosity, consistent with our findings. We further find that the higher supply is driven by new organizations being established as a consequence of the faith-based initiatives and to a larger extent in states where liaisons engaged in recruitment groups for new initiatives, dealt with start-up funds for faith-based organizations, and were in frequent contact with the White House.

5   Social Views and Outcomes

The faith-based initiatives seem to have bolstered the role of religion among Americans. If this impact was sufficiently strong, we would expect a strengthening of certain social views associated with religious beliefs. More generally, social movements often work to re-frame debates and shape perceptions, especially if they manage to penetrate government (Andrews, 2001; Giugni, McAdam and Tilly, 1999; Jenkins and Eckert, 1986; McCammon et al., 2001; Piven and Cloward, 2012; Sager, 2010; Tarrow, 2011). Moreover, the implementers of the faith-based initiatives may have had a more general agenda of bringing Christian principles back into American life (Lindsay, 2007). These conservative religious groups view faith-based organizations as representing values that have been displaced in the modern world (Sager, 2010, 137). The rise in the number of faith-based organizations may thus have further reinforced social views associated with these religious groups.

To examine, we focus on a set of views that are particularly pronounced among Evangelicals. Restricting the choice set to social views documented in the GSS for at least 20,000 respondents, we end up with a set of social views against homosexuality, modern gender roles, science, abortion, and preferences for conservatism and prayer in public schools, all of which are more pronounced among Evangelicals on average, compared to the rest of the population (Table A15).[20] These values are also more pronounced among

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[20]The 20,000 are set as a bar to limit our choice set. Since the GSS holds more questions on social values than

the broader group of Protestants. Two of these views align with key policy areas expressed as particularly important among Evangelicals: Views against homosexuals and abortion (Lindsay, 2007).

We measure views against homosexuality using a categorical variable reflecting answers to the question concerning whether homosexual sex relations are "not wrong at all", "sometimes wrong", "almost always wrong", and "always wrong" (we scale the GSS variable homosex to take values 0, 0.33, 0.66, and 1). We measure views against working women by constructing a dummy variable equal to one if respondents disapprove of working women in two of four of the following questions: "Do you approve or disapprove of a married woman earning money in business or industry if she has a husband capable of supporting her?", "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", "Tell me if you agree or disagree with this statement: Most men are better suited emotionally for politics than are most women," and "If your party nominated a woman for President, would you vote for her if she were qualified for the job?" (cf. Appendix B.3).[21] We measure views against science using a dummy variable taking the value one if the respondent has "only some" or "hardly any" confidence in science and zero if the respondent has "a great deal" of confidence in science.[22] Views against abortion is measured by a dummy equal to one if the respondent expressed views against abortion for any of the reasons included in the survey, such as the woman was raped, is too poor to take care of the child, is unmarried, has serious health issues, does not want more children, or if the child is likely to have a serious defect. Politically conservative views are measured using a dummy variable equal to one if the respondent answered that they view themselves as conservative or extremely conservative, zero otherwise. Views supporting bible prayer in schools is based on a dummy variable equal to one if the respondent disapproves of a US Supreme Court ruling stating that public schools cannot require Bible reading.

For each of the values, we estimate DD equations of type (1) and DDD equations of type (3). The inclusion of state by treatment cohort and treatment cohort by event time fixed effects in the DD specifications eliminates variation in *levels* of unobservables across states, changes in unobservables across states and treatment cohorts, and general trends

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on religiosity, we set the bar higher for the social values, compared to the religiosity measures.

[21]There is large variation in responses to these questions. For instance, as many as 40% of respondents agree or strongly agree with the statement "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." On the other hand, 22% disapprove of working women and 12.4% would not vote for a female President candidate. Given these idiosyncrasies, we judge that a combination of responses reveal preferences for traditional gender roles better than the individual responses.

[22]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".

in the variables. The DDD specification additionally includes state by event time fixed effects, which additionally removes any variation in unobservables at the state-event time level. For instance, this takes care of the concern that states that are already more (or less) conservative or on a more conservative-trending path would be more likely to implement the initiatives. Since these data are available for much fewer individuals, we aggregate event times into brackets of four years, mimicking our analysis of alternative measures of religiosity, which also suffers from few observations.

We find no signs of pre-trends in the DD specifications, except for views against women which has a large positive pre-trend and views against homosexuals with a small negative pre-trend (Figure A27). This means that we should be cautious interpreting the post effects for these views.

Figure 10 shows the main DDD results. Both of the mentioned pre-trends cancel out, meaning that Protestants and non-Protestants were trending in similar ways before treatment. We find no signs of pre-trends for any of the views except that social views against science have a negative pre-trend in the DDD estimates, indicating that these results may be downward biased. Gauging the treatment effects, none of the social views are influenced in the period immediately after treatment, consistent with a slower impact than the effects on church attendance and the supply of faith-based organizations. However, four of the six views are influenced significantly in the last period 4-7 years after treatment. Three become more conservative; views against homosexuals and abortion and general conservative views. For instance, self-reported conservative views strengthen by 5.8 percentage points more for Protestants compared to the rest, which amounts to a third of the average share of conservatives. Interestingly, social views towards women become more progressive. Computing average effect sizes across all six social views using the method by Clingingsmith, Khwaja and Kremer (2009), we find that social views become significantly more conservative overall in both DD and DDD specifications (Figures A28 and A29).

[FIGURE 10 ABOUT HERE.]

To examine whether these changes had real implications, we proceed to identify whether the change in social views was accompanied by a change in associated outcomes. For two of the views – skepticism towards homosexuality and traditional gender roles – we found direct measurable outcomes. During the period from 1998 to 2015, 31 states changed their constitutions to ban gay marriages, prior to *Obergefell v. Hodges* (data description in Section B.7 of the Appendix). We construct a dummy variable equal to one in the year the state implemented a constitutional ban on gay marriage and thereafter, zero otherwise. Using this dummy as dependent variable, we estimate DD estimates in type (1) Equations.

These are presented in panel (a) of Figure 11. Prior to treatment with the faith-based initiatives, states were similar in terms of the likelihood of having banned gay marriages (*i.e.*, no pre-trends), but after treatment, we find higher likelihood of bans on gay marriages in treated states. This is consistent with the finding of strengthened social views against homosexuals in Figure 10.

To examine gender gaps, we use data from IPUMS on education and wage incomes for men and women. We compute gaps in wages and education levels in panels (b) and (c) of Figure 11 (results are robust to using gaps expressed as percentages of male wages or education, respectively, Figure A30). While there seem to be some breaks in the data for several of the education categories, we find no data breaks when observing the share of individuals with four years of college or more. This is therefore our preferred measure of education and the one shown in panel (c). Before the faith-based initiatives, we find no significant differences between treatment group and control group in terms of gender gaps in wages or education (*i.e.*, no pre-trends). After treatment, though, gender gaps in wages and education start rising among the treated states, compared to the never treated states. These differences become significant around 2-3 years after treatment and remain high throughout the period. Although these DD results are not directly comparable to the DDD results of Figure 10, they may seem inconsistent with the finding of more liberal views on gender roles. We are not the first to document such inconsistencies between self-reported views and actual behavior. Although evidence is mixed, some research finds that social desirability bias is larger among the religious, who may have a stronger self-regard of morality (Regnerus and Uecker, 2007; Batson, Naifeh and Pate, 1978).

[FIGURE 11 ABOUT HERE.]

We conclude that social views aligning with policy areas central to Evangelicals strengthened in the aftermath of the faith-based initiatives: Views against abortion and homosexuals and general conservative views. While views on gender roles became more inclusive, actual gender gaps in education and wages increased.

6 Effect sizes

To grasp the size of the impact of the faith-based initiatives, we use insights from the persuasion literature (DellaVigna and Kaplan, 2007). Since church attendance rates were already high before the faith-based initiatives, a large share of treatment recipients were already persuaded. The persuasion rate measures persuasion among those who were not already convinced and refers to the proportion of individuals who change their attitudes, beliefs, or behaviors as a result of a persuasive message compared to those who were not

exposed to the message, in this case the faith-based initiatives. In a setting with a binary behavioral outcome, the persuasion rate is computed as

$$f = 100 \times \frac{y_T - y_C}{e_T - e_C} \frac{1}{1 - y_0}, \tag{7}$$

which captures the effect of the persuasion treatment on the relevant behavior ($y_T - y_C$), adjusting for exposure to the message ($e_T - e_C$) and for the size of the population left to be convinced ($1 - y_0$) (DellaVigna and Gentzkow, 2010). To compare with a list of 24 persuasion rates computed by DellaVigna and Gentzkow (2010) based on 17 of the best research papers on persuasion, we consider binary outcomes. We focus on monthly or more frequent attendance, since the rise in church attendance was driven by rising weekly and monthly attendance (Figure A14 and Tables A5-A6). The share of monthly or more frequent church attenders rose by 3.1 percentage points in the full population in response to the faith-based initiatives, relative to the control group ($y_T - y_C = 0.031$). After 1996, 68% of the respondents lived in state-years that had implemented at least one faith-based initiative ($e_T - e_C = 0.68$). Prior to the initiatives, 50% attended church monthly or more often ($1 - y_0 = 0.5$).[23] The implied persuasion rate is $100 \times (0.031)/(0.68 \times 0.5) = 9\%$, which means that 9% of Americans who did not already attend church monthly or more often were persuaded by the treatment. This rate belongs among the 45% highest persuasion rates in the list by DellaVigna and Gentzkow (2010) and is at par with the persuasion rate of a randomized fund-raising campaign (List and Lucking-Reiley, 2002). While this is rather high, we note that the treatment period of 7 years of the faith-based initiatives is longer than most of the papers.

To evaluate whether the change in faith-based organizations can generate these sizable persuasion rates, we compute the persuasion rate among the nonprofit organizations, which amounts to $100 \times (0.00366)/(0.758 \times 0.886) = 0.53\%$.[24] This is a rather low rate (among the 10% lowest persuasion rates in the DellaVigna-Gentzkow ranking). Nevertheless, the following - arguably, very crude - back-of-the-envelope calculation illustrates that this may amount to large total numbers of potentially treated individuals. As there are 382,943 organizations in our data established before 2013, this would amount to an additional 2052 faith-based organizations.[25] If the size of the average faith-based organization in the

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[23]These numbers are computed based on the raw data before stacking. The 68% is computed in the 1996-2012 sample, as 2012 is the end of the window of analysis. The 50% is computed for the sample 1990-1996.

[24]From Figure 7, we get the average post-treatment effect of 0.366. After 1996, 75.8% of the organizations were located in state-years that had implemented at least one faith-based initiative ($e_T - e_C = 0.758$). Prior to the initiatives, 11.4% of the organizations were faith-based ($1 - y_0 = 0.886$). To compute these numbers, we use the raw data before stacking. We end the data in 2012, as this is the end-year in the GSS data.

[25]Instead of using the total of 450,072 organizations in our dataset, we focus on the 382,943 organizations established before 2013 in order to compare to the GSS results for church attendance.

NCCS data is the same as that of the average congregation, they would each serve 2390 persons yearly.[26] This implies that an additional 4.9 million individuals would potentially be served by a faith-based organization yearly. Among the current 140 million Protestant Americans, these numbers indicate that 3.5% could have been in contact with one of the new faith-based organizations each year. While indeed very crude, these numbers may contribute to our understanding of the large persuasion rates resulting from the faith-based initiatives.

7 Conclusion

The faith-based initiatives were a conservative religious movement aiming to strengthen the position of religion in the US. Proponents argued that faith-based organizations provide better for the needy than the secular state, while opponents feared the initiatives implicitly allowed faith-based organizations to proselytize for government funds. The initiatives seem to have altered the beliefs, practices, and social views of the American population towards stronger religious beliefs and associated conservative-religious social views.

We can understand these effects through the main purposes of the initiatives; to improve conditions for faith-based organizations and increase their numbers. The initiatives reduced regulatory burdens of faith-based organizations, facilitated their access to funding, encouraged government partnerships with them for social service delivery, and improved overall government engagement with faith-based organizations. Perhaps not surprisingly, we find that the initiatives increased the number of new faith-based organizations. In addition to standard religious market mechanisms, the rising number of faith-based organizations may also have strengthened the role of religion through proselytizing as argued by the opponents.

The general trend in the USA is secularization, equality between the sexes, and modernized social views. However, this average trend covers rising polarization. Putnam and Campbell (2012) attributes the rising religious polarization in the US to the societal changes initiated by the 1960s sexual liberation movement, which paved the way for conservative religious groups—particularly evangelicalism—to gain prominence and become increasingly involved in politics. The faith-based initiatives are a quantifiable part of this trend of evangelicalism in politics, and our study can be seen as an empirical test of these arguments. The faith-based initiatives strengthened religiosity and conservative social

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[26]Chaves (2021). We presume the number is yearly. However, this is not directly clear from the data. The survey question in the Chaves (2021) dataset reads "How many persons would you say are associated in any way with the religious life of this congregation - counting both adults and children, counting both regular and irregular participants, counting both official or registered members and also participating nonmembers. What is the total number of persons associated with this congregation to any degree at all?"

views among Protestants, but religiosity continued to fall and social views modernized among the rest.

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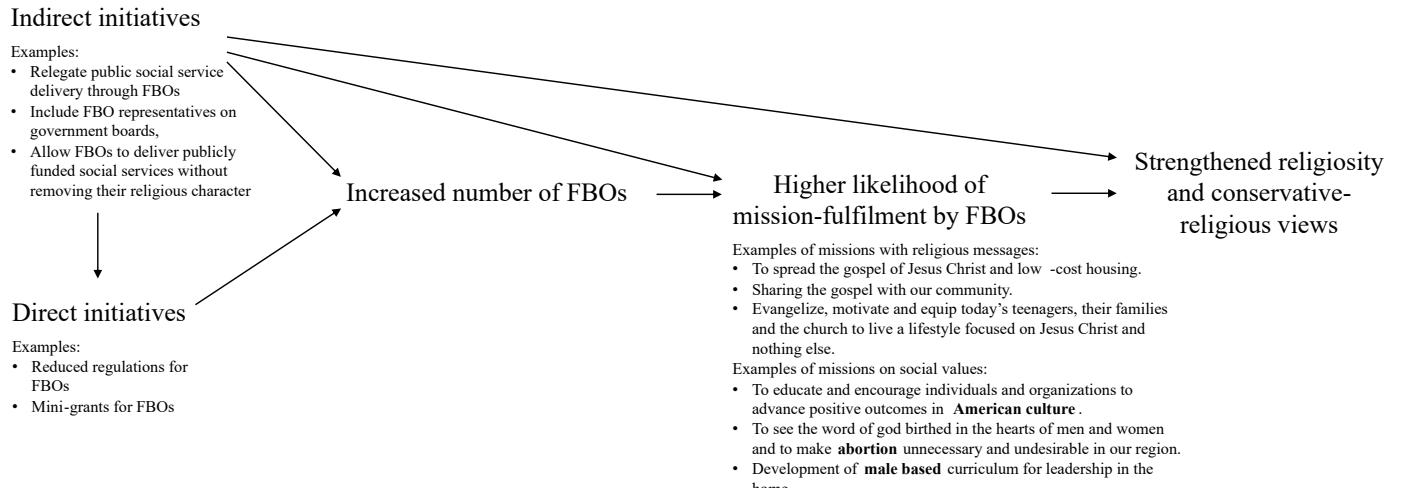
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Figures
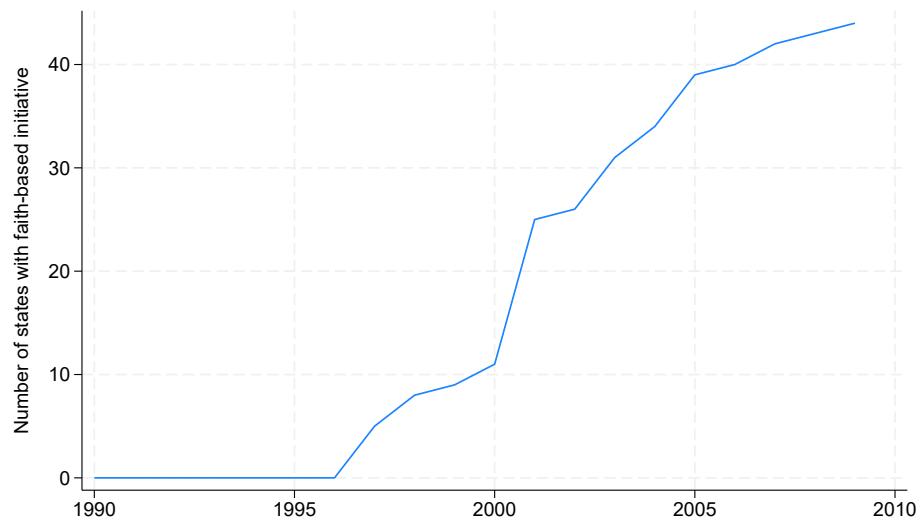
Figure 1: Overview of testable predictions

Indirect initiatives

Examples:
- Relegate public social service delivery through FBOs
- Include FBO representatives on government boards,
- Allow FBOs to deliver publicly funded social services without removing their religious character

Increased number of FBOs

Higher likelihood of mission-fulfilment by FBOs

Examples of missions with religious messages:
- To spread the gospel of Jesus Christ and low -cost housing.
- Sharing the gospel with our community.
- Evangelize, motivate and equip today's teenagers, their families and the church to live a lifestyle focused on Jesus Christ and nothing else.

Examples of missions on social values:
- To educate and encourage individuals and organizations to advance positive outcomes in **American culture** .
- To see the word of god birthed in the hearts of men and women and to make **abortion** unnecessary and undesirable in our region.
- Development of **male based** curriculum for leadership in the home.

Strengthened religiosity and conservative-religious views

Direct initiatives

Examples:
- Reduced regulations for FBOs
- Mini-grants for FBOs

Notes: Overview of testable predictions of the impact of faith-based initiatives on religiosity and conservative-religious views. The missions of the nonprofits are described in more detail in Table 1.

Figure 2: The spatial and temporal spread of the faith-based initiatives: 1997-2009

(a) Number of states with a faith-based initiative

(b) Number of faith-based initiatives implemented by 2009
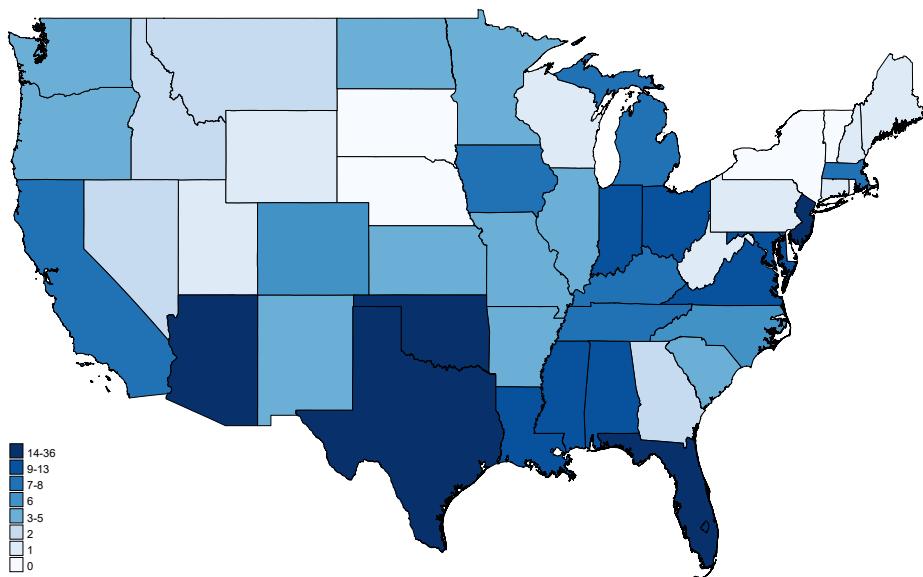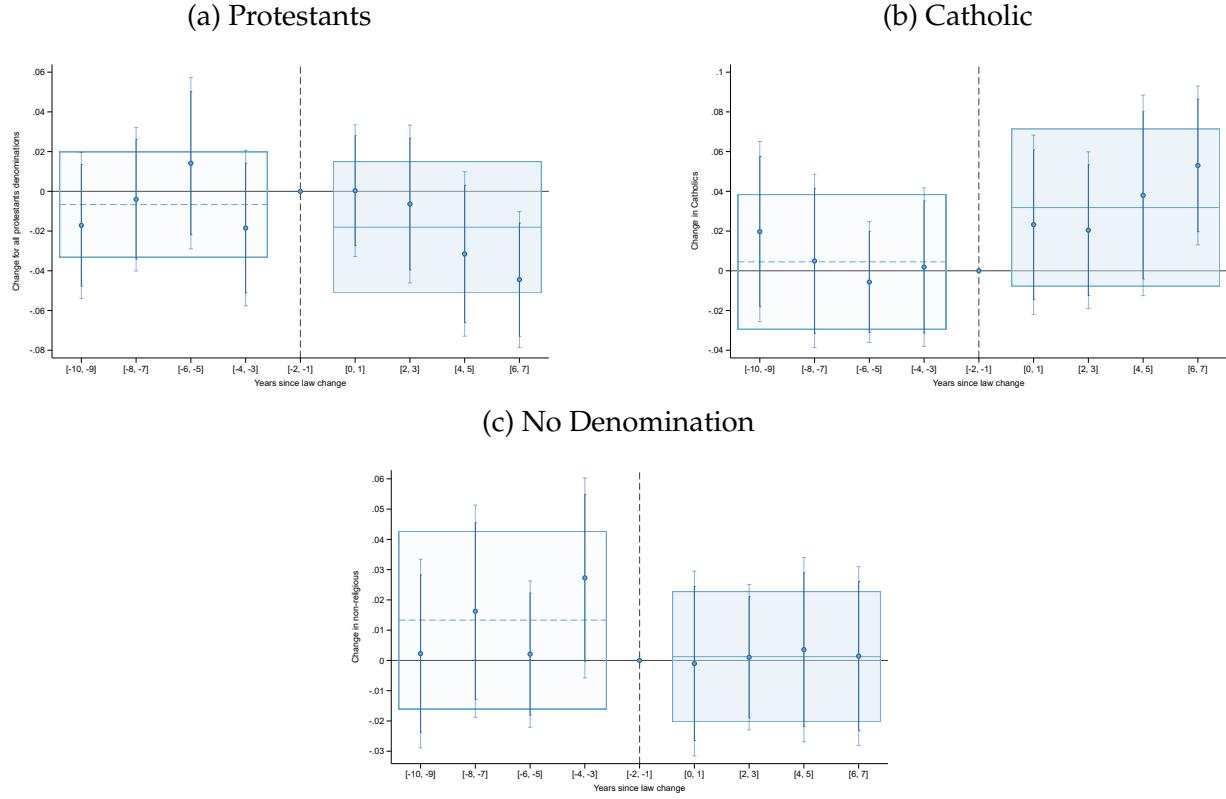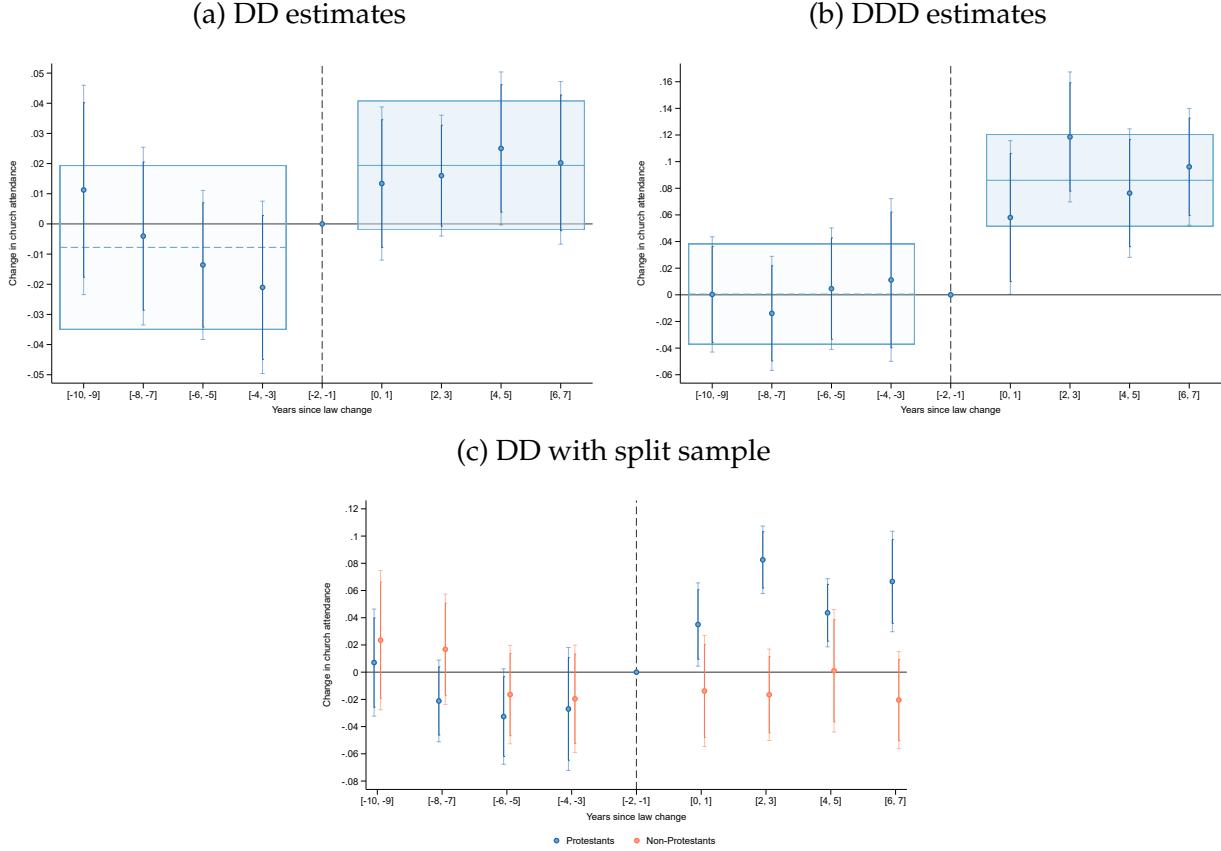
Figure 3: The impact of faith-based initiatives on the size of religious groups

(a) Protestants

(b) Catholic

(c) No Denomination

Notes: Regression estimates for the effect of faith-based initiative on the likelihood that respondents from the General Social Survey (GSS) identify with a given religious denomination using our main sample as in Figure 4a. Each panel presents the difference-in-differences estimates in Equation (1) and the aggregate interactions in Equation (2) where the outcome is an indicator variable equal to one if the respondent identifies as Protestant in panel (a), Catholic in panel (b) or with no religious denomination in panel (c), and zero otherwise. The coefficients of interest represent the change in the likelihood of identifying with the indicated religious denomination, relative to the time period before the introduction of faith-based initiatives. Regressions are estimated using ordinary least squares with sampling weights and the weighting scheme described in Wing, Freedman and Hollingsworth (2024). 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-period aggregate interactions are indicated as lines, with their 95% confidence intervals shown as boxes. Standard errors are clustered at the state level.
Result: The faith-based initiatives did not change the composition of religious groups, except for the last period 6-7 years after treatment, where some Protestants may have shifted to become Catholics. This means that we should be careful interpreting the last event period when moving forward.
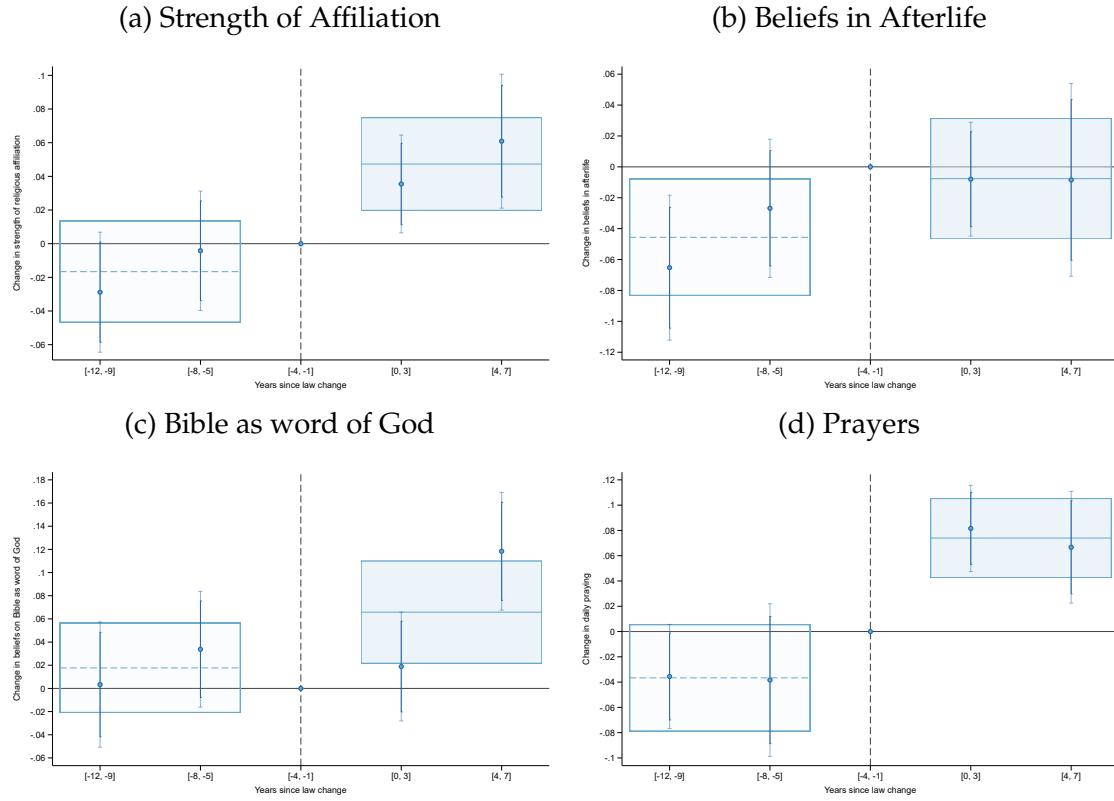
Figure 4: The impact of the faith-based initiatives on church attendance

(a) DD estimates

(b) DDD estimates

(c) DD with split sample

Notes: Regression estimates of the effect of faith-based initiatives on church attendance at the individual level using waves from the General Social Survey (GSS). We assemble the data as described in Section 3.2 using a fixed time window of ten years before and seven years after the implementation of a faith-based initiative and individuals from never-treated states as comparison group. The event timing is binned in two event time periods. The outcome variable is the frequency of religious attendance, rescaled between zero and one as explained in Section 3.1. Regressions are estimated using ordinary least squares with sampling weights and the weighting scheme described in Wing, Freedman and Hollingsworth (2024). Panel (a) presents the difference-in-differences estimates in Equation (1) and the aggregate interactions in Equation (2), representing the change in church attendance of individuals in states where a faith-based initiative was implemented, relative to individuals in states without the presence of faith-based initiatives. Individual-by-state-by-stack observations: 45,315; Average church attendance: 0.422; Change in church attendance post faith-based initiatives: -0.015. Aggregate post-treatment effect: 0.019 (*std. err.* = 0.011, *t* = 1.828). Panel (b) presents the triple difference estimates in Equation (3) and the aggregate interactions in Equation (4), describing the change in church attendance of Protestants in states that experienced a faith-based initiative compared to Protestants in states without the presence of faith-based initiatives, relative to equivalent change for non-Protestants. Average share of Protestants: 0.427; Aggregate post-treatment effect: 0.086 (*std. err.* = 0.017, *t* = 5.022). Panel (c) presents the difference-in-differences estimates in Equation (1), separately for Protestants and non-Protestants, illustrating the change in church attendance of individuals in states implementing a faith-based initiative, relative to individuals in the same religious group and in states not implementing it. **Protestants** – Average church attendance: 0.482; Change in church attendance post faith-based initiatives: 0.011. Aggregate post-treatment effect: 0.057 (*std. err.* = 0.011, *t* = 4.959). **Non-Protestants** – Average church attendance: 0.374; Change in church attendance post faith-based initiatives: -0.028. Aggregate post-treatment effect: -0.012 (*std. err.* = 0.016, *t* = -0.793). 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, since that is the level of our variation. **Result**: The faith-based initiatives raised churchgoing for Protestants. States did not differ systematically in terms of average church attendance prior to implementation. Church attendance of non-Protestants was unchanged.

Figure 5: The impact of the faith-based initiatives: alternative measures of religiosity

(a) Strength of Affiliation

(b) Beliefs in Afterlife

(c) Bible as word of God

(d) Prayers

Notes: Regression estimates of the impact of the faith-based initatives on religiosity measured using alternative measures than church attendance at individual level. Each panel presents separate triple difference estimates of the coefficients in Equation (3) and the aggregate interactions in Equation (4), where the data are assembled as described in Section 3.2 using a fixed time window of twelve years before and seven years after the implementation of a faith-based initiative and individuals from never-treated states as comparison group. While the event timing is binned in two event time periods, we estimate more aggregate coefficients pooling together two event time bins due to data restrictions. We express coefficients relative to the average change $\tau = [-4, -1]$ between one year and four years before the introduction of the faith-based initiatives. Regressions are estimated using ordinary least squares with sampling weights and the weighting scheme proposed by Wing, Freedman and Hollingsworth (2024). Panel (a): **Strength of affiliation** – Observations: 49,286; Mean dep. var.: 0.575; Average share of Protestants: 0.428; Change in dep. var. post faith-based initiatives: -0.026. Aggregate post-treatment effect: 0.047 (*std. err.* = 0.014, t = 3.453). Panel (b): **Beliefs in afterlife** – Observations: 32,516; Mean dep. var.: 0.753; Average share of Protestants: 0.444; Change in dep. var. post faith-based initiatives: 0.022. Aggregate post-treatment effect: -0.008 (*std. err.* = 0.019, t = -0.395). Panel (c): **Bible as the word of God** – Observations: 34,729; Mean dep. var.: 0.274; Average share of Protestants: 0.435; Change in dep. var. post faith-based initiatives: -0.020. Aggregate post-treatment effect: 0.066 (*std. err.* = 0.022, t = 3.003). Panel (d): **Prayers** – Observations: 28,755; Mean dep. var.: 0.606; Average share of Protestants: 0.429; Change in dep. var. post faith-based initiatives: 0.002. Aggregate post-treatment effect: 0.074 (*std. err.* = 0.016, t = 4.754). 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**: Three of four alternative measures of religiosity also rise after the faith-based initiatives. States do not differ in terms of these measures prior to implementation, except that beliefs in Afterlife may have been somewhat lower 9-12 years before treatment.
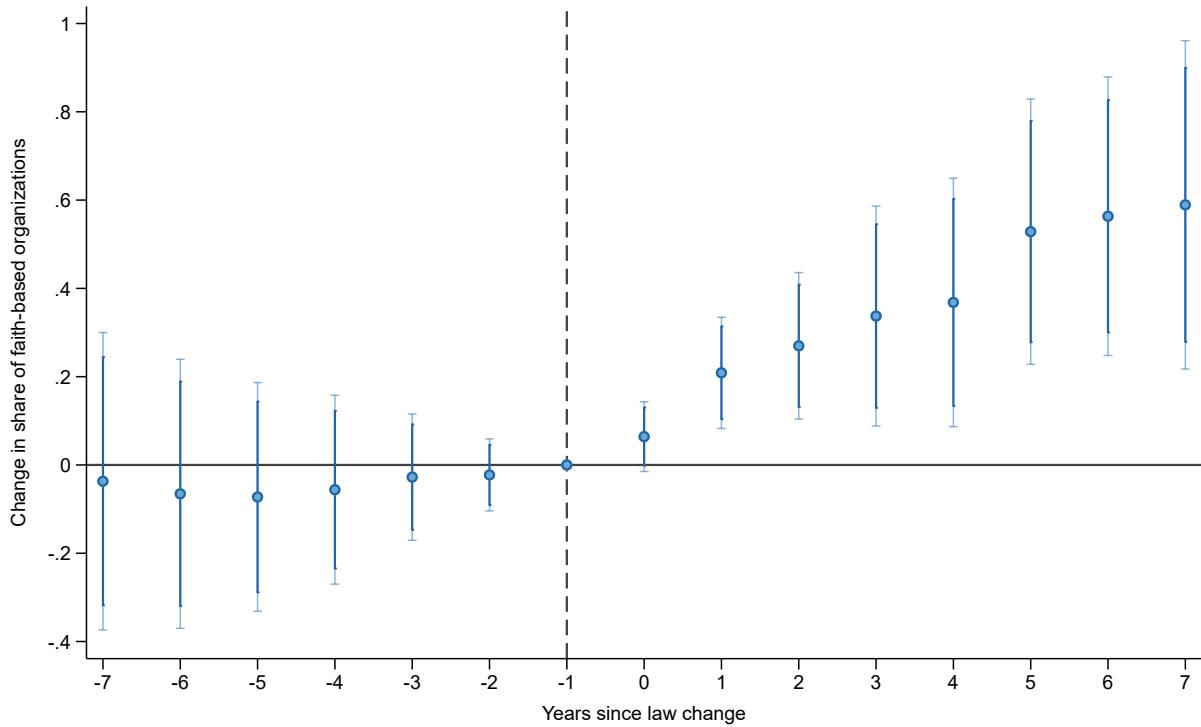
Figure 6: Composition of religious words in the names of nonprofit organizations

Notes: The wordcloud reports the most frequent religious words that we pick up in the corpus of the unique names of the nonprofit organizations in the collected sample after pre-processing, tokenization, and stemming of the strings, based on our bag-of-words of religious terms. The colours and the size of the words illustrate their frequency, indicated for the top term (in red), the next top five terms (in orange), the remaining top ten terms (in blue), the leftover top twenty terms (in light blue), and all other terms (in lighter blue). **Result**: The majority of the faith-based organizations were Christian ministries, missions, and churches.
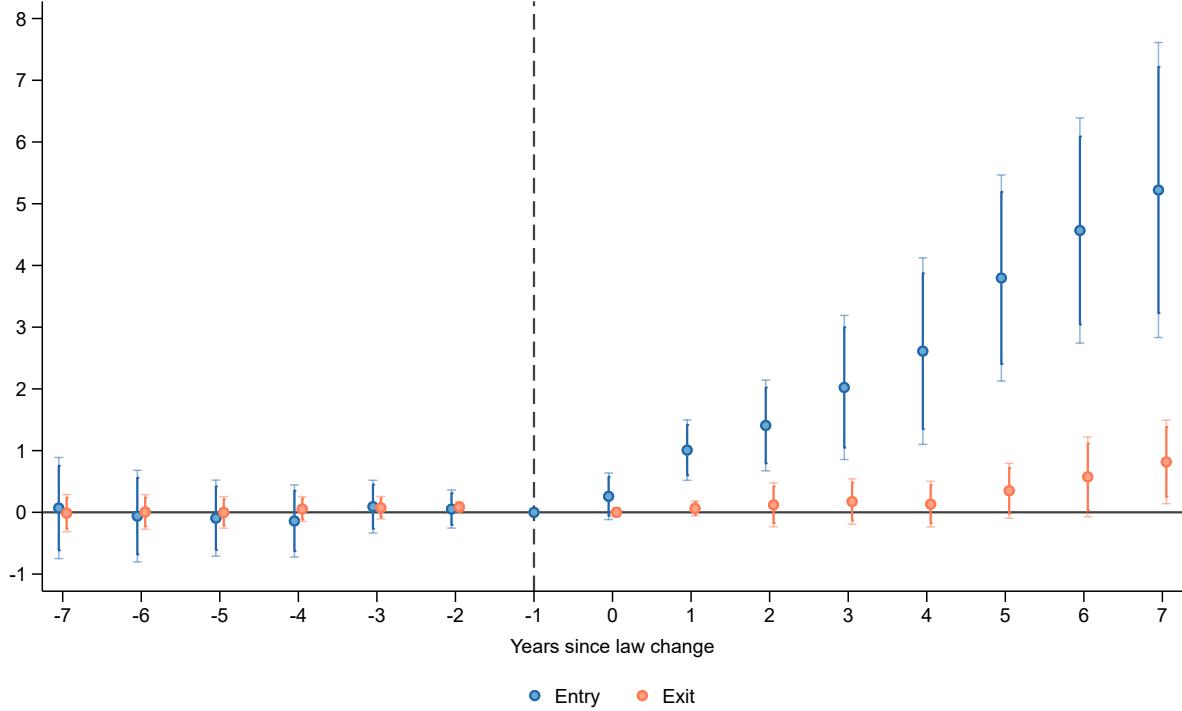
Figure 7: The impact of faith-based initiatives on the presence of faith-based organizations

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 nonprofit organization level where the outcome is an indicator variable equal to one (rescaled to one hundred) if the nonprofit organization is categorized as a faith-based organization either by its NTEE code or by the presence of religious words in its name as described in Section 4.1, and zero otherwise. Sample: 7,092,224 organization-by-stack-by-year observations. Regressions are estimated using ordinary least squares with sampling weights and the weighting scheme proposed by Wing, Freedman and Hollingsworth (2024). We include fixed effects for the number of tokens in the organization's name by treatment cohort. 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 represent the differential probability (in percentage points) that nonprofit organizations from states implementing a faith-based initiative are faith-based organizations, relative to nonprofit organizations in states that remain without the presence of faith-based initiatives. Average share of faith-based organizations: 10.945%. Change in the share of faith-based organizations post faith-based initiatives: 1.166%. Aggregate post-treatment effect: 0.366 (*std. err.* = 0.112, *t* = 3.264). 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.
Result: The faith-based initiatives increased the share of faith-based organizations, whereas there was no systematic difference in the probability of being a faith-based organization prior to implementation.
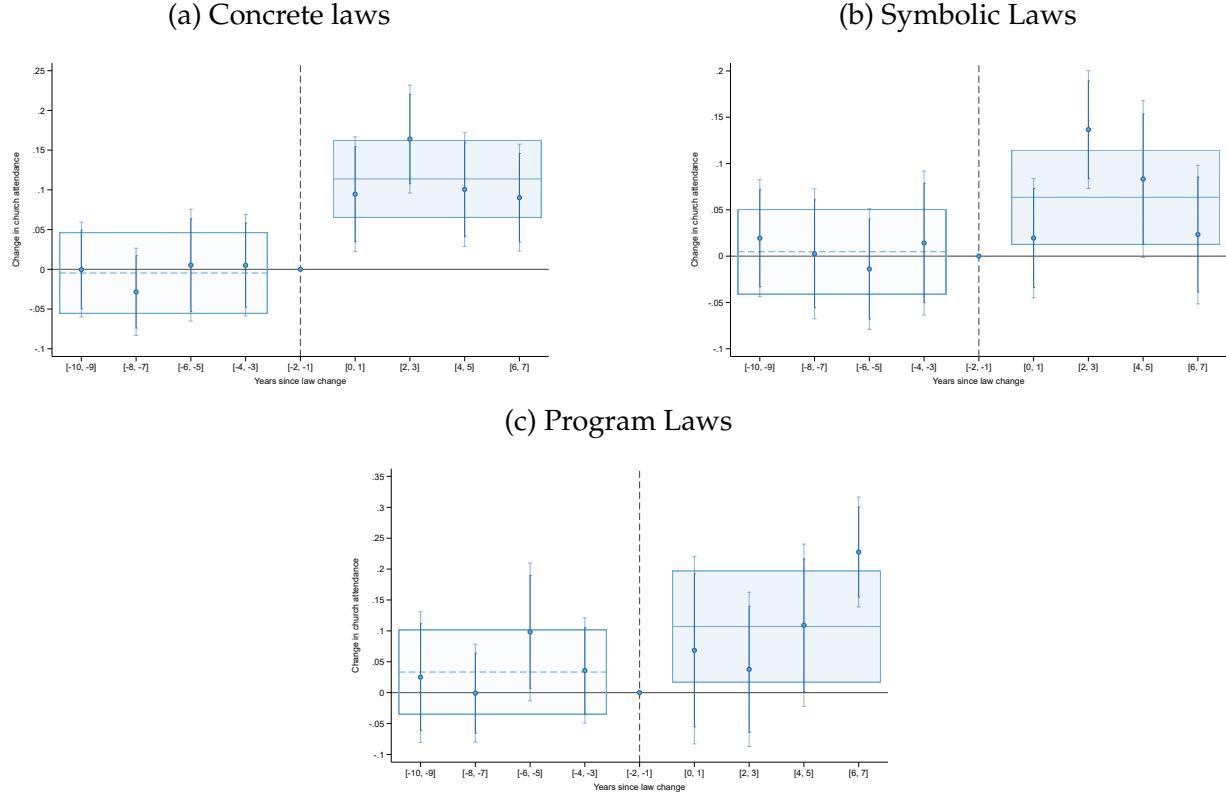
Figure 8: The impact of faith-based initiatives on faith-based organizations' dynamics

Notes: Regression estimates of the effect of faith-based initiatives on the dynamics of nonprofit organizations' entry and survival. Difference-in-differences estimates of a version of Equation (1) at faith-based organization level, identified among nonprofit organizations by its NTEE category or by the presence of religious words in its name, where the outcome is an indicator variable equal to one (rescaled to one hundred) from the time period in which the organization starts up (*entry*, in blue) or ceases to exist (*exit*, in orange), and zero otherwise or if the organization is incumbent throughout the period. The sample is the panel of the full history of faith-based organizations, re-balanced in a way that each organization is present throughout the event time window, with the only variation in the timing of entry or exit, using a fixed time window of seven years before and seven years after the occurrence of a faith-based initiative, and using nonprofit organizations in never-treated states as comparison group, for a total of 1,272,000 organization-by-stack-by-year observations. Regressions are estimated using ordinary least squares with sampling weights and the weighting scheme proposed by Wing, Freedman and Hollingsworth (2024). 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 differential probability (in percentage points) that faith-based organizations have to start their activities (or cease to exist), relative to faith-based organizations in states that do not experience faith-based initiatives. **Entry** – 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). **Exit** – Average share of faith-based organizations exiting 2.580%. Change in the share of exiting faith-based organizations post faith-based initiatives: 3.525%. Aggregate post-treatment effect: 0.279 (*std. err.* = 0.589, *t* = 1.527). 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. **Result**: More new faith-based organizations were established in response to the faith-based initiatives. There were no differences in entry and exit rates before the initiatives.

Figure 9: The impact of the faith-based initiatives on church attendance: heterogeneity of the first type of laws, DDD estimates

(a) Concrete laws

(b) Symbolic Laws

(c) Program Laws
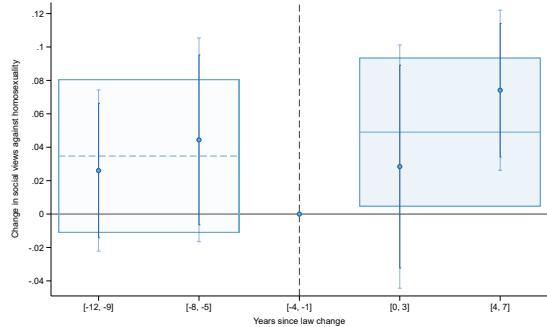
Notes: Regression estimates on the effect of the different types of faith-based initiatives on church attendance. Each panel shows triple difference estimates for a version of Equation (3) and the aggregate interactions in Equation (4). The treatment consists of an indicator variable on the presence of the indicated type of laws in the headers as the first faith-based initiative. We construct a different sample for each panel without including respondents from treated states that do not introduce as first law the indicated type of faith-based initiative, to avoid contamination in the post-treatment period. Regressions are estimated using ordinary least squares with sampling weights and the weighting scheme described in Wing, Freedman and Hollingsworth (2024). Panel (a): **Concrete law** as first initiative – Observations: 31,328; Average church attendance: 0.415; Change in church attendance post faith-based initiatives: -0.025. Aggregate post-treatment effect: 0.114 (*std. err.* = 0.024, *t* = 4.836). Panel (b): **Symbolic law** as first initiative – Observations: 21,118; Average church attendance: 0.423; Change in church attendance post faith-based initiatives: -0.026. Aggregate post-treatment effect: 0.064 (*std. err.* = 0.025, *t* = 2.573). Panel (c): **Program law** as first initiative – Observations: 16,405; Average church attendance: 0.402; Change in church attendance post faith-based initiatives: -0.022. Aggregate post-treatment effect: 0.107 (*std. err.* = 0.041, *t* = 2.590). 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 aggregate interactions are indicated as lines, with their 95% confidence intervals shown as boxes. Standard errors are clustered at the state level. **Result**: The rise in church attendance was mainly driven by the concrete laws.
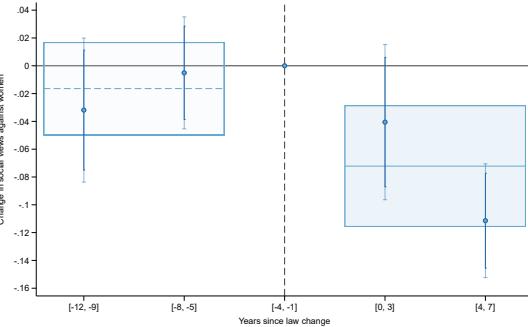
Figure 10: Impact of the faith-based initiatives on conservative-religious social views: DDD estimates
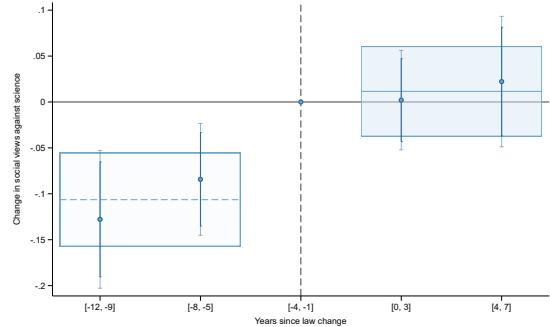
(a) Social views against homosexuals
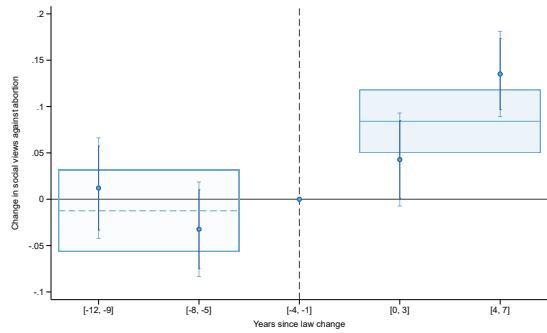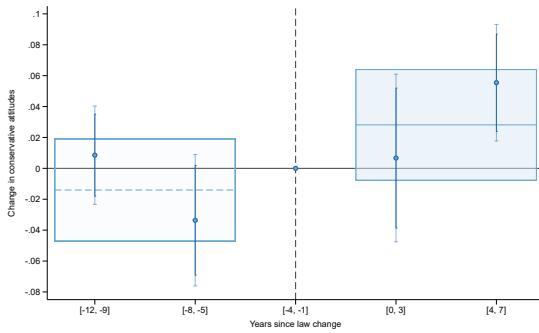
(b) Social views against women

(c) Social views against science
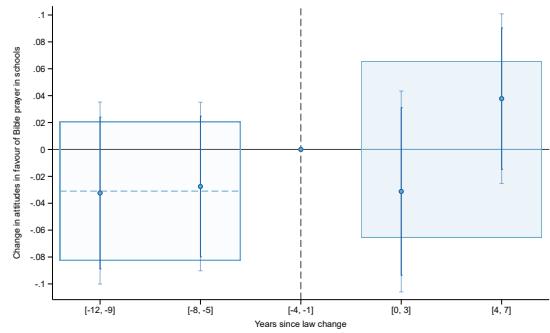
(d) Social views against abortion

(e) Conservative views

(f) In favor of prayers in schools
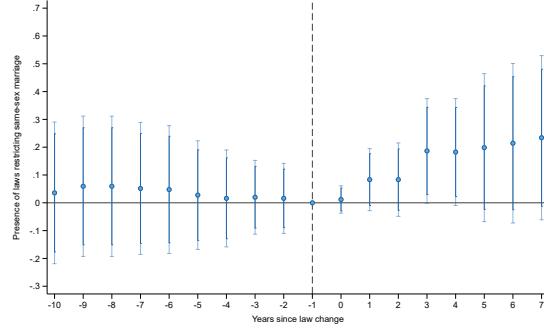
**Notes:** Regression estimates of the impact of the faith-based initiatives on social values. Each panel presents triple difference estimaates as in Equation (3) and aggregate interactions in Equation (3), where data are assembled as described in Section 3.2 using a fixed time window of twelve years before and seven years after the implementation of a faith-based initiative. As for estimates in Figure 5, we estimate more aggregate coefficients in Equation (3) pooling together two event time bins, relative to the average change $\tau = [-4, -1]$ between one year and four years before the introduction of the faith-based initiatives. Regressions are estimated using ordinary least squares with sampling weights and the weighting scheme proposed in Wing, Freedman and Hollingsworth (2024). Panel (a): **Social views against homosexuals** – Observations: 29,950; 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: 0.049 (*std. err.* = 0.022, *t* = 2.228). Panel (b): **Social views against women** – Observations: 28,343; 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: -0.072 (*std. err.* = 0.022, *t* = -3.342). Panel (c): **Social views against science** – Observations: 27,440; 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: 0.012 (*std. err.* = 0.024, *t* = 0.476). Panel (d): **Social views against abortion**: Observations: 33,641; 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: 0.084 (*std. err.* = 0.017, *t* = 4.991). Panel (e): **Conservative views** – Observations: 43,856; 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: 0.028 (*std. err.* = 0.018, *t* = 1.581). Panel (f): **Views in favor of prayers in schools** – Observations: 28,210; 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: 0.000 (*std. err.* = 0.033, *t* = 0.000). 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.
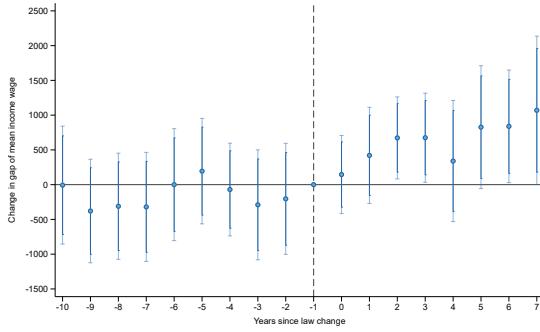
Result: Conservative-religious values singled out by Evangelicals (views against homosexuals, abortion, and general conservatism) seem to strengthen (particularly in the last period), while others weaken (views against women) and others are unchanged (views against science and favoring prayer in school). Prior to the initiatives, states do not differ in terms of most of the views. However, states were more progressive in terms of views on science, meaning that the post-effects here are under-estimated.

Figure 11: Effects of faith-based initiatives on state outcomes
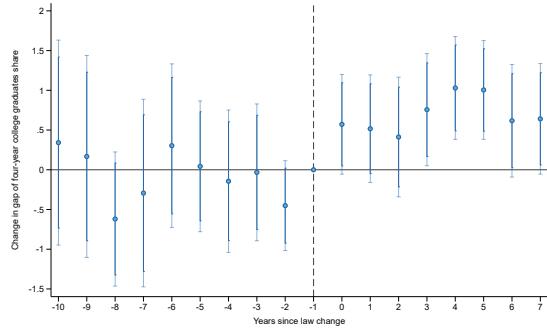
(a) Bans on gay marriage

(b) Gender wage gap

(c) Gender education gap

Notes: Regression estimates of the impact of the faith-based initiatives on different outcomes at state level. Each panel shows difference-in-difference estimates of Equation (1) on balanced stacked data at state and year level assembled as described in Section 3.2 using a fixed time window of ten years before and seven years after the introduction of the faith-based initiatives. The coefficients of the presence of faith-based initiatives interacted with the time periods are expressed relative to the year before treatment. In panel (a), the outcome is an indicator variable equal to one in the presence of laws restricting same-sex marriage (bans). Average presence of same-sex marriage bans: 0.142; change in presence of same-sex marriage bans after faith-based initiatives 0.241; aggregate post-treatment effect: 0.149 (*std. err.* = 0.086, *t* = 1.740). In panel (b), the outcome is a gender wage gap expressed as the difference between the average wage at state level between male and female at a given time. Average gender wage gap: 11915.460; change in gender wage gap after faith-based initiatives 3,253.867; aggregate post-treatment effect: 623.736 (*std. err.* = 284.957, *t* = 2.189). In panel (c) the outcome is an education gender gap expressed as the difference between the average share of male with a four years college degree and the equivalent for female. Average education gender gap: 1.763; change in education gender gap after faith-based initiatives -2.730; aggregate post-treatment effect: 0.694 (*std. err.* = 0.236, *t* = 2.941). Coefficients estimates on the event time interactions are plotted as dots with their 90% (95%) confidence intervals drawn as thick (thin) vertical lines. Standard errors are clustered at state level. **Result**: States are more likely to implement laws restricting gay marriage and see rising gender gaps in wages and education after implementation of the faith-based initiatives.

Tables

Table 1: Examples of missions of the faith-based nonprofit organizations

Social value	Mission	Organization
Christianity	To spread the Gospel of Jesus Christ and low cost housing.	Shepherd Ministries Inc.
	Centered around evangelism and sharing the gospel with our community.	We Will Go
	Evangelize, motivate and equip today's teenagers, their families and the church to live a lifestyle focused on Jesus Christ and nothing else.	Infocus Ministries Inc
	Spread the gospel of Jesus Christ by conducting religious worship services, preaching the word, and ministering through musical performances.	Steve & Mary Alessi Ministries Inc
Abortion	Providing truthful information to men, women, and families about adoption and the medical, emotional, and spiritual consequences of abortion.	Flowers For Heaven Inc
	Constructive alternatives to abortion and assisting women in crisis pregnancies. The mission of First Choice Center for Women, inc. is to see the word of god birthed in the hearts of men and women and to make abortion unnecessary and undesirable in our region.	First Choice Center for Women Inc
Traditional gender roles	Development of male based curriculum for leadership in the home and the church through small group mentoring and educating the family.	Oaks Of Righteousness Inc
	Under Christ, The Kings Universal call to serve, as men, pledge to unite and build men in the mold of a leader protector and provider, through education, formation, healing.	The Kings Men
Discipleship	We are developing relationships with teenagers in East St. Louis to inspire them to stay in school, further their education beyond high school, and bring them on as volunteers to our programs as mentors to other young people. Youth leadership development through religion based programs	Rebirth East Saint Louis
	To equip 7000 fathers to consistently lead their families in God's Word using ActorsBible by the year 2032. We envision fathers mentoring other fathers, who then mentor others, and so on to create a growing movement of men who lead well.	Inner-City Movement Inc
General culture	To educate and encourage individuals and organizations to advance positive outcomes in American culture.	USATransform (Ziklag)

Notes: Examples of missions of faith-based nonprofit organizations retrieved from the Nonprofit Explorer on the ProPublica website, https://projects.propublica.org/nonprofits/.

Table 2: The number of faith-based initiatives by type

Program laws	**45**
Prison	30
Youth/school	6
Drug/alcohol	9
Concrete laws	**136**
Allocate positions in state advisory boards to faith-based representatives	70
Appropriations to FBOs	58
Exempt FBOs from standard regulations	6
Assist FBOs with grant writing process	2
Symbolic laws	**151**
Office of Faith Based Initiative	11
Encourage the state to partner with FBOs	132
Create a faith-based advisory board	8
Total	**332**

Notes: Data on faith-based initiatives retrieved from LexisNexis for the period 1996-2009 by Sager (2010).

Table 3: Validity checks of law measure based on central faith-based institutions

Dependent variable:	Liaison	Office	Grant	Grant ($1000)
	(1)	(2)	(3)	(4)
Panel A. Faith-based initiative in year $t = 0$				
$FBI_{sc} \times Post_{ct}$	0.200*	0.264***	0.267***	479.382**
	(0.110)	(0.055)	(0.060)	(206.070)
Observations	2,196	2,196	2,196	2,196
State-Cohort FE	✓	✓	✓	✓
Event Time-Cohort FE	✓	✓	✓	✓
Panel B. Faith-based initiative in year $t = 1$				
$FBI_{sc} \times Post_{ct}$	0.233**	0.267***	0.159***	280.896*
	(0.104)	(0.055)	(0.046)	(142.614)
Observations	2,196	2,196	2,196	2,196
State-Cohort FE	✓	✓	✓	✓
Event Time-Cohort FE	✓	✓	✓	✓

Notes: This table examines the relationship between our measure of faith-based initiatives and the implementation of both institutions and grants at state level. For columns 1-3, the dependent variable is an indicator variable equal to one if a state hosts at a given time the indicated institution in the header, and zero otherwise. In column 4, the dependent variable is the total grant amount in thousand dollars assigned to a state liaison at a given time. The sample is assembled as described in Section 3.2 for a balanced panel at state year level using a fixed time window of ten years before and seven years after the introduction of the faith-based initiatives. Each panel presents difference-in-differences estimates of a version of Equation (2) at state by event time by treatment cohort level. The coefficient of interest is the differential probability of having the specific institution, or the marginal change in grant amount, for states implementing faith-based initiatives after their implementation, compared to states that did not implement any. We express coefficients relative to the latest year before the faith-based initiatives, absorbing pre-treatment coefficients as in Equation (1) and aggregating the post-treatment period. The two panels differ for the timing of the treatment: while in Panel (a) we assume treatment to start in the year of implementation of the first faith-based initiative, in Panel (b) we consider the year after the first faith-based initiative as starting year. Regressions are estimated using ordinary 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: The timing and spread of the faith-based initiatives correlates with the timing and spread of the faith-based liaisons, their offices, and their grants.

Table 4: The impact of the faith-based initiatives on church attendance: specification using contiguous counties

	(1)	(2)	(3)	(4)
$FBI_{nk} \times Post_{nt}$	0.034		0.035	
	(0.031)		(0.028)	
$FBI_{nk} \times Post_{nt} \times \mathbf{1}\,(r = Protestant)_{inkt}$		0.116**		0.124**
		(0.050)		(0.052)
Observations	4,188	4,158	4,178	4,148
Mean Dep. Var.	0.452	0.452	0.453	0.453
Share of Protestants	0.408	0.408	0.408	0.408
Number of States	11	11	11	11
Number of County Pairs	28	28	28	28
County Pair-Time FE	✓		✓	
County FE	✓		✓	
County-Protestant FE		✓		✓
County-Time FE		✓		✓
County Pair-Protestant-Time FE		✓		✓
Individual Controls			✓	✓

Notes: Regression estimates of the impact of the faith-based initiatives on church attendance matching respondents of the General Social Survey (GSS) from contiguous counties. The sample consists of respondents from pairs of contiguous counties separated by a state border, where we compare respondents in a county belonging to a state introducing the faith-based initiatives with respondents of a neighbour county belonging to a state that have not introduced them yet. When assembling the data, we trim the panel within a county pair to exclude years where the comparison group would get treated, to avoid contamination in the post-treatment period. In columns 1 and 3, we present estimates of the coefficient of interest in Equation (5) with and without individual controls, respectively. In columns 2 and 4, we show estimates of the coefficient of interest in Equation (6) with and without individual controls, respectively. Regressions are estimated using ordinary least squares with sampling weights at individual level. The different number of observations within no controls and controls is given by singleton observations dropped in the estimation. We two-way cluster standard errors at the state and county pair level. *, **, and *** indicate significance at the 10%, 5%, and 1% level. **Result**: Even when restricting comparison to counties on either side of a state border, the faith-based initiatives seem to have increased church attendance.

Divine Policy: The Impact of Religion in Government
SUPPLEMENTAL APPENDIX

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Contents

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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/`).

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[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/gov ernment/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,

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[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

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[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 desirable, 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

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[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"

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[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
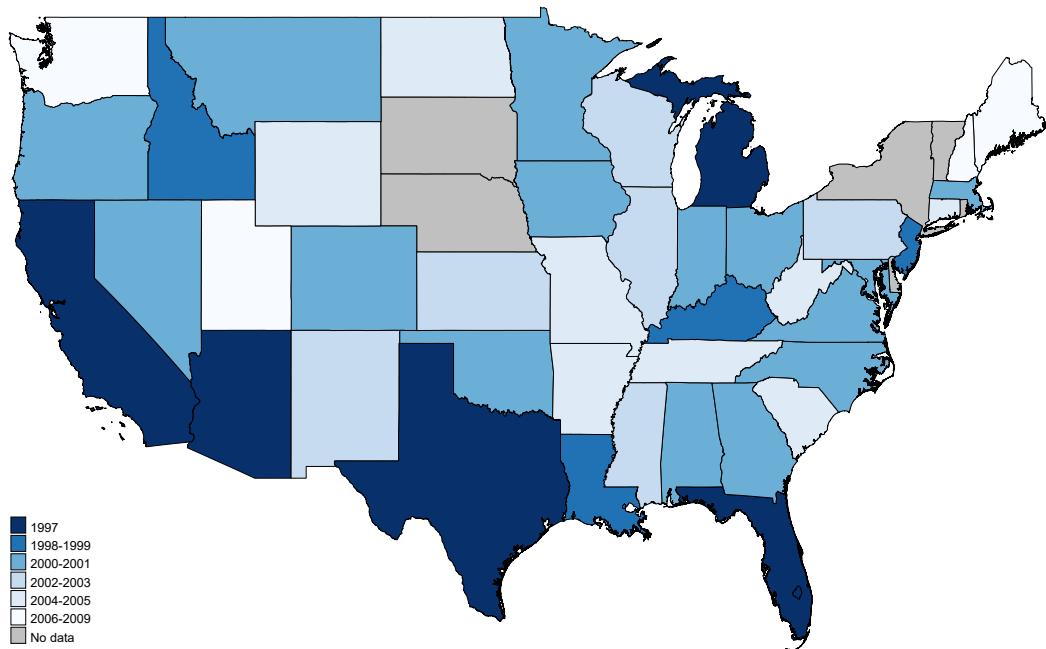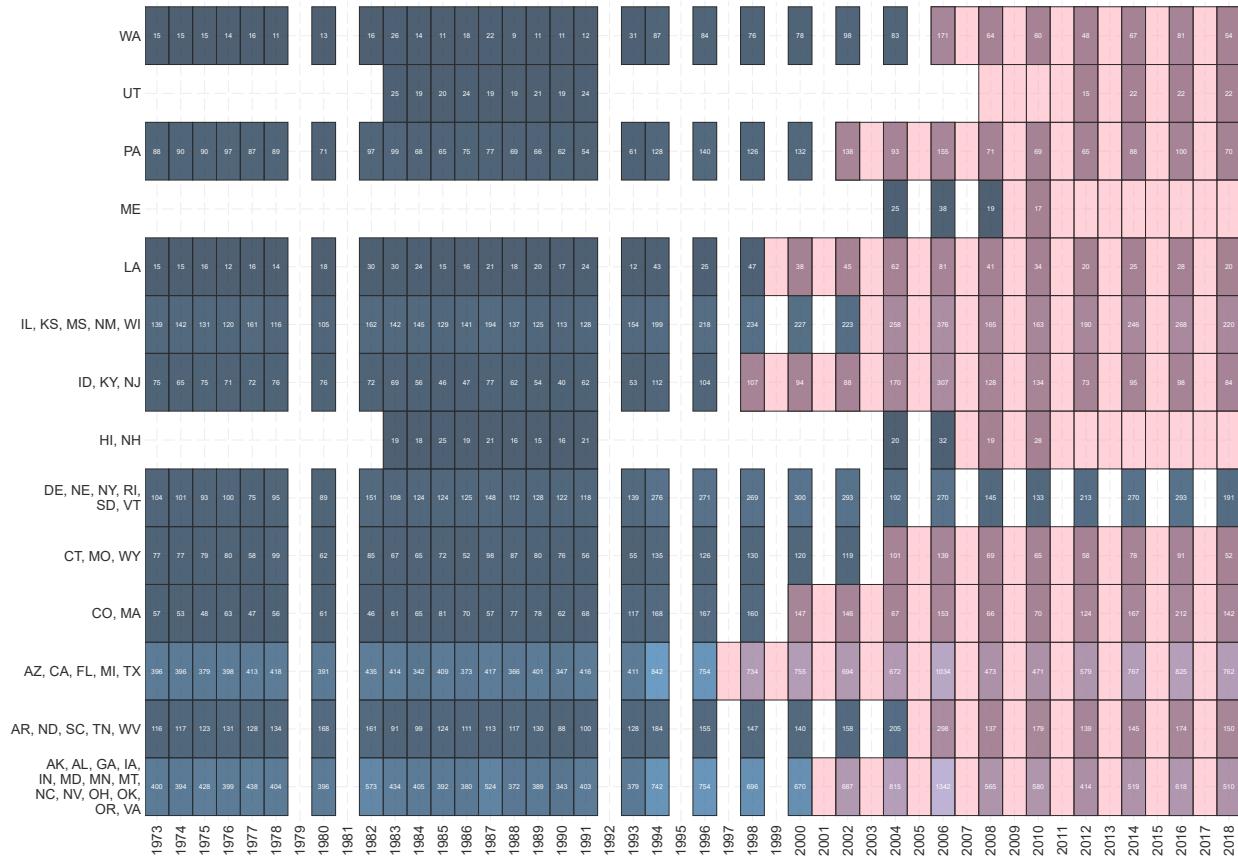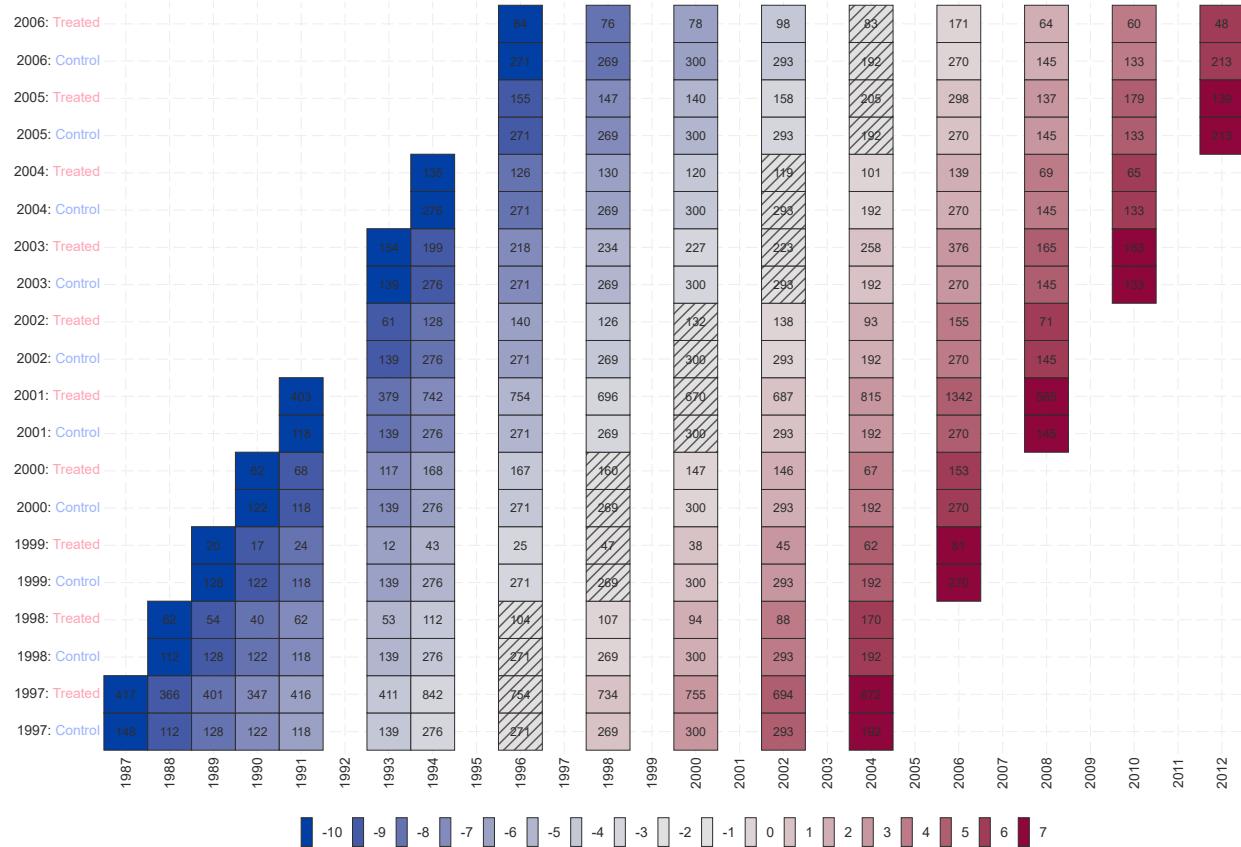
Figure A2: Sample composition of the General Social Survey (GSS): any type of faith-based initiatives
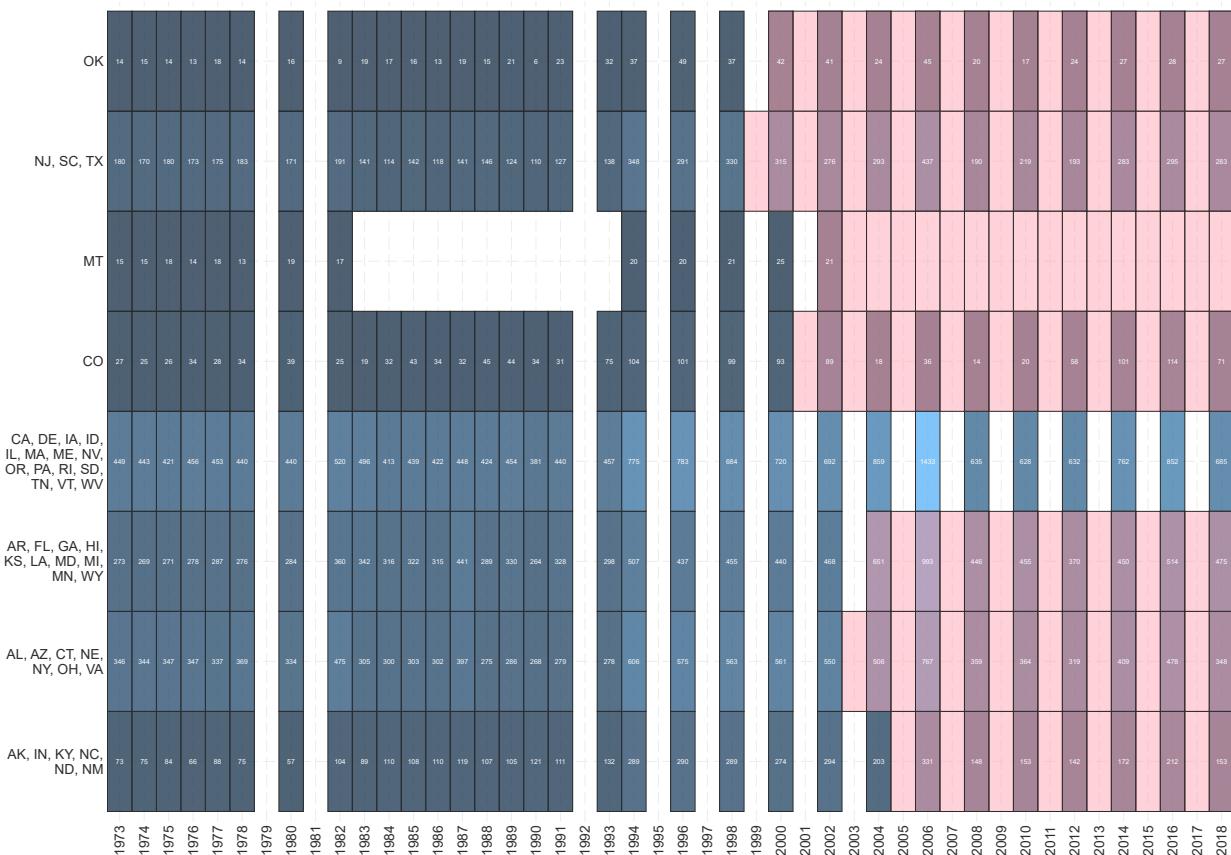
Notes: This figure illustrates the composition of the baseline sample of the General Social Survey (GSS) from the earliest wave available (1973) to the latest (2018), matched with the faith-based initiatives in Sager (2010). Each horizontal group defines a treatment cohort of the indicated states (or 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)
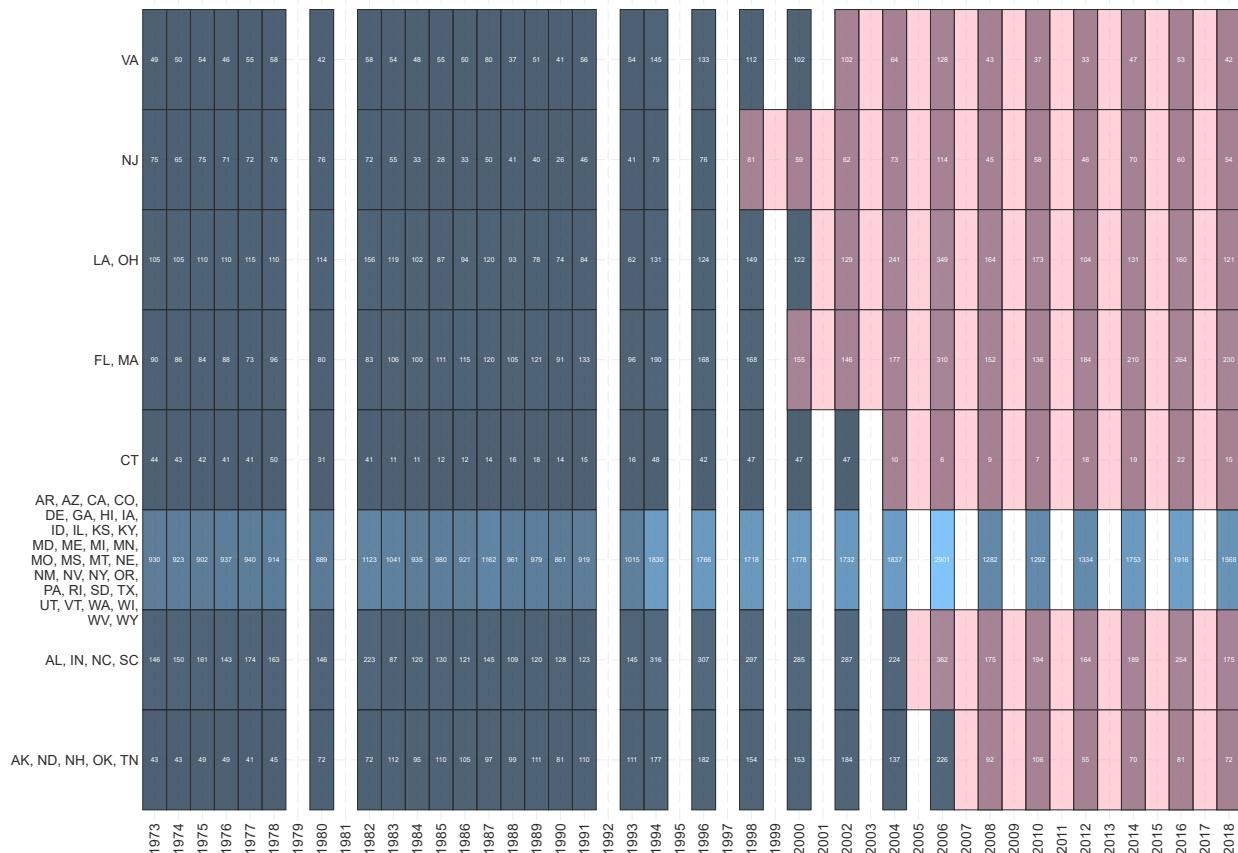
Notes: This figure illustrates the structure of the General Social Survey (GSS) data for measuring church attendance after the stacking procedure described in Section 3.2, which is our baseline sample trimming the data on a fixed time window of ten years before and seven years after the introduction 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
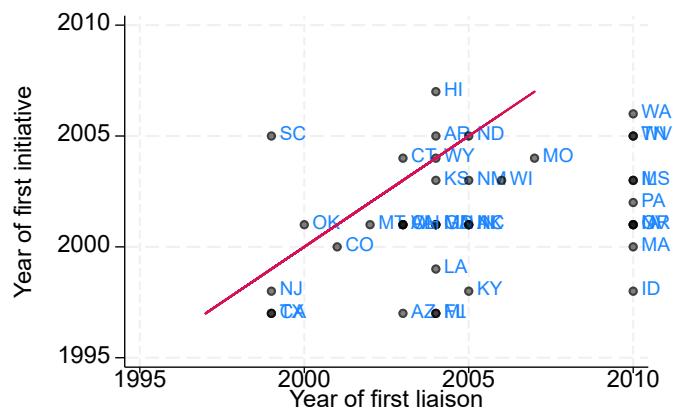
Notes: This figure illustrates the composition of the baseline sample of the General Social Survey (GSS) from the earliest wave available (1973) to the latest (2018), matched with the faith-based liaisons described in Section 2.3. Each horizontal group defines a treatment cohort of the indicated states (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).
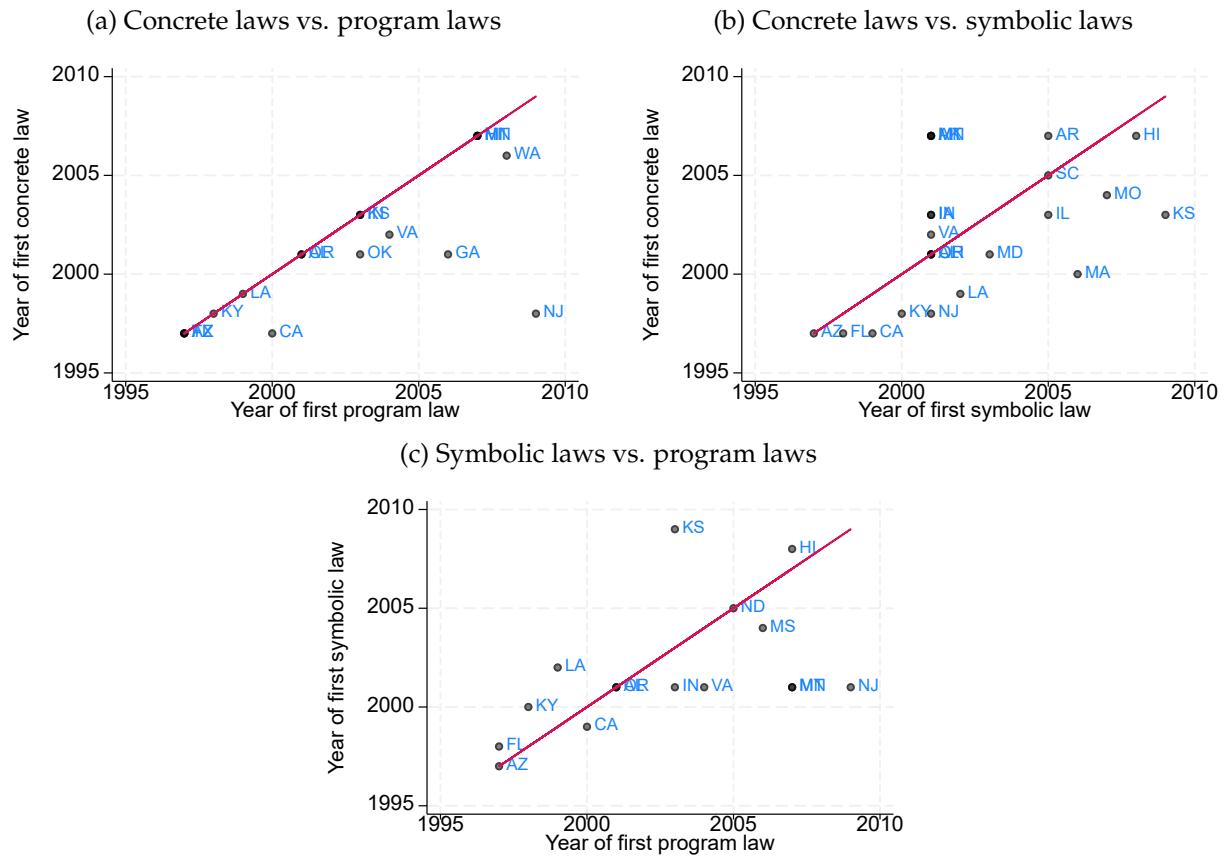
Figure A6: Timing of faith-based initiatives versus timing of liaisons

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.
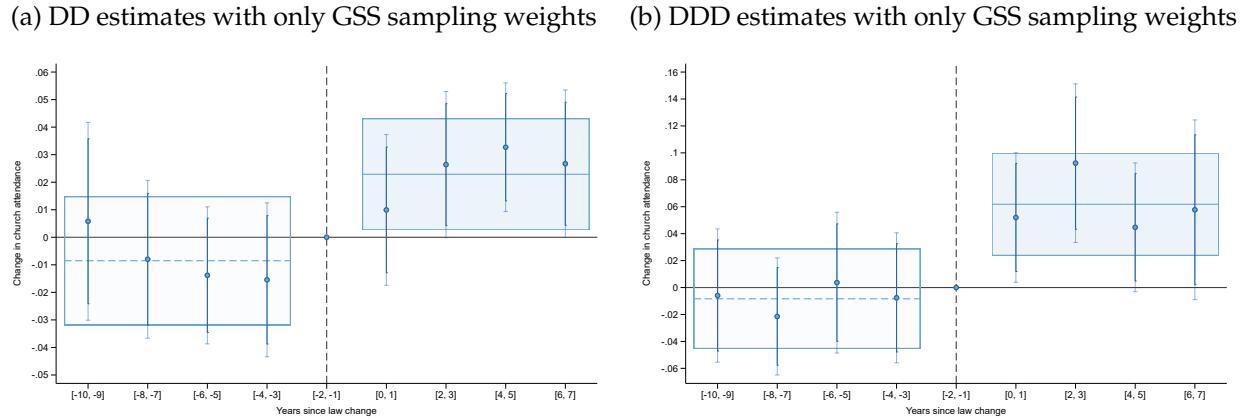
Figure A7: The timing of the three types of initiatives

(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.
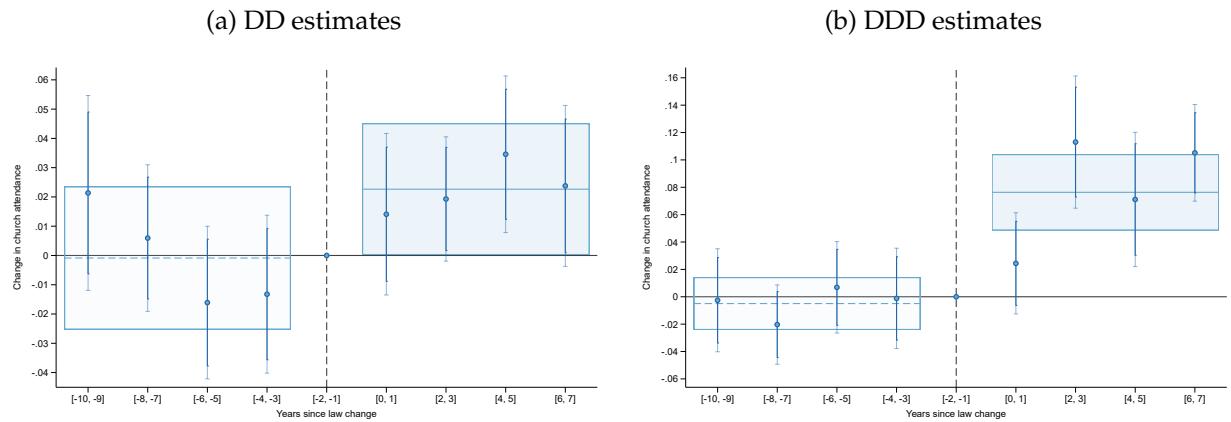
Result: All program laws and most symbolic laws are implemented after a concrete law. There is no pattern in implementation timing between symbolic and program laws.

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
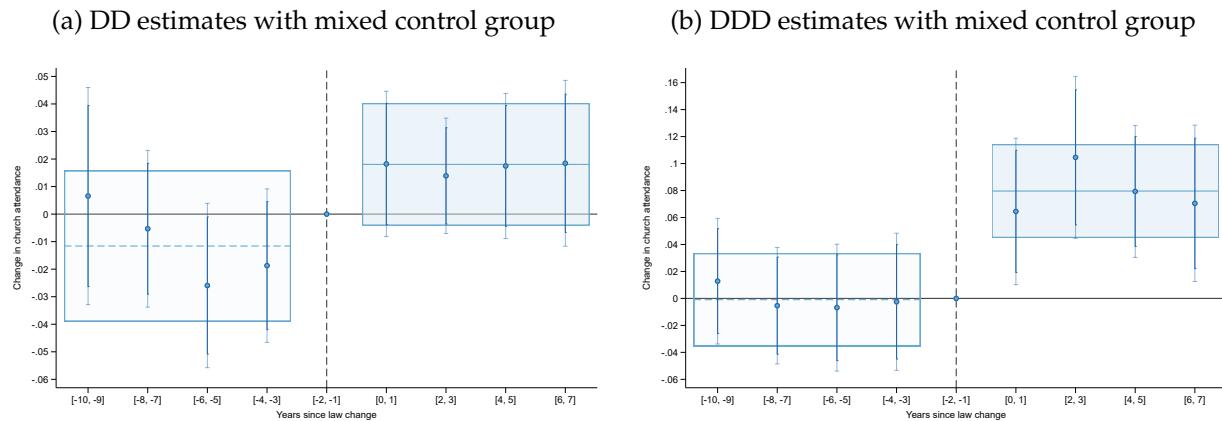
**Notes:** This figure shows estimates for the same specification and sample of Figure 4, with the single difference that in this case we do not apply the weighting scheme described in Wing, Freedman and Hollingsworth (2024). Regressions are still estimated using weighted least squares, but only with 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
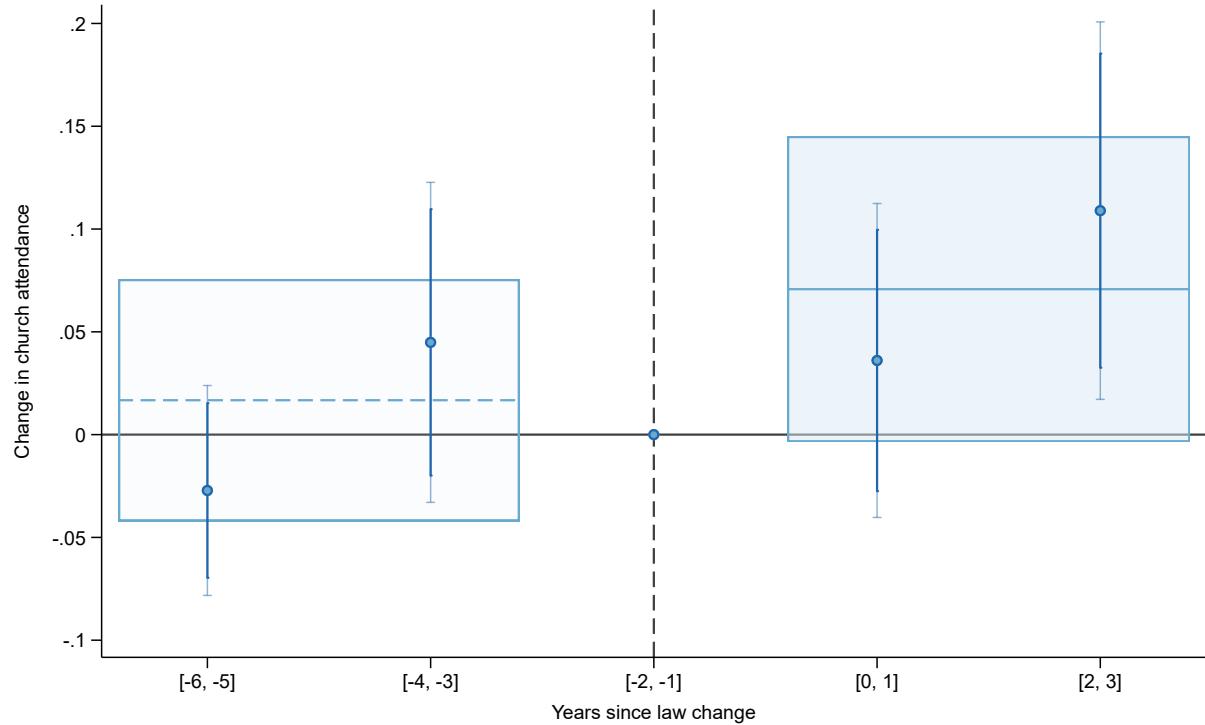
Notes: In this figure, we present estimates analogous to Figure 4 with the difference that, building the stacked sample, we exclude respondents in states with "incomplete" waves – that we cannot fully follow for the entire pre-treatment and post-treatment period within a treatment cohort – or with less 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
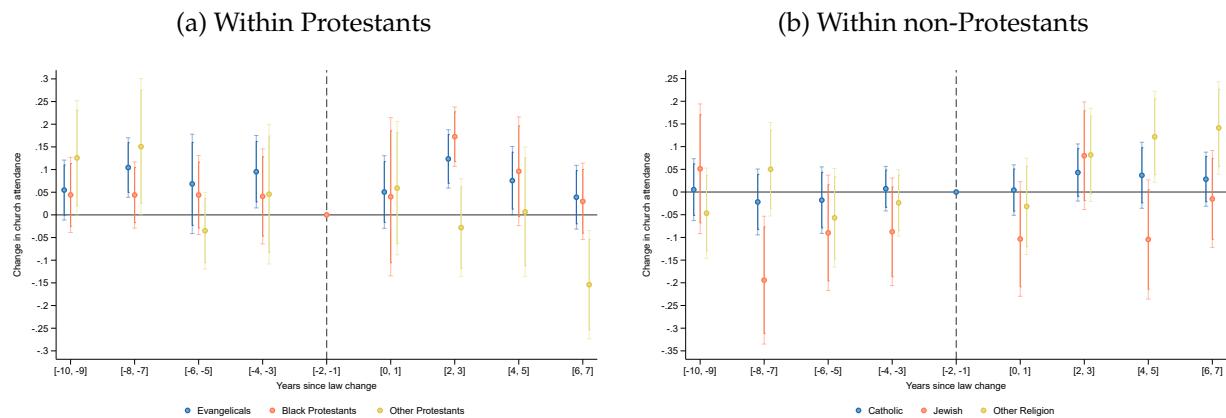
Notes: In this figure, we show estimates analogous to Figure 4 but changing the comparison group. We build the sample of interest in the same way as described in Section 3.2, but in this case we consider as comparison group respondents that are both in states that do not introduce faith-based initiatives 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).

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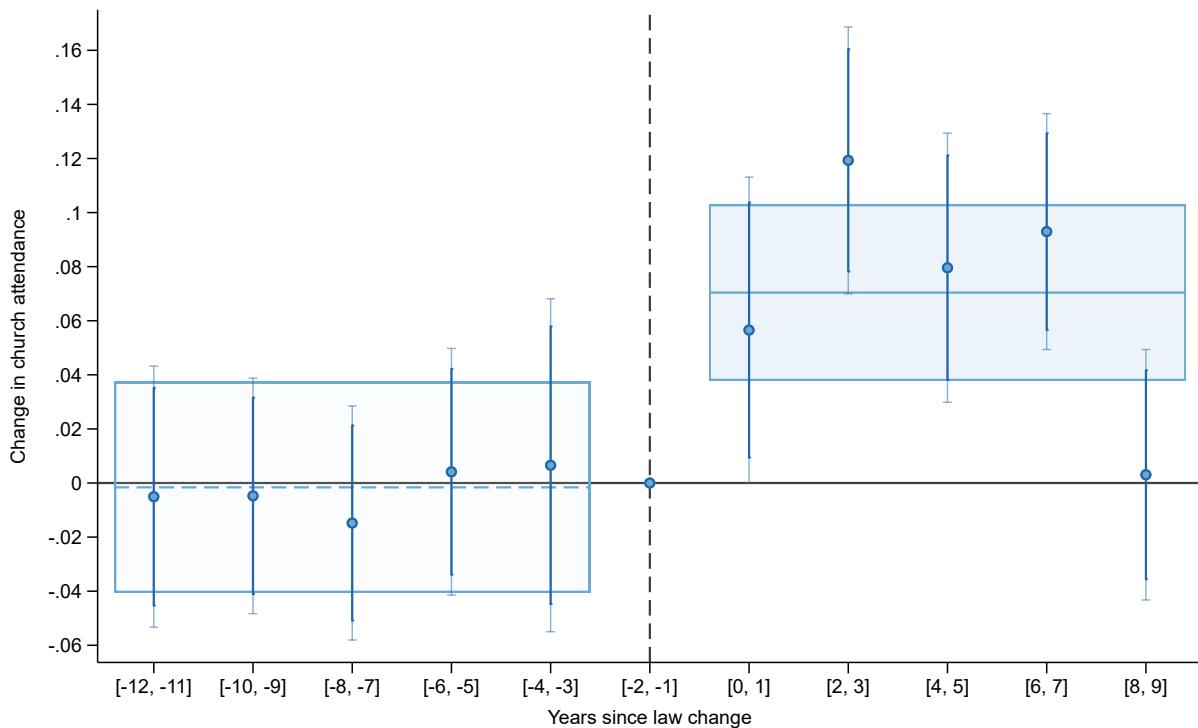
Figure A12: The impact of the faith-based initiatives on church attendance: within-Protestant and within non-Protestant comparisons

(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.
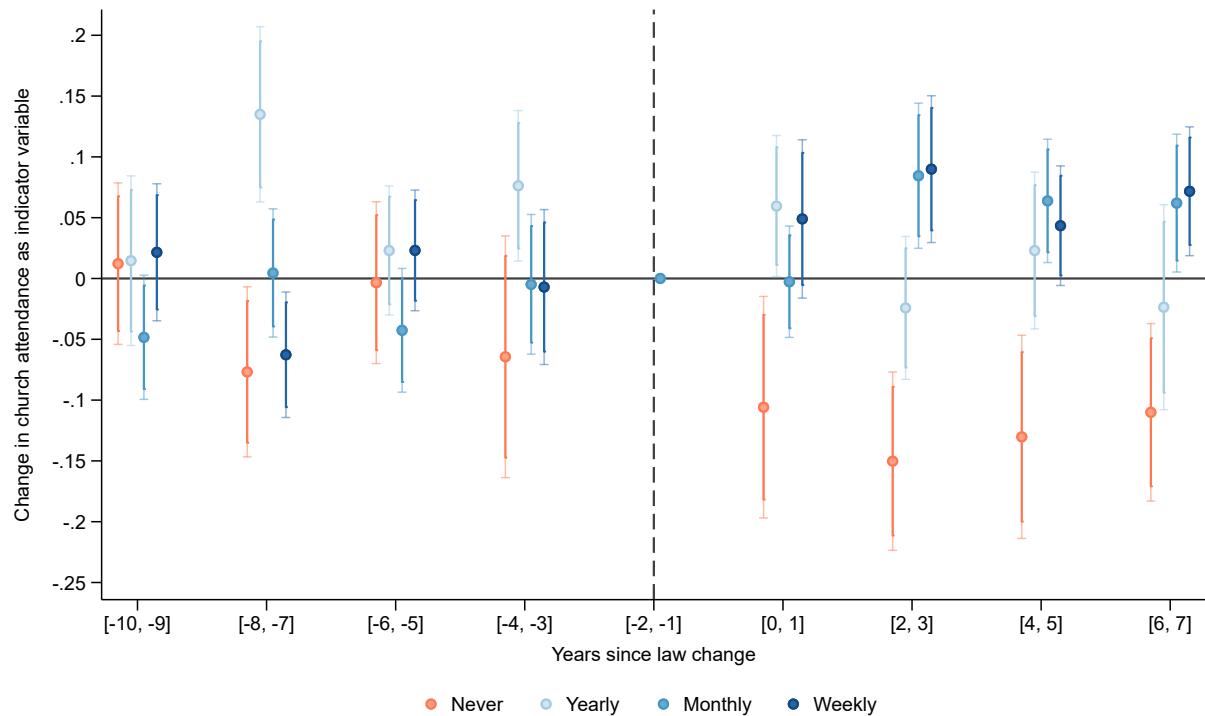
Figure A13: The impact of the faith-based initiatives on church attendance: extended event time window
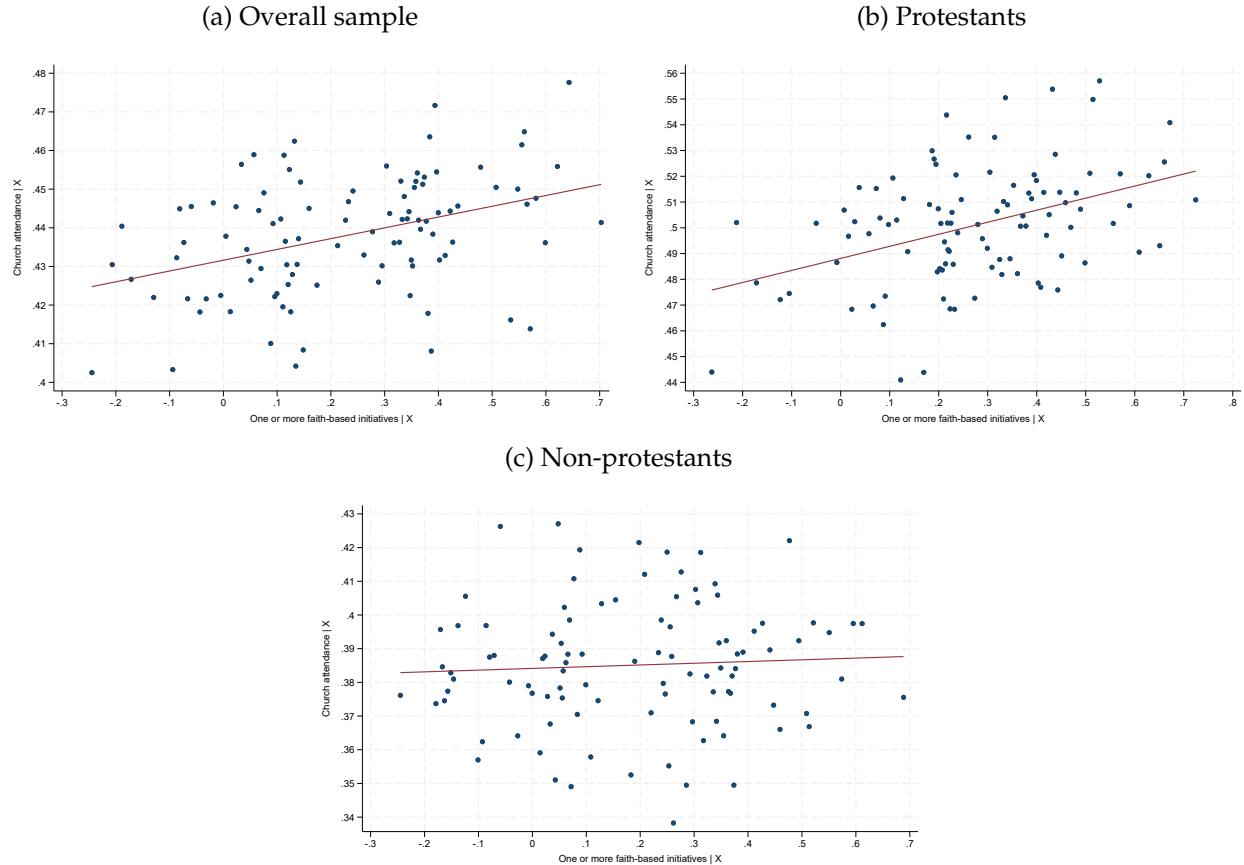
Notes: This figure depicts the same estimates of Figure 4b, with the only difference of using an extended time window. We find estimates of Equation 3 and the aggregate interaction in Equation 4 on individual-level data built using a fixed time window of twelve years before and nine years after the implementation 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.
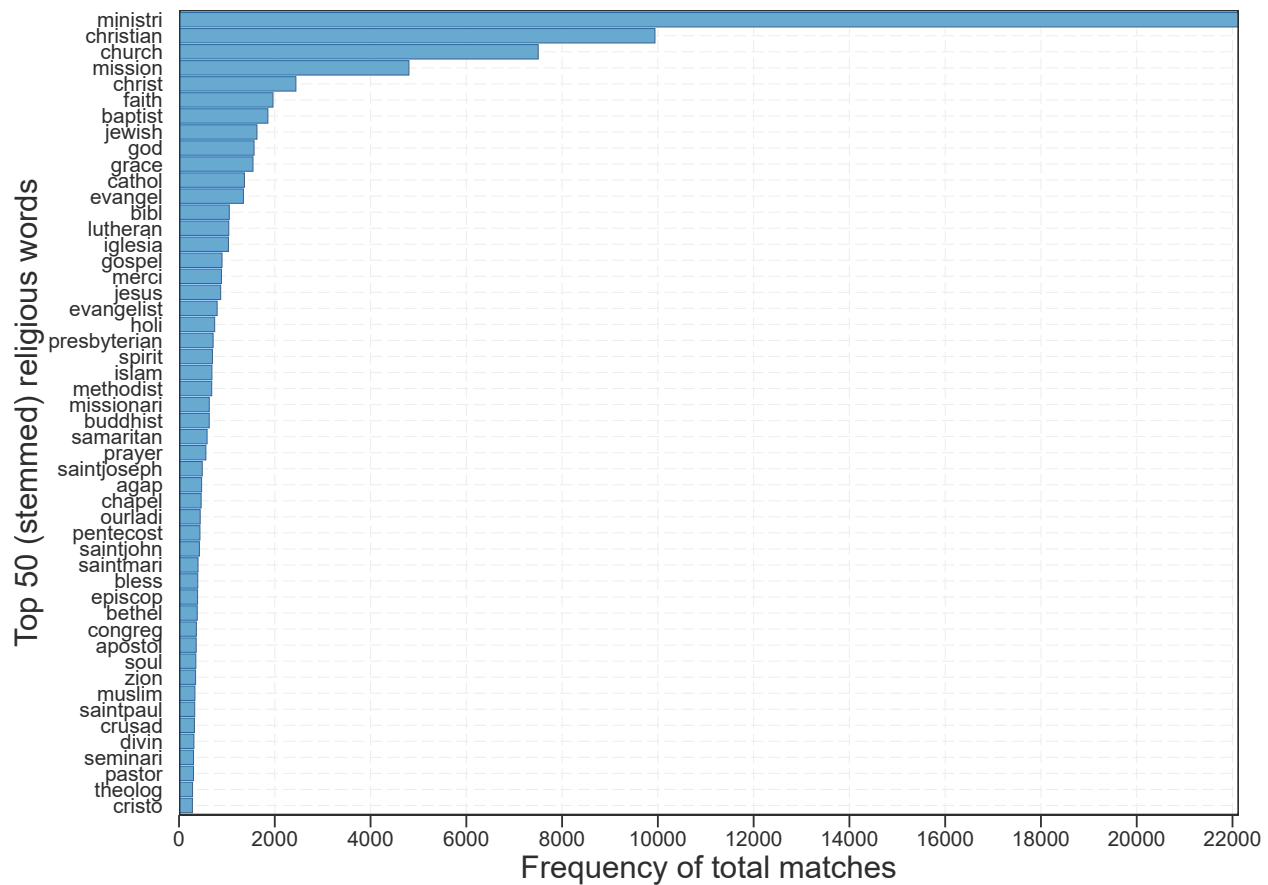
Figure A15: Binned added-variable plots of the impact of the faith-based initiatives on church attendance

(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.
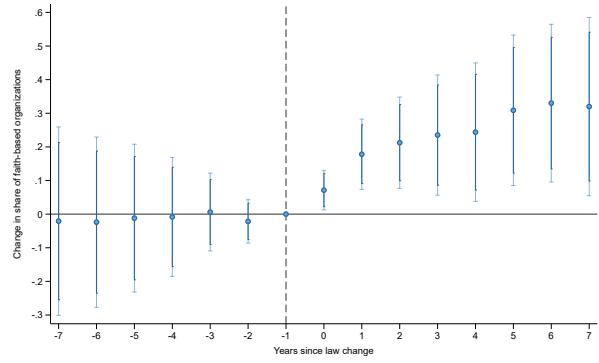
Figure A16: Top 50 religious terms in the names of nonprofit organizations
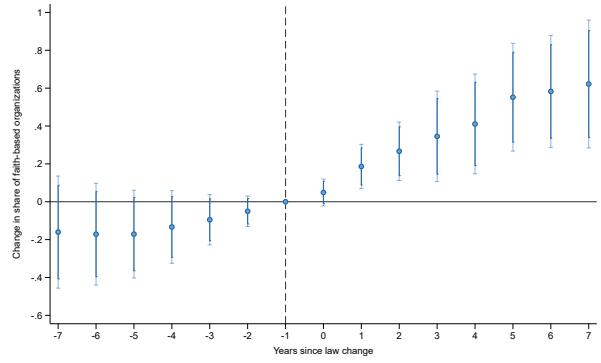
Notes: This bar graph shows the ranked frequencies (the number of times a word appears in the corpus) of the top fifty religious terms in the unique names of the nonprofit organizations in the collected sample after pre-processing, tokenization and stemming of the strings, similar to Figure 6.

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
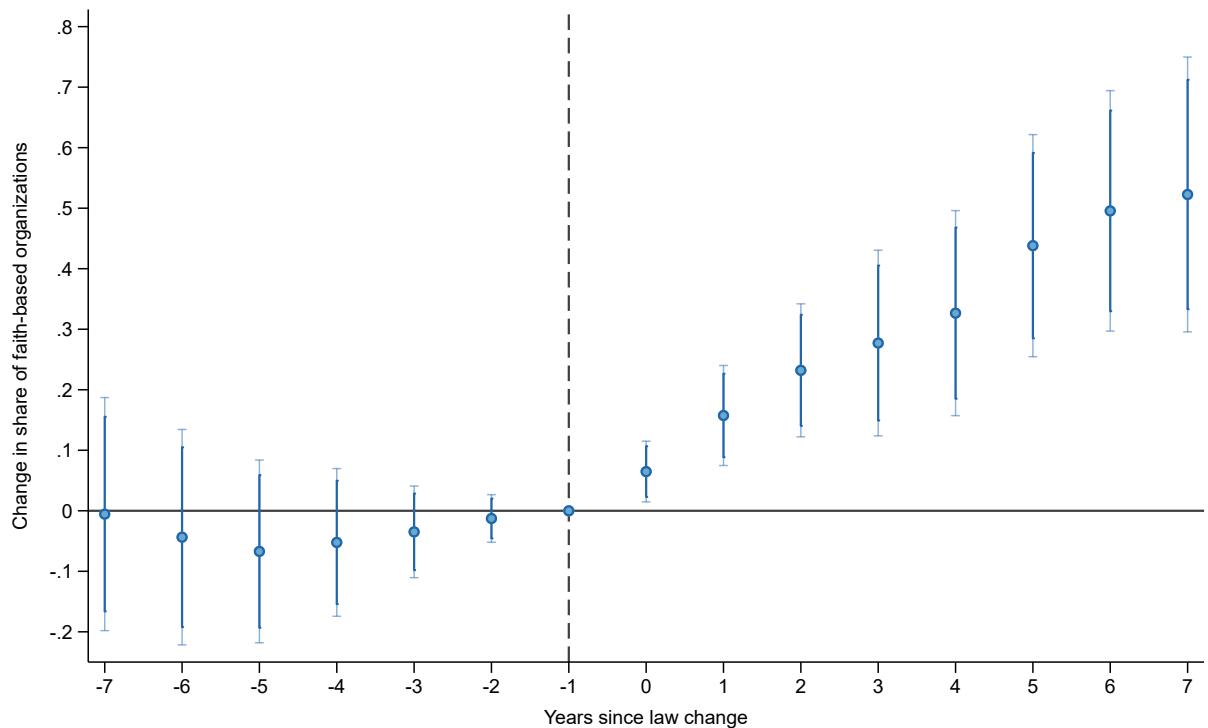
(b) Religious words in name
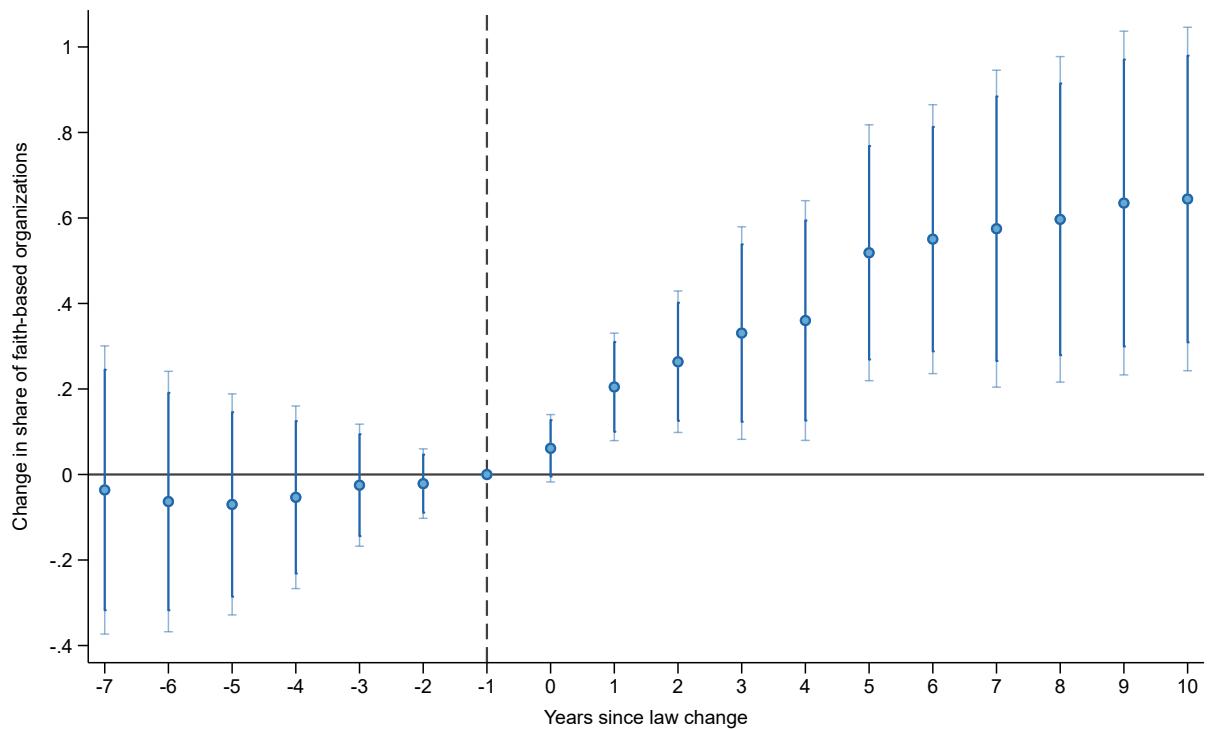
Notes: This figure is analogous to Figure 7, with the only difference that we decompose the previously shown outcome in two separate outcomes for labelling a nonprofit organization as religious. Panel (a) presents the difference-in-differences estimates using as outcome an indicator variable equal to 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).

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Figure A18: The impact of faith-based initiatives on the presence of faith-based organizations: Full sample of nonprofits
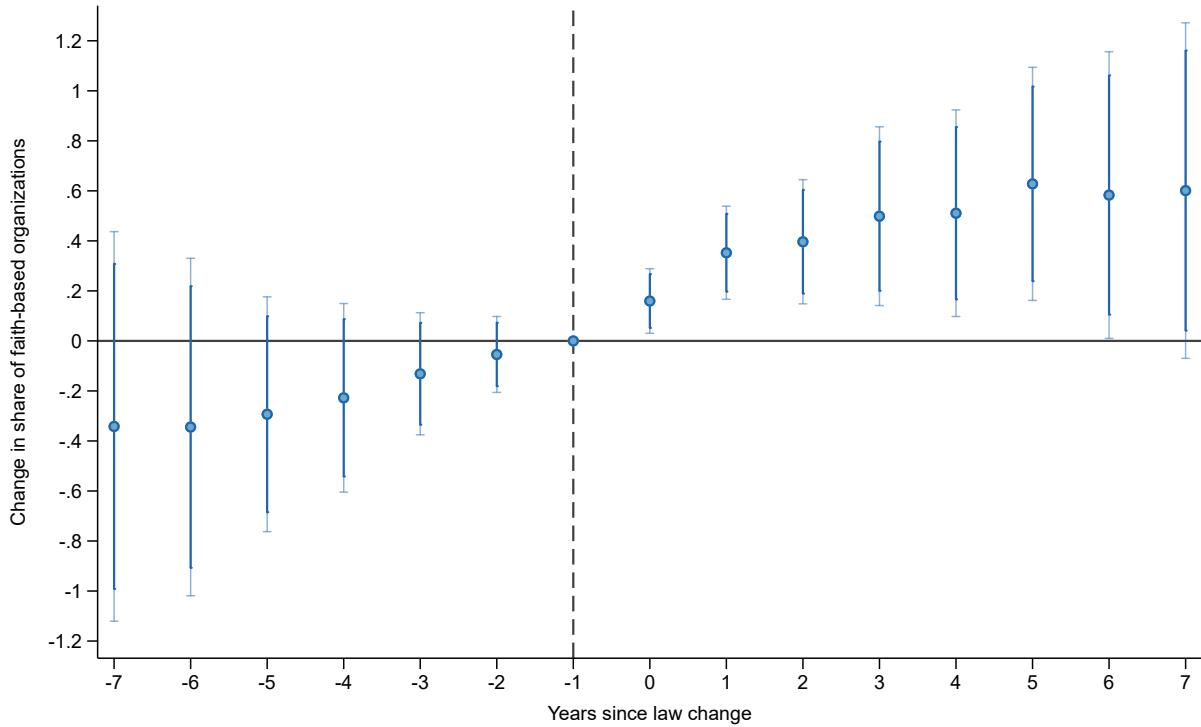
Notes: This figure presents similar estimates as in Figure 7, but considering the full unbalanced sample of nonprofit organizations instead of solely 501(c)(3) operating public charities. We include fixed effects at nonprofit organization level for the IRS subsection by treatment cohort, and the nature 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
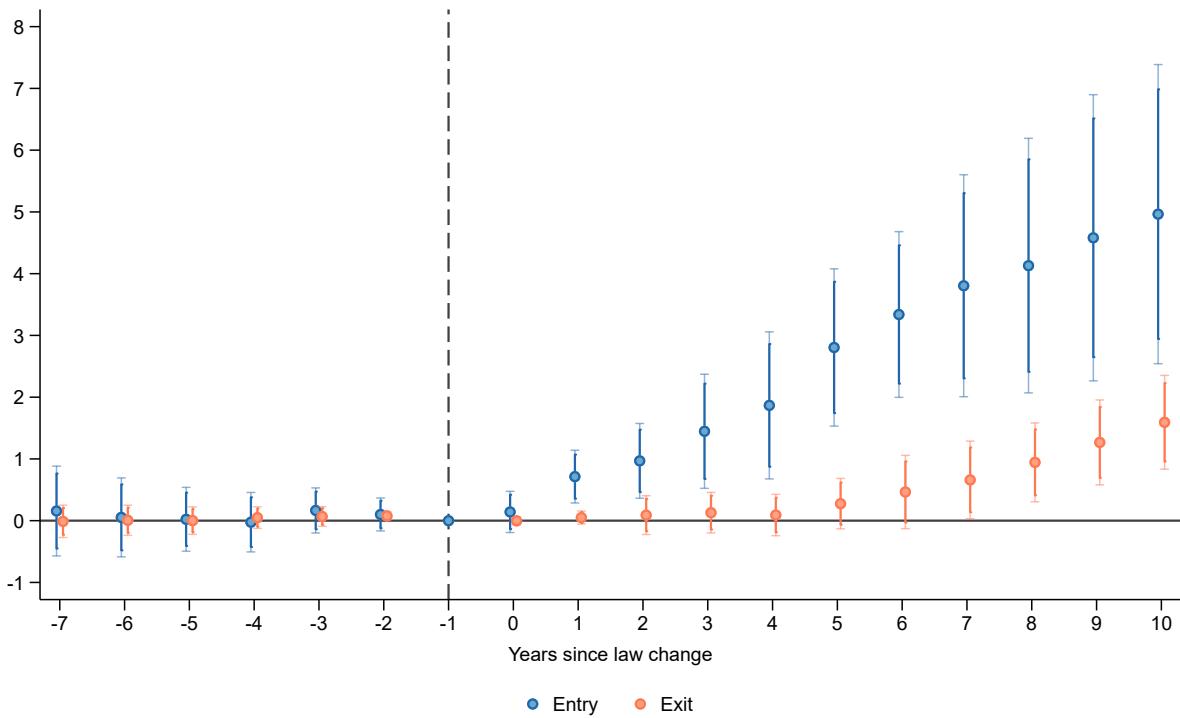
Notes: This figure shows similar estimates as in Figure 7, with the difference that the sample is stacked using a fixed time window up to ten years after the occurence of a faith-based initiative – with 2019 as the ending calendar year in the latest treatment cohort – for a total of 9,190,147 organization-by-stack-by-year 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
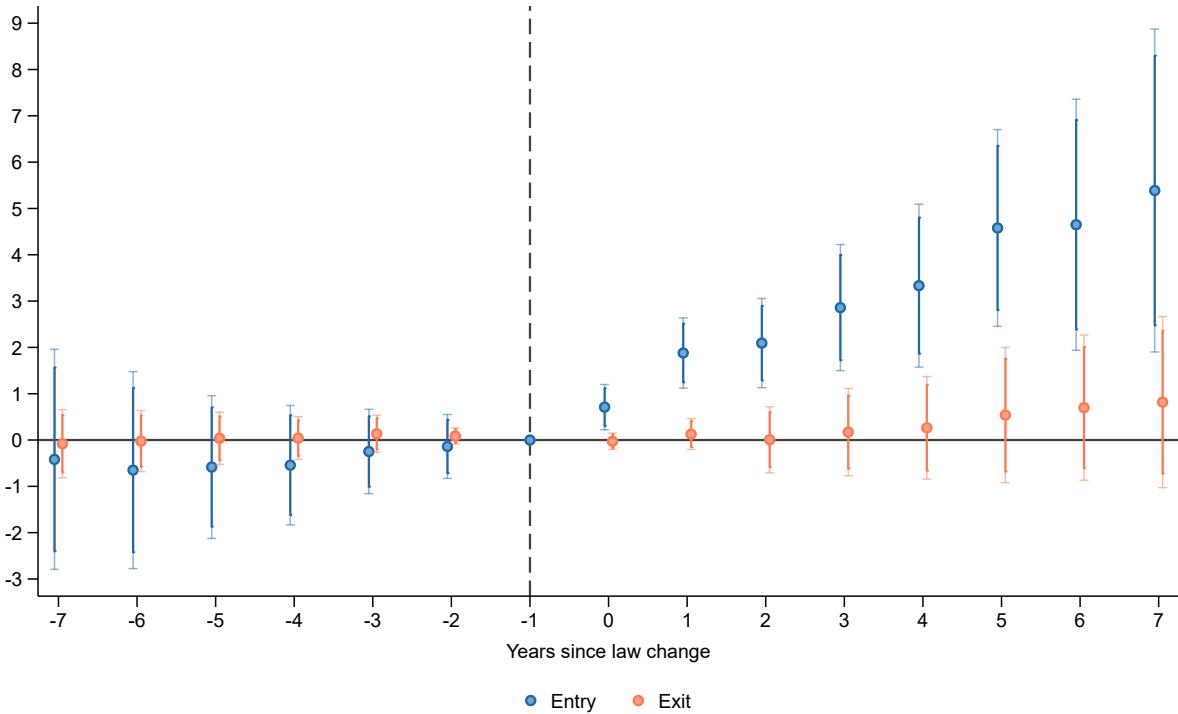
Notes: Regression estimates of the effect of faith-based initiatives on the presence of faith-based organizations. We present results of the difference-in-differences estimates of a version of Equation 1 at state level where the outcome is the (percentage) share of faith-based organizations at a given 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

Years since law change

● Entry   ● Exit
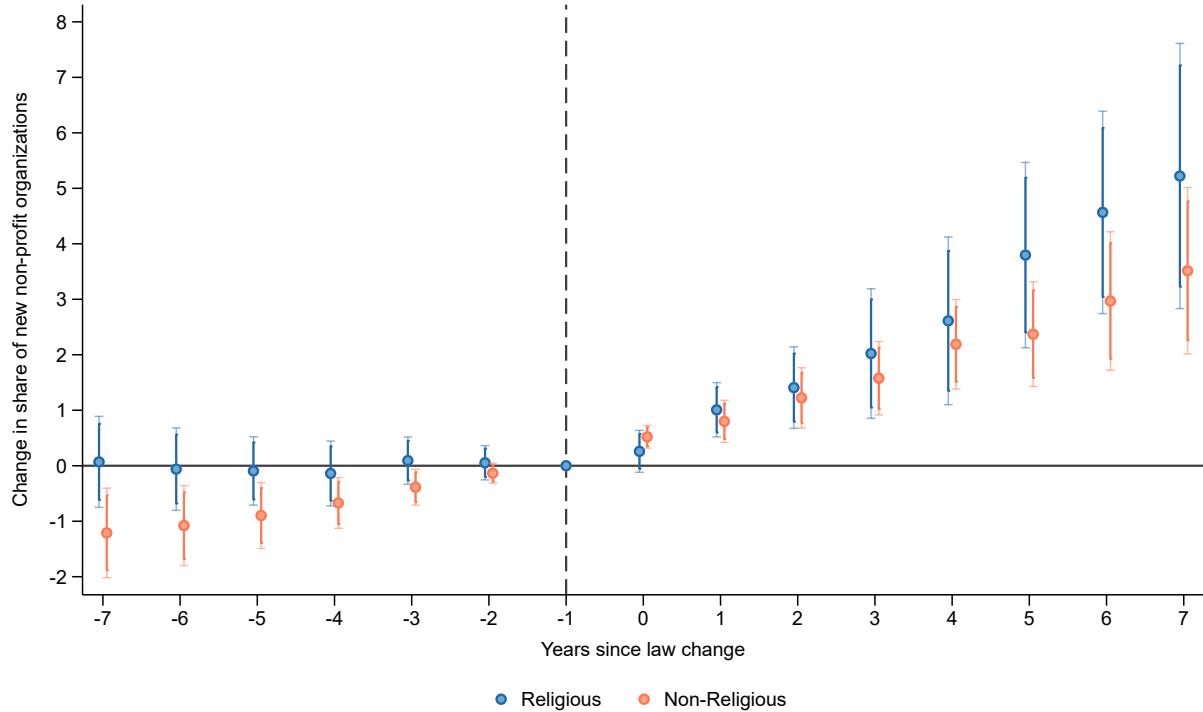
Notes: This figure is equivalent to Figure 8, with the only difference that for these estimates we use a fixed time window of three additional time periods, up to ten years after the occurrence of a faith-based initiative, for a total of 1,771,146. **Entry** – Average share of faith-based organizations 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
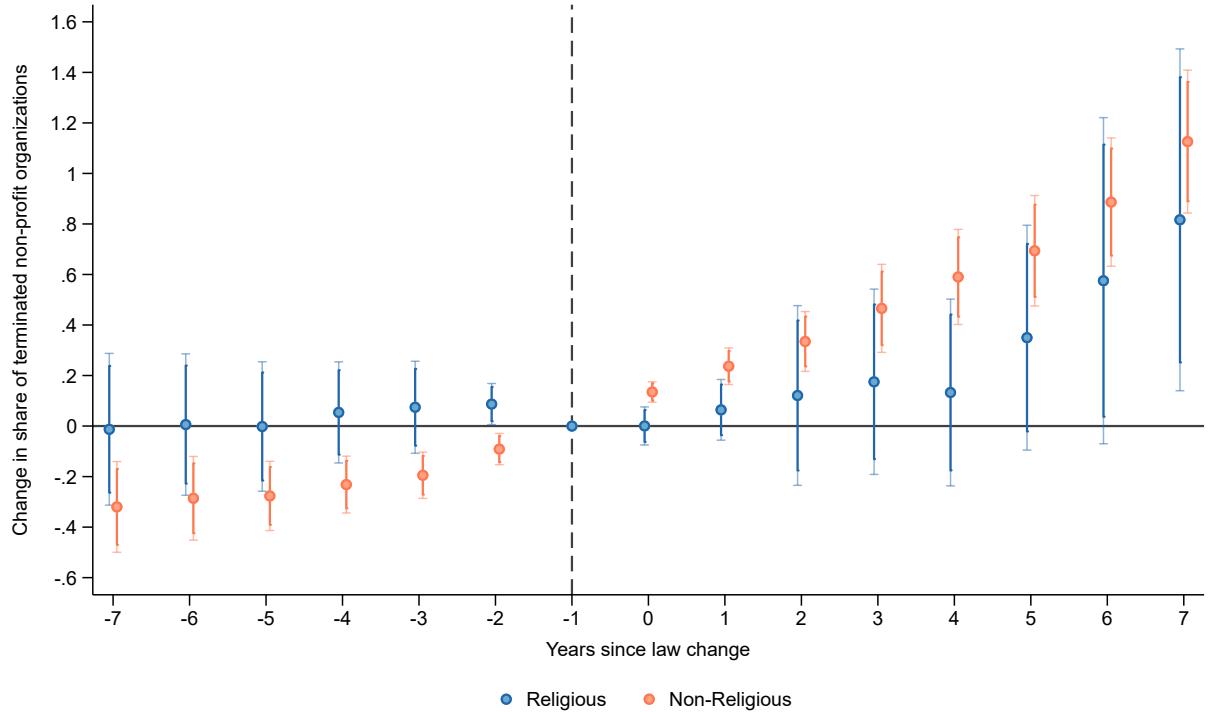

Notes: Regression estimates of the effect of faith-based initiatives on the dynamics of faith-based organizations. We show results of the difference-in-differences of a version of Equation 1 at state level where the outcome is the (cumulative) share of entering (or terminating) faith-based organizations 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.
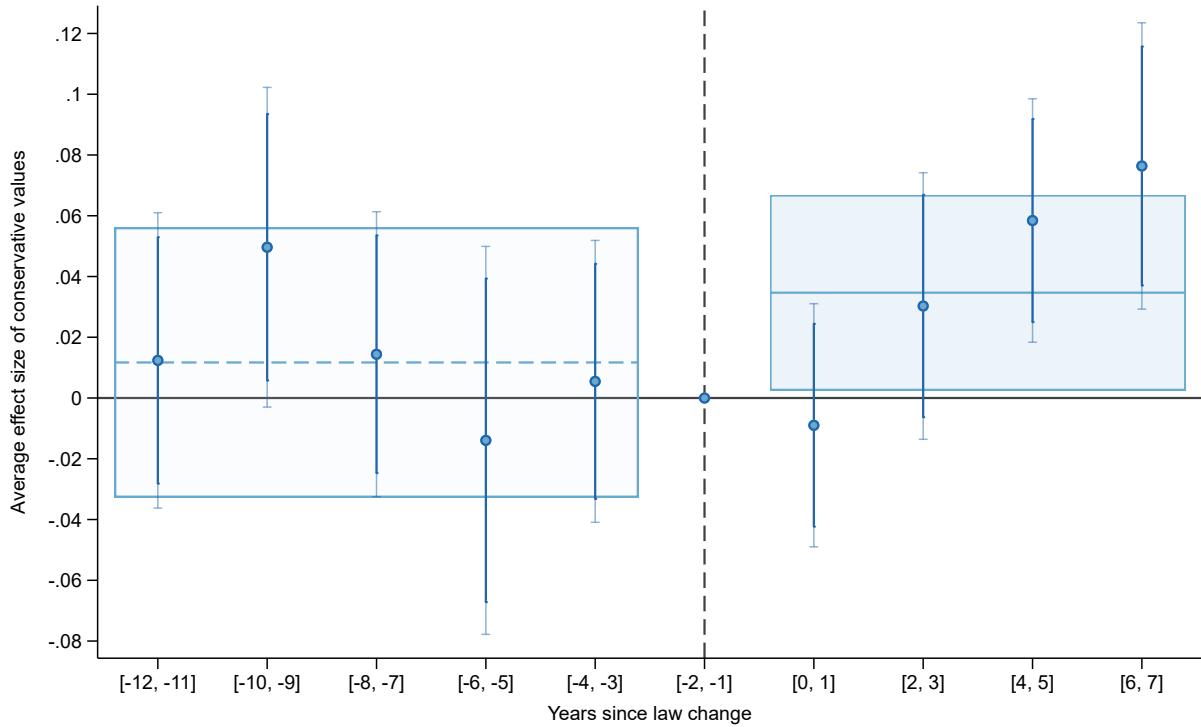
Figure A27: The impact of the faith-based initiatives on conservative-religious social views: DD estimates

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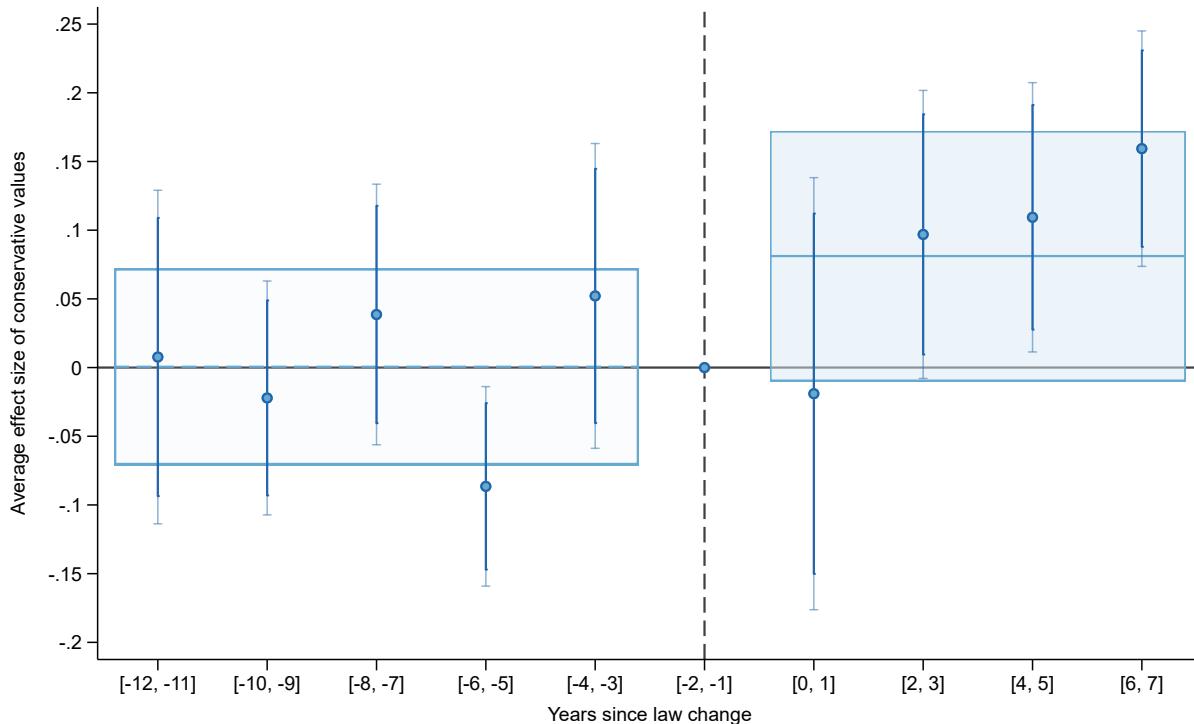
Figure A28: Average effect size of conservative-religious social views: DD estimates

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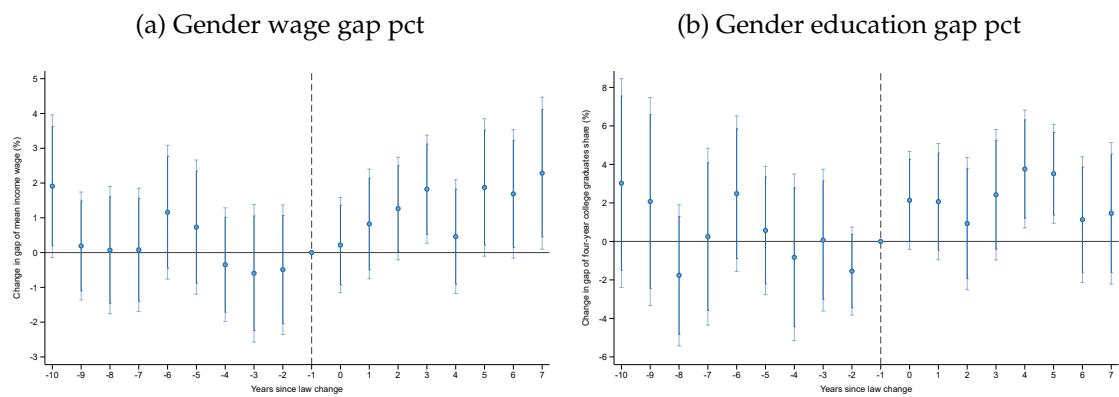
Figure A29: Average effect size of conservative-religious social views: DDD estimates

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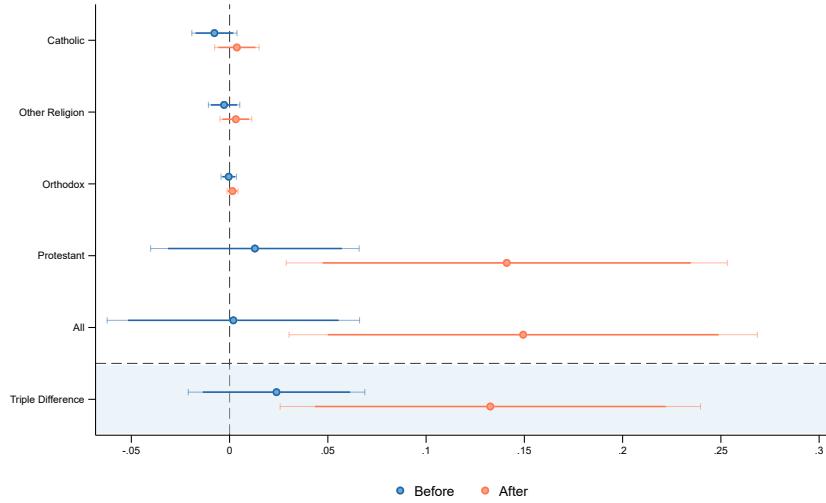
Figure A30: The impact of the faith-based initiatives on gender gaps: state-level outcomes expressed in percentage

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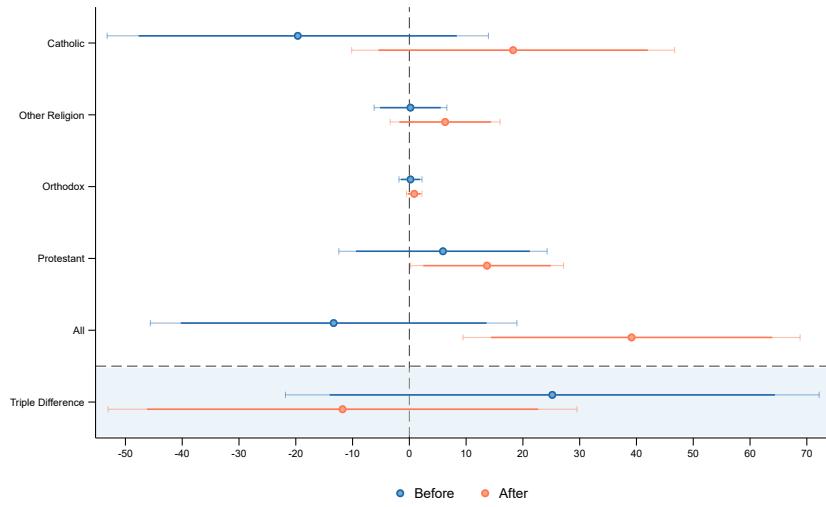
Notes: This figure replicates panels (b) and (c) of Figure 11 using the same sample at state year level. However, in this case we express gender gap outcomes as percentage change of male vs. female in a given state on a given year, *i.e..*, $wage_{male} - wage_{female}/wage_{male}$, for instance.

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)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\,(r = Protestant)_{isct}$		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):

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

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

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

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 (0.015)	0.044** (0.020)	-0.019 (0.013)	-0.015 (0.010)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Protestant\right)_{isct}$	-0.085*** (0.019)	-0.038** (0.018)	0.057*** (0.016)	0.067*** (0.017)
Composite Effect for Protestants	-0.095*** (0.016)	0.006 (0.017)	0.038*** (0.011)	0.051*** (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 (0.011)	0.034** (0.016)	-0.027** (0.012)	0.005 (0.013)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Protestant\right)_{isct}$	-0.059*** (0.017)	-0.030** (0.014)	0.045*** (0.016)	0.036** (0.016)
Composite Effect for Protestants	-0.072*** 0.013	0.004 0.011	0.018*** 0.009	0.041*** 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)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\,(r = Protestant)_{isct}$	-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	Republican		Income		Education		Foreign Born	
Subgroup of interest	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)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Republican\right)_{isct}$	0.001	-0.012						
	(0.011)	(0.016)						
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Income \geq 25^{th}\right)_{isct}$			0.041**	0.026				
			(0.016)	(0.025)				
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Education \geq 25^{th}\right)_{isct}$					-0.012	0.004		
					(0.012)	(0.023)		
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Foreign\right)_{isct}$							-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 independently 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)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Protestant\right)_{isct} \times \mathbf{1}\left(\#Laws \leq 50^{th}\right)_{sc}$	0.079*** (0.020)		
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Protestant\right)_{isct} \times \mathbf{1}\left(\#Laws > 50^{th}\right)_{sc}$	0.089*** (0.023)		
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Protestant\right)_{isct} \times \mathbf{1}\left(\#Laws \leq 10^{th}\right)_{sc}$		0.091** (0.040)	
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Protestant\right)_{isct} \times \mathbf{1}\left(10^{th} < \#Laws \leq 25^{th}\right)_{sc}$		0.087*** (0.021)	
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Protestant\right)_{isct} \times \mathbf{1}\left(25^{th} < \#Laws \leq 50^{th}\right)_{sc}$		0.040 (0.034)	
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Protestant\right)_{isct} \times \mathbf{1}\left(50^{th} < \#Laws \leq 75^{th}\right)_{sc}$		0.126*** (0.030)	
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Protestant\right)_{isct} \times \mathbf{1}\left(75^{th} < \#Laws \leq 90^{th}\right)_{sc}$		0.034 (0.021)	
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Protestant\right)_{isct} \times \mathbf{1}\left(\#Laws > 90^{th}\right)_{sc}$		0.119*** (0.021)	
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Protestant\right)_{isct} \times \mathbf{1}\left(\#Laws = 1\right)_{sc}$			0.048 (0.039)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Protestant\right)_{isct} \times \mathbf{1}\left(\#Laws = 2\right)_{sc}$			0.164*** (0.028)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Protestant\right)_{isct} \times \mathbf{1}\left(\#Laws = 4\right)_{sc}$			0.157*** (0.028)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Protestant\right)_{isct} \times \mathbf{1}\left(\#Laws = 5\right)_{sc}$			0.072*** (0.021)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Protestant\right)_{isct} \times \mathbf{1}\left(\#Laws = 6\right)_{sc}$			0.034 (0.031)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Protestant\right)_{isct} \times \mathbf{1}\left(\#Laws = 8\right)_{sc}$			0.154*** (0.041)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Protestant\right)_{isct} \times \mathbf{1}\left(\#Laws = 9\right)_{sc}$			0.112*** (0.033)
$FBI_{sc} \times Post_{ct} \times \mathbf{1}\left(r = Protestant\right)_{isct} \times \mathbf{1}\left(\#Laws = 10\right)_{sc}$			0.011 (0.018)
$\mathbf{1}\left(r = Protestant\right)_{isct} \times \mathbf{1}\left(\#Laws > 10\right)_{sc}$			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

Initiative:	Liaison's Activities								Contacts with WH		FBL Position	
	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

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

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:

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

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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