Churches and Local Economies

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Abstract

How do local churches shape economic and social life? We study this question in the Southern Baptist Convention (SBC), the nation's largest Protestant denomination, exploiting two shocks to congregational life: pastor deaths and pastor convictions for sexual abuse. Using matched-controls event study designs, we show that pastor deaths lead to church closures, while convictions generate large and persistent reductions in Baptist attendance and engagement. Both shocks reduce labor-force participation, raise unemployment, and lower nearby housing prices for several years. The shocks diverge on outcomes plausibly affected by religious beliefs rather than networks: convictions shift two-party Democratic vote share leftward and increase opioid mortality, while deaths produce no detectable changes in either. This pattern supports a framework in which deaths primarily disrupt access to congregational networks, while convictions additionally erode religious norms that shape political and health behaviors.

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1 Introduction

Religious congregations sit at the center of community life for tens of millions of Americans. In addition to providing opportunities for conventional worship, they knit together social networks that transmit information, referrals, and support and reinforce community norms and beliefs that shape economic, health, and civic behavior (Iyer, 2016). Religious beliefs have long been argued to emphasize work ethic, trust, and altruism in ways that shape economic behavior and prosperity (Weber, 1930). At the same time, religious observance is among the strongest correlates of broader social engagement. Regular church attendees interact with 40 percent more people throughout the day than the religiously unaffiliated, are more politically engaged, and are more likely to be involved in a variety of other secular organizations (Putnam, 2000). Understanding the role of religion in contemporary American life is important, particularly considering a recent nationwide shift away from religion. Church membership has declined steadily in the United States over recent decades, and dropped below a majority in 2020 for the first time in Gallup's eight decades of tracking (Jones, 2021). Yet, because religious participation is endogenous to the individual and the community characteristics it may affect, credible causal evidence on what is lost when congregations weaken remains limited.[1]

In this paper, we provide new causal evidence on how churches shape local economies, and we provide evidence consistent with two channels through which they operate: access to congregational networks and the force of religious belief. We study the Southern Baptist Convention (SBC), the largest Protestant denomination and second-largest Christian body in the United States, exploiting two complementary shocks to congregational life: pastor deaths and pastor convictions for sexual abuse. Pastor deaths precipitate church closures, reducing local access to the congregation and its network. Pastor convictions for sexual abuse cause sharp, lasting declines in attendance and decrease church affiliation. Because the SBC is highly decentralized, where each congregation is autonomous and there is no central body to reassign pastors or manage local crises, both of these shocks are absorbed locally rather than smoothed by denominational intervention.[2] Using newly digitized directories of the

[1] For example, Robert Putnam writes in *Bowling Alone*, "These studies cannot show conclusively that churchgoing itself 'produces' social connectivity—probably the causal arrow between the two points in both directions—but it is clear that religious people are unusually active social capitalists" (Putnam, 2000).

[2] On the SBC's Frequently Asked Questions webpage, in response to the question "I believe our pastor (or my church) has acted inappropriately. What can the SBC do about it?" the church says this: "Since each

universe of SBC pastors from 1970 to 2008, conviction records from investigative reporting, and matched-controls event study designs, we trace the effects of both shocks on religious participation, labor markets, voting, health, and housing markets.

We show that pastor deaths lead to church closures at a rate of roughly one closure per three deaths (nearly one-for-one when the pastor was active at the time of their death), while convictions cause immediate declines in Baptist weekly church attendance of nearly 20 percentage points that persist for the duration of our sample. Both shocks reduce labor force attachment: labor force participation falls by 0.29 percentage points following deaths and 0.70 percentage points following convictions, while unemployment rises by 0.19 and 0.17 percentage points, respectively, with effects persisting at least five years. The two shocks have sharply different effects on outcomes that are plausibly sensitive to beliefs rather than networks. Convictions shift two-party Democratic vote shares upward in presidential elections by approximately 0.7 percentage points and increase opioid mortality by 1.7 deaths per 100,000—a 30% increase over the baseline mean—while pastor deaths produce no detectable changes in either voting or mortality. Both shocks reduce nearby home prices, with convictions generating declines of roughly 15 percent within one mile of the implicated church. A hedonic model, exploiting changes in the distance from the nearest SBC church using variation from pastor deaths in a nonparametric instrumental variables (NPIV) framework, implies an average willingness to pay of $3,235 to live one mile closer to an SBC church.

This pattern, where both shocks generate similar labor market deterioration, but only convictions evoke voting and health responses, is consistent with a framework in which pastor deaths primarily disrupt the church access channel while convictions additionally operate through a belief and norms channel. This is consistent with Barro and McCleary's argument that religious belief itself is the core driver of religion's effect on economic outcomes (2003; 2006). We formalize these mechanisms in a framework, related to models of religion as a norm-enforcement institution as in Carvalho (2012), where church attendance provides job-referral networks and religious belief sustains norms about labor force participation and health behaviors, generating testable predictions that are borne out in the data.

local Baptist church is autonomous, the Convention has no authority to monitor or investigate the actions within that church or allegations against its pastor or any member of the church. The proper governing body to exercise discipline over any Southern Baptist is the congregation of which that Southern Baptist is a member, whether the person is the pastor of the church or any other member of the church. The SBC is not a church and has no authority to renounce, censure, investigate, or otherwise attempt to discipline members of any local church" (SBC, 2025).

Our paper contributes to a long tradition, from Adam Smith's writing on religious competition to Max Weber's thesis on the economic salience of religious belief and beyond, arguing that religious life shapes behaviors and beliefs relevant to individual and communal prosperity (Smith, 1776; Weber, 1930).[3] Macro-level analyses have linked religious belief to pro-market attitudes and growth (Guiso et al., 2003; McCleary and Barro, 2006), though historical work on the Protestant Reformation and Jesuit missions in South America find that economic effects are often tied to human capital rather than belief per se, or find no effects at all (Becker and Woessmann, 2009; Valencia Caicedo, 2019; Cantoni, 2015).

More recently, a growing quasi-experimental literature has studied the causal effects of religion directly by isolating specific mechanisms. Research has shown that secular policy changes can shift religious participation and charitable giving (Gruber and Hungerman, 2008), and that shocks to religious practice, such as pilgrimage or fasting intensity, affect beliefs and well-being but decrease short-term output growth (Clingingsmith et al., 2009; Campante and Yanagizawa-Drott, 2015). Other work highlights religion's role as an informal safety net: religious participation provides crucial social smoothing during economic crises (Chen, 2010), a function further supported by evidence that formal insurance provision crowds out religious offerings (Auriol et al., 2020). Additionally, religious scandals have been shown to reduce attendance and giving, prompting localized religious substitution (Hungerman, 2013; Bottan and Perez-Truglia, 2015).

Despite these insights, identifying religion's causal effects on core economic aggregates has proven elusive. Randomized evangelical outreach in the Philippines increased religious practice and income but had no significant effect on consumption or labor supply (Bryan et al., 2021), while historical shifts in secular competition primarily induced a reallocation of time rather than employment effects (Gruber and Hungerman, 2008). A complementary literature within health economics links religious decline to rising "deaths of despair" (Case and Deaton, 2021); Giles et al. (2023) show that attendance declines among middle-aged Whites precede rising mortality in the 1990s, and use state-level policy changes to show that religious participation causally affects these mortality rates. We contribute by leveraging highly local variation within a major decentralized denomination to measure effects on outcomes previously less studied in causal work:labor force participation, unemployment, and home prices. We use our two-shock design to distinguish shocks that primarily reduce

[3]For comprehensive overviews of this literature, see Iannaccone (1998) and Iyer (2016).

access to congregational networks (the 'belonging' channel) from shocks that additionally weaken religious legitimacy and norms (the 'believing' channel), providing new traction on the distinct roles of religious social networks and religious belief systems.

Our findings regarding church access connect to network-based models of job search and information diffusion in which repeated contact and weak-tie referrals facilitate employment (Granovetter, 1973; Topa, 2001; Calvo-Armengol and Jackson, 2004) and operate through screening and recommendation margins (Pallais and Sands, 2016), and also connect to recent evidence linking cross-class social connectedness to economic mobility and labor-market attachment (Chetty et al., 2022a,b, 2024). Congregations are natural venues for this sort of network formation, and our evidence that pastor deaths, which lead to church closures without obviously shifting beliefs, produce sustained labor market deterioration is consistent with the erosion of these ties. In treating church proximity as a local amenity that capitalizes into housing prices, we also contribute to the hedonic tradition (Rosen, 1974; Roback, 1982) and to quasi-experimental work estimating place-based amenity values.[4] Our NPIV approach recovers willingness-to-pay for church proximity and shows that reputational costs from convictions are localized disamenities.

The remainder of the paper proceeds as follows. Section 2 describes our setting and provides a conceptual model to contextualize results, Section 3 describes the data used in this project, and Section 4 describes the empirical strategies. In Section 5, we discuss our main results. In Section 6, we show spatial heterogeneity in effects on housing markets, which we formalize in a hedonic model to estimate preferences for church access and credibility. Section 7 concludes.

2 Setting and Conceptual Framework

2.1 The Southern Baptist Convention

The SBC is the largest Protestant denomination in the United States. It formed in 1845 after separating from the national Baptist body (the "Triennial Convention") amid disputes that included the legitimacy of slaveholding missionaries. Over the twentieth century the SBC expanded nationwide: as shown in Panel A of Appendix Figure A1, membership peaked at

[4]See Chay and Greenstone, 2005; Greenstone and Gallagher, 2008; Linden and Rockoff, 2008; Diamond and McQuade, 2019; Bartik et al., 2019; Kashner and Ross, 2025.

roughly 16 million in 2006 and has fallen each year since, with 12.7 million members and about 47,000 congregations reported in 2024 (Earls, 2025). This decline is consistent with a broader decline in religiosity in the United States, as presented in Panel B of Figure A1. From 2008 to 2023, the fraction of individuals who report attending church at least monthly dropped from over 60 percent to around 45 percent, while the number *never* attending church rose from 4 percent to 14 percent (Jones, 2021).

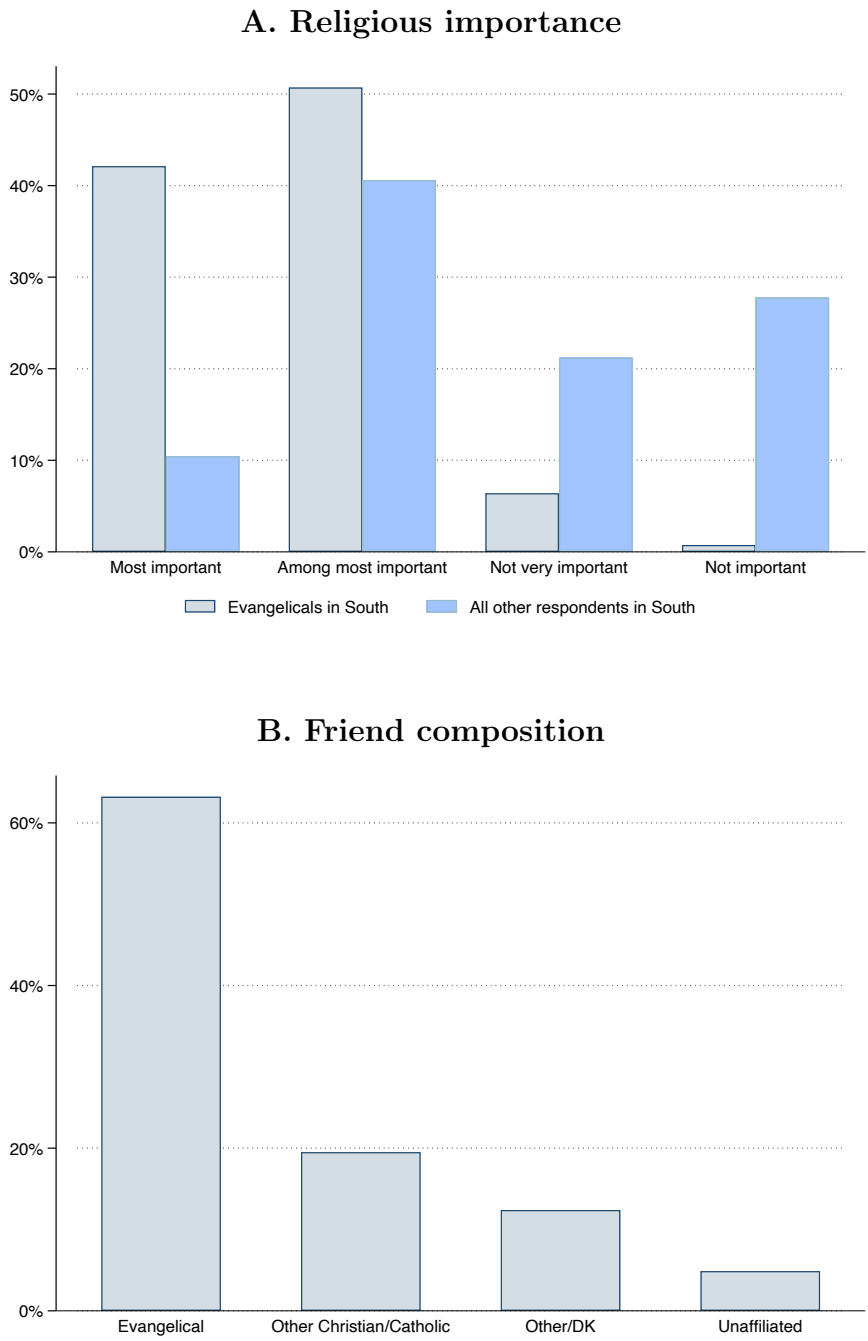
Doctrinally, the SBC's touchstone is the Baptist Faith and Message (SBC, 2000). It affirms believer's baptism, biblical inerrancy, and the primacy of the local church as the locus of Christian community and discipline. In the SBC, there are two scriptural offices: pastor and deacon. While both men and women are allowed to serve a local congregation as deacons, the office of pastor is limited to men "as qualified by scripture" (SBC, 2000). Churches commonly "call" a pastor by congregational vote, elect deacons, and conduct business meetings where members approve budgets, ministries, and major decisions. Worship practices are decided at the church level and are highly heterogeneous, with small churches most likely to remain traditional (Sellers, 2004).

The SBC is congregationally governed. Each congregation is an autonomous nonprofit that owns its property (generally via corporate title or trustees), hires and dismisses its pastors, and decides on doctrine and practice within the broad bounds of Baptist confessional statements. There is no centralized hierarchy within the SBC; it is up to the individual congregation to appoint clergy, allocate local assets, and discipline church members. Southern Baptists are an evangelical Protestant tradition in which congregational life is organized around pastoral preaching and leadership, making the individual pastor especially central. As shown in Appendix Figure A2, many churches include their pastor's name in church advertisements. The pastor's primacy within the decentralized network of SBC churches has two main implications for our study: first, many congregations are single-pastor churches, so a leadership shock can create immediate capacity constraints. Second, congregations are often imperfect substitutes even within short distances: members build relationships, routines, and worship styles around a particular pastor and church community.

While only 4% of U.S. adults are Southern Baptists, they are very spatially concentrated (Pew, 2025). Consistent with the name, over 78% of all Southern Baptist congregants live in the South (Pew, 2025). In Appendix Figure A3 we present a map of the percentage of the county population who are Southern Baptists. In many counties, particularly in Texas,

Louisiana, Mississippi, Alabama, and Kentucky, more than half of the population belong to the SBC. Surveys indicate that for Southern Baptist adherents, religion is highly salient and very socially embedded. Panel A of Figure 1 shows that among evangelicals living in the Southern U.S. (a population that overlaps substantially with SBC congregants), over 40% report that religion is the single most important part of life, and more than 90% place it among the most important parts of life. For comparison, the remaining Southern population reports markedly lower importance on both margins. Panel B of Figure 1 documents the composition of friends for Southern Evangelicals, which are highly segregated. Over 60% of their friends are also evangelical, with the remainder split across other Christians/Catholics ($\sim$ 20%), those of other/unknown faiths ($\sim$ 15%), and the religiously unaffiliated ($\sim$ 5%). These patterns confirm the centrality of religion in everyday life for our target population and the substantial in-group concentration of their immediate social networks.

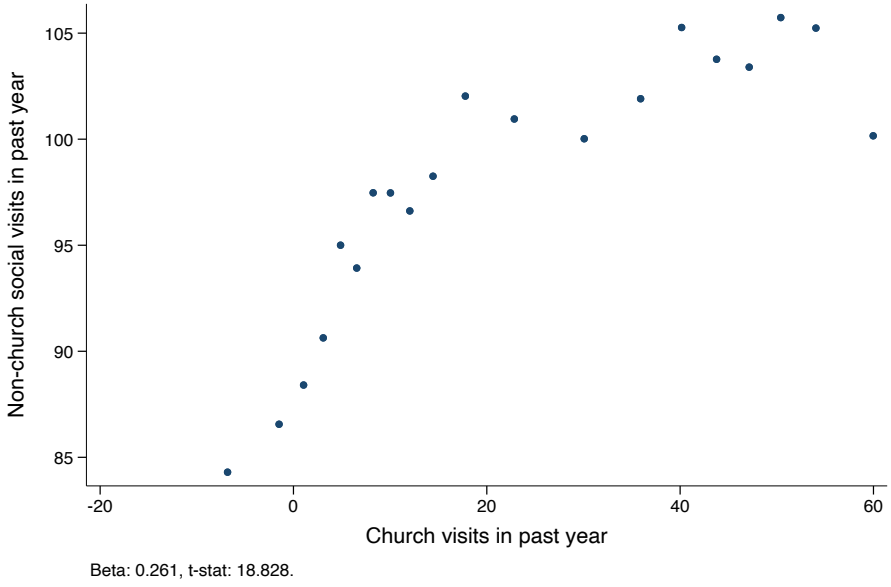
Figure 1: Survey reports of role of religion in life



Notes: Data come from the 2022 Public Religion Research Institute (PRRI) Social Networks Survey and use PRRI survey weights. Panel A plots the share of respondents in the U.S. South reporting that religion is (i) "most important", (ii) "among the most important", (iii) "not very important" or (iv) "not important" in their lives. "Evangelicals in South" are respondents living in the South who identify as evangelical Protestant. "All other respondents in South" are all other Southern respondents. Panel B plots the average religious composition of close friends for evangelical respondents only. Respondents list up to seven friends. Each friend is categorized as Evangelical, Other Christian/Catholic, Unaffiliated, or Other/Don't know based on the friend's reported religion, and the bars report the mean share of listed friends in each category.

We next relate religious participation to overall non-church social activity using repeated cross-sectional data from the DDB Life Style Survey (1975-1998). Figure 2 shows a binned scatterplot of the count of non-church social visits in the past year (e.g. meals at friends' houses, barbecues, bowling, playing sports with friends or neighbors) alongside reported church visits in the prior year. The estimated slope is 0.261 (t=18.8) after conditioning on county, state-by-year, and demographic-by-year (age, gender, education, race) fixed effects. While purely descriptive, the strong positive gradient is consistent with the idea that church-going co-moves with broader social engagement rather than crowding it out. In the context of our later empirical designs, this provides a tangible baseline for the role of churches in creating and sustaining social networks and activity. Since congregations are hubs where repeated contacts, information, and support circulate, then shocks to local access to congregational life may spill over to non-church social activity and networks (especially when networks are religiously segregated as those among Southern Evangelicals) and to outcomes that depend on network density, like labor-market attachment (Granovetter, 1973).

Figure 2: Church attendance and social activity



Notes: Data come from the DDB Life Style Survey and use survey weights. The figure is a binned scatterplot of non-church social visits in the past year against church visits in the past year, after residualizing both variables with county fixed effects, state-by-year fixed effects, and demographic-by-year fixed effects (race, age group, sex, and city size). The plotted series adds back the overall means after residualization so axes remain in original units. The reported coefficient and t-statistic in the figure are from the corresponding weighted regression using the same fixed-effects specification.

These institutional features imply that pastor-level disruptions can plausibly generate meaningful local shocks. Here, we study two distinct events. First, pastor deaths can reduce access to congregational life by creating immediate leadership and capacity constraints in single-pastor churches, potentially lowering participation and weakening church-based referral networks. Second, pastor convictions plausibly erode the SBC's perceived credibility and moral authority, which may reduce participation and also reduce the salience of church norms, both for those who leave the church and remaining attendees. The next subsection formalizes these distinctions and generates testable predictions for the effects of these shocks on labor-market outcomes and behavior.

2.2 Conceptual Model

To organize interpretation and guide empirical tests, we develop a simple framework to clarify how churches may shape labor market outcomes and to contrast the effects of two types of shocks: those that limit access, such as pastor deaths, and those that erode belief, such as pastor convictions. In the model, religion enters through three channels: it provides social capital, it functions as a local amenity, and it establishes community norms. This structure generates clear, testable predictions for the distinct impacts of each shock.

In the model, individuals have some exogenous intensity of church attendance $a \in [0,1]$. They choose whether to participate in the labor force, $l \in \{0,1\}$, and a level of behavioral alignment with church values, $y \in [0,1]$ (e.g., voting Republican, abstaining from drugs). Church attendance affects labor market outcomes through its effect on the job-finding rate in a matching model:

$$m(a) = \underbrace{m_0}_{\substack{\text{baseline} \\ \text{match rate}}} + \underbrace{\beta a}_{\substack{\text{church} \\ \text{referrals}}}. \tag{1}$$

where m_0 is the baseline match rate and βa captures referrals through church networks. In steady state, the unemployment rate is $\mu(a) = \frac{\sigma}{\sigma + m_0 + \beta a}$, with σ denoting an exogenous

job-separation rate. Individuals receive per-period utility from their choices of y and l:

$$U_i(y,l) = \begin{cases} \underbrace{[(1-\mu(a))\,w + \mu(a)\,b] + \alpha a}_{\substack{\text{attendance} \\ \text{channel}}} + \underbrace{\lambda y - \frac{1}{2}\phi y^2}_{\substack{\text{norms} \\ \text{channel}}} & \text{if } l = 1, \\ \underbrace{R_i + \alpha a}_{\substack{\text{attendance} \\ \text{channel}}} + \underbrace{\lambda y - \frac{1}{2}\phi\,(y+\rho)^2}_{\substack{\text{norms} \\ \text{channel}}} & \text{if } l = 0, \end{cases} \tag{2}$$

where $w > b$ is the wage if employed and b is the benefit if unemployed. The reservation value of not participating in the labor force is given by $R_i \sim F$. Church participation provides a direct social amenity $\alpha > 0$. The behavioral choice y generates utility directly with marginal value $\lambda > 0$, and provides a cost $\phi \in [0,1]$, of deviation from church values, which may stem from religious weight on moral rewards or penalties which decreases when belief falls. $\rho > 0$ represents the additional deviation from church values from not participating in the labor force, consistent with a Weberian work-ethic channel (Weber, 1930). Individuals choose to participate in the labor force when $l = \mathbb{I}\left\{\max_y U_i(y,1) \geq \max_y U_i(y,0)\right\}$.

The model generates a number of simple stylized predictions. First, since $\frac{\partial \mu}{\partial a} < 0$, a reduction in church attendance should raise the unemployment rate. The second is that since $\frac{\partial l}{\partial a} > 0$, reduction in church attendance lowers the labor force participation rate, consistent with the interpretation of churches as facilitators of social capital and job referrals. Since $\frac{\partial U}{\partial a} > 0$, in a spatial equilibrium where marginal utilities are equalized across space, this channel implies that equilibrium home prices P should capitalize the amenity value of church attendance (Rosen, 1974; Roback, 1982).

Third, changes in belief, parameterized by ϕ, affect outcomes through a norms channel.[5] Equation 2 implies an optimal deviation $y^* = \frac{\lambda}{\phi} - \rho\,(1-l)$. Because $\frac{\partial y^*}{\partial \phi} < 0$, weaker belief (lower ϕ) induces greater deviations from church norms. Further, $\frac{\partial U_i(1)}{\partial \phi} - \frac{\partial U_i(0)}{\partial \phi} = \frac{\lambda \rho}{\phi^2} > 0$, so stronger belief raises labor force attachment and $\frac{\partial l}{\partial \phi} > 0$.

Taken together, the stylized model delivers a set of testable predictions. First, shocks that reduce attendance (e.g. pastor deaths) should increase unemployment rates and lower labor force participation. Second, shocks that weaken belief (e.g. pastor convictions) operate through both the attendance and norms channels, producing similar unemployment effects

[5]Here, we parameterize religious belief as the costs of deviating from church values: the greater the religious belief, the greater the costs for making choices y not in accordance with church values.

but a larger decline in participation, alongside greater behavioral deviations from church norms. Finally, capitalization into housing markets is unambiguous for attendance shocks, but for belief shocks the effect depends on the relative strength of the amenity value α, the material return to the (potentially non-conforming) action y, and the salience of the penalty parameter ϕ, leading to theoretically ambiguous price responses.

3 Data

We assemble a multi-source dataset that connects shocks to Southern Baptist congregations with proximate religious measures and downstream economic, political, health, and housing outcomes. Broadly, our data fall into four categories: (i) pastor deaths, (ii) pastor convictions, (iii) proximate religious outcomes (church counts and adherence), and (iv) other outcomes (labor markets, politics, health, housing). Below we describe each in turn, then outline how we link sources across space and time and construct our analysis samples.

3.1 Pastor death data

Data on the location of Southern Baptist churches and pastors comes from a novel dataset we construct from the *Annuals* of the Southern Baptist Convention, obtained from the Southern Baptist Historical Library. We construct a new panel of the universe of Southern Baptist churches in the United States by digitizing these directories, which record the name of each pastor and their church's location at the zip code level, for the years 1950, 1954, 1958, 1962, 1966, 1970, 1974, 1980, 1982, 1984, 1986, 1990, 1994, 1998, 2000, and 2008. We combine these data with two additional data sources to construct our measure of pastor deaths. First, we use a quasi-panel of individual addresses from Infutor, as in Diamond et al. (2019), Mast (2023), Asquith et al. (2023), and others. The dataset links individuals to their full name, gender, age, address, and dates of residence. This allows for measurement of extremely local residency decisions. We combine the Infutor data with the Social Security Death Master File as in Bernstein et al. (2022), which contains more than 85 million death records reported to Social Security from 1936 until 2013.[6] Together, these data allow us to determine an individual's full name, gender, age, and location of death. These data are then merged to our panel of SBC pastors and locations to construct a measure of SBC pastor deaths.

[6]We access a public use copy of Social Security Death Master File from SSDMF.INFO.

3.2 Pastor convictions data

Data on Southern Baptist pastors' convictions for sexual abuse come from the *Houston Chronicle's* "Abuse of Faith" database, which contains convictions of pastors, employees, and volunteers of sexual abuse, as well as the county and year in which the offender was convicted. We consider only cases of sexual abuse among Southern Baptist pastors, leaving our sample with 125 unique conviction events. We additionally record the church the convicted pastor was working at most recently prior to their conviction, and manually geocode the locations of these churches. When multiple convictions occur in the same county, we define the treatment time to be the earliest known conviction year.

3.3 Proximate outcomes data

Southern Baptist church presence. From the digitized SBC Annuals described in 3.1, we construct a panel of open SBC congregations by year and location at the zip code, as well as the county, level.

Religious adherence (all denominations). Our data on religious observance comes from two main sources. First, we measure county-level religious adherence and congregations from the U.S. Religious Census (formerly the Census of Religion) for 1952, 1971, 1980, 1990, 2000, and 2010. For each wave, we observe counts of adherents and congregations by denomination. We use these to construct (i) the Baptist adherence share, (ii) the non-affiliation share (where available), and (iii) total congregations per capita. Additionally, we track religious attendance over time by denomination using the Cooperative Election Study (CES) Cumulative Common Content dataset, available from 2006 through 2024. This survey dataset contains detailed information about religious behavior and belief from over 700,000 respondents, and includes granular demographic (such as age, race, gender, income, and religious affiliation) and geographic (county and zip code identifiers) information. These data are used to observe changes in religious attendance among Protestants following pastor conviction events.

Social connectedness. We observe responses to the DDB Life Style survey, collected yearly by the advertising agency DDB Needham from 1975-1998.[7] Each year has $\sim 3,000$

[7]This data underlies much of Robert Putnam's book *Bowling Alone*.

respondents' answers to 237 questions, which includes information on social connectedness and religious adherence. We use these surveys to construct indices of non-church social activity and civic participation for exploratory analyses of possible mechanisms. We supplement this data with the 2022 Social Networks Survey compiled by the Public Religion Research Institute (PRRI), which tracks relationships between religious affiliation, religious practice intensity, social networks, and social behaviors. This survey data is used to estimate cross-sectional relationships between religious belief (specifically affiliation with Southern Evangelical churches) and general social connectedness and social behavior.

3.4 Other outcomes data

Labor markets. County-level labor force participation and unemployment rates are from the U.S. Bureau of Labor Statistics Local Area Unemployment Statistics (LAUS), 1990–2023.

Wages and jobs. From the Census' Quarterly Workforce Indicators time series, we observe data about wages and jobs at the county level by quarter. We measure wages by gender by education level, as well as change in employment.

Health. Age-adjusted mortality rates are from the National Center for Health Statistics (NCHS) Multiple Cause of Death files. We construct age-adjusted county-level overall mortality and opioid-involved mortality using standard ICD-based definitions (harmonized across ICD-9/ICD-10). We report rates per 100,000.

Politics. We use presidential and Senate voting outcomes from 1964-2022 from ICPSR's General Election Data for the United States and Dave Leip's Atlas of U.S. Elections. The ICPSR data contain election returns from 1964-1990, while the Atlas of U.S. Elections data contain election returns from 1992-2022.

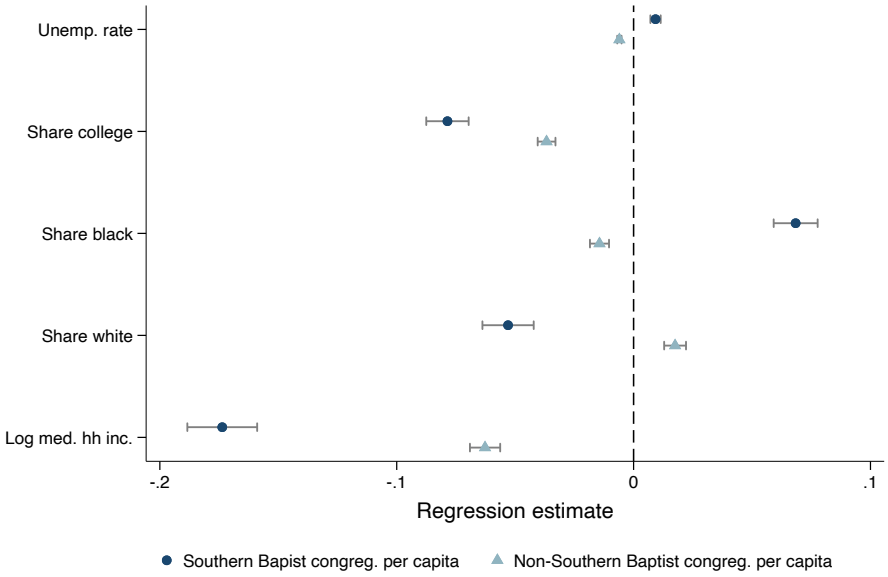
Housing. We record data on home transactions from CoreLogic Deeds, which capture the universe of property deed transfers from before 1900 through 2019. For each transaction we observe sale date and price and, for most properties, acreage, building square footage, and counts of bedrooms and bathrooms. We assign properties precise distances from the nearest SBC church over time.

4 Empirical strategy

Our empirical strategy centers around event study designs that leverage the sharp timing of pastor deaths and pastor convictions. We estimate dynamic treatment effects by comparing the evolution of outcomes in treated locations to outcomes in untreated locations in event time, testing for pre-trends and tracing out post-event dynamic effects.

Identification in this event study setting requires that, absent the event, treated locations would have followed the same outcome trend as untreated locations over the event window. The credibility of this assumption depends critically on the choice of control units that enter our specifications. Appendix Figure A3 shows that SBC presence is highly geographically concentrated, and Figure 3 shows that locations with more Southern Baptist churches tend to be negatively selected on a variety of measures of economic opportunity. We find that a higher level of Southern Baptist congregations per capita is associated with higher rates of unemployment, lower household income, as well as lower rates of college education, higher Black population shares, and lower White shares. These results hold both when comparing high-SBC counties to the average county in the United States and when comparing to counties with substantial presence of other religions: across all of these margins, the relationship between SBC presence and economic disadvantage is substantially larger than the relationship between presence of other churches and economic disadvantage. If treated locations differ systematically in levels and trends from a national control pool, then a standard event study using all untreated areas as controls may attribute these underlying differences to the pastor shock.

Figure 3: Correlations between area characteristics and Southern Baptist churches



Notes: Data on number of congregations from the 2010 Census of Religions. Data on county characteristics comes from the 2010 Decennial Census. The figure plots coefficients from separate county-level OLS regressions of each outcome listed on the vertical axis on congregations per capita, run one at a time. Circles report estimates for Southern Baptist congregations per capita; triangles report coefficients for non-Southern Baptist congregations per capita. Unadjusted 95% confidence intervals are shown in gray bands surrounding each coefficient.

To improve the suitability of our counterfactual trends, we pair each treated location with a small set of observably similar untreated locations and estimate each event study within these matched sets. For each treated unit we restrict the pool of potential controls to never-treated units within the same state as the treated unit and select as controls the five nearest never-treated units in standardized distance over a vector of pre-event characteristics capturing population and density, demographic composition, baseline SBC shares and overall religious environment, and baseline political preferences. For each treated unit, we measure our pre-treatment covariates used for matching as those measured in the *most recent* pre-treatment year in our data. For events such as pastor deaths, which have substantial variation across places in when they are first measured, matching on the most-recent available pre-treatment data avoids issues from selecting a single pre-treatment baseline year which may have a substantial time lag from when treatment occurs for some counties. The full list of matching variables is shown in Table 1.

Table 1 summarizes pre-period characteristics for treated units, their matched controls, and the full pool of untreated units. For each treated county, we compute the mean of each covariate among (i) its five matched controls and (ii) the national pool of all untreated counties, and then average these treated-specific control means across treated counties. The absolute standardized mean difference (SMD) between treated counties and the relevant control means are displayed in parentheses. The table highlights two patterns. First, treated counties are substantially different from the national untreated pool along both religious composition and socioeconomic characteristics. For example, treated counties have much higher SBC member shares than the national control pool in both designs (Panel A: 0.205 vs 0.136; Panel B: 0.241 vs 0.048), and the associated SMDs relative to the national pool are large for many variables, indicating pronounced imbalance. Second, restricting attention to within-state matched controls substantially improves comparability: matched controls closely track treated counties on SBC exposure and are markedly closer on population density, racial composition, education, and baseline political preferences. While some differences remain, particularly in the pastor deaths design where treated counties are more SBC-intensive than our matched controls, the matched sample substantially reduces imbalance relative to the national pool across nearly all covariates, supporting our use of matched controls as the comparison group in the event-study specifications below.

4.1 Event study specifications

Our empirical strategy exploits the timing across areas of pastor deaths and pastor convictions to study their effects using stacked event studies. We estimate dynamic treatment effects by comparing outcome paths in treated locations to those in observably similar never-treated locations within the same state, using the matching procedure described above. For each matched set g, we define the event year T_g. For area-level outcomes (zip codes for church closures, and counties for all other outcomes), we estimate the following event study specification:

$$Y_{u,g,t} = \delta_{u,g} + \sum_{k \neq -1} \beta_k (D_{u.g} \times \mathbf{1}\{t - T_g = k\}) + \lambda \mathbf{X}_{u,t} + \theta_{s(u),t} + \varepsilon_{u,g,t}, \tag{3}$$

where u indexes the unit, $D_{u,g}$ indicates the treated unit within the matched set g, $\delta_{u,g}$

Table 1: Pre-period county characteristics by sample and identification strategy

	Treatment	Matched controls (—SMD—)	All controls (—SMD—)
Panel A: Pastor convictions analysis			
SBC member share	0.205	0.206 (0.005)	0.136 (0.612)
SBC congregations (per 1,000)	0.446	0.473 (0.063)	0.475 (0.094)
Non-SBC member share	0.141	0.141 (0.004)	0.199 (1.227)
Non-SBC congregations (per 1,000)	1.444	1.585 (0.158)	2.625 (1.734)
Share college	0.301	0.269 (0.189)	0.238 (0.418)
Share white	0.790	0.809 (0.152)	0.844 (0.535)
Share black	0.124	0.112 (0.095)	0.085 (0.439)
Republican pres. share	0.610	0.615 (0.039)	0.601 (0.082)
Log population	11.474	11.118 (0.280)	10.145 (1.309)
Log population density	5.108	4.675 (0.333)	3.721 (1.353)
N treated units	88	–	–
Avg. controls per treated unit	–	5	3013
Panel B: Pastor deaths analysis			
SBC member share	0.241	0.185 (0.380)	0.048 (1.557)
SBC congregations (per 1,000)	0.671	0.590 (0.171)	0.287 (0.959)
Non-SBC member share	0.115	0.136 (0.239)	0.233 (1.820)
Non-SBC congregations (per 1,000)	1.820	2.049 (0.251)	2.793 (1.343)
Share college	0.170	0.153 (0.225)	0.195 (0.332)
Share white	0.821	0.797 (0.136)	0.920 (0.846)
Share black	0.143	0.172 (0.161)	0.040 (0.896)
Republican pres. share	0.522	0.505 (0.145)	0.563 (0.445)
Log population	10.555	9.809 (0.695)	9.770 (0.872)
Log population density	4.106	3.518 (0.510)	3.180 (0.976)
N treated units	1411	–	–
Avg. controls per treated unit	–	5	1726

Notes: Control means are computed at the treated-unit level: for each treated county, we compute the mean covariate among its control pool (matched or national), then average across treated counties. Parentheses report absolute standardized mean differences comparing treated values to treated-specific control means. All covariates are pre-period values. Pre-period data comes from the 1970-2000 Decennial Censuses, the 2009-2019 annual 5-year ACS files, the 1971, 1980, and 1990 Churches and Church Membership in the United States datasets, and the 2000 and 2010 U.S. Religious Congregations and Membership studies. For each treated unit, we define the relevant pre-period covariate value as being the latest pre-period year in the sample, and construct our controls sample using data from that reference year. Our matched controls sample computes controls for the 5 nearest counties within the treated unit's state on these pre-period characteristics.

are unit-by-matched-set fixed effects,[8] and $\theta_{s(u),t}$ are state-by-year fixed effects. We cluster standard errors at the state level. We also report a pooled difference-in-differences analogue that collapses the event time indicators into a single post-period indicator:

$$Y_{u,g,t} = \delta_{u,g} + \beta(D_{u,g} \times \mathbf{1}\{t \geq T_g\}) + \lambda \mathbf{X}_{u,t} + \theta_{s(u),t} + \varepsilon_{u,g,t}. \tag{4}$$

For individual-level outcomes in the CES, treatment is assigned at the county level and individuals inherit their county's event time within the matched-set stack. Here, we estimate the analogous specification

$$Y_{i,g,t} = \delta_{c(i),g} + \sum_{k \neq -1} \beta_k (D_{c(i),g} \times \mathbf{1}\{t - T_g = k\}) + \lambda \mathbf{X}_{i,t} + \theta_{s(i),t} + \varepsilon_{i,g,t}, \tag{5}$$

using CES survey weights and clustering standard errors at the state level. In all specifications, time-varying controls in $\mathbf{X}_{u,t}$ enter by interacting pre-period levels of the control variable with year fixed effects to avoid controlling for potentially endogenous outcomes.

Under the standard parallel trends assumption that treated units and their matched controls would have followed similar trends absent the event, the coefficients β_k trace out the dynamic effects of pastor deaths or convictions on the outcome. Restricting comparisons to within-state matched sets makes this assumption more plausible by focusing on ex-ante observably similar places, and the pre-shock event study coefficients provide a direct check for differential trends.

5 Main results

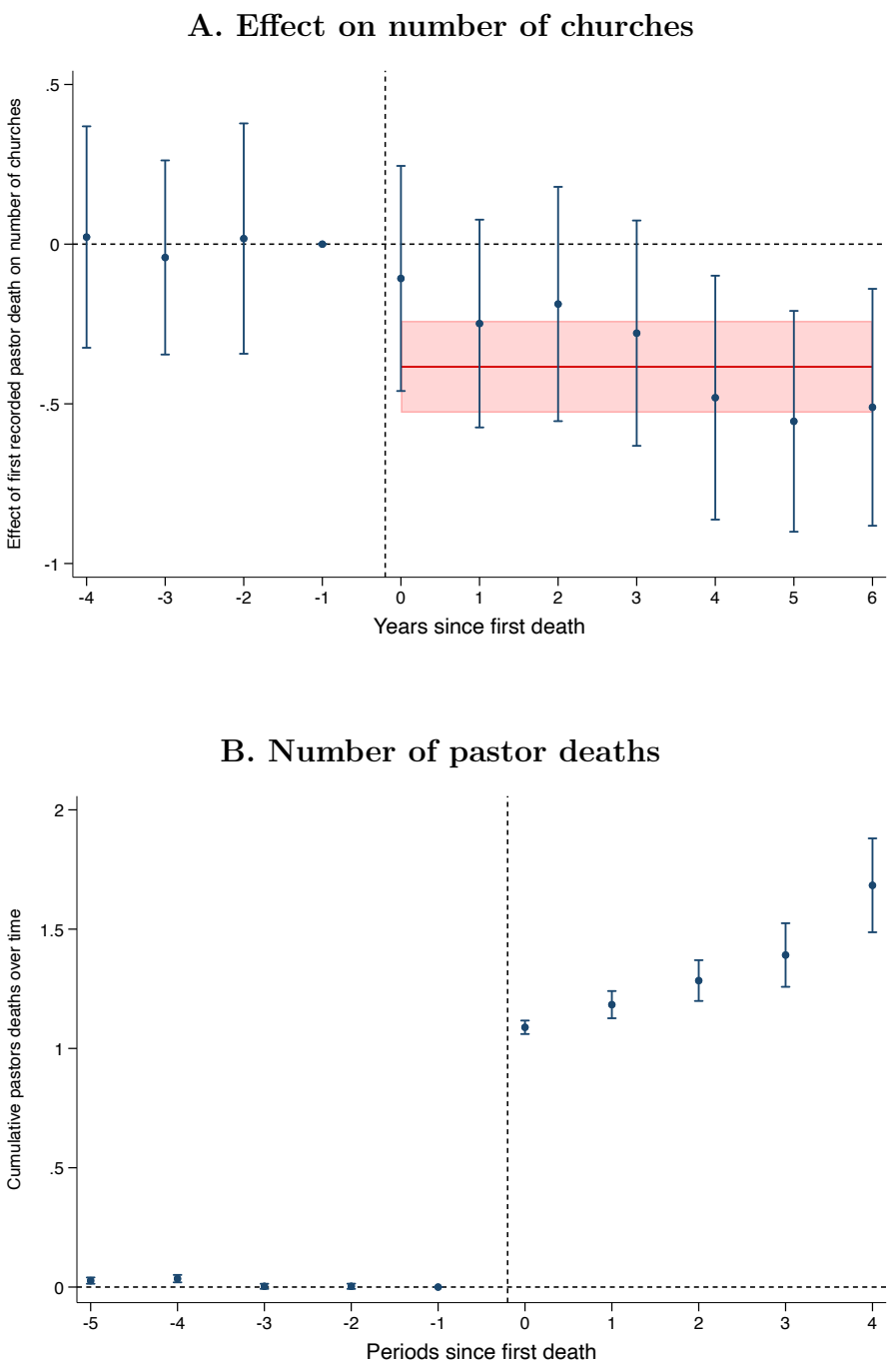
We use the matched controls event study designs described in Section 4.1 to present our main results. We begin with proximate religious outcomes that validate the first-stage links emphasized in our conceptual framework: pastor deaths lead to church closures, and pastor convictions lead to sharp, persistent declines in Baptist church attendance. We then compare the two designs to one another using the decennial *Churches and Church Membership in the United States* data, and finally show results on downstream economic, social, political, and health outcomes.

[8] These are included because matching is done with replacement, so controls can appear in multiple sets.

5.1 Proximate outcomes

In Panel A of Figure 4 we present the effect of pastor deaths on the number of churches that are open in the relevant county. We find stable pre-trends before a sustained drop that is slightly smaller in magnitude than −0.5, indicating a close mapping between pastor deaths and church closures. In Panel B of Figure 4 we present the associated cumulative number of deaths after the first observed death. We find—beyond the mechanical increase of around 1 at event time zero—a steady increase in deaths over time, which helps to explain the dynamic treatment effect in Panel A. For each pastor's death, we observe around a one in three chance of a church closure. This appears to be largely driven by the closure of churches where the pastor was still active the period before their death. In Appendix Figure A8, we find that when a sitting pastor passes away, afterward around one fewer church operates in that zip code. In Appendix Figure A9 we find qualitatively similar results using an event study with a continuous non-absorbing treatment, which allows for identification from multiple deaths across time within a treated unit.

Figure 4: Effect of pastor death on church closures

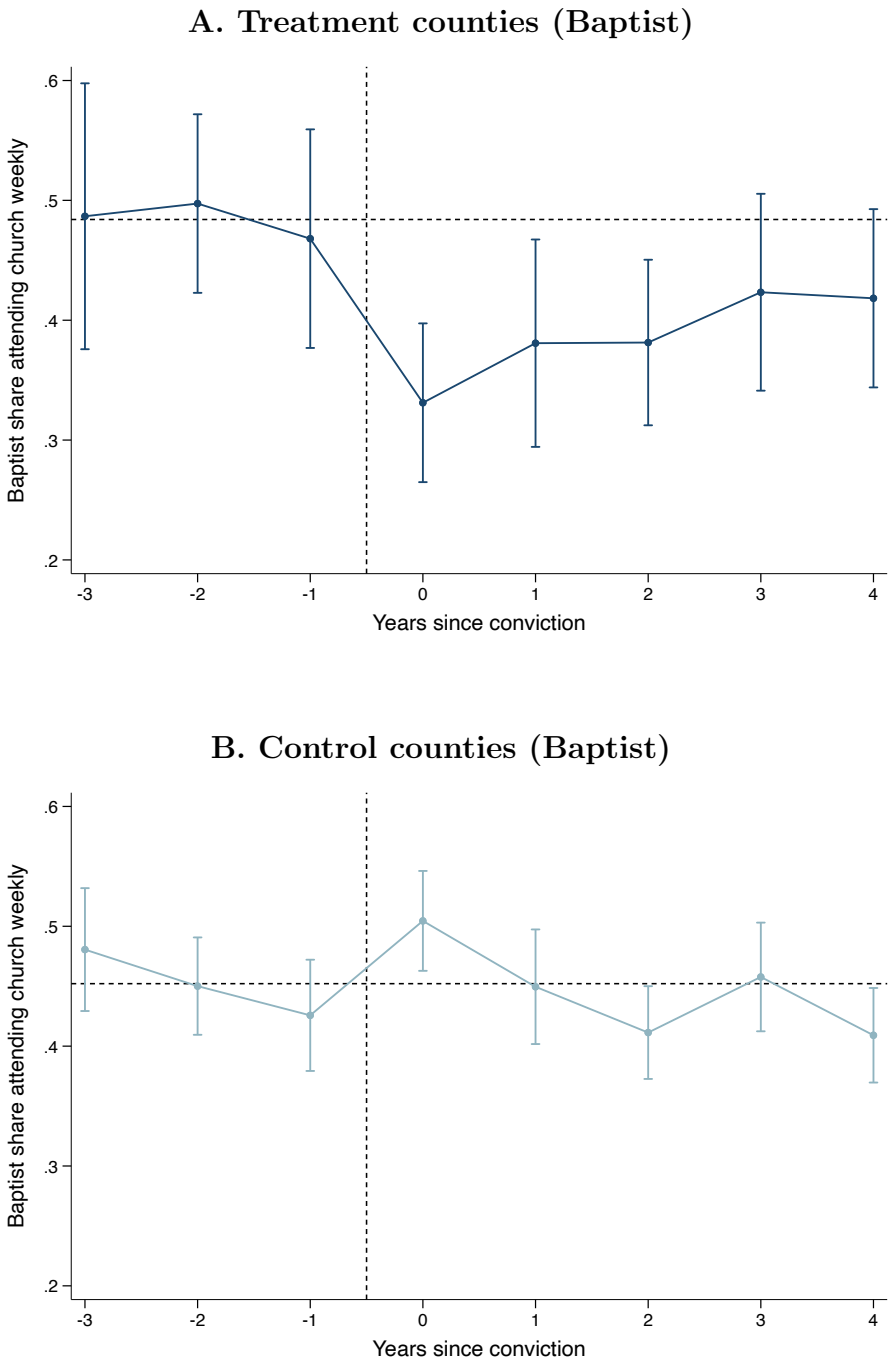


Notes: Panel A plots event study estimates of the first recorded pastor death on the number of Southern Baptist churches in the zip code, measured from the SBC annuals. The unit of observation is a zip code-year panel stacked by the matched set. For each treated zip code, we select five closest within-state control zip codes using nearest-neighbor matching on standardized pre-event covariates measured just before the shock. Event time is relative to the first observed pastor death. Each regression includes year fixed effects, zip code fixed effects, and state-by-year fixed effects. Standard errors are clustered at the state level. Panel A also reports the pooled difference-in-differences estimate and corresponding 95% confidence interval in red. Panel B shows the corresponding event study for cumulative pastor deaths, illustrating the timing and persistence of treatment intensity around the first death.

To estimate the effects of pastors' convictions for sexual abuse we use a method similar to the analysis of pastor deaths, exploiting both variation in treatment assignment and timing using our matched controls strategy. Using individual-level information about church attendance by denomination from CES survey data, we begin by showing raw trends in church attendance among Baptists in our treatment counties and matched control counties in Figure 5. We find an immediate decrease in weekly church attendance among Baptists of nearly 20 percentage points immediately following a conviction, which persists in all later periods. Among Baptists in our matched control counties, we see similar pre-period levels of church attendance, but no comparable decline: church attendance stays mostly constant throughout the entire sample period.

This effect is not driven by small reductions on the intensive margin (e.g. substituting to monthly attendance): we show in Appendix Figure A10 that following a pastor conviction, the share of Baptists who attend church seldom or never more than doubles, rising from 8% in the pre-period to nearly 20% immediately following the shock, and stays elevated near this level through the end of our sample period. In addition, we find effects on the extensive margin of overall religious affiliation: in Appendix Figure A11, we find that following a shock, there is an increase in the share of respondents reporting that they are atheist or agnostic. As shown in Figure A12, we see no observable declines in church attendance among non-Baptists, who are not directly affected by SBC pastor convictions, in either treatment or control counties following a conviction, giving credence to our claim that these shocks were localized within the Baptist population.

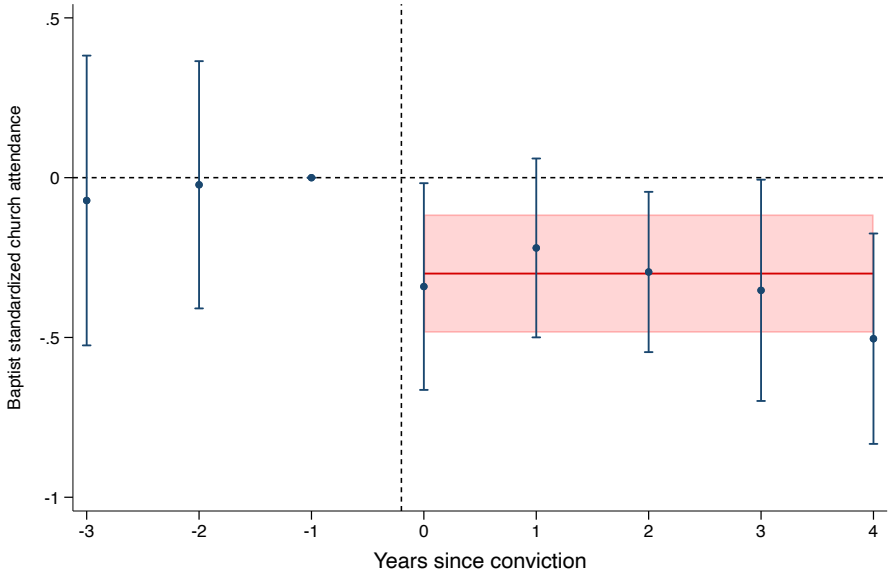
Figure 5: Effect of pastor convictions on weekly church attendance (Baptists)



Notes: Data on church attendance from the Cooperative Election Study. Panel A displays raw trends and 95% confidence intervals for average weekly attendance among Baptists in treatment counties around the year of the first recorded pastor conviction. Panel B shows the analogous trends for Baptist attendance in matched control counties. Each specification uses CES survey weights in constructing annual average attendance rates.

We also estimate the event study specification from Equation 5 in the CES data, allowing us to move beyond raw trends and more easily control for individual- and area-level factors. In Figure 6 we present the estimated effect of pastor convictions on overall standardized church attendance among Baptists.[9] We find an immediate decrease in attendance of over 0.3 standard deviations that, similar to what was shown in the raw trends, persists until the end of the study period.[10]

Figure 6: Effect of pastor convictions on church attendance



Notes: This plot displays the event study analogue to Figure 5. The outcome is standardized church attendance from the CES six-category question on religious service attendance: (1) Never, (2) Seldom, (3) A few times a year, (4) Once or twice a month, (5) Once a week, (6) More than once a week. We code the measure so higher values indicate more frequent attendance and then standardize it over the pooled CES sample, so a 1-unit decrease corresponds to 1 standard deviation. The regression is run using individual-level CES data with unit (e.g. treatment county or matched control county) fixed effects, year fixed effects, and state-by-year fixed effects. We include additional individual-level controls: race-by-year fixed effects, gender-by-year fixed effects, and age-bin-by-year fixed effects (10 year bins). The event study follows the specification given in Section 4.1 with matched control units constructed as described in Section 4. The pooled difference-in-differences estimate and 95% confidence interval are shown in red.

[9]Given the non-overlapping time frames of our data on church locations and church attendance, each of these analyses—showing effects on church presence and church attendance—can only be estimated with their respective identification strategies.

[10]Within the CES data, moving from church attendance "Once or Twice a Month" to "A Few Times a Year" is roughly 0.6 standard deviations, so 0.3 SD corresponds roughly to a 0.5-point change on the underlying 1–6 scale (about half of one response category).

The prior results show substantial effects on proximate religious outcomes for both analyses. However, the relevant outcomes are distinct and come from different sources. As shown in Appendix Figure A6, the vast majority of the *first* deaths of the pastors from 1970 occur prior to the beginning of the CES data in 2006. Similarly, the vast majority of pastor convictions, as presented in Appendix Figure A7, occur after the end of the SBC annuals in 2008. To put both designs on a common footing, we complement our event study results on religious outcomes with a simple difference-in-differences design using the decennial *Churches and Church Membership in the United States* (CCM), which covers the years 1952, 1971, 1980, 1990, 2000, 2010, and 2020.[11]

Table 2 reports these first stage difference-in-differences results for our common outcomes. Following pastor deaths, SBC adherents fall by 9.48 per 1,000 from a baseline of 181 (a 5.24% decline), and SBC congregations decline by 0.046 per 1,000 from 0.704 (a 6.6% decline), and both effects are estimated precisely. After pastor convictions, SBC adherents fall by 6.69 per 1,000 (a 3.83% decline) though this effect is substantially less precise given the much smaller sample and decennial timing relative to observed conviction dates.[12] Non-SBC adherents increase in both settings, indicating a potential reallocation toward existing non-SBC churches rather than new church formation. Taken together, the CCM evidence broadly corroborates the event-study results: both shocks reduce SBC presence at the county level, with suggestive offsetting growth among non-SBC adherents, and the deaths design delivers the sharpest decennial estimates due to better alignment between the data window and the timing of shocks.

[11] The CCM is the only source of local religious data that we are aware of that spans both periods we need—the CES does not reach back far enough to evaluate pastor deaths, and the SBC annuals do not extend far enough forward to study pastor convictions—so this additional data allows us to evaluate the same set of religious outcomes for deaths and convictions in a comparable way.

[12] Because the CCM data are decennial estimates and the majority of convictions happened after 2010, many treated units only have one available post-period for which to estimate the difference-in-differences coefficient.

Table 2: First Stage Difference-in-Differences Results

	SBC adherents (per 1,000)	**SBC congreg.** (per 1,000)	**Non-SBC adherents** (per 1,000)	**Non-SBC congreg.** (per 1,000)
A. Pastor deaths				
Coefficient	-9.48	-0.046	4.76	-0.065
(Std. error)	(1.53)	(0.008)	(2.20)	(0.013)
Percent chg.	-5.24%	-6.57%	1.35%	-4.36%
Baseline mean	181.00	0.704	353.40	1.49
N	50,386	50,386	50,386	50,386
B. Pastor convictions				
Coefficient	-6.69	-0.009	18.32	-0.004
(Std. error)	(6.07)	(0.025)	(8.81)	(0.042)
Percent chg.	-3.83%	-1.75%	5.31%	-0.33%
Baseline mean	174.72	0.511	345.00	1.21
N	3,159	3,678	3,159	3,678

Notes: The table reports difference-in-differences estimates of the effects of pastor deaths and pastor convictions on church activity. Church data are from the near-decennial *Churches and Church Membership in the United States* (1952, 1971, 1980, 1990, 2000, 2010, 2020). All specifications include state-by-year fixed effects. Standard errors are clustered at the unit level (county-by-match group), as the near-decennial structure makes state clustering unreliable with few time periods.

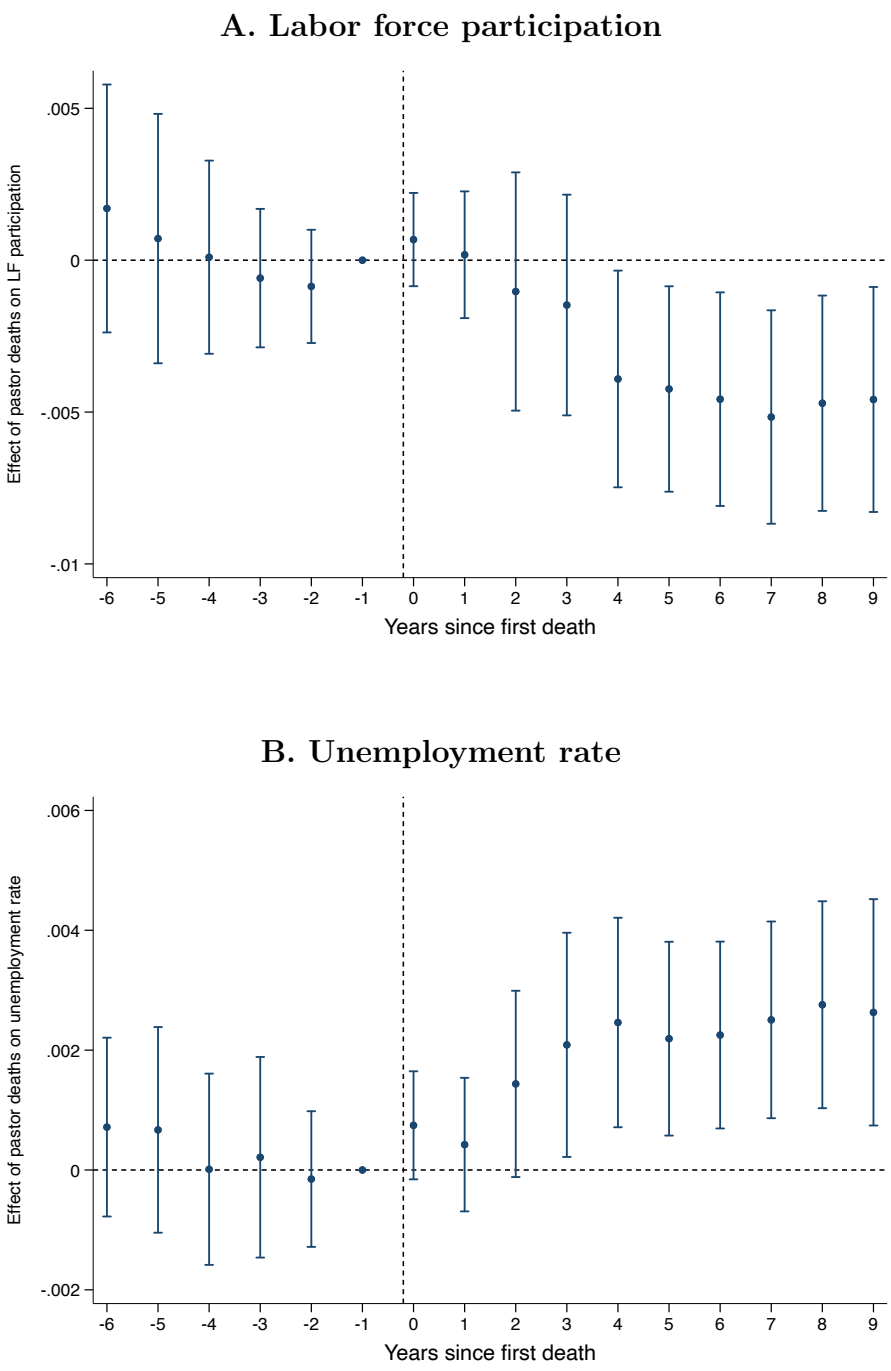
5.2 Labor outcomes

We now study whether the religious shocks documented in Section 5.1 translate into meaningful changes in local labor markets. Our primary outcomes here are the labor force participation (LFP) rate and the unemployment rate, each of which is measured at the county-year level. Throughout, we estimate stacked event study models using the estimating equation in Equation 3 on our matched controls samples defined in Section 4.

For pastor deaths, we define event time to be the first pastor death associated with an SBC congregation in the county, starting from the universe of SBC pastors in the United States from the 1970 Southern Baptist Annual Directory (the earliest directory year in our study). When multiple deaths occur, our specification treats exposure as absorbing at the first event. In Figure 7 we show the dynamic treatment effects of pastors deaths on labor market outcomes, which, as shown in Figure 4, precipitate church closures. For both outcomes, we observe no evidence of differential pre-trends. Following the first death, Panel A shows that LFP declines gradually and remains below the pre-period level for the remainder of the window, while Panel B shows that unemployment rises on a similar timeline. The shapes are consistent with a persistent deterioration in labor market attachment coupled

with worse job-finding among those remaining active in the labor market. The magnitudes are economically small: LFP declines by 0.29 percentage points while the unemployment rate grows by 0.19 percentage points, but are statistically significant and highly persistent. In Appendix Figure A13, we show that the results are not driven by church employment: in both series, when we remove employment related to churches, both results remain statistically significant and persistent. Overall, the patterns align with a mechanism that emphasizes the role of the church in facilitating social contact: pastor deaths precipitate church closures (Section 5.1), which reduce congregation-mediated information flows and weak-tie referrals, dampening church participation and raising unemployment.

Figure 7: Event studies of death effects on labor market outcomes



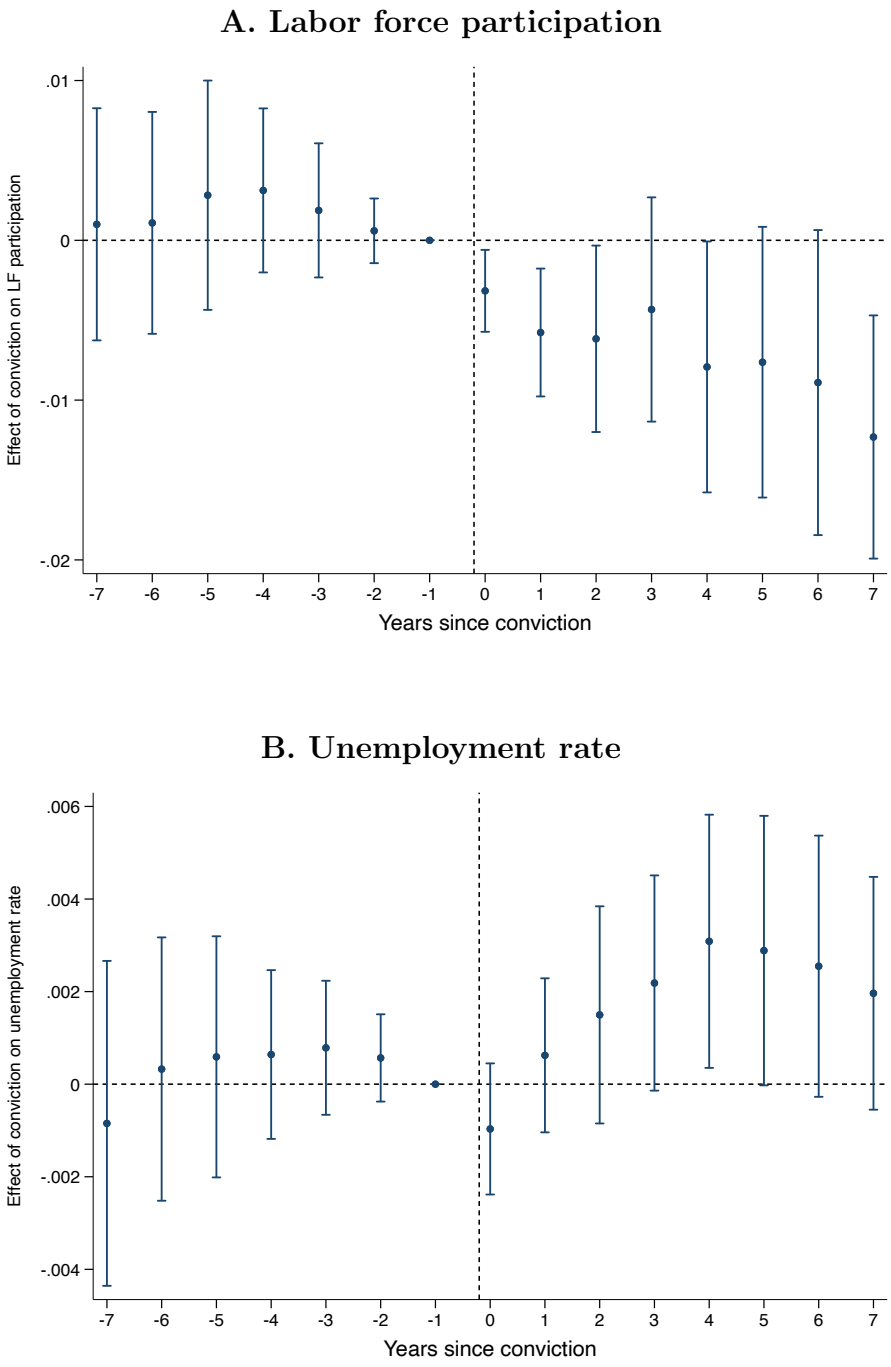
Notes: Panels plot event study coefficients for the effect of the first observed pastor death on county labor-market outcomes, estimated using the matched controls design described in Section 4.1. The unit of observation is county-year; each treated county is matched to within-state nearest never-treated controls as described in Section 4. Each plot shows estimated event study coefficients and 95% confidence intervals. Regressions include match-group x county fixed effects, year fixed effects, and state-by-year fixed effects. Standard errors are clustered at the state level. Unemployment and labor force size are from the BLS Local Area Unemployment Statistics. Annual county population is taken from the National Cancer Institute's SEER population data.

Similar to our analysis of pastor deaths, for convictions, we also define event time to be the first conviction recorded in the county. Because convictions produce large, localized declines in Baptist attendance (Section 5.1), this treatment effect captures both network-access losses (fewer church interactions), and also shocks to social norms (driven by lowered church credibility) that may change behavior beyond the congregation. Estimation mirrors the deaths specification: matched controls within state, with county and state-by-year fixed effects, and stacked event windows.

Figure 8 displays the dynamic treatment effects for LFP and the unemployment rate. We again observe no evidence of differential pre-trends with either series. Consistent with the trends displayed in Figure 7, we find that in the years following a conviction, LFP declines and the unemployment rate increases. With convictions, the post-event increases in the unemployment rate are very similar to the comparable effects of pastor deaths, with the unemployment rate rising 0.17 percentage points. For LFP, the estimated effect is slightly more than two times larger, declining by 0.70 percentage points. This is consistent with our model in Section 2.2, where convictions are predicted to have a larger effect on LFP than deaths due to the additional Weberian work-ethic force that aligns labor force participation with church values. For both outcomes, the direction and persistent dynamics are similar to what we observe following pastor deaths. The timing lines up closely with the immediate and sustained drop in Baptist weekly attendance documented in Section 5.1, indicating a tight first-stage link. As before, results are robust to the removal of church employment: these series are shown in Appendix Figure A14.

We find persistent deterioration in local labor market attachment across two distinct sources of plausibly exogenous variation. Both strategies indicate decreases in LFP and increases in the unemployment rate relative to our matched controls, with flat pre-trends and enduring changes after the shock. These patterns support the view of local religious congregations as economically consequential institutions: by shaping social networks, credibility, and norms, shocks to congregational life spill over into the core margins of local labor markets. The full set of difference-in-differences estimates are included in Table 3.

Figure 8: Event studies of conviction effects on labor market outcomes



Notes: Panels plot event study coefficients for the effect of the first observed pastor conviction on county labor-market outcomes, estimated using the matched controls design described in Section 4.1. The unit of observation is county-year; each treated county is matched to within-state nearest never-treated controls as described in Section 4. Each plot shows estimated event study coefficients and 95% confidence intervals. Regressions include match-group x county fixed effects, year fixed effects, and state-by-year fixed effects. Standard errors are clustered at the state level. Unemployment and labor force size are from the BLS Local Area Unemployment Statistics. Annual county population is taken from the National Cancer Institute's SEER population data.

Table 3: Difference-in-differences estimates for economic outcomes

Outcome	Effect on outcome			Control mean (pre)	% of control mean
	Pooled DiD	Avg ES (all post)	Post (t+3 and later)		
Panel A: Pastor convictions analysis					
LF participation rate	-0.0075**	-0.0070**	-0.0082**	0.478	-1.5%
	(0.0027)	(0.0030)	(0.0039)		
Unemployment rate	0.0010	0.0017*	0.0025*	0.067	2.5%
	(0.0009)	(0.0010)	(0.0013)		
Empl-pop ratio	-0.0077**	-0.0075**	-0.0090**	0.447	-1.7%
	(0.0028)	(0.0030)	(0.0039)		
Panel B: Pastor deaths analysis					
LF participation rate	-0.0031*	-0.0029**	-0.0041**	0.465	-0.6%
	(0.0016)	(0.0014)	(0.0016)		
Unemployment rate	0.0005	0.0019***	0.0024***	0.066	2.9%
	(0.0010)	(0.0007)	(0.0008)		
Empl-pop ratio	-0.0031*	-0.0035**	-0.0049***	0.435	-0.8%
	(0.0018)	(0.0014)	(0.0017)		

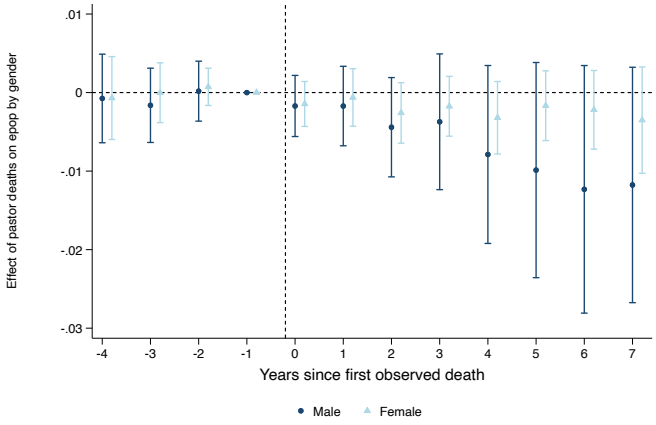
Notes: Cells report coefficients with standard errors in parentheses. "Pooled DiD" is a standard two-way DiD using a treated×post indicator. "Avg ES (all post)" is a simple average of post-event coefficients from the event-study specification used in the corresponding figures. "Post (t+3 and later)" averages event-study coefficients from year t+3 onward (convictions: t+3 to t+7; deaths: t+3 to t+9). Control means are computed over control units in the pre-period within the estimation sample. The final column reports the Avg ES (all post) estimate as a share of the pre-period control mean. All specifications include unit (match group × county), year, and state-by-year fixed effects. Standard errors are clustered at the state level.

Using establishment employment data from the Quarterly Workforce Indicators (QWI), we additionally examine heterogeneity in employment declines following pastor deaths and convictions. In Panel A of Figure 9, we see a clear post-shock decline in the employment-to-population ratio for men, with null effects on employment for women. Panel B shows similar gender dynamics when looking to pastor convictions: declines in the employment-to-population ratio are localized in men.[13]

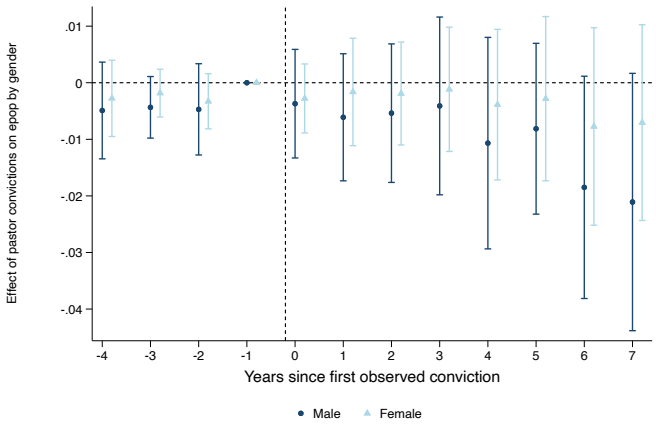
[13]By contrast, in both samples, we do not uncover differential effects by race, shown in Appendix Figure A15. In all race subsamples, standard errors are large, and given the economically small magnitude of observed economic effects, we likely have insufficient power to uncover racial heterogeneity.

Figure 9: Gender heterogeneity in effects of pastor shocks

A. Pastor deaths: employment-to-population ratio, male vs. female



B. Pastor convictions: employment-to-population ratio, male vs. female



Notes: Panels plot event-study estimates of the effect of pastor shocks on the employment-to-population ratio separately for men and women. Panel A uses the first recorded pastor death as the event; Panel B uses the first pastor conviction. Outcomes are constructed from the Longitudinal Employer–Household Dynamics (LEHD) Quarterly Workforce Indicators and are aggregated to the county-year level. Event studies are estimated using the matched-controls design described in Section 4.1. Each plot shows estimated event study coefficients and 95% confidence intervals. Regressions include match-group x county fixed effects, year fixed effects, and state-by-year fixed effects. Standard errors are clustered at the state level.

5.3 Health and social outcomes

We next examine whether shocks to congregational life affect social behavior, political participation, and mortality. Guided by the framework outlined in Section 2.2, where congregations supply both social opportunities and norms, we treat pastor deaths as primarily reducing ac-

cess to congregational networks and pastor convictions as potentially also altering perceived norms.

We begin by studying the effects of pastor deaths on these outcomes. As shown in Figure 2, reported churchgoing is tightly correlated with overall non-church social activity, though the results are purely descriptive. To probe causality, we estimate the effect of pastor deaths on reported church attendance, non-church socialization, and the frequency with which individuals go bowling, a nod to the motif in Putnam (2000) which uses the same data from the DDB Life Style survey. We estimate a Poisson specification with a continuous, non-absorbing treatment and study within-county changes in social behavior, including the same fixed effects as the main design as well as other time-varying demographic controls. Table A1 shows that following deaths, church visits decline, as do non-church social engagements and bowling frequency. The patterns are indicative of a decrease in local social capital: following deaths, we see less measured social interaction overall.

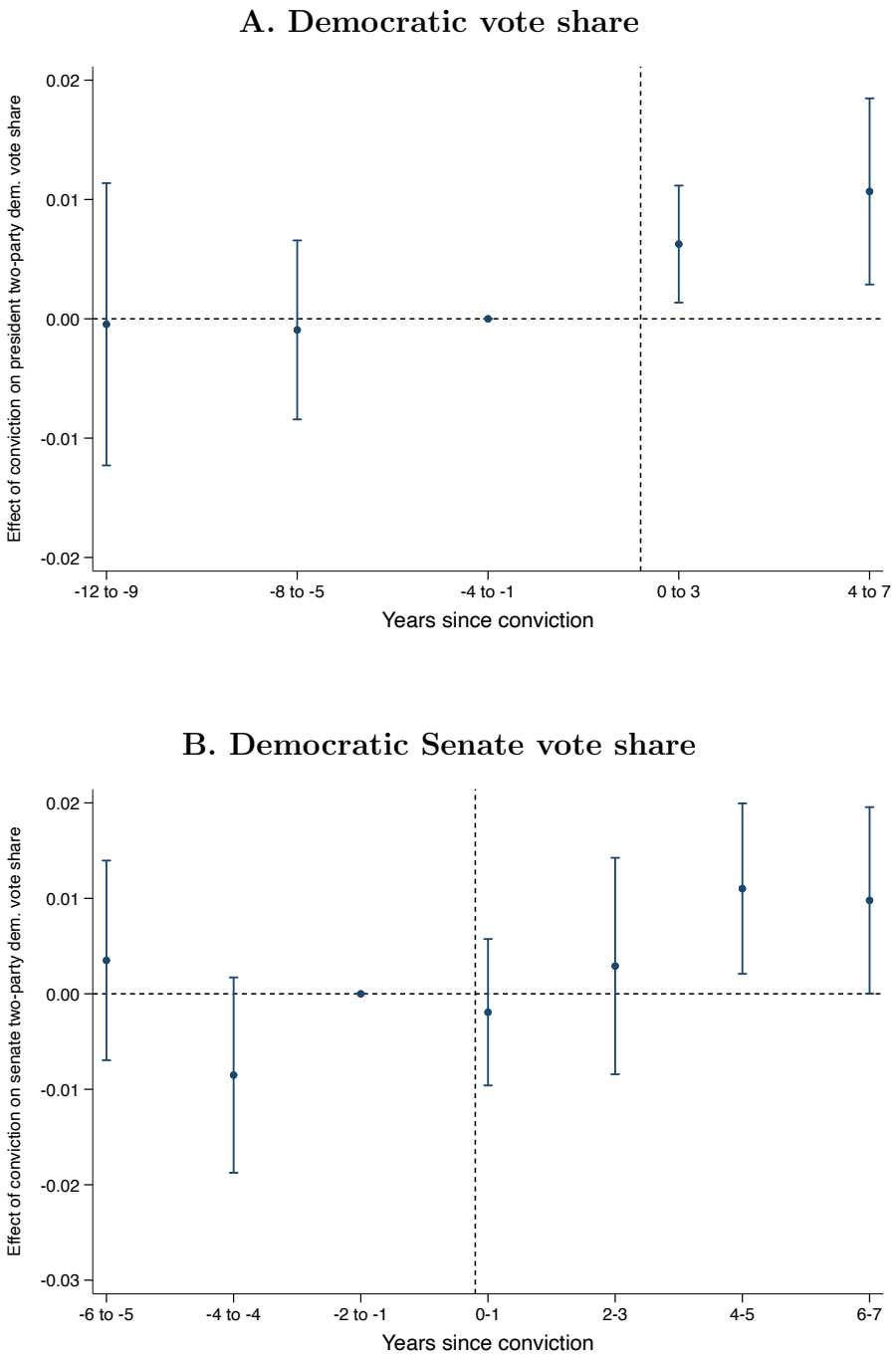
We next estimate Equation 3 using two-party Democrat vote share (presidential and Senate), all-cause mortality, and opioid-involved mortality, outcomes whose relationship to religion has been studied in prior work such as Giles et al. (2023). We find no effects on all-cause mortality (Panel A of Appendix Figure A16), no effects on opioid mortality (Appendix Figure A17), and no effects on Democratic vote shares (Appendix Figure A18), all of which shows flat pre-trends and no systematic post-event changes through medium-run horizons. Thus, while deaths measurably reduce social activity, we do not detect county-wide changes in partisan vote shares or mortality that would be consistent with broader shifts in political preferences or health risk in this design. These null effects are consistent with the model in Section 2.2 in which pastor deaths mainly operate through an attendance channel rather than affecting norms or utility from alignment with the positions of the church.

We now turn to the health and social consequences of pastor convictions. Section 5.1 showed that convictions are followed by large and persistent declines in Baptist weekly attendance. We examine whether these shocks coincide with broader changes in politics and health. Conceptually (Section 2.2), convictions deliver two forces: (i) a network-access loss as attendance drops, and (ii) a credibility or norms shock that can shift preferences and behaviors outside church. As before, we estimate stacked event studies on our matched control samples, using the outcomes described in the preceding analysis of pastor deaths.

Figure 10 plots event study results for the effects of convictions on voting Democratic

in elections; Panel A shows effects of two-party presidential vote shares, while Panel B shows effects on Senate vote shares. In both cases, we see a leftward shift that rises to about 1 percentage point in both offices following the first observed conviction. The fact that effects persist in later elections suggests county-wide changes in preferences rather than temporary turnout or motivation shocks. Consistent with this, we find no significant effects of convictions on voter turnout in either election (Appendix Figure A19). Our findings that loss of belief leads to a decrease in the likelihood of voting for Republicans are consistent with cross-sectional evidence from the General Social Survey in Glaeser et al. (2005). These political responses are absent in our deaths design, consistent with the idea that convictions affect norms as well as access.

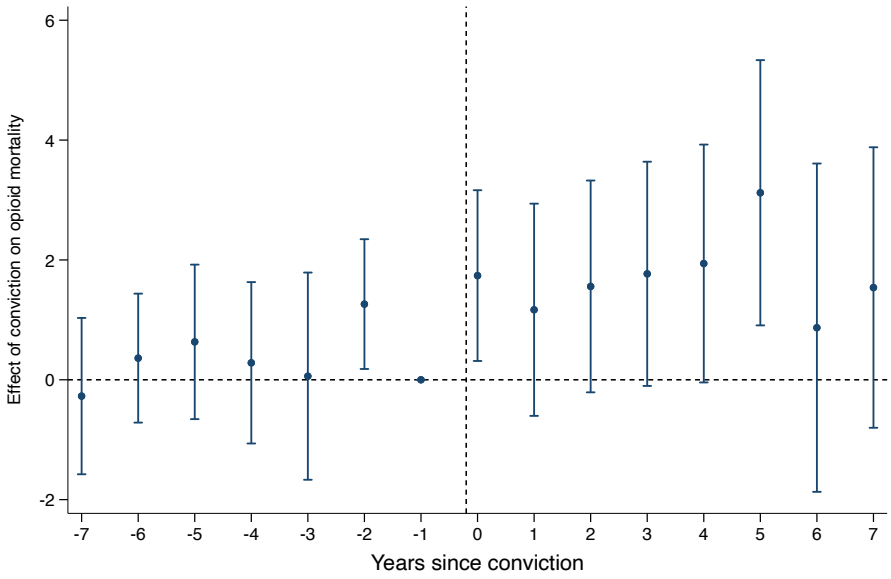
Figure 10: Event studies of conviction effects on two-party Democratic vote shares



Notes: Panels plot event study coefficients for the effect of the first observed pastor conviction on county voting outcomes, estimated using the matched controls design described in Section 4.1. The unit of observation is county-year; each treated county is matched to within-state nearest never-treated controls as described in Section 4. Each plot shows estimated event study coefficients and 95% confidence intervals. Regressions include match-group x county fixed effects, year fixed effects, and state-by-year fixed effects. Standard errors are clustered at the state level. Electoral outcomes are two-party Democratic vote shares in presidential elections (Panel A) and U.S. Senate elections (Panel B), constructed from the ICPSR General Election Data and Dave Leip's Atlas of U.S. Elections.

We find clear and lasting increases in age-adjusted opioid mortality following convictions, as shown in Figure 11. The average post-period event study estimate is 1.7 deaths per 100,000 people relative to a baseline mean of 5.7 deaths per 100,000, indicating a 30 percent increase in opioid mortality. By contrast, all-cause mortality does not change (Panel B of Appendix Figure A16), indicating that our analyses are specific to risky health behaviors with potentially immediate consequences, and are not driven by secular changes over time in area health. As with voting, the opioid results are specific to convictions: we find no comparable change in opioid mortality following pastor deaths.

Figure 11: Effect of pastor conviction on opioid deaths



Notes: The above plot shows event study coefficients for the effect of the first observed pastor conviction on county opioid mortality, estimated using the matched controls design described in Section 4.1. The unit of observation is county-year; each treated county is matched to within-state nearest never-treated controls as described in Section 4. The plot shows estimated event study coefficients and 95% confidence intervals. The regression includes match-group x county fixed effects, year fixed effects, and state-by-year fixed effects. Standard errors are clustered at the state level. Opioid deaths are from the National Center for Health Statistics' Multiple Cause of Death data, constructed using standard ICD-based definitions.

Consolidating the results described in these event studies, we provide a table of difference-in-differences estimates and aggregated event study coefficients in Table 4. As with the event studies, we find sizable and statistically significant effects of pastor convictions on overdose

deaths, opioid deaths, and voting outcomes, with no significant effects on any outcomes following pastor deaths. A full differences-in-differences table combining all outcomes shown in Table 4 and Table 3 is shown in Appendix Table A2, which also includes null effects across both identification strategies on aggregate mortality.

Table 4: Difference-in-differences estimates for health and voting outcomes

	Effect on outcome			Control mean (pre)	% of control mean
Outcome	Pooled DiD	Avg ES (all post)	Post (t+3 and later)		
Panel A: Pastor convictions analysis					
Health outcomes					
Opioid mortality (age-adj)	1.3656**	1.7129**	1.8476*	5.703	30.0%
	(0.5200)	(0.7959)	(0.9395)		
Overdose mortality (age-adj)	1.3649***	1.2699**	1.3506*	11.833	10.7%
	(0.4464)	(0.5868)	(0.6893)		
Voting outcomes					
Pres. Dem vote share	0.0074**	0.0067**	0.0096**	0.399	1.7%
	(0.0028)	(0.0029)	(0.0035)		
Senate Dem vote share	0.0059*	0.0054	0.0104**	0.406	1.3%
	(0.0033)	(0.0038)	(0.0043)		
Panel B: Pastor deaths analysis					
Health outcomes					
Opioid mortality (age-adj)	-0.1743	-0.0136	-0.0403	0.404	-3.4%
	(0.4303)	(0.1158)	(0.1349)		
Overdose mortality (age-adj)	-0.0256	-0.1612	-0.1928	2.033	-7.9%
	(0.5493)	(0.1981)	(0.2232)		
Voting outcomes					
Pres. Dem vote share	-0.0048	-0.0020	-0.0025	0.476	-0.4%
	(0.0030)	(0.0018)	(0.0021)		
Senate Dem vote share	-0.0060	-0.0075	-0.0085	0.568	-1.3%
	(0.0065)	(0.0063)	(0.0079)		

Notes: Same conventions as Table 3. For annual outcomes, "Post (t+3 and later)" uses year t+3 onward (convictions: t+3 to t+7; deaths: t+3 to t+9). For election outcomes, post periods are binned as in the event-study figures, and "Post (t+3 and later)" corresponds to later post bins (convictions: yrs 4–7; deaths: yrs 4–11 for presidential elections and yrs 4–9 for senate elections). Control means are computed over control units in the pre-period within the estimation sample. The final column reports the Avg ES (all post) estimate as a share of the pre-period control mean. All specifications include unit (match group × county), year, and state-by-year fixed effects. Standard errors are clustered at the state level.

Taken together, convictions produce political and opioid mortality responses beyond the substantial declines in church attendance, while deaths primarily reduce observed social

activity without shifting county-level political or health outcomes. This contrast is consistent with our framework: both shocks erode congregational networks, but convictions may potentially also affect behavioral norms that travel beyond church boundaries.

6 Spatial effects and the value of church proximity

So far, we have examined how pastor deaths and convictions affect county-level outcomes such as labor force participation, unemployment, voting, and mortality. These analyses establish that religious disruptions have first-order consequences for local communities. We next investigate the spatial dimension of these effects, focusing on how they vary with distance from the affected church and how they affect local housing markets.

When a pastor dies and a church closes, nearby households experience a sudden increase in distance to the closest congregation. Similarly, when a pastor is convicted of abuse, the congregation loses credibility, creating a localized disamenity. Both shocks therefore generate quasi-experimental shifts in the spatial distribution of church access. By studying outcomes as a function of distance to the nearest church, we can trace the local geography of these effects and measure the extent to which churches are valued as place-based amenities.

6.1 Spatial heterogeneity in shock effects

We proceed in three steps. First, we estimate how pastor deaths affect home prices in nearby neighborhoods. We then turn to pastor convictions, documenting the disamenity value associated with proximity to implicated churches. Finally, we implement a nonparametric IV strategy to recover the causal effect of church proximity on both housing prices and employment per capita, which we use to recover the hedonic value of church proximity in Section 6.2.

We begin by examining how pastor deaths affect nearby home prices through changes in proximity to the nearest Southern Baptist church. When a pastor dies and the church subsequently closes, households in the surrounding area experience changes in the distance to the nearest congregation. If churches are valued as local amenities, this change in access should be reflected in nearby housing prices.

To estimate these effects, we implement a stacked event study following the observed death of a pastor. The treatment group consists of homes for which the pastor of the nearest

church passes away in year a. The control group is constructed from "not-yet-treated" homes whose nearest church experiences a pastor death at least seven years later, outside of the event study window. This approach yields a repeated cross-section of transacted homes, allowing us to estimate dynamic treatment effects under the assumption that, conditional on baseline church proximity in 1994, the pre-period in this analysis, the timing of pastor deaths is quasi-exogenous to local housing market trends.

Formally, we estimate two specifications. First, to capture the effect of pastor deaths on access, we regress the realized distance to nearest SBC church on event time indicators:

$$\delta_{i,t} = \sum_{k \neq -1} \beta_k^{d,\delta} B_{i,t-a}^{d} + \nu_{c(i)}^{\delta} + \mu_{S(i),t}^{\delta} + \varepsilon_{i,t}^{\delta}, \tag{6}$$

Where δ_{it} is the distance to the nearest SBC church for property i at time t. Second, we estimate the corresponding effect on home transaction prices:

$$\log(p_{i,t}) = \sum_{k \neq -1} \beta_k^{d} B_{i,t-a}^{d} + \tau X_i + \nu_{c(i)} + \mu_{S(i),t} + \varepsilon_{i,t}. \tag{7}$$

Here, $p_{i,t}$ is the transaction price, $X_{i,t}$ are logged home characteristics (acreage, bedrooms, bathrooms, and square footage), and we include census tract, $c(i)$, and year $\times$ state, $S(i)$, fixed effects as $\nu_{c(i)}$ and $\mu_{S(i),t}$. The coefficients of interest, the β_k, are estimated separately by bins of 1994 proximity, $\delta_{i,1994}$, discretized as d, and $B_{i,t-a}^{d}$ is an indicator for the years from death time a for a property in distance bin d.

A potential concern is that the composition of churches differs systematically across distance bins. For example, areas with sparser SBC presence tend to have homes located farther from the nearest congregation, which may also correlate with lower density of economic activity. Such heterogeneity affects the share of churches that close following a pastor's death and the scope for new entries to offset exits.[14] To alleviate these concerns, the treatment effects β_k are estimated separately for each distance bin.

Despite these considerations, the data reveal strong first-stage effects: pastor deaths increase distance to the nearest SBC church, with the largest changes observed for homes

[14]Furthermore, some component of the difference in strength of the first stage may be related to the quality of the name match between the Social Security Death Master File and the SBC Annuals. More erroneous matches and thus more erroneous pastor death events could be correlated with the distance to the nearest SBC church for the reasons discussed above, which could affect the strength of both the first and second stages.

that were already farther from a congregation (Panel A of Appendix Figure A20). This pattern is consistent with the geography of churches: in sparser areas the gap between the nearest and second-nearest church is larger, so the loss of the closest congregation produces a larger increase in effective distance. Panel B of Appendix Figure A20 shows that these increases in distance translate into declines in home transaction prices, with bins experiencing the largest distance shocks also experiencing the largest decline in home prices, while bins with no effective change in distance do not change in price. Taken together, Panels A and B display a clear and intuitive link between our first stage results on church distance and our reduced form results on home prices. Panel C of Appendix Figure A21 directly links the magnitude of the price response to the strength of the first stage.

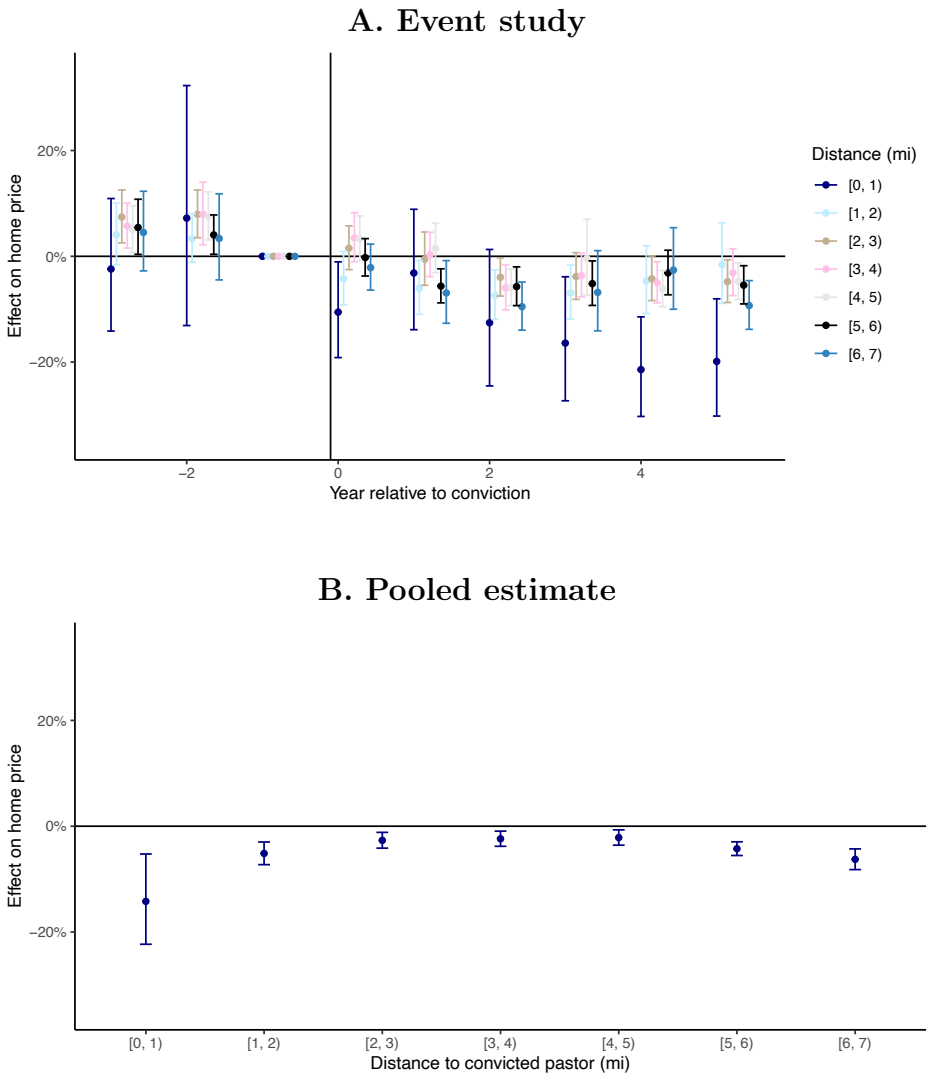
In sum, pastor deaths that lead to the closure of nearby churches cause measurable reductions in local home values. The evidence suggests that these effects operate through changes in the distance to the nearest church, consistent with households valuing proximity as a place-based amenity. Additionally, the fact that there is no measurable change in home transaction price for distance bins with small or no changes in the distance to the nearest church (Panel C of Appendix Figure A21) suggests that home price effects are driven by changes in access to churches, rather than consequences of the pastor's death itself.

We next examine how pastor convictions affect nearby home prices. The design parallels the previous pastor deaths analysis: we implement an event study specification as in Equation 7 around the year of conviction, comparing homes proximate to the convicted pastor's church with "not-yet-treated" homes at the same eventual distance from a church whose pastor was implicated later. Similar to before, identification requires that the timing of convictions is quasi-exogenous to local housing trends, conditional on eventual conviction.

Panel A of Figure 12 presents the dynamic treatment effects. We find relatively stable pre-trends, followed by a sharp and sustained decline in home prices following a conviction, with the effects growing in magnitude before stabilizing. Panel B of Figure 12 pools post-conviction outcomes by distance from the convicted pastor's church. The largest price declines occur within one mile of the church and diminish over greater distances. The estimated home price decline within the first mile is roughly 15 percent, considerably larger than the 4 percent decline found by Linden and Rockoff (2008) for homes located within 0.1 miles of any sex offender. This disparity suggests that the estimated effect shown here is not simply the stigma of living near an offender, but rather the concentrated amenity loss

associated with a pastor's conviction, given the pastor's role as a community leader and the congregation's centrality in neighborhood social life.

Figure 12: Effects of pastor conviction on home prices by distance



Notes: Event study estimated using a control group of houses in the same eventual distance bin from a convicted pastor that have not yet been treated. Standard errors clustered at the stack × distance bin × treatment assignment level.

Finally, we combine the variation from pastor deaths with nonparametric instrumental variables methods to recover the causal effect of SBC church proximity on home prices and the employment-to-population ratio. We estimate the following specification:

$$\log\left(p_{i,t}\right) = \underbrace{\theta\left(\delta_{i,t}\right)}_{\text{Church dist}} + \underbrace{\phi\left(\delta_{i,1994}\right)}_{\text{1994 dist}} + \underbrace{\beta X_i}_{\text{Chars.}} + \mu_{C(i),t} + \nu_{c(i)} + \varepsilon_{it}. \tag{8}$$

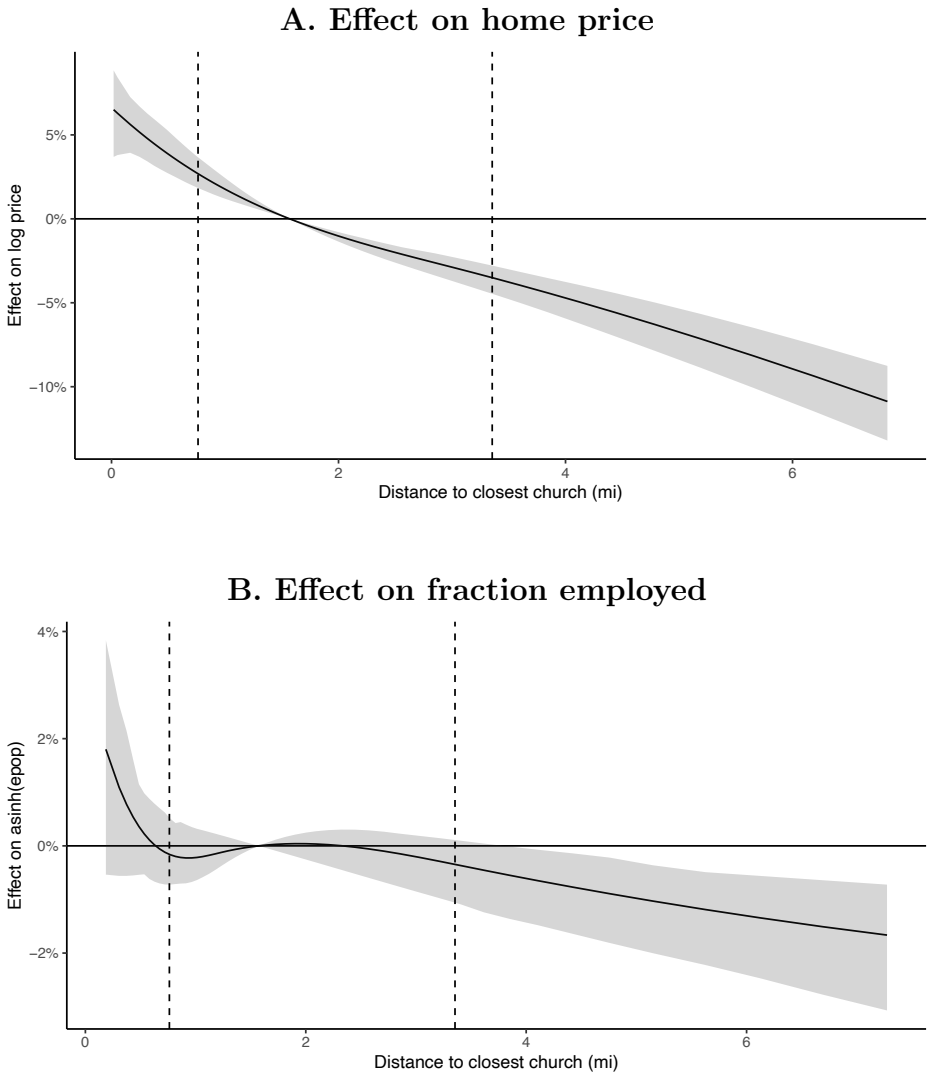
where $\delta_{i,t}$ is distance to the nearest SBC church, instrumented by the interaction of pre-period distance $\delta_{i,1994}$ and pastor death timing. We approximate the logarithm of the church distance price effect function $\theta(.)$ flexibly using basis functions and employ higher-order expansions of pre-period distance interacted with death timing, including for the second-nearest church, as instruments, following Newey and Powell (2003) and Chen and Christensen (2018). Here the characteristics X_i are observables of the home as well as fixed effects for county × year, $\mu_{C(i),t}$, and census tract $\nu_{c(i)}$. We also flexibly control for pre-period distance from the nearest SBC church with ϕ following Borusyak and Hull (2023). The estimated effects relate closely to the event study estimates of the effects on distance to the nearest church and the change in home transaction price of pastor deaths, as shown in Appendix Figure A20. The estimand of interest, θ, non-parametrically relates the heterogeneous first stages and reduced forms to recover the curvature of the effect on price of distance to the nearest SBC church. The sample includes 7.47 million transactions, and the corresponding linear IV F-statistic is 124,645, indicating strong first-stage relevance.

We estimate an analogous specification to relate church proximity to tract-level employment-to-population ratios:

$$\log\left(\text{epop}_{i,\ t}\right) = \underbrace{\theta\left(\delta_{i,t}\right)}_{\text{Church dist}} + \underbrace{\phi\left(\delta_{i,1994}\right)}_{\text{1994 dist}} + \underbrace{\beta X_i}_{\text{Chars.}} + \mu_{C(i),t} + \nu_{c(i)} + \varepsilon_{it}. \tag{9}$$

This sample includes 31,681 observations from census and ACS years and has a linear IV F-statistic of 950.

Figure 13: Effect of distance from nearest church on outcomes



Notes: Point-wise confidence intervals are calculated via 120 draws of a Bayesian bootstrap. Non-parametric IV effect estimated consistent with the specifications in Equations 8 and 9 using a basis expansion of the distance to the nearest SBC church.

Figure 13 plots the nonparametric IV estimates. Panel A shows that home prices decline significantly with distance from the nearest SBC church, with effects concentrated within the first few miles. Panel B shows a similar but smaller pattern for employment per capita, where employment effects are concentrated in a small radius around the church. These results align closely with the event study evidence above: pastor deaths increase distances to SBC churches, which in turn reduces nearby home values, and to a lesser extent local employment rates. Taken together, the results provide direct evidence that proximity to

churches is capitalized into housing markets and carries measurable economic value.

6.2 Value of proximity

The event study and NPIV results in Section 6 show that pastor deaths increase distance to the nearest church, and that greater distance translates into lower home prices. We use these estimated reduced-form slopes to recover households' willingness-to-pay (WTP) for proximity to an SBC church, allowing us to move from documenting capitalization effects to quantifying their magnitude in interpretable dollar-denominated welfare terms.

We embed the results in a structural hedonic framework to translate our reduced-form estimates into a measure of households' preferences for church proximity. In this setting, proximity to a SBC church is a local amenity whose value is revealed by household decisions, considering transaction prices. Households choose among housing options, each characterized by observable attributes, unobserved neighborhood quality, and distance to the nearest SBC church. By comparing how transaction prices vary with distance in the quasi-experimental setting generated by pastor deaths, we can recover household WTP for church proximity.

Formally, let $\mathcal{J}$ denote the set of housing options available to households and index options by j. Each house j is defined by its distance to the nearest SBC church δ_j , observable characteristics $A_j \in \mathbb{R}^D$, neighborhood attributes $\eta_{n(j)}$, unobserved vertical quality $\xi_j \in \mathbb{R}$, and a price $P_j > 0$. A household i, with wealth W_i, chooses a housing option $j_i^* \in \mathcal{J}$ and consumption of other goods E_i to maximize an individual-specific, hedonic utility function with the following quasilinear functional form:

$$(j_i^*, E_i) \coloneqq \arg\max_{j \in \mathcal{J},\, e>0} U_{ij} + e \quad \text{such that} \quad e + P_j \leq W_i, \tag{10}$$

$$U_{i,j} \coloneqq -\beta_i \, \delta_j + U_i^{(-\delta)}\left(A_j, \eta_{n(j)}\right) + \xi_j. \tag{11}$$

Where β_i denotes household i's willingness to pay to be one mile closer to the church. We can define $X_j \coloneqq \left(\delta_j, A_j, \eta_j, \xi_j\right)$. The function $U_i^{(-\delta)}$ represents household i's utility over all observable and unobservable attributes of the housing option j other than distance to the nearest SBC church.

Under our model, household i receives the following indirect utility from living in j:

$$U_{i,j} = -\beta_i \, \delta_j + U_i^{(-\delta)}\left(A_j, \eta_{n(j)}\right) + \xi_j - P_j. \tag{12}$$

Since there is no household-specific idiosyncratic component of utility, household i's indirect utility from living in j is determined fully by the price P_j and the characteristics X_j. Thus, we can view household i as choosing a bundle of housing characteristics $X_j \in \mathcal{X}$ and paying P_j for that bundle (Rosen, 1974; Bajari and Benkard, 2005). Applying a result from Bajari and Benkard (2005), with no assumptions on the supply of housing, if households are maximizing their utility consistent with Equation 10, then there exists a unique, smooth price function $p : \mathcal{X} \to \mathbb{R}_+$ mapping characteristics X_j to price P_j such that $P_j = p\left(X_j\right)$. In this environment, Equation 10 can be transformed to:

$$\max_{X_j \in \mathcal{X}} u_i\left(X_j\right) - p\left(X_j\right) \tag{13}$$

$$u_i\left(X_j\right) \coloneqq -\beta_i \delta_j + U_i^{(-\delta)}\left(A_j, \eta_{n(j)}\right) + \xi_j, \tag{14}$$

which is a direct optimization problem over housing characteristics.

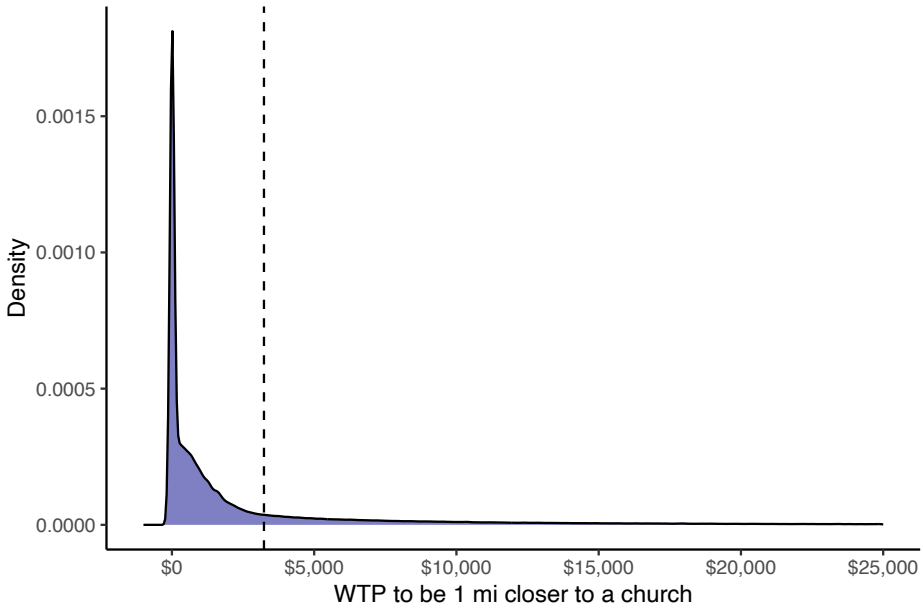
We assume that households make an essentially continuous choice over the distance to their nearest SBC church δ_j. This is motivated by the fact that we observe over 6.7 million housing transactions in the United States within 10 miles of an SBC church. As such, so long as p is convex, we can identify household i's willingness to pay to be one mile closer to the nearest SBC church, β_i, using the first order condition of their housing choice problem with respect to church distance:

$$\left.\frac{\partial u_i}{\partial \delta}\right|_{x=X_{J_i}} - \left.\frac{\partial p}{\partial \delta}\right|_{x=X_{J_i}} = 0 \implies \beta_i = \left.\frac{\partial u_i}{\partial \delta}\right|_{x=X_{J_i}} = \left.\frac{\partial p}{\partial \delta}\right|_{x=X_{J_i}}. \tag{15}$$

Given a consistent estimate of the causal price gradient with respect to church distance, we can back out the distribution of β_i across households. Section 6.1 presented the causal effect of distance to the nearest SBC church on home prices, presented in Figure 13. Using the estimated derivative of this function (Appendix Figure A23), we compute each household's implied β_i.[15] We display our estimates of the full distribution of β_i in Figure 14. The mean WTP to live one mile closer to the nearest SBC church is approximately \$3,235, with substantial heterogeneity: many households are nearly indifferent, while a right tail values proximity at over \$15,000.

[15] We calculate this for all homes within 7.33 miles of the nearest SBC church, so this sample can be interpreted as the valuation amongst those living roughly proximate to *any* SBC church.

Figure 14: Hedonic valuation of distance to church



Notes: The effect is estimated by scaling the estimated slope of the relationship between distance from the nearest SBC church and home price, presented in Figure A23 by each house's counterfactual value.

7 Conclusion

Organized religion is a central component of people's lives across the United States and beyond. In this paper, we provide new causal evidence on how religious access and belief shape local economic and social outcomes. We study this within the Southern Baptist Convention by evaluating two distinct shocks to local congregations: pastor deaths and convictions. Using matched-controls event studies and a hedonic demand model, we show that both shocks weaken congregational networks and have measurable consequences that span labor markets, health, politics, and housing.

We show that SBC pastor deaths lead to church closures, and SBC pastor convictions lead to substantial decreases in church attendance. After each shock, labor force participation declines and unemployment increases. The labor market effects across both specifications emphasize the importance of social networks in hiring, where churches serve as an important community institution in which individuals can form both strong and weak ties that have been shown to be important for hiring (Granovetter, 1973). We find similar effects on unemployment rates (increases of ~ 0.2 percentage points) across both specifications, but

labor force participation reductions that are twice as large when pastors are convicted than when pastors die (decreases of 0.7 and 0.3 percentage points, respectively). This asymmetry is consistent with an additional credibility channel following convictions that affects religious belief, in line with a Weberian work-ethic mechanism (Weber, 1930).

The two shocks have divergent effects on behaviors that are norm-sensitive. Pastor deaths are followed by less non-church socialization but do not shift voting or opioid mortality, suggesting primarily a shock to access. Pastor convictions precipitate large declines in Baptist attendance and produce behavioral changes counter to SBC teachings: Democratic vote shares increase and opioid mortality rises, patterns indicative of a belief shock that weakens the persuasive force of local doctrine.

Local housing markets respond to these shocks. Following convictions, home prices fall most within one mile of the convicted pastor's church (nearly 15%) and attenuate with distance. Following deaths, prices decline only where closures increase distance to the nearest SBC church, consistent with an access channel. A hedonic model implies an average willingness to pay of about $3,200 to live one mile closer to an SBC church, with substantial heterogeneity across households.

These results underscore the role of congregations as a source of place-based social capital: they knit together weak ties that support job search and embed norms that shape behavior. In an era of declining religious participation, the erosion of either access (fewer congregations, greater distances) or credibility (weaker doctrinal authority) can ripple through employment, health, politics, and housing demand. Recognizing these distinct channels helps explain why otherwise similar places diverge and clarifies what is lost when community institutions unravel.

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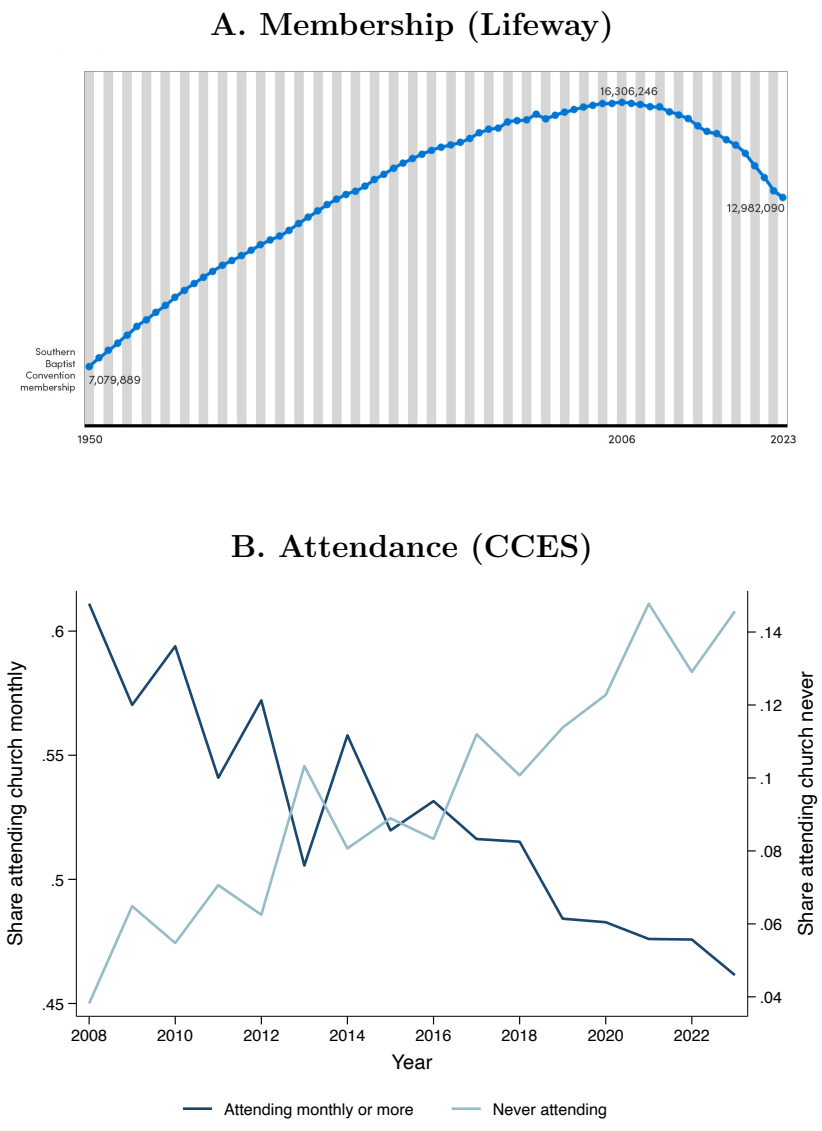
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A Appendix Figures

Figure A1: Trends in SBC membership and attendance



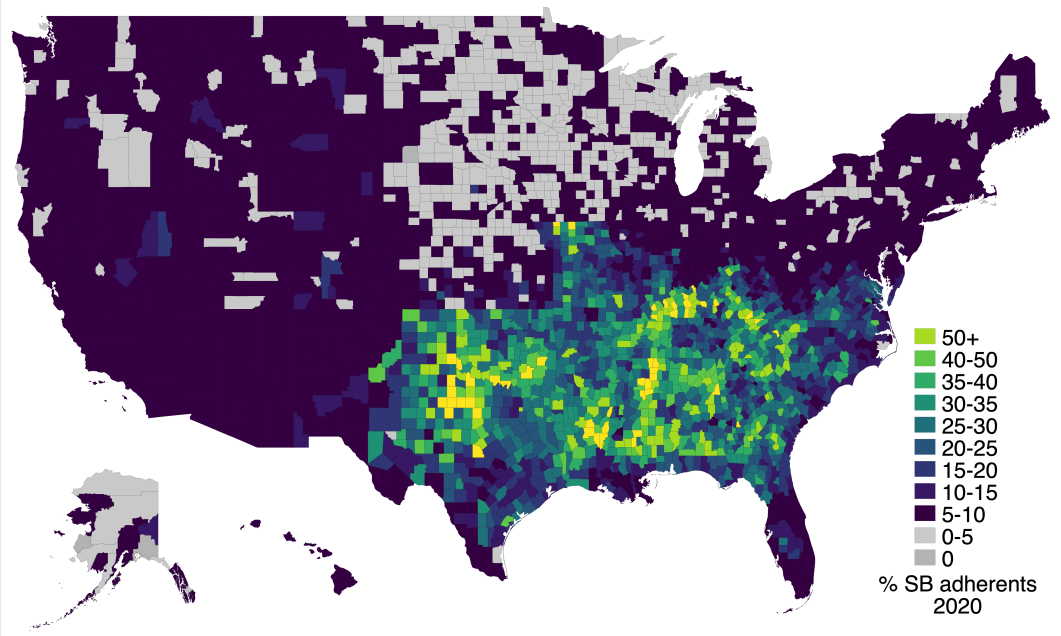
Notes: Data in Panel A comes from Lifeway Research; data in Panel B comes from the Cooperative Election Study. Panel B is constructed by subsetting the CES data to respondents who identify as Baptist, and plotting annual average attendance variables. The dark blue line plots share attending church monthly, defined as share responding that attendance is "Once or Twice a Month", "Once a Week", or "More Than Once a Week". The light blue line plots never attending church, defined as the share responding that church attendance is "Never" to the same question.

Figure A2: Example Southern Baptist Church



Notes: Pictured is the First Southern Baptist Church of Ash Fork, Arizona, led by Pastor David Cox.

Figure A3: Southern Baptist Adherence: 2020



Notes: County-level share of population who are Southern Baptist adherents in 2020. Data come from the 2020 U.S. Religion Census - Religious Congregations and Membership Study.

Figure A4: Example SBC Annual



PASTORS

Aaron Joseph L Rt 1 Bx 69 Geneva Al 36340
Aaron Lafayette Rt 1 Cussetta Al 36852
Aaron Marvin 3005 Sheldon Clovis NM 88101
Abanathy Charles PO Bx 97 Black Rock Ar 72415
Abbott Barney 305 E 4th Penn W Holdenville Ok 74848
Abbott Clifford E Modena Bp Ch Modena Mo 64663
Abbott Cloyd Rt 3 Tazewell Tn 37879
Abbott Don PO Bx 334 Forrest City Ar 72335
Abbott E A 705 No Jackson St Albany Ga 31705
Abbott Earl Rt 5 Bx 922 London Ky 40741
Abbott Eugene Thompsonville Il 62890
Abbott H W 3162 Pershall Rd Saint Louis Mo 63136
Abbott Ivan J 411 E Waggoner St Sullivan Il 61951
Abbott James 105 Bullard St Enterprise Al 36330
Abbott John B Bx B B Espanola NM 87532
Abbott Kenneth L 509 E Woodard Dennison Tx 75020
Abbott Lewis 429 So Riverhills Dr Temple Terrace Fl 33617
Abbott Raymond C-26 Madison Square Apt Madison Tn 37115
Abbott Wilburn 1906 Lydia Dr Owensboro Ky 42301
Abbott Frank A Jr 1445 Downing St Charleston SC 29407
Abel H B Rt 1 Vance Ms 38964
Abel Harlan A 6503 W Cameron Tulsa Ok 74127
Abel Jim PO Bx 37 SBC Walnut Ridge Ar 72476
Abel Richard St Johns Bp Ch Ehrhardt SC 29081
Abel W B Noxapater Ms 39346
Abel William M PO Bx 582 Newland NC 28657
Abell Troy D Bx 117 Wheatley Ky 40389
Abercombie Alex N Collins Ms 39428
Abercombie C L 1310 No Crain Altus Ok 73521
Abercrombie C M Wheeler Tx 79096
Abercrombie J B Bellview Bp Ch Laurens SC 29360
Abercrombie M L Rt 4 Pickens SC 29671
Abercrombie Robert M Rt 1 Bx 6236 McCalla Al 35111
Abernathy D J 741 No Mills Av Orlando Fl 32803
Abernathy Dan Rt 1 Bx 211 New London NC 28127
Abernathy Donald Rt 1 Fyffe Al 35971
Abernathy Eugene 2611 SW 13 Pl Fort Lauderdale Fl 33312
Abernathy James W 3407 Kester Wood Dr Knoxville Tn 37918
Abernathy Jerry D 706 Pecan Crossett Ar 71635
Abernathy Paul J 700 Morgan Falls Rd Dunwoody Ga 30043
Abernathy Roy Rt 3 Canton Ga 30114

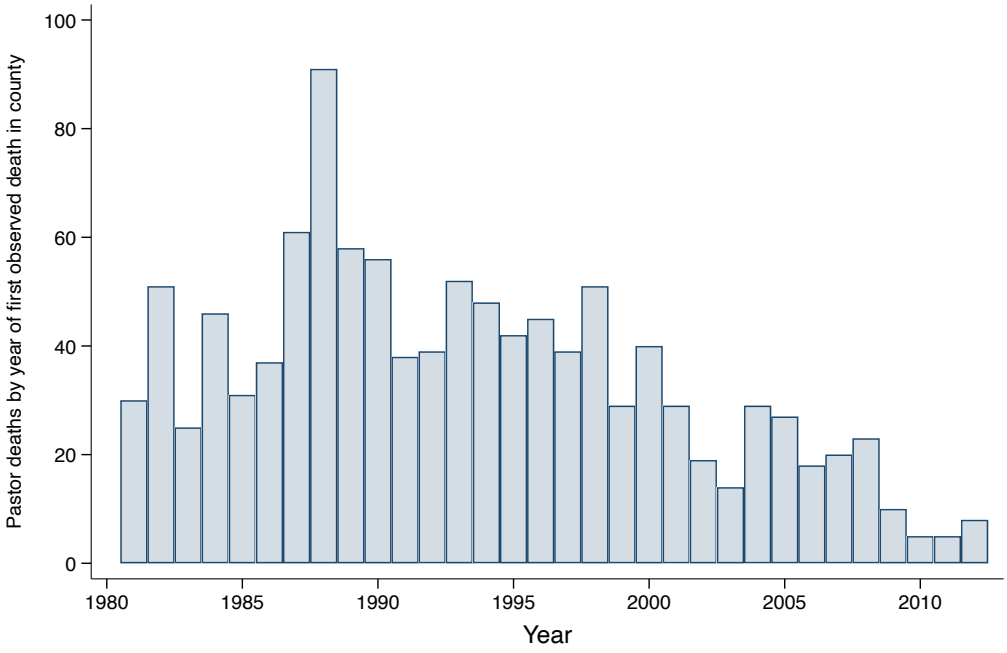
Notes: Image of the 1970 SBC Annual digitized by the authors to create a directory of churches and pastor names.

Figure A5: Abuse of Faith Database



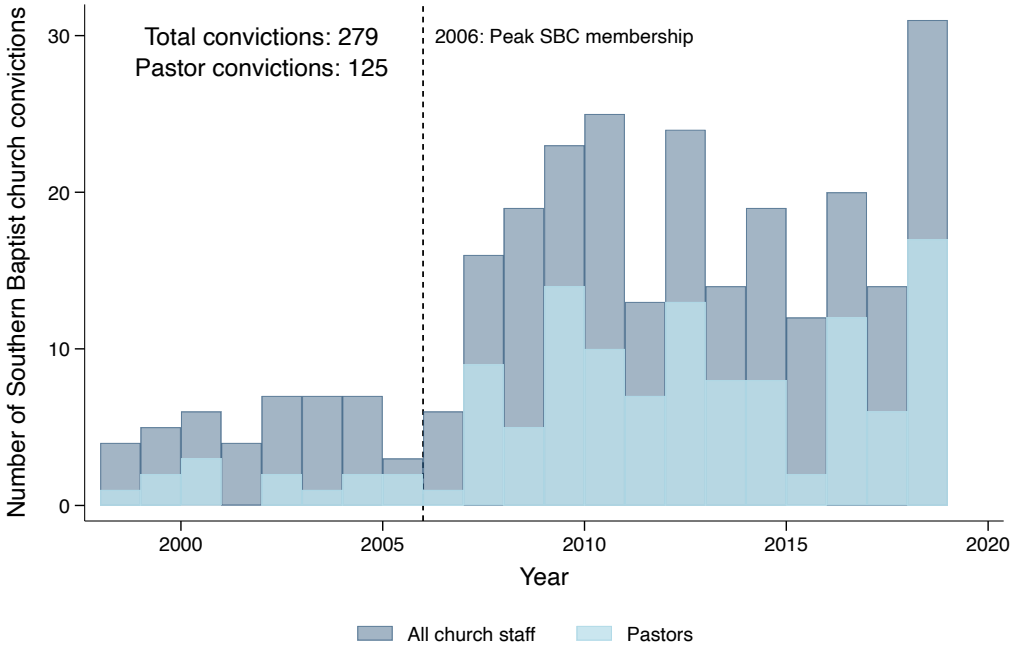
Notes: Screenshot of the Abuse of Faith database.

Figure A6: First observed pastor death in county over time



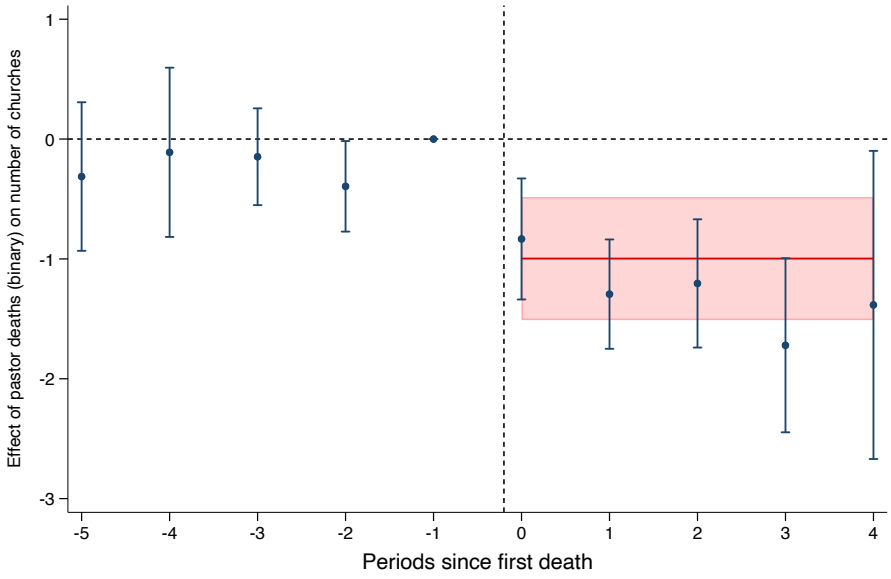
Notes: This plot shows the distribution of years for first-observed pastor deaths in a given county in our analysis sample.

Figure A7: Timing of convictions



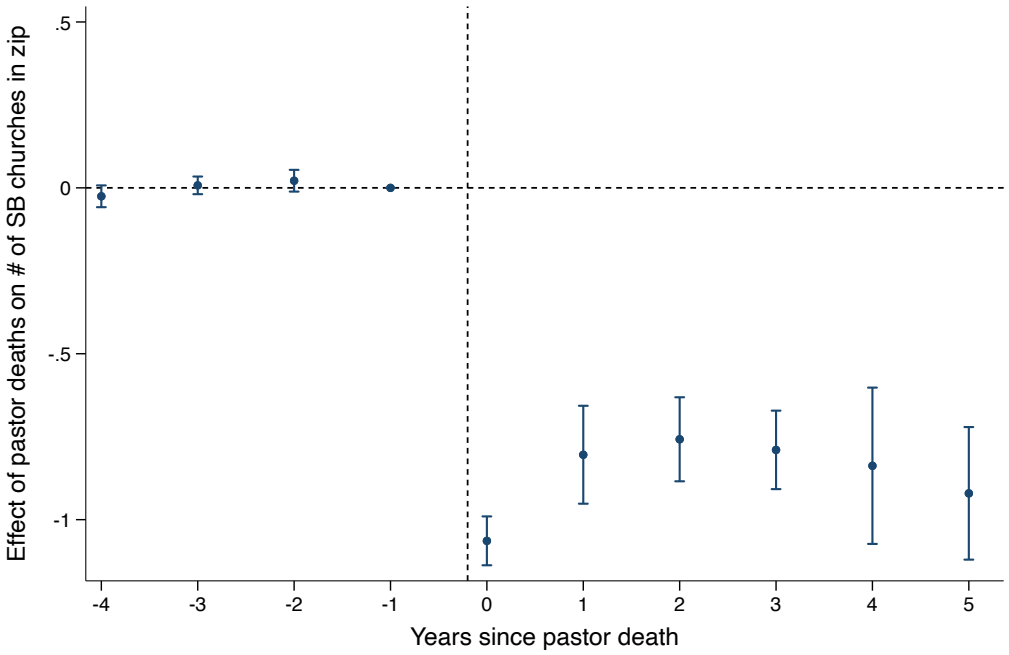
Notes: Data on convictions from the *Houston Chronicle's* "Abuse of Faith" database.

Figure A8: Effect of pastor death on church closures



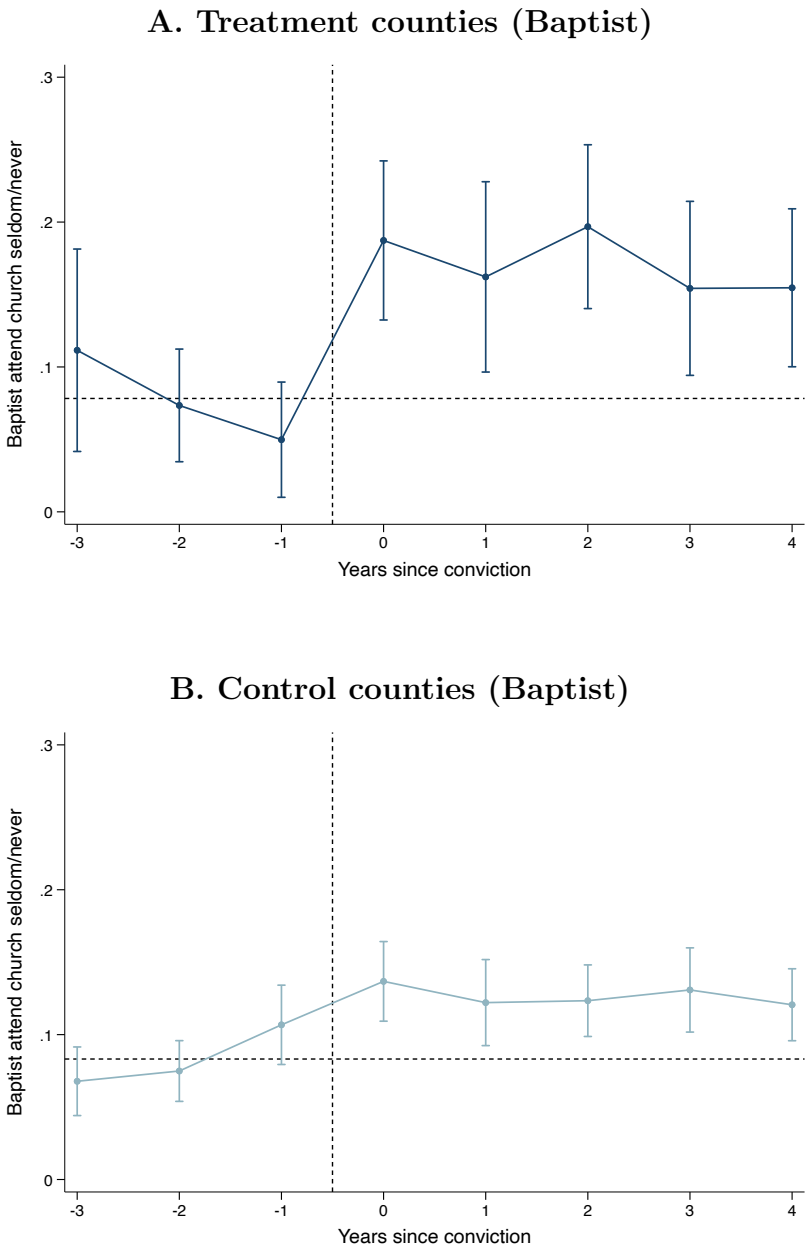
Notes: The figure plots event-study estimates of the effect of the first recorded pastor death on the number of Southern Baptist churches in a zip code, measured from the SBC Annuals. The unit of observation is a zip code–year panel stacked by the matched set. For each treated zip code, we select five closest within-state control zip codes using nearest-neighbor matching on standardized pre-event covariates measured just before the shock as described in Section 4. The sample is restricted to deaths of active pastors, defined as pastor deaths in congregations for which the pastor is observed as active in period t=-1 . The regression includes year fixed effects, zip code fixed effects, and state-by-year fixed effects; standard errors are clustered at the state level. The pooled difference-in-differences estimate and its 95% confidence interval are shown in red.

Figure A9: Effect of pastor death on number of churches (continuous)



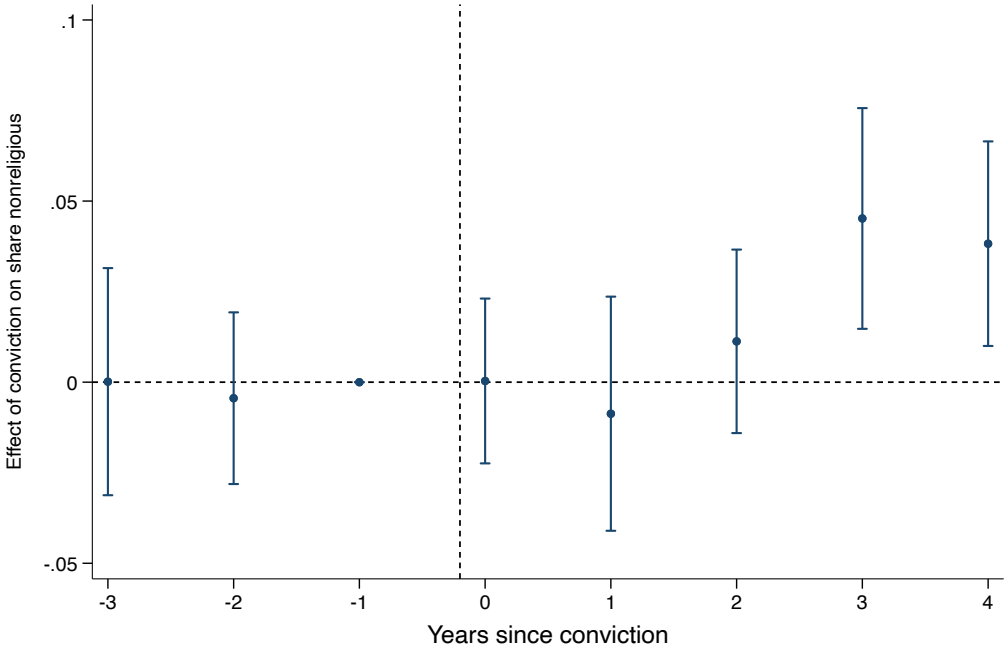
Notes: In this specification, we estimate the dynamic treatment effect of $Y_{zt} = \gamma_t + \delta_z + \sum_{k \neq -1} \beta_k \left[\log\left(\frac{PastorDead_{zt}+0.1}{Matched_z+0.1}\right) \times \mathbf{1}\{t - T_{zt} = k\} \right] + \varepsilon_{zt}$ where we define a continuous treatment effect rather than a binary as in Figure 4. We estimate this treatment effect using the De Chaisemartin and d'Haultfoeuille (2024) estimator which allows for multiple treatments over time within a treated unit. The regression includes year fixed effects, zip code fixed effects, and state-by-year fixed effects; standard errors are clustered at the state level.

Figure A10: Effect of pastor convictions on share seldom or never attending church (Baptists)



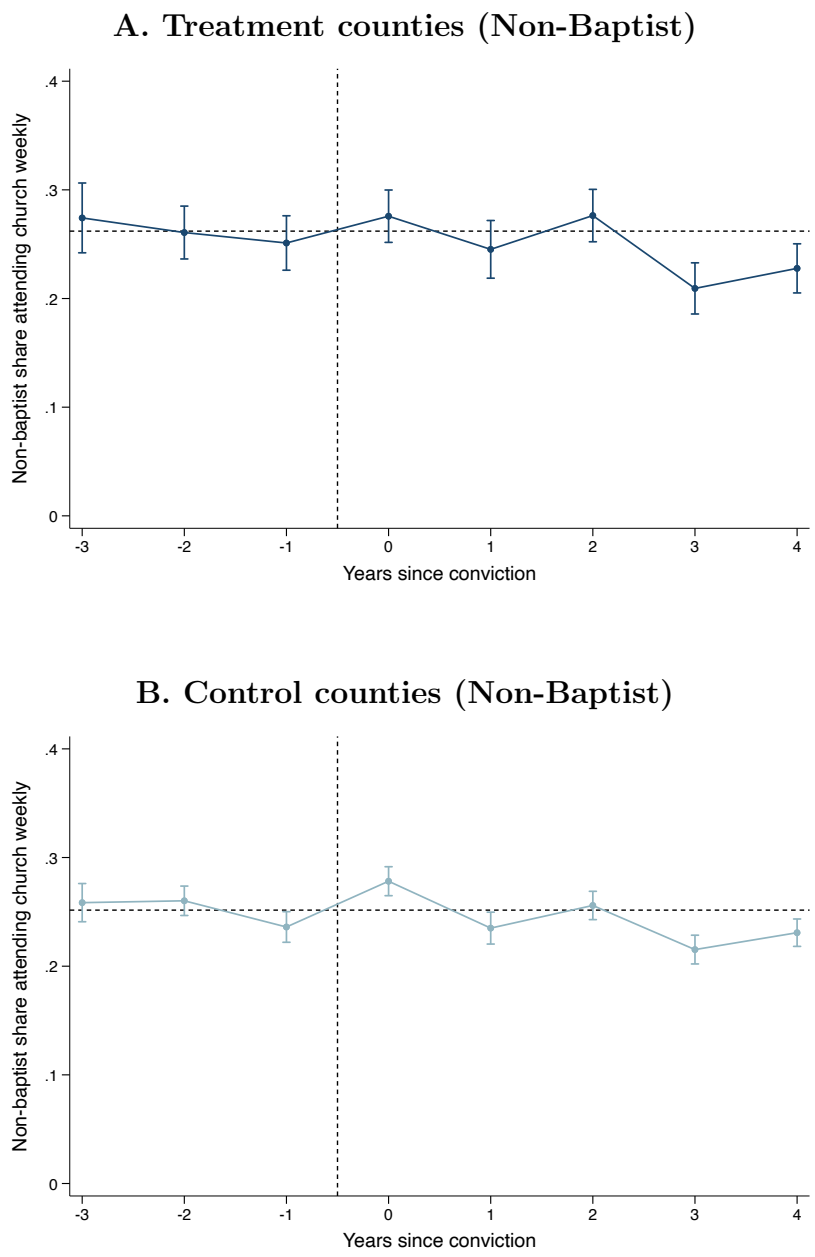
Notes: Data on church attendance from the Cooperative Election Study. Panel A displays raw trends and 95% confidence intervals for share of Baptists in treatment counties attending church seldom or never around the year of the first recorded pastor conviction. Panel B shows the analogous trends for Baptist shares attending seldom or never in matched control counties. Each specification uses CES survey weights in constructing annual average attendance rates.

Figure A11: Effect of pastor convictions on share nonreligious



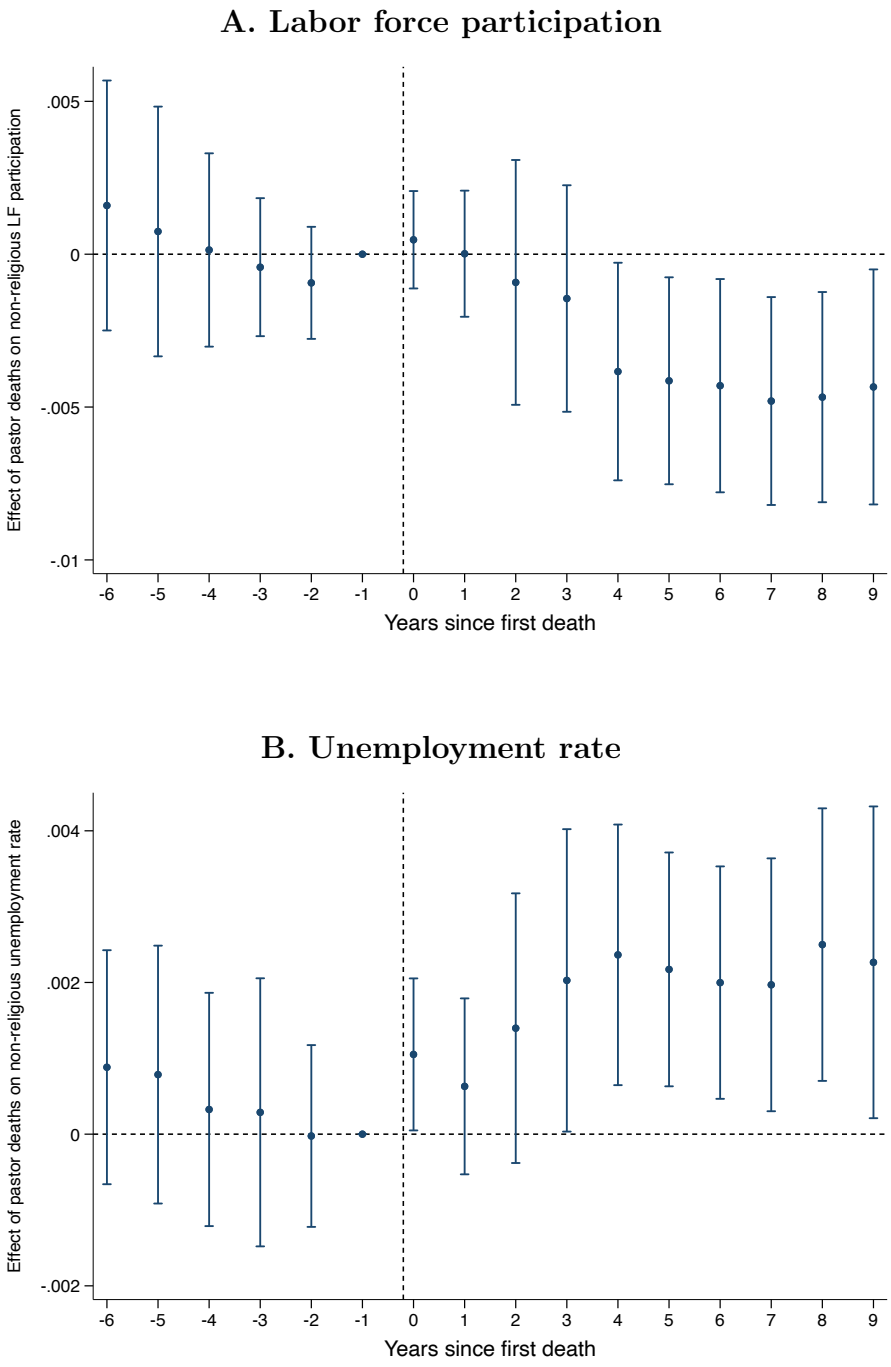
Notes: The above figure uses CES data to run an event study whose outcome variable is share of respondents reporting they identify as "Atheist" or "Agnostic". The regression is run using individual-level CES data with unit (e.g. treatment county or matched control county) fixed effects, year fixed effects, and state-by-year fixed effects. We include additional individual-level controls: race-by-year fixed effects, gender-by-year fixed effects, and age-bin-by-year fixed effects (10 year bins). The event study follows the specification given in Section 4.1 with matched control units constructed as described in Section 4. Coefficients and 95% confidence intervals are displayed; standard errors are clustered at the state level.

Figure A12: Effect of pastor convictions on church attendance (Non-Baptists)



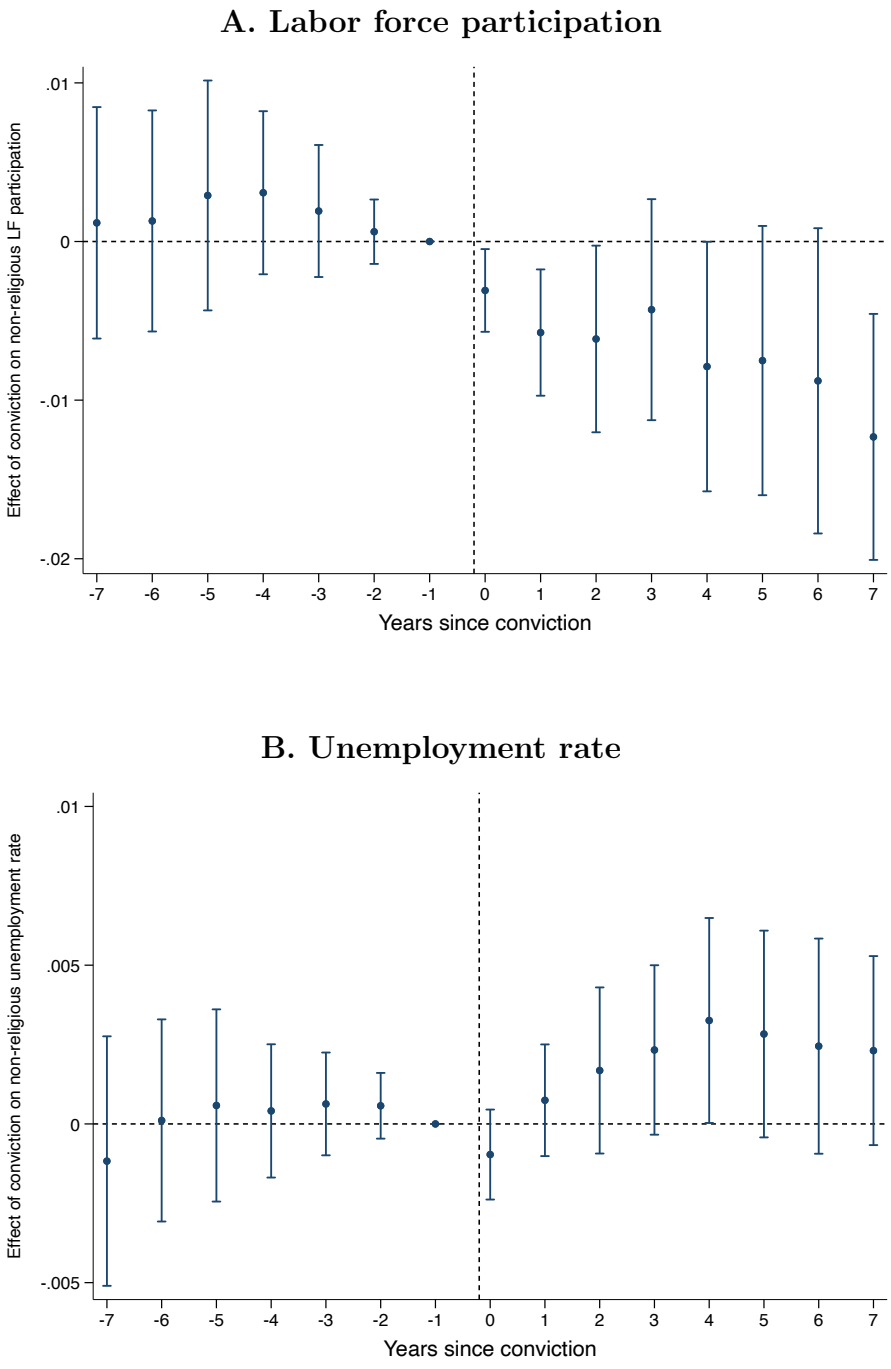
Notes: Data on church attendance from the Cooperative Election Study. Panel A displays raw trends and 95% confidence intervals for average weekly attendance among non-Baptists in treatment counties around the year of the first recorded pastor conviction. Panel B shows the analogous trends for non-Baptist attendance in matched control counties. Each specification uses CES survey weights in constructing annual average attendance rates.

Figure A13: Effect of pastor deaths on non-church employment outcomes



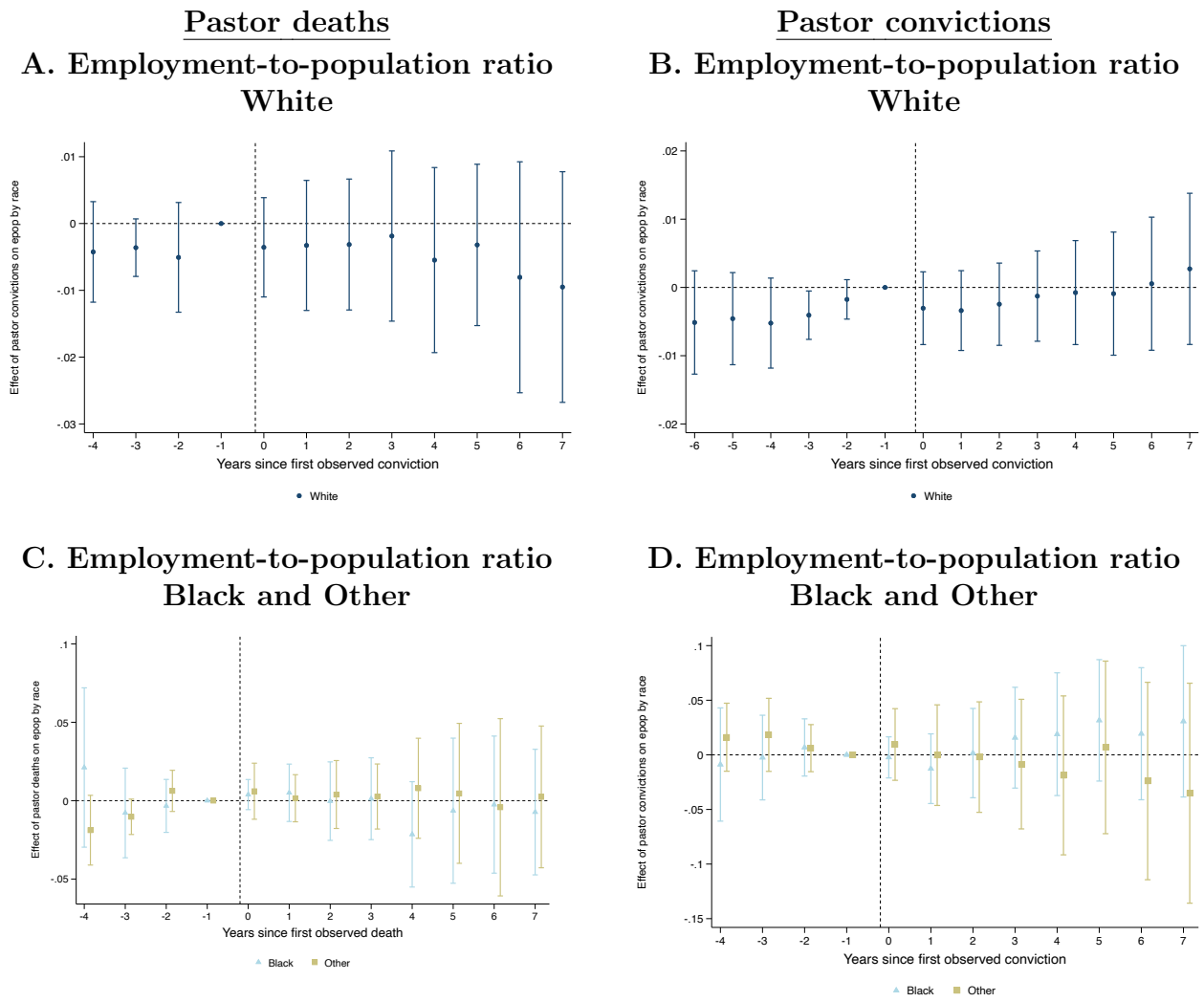
Notes: The above figures show the analogous results to Figure 7, subtracting off church-related employment using data from the County Business Patterns dataset.

Figure A14: Effect of pastor convictions on non-church employment outcomes



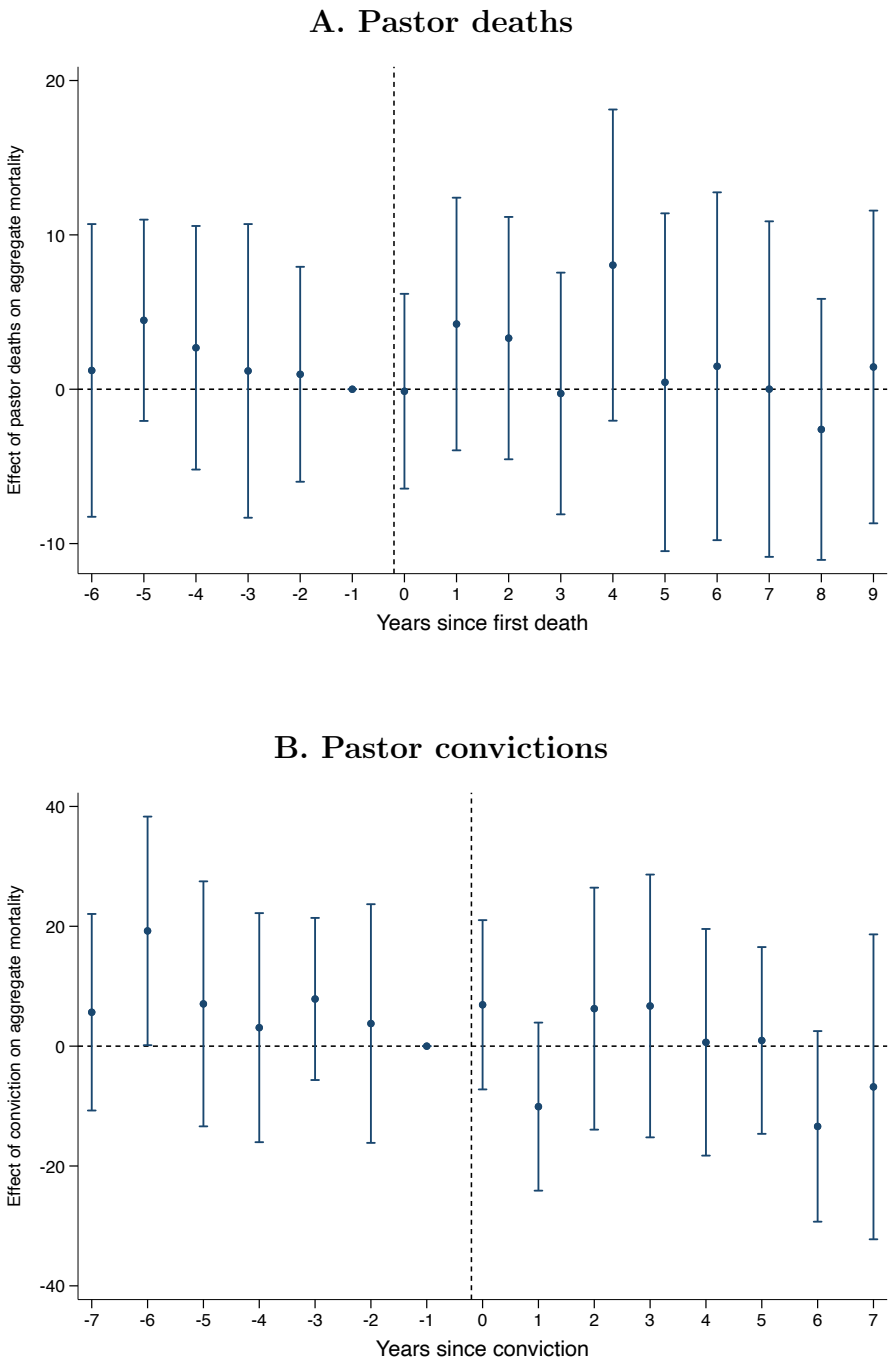
Notes: The above figures show the analogous results to Figure 8, subtracting off church-related employment using data from the County Business Patterns dataset.

Figure A15: Effects of pastor deaths and convictions on employment-to-population ratio by race



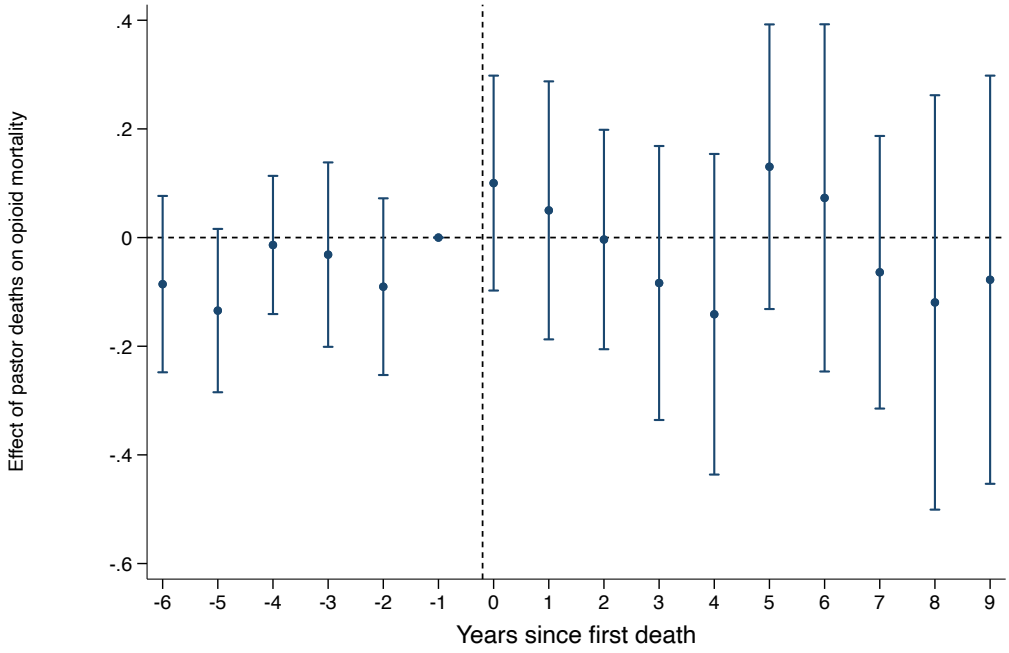
Notes: Employment data by race and by gender are from the Longitudinal Employer-Household Dynamics Quarterly Workforce Indicators. Regressions are constructed analogously to those reported in Figure 9, estimating regressions separately here by race instead of gender. Race is grouped into three categories: White, Black, and Other. Standard errors are clustered at the state level.

Figure A16: Effects of pastor deaths or conviction on all-cause mortality



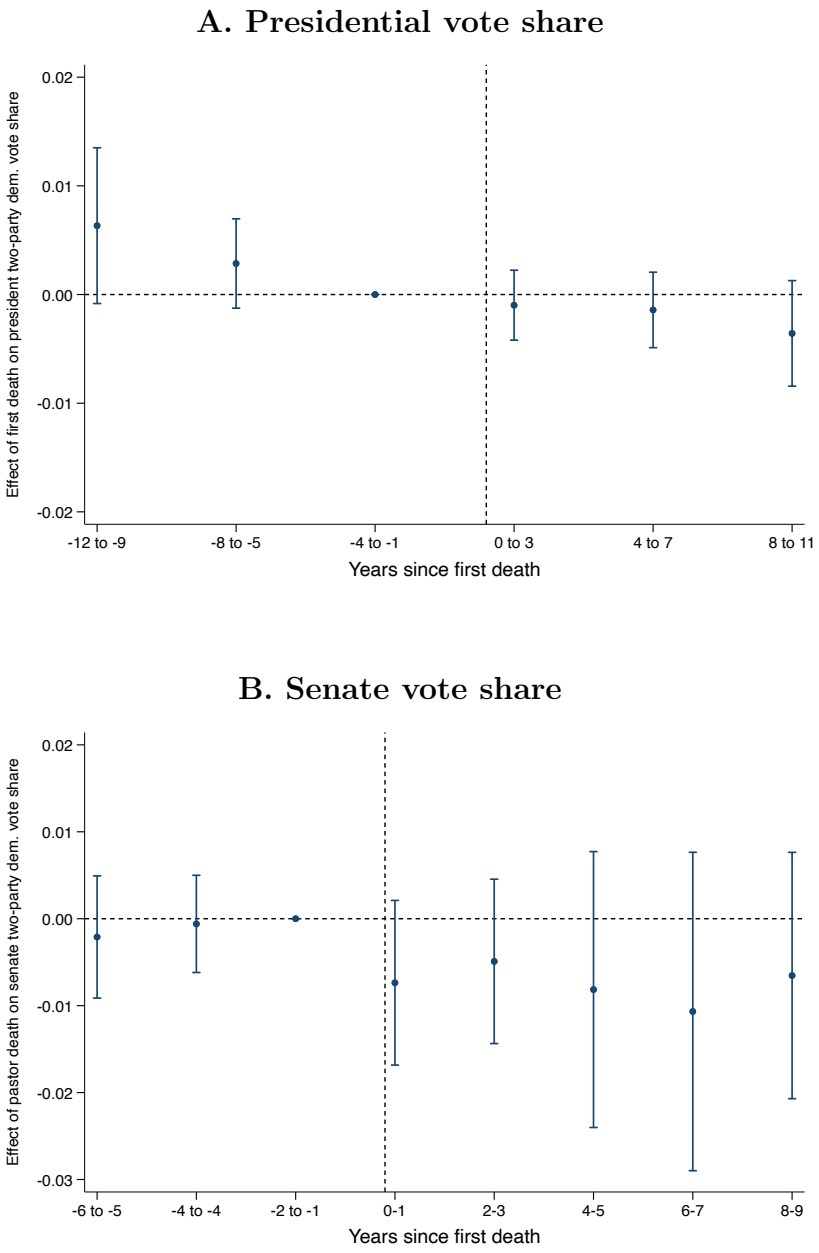
Notes: Panels plot event study coefficients for the effects of pastor shocks on county all-cause mortality, estimated using the matched controls design described in Section 4.1. Panel A uses the first recorded pastor death as the event; Panel B uses the first observed pastor conviction. The unit of observation is county-year; each treated county is matched to within-state nearest never-treated controls as described in Section 4. Each plot shows estimated event study coefficients and 95% confidence intervals. Regressions include match-group x county fixed effects, year fixed effects, and state-by-year fixed effects. Standard errors are clustered at the state level. All-cause deaths are from the National Center for Health Statistics' Multiple Cause of Death data, constructed using standard ICD-based definitions.

Figure A17: Effects of pastor deaths on opioid mortality



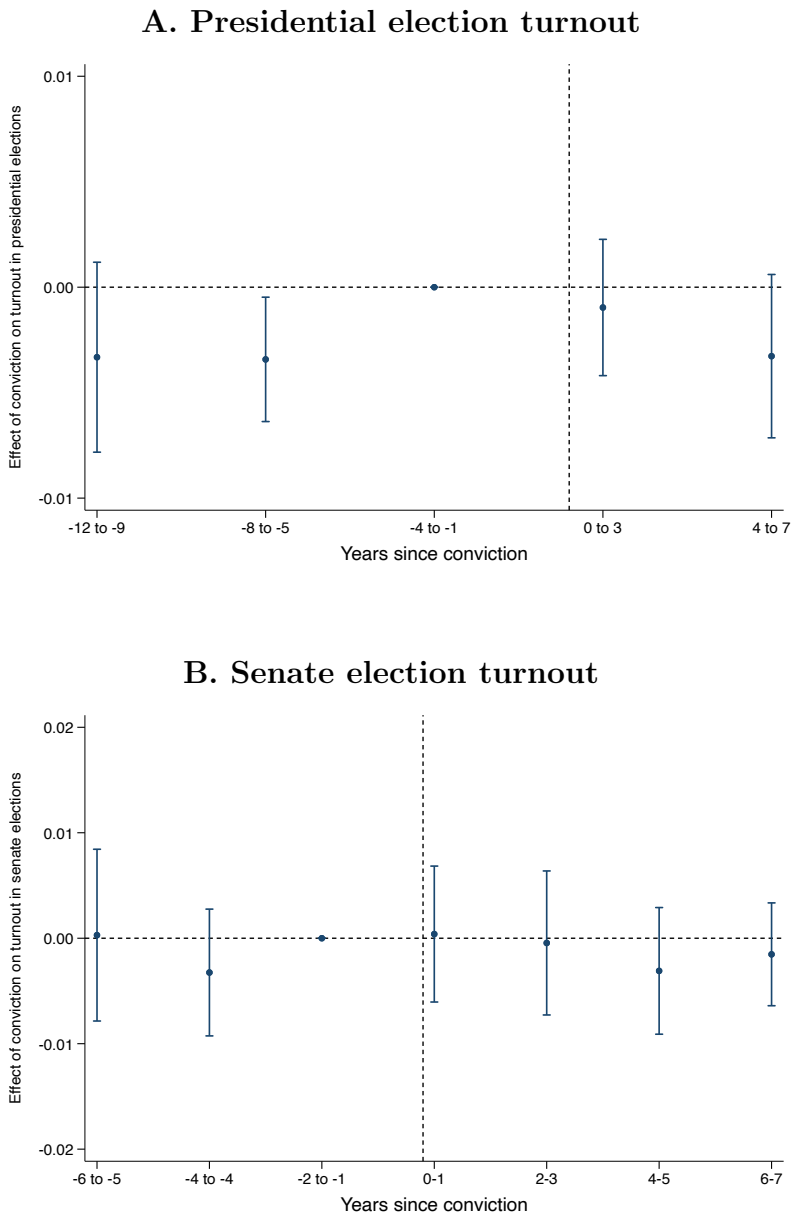
Notes: The above plot shows event study coefficients for the effect of the first observed pastor death on county opioid mortality, estimated using the matched controls design described in Section 4.1. The unit of observation is county-year; each treated county is matched to within-state nearest never-treated controls as described in Section 4. The plot shows estimated event study coefficients and 95% confidence intervals. The regression includes match-group x county fixed effects, year fixed effects, and state-by-year fixed effects. Standard errors are clustered at the state level. Opioid deaths are from the National Center for Health Statistics' Multiple Cause of Death data, constructed using standard ICD-based definitions.

Figure A18: Effect of pastor deaths on vote share



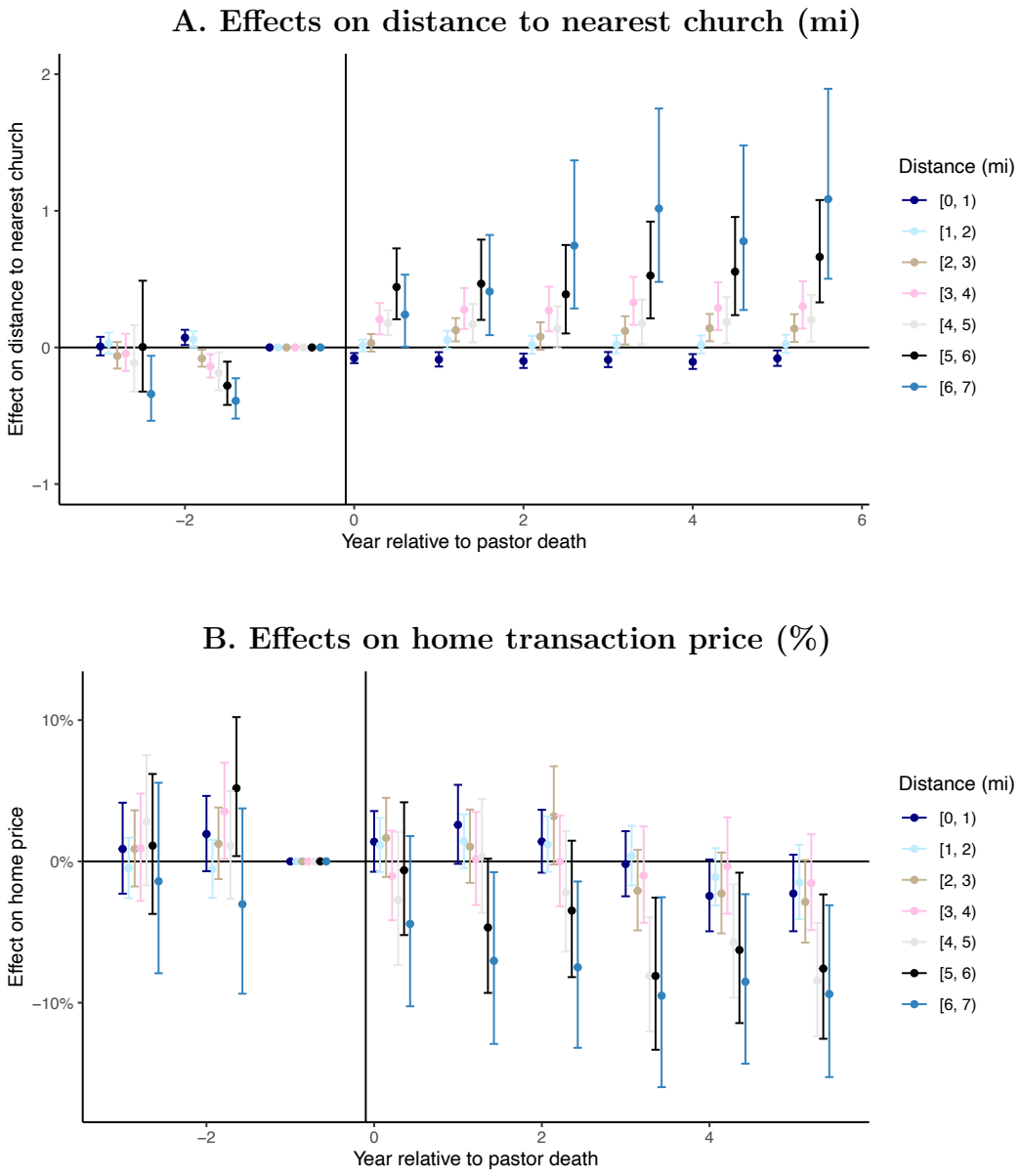
Notes: Panels plot event study coefficients for the effect of the first observed pastor death on county voting outcomes, estimated using the matched controls design described in Section 4.1. The unit of observation is county-year; each treated county is matched to within-state nearest never-treated controls as described in Section 4. Each plot shows estimated event study coefficients and 95% confidence intervals. Regressions include match-group x county fixed effects, year fixed effects, and state-by-year fixed effects. Standard errors are clustered at the state level. Electoral outcomes are two-party Democratic vote shares in presidential elections (Panel A) and U.S. Senate elections (Panel B), constructed from the ICPSR General Election Data and Dave Leip's Atlas of U.S. Elections.

Figure A19: Event studies of conviction effects on turnout



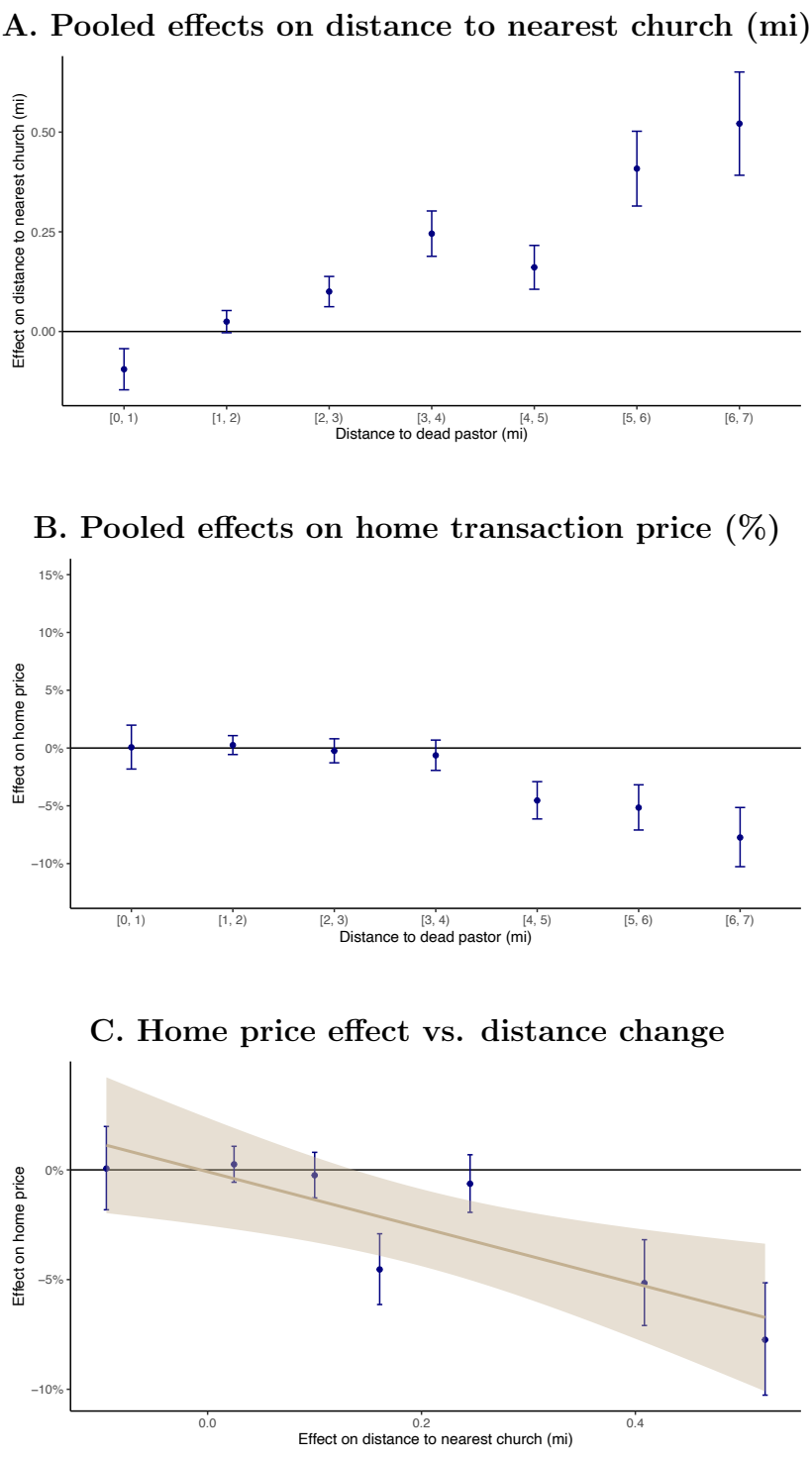
Notes: Panels plot event study coefficients for the effect of the first observed pastor conviction on county voting turnout, estimated using the matched controls design described in Section 4.1. The unit of observation is county-year; each treated county is matched to within-state nearest never-treated controls as described in Section 4. Each plot shows estimated event study coefficients and 95% confidence intervals. Regressions include match-group x county fixed effects, year fixed effects, and state-by-year fixed effects. Standard errors are clustered at the state level. Electoral outcomes are total turnout in Presidential elections (Panel A) and U.S. Senate elections (Panel B), constructed from the ICPSR General Election Data and Dave Leip's Atlas of U.S. Elections and annual county-level population data from the SEER database.

Figure A20: Event study of pastor death on proximity to church and home prices



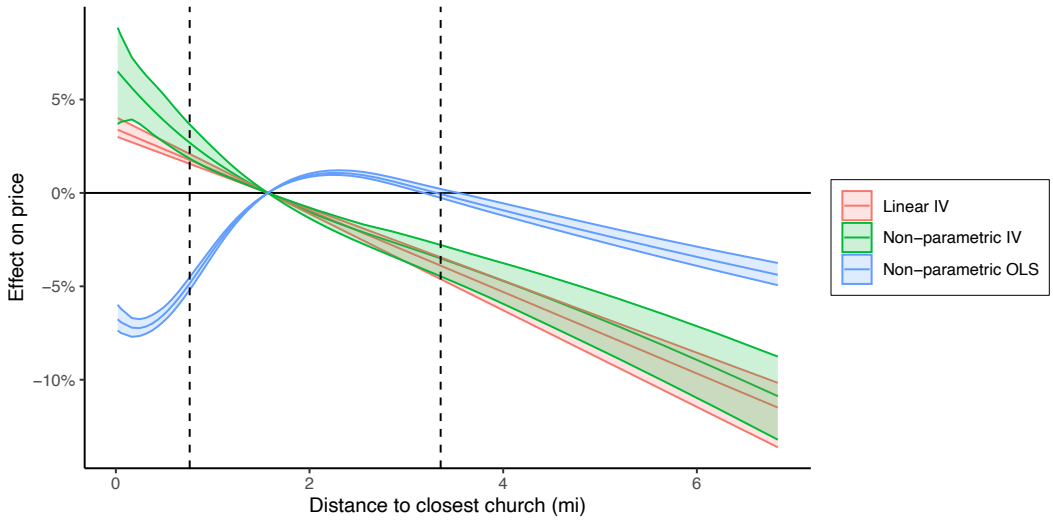
Notes: Event study estimated using a control group of houses in the same distance bin from a church in 1994 that have not yet been treated. Standard errors clustered at the stack × distance bin × treatment assignment level.

Figure A21: Pooled effects of pastor death on proximity to church and home prices



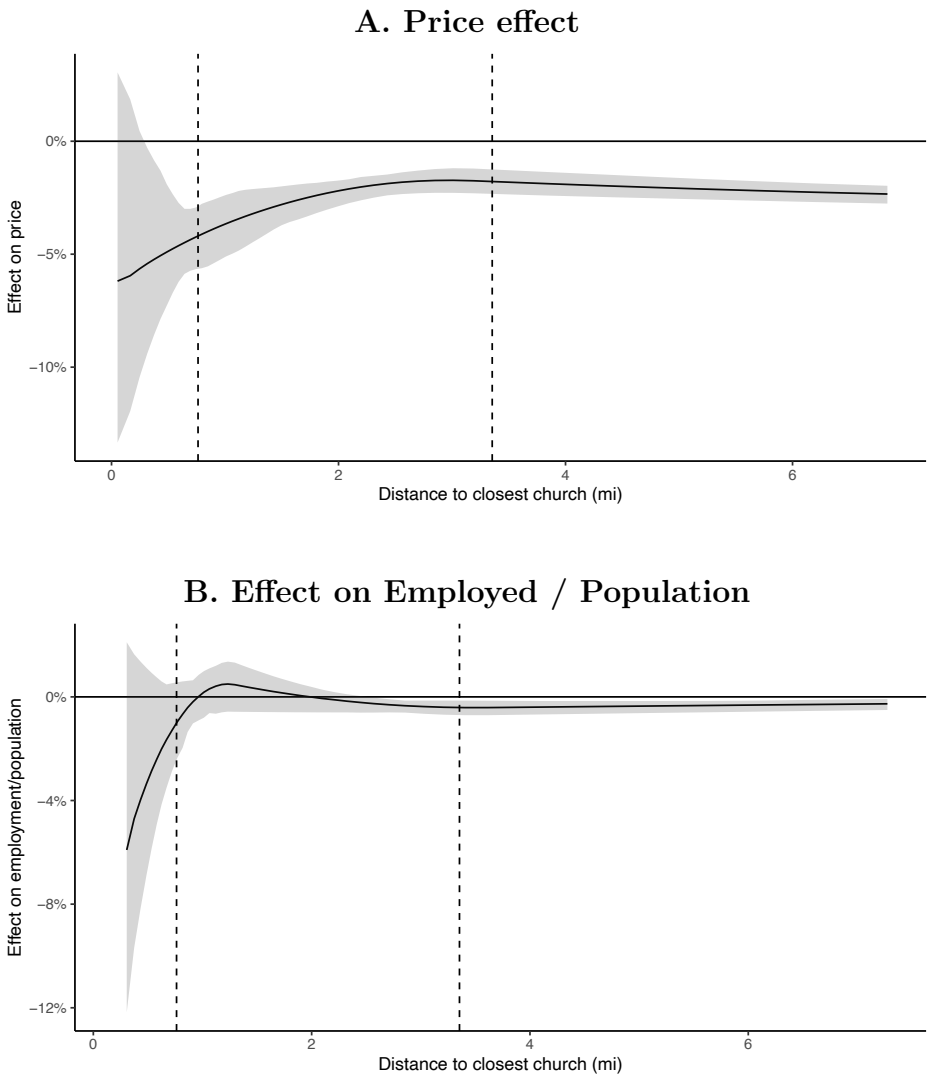
Notes: Event study estimated using a control group of houses in the same distance bin from a church in 1994 that have not yet been treated. Standard errors clustered at the stack × distance bin × treatment assignment level. Regression line in panel (C) compares the first and second stage effects, weighting by the inverse of the variance of the price effect estimate.

Figure A22: Effects on home price of distance from closest church



Notes: Point-wise confidence intervals calculated via 120 draws of a Bayesian bootstrap. In red we present the linear IV estimate, in blue we present the non-parametric OLS estimate, and in yellow we present the non-parametric IV estimate.

Figure A23: Slope of distance effects



Notes: Point-wise confidence intervals calculated via 120 draws of a Bayesian bootstrap.

B Appendix Tables

Table A1: Effect on social visits of pastor deaths

Dependent Variables:	**Church visits**	**Social visits**	**Bowling visits**
	(1)	(2)	(3)
Variables			
$\log(\frac{\text{PastorDead}_{zt}+0.1}{\text{Matched}_{z}+0.1})$	-0.031	-0.014	-0.173
	(0.014)	(0.006)	(0.067)
Fixed effects			
County	X	X	X
State × Year	X	X	X
Race × Year	X	X	X
Age group × Year	X	X	X
Sex × Year	X	X	X
City size × Year	X	X	X
Fit statistics			
Observations	20,650	17,768	19,678
Pseudo R^2	0.12	0.05	0.14

Notes: Data on the frequency of social visits per year from the DDB Life Style survey. Estimated as a Poisson regression. Social visits is defined as the sum of yearly instances of participating in, visiting, or attending a community project, bar, bowling alley, club meeting, dinner party, entertaining at home, cookout, dinner reservation, lunch reservation, golf, picnic, or sporting event.

Table A2: Difference-in-differences estimates for full set of outcomes

Outcome	Effect on outcome: Pooled DiD	Effect on outcome: Avg ES (all post)	Effect on outcome: Post (t+3 and later)	Control mean (pre)	% of control mean	Pre-trend p-value	N
Panel A: Pastor convictions analysis							
Economic outcomes							
LF participation rate	-0.0075** (0.0027)	-0.0070** (0.0030)	-0.0082** (0.0039)	0.478	-1.5%	0.429	5,812
Unemployment rate	0.0010 (0.0009)	0.0017* (0.0010)	0.0025* (0.0013)	0.067	2.5%	0.444	5,812
Empl-pop ratio	-0.0077** (0.0028)	-0.0075** (0.0030)	-0.0090** (0.0039)	0.447	-1.7%	0.416	5,812
Health outcomes							
Opioid mortality (age-adj)	1.3656** (0.5200)	1.7129** (0.7959)	1.8476* (0.9395)	5.703	30.0%	0.260	6,378
Overdose mortality (age-adj)	1.3649*** (0.4464)	1.2699** (0.5868)	1.3506* (0.6893)	11.833	10.7%	0.525	6,378
All-cause mortality (age-adj)	-7.0248** (2.7685)	-1.1028 (7.4388)	-2.3781 (8.6947)	903.226	-0.1%	0.066	6,378
Voting outcomes							
Pres. Dem vote share	0.0074** (0.0028)	0.0067** (0.0029)	0.0096** (0.0035)	0.399	1.7%	0.395	2,525
Senate Dem vote share	0.0059* (0.0033)	0.0054 (0.0038)	0.0104** (0.0043)	0.406	1.3%	0.046	2,664
Panel B: Pastor deaths analysis							
Economic outcomes							
LF participation rate	-0.0031* (0.0016)	-0.0029** (0.0014)	-0.0041** (0.0016)	0.465	-0.6%	0.455	79,844
Unemployment rate	0.0005 (0.0010)	0.0019*** (0.0007)	0.0024*** (0.0008)	0.066	2.9%	0.537	79,918
Empl-pop ratio	-0.0031* (0.0018)	-0.0035** (0.0014)	-0.0049*** (0.0017)	0.435	-0.8%	0.483	74,998
Health outcomes							
Opioid mortality (age-adj)	-0.1743 (0.4303)	-0.0136 (0.1158)	-0.0403 (0.1349)	0.404	-3.4%	0.469	396,980
Overdose mortality (age-adj)	-0.0256 (0.5493)	-0.1612 (0.1981)	-0.1928 (0.2232)	2.033	-7.9%	0.205	396,980
All-cause mortality (age-adj)	1.8748 (5.5320)	1.5960 (4.0252)	1.2218 (4.5380)	916.044	0.2%	0.455	396,980
Voting outcomes							
Pres. Dem vote share	-0.0048 (0.0030)	-0.0020 (0.0018)	-0.0025 (0.0021)	0.476	-0.4%	0.235	38,495
Senate Dem vote share	-0.0060 (0.0065)	-0.0075 (0.0063)	-0.0085 (0.0079)	0.568	-1.3%	0.689	49,400

Notes: "Pre-trend p-value" is from the joint F-test of the pre-period event-study coefficients (excluding the omitted period/bin). "Post (t+3 and later)" averages event-study coefficients from year t+3 onward (windows match the event-study figures). Control means are computed over control units in the pre-period within the estimation sample. The "% of control mean" column reports the Avg ES (all post) estimate as a share of the pre-period control mean. All specifications include unit (match group × county), year, and state-by-year fixed effects. Standard errors are clustered at the state level.