

Churches and Local Economies

Zane Kashner Max Pienkny*

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Abstract

Do local churches shape economic 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. We show that deaths translate into church closures and convictions cause large, lasting declines in Baptist attendance. These disruptions affect local economies, with labor-force participation falling, unemployment rising and nearby home prices declining for several years following both shocks. They also lead to changes in political and health behaviors, counties exposed to a conviction shift left in two-party vote share and experience higher opioid mortality. Taken together, the results document sizable local consequences of shocks to congregational life for economic, political, and health outcomes, underscoring the importance of churches as a determinant of place-based social capital.

*Contact: zkashner@stanford.edu, mpienkny@u.northwestern.edu. We are especially grateful to our advisors, Hunt Allcott, Rebecca Diamond, David Dranove, Matthew Gentzkow, Igal Hendel, Hannes Schwandt, Molly Schnell, and Shoshana Vasserman, for their guidance and support. Special thanks to Taffey Hall from the Southern Baptist Historical Library & Archives for sharing archival documents.

1 Introduction

Religious congregations are major local institutions in the United States. 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). Religion can affect individual and community-level outcomes in a multifaceted manner: first, it has long been argued that actual religious belief emphasizes characteristics—such as altruism, trust, and work ethic—that are associated with economic success (Weber, 1930). Second, religious observance is a strong correlate of overall 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). Uncovering the causal role of religion has been a difficult problem, despite its importance across many aspects of American life.¹

In this paper, we measure the local effects of religious access and belief on a variety of economic, social, and health outcomes. We exploit two complimentary plausibly exogenous shocks to the Southern Baptist Convention (SBC), the largest Protestant and second-largest Christian body in the United States. First, using newly-digitized data from church directories containing the universe of SBC pastors in the United States, we show that SBC pastor deaths precipitate church closures. Second, we study the consequences of pastor convictions for sexual abuse on local congregations and find immediate and persistent declines in church attendance. Using matched-controls event study analyses, we show that both of these shocks—pastor deaths leading to church closures and pastor convictions for sexual

¹For 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).

abuse—have appreciable effects on downstream outcomes. Both reduce labor force attachment: labor force participation declines and unemployment rises, and these effects persist more than five years following each shock. Convictions additionally shift voting patterns leftward and increase opioid mortality rates, while pastor deaths show no comparable effects. Finally, using data on the universe of home sales, we show that both shocks lead to substantial declines in home values, which are localized around the exact church subject to the shock. We formalize our home price analyses in a hedonic demand framework and show substantial willingness-to-pay to live closer to a church. Together, the evidence implies both sizable local value from church proximity and distinct mechanisms for the effects of each shock: church access and network erosion from deaths and a broader credibility and norms channel from convictions.

The Southern Baptist Convention is an evangelical Christian denomination that was formed in 1845, splitting from the mainline Baptist denomination over disputes over slavery, which the SBC wanted to uphold. In the years since, the SBC has grown to be the largest Baptist organization in world, and the second largest Christian organization in the United States, following the Catholic church. Membership in the United States peaked in 2006 at 16 million people, and has declined each year since, with only 12.7 million members reported as of 2024 ([Earls, 2025](#)). Unlike the Catholic church, the organizational structure of the SBC is highly decentralized, each church is “an autonomous local congregation” where “each congregation operates … through democratic processes” ([Southern Baptist Convention, 2000](#)). Due to this independence, there is no denominational control over local churches. The SBC does not assign pastors, recommend pastors to churches, or remove pastors from churches: all is decided by the local congregation ([Southern Baptist Convention, 2025](#)). The SBC’s decentralized nature has led to a proliferation of congregations—there are nearly 47,000 operating churches in the United States—as well as a diversity of practice styles and beliefs tailored to local congregations ([Sellers, 2004](#)).

This decentralized governance is critical for our identification strategies. Because there is no central authority to reassign pastors, consolidate resources, or immediately intervene after leadership shocks, pastor deaths can lead directly to church closures in ways that are plausibly unrelated to local economic conditions. Likewise, the absence of a centralized

investigative authority or media management means that pastor convictions for sexual abuse can generate genuine credibility shocks that reverberate locally without being smoothed over by denomination-wide intervention.² These institutional features help us isolate sharp local changes in both access to congregational networks and belief in local religious organizations.

Our empirical approach combines newly-digitized SBC administrative records and media reporting with local outcomes to estimate the dynamic effects of these two shocks. Using approximately biennial SBC directories, we construct a panel of the universe of congregations, pastoral assignments, and—after linking pastors to administrative death records—deaths over time. Convictions are assembled from the “Abuse of Faith” investigative reporting compiled by *The Houston Chronicle*. We link these shocks to county-level outcomes that measure church attendance, labor market attachment (labor force participation, unemployment), politics (two-party vote shares), health (opioid mortality), and housing (deed-level transactions), including standard sociodemographic controls. Identification in both analyses leverages the timing and geography of the shocks in the event studies, which we use to evaluate pre-trends and control flexibly for state-level trends in outcomes. For housing, we complement our panel analyses with a hedonic framework on the universe of home sales and a nonparametric instrumental variables (NPIV) strategy that instruments distance to nearest SBC church using pre-period distance interacted with the timing of pastor deaths, allowing us to recover willingness-to-pay (WTP) for church proximity ([Bajari and Benkard, 2005](#); [Newey and Powell, 2003](#)).

We demonstrate that pastor deaths operate as a shock to church access. To avoid endogeneity in which churches retain older pastors, we define a pastor death for this analysis to be the death of whomever was the pastor in 1970. Event studies show flat pre-trends in the stock of SBC churches within a zip code in the years prior to a pastor death, with a discrete decline following the first observed pastor death. The decline in local church availability persists

²On 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 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.” ([Southern Baptist Convention, 2025](#)).

years after the pastor death. Difference-in-difference estimates indicate a nearly three-to-one mapping between pastor death and church closure. This rate is nearly one-to-one when we consider only active pastors' deaths, consistent with a leadership-vacuum mechanism. In downstream outcomes, treated areas exhibit a sustained decrease in labor force participation of 0.3 percentage points and an increase in unemployment of 0.3 percentage points, with effects remaining significant nearly a decade following the observed death. We find no effects on voting patterns or opioid mortality around deaths, suggesting the primary mechanism is erosion of community networks rather than belief-driven shifts in political attitudes or risky health behaviors. In the housing market, transaction-level estimates show localized price declines particularly for houses for which pastor deaths substantially increase the distance to the nearest SBC church. The estimated hedonic model implies a positive WTP to live 1 mile closer to a SBC church of about \$3,235, with the majority of the valuation attributable to non-employment amenities rather than employment channels.

We additionally consider pastor convictions for sexual abuse as a shock to local church credibility. Following a conviction, church attendance for Baptists in the county of the conviction drops sharply and remain depressed for several years, with some evidence of real-location toward non-affiliation. Downstream labor market effects mirror the deaths analysis, with slightly larger magnitudes: labor force participation declines by 0.6 percentage points, while the unemployment rate increases by 0.4 percentage points, both of which persist long after the shock. We also find that convictions affect outcomes that are plausibly sensitive to changes in beliefs: Democratic vote shares in presidential and Senate elections increase by 1 percentage point in elections following convictions, and opioid mortality increases by 1.2 deaths per 100,000 residents, a 25.6 percent increase relative to baseline. Housing prices decline in tight radii around the implicated church, indicating capitalized disamenities. Taken together, we find similar labor market deterioration effects of pastor deaths and convictions. However, voting and health behaviors being affected only by convictions supports the interpretation that deaths primarily disrupt church access and community networks, whereas convictions additionally shift beliefs and norms with broader social and health consequences. This is consistent with the view espoused by McCleary and Barro (2006) that religious belief itself is a core driver of religion's effect on economic outcomes.

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 presented mixed evidence about effects and mechanisms: religious beliefs correlate with pro-market attitudes and growth in some settings (Guiso et al., 2003; McCleary and Barro, 2006), while historical work has argued the economic effects of religion are tied to differences in human capital accumulation (e.g. overall advantages from literacy gained from Bible study) or has found no effects at all (Becker and Woessmann, 2009; Cantoni, 2015). Closer to our approach, a series of other papers have used quasi-experimental or experimental approaches to study religion’s causal impact. Some focus on religious outcomes themselves, showing that institutional or environmental changes move participation: for example, Blue law repeals reduced church attendance and giving by increasing secular alternatives (Gruber and Hungerman, 2008), and the Catholic sex abuse scandal reduced Catholic participation and church-related charitable giving (Hungerman, 2013; Bottan and Perez-Truglia, 2015). Other work links exogenous changes in religious activity to shifts in beliefs, morals, and health and civic behaviors. Winning a lottery to obtain a visa to complete the Hajj pilgrimage increased religious observance, increased belief in equality, and led to more favorable attitudes towards women, while exogenous variation in Ramadan fasting intensity reduced short-run output while increasing subjective well-being, highlighting non-pecuniary channels (Clingingsmith et al., 2009; Campante and Yanagizawa-Drott, 2015).

A complementary literature links exogenous changes in religiosity to health, and, in particular, “deaths of despair” (Case and Deaton, 2021). Using state Blue Law repeals as shocks to religious practice, Giles et al. (2023) show that declines in attendance among middle-aged Whites precede and help explain rising deaths from suicide, poisonings, and alcoholic liver disease in the 1990s. By contrast, direct economic outcomes such as labor supply and earnings typically move little, if at all, in randomized settings over the horizons studied. Randomized evangelical outreach in the Philippines increased religious practice and some prosocial behaviors but produced limited changes in income (Bryan et al., 2021). Quasi-

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

experimental work often detects reallocation margins—e.g., time and charitable resources (Gruber and Hungerman, 2008)—or macro/sectoral production responses (Campante and Yanagizawa-Drott, 2015), rather than effects on local labor-market aggregates like labor-force participation or unemployment. We contribute to this literature by leveraging novel local variation—pastor deaths and pastor convictions—within a large, decentralized denomination and by tracing outcomes that are understudied in causal work, including labor-force participation, unemployment, and home prices, alongside religion, politics, and public health.

Our access results speak directly to network-based models of job search and information diffusion. Repeated contact and weak-tie referrals can move employment (Granovetter, 1973; Topa, 2001; Calvo-Armengol and Jackson, 2004) and operate through screening and recommendation margins (Pallais and Sands, 2016). Recent administrative evidence links cross-class connectedness to upward mobility and labor-market attachment (Chetty et al., 2022a,b, 2024). Congregations are natural venues for this kind of network formation; in our setting, pastor deaths that precipitate closures plausibly erode these ties, consistent with the sustained declines we find in labor-force participation and the increases in unemployment. The absence of detectable changes in mortality or voting after deaths further supports a network rather than norm channel at this margin. The analogous labor market effects of pastor convictions that decrease church attendance supports the view that this form of neighborhood social capital may deteriorate quickly.

We treat proximity to a church as a local amenity whose value is capitalized into prices, situating our hedonic analysis within the Rosen–Roback tradition (Rosen, 1974; Roback, 1982) and the empirical literature that sharpens identification using quasi-experimental shocks (Chay and Greenstone, 2005; Greenstone and Gallagher, 2008; Linden and Rockoff, 2008; Diamond and McQuade, 2019; Bartik et al., 2019; Kashner and Ross, 2025; and others). Our transaction data and IV/NPIV approach recover a willingness-to-pay for church proximity and show that reputational shocks are priced as disamenities in tight radii around implicated churches. We also connect to work on the determinants of amenities and neighborhood sorting (Diamond, 2016; Almagro and Dominguez-Iino, 2025): unlike schools or parks, religious amenities are endogenously supplied by autonomous congregations; using pastor deaths and convictions gives us plausibly exogenous, highly local variation to sepa-

rate amenity value from compositional changes.

The remainder of the paper is organized as follows. Section 2 provides institutional background about the Southern Baptist Convention; Section 3 introduces a conceptual model to contextualize results, and Section 4 describes the data used in this project. In Section 5 we describe the empirical strategies used and provides results on proximate religious outcomes which can be considered to be the first stages; Section 6 discusses results on labor outcomes; Section 7 discusses results on social and health outcomes. In Section 8 we show effects on housing markets and labor force participation by proximity to the shock; Section 9 introduces and estimates our hedonic model to estimate preferences for church access and credibility; and Section 10 concludes.

2 The Southern Baptist Convention

The Southern Baptist Convention 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 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 broader trends in religiosity within 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% to around 45%. At the same time the number *never* attending church rose from 4% to 14%. (Jones, 2021).

Doctrinally, the SBC’s touchstone is the Baptist Faith and Message (Southern Baptist Convention, 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” (Southern Baptist Convention, 2000). In congregational life, churches commonly “call” a pastor by congregational vote, elect deacons, and conduct business meetings where members

approve budgets, ministries, and major decisions. Because worship practices are decided at the church level, they are highly heterogeneous, with small churches most likely to remain traditional (Sellers, 2004).

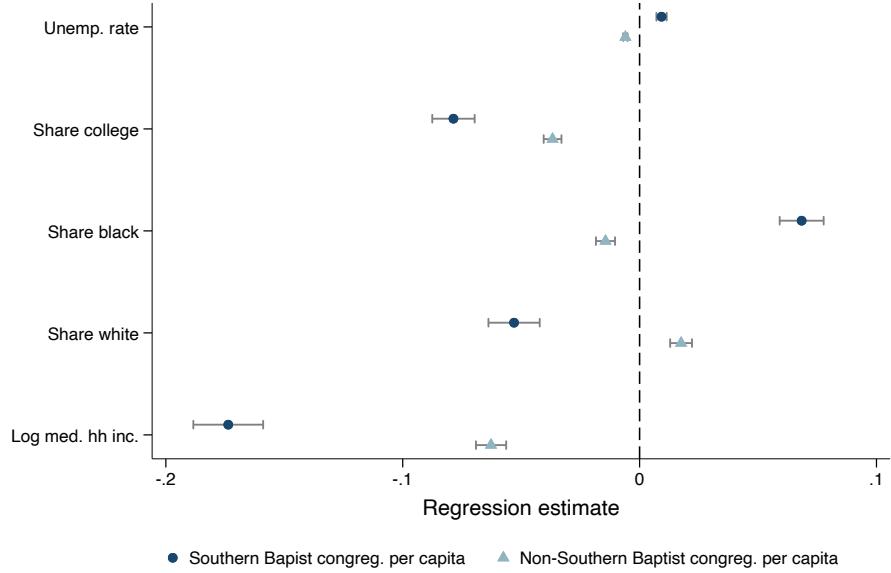
The SBC is congregationally governed. Each congregation is an autonomous nonprofit body that owns its property (generally via corporate title or trustees), hires and dismisses its pastors, and decides on doctrine and practice within the broad bounds of Baptist confessional statements. There is no centralized hierarchy within the SBC; it is up to the individual congregation to appoint clergy, allocate local assets, and discipline church members. Southern Baptists are within the so-called “charismatic Christian” tradition, which places a large emphasis on the individual role of the pastor. As shown in Appendix Figure A2, many churches include the name of their pastor 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 adults in the United States overall are Southern Baptists, its prominence is 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, over 50% of the population belong to the SBC.

1 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 per capita Southern Baptist congregations is associated with high 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.

Figure 1: Correlations between area characteristics and Southern Baptist churches

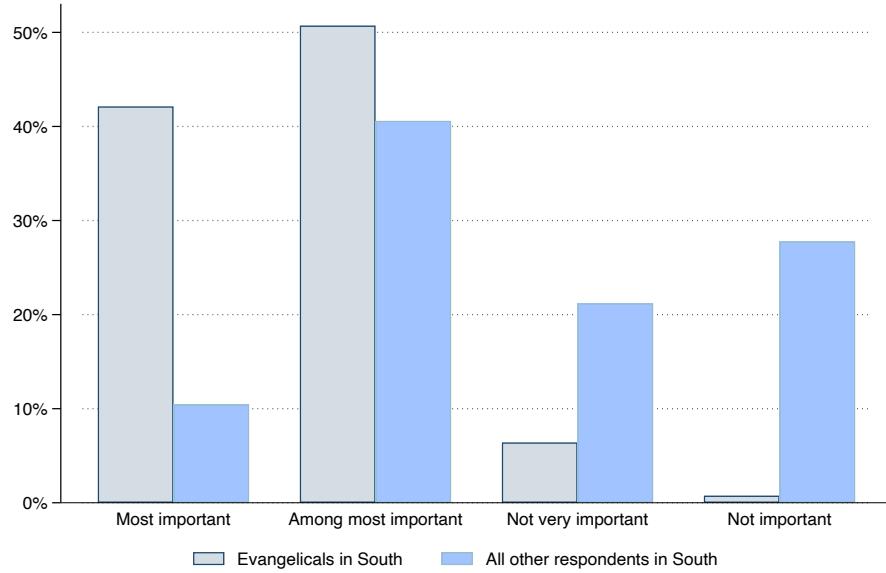


Notes: Data on number of congregations from the Census of Religions. Data on area characteristics from the Census and ACS.

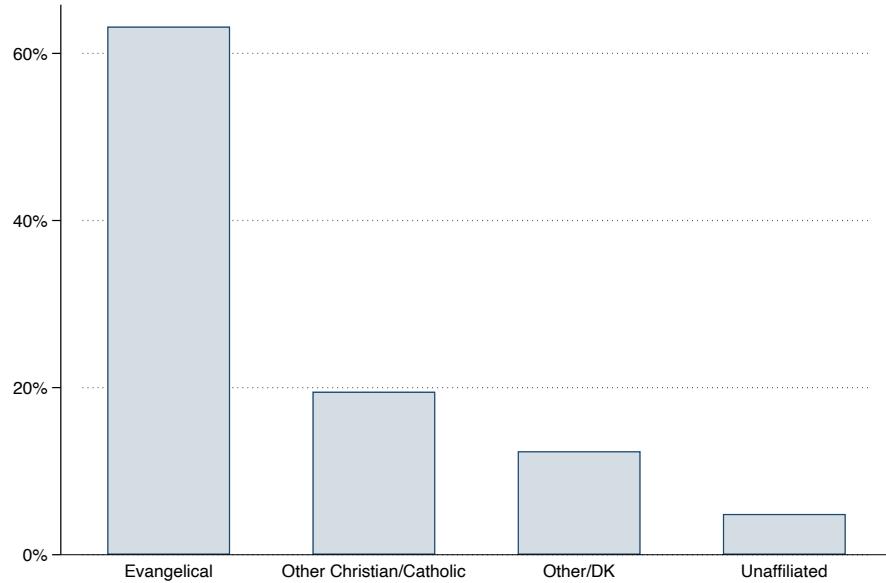
Despite the evident negative selection of Southern Baptist congregations, surveys indicate that for Southern Baptist adherents, religion is both highly salient and very socially embedded. Panel A of Figure 2 shows that among evangelicals living in the Southern United States (a population that overlaps 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 2 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 2: Survey reports of role of religion in life

A. Religious importance



B. Friend composition

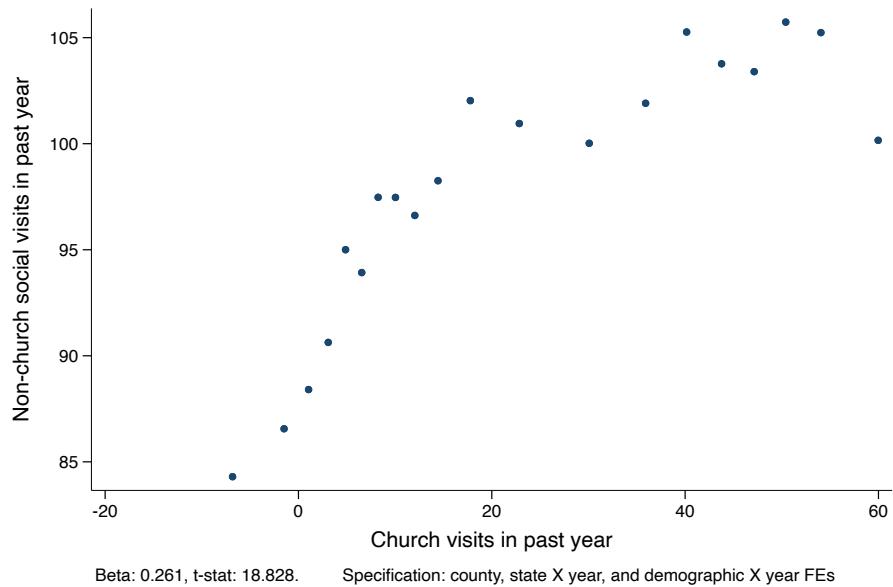


Notes: Data comes from the 2022 Public Religion Research Institute Social Network Survey.

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 3 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 with networks are religiously segregated as those among Southern Evangelicals) and to outcomes that depend on network density, like labor-market attachment (Granovetter, 1973).

Figure 3: Church attendance and social activity



Notes: Binned scatterplot of reported church attendance and the count of non-church social visits from the DDB Life Style Survey.

3 Conceptual Framework

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:

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

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 :

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

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 $l = \mathbb{I}\{\max_y U_i(y, 1) \geq \max_y U_i(y, 0)\}$.

The model generates a number of simple stylized predictions. First, since $\frac{\partial \mu}{\partial a} = -\frac{\sigma \beta}{(\sigma + m_0 + \beta a)^2} < 0$, a reduction in church attendance should raise the unemployment rate. The second is that since

$$\begin{aligned}\frac{\partial U_i(., 1)}{\partial a} &= \left(\frac{\partial \mu}{\partial a} (b - w) \right) + \alpha, \\ \frac{\partial U_i(., 0)}{\partial a} &= \alpha,\end{aligned}$$

and $b - w < 0$ then $\frac{\partial U_i(., 1)}{\partial a} > \frac{\partial U_i(., 0)}{\partial a} > 0$. Thus, $\frac{\partial l}{\partial a} > 0$. Finally, since both of these utilities are positive we note that $\frac{\partial U}{\partial a} > 0$. Thus higher attendance unambiguously increases labor force participation, consistent with the interpretation of churches as facilitators of social capital and job referrals. In a Rosen-Roback framework where marginal utilities are equalized across space, this channel further 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.⁴ The first-order condition for y is

$$\frac{\partial U}{\partial y} = \lambda - \phi(y + \rho(1 - l)),$$

which yields 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. Substituting y^* into i 's indirect utility gives

$$U_i(l) = \begin{cases} [(1 - \mu(a))w + \mu(a)b] + \alpha a + \frac{\lambda^2}{2\phi} & \text{if } l = 1, \\ R_i + \alpha a + \frac{\lambda^2}{2\phi} + \frac{\lambda\rho}{\phi} - \rho\left(\lambda - \frac{\rho}{2}\right) & \text{if } l = 0, \end{cases}$$

and differentiating shows that the relative payoff from labor market participation is increasing in ϕ . Formally, $\frac{\partial U_i(1)}{\partial \phi} - \frac{\partial U_i(0)}{\partial \phi} = \frac{\lambda\rho}{\phi^2} > 0$, so stronger belief raises labor force attachment and $\frac{\partial l}{\partial \phi} > 0$.

⁴Here, we parameterize religious belief as the costs of deviating from church values: the greater the religious belief, the greater the costs for making choices ynot in accordance with church values.

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 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.

4 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.

4.1 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 or 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 2013.⁵ 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.

4.2 Pastor convictions data

We use convictions of Southern Baptist pastors for sexual abuse as additional identifying variation, as we discuss further in Section 6.2. We compile our dataset from the *Houston Chronicle*’s “Abuse of Faith” database, which contains convictions of Southern Baptist pastors, employees, and volunteers of sexual abuse, as well as the county and year in which the offender was convicted. We subset our data to the 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 geocode the locations of these churches manually. When multiple convictions occur in the same county, we assign the earliest known conviction year as the relevant treatment time in all main analyses.

4.3 Proximate outcomes data

Southern Baptist church presence. From the digitized SBC Annuals described in 4.1, we construct a panel of open SBC congregations by year and location at the zip code level. For event-study analyses of church closures following pastor deaths, we aggregate this data to the zip-code-by-year level. For analyses of downstream outcomes, we aggregate to the county-by-year 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-

⁵We access a public use copy of Social Security Death Master File from SSDMF.INFO.

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. This data is what underlies much of Robert Putnam’s book *Bowling Alone*. Each year has $\sim 3,000$ respondents’ answers to 237 questions—which includes information on social connectedness, religious adherence, and general thermometer feelings. 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.

4.4 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 are able to measure a wealth of 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. Voting data used in this paper are presidential and Senate voting outcomes from 1964-2022 that come 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. These county-level data are used in event studies to observe changes in voting behaviors following either pastor deaths or pastor convictions.

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, lot size/acreage, building square footage, and bedroom/bath counts. We assign properties precise distances from the nearest SBC church, and decompose transaction price effects by distance for highly-localized localized capitalization analyses.

5 Empirical strategy and direct effects on religious outcomes

As shown in Figure 1 and Appendix Figure A3, counties where the SBC is prominent are distinct from the average county in the United States: the church is overwhelmingly concentrated in southern states, and SBC church presence has a strong negative correlation with a variety of socioeconomic area characteristics. Because of this, a simple comparison of counties that happen to experience either SBC pastor deaths or pastor convictions may differ in ways that would violate the parallel trends assumption of a standard difference-in-differences specification. To address this potential issue, we construct a matched-controls sample for each of our empirical strategies, exploiting the sharp timing of pastor deaths and convictions events with observably similar control counties. For each analysis, we construct our matched controls sample by finding the five counties located within the treated county’s state most similar to each treated county with respect to the following pre-period characteristics: SBC member share, log population density, median age, share White, high school graduation rate, median family income, and share in poverty. In all matched controls analyses, we sample from our donor pool with replacement, so the same county may appear as a matched control unit for multiple treated units. Using a matched-controls approach allows our sample of

control counties to much more closely resemble our treatment areas, more closely matching both overall presence of the SBC in each area, as well as matching the socioeconomic disadvantage that is correlated with SBC church presence.

A comparison of pre-period levels for various demographic and economic indicators for our treated sample, matched control sample, and overall “naïve” control sample for both identification strategies is shown in Table 1. In both designs, we see that treated counties have a substantial SBC presence: in the deaths sample, 24% of the population are SBC members, while 20.5% are in the convictions sample, whereas the universe of non-treated counties exhibits far less exposure (4.9% and 13.9%, respectively). By construction, the matched controls sample is much closer on this dimension, with about 20% of the population being SBC members across each design. The same pattern holds across other observables used in matching—population density, age structure, racial composition, education, income, and poverty—so that matched-controls reflect the demographic and economic profile of treated counties far better than the naïve pool. Importantly, alignment also improves for variables not used in matching (e.g., housing vacancies and unemployment). Overall, matching allows us to leverage the sharp timing of pastor deaths and convictions while comparing treated counties to observably similar places, strengthening the credibility of the event-study counterfactual.

In our deaths analyses, we define a pastor death as the death of the individual listed as a pastor in 1970, identified through a name match with the SSMDF-Infutor database. This method, as opposed to considering the deaths of active pastors, allows us to avoid endogeneity issues arising from primarily considering churches with older leadership, and thus a higher likelihood of treatment. Moreover, because treatment and control areas are matched on the 1970 SBC member share, they have a similar ex-ante likelihood of experiencing a pastor death.

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

	Pastor deaths analysis			Pastor convictions analysis		
	Treatment	Matched control	All controls	Treatment	Matched control	All controls
<i>Demographic</i>						
SBC member share	0.240	0.197	0.049	0.205	0.206	0.139
Log population density	3.964	3.376	3.081	4.832	4.407	3.571
Share white	0.807	0.768	0.909	0.838	0.846	0.873
Share black	0.143	0.184	0.040	0.117	0.109	0.084
Median age	28.736	29.426	29.973	33.253	33.271	33.867
Share male	0.489	0.489	0.495	0.489	0.490	0.491
<i>Education</i>						
Share HS graduate	0.247	0.240	0.314	0.312	0.319	0.344
Share HS dropout	0.219	0.230	0.150	0.122	0.126	0.109
<i>Economic</i>						
Median family income (\$1,000s)	7.121	6.559	7.805	32.629	30.468	28.267
Share under 18 in poverty	0.267	0.307	0.181	0.185	0.200	0.214
Unemployment rate	0.045	0.047	0.046	0.063	0.066	0.067
Share housing vacant	0.102	0.118	0.147	0.128	0.122	0.150
<i>N</i>	1411	6993	1722	87	439	3014

Notes: Table reports pre-period means for counties in the treatment group and two comparison samples (matched controls and all controls). The first three columns correspond to the pastor-death strategy; the final three columns correspond to the convictions strategy. Pre-period data for the pastor deaths analysis is taken from the 1970 Decennial Census and 1971 Churches and Church Membership in the United States dataset. Pre-period data for the pastor convictions analysis is taken from the 1990 Decennial Census and 1990 Churches and Church Membership in the United States dataset. N refers to the number of units included in the analysis (at the county-level). Since we allow for up to five matched counties per treatment county, the number of matched control units can be larger than the number of unique counties in the United States.

5.1 Pastor death shocks

For both of our identification strategies, we begin by estimating effects on proximate religious outcomes, akin to a first stage. For our analysis of pastor deaths, we estimate the effect of pastor deaths on church closures. Implementing the matched controls strategy outlined above at the zip code level, we estimate the following event study specification:

$$Y_{zgt} = \gamma_t + \delta_{zg} + \sum_{k \neq -1} \beta_k \text{Pastor Death}_z \times \mathbf{1}\{t - T_g = k\} + \lambda \mathbf{X}_{zt} + \theta_{s(z)t} + \varepsilon_{zgt}, \quad (3)$$

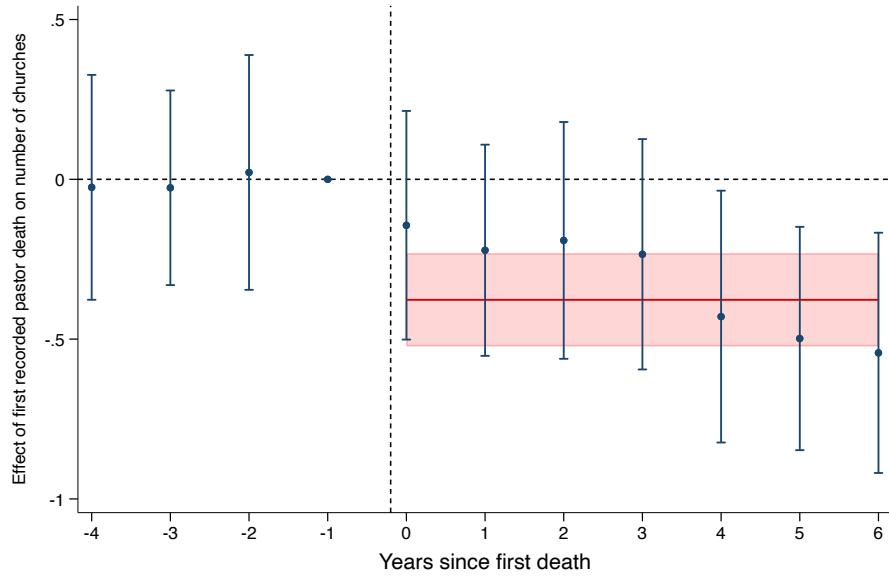
Where Y_{zgt} is the number of operating SBC churches in zip code z in year t in match

group g , $t - T_g$ is the time from first observed pastor death, β_k is the dynamic treatment effect of pastor deaths, δ_{zg} are zip code \times match group fixed effects, $\theta_{s(z)t}$ are state-by-year fixed effects, and \mathbf{X}_{zt} are time-varying controls. Standard errors are clustered at the zip code by group level.

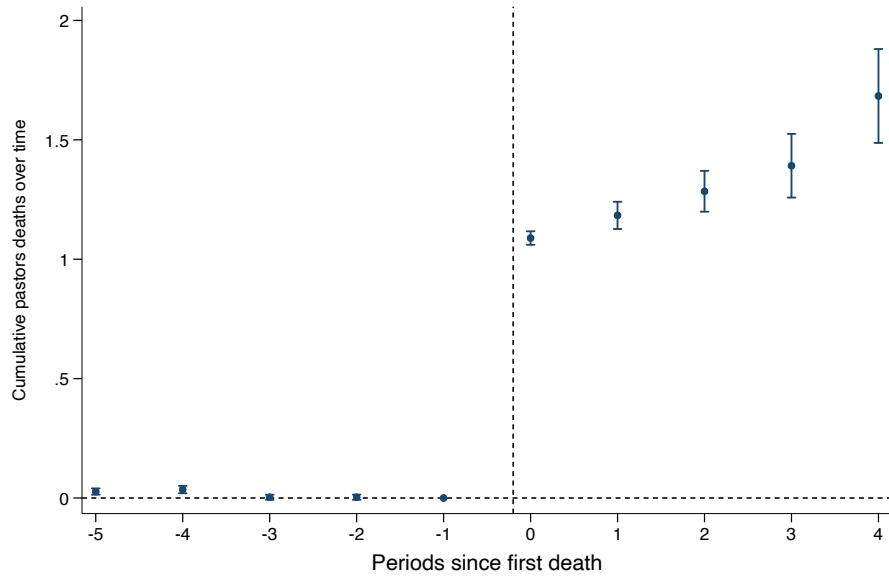
In Panel A of Figure 4 we present the effect of pastor deaths on the number of churches that are open in the relevant county. We find stable pre-trends before a sustained drop that is slightly smaller in magnitude than -0.5 , indicating a close mapping between pastor deaths and church closures. In Panel 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 A7, we present the effect of the sitting pastor passing away on the number of churches operating in a zip code. We find a nearly one-to-one mapping of pastor deaths to church closures in this sample. In Appendix Figure A8 we present an event study with a continuous non-absorbing treatment, which allows for identification from multiple deaths across time within a treated unit, and gives qualitatively similar results.

Figure 4: Effect of pastor death on church closures

A. Effect on number of churches



B. Number of pastor deaths



Notes: Data on church presence from the SBC Annuals. Event studies estimated at the county level using the matched controls method described in Section 5.

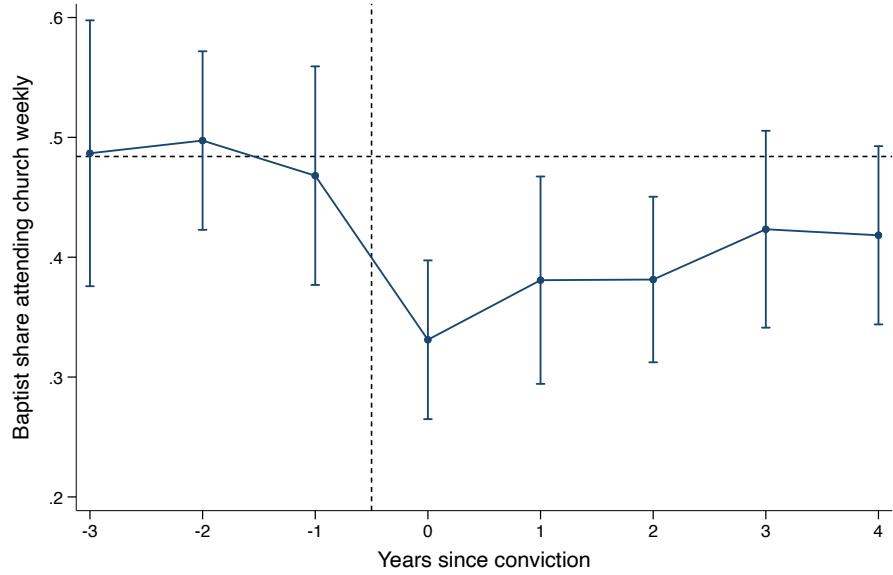
5.2 Pastor conviction shocks

Similar to the analysis of pastor deaths, we exploit both variation in treatment assignment and timing using a matched controls strategy to estimate the effects of pastors' convictions for sexual abuse crimes. Using individual-level survey data from the Cooperative Election Study (CES), which records detailed information about church attendance by denomination, we begin by showing raw trends in church attendance among Baptists in our treatment counties and matched control counties. The results are shown in Figure 5. Using weekly church attendance among Baptists as our outcome variable, we see an immediate decrease in weekly church attendance of nearly 20 percentage points immediately following a conviction, a decrease that persists in all later periods. Among Baptists in our matched control counties, we see similar levels of church attendance in the pre-period but no comparable decline: church attendance stays mostly constant throughout the entire sample period.

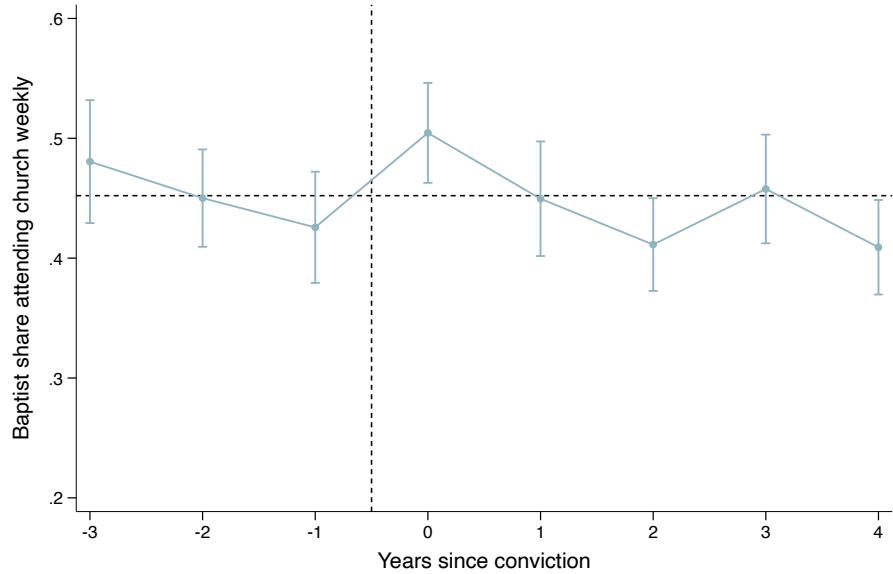
This effect is not driven by small reductions on the intensive margin (e.g. substituting to monthly attendance): we show in Appendix Figure A9 that following a pastor conviction, the share of Baptists attending church seldom or never more than doubles, rising from 8% in the pre-period to nearly 20% immediately following the shock, and stays elevated near this level through the end of our sample period. As a robustness check, we also explore church attendance in treatment and control counties among non-Baptists, who are not directly affected by SBC pastor convictions. As shown in Figure A10, we see no observable declines in church attendance among non-Baptists 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)

A. Treatment counties (Baptist)



A. Control counties (Baptist)



Notes: Data on church attendance from the Cooperative Election Study. Event studies estimated at the county level using the matched controls method described in Section 5.

Moving beyond raw trends to more easily control for individual- and area-level factors,

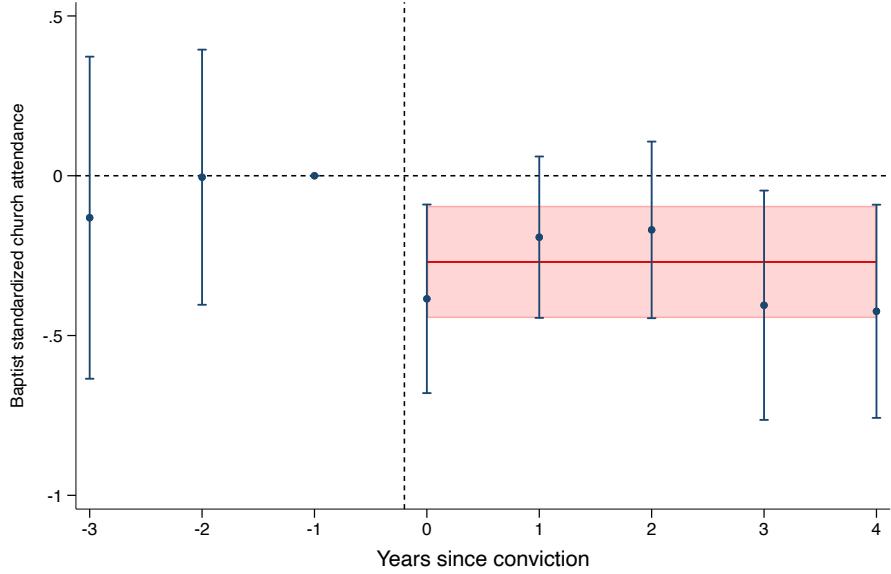
we also estimate the following event study specification:

$$Y_{igt} = \alpha + \delta_{c(i)g} + \gamma_t + \sum_{k \neq -1} \beta_k \text{Conviction}_{c(i)} \times \mathbf{1}\{t - T_g = k\} + \lambda \mathbf{X}_{it} + \theta_{s(i)t} + \varepsilon_{igt}, \quad (4)$$

where Y_{igt} is an outcome for individual i in match group g in year t , $t - T_g$ is the time in years from year of conviction T_g , β_k is the dynamic treatment effect of the pastor conviction, and as before $\theta_{s(z)t}$ are state-by-year fixed effects and \mathbf{X}_{it} are time-varying controls. Standard errors are clustered at the county-by-match group level.

In Figure 6 we present the estimated effect of pastor convictions on overall standardized church attendance among Baptists. We find an immediate decrease in attendance of nearly 0.5 standard deviations that, similar to what was shown in the raw trends, persists until the end of the study period.⁶

Figure 6: Effect of pastor convictions on church attendance



Notes: Data on church attendance from the Cooperative Election Study. Event studies estimated at the county level using the matched controls method described in Section 5.

⁶Given 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.

5.3 Comparing pastor shocks

While the prior results show substantial effects on proximate religious outcomes for both analyses, the relevant outcomes are distinct and come from different sources. 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 A6, 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* series (CCM), which covers the years 1952, 1971, 1980, 1990, 2000, 2010, and 2020. This dataset is the only source of local religious data 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.

Table 2 reports these first stage difference-in-differences results for our common outcomes. Following pastor deaths, SBC adherents fall by 9.79 per 1,000 from a baseline of 181 (a 5.41% decline), and SBC congregations decline by 0.023 per 1,000 from 0.704 (a 3.3% decline), and both effects are estimated precisely. After pastor convictions, points estimates move in the same direction. SBC adherents fall by 7.89 per 1,000 (a 4.52% decline) and SBC congregations decline by 0.032 per 1,000 (a 6.22% decline), though these effects are substantially less precise given the much smaller sample and decennial timing relative to observed conviction dates.⁷ Non-SBC adherents increase and non-SBC congregations decrease slightly, indicating a potential reallocation toward existing non-SBC churches rather than new church formation. Taken together, the CCM evidence 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

⁷Because 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.

due to better alignment between the data window and the timing of shocks.

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	-9.79	-0.023	7.90	-0.047
(Std. error)	(1.56)	(0.009)	(2.13)	(0.012)
Percent chg.	-5.41	-3.30	2.24	-3.13
Baseline mean	181.00	0.704	353.40	1.49
N	50,360	50,360	50,360	50,360
B. Pastor convictions				
Coefficient	-7.89	-0.032	11.26	-0.011
(Std. error)	(6.45)	(0.023)	(8.20)	(0.034)
Percent chg.	-4.52	-6.22	3.26	-0.87
Baseline mean	174.72	0.511	345.00	1.21
N	3,159	3,679	3,159	3,679

Notes: The above table shows difference-in-differences estimates for the effects of pastor deaths and pastor convictions on church activity. Church data comes from the near-decenrial "Churches and Church Membership in the United States" data from 1952, 1971, 1980, 1990, 2000, 2010, and 2020. All specifications include state-by-year fixed effects.

6 Effects on labor force outcomes

We now study whether the religious shocks documented in Section 5 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 on our matched controls samples defined in Section 5. Formally, for county c in match group g and year t , we estimate:

$$Y_{cgt} = \gamma_t + \alpha_{cg} + \sum_{k \neq -1} \beta_k \mathbf{1}\{t - T_g = k\} + \lambda \mathbf{X}_{ct} + \theta_{s(c)t} + \varepsilon_{cgt}, \quad (5)$$

Where $t - T_g$ is event time in years relative to the first observed shock in group g , $s(c)$ denotes state, and \mathbf{X}_{ct} refer to various time-varying controls, constructed by interacting pre-period county characteristics with year fixed effects. All variables are analogous to prior specifications, with only the unit of observation changing. Standard errors are clustered at

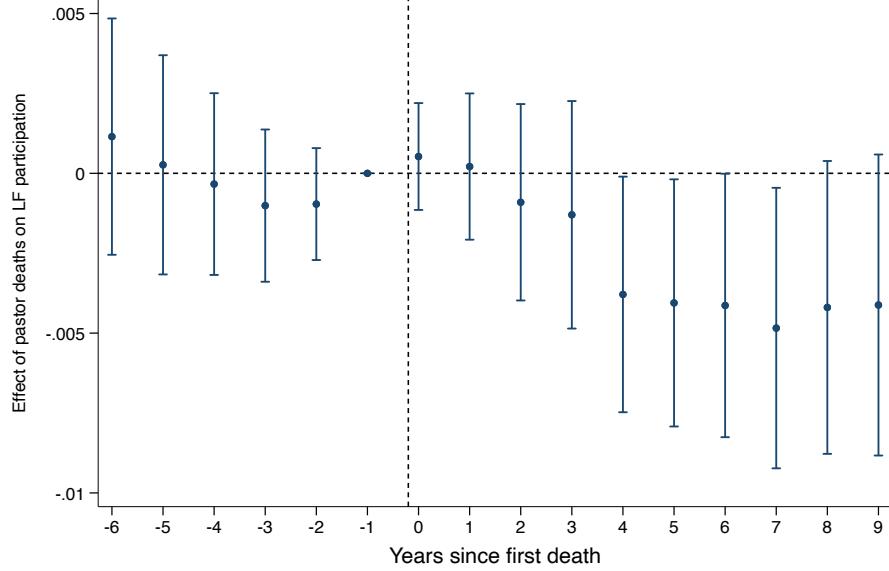
the county-by-group level. This specification applies to both the pastor deaths and pastor convictions analyses, with event time zero referring to the first observed occurrence in each treated county of each respective shock in our sample.

6.1 Effects of pastor deaths

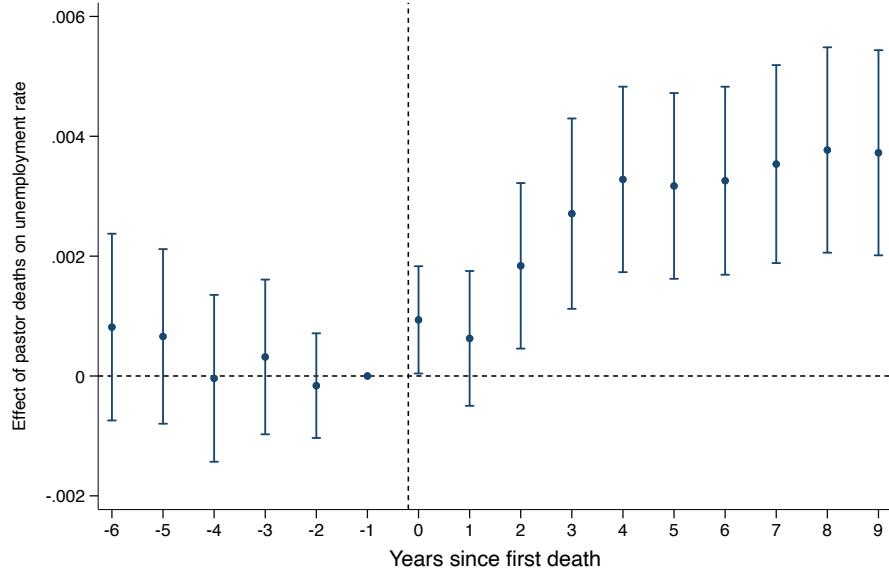
We anchor event time at the first pastor death associated with an SBC congregation in the county, starting from the universe of SBC pastors in the United States from the 1970 Southern Baptist Annual Directory (the earliest directory year in our study). When multiple deaths occur, our specification treats 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, map nearly 1:1 to 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 0.5 percentage points while unemployment grows by 0.4 percentage points—but are statistically significant and highly persistent. In Appendix Figure A11, 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

A. Labor force participation



B. Unemployment rate



Notes: 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. Event studies are estimated with the matched controls method as described in Section 5.

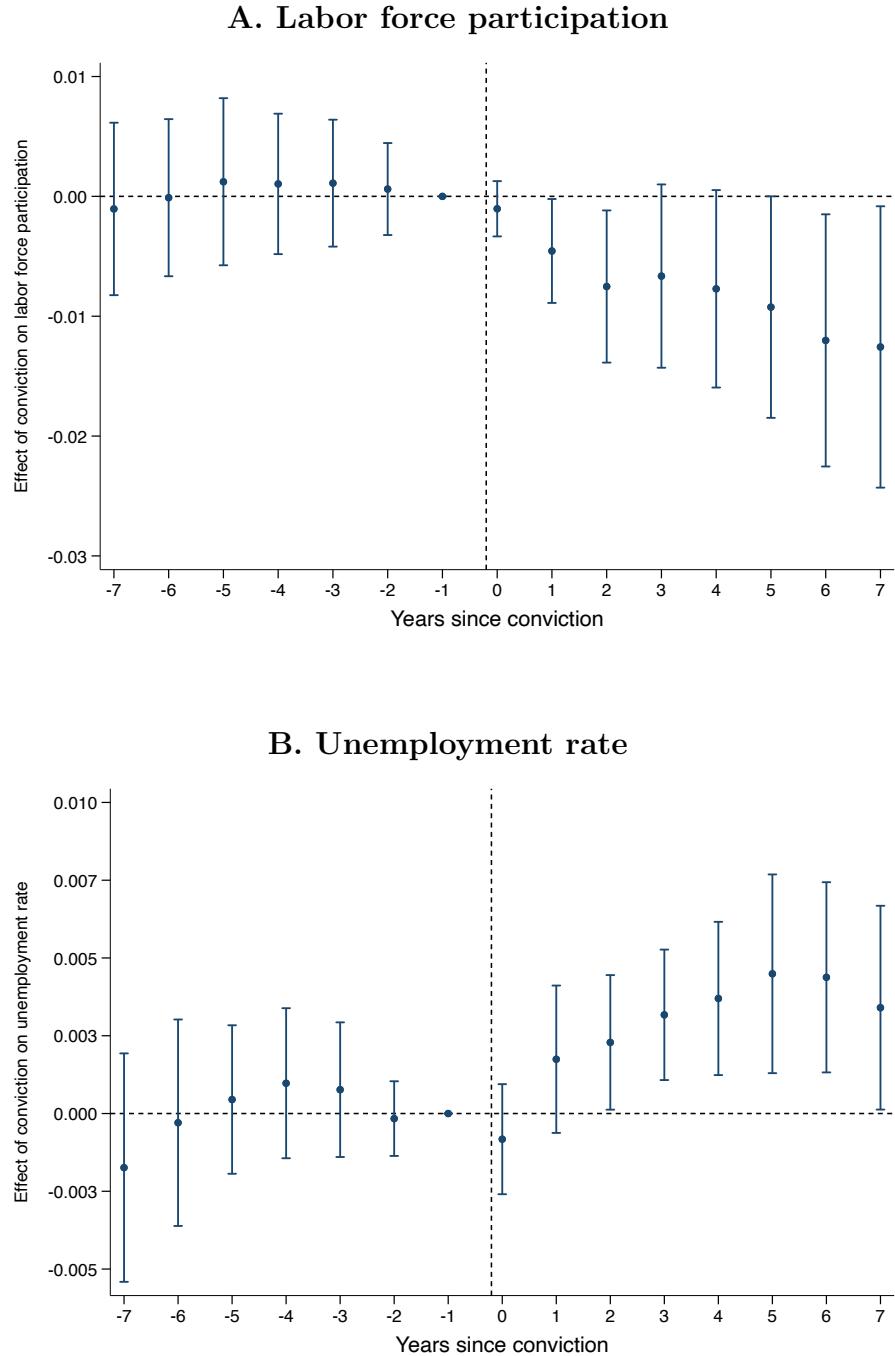
6.2 Effects of pastor convictions

Similar to our analysis of pastor deaths, for convictions, we also anchor event time at the first conviction recorded in the county. Because convictions produce large, localized declines in Baptist attendance (Section 5.2), this treatment effect captures both network-access losses (fewer church interactions), and also social norm shocks (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 unemployment. 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 unemployment are very similar to the estimated unemployment effects of pastor deaths, and for LFP, we see a decline on the order of two times larger. This is consistent with our model in Section 3, 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 5.2, indicating a tight first-stage link. As before, results are robust to the removal of church employment: these series are shown in Appendix Figure A12.

Across two distinct sources of plausibly exogenous variation, we find persistent deterioration in local labor market attachment: both strategies indicate decreases in LFP and increases in unemployment 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.

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



Notes: 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. Event studies are estimated with the matched controls method as described in Section 5.

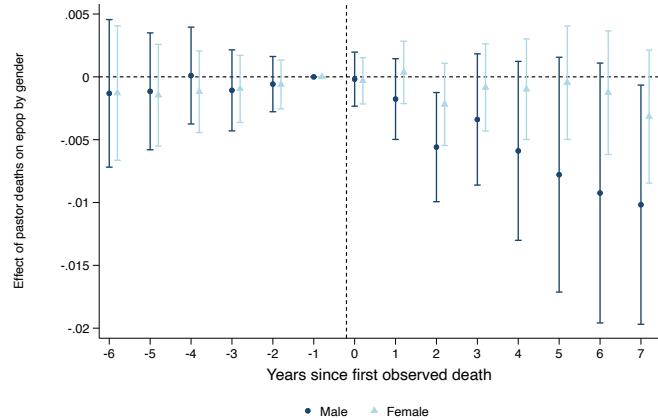
6.3 Heterogeneity of effects

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. Panels B and C show that there is additional racial heterogeneity: employment declines are concentrated entirely in White residents, consistent with the SBC’s local composition in connecting working-age White men to jobs in small, high-SBHC counties with limited substitution. These patterns mirror our earlier employment results estimated from the LAUS and further indicate that deaths reduce not only residence-based employment data, but also establishment payroll jobs in treated labor markets.

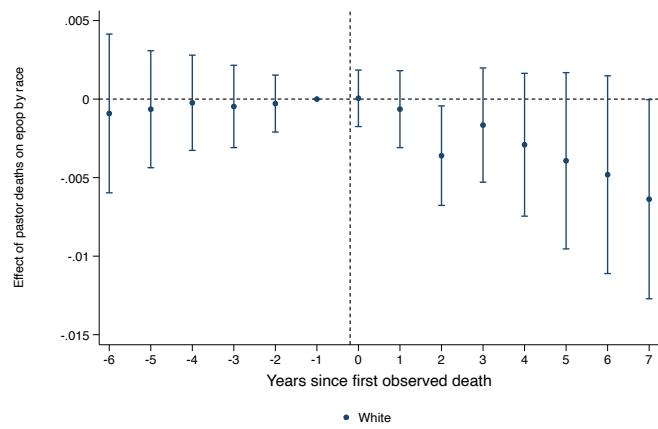
By contrast, we show in Appendix Figure A13 that QWI employment after pastor convictions is flat by gender and race, even though LAUS results show weaker labor market attachment. This divergence follows naturally from differences in both data coverage and the shocks themselves. LAUS measures employment of county residents, while QWI captures jobs located in the county. Convictions appear to discourage resident participation without reducing local labor demand; establishments can backfill vacancies with in-commuters or non-Baptist workers. Moreover, the conviction sample covers areas that are roughly 230 percent more densely populated than those in the deaths analysis (see Table 1), making the margin of hiring in-commuters less costly.

Figure 9: Heterogeneity in effects of pastor deaths

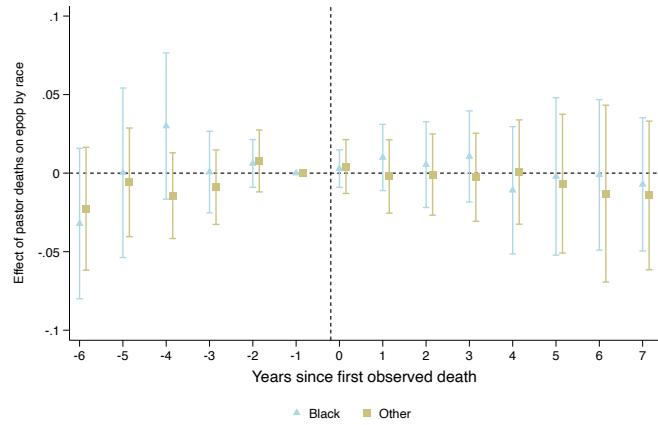
A. Employment-to-population ratio, male vs. female



B. Employment-to-population ratio, White



C. Employment-to-population rate, Black and Other



Notes: Employment data by race and by gender are from the Longitudinal Employer-Household Dynamics Quarterly Workforce Indicators. Event studies are estimated with the matched controls method as described in Section 5.

7 Effects on social and health outcomes

We next examine whether shocks to congregational life affect to social behavior, political participation, and mortality. Guided by the framework outlined in Section 3, where congregations supply both social opportunities and norms, we treat pastor deaths as primarily reducing access to congregational networks and pastor convictions as potentially also altering perceived norms. In Section 7.1 we present results for the effects of pastor deaths, and in Section 7.2 we present the effects of pastor convictions for sexual abuse.

7.1 Effect of pastor deaths

We begin with social engagement. As shown in Figure 3, 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 underlying 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 was well as other time-varying demographic controls. Table 3 shows that following deaths, church visits decline, and so 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 5 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 (e.g. Giles et al. (2023)). We find no effects on all-cause mortality (Panel A of Appendix Figure A14), no effects on opioid mortality (Appendix Figure A15), and no effects on Democratic vote shares (Appendix Figure A16), 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 3 in which pastor deaths mainly operate through an attendance channel rather than affecting norms or utility from alignment with the positions of the church.

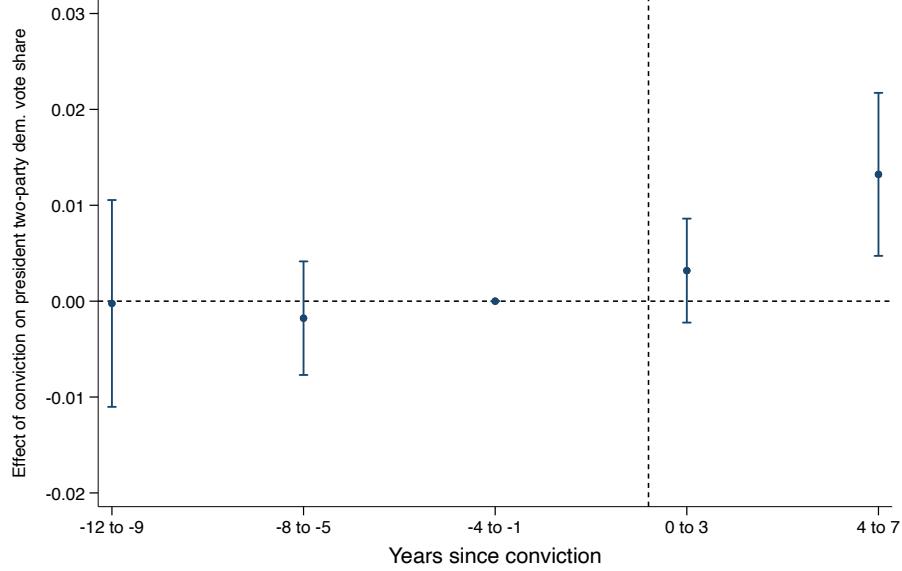
7.2 Effect of pastor convictions

Section 5.2 showed that convictions are followed by large and persistent declines in Baptist weekly attendance. We now examine whether these shocks coincide with broader changes in politics and health. Conceptually (Section 3), convictions plausibly 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 same outcomes as described in Section 7.1.

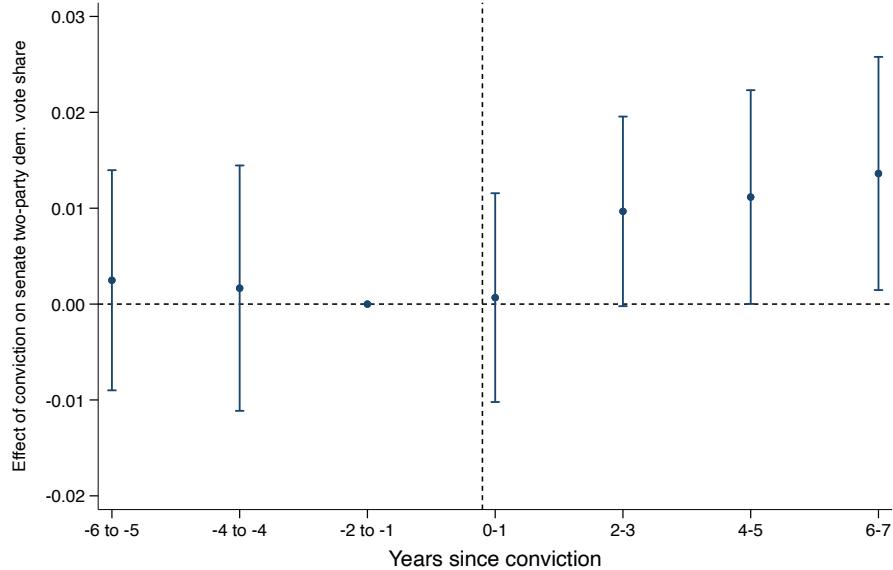
Figure 10 plots event study results for the effects of convictions on voting Democratic in elections; Panel A shows effects of two-party presidential vote shares, while Panel B shows effects on Senate vote shares. In both cases, we see no evidence of pre-trends and a sustained leftward shift of about 1 percentage point in both offices following the first observed conviction. The timing, which appears in the first election following a conviction and persists in later elections, suggests durable, 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 elections (Appendix Figure A17). 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, aligning with the idea that convictions add a norms component beyond a pure access channel.

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

A. Democratic presidential vote share



B. Democratic Senate vote share

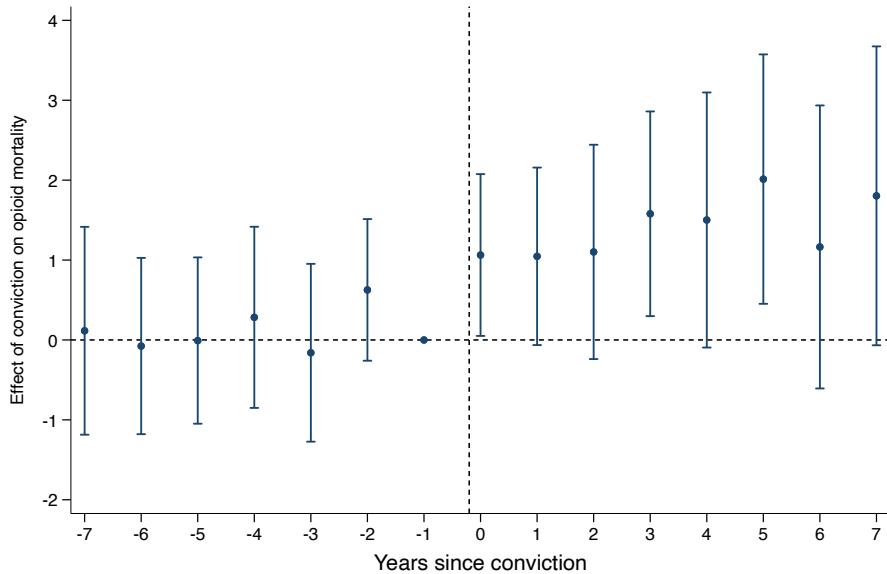


Notes: Electoral outcome data is from the ICPSR's General Election Data and Dave Leip's Atlas of U.S. Elections. Event studies are estimated with the matched controls method as described in Section 5.

Turning to mortality, Figure 11 shows clear and lasting increases in age-adjusted opioid mortality following convictions. The difference-in-differences estimate is 0.97 deaths per

100,000 people relative to a baseline mean of 4.77 deaths per 100,000 indicating a larger than 20 percent increase in opioid mortality. Dynamics are stronger at the peak: the event study maximum coefficient is 1.63 deaths per 100,000, or about 34 percent of baseline, and this effect remains elevated over several years. By contrast, all-cause mortality does not change (Panel B of Appendix Figure A14), 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: Opioid deaths are from the National Center for Health Statistics' Multiple Cause of Death data using standard ICD-based definitions. Event studies are estimated with the matched controls method as described in Section 5.

Taken together, convictions produce political and opioid mortality responses on top of 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.

8 Local geography of effects

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 are capitalized into local housing markets.

The intuition is straightforward. 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.

We proceed in three steps. Section 8.1 estimates how pastor deaths affect home prices in nearby neighborhoods. Section 8.2 turns to pastor convictions, documenting the disamenity value associated with proximity to implicated churches. Section 8.3 then implements a nonparametric IV strategy to recover the causal effect of church proximity on both housing prices and employment per capita.

8.1 Effects of pastor deaths

We begin by examining how pastor deaths affect local 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 an increase in 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 nearest church loses its pastor 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:

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

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:

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

Here, p_{it} is the transaction price, X_{it} are logged home characteristics (lot size, 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, β_k^d 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.⁸ To alleviate these concerns, the treatment effects β_k^d 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 that

⁸Furthermore, 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.

were already farther from a congregation (Panel A of Appendix Figure A18). This pattern is consistent with the geometry of church networks: in sparser areas the gap between the nearest and second-nearest church is larger, so the loss of the closest congregation produces a bigger increase in effective distance. Panel B of Appendix Figure A18 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 A19 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 effective 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 A19) suggest that home price effects are driven by changes in access to church, rather than consequences of the pastor's death itself.

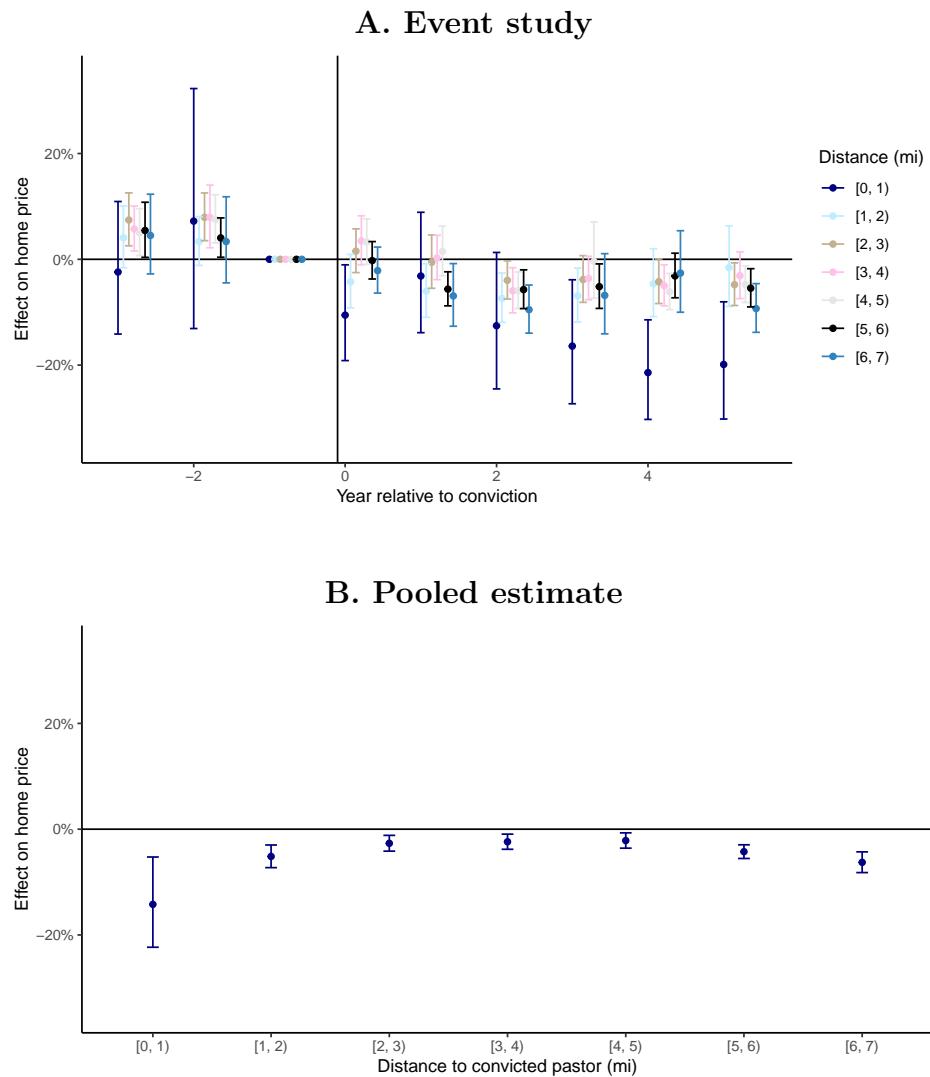
8.2 Effects of pastor convictions

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 that will be 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 es-

timated 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 \times distance bin \times treatment assignment level.

8.3 Effect of distance to church

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:

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

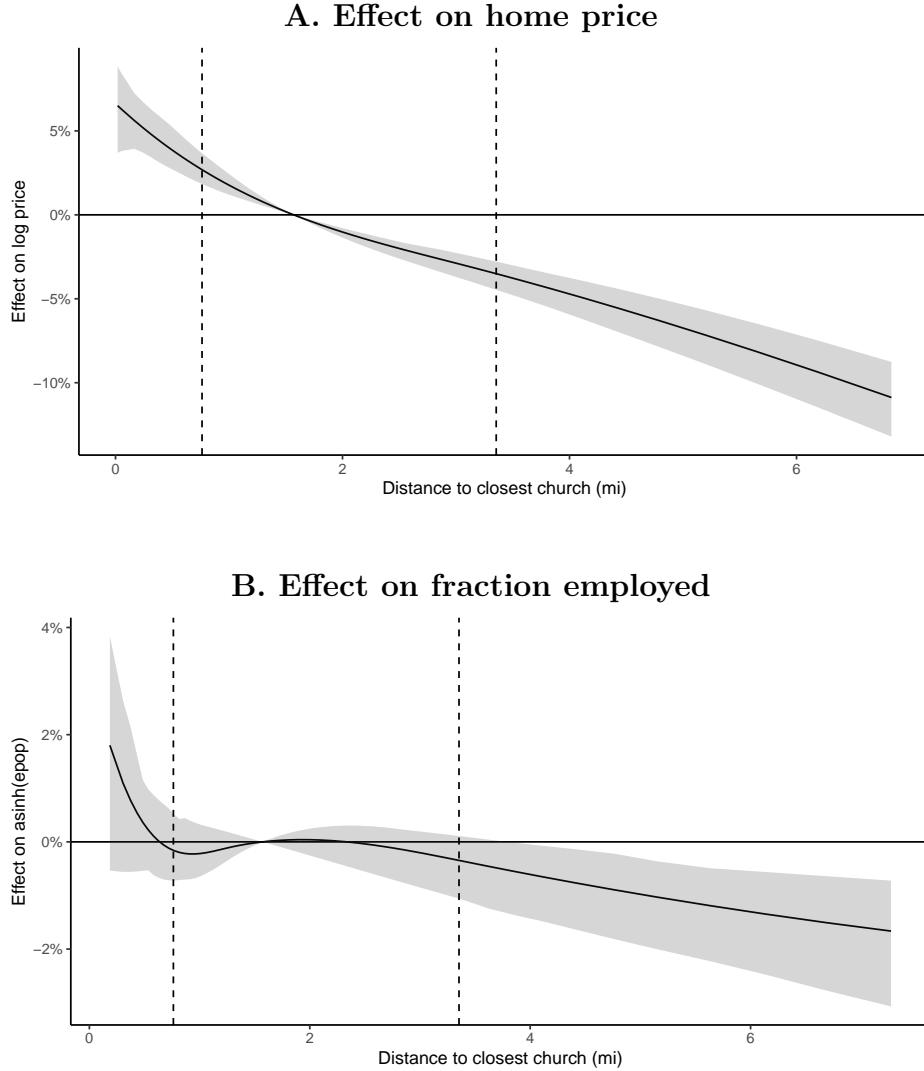
where δ_{it} is distance to the nearest SBC church, instrumented by the interaction of pre-period distance $\delta_{i,1994}$ and pastor death timing. We approximate the logarithm of the church distance price effect function $\theta(\cdot)$ flexibly using basis functions and employ higher-order expansions 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 \times year, $\mu_{C(i),t}$, and census tract $\nu_{c(i)}$. Additionally, we flexibly control for pre-period distance from the nearest SBC church with ϕ following [Borusyak and Hull \(2023\)](#). The estimated effects are closely related 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 estimated in Section 8.1. 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:

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

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 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 patterns 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 from Section 8.1: 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.

9 Model

The event study and NPIV results in Section 8 show that pastor deaths increase distance to the nearest church, and that greater distance translates into lower home prices. We now 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.

To translate our reduced-form estimates into a measure of households' preferences for church proximity, we embed the results in a structural hedonic framework. In this setting, proximity to a SBC church is treated as a local amenity whose value is revealed in 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 recover WTP for church proximity.

Formally, let \mathcal{J} denote the set of housing options available to households and index options by j . Each house 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 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:

$$(J_i, E_i) := \arg \max_{j \in \mathcal{J}, e > 0} U_{ij} + e \text{ such that } e + P_j \leq W_i, \quad (10)$$

$$U_{i,j} := -\beta_i \delta_j + U_i^{(-\delta)}(A_j, \eta_{n(j)}) + \xi_j. \quad (11)$$

Where β_i denotes household i 's willingness to pay to be one mile closer to the church. We can define $X_j := (\delta_j, A_j, \eta_j, \xi_j)$. 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 gets the following indirect utility from living in j :

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

Since there is no person-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 $p : \mathcal{X} \rightarrow \mathbb{R}_+$ mapping characteristics X_j to price P_j such that $P_j = p(X_j)$. In this environment, Equation 10 can be transformed to:

$$\max_{X_j \in \mathcal{X}} u_i(X_j) - p(X_j) \quad (13)$$

$$u_i(X_j) := -\beta_i \delta_j + U_i^{(-\delta)}(A_j, \eta_{n(j)}) + \xi_j, \quad (14)$$

which is a direct optimization problem over housing characteristics.

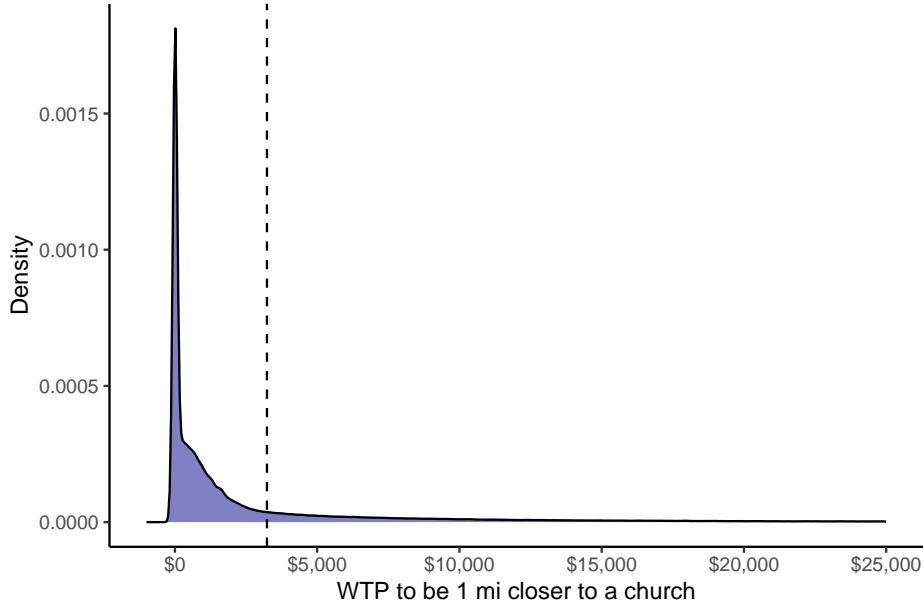
We assume that households make an essentially continuous choice over the distance to their nearest SBC church δ_j . This is motivated by the fact that we observe over 6.7 million housing transactions in the United States within 10 miles of an SBC church. As such, so long as p is convex, we can identify 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:

$$\frac{\partial u_i}{\partial \delta} \Big|_{x=X_{J_i}} - \frac{\partial p}{\partial \delta} \Big|_{x=X_{J_i}} = 0 \implies \beta_i = \frac{\partial u_i}{\partial \delta} \Big|_{x=X_{J_i}} = \frac{\partial p}{\partial \delta} \Big|_{x=X_{J_i}}. \quad (15)$$

Thus, 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 8.3 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 A21), we compute each

household's implied β_i .⁹ 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.

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 A21 by each house's counterfactual value.

10 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.

⁹We 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.

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 ~ 0.3 p.p.) across both specifications, but labor force participation reductions that are twice as large when pastors are convicted than when pastors die (decreases of ~ 1 p.p. and ~ 0.5 p.p., 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 diverge on behaviors that are norm-sensitive. Pastor deaths are followed by fewer non-church social events but do not shift voting or opioid mortality, suggesting a primarily access shock. 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 capitalize these shocks. Following convictions, 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 a pure access channel. A hedonic model implies an average willingness to pay of about \$3,235 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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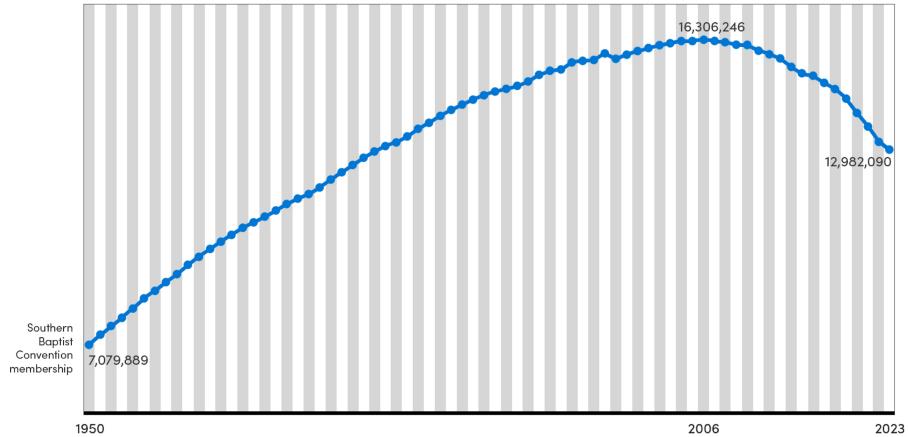
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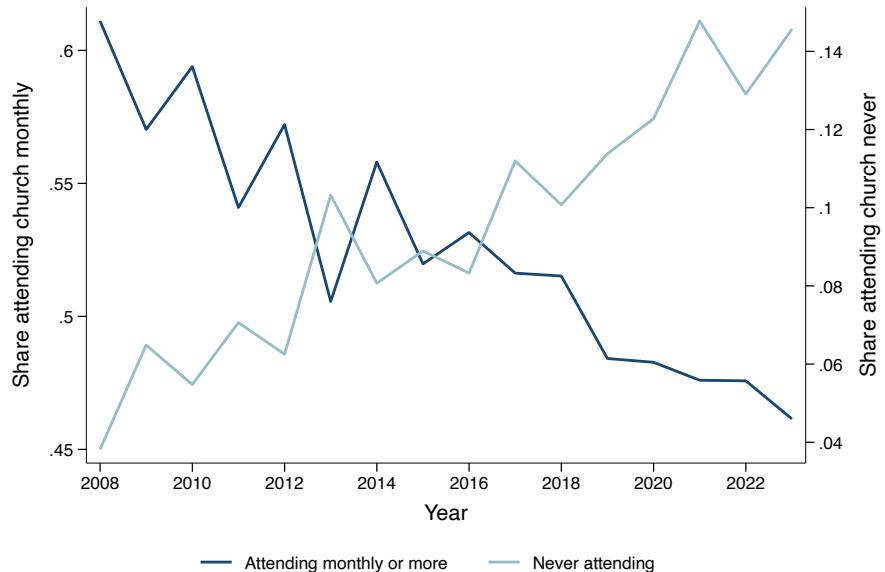
A Appendix Figures

Figure A1: Trends in SBC membership and attendance

A. Membership (Lifeway)

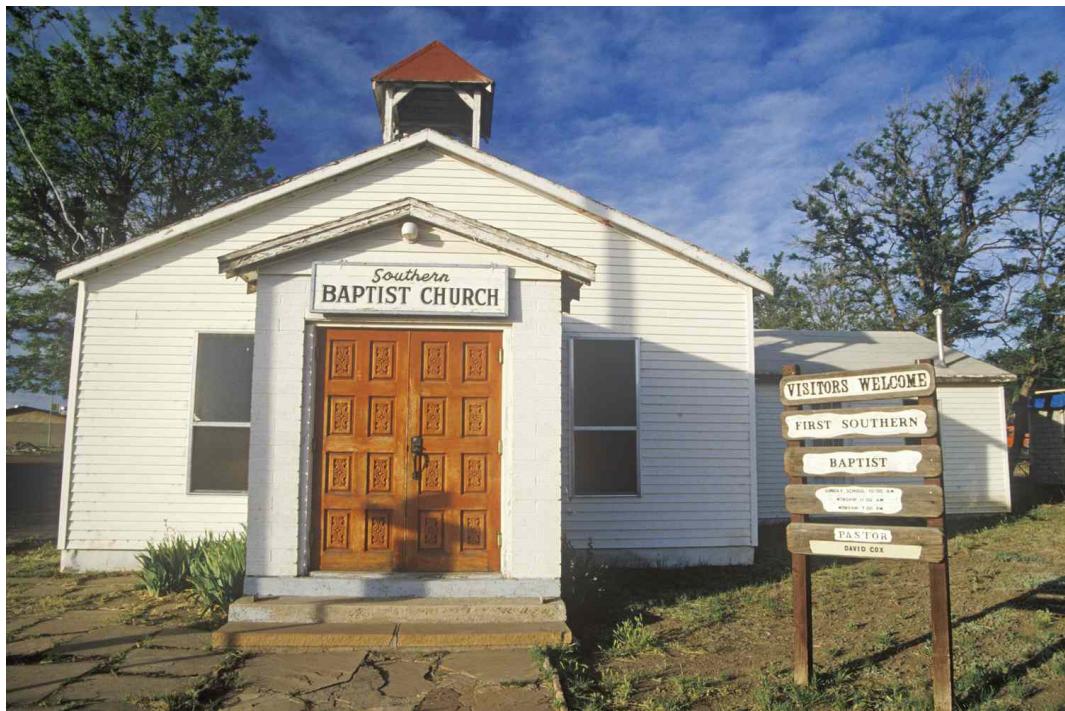


B. Attendance (CCES)



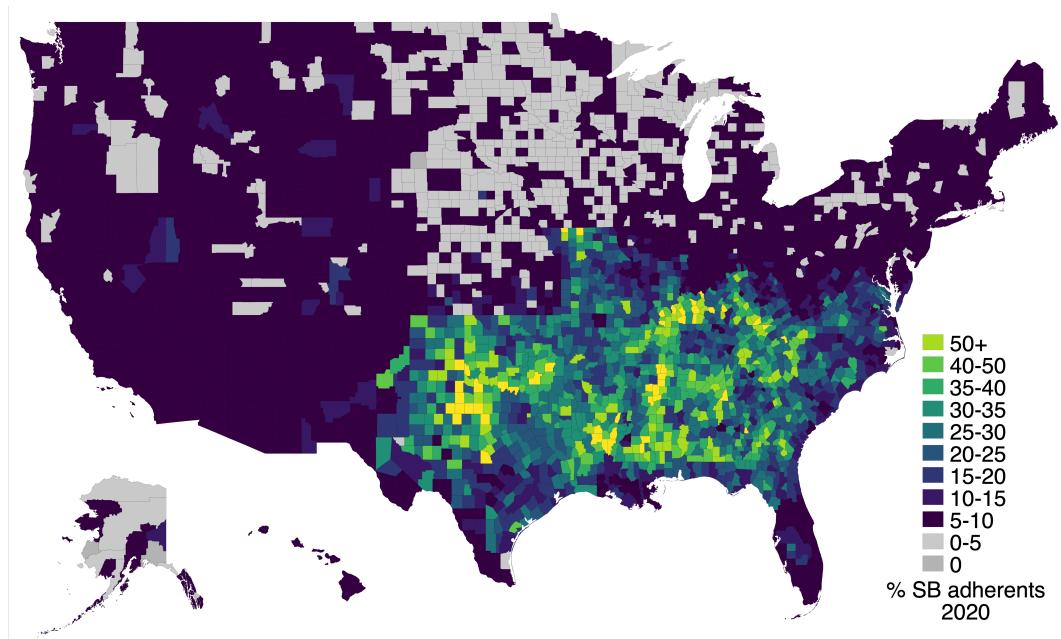
Notes: Data in Panel A comes from Lifeway Research; data in Panel B comes from the Cooperative Election Study.

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	Abbott James 105 Bullard St Enterprise Al 36330
Aaron Lafayette Rt 1 Cussetta Al 36852	Abbott John B Bx B B Espanola NM 87532
Aaron Marvin 3005 Sheldon Clovis NM 88101	Abbott Kenneth L 509 E Woodard Dennison Tx 75020
Abanathy Charles PO Bx 97 Black Rock Ar 72415	Abbott Lewis 428 So Riverhills Dr Temple Terrace Fl 33617
Abbott Barney 305 E 4th Penn W Holdenville Ok 74848	Abbott Raymond C-26 Madison Square Apt Madison Tn 37115
Abbott Clifford E Modena Bp Ch Modena Mo 64663	Abbott Willburn 1906 Lydia Dr Owensboro Ky 42301
Abbott Cloyd Rt 3 Tazewell Tn 37879	Abbott Frank A Jr 1445 Downing St Charleston Sc 29407
Abbott Don PO Bx 334 Forrest City Ar 72335	Abel H B Rt 1 Vance Ms 38964
Abbott E A 705 No Jackson St Albany Ga 31705	Abel Harlan A 6503 W Cameron Tulsa Ok 74127
Abbott Earl Rt 5 Bx 922 London Ky 40741	Abel Jim PO Bx 37 SBC Walnut Ridge Ar 72476
Abbott Eugene Thompsonville Il 62890	Abel Richard St Johns Bp Ch Ehrhardt SC 29081
Abbott H W 3162 Pershall Rd Saint Louis Mo 63136	Abel William M PO Bx 582 Newland NC 28657
Abbott Ivan J 411 E Waggoner St Sullivan Il 61951	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

Abuse of Faith: The database

[Part 1](#) [Part 2](#) [Part 3](#) [Part 4](#) [Part 5](#) [Part 6](#) [Database](#) [Videos](#)

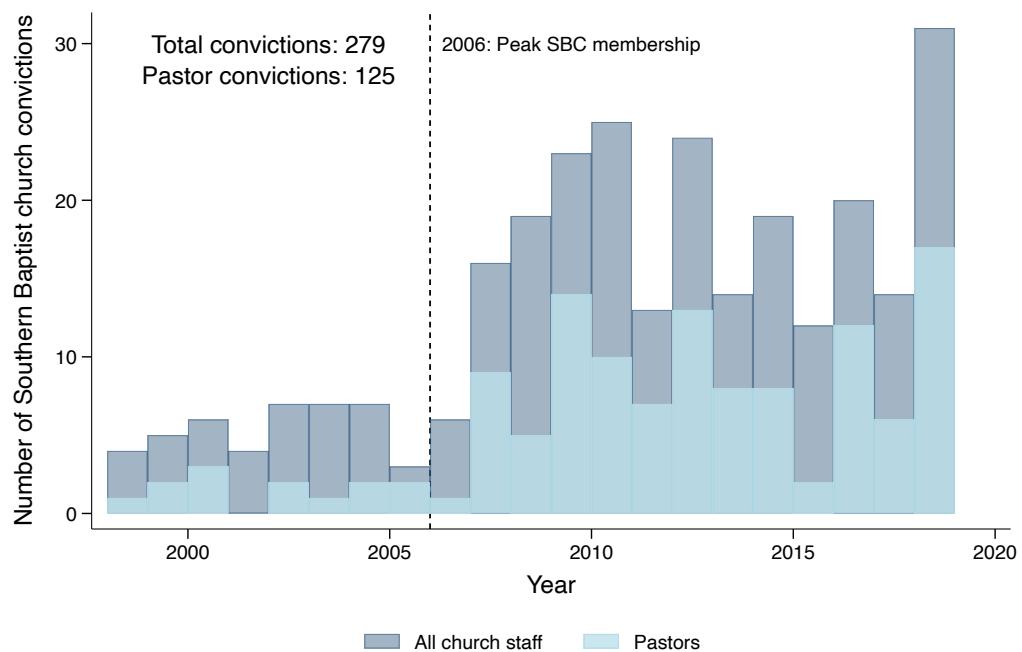


ABUSE *of* FAITH

In the past 20 years, hundreds of Southern Baptists with formal church roles have engaged in sexual misconduct, [a new investigation by the Houston Chronicle and San Antonio Express-News reveals](#). They were pastors. Deacons. Youth pastors.

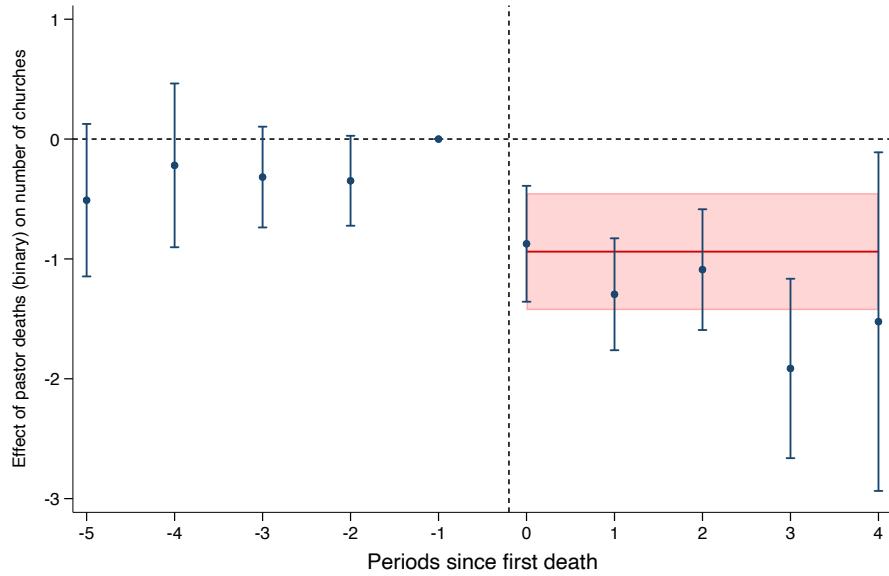
Notes: Screenshot of the Abuse of Faith database.

Figure A6: Timing of convictions



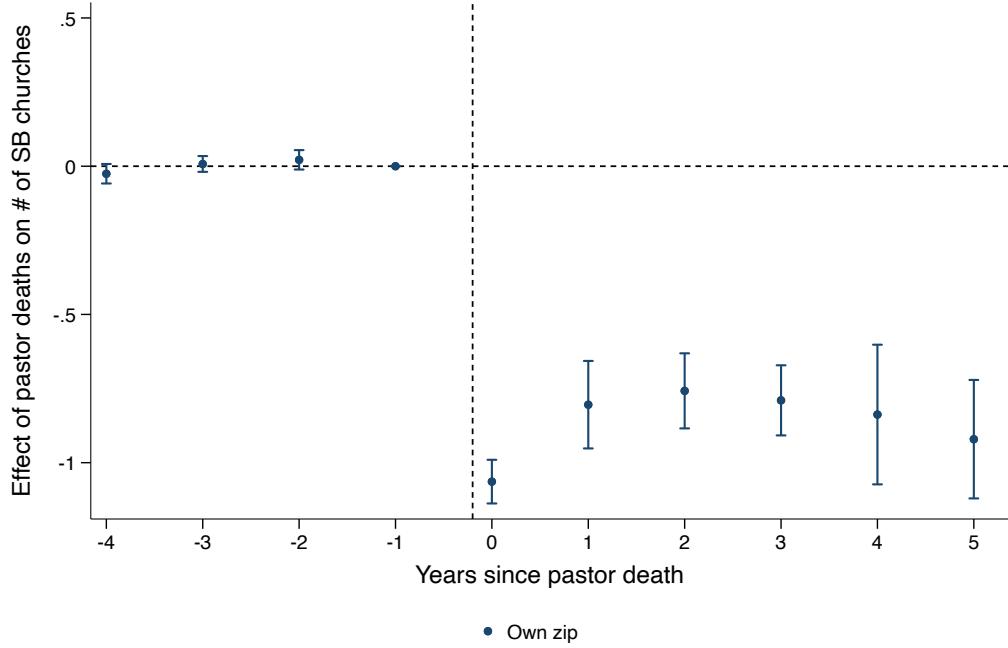
Notes: Data on convictions from the *Houston Chronicle's* “Abuse of Faith” database.

Figure A7: Effect of pastor death on church closures



Notes: Data on church presence from the SBC Annuals. Event studies estimated at the zip code level using the matched controls method described in Section 5.

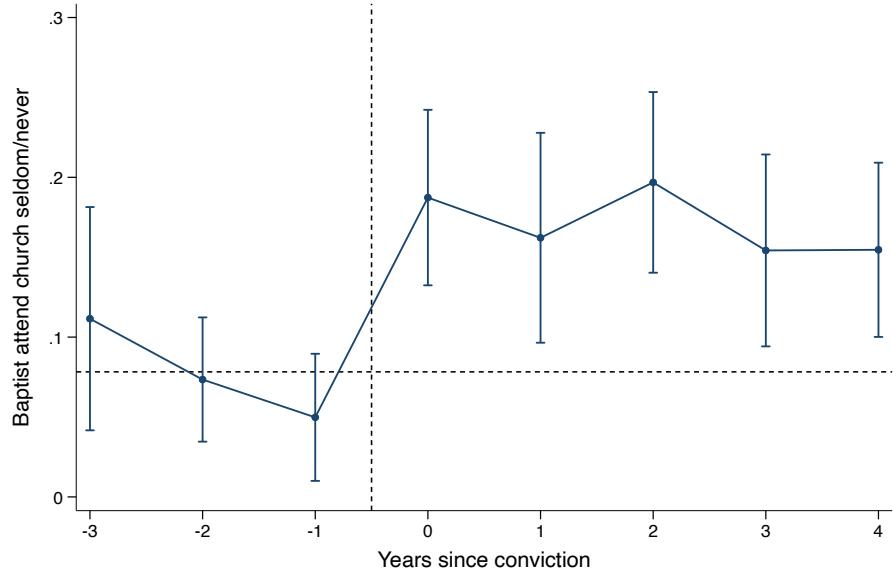
Figure A8: Effect of pastor death on number of churches (continuous)



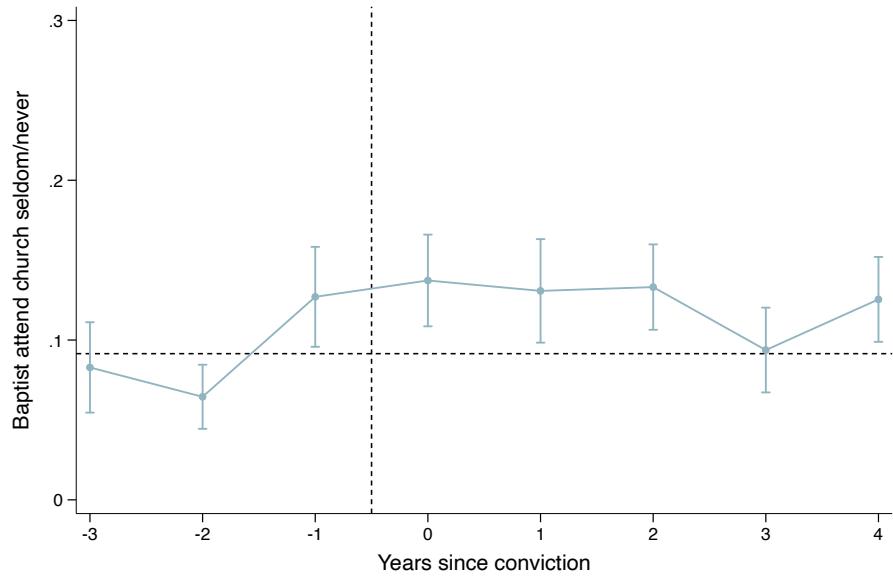
Notes: In this specification, we estimate the dynamic treatment effect of $Y_{zt} = \gamma_t + \delta_z + \sum_{k \neq -1} \beta_k \left[\log \left(\frac{PastorDead_{zt}+0.1}{Matched_{zt}+0.1} \right) \times \mathbf{1}\{t - T_{zt} = k\} \right] + \varepsilon_{zt}$ where we define a continuous treatment 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.

Figure A9: Effect of pastor convictions on share seldom or never attending church (Baptists)

A. Treatment counties (Baptist)



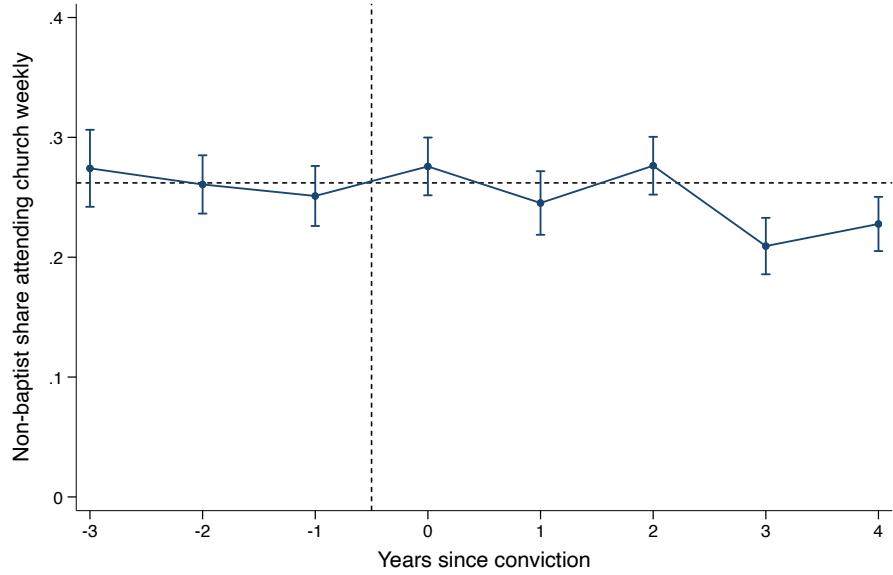
A. Control counties (Baptist)



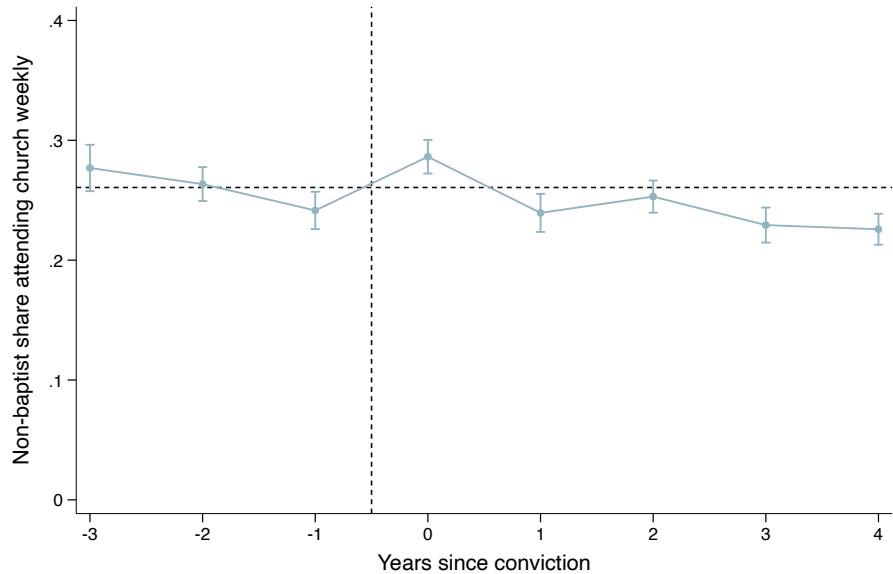
Notes: Data on church attendance from the Cooperative Election Study. Event studies estimated at the county level using the matched controls method described in Section 5.

Figure A10: Effect of pastor convictions on church attendance (Non-Baptists)

A. Treatment counties (Non-Baptist)



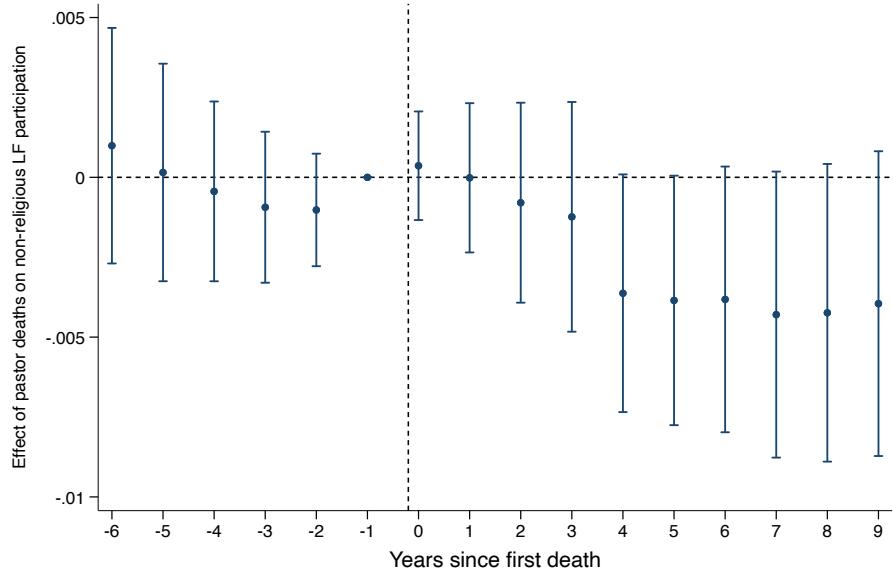
A. Control counties (Non-Baptist)



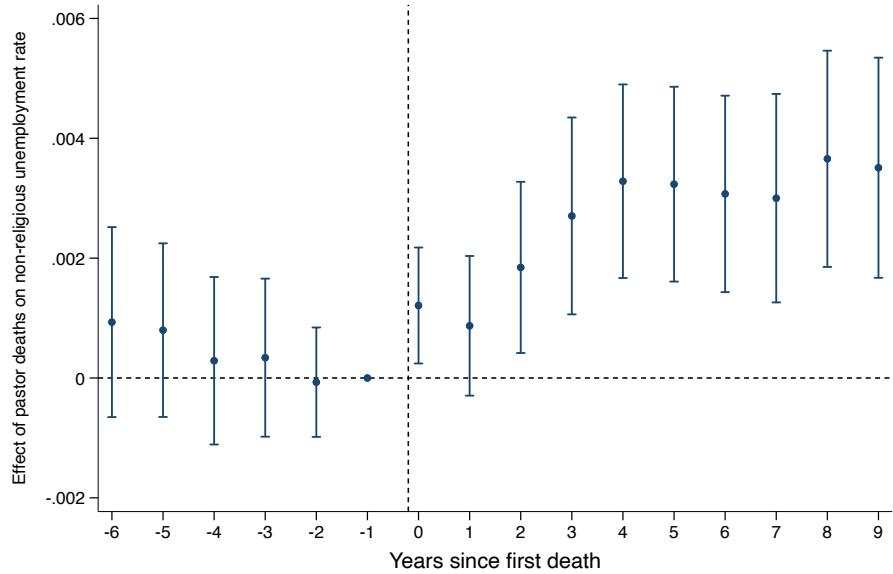
Notes: Data on church attendance from the Cooperative Election Study. Event studies estimated at the zip code level using the matched controls method described in Section 5.

Figure A11: Effect of pastor deaths on non-church employment outcomes

A. Labor force participation



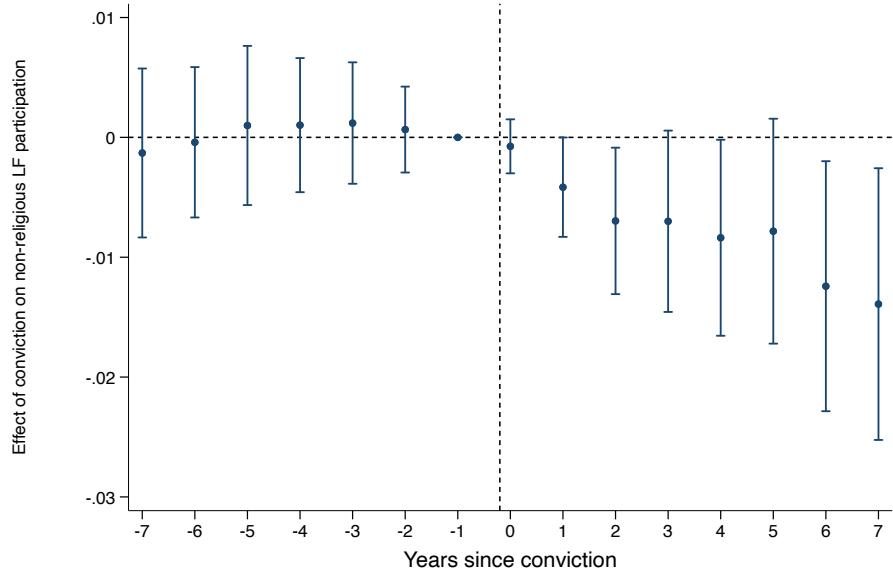
B. Unemployment rate



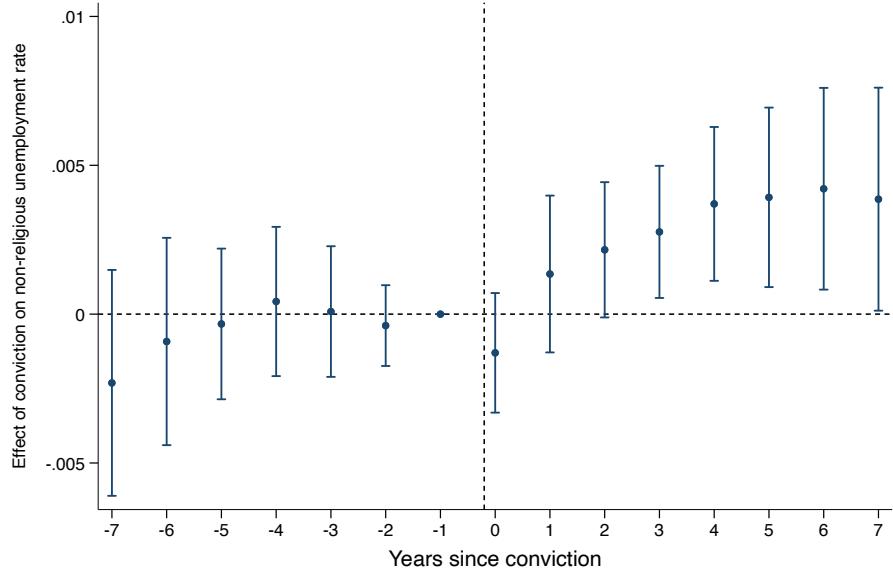
Notes: The above figures show the analogous results to Figure 7, subtracting off church-related employment using data from the County Business Patterns dataset.

Figure A12: Effect of pastor convictions on non-church employment outcomes

A. Labor force participation



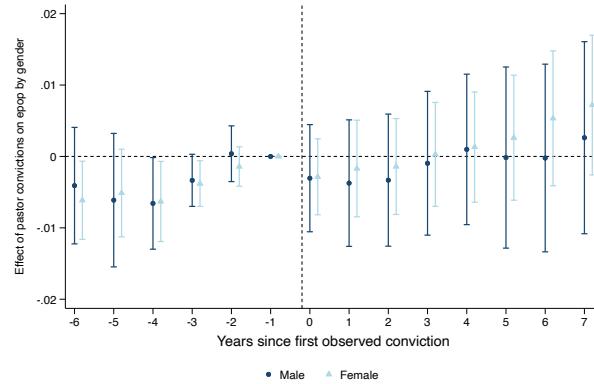
B. Unemployment rate



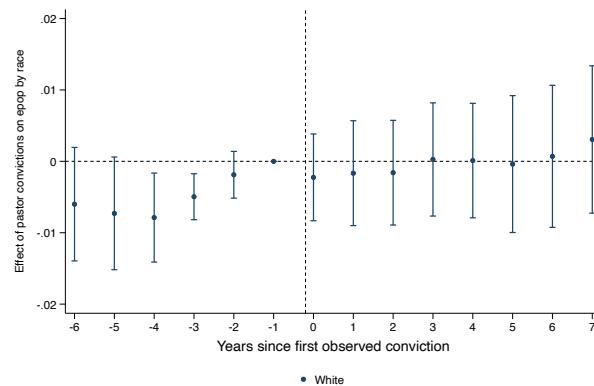
Notes: The above figures show the analogous results to Figure 8, subtracting off church-related employment using data from the County Business Patterns dataset.

Figure A13: Heterogeneity in effects of pastor convictions

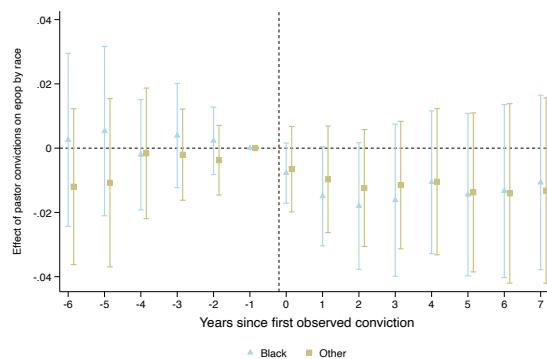
A. Employment-to-population ratio, male vs. female



B. Employment-to-population ratio, White

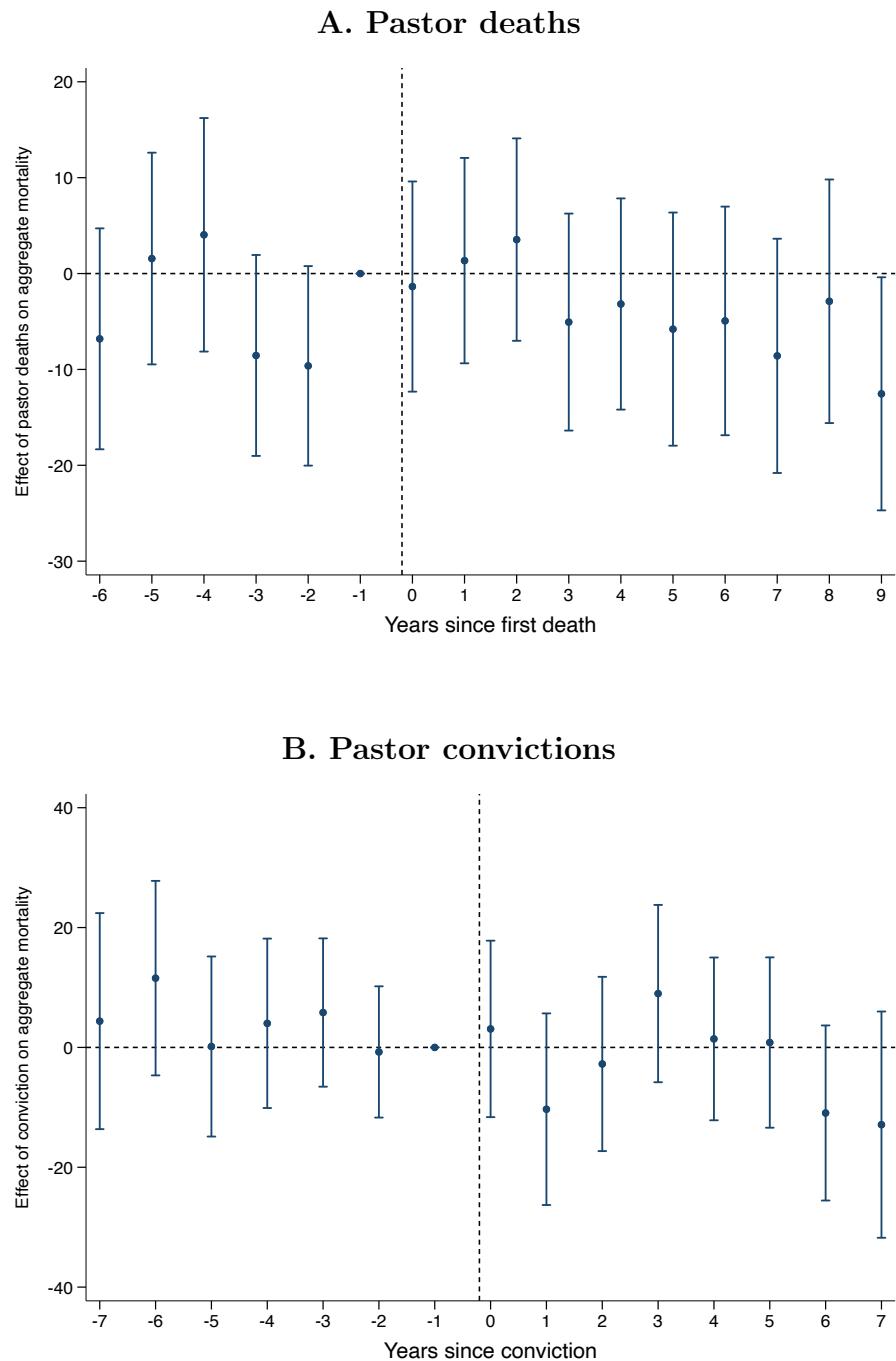


C. Employment-to-population rate, Black and Other



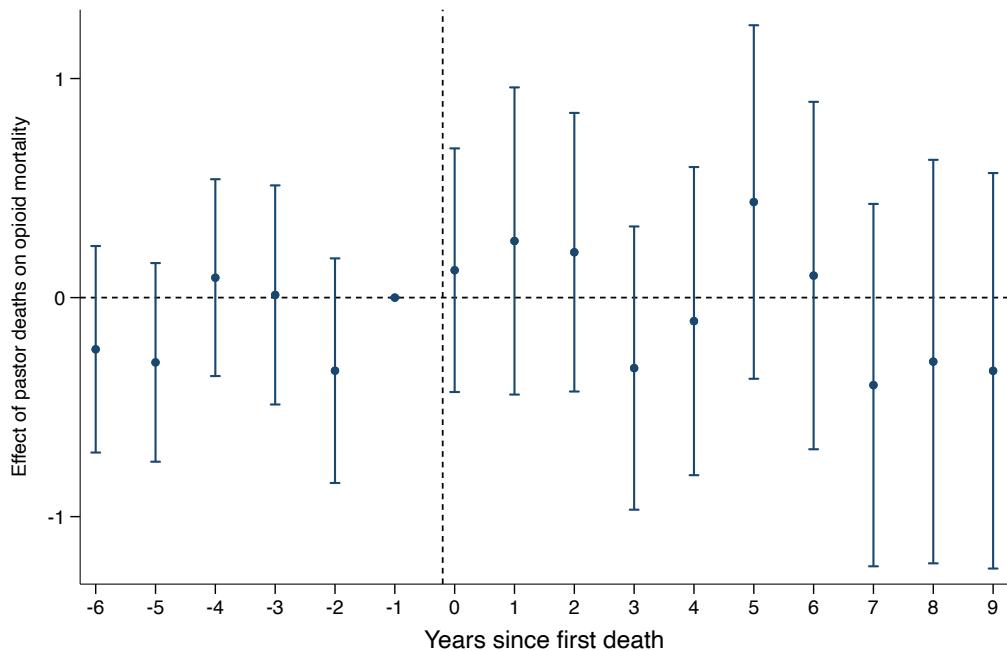
Notes: Employment data by race and by gender are from the Longitudinal Employer-Household Dynamics Quarterly Workforce Indicators. Event studies are estimated with the matched controls method as described in Section 5.

Figure A14: Effects of pastor deaths or conviction on all-cause mortality



Notes: All-cause deaths are from the National Center for Health Statistics' Multiple Cause of Death data using standard ICD-based definitions. Event studies are estimated with the matched controls method as described in Section 5.

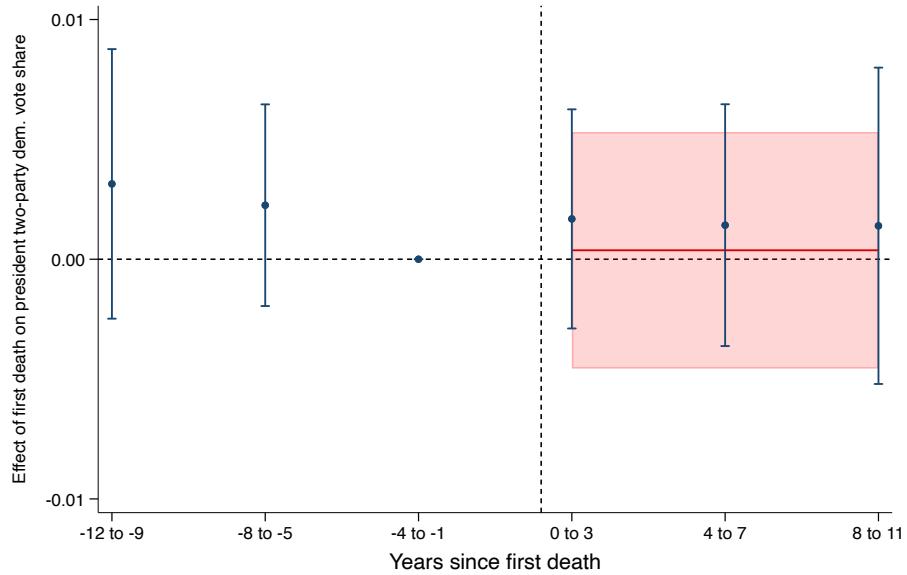
Figure A15: Effects of pastor deaths on opioid mortality



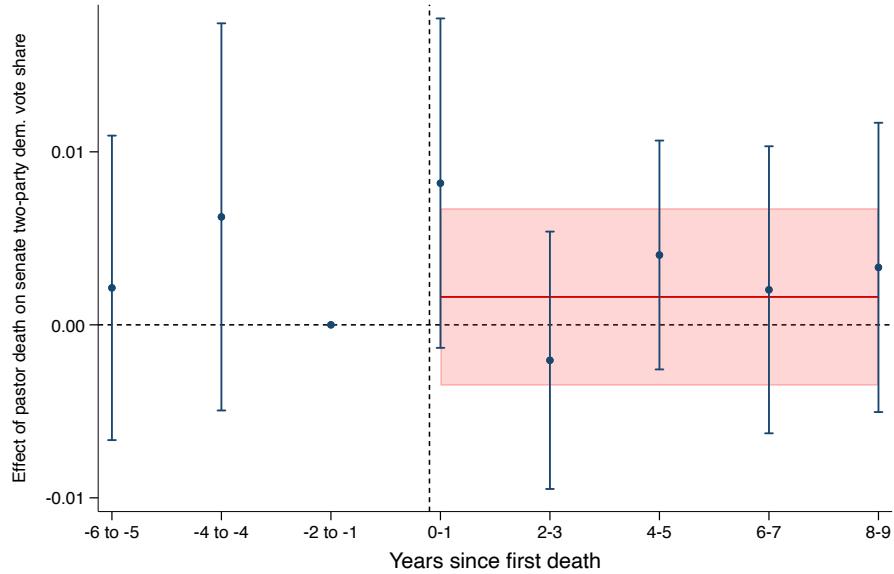
Notes: Opioid deaths are from the National Center for Health Statistics' Multiple Cause of Death data using standard ICD-based definitions. Event studies are estimated with the matched controls method as described in Section 5.

Figure A16: Effect of pastor deaths on vote share

A. Presidential vote share



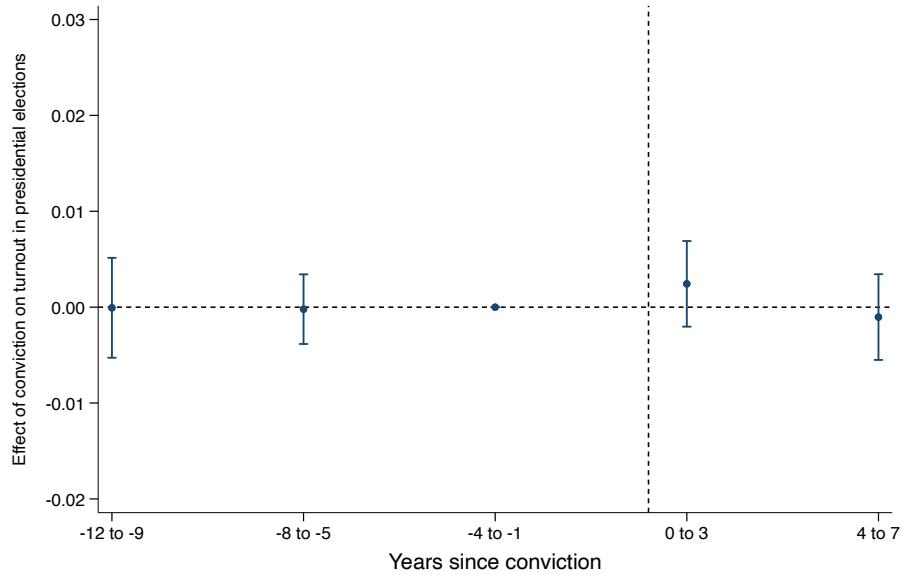
B. Senate vote share



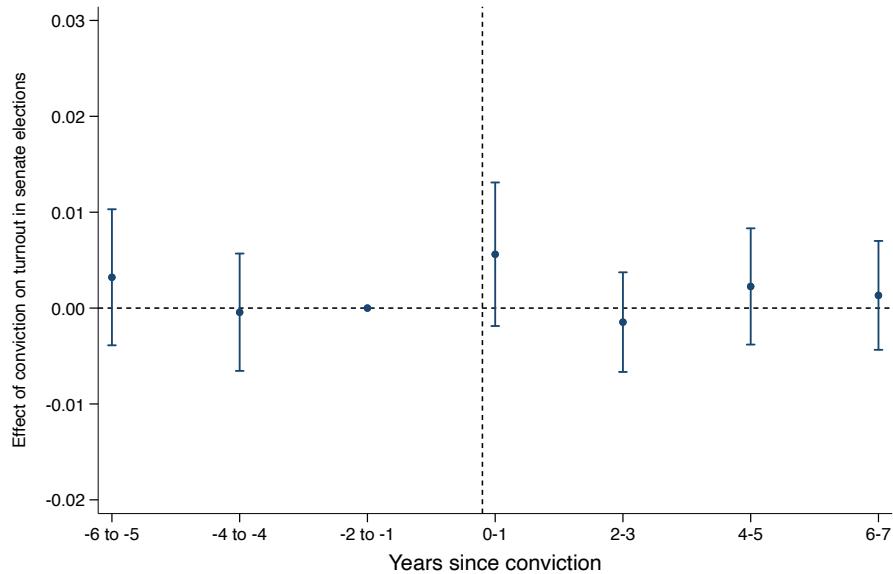
Notes: Electoral outcome data is from the ICPSR's General Election Data and Dave Leip's Atlas of U.S. Elections. Event studies are estimated with the matched controls method as described in Section 5.

Figure A17: Event studies of conviction effects on turnout

A. Presidential election turnout

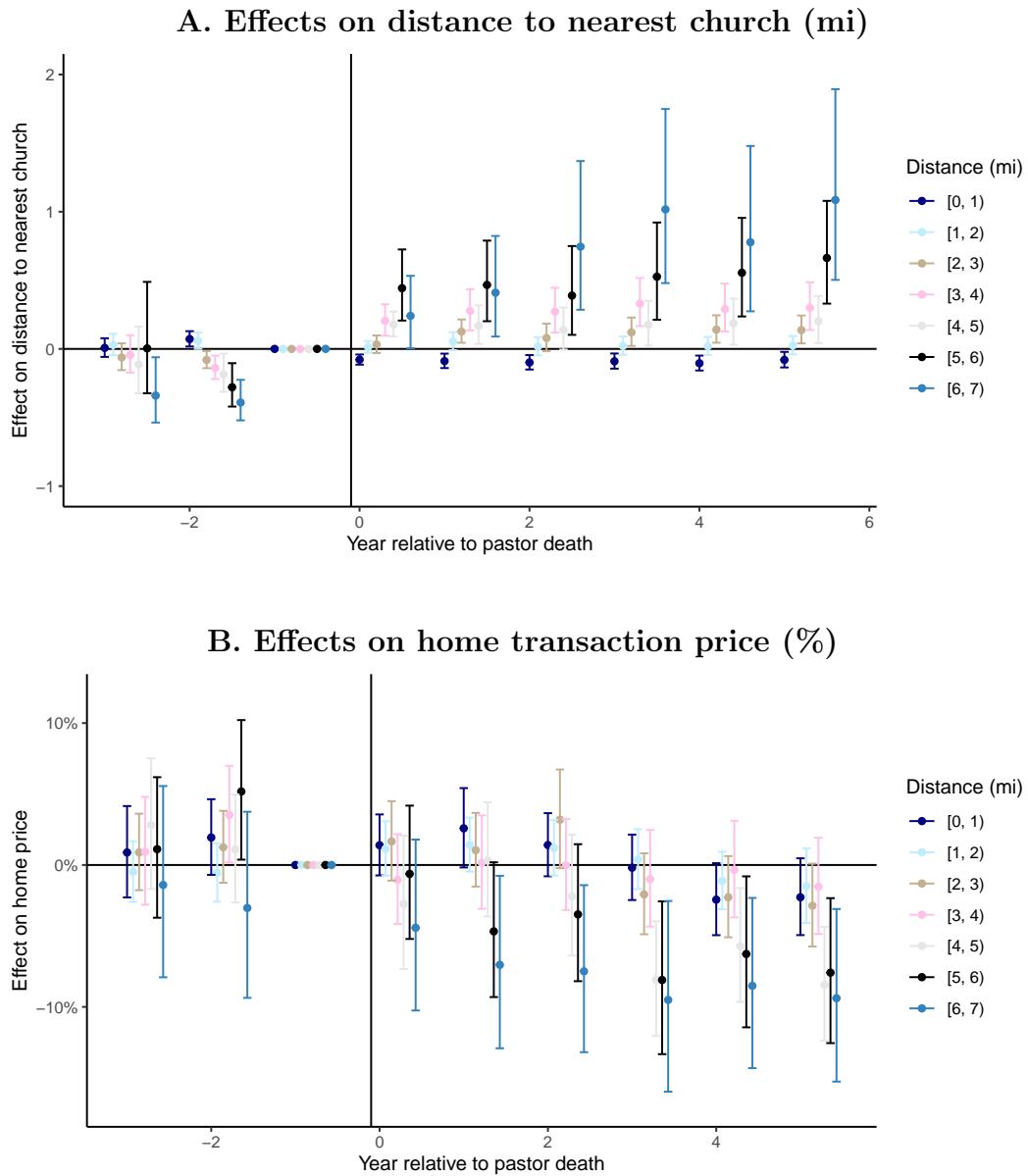


B. Senate election turnout



Notes: Electoral outcome data is from the ICPSR's General Election Data and Dave Leip's Atlas of U.S. Elections. Event studies are estimated with the matched controls method as described in Section 5.

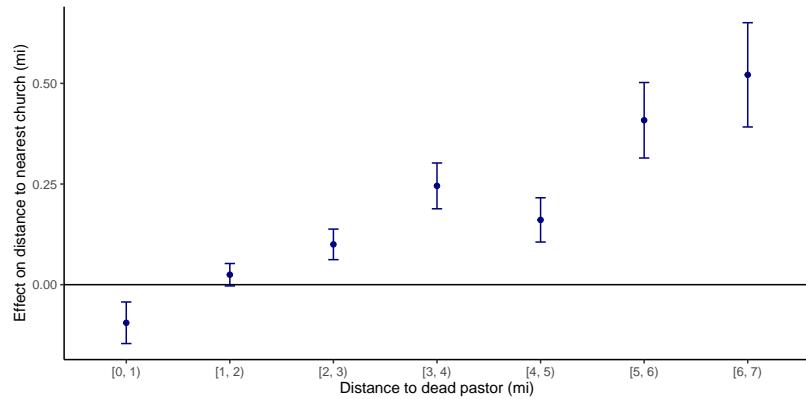
Figure A18: Event study of pastor death on proximity to church and home prices



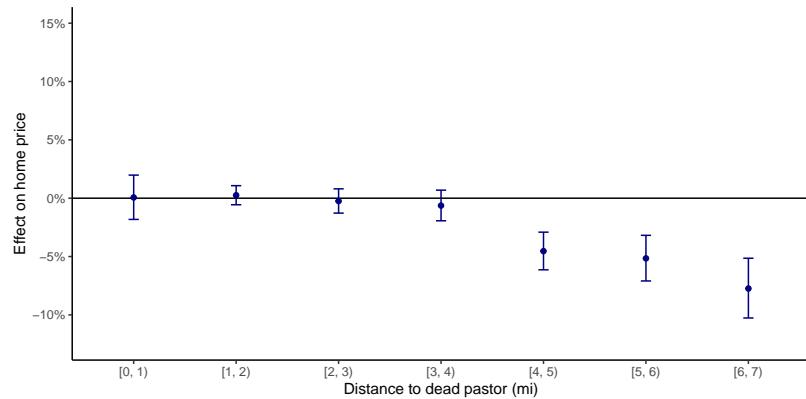
Notes: Event study estimated using a control group of houses in the same distance bin from a church in 1994 that have not yet been treated. Standard errors clustered at the stack \times distance bin \times treatment assignment level.

Figure A19: Pooled effects of pastor death on proximity to church and home prices

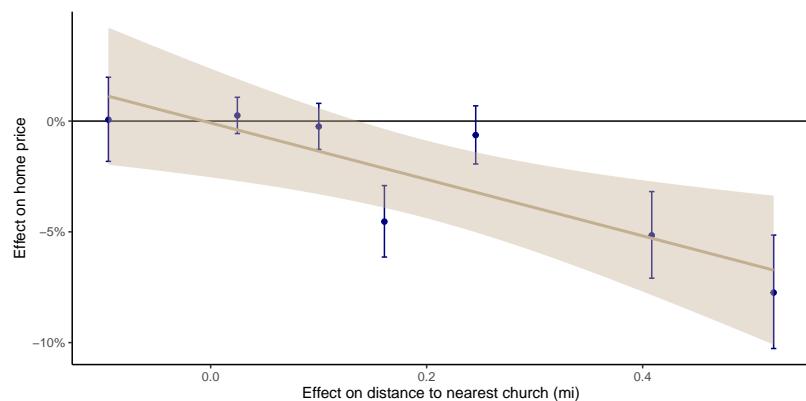
A. Pooled effects on distance to nearest church (mi)



B. Pooled effects on home transaction price (%)

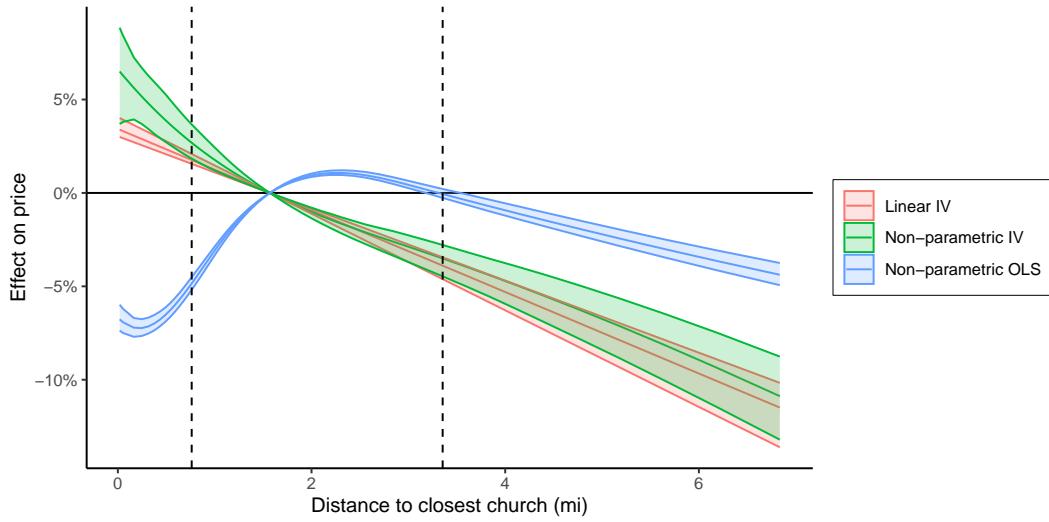


C. Home price effect vs. distance change



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 \times distance bin \times 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 A20: 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 A21: Slope of distance effects



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

B Appendix Tables

Table 3: Effect on social visits of pastor deaths

Dependent Variables:	Church visits	Social visits	Bowling visits
	(1)	(2)	(3)
<i>Variables</i>			
$\log\left(\frac{\text{PastorDead}_{zt}+0.1}{\text{Matched}_{z+0.1}}\right)$	-0.031 (0.014)	-0.014 (0.006)	-0.173 (0.067)
<i>Fixed effects</i>			
County	X	X	X
State \times Year	X	X	X
Race \times Year	X	X	X
Age group \times Year	X	X	X
Sex \times Year	X	X	X
City size \times Year	X	X	X
<i>Fit statistics</i>			
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.