Deaths of Despair and the Decline of American Religion

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July 2024

Abstract

In recent decades, death rates from suicides, drug poisonings, and alcoholic liver disease have dramatically increased in the United States. We show that these "deaths of despair" began to increase relative to trend in the early 1990s, that this increase was preceded by a decline in religious participation, and that both trends were driven by middle-aged white Americans. Using repeals of blue laws, we find that a shock to religious participation has significant effects on these mortality rates. Our findings show that social factors such as organized religion can play an important role in understanding deaths of despair.

JEL classification: Z12, J11, I18

We thank audiences at the ASREC Conference, BFI Health Economics Initiative Annual Conference, BFI Women in Empirical Microeconomics Conference, Brown University, Colgate University, Florida State University, Harvard University, Indiana University, IU East, SEA Conference, Stanford University, and the University of Michigan. We also thank Sarah Abraham, David Autor, Ivan Badinski, Feler Bose, Amy Finkelstein, Colin Gray, Jonathan Gruber, Simon Jaeger, Adriana Lleras-Muney, Matt Lowe, Frank Schilbach, Carolyn Stein, and Heidi Williams for their helpful comments. Funding from the National Institute on Aging through Grant Number T32-AG000186, the National Science Foundation Graduate Fellowship Program under Grant Number 1122374, and the Church Sexual Abuse Crisis Research Grant Competition is gratefully acknowledged. Email the authors at dhungerm@nd.edu, tg105@wellesley.edu, and oostrom.1@osu.edu. The authors declare that they have no relevant or material interests that relate to the research described in this paper.

1. Introduction

Over the past few decades, the death rate from suicides, drug poisonings, and alcoholic liver disease has dramatically increased in the United States. For middle-aged white Americans, increases in deaths from these causes, known as "deaths of despair," have been so dramatic that at the turn of the century all-cause mortality rates began to rise, reversing decades of decline (Case and Deaton, 2015). This rise in mortality has been called "one of the most important economic and demographic issues of our time" (Cutler and Lleras-Muney, 2017). In searching for explanations, researchers have acknowledged the potentially important role of social or cultural phenomena, but assessing these factors has proven difficult.[1]

In this paper, we provide new empirical evidence relating changes in the social fabric of American communities, and religious participation in particular, to changes in mortality. Much of the work on deaths of despair focuses on changes during the 21^{st} century; we instead focus on the late 20^{th} century.[2] We begin with the empirical observation that, for middle-aged white Americans, deaths of despair began to break from trend at the start of the 1990s. This early-1990s break from trend has received relatively little attention but was large in magnitude.

This period also saw important changes in religious participation. Beginning in the late 1980s, many measures of religious adherence in the United States began a sharp downturn (e.g. Hout and Fischer, 2002). This large and widespread decline has been noted by researchers studying religion, and religiosity is well known to be strongly correlated with health outcomes (Lowe, 2020; Bentzen, 2019; Hungerman, 2014; Iyer, 2016). But most work on this decline has not considered its proximity to the initial rise of deaths of despair.

We then use data from the General Social Survey (GSS) to show this decline was driven

[1]Maclean et al. (2017) note that few studies on rising mortality examine changes in culture or social cohesion, which they describe as a "substantial gap in the literature." See also Zoorob and Salemi (2017), Sundquist et al. (2016), Case and Deaton (2017), Scutchfield and Keck (2017), Social Capital Project (2019), Ruhm (2021) and Case and Deaton (2020).

[2]In 1999, the 10^{th} version of the International Classification of Diseases was adopted for classifying cause of death. Much of the work on deaths of despair considers the years after 1999 and using earlier mortality data requires the use of different classification codes.

by white middle-aged Americans without a college degree, the same group that experienced subsequent increases in mortality. Further, we find this decline is *not* specifically driven by men or by women, it is not driven by only rural or urban residents, and it is not initially observed for non-white Americans. We show these null results are also observed in the initial rise of deaths of despair.[3] While the general decline in religious participation is well known, these demographic results are not.

We also show that there is a strong negative relationship between religious attendance and deaths of despair across states. We further find that states that experienced larger declines in religious attendance in the last 15 years of the century saw larger increases in deaths of despair. Overall, the mortality increase and religious participation decrease were happening at the same time, in the same places, and among the same group of individuals.

Finally, we examine a policy-based shock to religiosity that prefaced the decline in religious participation: the repeal of blue laws. As first discussed in Gruber and Hungerman (2008), these laws regulated commerce at certain times of the week, often Sunday mornings. Today, blue laws are often focused on alcohol sales, but the blue laws we study imposed broad restrictions such as prohibition of all labor on Sundays; we discuss these laws more in Section 3. After a Supreme Court decision provided a test by which these laws could be found unconstitutional, many blue laws were repealed.

Blue laws have been shown to be strongly related to religious participation, creating discrete changes in incentives to attend religious services that are plausibly unrelated to other drivers of religiosity. Following several prior studies, we show that the repeal of these laws lowered religious participation. Then, using simple graphical analysis along with difference-in-difference specifications, we find that repeal led to an increase in deaths of despair; we generally do not see changes in other causes of mortality. Our results hold across a variety of specifications and are robust to issues raised in recent work on the difference-in-differences methodology. The estimates suggest that the increase in mortality was driven by white

[3]These trends match trends in Case and Deaton (2017) (and are different from more recent opioid trends following the rise of fentanyl).

and middle-aged individuals. We find that for middle-aged Americans, the repeal of blue laws had a 5- to 10-percentage-point impact on weekly attendance of religious services and increased the rate of deaths of despair by 2 deaths per 100,000 people. Applying these results to the decline in religious participation at the end of the century suggests that this decline can explain a reasonably large share of the initial rise in deaths of despair.

These results have several implications. First, in the large literature on deaths of despair, there is a need for rigorous evidence exploring the importance of religion or the social fabric of communities generally. Existing work has focused mainly on factors such as deteriorating economic prospects for working-class Americans (for instance, Hollingsworth et al., 2017; Pierce and Schott, 2020), or opioid availability (for instance, Barnett et al., 2017; Khan et al., 2019), yet many researchers have concluded that the picture of the crisis that they offer is incomplete.[4] Our work provides evidence that religious participation matters.

Second, our findings address an issue facing any potential account of this mortality crisis: explaining why the crisis predominantly affected non-Hispanic white individuals without a bachelor's degree. Ruhm (2021) notes the difficulty in reconciling changes in social forces with mortality patterns for less-educated middle-aged white Americans. We offer a potential explanation, as the changes in religious participation that we study were driven by middle-aged white Americans. This finding is made possible by our use of data on religiosity and mortality in the late 20th century; future work considering etiological factors in deaths of despair should consider the break from trend in mortality observed in the early 1990s.

Some potential causes could not logically have mattered at this early date. For example, Alpert et al. (2022) show that triplicate prescription laws affected the marketing decisions taken by Purdue Pharma during the introduction of OxyContin and the subsequent spread of opioid abuse. However, OxyContin was introduced in 1996, when deaths of despair for middle-aged white Americans were already well above trend, with over 15% more deaths than

[4] Deaton stated regarding the drivers of deaths of despair, "One thing we very strongly resist is that short-term destruction of economic opportunities is what drove deaths of despair. We know that's not true. It has to be this long-term drip of losing opportunities and losing meaning and structure in life." (Karma, 2020).

one would forecast using data from the 1980s. Our paper should be considered separately from the effects of OxyContin. Our key contribution is not in explaining the current opioid crisis, but in identifying and helping to explain mortality patterns that predate this crisis. We discuss how the trends we consider could interact with the subsequent introduction of OxyContin in Section 4.

Next, a large literature in economics and other fields has documented a relationship between religiosity and health, or between social networks and health (Smith and Christakis, 2008). Much recent work focuses on the health benefits of religious participation for young adults (for instance, Fletcher and Kumar, 2014; Cooley Fruehwirth et al., 2019; Mendolia et al., 2019; Mellor and Freeborn, 2011; Gruber and Hungerman, 2008; Pope et al., 2014).[5] Our work highlights how changes in religious participation can have large consequences for the health of middle-aged individuals.

Many studies have also highlighted the salutary effect of religiosity for societies, often by studying the role of religious participation as a source of comfort, stability, or mutual insurance in the face of enormous negative shocks such as combat experience, natural disasters, or economic crises (Bentzen, 2019; Ager et al., 2015; Cesur et al., 2020; Chen, 2010). The large negative outcomes that we observe for the United States in the 1990s highlight that changes in religious participation can play an important role in well-being in a highly developed society even absent large-scale wars and natural disasters.

Lastly, many papers have discussed the recent decline of religious participation in America. We move beyond the characterization of this decline to consider its consequences. Our study indicates that this decline may have had large and negative effects on well-being. In Section 4, we show that these effects may have occurred through both a weakening of personal religious belief and a weakening of religious proscriptions and religious-based mutual insurance. We find no evidence that shocks to religious attendance generated subsequent

[5] Some notable studies, such as Becker and Woessmann (2018), Campante and Yanagizawa-Drott (2015), and Bryan et al. (2021), do not focus only on young adults. However, these studies do not consider religiosity in our setting, nor do they consider the outcomes examined here.

declines (or increases) in other types of social capital. We note that several aspects of the US context, including its relatively weak welfare state and the nature of the decline in religious participation itself, may have amplified the patterns we document.

Section 2 of this paper provides a descriptive analysis of trends over time in mortality and religious participation. In Section 3, we discuss our empirical strategy using blue laws. In Section 4, we present the effects of blue laws on deaths of despair, discuss potential channels for this effect and the US context of our study, and in Section 5, we conclude.

2. Descriptive Analysis

2.1. Mortality and Religiosity Data

We begin with a descriptive analysis of trends over time in mortality and religious participation. Our mortality data come from the Centers for Disease Control and Prevention's (CDC's) Multiple Cause of Death files. These files provide a near-census of all deaths in the United States from 1969 to 2016.[6] The data identify causes of death using several revisions of the International Classification of Diseases (ICD): ICD-8 for the years 1969–1978, ICD-9 for 1979–1998, and ICD-10 for 1999–2015. We focus on three subcategories of mortality—nondrug suicides, liver cirrhosis, and drug poisonings—and link these categories across the ICD revisions.[7] For brevity and consistency with past work, we refer to these causes collectively as "deaths of despair." While it is well known that trends in these deaths have been

[6]We use the Multiple Cause of Death (MCOD) files because they identify age-at-death within five-year age bins across our time period, whereas the CDC's Underlying Cause of Death (UCOD) files use only ten-year bins. In 1972, data in both the MCOD and UCOD files were coded and processed for only 50 percent of all deaths. In line with convention, we double our mortality rates in this year. For 1981 and 1982, the MCOD files are missing some deaths for a subset of states. We check our mortality rates against those calculated using the UCOD files and find negligible differences at aggregate levels.

[7]We link across revisions using the following crosswalk: for nondrug suicides, we use ICD-8 codes E950.4-E950.9 and E951-E959, ICD-9 codes E950.6-E950.9 and E951-E959, and ICD-10 codes X65-X84 and Y87.0. For liver cirrhosis, we use ICD-8 and ICD-9 code 571 and ICD-10 codes K70 and K73-74. For drug poisonings, we use ICD-8 codes E850-E859, E950.0-E950.3, E962, and E980.0-E980.3, ICD-9 codes E850-E858, E935, E937, E939, E950.0-E950.5, E962.0, and E980.0-E980.5, and ICD-10 codes X40-X44, X60-X64, X85, Y10-Y15, Y45, Y47, and Y49. Our results are robust to the use of other, slightly different ICD classifications, including those used in the 2019 Social Capital Project report (Social Capital Project, 2019).

driven by less-educated white Americans (Case and Deaton, 2020), education data were not available on death certificates prior to 1989 (NCHS, 2010). In addition, when we consider results by race, we group Hispanic and non-Hispanic white individuals together, as data on Hispanic origin were not widely available until the late 1990s. To create mortality rates per 100,000, we use population counts from the Surveillance, Epidemiology, and End Results (SEER) U.S. Population Data.

Our main measures of religiosity are based on two different types of religious participation recorded in the GSS: attendance, and strength of affiliation.[8] The GSS is a generally biennial survey that was started in 1973 and is still ongoing. Each wave asks a nationally representative sample of approximately 1,500 respondents questions about social attitudes and religiosity. While moderately sized, its sample is large enough to permit some comparisons of trends in religiosity across different groups.

Each year, respondents are asked how often they attend religious services. Answers are coded into a 9-point index, from 0 to 8, with 0 being "never" and 8 being "more than weekly."[9] We consider an indicator for whether the respondent attended once a year or less as a measure of low religious participation and an indicator for whether the respondent attended church every week as a measure of high participation.

We also use an alternative measure of religious participation based on individuals' self-stated religious affiliation. Respondents in the GSS are asked to rate the strength of their religious affiliation as "strong," "somewhat strong," "not very strong," or "no religion." We consider an indicator for whether an individuals' self-stated religious affiliation is "not very strong" or "no religion," which we refer to collectively as weak affiliation. As a measure of high religiosity, we consider an indicator for whether an individual's self-stated religious affiliation is "strong." We examine alternate intermediate outcomes for both the attendance and affiliation measures in the Appendix.

[8]The term "religiosity" is often understood as applicable beyond participation in organized religion; in section 4.3 we broaden our measures of religiosity to include prayer and belief in the afterlife.

[9]The exact categories are "never," "less than once a year," "once a year," "several times a year," "once a month," "2–3 times a month," "nearly every week," "every week," and "more than once a week."

2.2. *Trends in Deaths of Despair and Religious Participation*

In figure 1, we depict the average mortality rate due to deaths of despair for white Americans ages 45–64 from 1979 to 2002.[10] Deaths in these categories had declined steadily throughout the 1980s; the dotted line in the picture shows a linear trend fitted through the year 1989 and then projected forward. The dotted line fits the data very well in the 1980s but not thereafter. Starting in the early 1990s, deaths increased both absolutely and relative to trend. The departure from trend is large in magnitude. By 1996, at the time of the introduction of OxyContin and before the start of the period considered by Case and Deaton (2015), the rate of deaths of despair was 38.5 per 100,000, about 17% higher than the counterfactual trendline of 33 deaths per 100,000.[11]

While the post-1999 mortality increase has justifiably garnered a large amount of attention,[12] the change in the early 1990s is perhaps as striking but has received comparatively less attention in prior work, although it is consistent with the findings in a number of other studies.[13] This figure aims to contribute toward a more rigorous and complete study of the timing of deaths of despair that builds upon prior work; by considering the first half of this

[10]This figure resembles figure 1 in Case and Deaton (2015), but their figure is for all-cause mortality rather than deaths of despair. Our figure is also connected to figure 2 in Case and Deaton (2017), although that figure contains data only from 1993 onward, after the trend change that we document.

[11]OxyContin is the brand name of oxycodone, a opioid used for the treatment of pain. While various versions of oxycodone have been in use for over a century, OxyContin was a long-acting reformulation developed by Purdue Pharma and approved by the US FDA in December 1995 and brought to market in 1996. It was a major blockbuster drug, producing $35 billion in revenue. Research suggests OxyContin has played a substantial role in the subsequent opioid crisis (Alpert et al., 2022) and its approval was described by the FDA commissioner as one of the "great mistakes" of modern medicine (Keefe, 2017).

[12]Mortality for this group increased by nearly 10 deaths per 100,000 people from 2000 to 2004 alone.

[13]Several extant pictures suggest this pattern, such as figure 4.1 in Case and Deaton's book (2020, pg. 50) and figure 2 in Case and Deaton (2017), which only goes back to 1993 and suggests by implication that a figure like this must be possible if deaths were falling for most of the century. Their text notes that "overdose deaths began to rise in the early 1990s" and that this raises the possibility that "opioids became the opium of the masses" for those leaving faltering religions (p.118) although they provide no rigorous investigation of this possibility. They also confine this observation to opioids; we find ties between religion and non-opioid deaths of despair in our later analysis. Figure 3 in the Social Capital Project (2019) also indicates the pattern here, but that report argues that "it is no wonder that no one spoke of 'deaths of despair'" at the end of the century, a conclusion very different from the one suggested here. Finally, table 1 in Ruhm (2021) shows that counterfactual mortality rates for this age group are sensitive to whether early 1990s data are used, again fitting indirectly the pattern in the figure here.

picture, we provide evidence on factors that precipitated the break from trend.[14]

Specifically, we focus on religious participation and consider how this break in the mortality trend compares to time trends in religiosity in the United States. Since work by Hout and Fischer (2002), the late-century decline of religiosity in America has been extensively studied. Here, we use data from the GSS to explore this decline and note its coincident timing with the mortality trends in figure 1.

Figure 2 plots religious attendance and affiliation over time in the GSS. Given the sample size of the GSS and our interest in focusing on a relatively small subgroup, we show this trend for all respondents and then focus on the group that drives deaths of despair: 45- to 64-year-old white individuals without a college degree. The figure plots responses for each even-numbered year; for surveys conducted in consecutive calendar years, we combine survey responses from odd-numbered years with those from the year before to smooth the picture. Panels (a) and (b) plot our measures of low religiosity—whether the respondent attends a worship service once a year or less (panel (a)) and whether the respondent has weak or no religious affiliation (panel (b)). Panels (c) and (d) plot the measures of high religiosity: weekly service attendance (panel (c)) and strong religious affiliation (panel (d)). See Section 2.1 for more information.

Figure 2 panel (a) shows an overall increase in low religious attendance in the last half of the 1980s, with the share of respondents who attend religious services once a year or less increasing by 30 percent by the end of the 1990s. This increase is larger for less-educated white individuals (thin solid line) and notably stronger for the middle-aged less-educated white group (thick solid line) (there are approximately 500 such individuals observed at each point in time in the figure). Panel (b) shows a similar pattern for strength of religious affiliation. Middle-aged, less-educated white Americans had the lowest levels of weak religious

[14]This figure may also provide useful context for understanding subsequent mortality trends into the 21^{st} century. The state-level correlation between deaths of despair in 1995 and deaths of despair in 2015 is quite strong, as displayed in appendix figure A1. In figure A1, we estimate and visualize a correlation coefficient of 0.617 between the death rates for middle-aged white individuals in these two years, even after we control for a simple set of economic measures. We return to this correlation in Section 4.

affiliation in the 1970s and had much higher levels, similar to those of other groups, by the end of our sample in the late 1990s.

Panels (c) and (d) use our two measures of high religiosity. We find that both metrics decrease, consistent with declining religious participation. At the start of our time period, middle-aged less-educated white Americans were about 3 percentage points more likely to report weekly attendance of religious services than other groups. Both groups show declines in the late 1980s and 1990s, but for the middle-aged less-educated white group, the decline starts sooner and is much larger in magnitude. By the end of the decade, this group was no more likely to report weekly attendance than other groups. The decline in weekly attendance for the group represents a 32 percent decline from its peak value. These panel (c) results are qualitatively similar to those in figure 12.3 of chapter 12 of Case and Deaton (2020), although their results do not consider all respondents. We see similar patterns in panel (d), which shows that the share of middle-aged less-educated white individuals who reported a strong religious affiliation fell by 20 percent, a much larger decline than that for other groups.

Figure 2 shows that the rise in mortality was preceded by a decline in religiosity among the middle-aged less-educated white demographic. This change in religious participation and beliefs was large, concerns a phenomenon well known to be related to health and well-being, was driven by the same group whose mortality subsequently began to rise and occurred just before the increase in mortality. In appendix section A1 and figure A2, we show that similar results can be obtained using the American National Election Studies, or ANES. Results from this alternate dataset again show relatively large declines over the same time period for the same group of individuals: less-educated, middle-aged white Americans.

Why did religiosity decline in the United States? The origins of the decline in religious adherence have been touched upon by a number of works (Hout and Fischer, 2002, D. Campbell and Putnam, 2012, R. D. Putnam and Campbell, 2010); Hungerman (2020) discusses this research. Changes in income or economic advancement (e.g,. Buser, 2015) do not appear to be important drivers. Similarly, the onset of religious scandals may have lowered religiosity, but most major scandals occurred after the large decline began (Bottan and Perez-Truglia, 2015; Cools, 2020; Hungerman, 2013). The change may have been accelerated by demographic patterns (Voas and Chaves, 2016) in the US and by changes in educational attainment (Hungerman, 2014). But the seminal contribution of Hout and Fischer and subsequent works have concluded that this decline was importantly driven by responses to changes in the US political landscape in the 1980s, as religious and political affiliation became much more correlated than they had been earlier (cf. Chen and Lind, 2016). To quote Hout and Fischer: "The disaffinity of liberals and moderates for the social agenda of the Religious Right led the ones who had weak religious attachments to disavow organized religion"; Case and Deaton (2020) state that "large numbers of Americans seem to choose their religion to suit their politics" (see also Hout and Fischer, 2014 and D. Campbell et al., 2018). This US-specific basis for the decline is notable, as Case and Deaton's original study (2015) notes that the increase in middle-aged mortality was not observed in other countries.[15]

Changes in regulation may also have contributed to changes in religiosity. While the First Amendment of the US constitution prohibits Congress from making laws "respecting the establishment of religion," regulation can nonetheless affect religiosity indirectly, for example by affecting activities likely to compete with or substitute for religious observance. Perhaps the most studied regulation of this kind concerns blue laws. We exploit such regulatory changes in Section 3.2 to quantify the connection between religiosity and mortality outcomes. First, however, we consider descriptive patterns in attendance for different subgroups.

[15]In Section 4, we compare the U.S.'s changes in religiosity and mortality to those seen in other countries in more detail.

2.3. Demographic Patterns in Religious Participation by Race, Gender, and Location

In this section, we show that the decline in religious participation is also consistent with several other demographic patterns that characterize deaths of despair. Figure 3 considers weekly service attendance by race (all white respondents versus all nonwhite respondents) in panel (a), by gender in panel (b), and by urban/rural status in panel (c). The analogous trends in deaths of despair are shown in appendix figure A3.

In panel (a), the sample of nonwhite respondents in the GSS is small enough that the results have more noise, so we calculate a running average, pooling the results for respondents in each period t with those in periods $t-1$ and $t+1$. For purposes of comparison, we do the same with white respondents. The figure shows that the drop in attendance in the 1990s was stronger for white individuals. White respondents had higher weekly attendance rates at the start of this period and lower ones by the end. The decline for nonwhite respondents is smaller in magnitude. This fits with evidence finding a smaller rise in mortality for non-white groups during this period. (Appendix Figure A3, panel (a)).[16] As Ruhm (2021) notes, a "major challenge" in addressing recent drinking, suicide, and poisoning mortality patterns "is to explain why it is largely white Americans who have been so adversely affected, even though the conditions for non-whites have often been far worse and longer lasting." By documenting different changes in attendance between these groups, figure 3 offers a potential explanation.

Next, panel (b) examines differences by gender. Women consistently have higher rates of weekly service attendance than men, as has been previously documented. Both groups see similar drops in absolute magnitude occurring at about the same time (the women fall from .26 in 1990 to about .2 by the late 1990s, the men falling from .2 to around .15). We observe no evidence that these declines are driven by a particular gender. This again matches evidence on deaths of despair (appendix figure A3, panel (b)).

[16] See also figure 2 in Case and Deaton (2017)

Panel (c) compares trends for all respondents by urban versus rural status. The figure may suggest a slightly larger drop for rural respondents—this group had slightly higher attendance in the early 1980s (but not the 1970s), with this difference disappearing in the 1990s—but both groups show approximately similar declines. This again matches evidence on deaths of despair; Appendix figure A2, panel (c) shows similar trends in mortality for rural and non-rural areas, though the increase in mortality, like the decrease in religiosity, may be slightly larger in rural areas.[17]

2.4. Relationship between Religious Participation and Mortality by State

Figure 4 presents the correlation between the GSS religious attendance index from Section 2.1 and the mortality rate due to deaths of despair. The average attendance levels are mostly close to a value of 4 (which would represent monthly attendance), although there is variation across states. North Dakota and Tennessee have the highest levels of religious attendance (averaging several times a month) while California, a state sometimes regarded as relatively less religious than many others, has the lowest (average attendance is close to "several times a year"). In this late-1980s time period, the states with the highest deaths of despair rate are Florida and Arizona. The relationship between religious attendance and deaths of despair is negative, indicating that states with the highest attendance generally experienced the lowest mortality due to deaths of despair.

We can also relate the decline in religious attendance over time to the rise in mortality; figure 5 presents the changes in religious attendance against the changes in total deaths of despair for states from the late 1980s until 2000. This spans the period when both behaviors first exhibit large changes and is earlier than the time period considered by most studies on deaths of despair. Since the GSS does not survey residents of every state in every wave, we combine responses from surveys at the end of the 1980s and then combine responses from the

[17]Case and Deaton (2017) note that although media coverage suggests that these trends are a rural phenomenon, mortality increases have in fact been seen "at every level of residential urbanization in the United States" and reflect "neither an urban nor a rural epidemic, but rather both" (p. 409).

1998 and 2000 waves. We take the difference between the death rate averaged over 1998 and 2000 in the later period and the death rate averaged over 1986, 1987, and 1988 in the earlier period. The figure includes 32 states with usable GSS data. In appendix section A2, we construct similar figures to figures 4 and 5 by using the Longitudinal Religious Congregations and Membership File (LRCM). This is a voluntary survey of religious denominations that allows broad geographic coverage; results using the LRCM (in appendix figure A4) are qualitatively similar to the figures shown here.

There are three takeaways from figure 5. First, the levels of the figure fit prior work: attendance was generally falling, while overall deaths of despair were either close to zero or rising. The crowded upper-left quadrant of the figure shows states that see rises in deaths of despair and falls in attendance. Second, there is reasonably large variation in both axes: states saw widely divergent trends in both behaviors.

Finally, and most importantly, the *relative* comparison of states across the scatterplot suggests a negative trend—states that had larger drops in religious attendance had larger increases in deaths of despair. One might however hesitate to infer the size of the relationship between religiosity and deaths of despair from the figure since such variation across states could be driven by a multitude of factors. As Case and Deaton (2020) note, religiosity over long periods of time will respond to the the social and economic environment. In the next section, we discuss our use of blue laws to provide new evidence on how shocks to religiosity affect these measures of mortality.

3. Religious Shocks and Deaths of Despair

3.1. History of Blue Laws

We use regulatory shocks to relate changes in religiosity to deaths of despair. The shocks that we use concern the repeal of blue laws. Blue laws restrict commerce during a certain time of the week, frequently Sunday mornings.[18] Blue laws are much less common today than they were decades ago; most states had blue laws in the middle of the 20th century. Today, blue laws are often focused on a subset of commercial activities (for instance, alcohol sales, as in Carpenter and Eisenberg, 2009). The historical blue law changes we use imposed much broader restrictions, such as prohibition of all labor on Sundays. State codes governing alcohol sales are frequently different from those governing retail sales; cf. Lovenheim and Steefel (2011) and chapter 3 of Laband and Heinbuch (1987), which both illustrate the distinction between the two types of regulations in their treatment of Sunday sales. Laband and Heinbuch (1987) observe that "many of the states that have passed general restrictions on Sunday activities" nonetheless "single out specific activities for prohibition under separate statutes" and that "by far and away the most common type of restriction concerns the sale of alcoholic beverages" (p. 49). The variation in this paper focuses on repeals to the type of law prohibiting broad classes of activity, and these laws were generally separate from laws focused on alcohol.

In 1961, the US Supreme Court issued several decisions on the constitutionality of blue laws. The most significant of these was *McGowan v. Maryland*. This ruling held that blue laws could be found unconstitutional if their prohibitions were based "on grounds wholly irrelevant to the achievement of the State's objective." Many blue laws were subsequently challenged on this basis and found to be unconstitutional. Upon the repeal of blue laws, there was an increase in the opportunity cost of attending religious services on Sundays, the common day of worship for many in the United States.

The factors behind these challenges to blue laws and their degree of success varied from state to state. Some challenges involved lengthy court battles, while others depended particularly on the efforts of an individual either supporting or opposing the laws. Businesses within a state often disagreed about the benefits of blue laws, and Price and Yandle (1987) conclude that there is no clear association between state socioeconomic characteristics (such

[18] It is not known for certain why these laws are called "blue laws."

as labor force characteristics or labor union strength) and successful repeal; Gruber and Hungerman (2008) and Gerber et al. (2016) discuss this further. Raucher (1994) similarly argues that the repeal or lack-of-repeal of these laws does not appear to be driven by public support. Along with conflicting public sentiment, the circumstances around repeal differed in different states. As noted in Theuman (2005), in many instances the repeal of blue laws concerned the technical details of the laws themselves. Many laws had confusing or hard-to-rationalize lists of prohibitions, such as one law that allowed the sale of radios but not television sets (Theuman, 2005); by such criteria it was argued that the law was irrelevant to the achievement of a "State's objective".[19] The decision in *McGowan* thus generated subsequent court decisions that directly or (through legislative response) indirectly generated repeals in a number of states.[20] In some cases, a single person was critical in maintaining blue laws (e.g., Sydney Schlesinger, discussed in Lynch, 1978) or repealing them (e.g., Tom Moseley, discussed in Associated Press, 1984).

Several studies have considered the use of blue laws in empirical work on religion.[21] Research, including our results below, has found declines in religiosity as a result of their repeal. Studies, again including the results below, typically fail to find evidence of pre-existing trends in religiosity. That is, repeals do not appear to have been driven by reverse causation, where declines in religion led to repeal. Results in these studies are also typically robust to controlling for trends over time and to comparing parsimonious versus more demanding specifications.

[19]The numerous examples of court cases concerning blue laws (for instance, Theuman, 2005 and Paine, 1962-1963) suggest that they were often enforced. To the extent that blue laws were not enforced, our results will be biased toward finding no effect.

[20]Theuman (2005) §8a also notes that in some cases the separate treatment of alcohol and general retail activity led to circumstances where Sunday alcohol sales were permitted by law even while general commerce was not; this subsequently led to court cases challenging the general-commerce prohibition.

[21]Examples include Pope et al. (2014), Lee (2013), Cohen-Zada and Sander (2011), Hungerman (2011), Park (2018), Gerber et al. (2016), Goos (2005), Laband and Heinbuch (1987), and especially Gruber and Hungerman (2008). See also McMullin (2013) and Moreno-Medina (2021) for work attesting to the importance of secular costs on Sundays in religious attendance and the social importance of such costs. These papers suggest several mechanisms by which religion could matter for mortality (for instance, the fall in religiosity may increase alcohol use or decrease happiness, both of which could affect mortality). While we lack the data necessary to distinguish between all the mechanisms suggested in prior work, we provide several new results on mechanisms in Section 4.3.

One might wonder whether blue law repeals affected mortality by increasing alcohol consumption, since today many blue laws concern alcohol sales. Alternately, these laws may have affected economic activity, which then impacted mortality. Such results would be interesting as they would indicate an important policy based role for deaths of despair (cf Dow et al., 2020). But prior evidence and our own results suggest that the channel by with blue laws affect mortality is via religiosity.

First, as noted above the laws we use are not focused on alcohol sales, which frequently are subject to different regulatory code than other commercial activities. Our regulations are focused on general commerce instead. Moreover, second, such a story would indicate effects for both religious and non-religious individuals. But this is at odds with several papers that contain data on outcomes for religious and non-religious individuals and conclude that blue laws operate through changes in religious participation. For example, Gruber and Hungerman (2008) show increased heavy drinking following repeal, but that this occurs *only* among initially religious individuals, and Cohen-Zada and Sander (2011) consider several channels and conclude that blue laws' effects on happiness are driven by changes in religious participation (cf. also Lee, 2013). Using a different identification strategy based on weather, Moreno-Medina (2021) reevaluates work on blue laws and concludes "the effects are more related with the reduction in religious participation" rather than alternate explanations. In contrast, there is little evidence that blue laws matter significantly for overall economic activity and firms often disagree on their value (Goos, 2005; Jacobsen and Kooreman, 2005; Laband and Heinbuch, 1987).

Third, while we do not have data on deaths-by-religiosity, we build on the robustness tests of prior work with several extensions, by age and cause of death. In a seminal study, Ruhm (2000) shows that the relationship between economic activity and mortality is much stronger for those ages 20-44 than for those ages 45-64. We find much larger effects of blue law repeal on the latter age group. Next, our results on cause of death also fail to fit a story based on economic activity. Ruhm finds strong effects of economic activity on both heart disease and vehicle accidents, but in our estimates these effects have opposite signs, the former positive and the latter negative (most causes of death see no response at all). Fifth, we note that Ruhm (2000) finds *opposite* signed effects for liver disease and suicide, while our estimates consistently find these causes respond in the same direction to blue laws, although in some cases the results are imprecise. This is not consistent with a economic-activity effect but is consistent with blue laws working through an impact on religiosity if religiosity mediates risk for these causes of death (cf. Johnson et al., 2002, Gearing and Lizardi, 2009). Taken together, the evidence of our work and prior studies suggests blue laws operate by impacting religiosity.

3.2. Methodology

We also consider the robustness of our methodology, which is similar to a standard difference-in-differences approach with two-way fixed effects (TWFE). A recent literature, surveyed by Chaisemartin and D'Haultfœuille (2022), has noted that TWFE estimates can be affected by treatment heterogeneity when the adoption of treatment (here, the repeal of blue laws) is staggered over time. In this case, the effect estimated by the coefficient is a weighted average of each treated group's treatment effect, with some of these averages receiving negative weight (Goodman-Bacon, 2021). If treatment effects are heterogeneous, the impact of this weighting may be first order, in that the sign of all treatment effects and the sign of the regression coefficient differ. Our results are robust to consideration of these concerns.

Are the groups of individuals who respond to changes in blue laws the same as the ones driving the trends in deaths of despair? In many states, blue law repeals preceded declines in religiosity, and our work suggests that these repeals lowered religious participation. However, the decline in religiosity described in Section 2 is much larger in size than the effect we would expect from the shock to blue laws alone and was driven by a variety of factors. We assess this connection by considering the effect of blue law repeals by age group.[22]

We expect to find changes in mortality for the same groups that saw changes in religious participation. If one set of results were driven by one age group and other results were driven by a completely different group, it would raise concerns about robustness. In addition, evidence that the declines in religiosity from repeals occurred mostly among middle-aged Americans would suggest that the same age groups have similar responses to incentives across year-of-birth cohorts (the subsequent decline in religiosity in the 1990s was also driven by the middle-aged). This would fit with our evidence on life cycle and age effects in religiosity and the evidence from Voas and Chaves (2016).

However, the importance of religion for health outcomes may vary across the life cycle. For example, even if different age groups responded similarly to the repeals in terms of religious participation, the subsequent effect on their health outcomes could differ depending on the importance of religious participation in their social life. Middle-aged groups have smaller social networks than younger adults (Wrzus et al., 2013), so the marginal effect of a loss in religious connections may be more severe in this age group. It is thus plausible that middle-aged adults may drive the results across both the more recent time period in our descriptive analysis and the time period used in our analysis of the blue law repeals below.

3.3. Data and Specifications

As in prior work, we focus on states that experienced both statewide changes in blue laws and prohibitions concerning a wide variety of economic activities. Many states relied on local (for instance, city-level) blue laws and are not included. The list of states with repeals that fit our criteria is given in table 1. The top section of the table shows states that had broad, state-wide repeals of blue laws and lists the year of each repeal. The list covers a diverse set of states with varied timing in the repeal of the laws. Along with these states, our sample includes several states whose repeal preceded the period of study and several "never-adopter"

[22] We observe age, but not education, in both the GSS and the mortality data.

states that never had any statewide blue laws (Arizona, California, Colorado, Iowa, Idaho, New Mexico, Nevada, Kansas, Oregon, Washington, and Wyoming), giving us 24 states in total. For our work on blue laws, the sample covers the years 1969–2000. Our mortality data begin in 1969 (and the GSS starts in 1973); the last blue law repeal was in 1991.

The next two sections of the table list the data used with our blue laws specifications. First, the table reports information on the GSS dataset. The GSS's coverage of available states is reasonably good; it is missing Idaho, New Mexico, and Nevada, giving us 21 states in total. Next, we report the variables, means, and standard deviations of variables that we use from the GSS. In the bottom panel, we report these same elements for our mortality data. Here, we cover all available states. The control variable means in this panel are very close to the means of the same state-level controls in the GSS sample.

For our GSS analysis, which analyzes the effects of blue laws on religiosity, our baseline specification is:

$$Y_{ist} = \alpha + \beta \ repeal_{st} + \gamma X_i + \lambda X_{st} + \tau_t + \tau_s + \Gamma_{st} + \epsilon_{ist} \quad (1)$$

where Y_{ist} is an outcome measuring religiosity for individual i living in state s in year t, $repeal_{st}$ is a dummy that goes from zero to unity when one of our treatment states repeals its blue laws, X_i are individual-level controls, X_{st} are state-by-year level controls, τ_t is a set of year dummies, τ_s is a set of state dummies, and Γ_{st} are state-specific time trends. The last term ϵ_{ist} is noise. Since the regression is at the individual level but the key covariate is at the state–year level, we cluster by state to capture both geographic correlation and correlation over time in the residuals. We also estimate (1) for individuals in different age groups, as noted above. Given the sample size, we define middle-aged as ages 45–64 for these regressions; we also look at younger (aged 25–44) and older (65 and older) individuals. Given the small samples in the GSS, looking across age groups is more feasible than looking across educational groups or race, where we pool survey years even when presenting simple trends using all states in Section 2.

When we analyze the effects of blue laws on mortality, we aggregate our data into state×age group×race×year cells and then estimate the following regression using each cell as an observation:

$$Y_{grst} = \alpha + repeal_{st} + X_{st} + \tau_g + \tau_r + \tau_s + \tau_t + \Gamma_{st} + \epsilon_{grst} \tag{2}$$

Here, Y_{grst} denotes the mortality rate for people in age group g of race r in state s and year t, $repeal_{st}$ is again a dummy indicating blue law repeals, X_{st} is a vector of state-level controls, τ_g is a set of age group dummies, τ_r is an indicator for cells for white respondents, τ_s is a set of state dummies, τ_t is a set of year dummies, and Γ_{st} are state-specific time trends. Each group g is a five-year age group (cf. Case and Deaton, 2017), ranging from 25–29 up to 80–84. We use five-year age groups to minimize any effects of aging with a group over time.[23]

As in our approach with the GSS, we estimate equation (2) for all age groups as well as separate regressions for respondents aged 25–44, 45–64, and 65–84. We weight each cell by population, and we drop observations corresponding to the year of each state's blue law repeal because of the ambiguity of the policy change timing within that year. As with the GSS results, we cluster the standard errors at the state level.

4. Results

4.1. Effect of Blue Laws on Religiosity and Mortality

Table 2 presents the estimates from equation (1) and shows how blue law repeals affected religiosity in the GSS. Each coefficient is from a separate regression. The religiosity outcomes in this table are the same as in Section 2.2. The first two columns look at measures of low religiosity: attending religious services once a year or less and reporting weak or no

[23] The results with ten-year age bins are qualitatively similar. For US white non-Hispanic individuals aged 50–54, the average age increased by only 0.09 years (33 days) from 1990 to 2015 (Case and Deaton, 2017).

religious affiliation. The next two columns examine our measures of high religiosity: weekly attendance and strong religious affiliation. Our results differ from previous estimates on blue laws and religiosity by presenting heterogeneity by age.

The table suggests that blue law repeals led to both an increase in measures of low religiosity and a decline in measures of high religiosity. The last row shows the aggregate effect for all age groups: each measure of religiosity declines by 6–8 percentage points, suggesting reasonably large effects similar to those in past work.

For each outcome, the effect for the middle-aged group is larger in magnitude than the overall effect. After the repeal of blue laws, those aged 45–64 are 7 percentage points more likely to attend religious services once a year or less ($p = 0.134$) and 19 percentage points more likely to report weak or no religious affiliation ($p < 0.001$).[24] We find similar effects for measures of high religiosity. The middle-aged group is 9 percentage points less likely to attend weekly religious services after the repeal of blue laws ($p = 0.11$) and 11 percentage points less likely to report strong religious affiliation ($p = 0.08$). In most specifications, we find smaller point estimates for both the younger and older age groups, though our estimates are not precise enough to rule out similar effects across age groups except in the case of weak religiosity.[25]

Did this decline in religiosity correspond with an increase in deaths of despair? We begin this analysis with figure 6, which plots trends in mortality over time. The largest repeal of

[24] As shown in table 1, the baseline mean for self-stated "weak" religious affiliation is also larger.

[25] Appendix tables A1-A3 presents several extensions of these estimates. The first four columns in A1 use dummies that span all different categories of attendance. The all-ages results (and most of the age-group-specific results) show a decline in weekly attendance and an increase in attending once a year or less, and little change in other categories (although with repeated cross-sections, we cannot rule out that weekly attenders began attending monthly while monthly attenders began attending once a year and thus that these changes result in no overall effect in monthly attendance). There is some evidence of a decline in more-than-weekly attendance for the oldest age group, but the effect for all ages is much smaller and marginally significant. The next four columns consider effects on the strength of religious affiliation, some of which were shown in table 2 columns (2) and (4). Across groups, there is modest evidence of an increase in the share with no religious affiliation (a result similar in spirit to the findings in Gruber and Hungerman, 2008) with the largest point estimate for the middle-aged group. We find little change in the share of respondents reporting somewhat strong religious affiliation, which mirrors our effects for the intermediate attendance category. Appendix table A2 redoes table 2 but includes an interaction of repeal with a female dummy, allowing for differential responses by gender; A3 uses an rural dummy to allow for differential responses by urban rural setting. In both cases we find little evidence of heterogeneity.

blue laws occurred in 1985, when Minnesota, South Carolina, and Texas all repealed their laws. In this figure, we label these three states "treatment states" and then present the population-weighted average rate of deaths of despair in these states, along with the average for all the other states ("control states" in the figure) each year before and after 1985.

Figure 6 shows that these treatment states had lower death rates than the controls, but the difference in mortality between the two groups appears steady prior to repeal. Starting in 1985, the year of repeal, the two groups began to converge. The treatment versus control gap modestly fell immediately after the law changed by about one or two deaths per 100,000 and in the next few years fell by several more deaths per 100,000.

Figure 6 is appealingly straightforward but does not reflect the information from all the other instances of repeal in the data. Table 3 presents the estimates of equation (2) using the full sample of states. Each coefficient is from a separate regression. The table indicates that mortality rose for some age groups following repeal, and the pattern generally fits the pattern for religiosity in table 2: we observe the most robust results for ages 45–64, smaller effects for ages 25–44, and less evidence of an effect for the oldest age group. The results decrease with the addition of trends but are similar under both linear and quadratic state trends.

For the middle-aged group (ages 45–64), the coefficient on blue law repeals in the last column is 2.15 ($p < 0.001$), suggesting an increase in deaths of despair of about 2 per 100,000 due to the repeals. This is moderate but nontrivial in size; for this age group, the mean mortality rate for these causes is 51 per 100,000, suggesting a 4 percent effect. The marginally significant estimate for all ages in the last row, last column, suggests a similar proportional effect. This table has a larger sample size than the GSS and, accordingly, higher precision, but as with the GSS results, the estimates here are driven by middle-aged individuals.

Figure 7 presents a finer breakdown of the effects of repeal across ages. We plot coefficients and 95% confidence intervals for the repeal variable from a series of separate regressions using each age bin. The equation specification matches the baseline specification in table

3 except that the age-bin dummies are now dropped as they would be collinear with the regression constant. Panel (a) presents results without trends and panel (b) includes linear and quadratic trends. Overall, the figure shows that the effects of repeal are the largest among the middle-aged group, a pattern similar to the results in table 3; the magnitudes shown here are also similar to those in the table.

Table 4 redoes the estimates in table 3 but breaks down deaths by our three major categories: poisoning, suicides, and liver disease. The first column shows effects of all causes combined, repeating the result in table 3. For both all ages and for the middle-aged group, the estimates suggest a relatively strong result for suicide and positive but less precise results for other causes. The connection between religiosity and suicide has been considered since at least the work of Émile Durkheim in the 1890s; recent work has considered the role of religiosity in dealing with stressors related to suicide (for instance, Cooley Fruehwirth et al., 2019, shows that religiosity lowers the probability of depression for adolescents; see Iyer and Rosso, 2022, for a discussion of work on religiosity and mental health in a variety of contexts). While a relationship like the one in table 4 is thus not surprising, the lack of a strong effect on other causes is noteworthy. This underscores the idea, raised earlier in this paper (and also by other scholars) that the term "deaths of despair" may be useful in many contexts but may also mask important variation in the causes and nature of different causes of death. An example here is the lack of an effect on poisonings, given the role of poisonings (and in particular opioids) in recent mortality trends. Moreover, it is worth noting that supply-side factors related to opioid availability changed importantly after the period of blue law repeals. This could matter for the causes of death that relate religiosity to mortality; we return to this issue below.

The increase in mortality presented in tables 3 and 4 corresponds to about 2 additional deaths per 100,000 middle-aged adults in the years just after repeal. To benchmark this effect, we observed a decline of about 10 percentage points in weekly attendance for this group following the repeal of blue laws in the GSS in table 2. Panel (c) of Figure 2 showed a

10-percentage-point decline in attendance for *all* middle-aged white people without a college degree between 1986 and 1992. As shown in figure 1, deaths of despair for this group were about 5 per 100,000 over trend by 1995. If one were to take an implied mortality effect of two deaths per 100,000 and a 10-percentage-point attendance effect, then the observed 10-percentage-point fall in weekly attendance in the US would explain about 40% (2/5=.4) of the rise in mortality prior to the introduction of OxyContin.

Put differently, a 10-percentage-point effect on religious attendance implies that following the blue law repeals, about 10,000 out of every 100,000 middle-aged adults stopped attending services weekly. If mortality grew by 2 per 100,000 as a result, and assuming that the subsequent increase in middle-aged deaths came from this group, about 1 out of 5,000 of these of "marginal attenders" would consequently die from suicide, liver disease, or poisoning annually.

Our back-of-the-envelope calculations suggest that declines in religious participation can explain an important part of the initial increase in mortality due to deaths of despair. Of course, after the introduction of OxyContin in 1996, deaths of despair for middle-aged white Americans have increased dramatically both overall and relative to trend. Changes in opioid availability may be relevant both for interpreting our results on causes of death (as noted above) and the magnitude of any effect on overall mortality. We discuss this further in Section 4.3.

4.2. Robustness and Extensions

The previous section showed that blue law repeals were associated with an increase in mortality for the middle-aged; here, we present several extensions and robustness tests.

Appendix table A4 presents robustness tests of the main result using alternate specifications. The first two columns replicate our baseline estimate from table 3. The table then re-estimates the main result for the middle-aged group using the mortality rate in logs; the results are qualitatively similar to the original estimates. Next, the table presents results that include not only state-specific but also age-group-specific time trends; again, the results are quite close to the main estimates. In their original article, Case and Deaton (2015) note that trends in middle-aged mortality could interact with Medicare. During the period of analysis here, Medicare and Medicaid both expanded, and the latter program in particular varied at the state level and could affect the health outcomes of middle-aged Americans. In columns 7 and 8 we redo our main estimates with controls for per-capita Medicaid and Medicare spending; the results are unchanged.[26] Changes in opioid treatment also do not fit the patterns in our data; naloxone was almost exclusively used in a hospital setting until the late 1990s and Narcan was approved by the FDA as a nasal spray in 2015, both after our analysis (Britch and Walsh, 2022). Lastly, the table presents estimates that, instead of including state fixed effects, add a dummy for each state-by-race-by-age-group; these absorb the standard state, age, and race fixed effects. Again, the results are close to the baseline estimates.

Next, appendix table A5 presents results by race and cause of death. The results for white individuals are close to the main estimates.[27] The results for the nonwhite group are perhaps surprisingly qualitatively similar, but these estimates are often more sensitive to whether and how trends are included (losing statistical significance under the basic trend specification) and are generally much less precise, making firm conclusions difficult to draw.[28] The sensitivity and precision of the estimates for the middle-aged nonwhite group may in

[26]Case and Deaton (2015) also discuss their trends in comparison to the AIDS epidemic. Scholars have concluded that the epidemic has limited explanatory power for trends in deaths of despair (Meara and Skinner, 2015). The onset of the epidemic disproportionately affected blacks in the early 1990s, which does not fit the trends we discuss, and large declines in AIDS mortality did not occur until the late 1990s (Singh et al., 2013) which is after the reforms considered here; below we show that our results are robust to limiting the sample to years close to our reforms.

[27]Estimates that allow for separate effects for above- and below-median urban population (based on the fraction of each state's 1970 population living in an urban area) are also typically close to the main estimates for both groups; the results are sometimes stronger for rural states but this is sensitive to specification. This fits with our earlier findings that religiosity declined and mortality increased in both urban and rural areas.

[28]The estimates for the nonwhite group can also be sensitive to the specification; for example, redoing the estimate in column 1 of appendix table A5 but using the logged death rate instead of levels produces a coefficient of .018 (.035) for nonwhite individuals but .037 (.011) for white individuals. The results for the middle-aged nonwhite respondents in the GSS, where we have only a few hundred observations in total, are often extremely imprecise.

part be due to their focus on a small set of mortality outcomes for a relatively small group.

We also explore the effect of blue laws on other causes of death. Interpreting the results on the other causes is complicated, as religiosity may directly affect behavior through many channels and hence affect many causes of mortality. In addition, even if the only direct mortality effects concern deaths of despair, there could be a competing-risks effect whereby changes in these deaths subsequently affect other death rates—for instance, if those who die from suicide were more likely than others to die in accidents, there could be a second-order effect on this latter outcome. With these caveats in mind, figure 8 presents results on several other common causes of mortality. We include the top 15 causes of death according to broad categories constructed by the CDC.[29] Rates for these other causes of death vary in magnitude much more than for our categories of deaths of despair, so we present estimates using logged rates, making the coefficients proportional across categories (results in levels are qualitatively similar).

For deaths of despair for the middle-aged, the estimates in this figure are similar to our main estimates. In fact, in this case, poisoning mortality is statistically significant. When we look at other causes of death, all of the coefficients are smaller than those for poisoning and suicide; one is positive and significant, one is negative and significant (heart-related and motor-vehicle deaths, respectively), while ten others are insignificant. Results using all ages produce one positive and significant result (breast neoplasms), one negative estimate (motor vehicle deaths), and ten insignificant estimates. As noted earlier, these results do not reconcile easily with past work on mortality and economic activity. But they are consistent with the GSS results earlier, and prior work on blue laws and religiosity.

Appendix table A6 shows results dropping each state one at a time from the main estimates. The top set of estimates excludes trends, and the bottom set includes them. The estimates are reasonably similar for each state dropped, with the largest drop in the coefficient coming from California in the top group and South Carolina in the bottom group. The

[29]We base our coding on the 34 Cause List Recodes given in the Multiple Cause of Death files code books and combine categories when necessary to ensure the best linkage possible across ICD-8 and ICD-9 codes.

results are not driven by any one state.[30]

Table A6 raises the issue of heterogeneity in treatment effects. As noted earlier, a recent literature has raised concerns that heterogeneity in treatment can, in a TWFE model, improperly weight treatment effects, so that a simple regression can produce a first-order biased estimate. This improper weighting is driven by the combination of (a) heterogeneity in treatment and (b) negative weights that are generated from so-called "forbidden comparisons," where the difference between newly-treated and already-treated groups are improperly used in the regression when the treatment effect is estimated.

We consider several responses. First, Jakiela (2021) observes that an implication of homogeneity in treatment is that the relationship between $\tilde{Y}_{grst}$ (the residuals of the outcome variable Y_{grst} in a regression excluding the treatment dummy) and the weights $\widetilde{repeal}_{st}$ (the residuals from regressing the treatment dummy on the other controls) is linear. Appendix figure A5 shows four estimates of the relationship between $\tilde{Y}_{grst}$ and $\widetilde{repeal}_{st}$. Panels (a) and (b) are from regressions without state trends, while panels (c) and (d) include linear and quadratic trends. The top row of the figures is fitted at decile values of the x axis (which is $\widetilde{repeal}_{st}$), and the bottom row uses percentile values (so that the top row figures have a smaller x-axis range and less noise in the tails). All the figures show a relationship between the two sets of residuals that is mostly increasing, consistent with largely homogeneous treatment effects.[31]

[30] We also explored geographic heterogeneity driven by abuse allegations against priests (for instance, Moghtaderi, 2018; Frick and Simmons, 2017; Bottan and Perez-Truglia, 2015; Dills and Hernández-Julián, 2012; Dills and Hernández-Julián, 2014; Carattini et al., 2012; Hungerman, 2013). (We thank Nicolas Bottan and Ricardo Perez-Truglia for generously sharing local scandal data.) Using the timing and location of these allegations as exogenous shocks to religiosity, we did not find a consistent relationship between allegations of abuse and mortality outcomes; our estimates were often imprecise and/or sensitive to the specification. Esparza (2020) relates abuse allegations to several outcomes including mortality and argues that allegations of abuse lead to higher death rates. His measure of allegations is based on a binary indicator for high-abuse dioceses, and his mortality data censor county–years with fewer than ten deaths of despair. Our analysis considered the effects of allegations in alternate, more flexible specifications and used restricted mortality data that allowed us to include years with few or no deaths of despair. While our analysis was not conclusive and is omitted from this paper, we note that the only other evidence on this relationship (Esparza, 2020) produces results consistent with the conclusion in this paper: negative shocks to religiosity can lead to greater deaths of despair.

[31] One could also perform a simple test of linearity by regressing the $\tilde{Y}_{grst}$ on $\widetilde{repeal}_{st}$ and the quadratic term $\widetilde{repeal}^2_{st}$. Both with and without trends, such a regression produces a positive and significant linear

Next, we consider estimations of the treatment effect with potential treatment heterogeneity in mind. In figure 9, we present estimates following an approach proposed in Callaway and Sant'Anna (2021). This method calculates average-treatment-on-the-treated (ATT) effects for each post-treatment group by comparing their outcomes to control group outcomes and avoids using negative weights. Panel (a) uses all observations for states that have not repealed blue laws as control groups, and panel (b) uses only never-adopters.

Each panel in the figure shows little evidence of a pre-trend prior to repeal, suggesting there was not any significant anticipatory change in mortality outcomes immediately before repeal. Each figure then shows a positive and increasing effect on mortality following repeal. The treatment effect is qualitatively similar to the one implied earlier in figure 6. One can further aggregate these treatment effects over time into a single post-treatment ATT; the overall ATT for panel (a) is 6.59 (se = 2.40) and for panel (b) is 7.50 (2.18). These estimates, which do not include trends or controls, are reasonably close to the most comparable estimate of 5.32 (0.98) from the second row of column 1 in table 3. (Redoing the regression estimate in table 3 without any controls at all produces the slightly larger but still similar estimate of 10.7 (3.35)).[32] Overall, then, results similar to the main estimates can be obtained with the DiD-alternative method proposed by Callaway and Sant'Anna (2021).[33]

Jakiela (2021) and Chaisemartin and D'Haultfœuille (2022) both observe that in a setting where treatment turns on and stays on, negative weights are more likely to occur as the average number of treated groups grows. This suggests that results driven by effects far after treatment should be considered carefully to make sure that they are not subject to

coefficient and a statistically insignificant coefficient on the quadratic term. A regression of the mortality residuals on the blue law repeal residuals and the square of the blue law repeal residuals in the no-trends case produces coefficients of 5.5 (se = 0.98) and 4.6 (2.8), respectively; for the with-trends case, the coefficients are 2.15 (.67) and -1.77 (2.16).

[32]It is possible to include controls with Callaway and Sant'Anna's method, but in this setting, the results typically become very imprecise; for instance, the overall ATT from redoing panel (b) in figure 9 with controls added is 3.0 (22.9), in comparison to the estimate from the closest specification in column 3, row 2, of table 3 of 2.15 (0.67).

[33]Estimates applying this method to attendance and affiliation in the GSS are presented in appendix figure A6 and are qualitatively similar to the regression estimates shown earlier; the results in figure A6 are notable for their absence of any pretend in religious affiliation or attendance.

negative weights. Such effects are potentially consistent with figure 9, although it is worth reiterating that this figure is *not* a standard event study figure but instead is produced with methods robust to negative weights.

We explore robustness to treatment windows further in table 5. In column 1, we limit treatment-group observations to the first three years following repeal, and we omit any state trend controls. If this produced very different results from the trend-based estimates earlier, it could be taken as evidence of a role for negative weights late in the sample and/or concerns about the functional-form role of the trend controls. However, the results are similar to our baseline estimate for the middle-aged group from column (3) in table 3 of 2.15. The next column expands the included treatment group sample to 10 years post-repeal and finds slightly larger estimates (which fits with the dynamic estimates in figure 9), but overall the results are again reasonably close to the main sample estimates in table 3 with trends. The third column further limits the treatment group to observations within 3 years of either the pre-treatment or the post-treatment period (that is, 3 years before or after repeal), in a spirit similar to the approach used for the Callaway and Sant'Anna estimates. Column 4 does the same with a 10-year window. The results are again similar.

Finally, the last result in table 5 presents a different test for the role of negative weights, exploiting the fact that negative weights are not only more likely to occur far after treatment but are also more likely when a group is an "earlier adopter" (in our setting, this means an observation from a state that repealed its blue laws earlier in our sample period). In this estimate, we redo the main results in row 2, column 3, of table 3, but we remove from the sample four early adopter states: Florida, Iowa, Kansas, and Washington (by design, these states are not used in the estimates in figure 9). Different results here would raise concerns about heterogeneity or the role of negative weights, but once again the result is similar to before, which is not surprising given the appendix results showing that the estimates are robust to dropping different states.

To summarize, we consider estimates produced under a variety of specifications and estimation methods, including those focused on recent difference-in-differences concerns. There is some evidence of an increasing effect of blue law repeals on mortality over time, but notwithstanding this, there is no evidence that our results are driven by the type of problematic variation that can bias difference-in-difference estimation. Moreover, none of the estimates suggest a negative relationship between the repeals and mortality, and most results adhere closely to the baseline estimates in table 3.

4.3. Channels and Context

The prior section documents a relationship between a negative shock to religious participation and an increase in middle-aged mortality; here we consider channels through which this relationship might have operated and then discuss the importance of our late 20^{th} century US context. We begin by estimating the effect of blue law repeals on other reported social behaviors and beliefs in the GSS. Table 6 reports the estimates from equation (1) for different age groups and alternate outcomes.

The first set of columns investigates whether blue laws affected behaviors related to alcohol consumption. None of the age groups, including the middle-aged, were significantly more likely to visit bars (column (1)) or engage in safe levels of drinking (column (2)) after blue laws were repealed;[34] this is consistent with the fact that the law changes we consider were not focused on alcohol sales. However, repeals did result in an increase in unsafe drinking for the middle-aged. Column (3) shows that this group was 9 percentage points more likely to report sometimes drinking too much (p <0.01).

Alternately, a negative shock to religious participation could increase risky behavior by reducing measures of social capital, such as social connections to other groups that are themselves beneficial, or trust in institutions or others.[35] Columns (4) and (5) explore

[34]We define safe drinking as reporting that they drink alcohol but they never drink more than they should. Unsafe drinking is defined as sometimes drinking more than they should.

[35]R. Putnam (2000) notes that "Religious institutions directly support a wide range of social activities well beyond conventional worship" and are an incubator for "civic norms, community interests, and civic recruitment"; see his chapter 4 and see also Gerber et al. (2016). Hawe and Shiell (2000) give an introduction

whether blue law repeals led to changes in socialization in general; we consider changes in trust and confidence in social institutions in columns (6) and (7). None of the age groups were statistically significantly less likely to socialize with their neighbors after repeal (column (4)). Column (5) combines the neighbor and bar socializing outcomes with socializing with friends or family; the coefficient for this aggregate measure of socialization is small (relative to a mean of 10) and insignificant for all groups.[36] Similarly, columns (6) and (7) show no significant change in trust or confidence in intuitions. Collectively, these columns do not provide evidence for declines in other measures of civic engagement or trust in society.

Religiosity might also affect well-being by reinforcing personal beliefs which help individuals cope with adverse shocks (cf. Bentzen, 2019). We test for this possibility in columns (8) and (9) of table 6 and find some support for it.[37] Blue law repeals decreased belief in the afterlife among the middle-aged by 10 percentage points (p =0.10). However, there were no significant effects on prayer either overall or among the middle-aged. This lack of change in prayer fits with evidence that the circa-1990 fall in religious participation did not coincide with a concurrent decline in personal spirituality. Belief in "God or a universal spirit" has remained constant at 95 percent for Americans in the Gallup polls since 1976 (Bishop, 1999). The results on unsafe drinking and belief in the afterlife are also internally consistent—evidence from the World Values Survey shows that belief in the afterlife is associated with a reduction in risky behavior such as driving under the influence (Atkinson and Bourrat, 2011).[38] Relatedly, it is possible that blue laws' effects on belief in the afterlife

to work on social capital and health.

[36]These results are evidence against the possibility that individuals substituted out of religious participation and into other types of civic engagement (in which case we might expect positive coefficients for other civic engagement). It is worth noting that this possibility would still be consistent with declines in health; Case and Deaton (2020) (p. 177) state that alternatives to churches "may not provide the reassurance or unquestioning acceptance that comes in mainline churches whose rituals and traditions have been familiar since childhood, have provided succor in time of trouble, and have done the same for previous generations."

[37]We thank a referee for suggesting that we test for an effect on belief in the afterlife.

[38]Both these measures are self reported in the GSS and are perhaps subject to reporting bias, although we note they offer different responses to blue laws not only from each other but also from the attendance and affiliation results earlier. Moreover, the LRCM results in the appendix are not individuals' self-reported survey responses, nor are (e.g.) the financial data studied in Gruber and Hungerman (2008), and these data produce results qualitatively similar to the self reported GSS data.

could alter beliefs on suicide and related outcomes (Stack, 2013, see also Sharp, 2019), which could be consistent with our results on suicide in table 4.

Religiosity may also affect mortality by facilitating insurance against negative shocks (Dehejia et al., 2007, Ager et al., 2015) or proscribing unhealthy behaviors. These channels are notoriously difficult to test, but suggestive evidence can be taken from table 2 earlier. Building on the canonical model of religiosity in Iannaccone (1992), Hungerman (2014) notes that if a secular shock causes a decline in religiosity among relatively highly religious individuals (as opposed to the moderately religious), then a subsequent increase in risky behavior is evidence that religious institutions matter.[39] Table 2 shows that the decline in religious participation here is driven by a fall in weekly attendance and strong affiliation (rather than a decline among the moderately religious, in which case these measures of high religiosity would have been unchanged), suggesting that religious institutions and proscriptions may mediate the behavior of the relatively religious.[40]

Table 6 and the earlier results thus provide some evidence that religiosity may matter both because of the institutions and incentives faced by participants and because it fosters belief in the afterlife. However, perhaps surprisingly, there is no evidence that changes in religiosity lead to changes in health by affecting other types of social capital. We consider this further in table 7; this table shows cross-sectional correlations between the measure of religious attendance used in figure 4 and different circa-1990 measures of social capital used to make the social-capital index described in Rupasingha et al. (2006). The left column lists measures of social capital that have a positive correlation with attendance; the right column

[39] Intuitively, if proscriptions or other features of religiosity did *not* matter, then a drop in religiosity by the highly religious who ex-ante avoided risky acts should not lead to an increase in risky behavior.

[40] This raises the possibility of comparing outcomes across different religious traditions, an idea that for suicide has been pursued as far back as the pioneering work of Durkheim (1951 (1897)). In appendix table A7 we redo the estimates in table 2 comparing Protestants and Catholics (finer breakdowns are not feasible given the size of the GSS), and we are unable to reject similar effects for both groups. We also investigated whether the main mortality outcome for blue laws repeal varied by more or less Catholic states by adding an interaction of blue laws repeal with a dummy for whether a state has an above-median Catholic population (based on OCD data in 1981), but again found no consistent evidence of a difference between above- or below-median Catholic states. Finally, breaking figure 5 apart by above-median and below-median Catholic states produces a negative association for both groups. Our findings suggest similar results for Catholics and Protestants.

lists measures with a negative correlation.

The strongest association is a positive one between attendance and religious associations.[41] But there are a large number of measures of social capital negatively correlated with attendance. This is consistent with table 6, which found little association between blue laws and other measures of social capital (although this consistency is not foregone, as table 7 shows a relationship in the cross-section while the earlier table depicted responses to a religious shock.) This is also consistent with the idea that different notions of social capital deserve separate treatment when considering health outcomes, an idea emphasized in Hawe and Shiell (2000). As discussed in Section 2.2, prior work has argued that the decline in American religion in the 1980s was partly in response to political trends; while some activities (such those related to sports and recreation) appear in both the left and right columns, all four of the measures related to political participation in this social capital index are negatively correlated with religious attendance.

This raises the issue of whether and how changes in religion and health were mediated by circumstances of the United States. Prior work both on mortality and on religiosity has considered the extent to which the US context is unique, with scholars often arguing that recent trends in mortality and religion may be different in the US than other countries, particularly in Western Europe. In appendix Table A8, the first ten columns present data from Table 3.5 in Norris and Inglehart (2004), who use data from the Mannheim Eurobarometer Trend File to create trends in religious attendance for European countries from 1973 to 1994 (three countries in their table lack data before 1980 and are discussed in the text below table A8). The last column reports calculations from the GSS.[42] Reported attendance in the US increased from 1973 to 1985; many European countries saw large declines over this period.

[41] One would not expect a perfect correlation between them however. The religious organizations variable (and most others) come from the US Census Bureau's County Business Patterns (CBP) dataset (see Rupasingha et al., 2006). Many religious groups may not be observed by the CBP and it has been argued that the CBP measure undercounts the number of congregations in the US (cf. Grammich, 2023.)

[42] As noted below the table, to make the GSS column as comparable as possible to the Eurobarometer data, the measure of attendance here differs slightly from Figure 2 earlier, but redoing Figure 2 using the measure of attendance here produces similar results.

Voas (2009) argues that these declines extended back even further, in many cases to the first half of the 20^{th} century. The table shows variety in attendance across countries at each point in time.

In appendix Figure A7, we show trends in middle-aged deaths of despair from 1979 to 2001 for the countries used in Table 3.5 of Norris and Inglehart (2004). The construction of these mortality rates is described in appendix Section A3. As with Table A8, there is variety across countries at any point in time. Many countries show steady, and frequently declining, mortality rates over this period. But there are exceptions; for example, mortality rises in the United Kingdom, which reflects high growth in liver disease that has been noted by prior scholars (e.g., Leon and McCambridge, 2006) and appears to have been driven by changes in retail and consumption patterns of alcohol. This led to new regulations in the sale of alcohol in the UK (see Hilton et al., 2014 and Williams et al., 2014); these retail and drinking patterns were distinct from contemporaneous patterns in the US (cf. Greenfield et al., 2000). While not featured in appendix Figure A7, it is also known that Eastern European countries at the end of the 20^{th} century saw large increases in mortality, including from liver diseases, poisoning, and suicides, among other causes (see King et al., 2022, and the end of chapter 8 in Case and Deaton, 2020).[43] In terms of its trends in religious attendance, trends in mortality, and the context in which these trends took shape, the US differed from many other countries.[44] Beyond the previously-mentioned political landscape of the US,

[43]The figure also includes mortality rates for Canada. US and Canadian mortality rates are virtually identical during the mid 1980s; trends then flatten in both countries in the late 1980s and onward, but the US experiences a larger relative increase, rising above Canada during this late 1980s period and remaining above it thereafter. A discussion of whether religious attendance broke trend in Canada in the late 1980s is hindered by data limitations. Using a retrospective survey-based approach, Frank and Iannaccone (2014) suggest that Canada may have experienced a sharp decline in attendance in the late 1980s, but their data does not extend past 1990. The Canadian GSS begins in 1986 and shows falling attendance in the late 1980s (Eagle, 2011). In the World Values Survey, weekly attendance in Canada is falling from 1982 (the first year observed) to the early 2000s. While a lack of continuous data make conclusions about a trend break difficult, the existing evidence is consistent with declining religiosity in Canada around the same time period as the United States. However, investigation of a causal connection between attendance and mortality in Canada is hampered by the lack of a blue laws type shock.

[44]In fact the same could be said for many countries. As Voas and Chaves (2016) note on the topic of secularization, "every country's experience of secularization is unique" and discussion of secularization "always requires a combination of the general and the particular"; see also Wilkins-LaFlamme (2021).

what are other important aspects of the US context in the late 20^{th} century? First, it is well-recognized that the US at the end of the 20^{th} century had a smaller welfare state with less redistribution than many other advanced countries (Alesina et al., 2001). As Case and Deaton (2020) write (p 179), "religion helps people do better, and they espouse religion in part *because* their environment is difficult" and that if religiosity declines "people lose the benefits that religion brings" (emphasis in original). If religious attendance allows access to goods that can substitute for goods provided by the welfare state, as suggested by Gill and Lundsgaarde (2004) and the above-mentioned work on religion and insurance, then a shock to religious participation in a country with a weaker welfare state could be more detrimental than a shock in a country with a stronger welfare state.

This matters not just for comparing the US to other countries, but also for comparing our blue laws evidence with our time series evidence. A religious insurance mechanism is an example of how religious participation can create positive external benefits for other adherents; this idea is foundational to work on religious communities in recent decades (cf. Iannaccone, 1992). The overall decline in religious participation in the 1990s was large relative to the decline from a shock like blue law repeals and may have created spillover effects where religious communities experiencing large drops in membership saw declines in external benefits such as their capacity to provide mutual insurance. In this way, larger shocks may have proportionally greater consequences than smaller shocks. Our blue laws estimates may in that case understate the relationship between declining religious participation and mortality observed in the 1990s.

Our blue laws estimates may understate the relationship between declining religious participation and mortality for another reason: the availability of opioids. There is a positive correlation between state mortality rates from the mid-1990s and the mid-2010s (shown in figure A1), suggesting that culture or other long-lasting traits matter for mortality. This also suggests that an analysis of culture in one time period may be informative in others; e.g. observed increases in drinking behavior might translate to other destructive decisions in other time periods. At the risk of repeating ourselves, this observation does not gainsay the massive "supply-side"-driven effects on mortality that opioid diffusion had, and our goal is not to explain or characterize fully the opioid crisis of the 21^{st} century. Moreover, a story where declines in religiosity led to an increase in risky behavior while changes to the market for opioids provided a new source of risk is outside the time period of our blue laws analysis. The last state to repeal its blue laws was North Dakota in 1991 (and the last changes before that were in 1985), well before both the introduction of OxyContin and its reformulation in 2010 (Alpert et al., 2022, Evans and Lieber, 2019). But we note that if those dropping out of religious participation after the period of our analysis had newly gained, relatively easy access to an addictive and dangerous narcotic, then the mortality effects for these individuals may have been larger than for those leaving in the pre-opioid period.

Lastly, as we have shown, the US shock to religious participation in the late 1980s was driven by a particular sub group (less educated white Americans), it occurred relatively rapidly, and it was driven by declines in relatively high levels of religious participation. For those affected by this decline, the experience may have been more dramatic than the effect of a more gradual decline in moderate attendance would have been, and this may also have encouraged deleterious health outcomes. The idea that social shocks themselves can matter for mortality was considered by Durkheim, who observed that "Whenever serious readjustments take place in the social order," individuals "are more inclined to self destruction" (Durkheim, 1951 (1897), p. 246).

Overall, our results support the idea that declines in religiosity mattered because of the inherent qualities and social attributes of religious participation. Our results also support the idea that different types of social capital vary across place and respond to shocks in different ways; we find no evidence that shocks to religious participation generated subsequent declines (or increases) in other types of social capital. Rather, prior work suggests that political forces may have encouraged religious disaffiliation, and the timing, size, concentration on a particular demographic group, and institutional context of this change in the United States at the end of the 20^{th} century may have exacerbated the effects of this disaffiliation on well-being.

Finally, these results raise the question of whether a return to participation in organized religion—or perhaps participation in other, secular community organizations—could help to reverse mortality trends. To our knowledge, findings on this point have so far been pessimistic. We know of no evidence that the general declines in community participation documented in R. Putnam (2000) have been reversed. The 21st century rise of social media, which has been associated with lessening attachment to one religious tradition (McClure, 2016) and worse mental health (Braghieri et al., 2022) may further hinder a reversal. Moreover, even if these trends were reversible, the literature suggests that the primary benefits of religious participation for life satisfaction are difficult to replicate with other forms of social engagement (Lim and Putnam, 2010).

5. Conclusion

This paper explores the importance of culture in explaining late 20^{th} century trends in US mortality and the relationship between cultural institutions and well-being more generally. As noted by Ruhm (2021), the discussion of causes of these mortality trends should consider whether any social phenomena coincide or predate the changes in mortality. We show that the initial rise in deaths of despair in the US was preceded by a large decline in religious participation and that both trends were driven by white middle-aged Americans. This decline in religious participation was seen for both men and women, and was seen in both rural and urban settings, but was driven primarily by middle-aged, less educated white individuals. The decline in religious participation matches mortality trends in all these characteristics. We also show that religious attendance and the rate of deaths of despair are negatively correlated across states; this negative relationship holds when we consider changes in religious attendance and mortality. States that experienced larger decreases in attendance have had the largest gains in the rate of deaths of despair at the end of the 20^{th} century.

Using shocks based on the repeal of blue laws, we then demonstrate that negative shocks to religiosity had relatively large impacts on deaths from poisonings, suicides, and liver cirrhosis for middle-aged Americans in the late 20^{th} century. These effects are compatible with a decline in religiosity affecting health through declining personal belief as well as declines in religious institutions, proscriptions, and mutual insurance. The decline also reflects the unique circumstances of the US in the late 1900s.

These results underscore the importance of cultural institutions such as religious establishments in promoting well-being. Whether other types of voluntary or community activities could have similar large-scale effects on health outcomes is unknown and represents an excellent topic for future research.

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6. Appendix

A1. Supplementary Analysis using the ANES

This analysis uses the American National Election Studies, Cumulative Data File, 1948-2004, made available by the Association of Religion Data Archives at www.thearda.com. The ANES is a bienniel nationally-representative cross section survey that has been conducted since 1948. The cumulative-data version of the dataset was prepared by the ANES Staff and merges into a single data file cases and variables from each of the biennial American National Election Studies. Our analysis goes from 1970 to 2004. In obtaining this data, we noted that thearda.com offers both a text-formatted version of the data and an Excel-formatted version, and the former was missing data from 1998 onwards; our analysis uses the data provided in the Excel-format. The ANES results are weighted. Using alternate post-stratification weights or no weights at all produces almost identical results.

The ANES attendance question breaks attendance responses down into six categories: 1) Every week; 2) Almost every week; 3) Once or twice a month; 4) A few times a year; 5) Never; 6) No religious preference. To make our responses as comparable as possible to the GSS results, we define weekly attendance as one for those responding "every week" and zero otherwise, and never attend as one for those who answer "never" or "no religious preference" and zero otherwise. The response of less than once a year in the GSS is not available in the ANES, the next highest level of attendance is "a few times a year"; this difference in how the surveys code low attendance may lead the prevalence of low attendance in the ANES to be slightly lower than in the GSS.

Two facts should be kept in mind when considering the ANES data. First, this survey is smaller than the GSS and many states have small (e.g., less than 20 observations) sample sizes in a given year, making the replication of figures 4 and 5 problematic. Second, in 1990 the ANES changed the wording of its attendance question in two ways. First, the question was moved to before questions about affiliation, and second, the question clarified that funerals, weddings, and baptisms were not being asked about. While it is not obvious if or how such questions would differentially bias answers from the less-educated white group (unless they already had falling levels of attendance, which would fit with the main results), the weekly-attendance and the never-attend figures both appear to show large changes in 1990.[45]

With these caveats in mind, figure A2 shows trends in weekly religious attendance and never attending for all respondents, white respondents, white respondents without a college degree, and whites without a college degree ages 45-64. The figures calculate the mean value of these variables in each year from 1970 to 2004; given the small samples, the figures are smoothed by using a three-year running average of these means. Panel A shows the fraction reporting that they never attend religious services, and panel B shows weekly attendance.

Both figures present results consistent with the main estimates earlier: in the early 1980s less-educated middle-aged whites had higher levels of religious participation than other groups, but this had changed by the end of the 1990s. Overall, the ANES has several limitations relative to the GSS, but this notwithstanding, both datasets show relatively large declines over the same time period for the same group of individuals: less-educated, middle-aged whites.

A2. Supplementary Analysis using the LRCM

This analysis uses the 1990 and 2000 waves of the Longitudinal Religious Congregations and Membership File (State Level), made available by the Association of Religion Data Archives at www.thearda.com. These are decennial datasets attempting to measure religious membership in the United States. We focus on the years 1990 and 2000; in both years this data in this dataset was collected by The Association of Statisticians of American Religious Bodies. As discussed in Bacon et al. (2018), challenges with using this dataset include the

[45] This matches output produced by ANES itself; see for example the ANES figure "Church Attendance, 5 Categories" at https://electionstudies.org/data-tools/anes-guide/anes-guide.html?chart=religious_attendance_5_cat

fact that the religious groups participating in this purely voluntary survey changed over time, so that comparisons over time may reflect changes in participation in the survey rather than participation by adherents. Moreover, several groups also changed how they counted membership, although several of the largest groups to do so changed in 2010, which is after our period of analysis (these include the United Methodist Church, the Catholic Church, and the Church of Latter Day Saints). Traditionally Black Protestant groups have low levels of participation. Given these issues, when using this data we focused on groups that report adherent levels in both 1990 and 2000.

Adherent totals are divided by the state population in each case. While the data is reported by religious groups themselves, there is still likely measurement error. Most notably, for Rhode Island there is an extraordinarily large change in adherence of over 12 percent of the entire state population during this period. This was driven by a large reported fall in Catholic adherents. The 1990 LRCM Rhode Island numbers for Catholics match almost exactly the numbers given in the Official Catholic Directory, but the 2000 LRCM numbers are much smaller; we replaced the 2000 Catholic RI numbers using the numbers from the Official Catholic Directory. Unlike the GSS figures, which capture both extensive and intensive margin changes in adherent behavior, the data here only capture extensive margin changes.

Figure A4 shows a cross-sectional across-state comparison of adherent rates (the x axis) and deaths of despair per 100,000 (the y axis) in 1990. Panel B reports the change in these values from 1990 to 2000, as in Figure 5 in the main text. While the data here is less reliable than the figures used in the main text, more states can be included. Both figures show a negative relationship that is qualitatively similar to the figures in the main text, suggesting that higher adherence rates are associated with lower levels of deaths of despair.

A3. International Mortality Trends

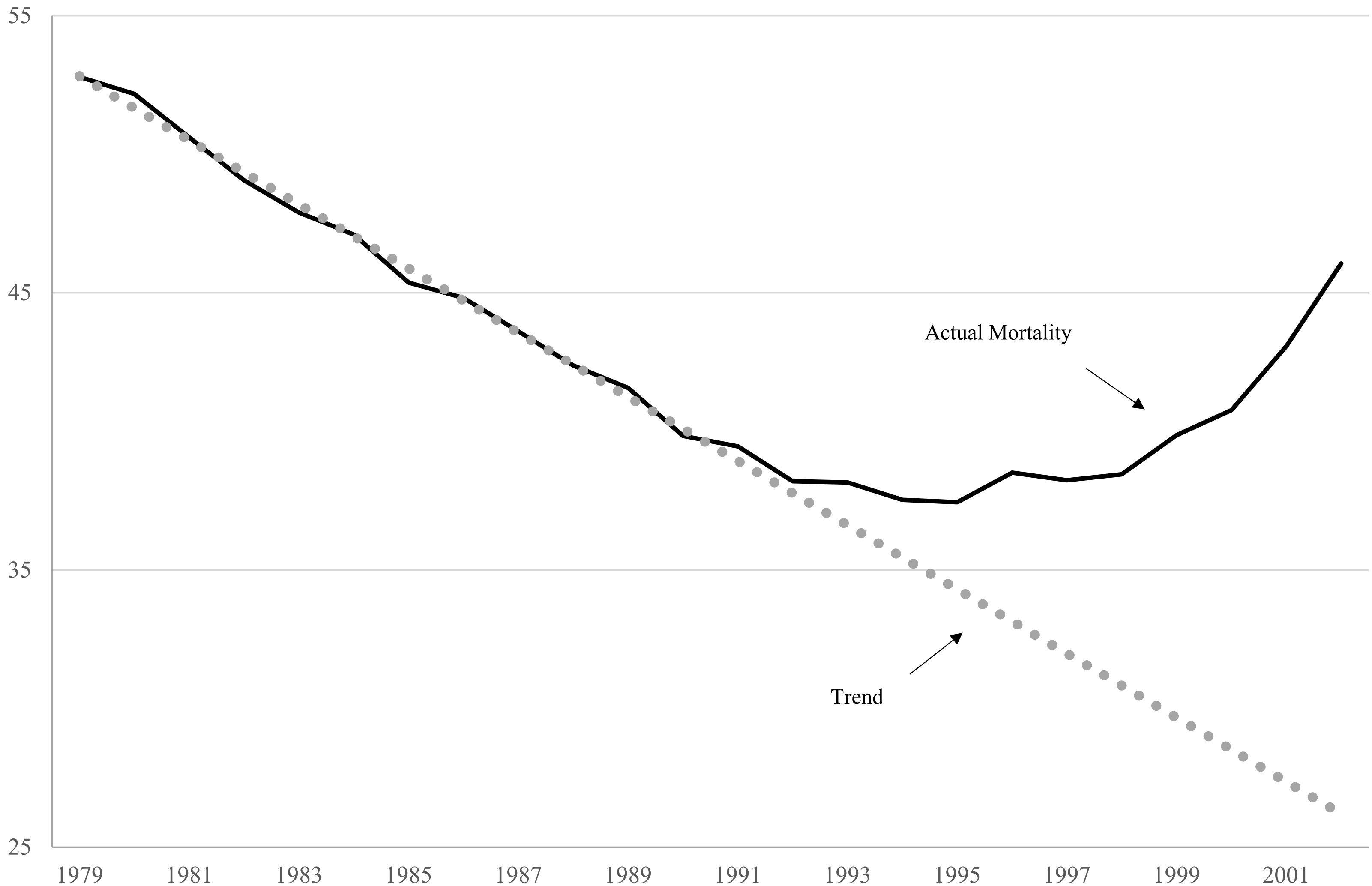
Our data on deaths of despair outside of the U.S. come from the World Health Organization's (WHO's) Mortality Database. This dataset contains mortality counts by year, age group, sex, and cause of death for a broad collection of countries. These counts come from each country's own vital statistics system. We aggregate the data to the country-year-age group-cause of death level. Cause of death is labeled using either ICD8, ICD9, or ICD10 classification codes, and the timing of the usage of each set of codes varies across countries.

Mortality counts reported using the ICD8 and ICD9 codes are only available in coarse categories that group together several causes of death; for example, in years when the ICD8 codes are used, accidental drug poisonings (E850-E859) cannot be isolated and are instead lumped into all accidental poisonings (E850-E877). As such, we must define deaths of despair slightly differently in our international analysis than in our main analysis. In the ICD8 codes, we define deaths of despair using the codes E950-E959 (suicide), 571 (liver cirrhosis), E850-E877 (accidental poisonings), and E980-E989 (injury, undetermined whether accidentally or purposely inflicted). In the ICD9 codes, we use the codes E950-E959 (suicide), 571 (liver cirrhosis), E850-E869 (accidental poisonings), and E980-E989 (injury, undetermined whether accidentally or purposely inflicted). In the ICD10 codes, we use the codes X60-X84 (suicides), K70, K73-K74 (liver cirrhosis), X40-X49 (accidental poisonings), and Y10-Y34 (event of undetermined intent).

To construct mortality rates, we use population data from the United Nations' Population Division Data Portal. Our mortality rates for the middle-aged (45 to 64 year olds) are age-adjusted; i.e., they are convex combinations of the four different five year age group rates within this broader age range and are calculated using the same set of weights across all countries and years. Our weights are based off the age distribution in the United States in 1980. This age adjustment prevents misleading comparisons of mortality rates across countries that are driven purely by differing age distributions.

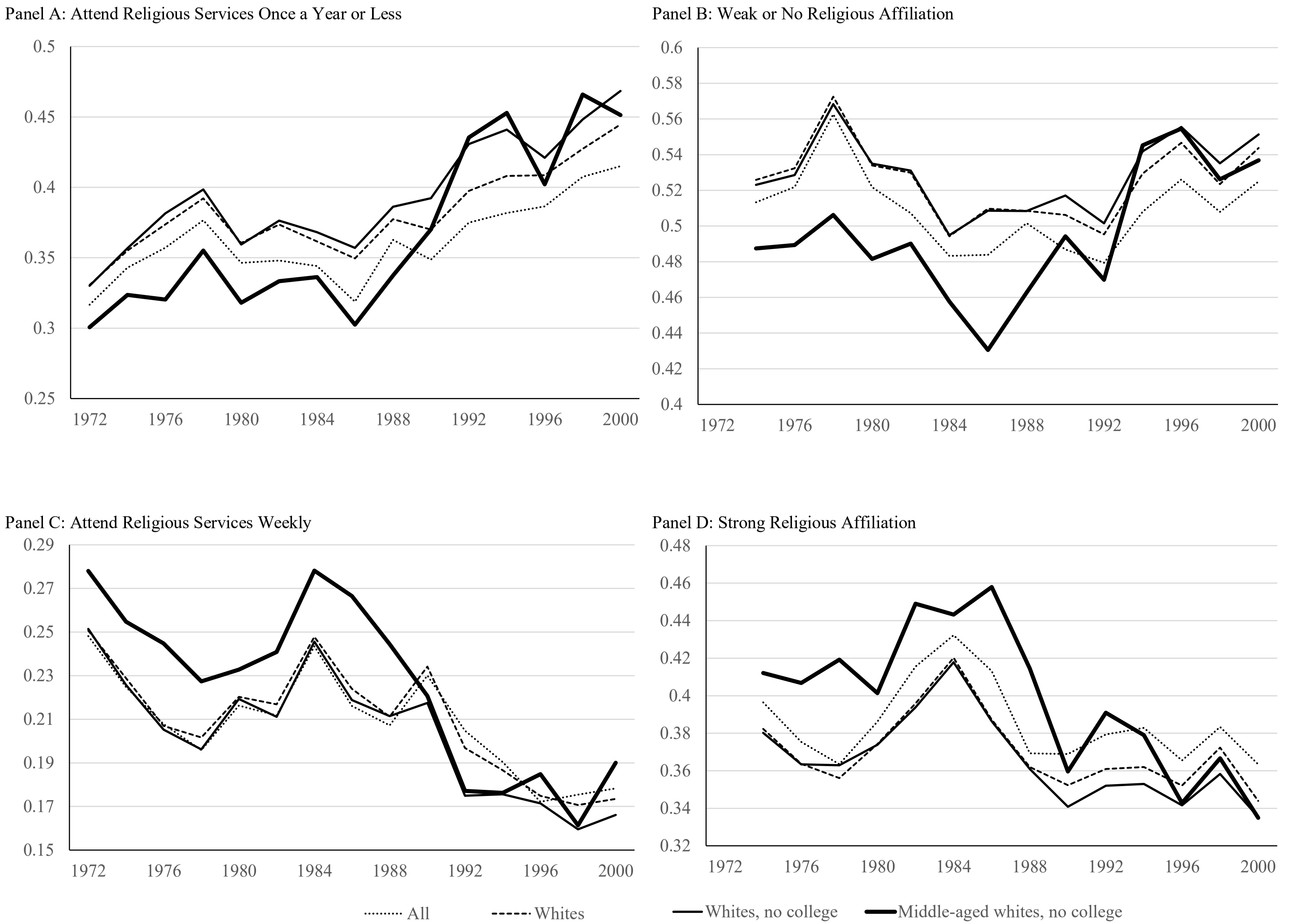
Prior to 1990, mortality counts in Germany are reported separately for the Former Democratic Republic (East Germany) and the Former Federal Republic (West Germany), and the causes of death in East Germany are reported using a slightly different coding system that does not identify accidental poisonings. In 1990, mortality counts are reported for Germany as a whole as well as East and West Germany; from 1991 onward, they are reported for Germany as a whole only. To construct mortality rates for Germany prior to 1990, we begin with the rate for West Germany in the given year and then add in the 1990 difference in rates between Germany and West Germany; in other words, the pre-1990 Germany mortality rate is the West Germany rate that has been level-shifted to connect with the overall German rate in 1990.

Figure 1: Mortality Rates for White Individuals Aged 45-64



Notes: The figure presents mortality rates, calculated as deaths due to suicide, poisonings, or liver disease per 100,000. The sample includes white individuals aged 45-64, from 1979 to 2001.

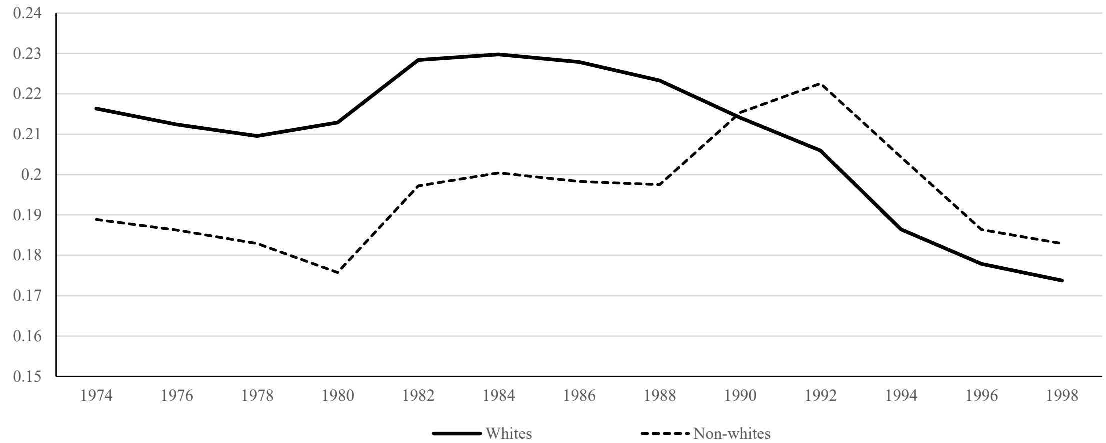
Figure 2: Religiosity Trends in the General Social Survey



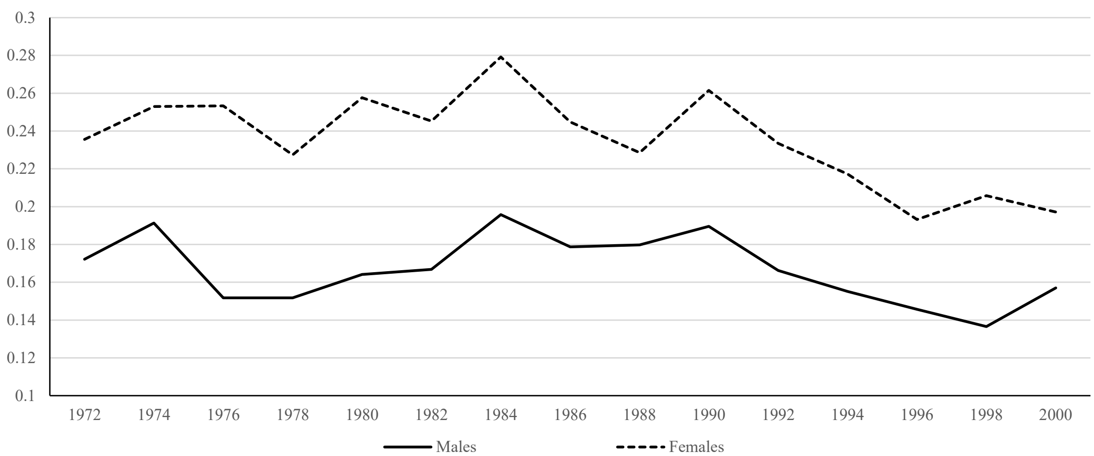
Notes: The figure presents, for different groups of respondents, measures of attendance and affiliation. The first two panels plot measures that correspond to low religiosity. Panel (a) plots the fraction of respondents who report attending worship services once a year or less. Panel (b) plots the fraction of respondents whose stated religious preference is "weak" or "none." The next two panels plot measures that correspond to high religiosity. Panel (c) plots the fraction of respondents who attend worship services weekly, and panel (d) plots the fraction of respondents whose stated religious preference is "strong." The label "no college" refers to those without a four-year degree, and "middle-aged" includes respondents aged 45-64.

Figure 3: Weekly Attendance by Demographic Group

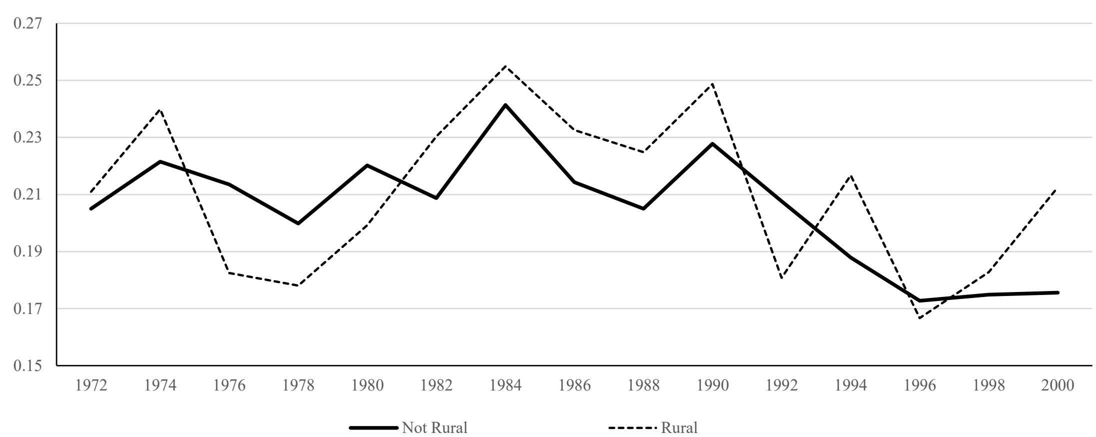
Panel A: By Race



Panel B: By Gender

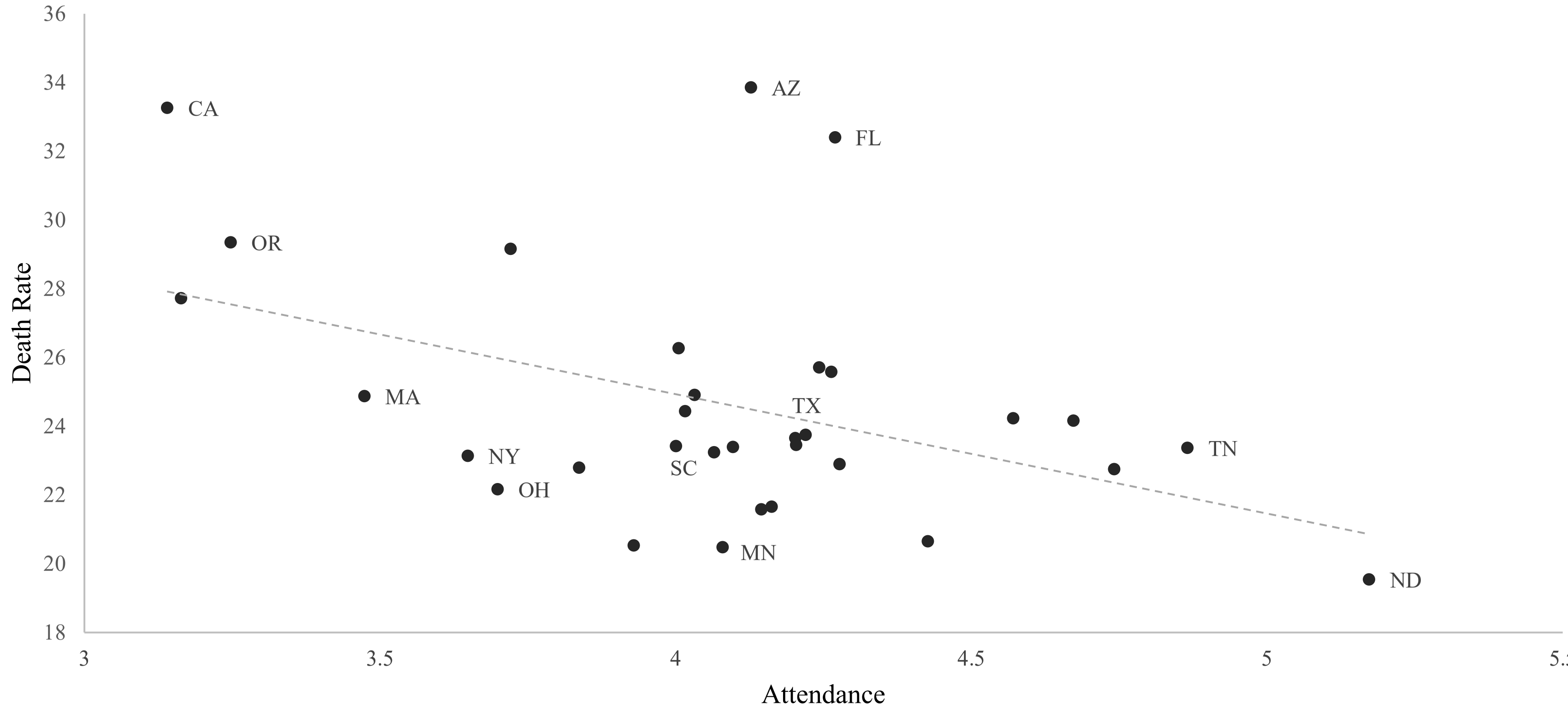


Panel C: By Rural Status



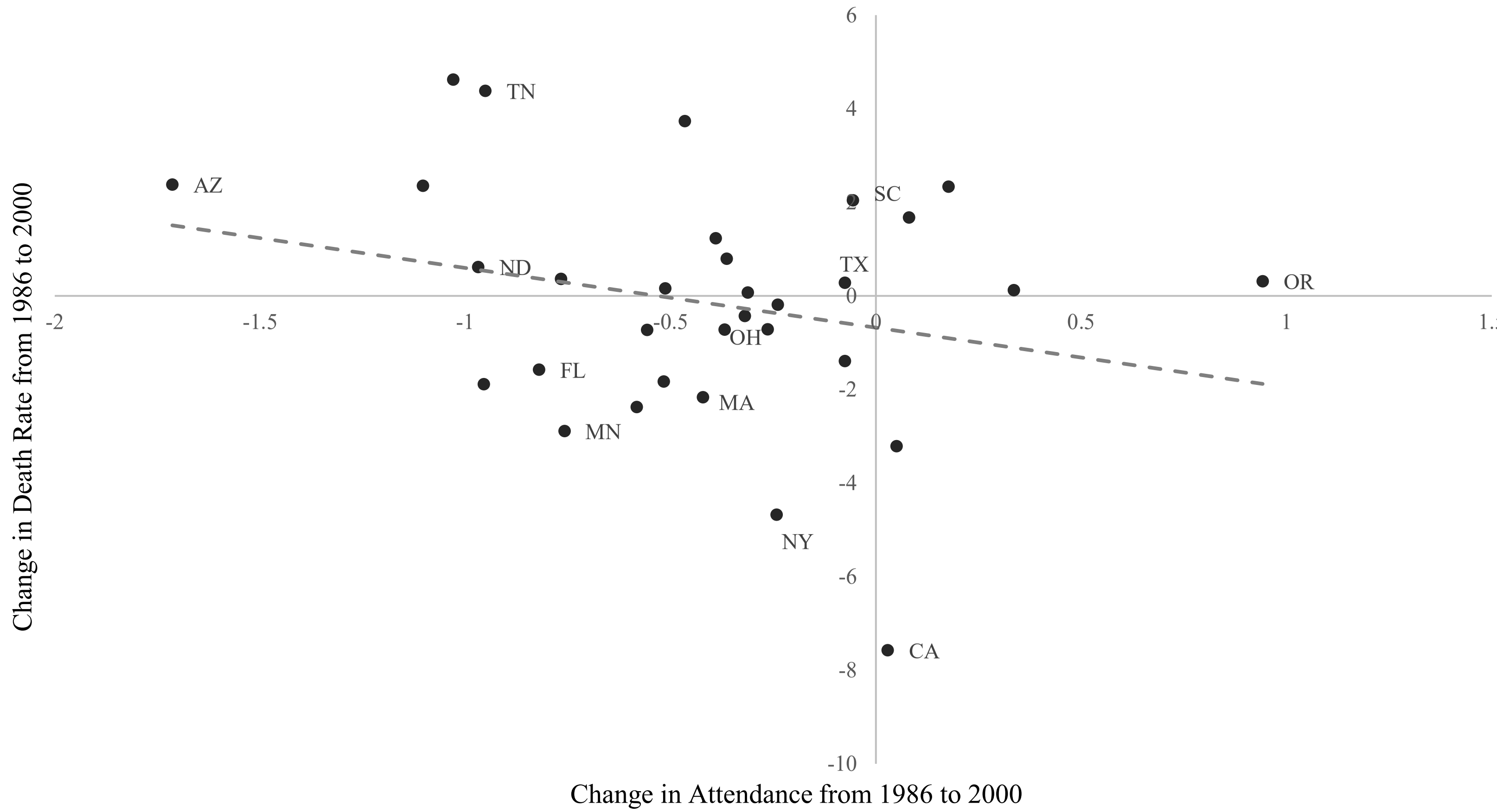
Notes: The figure presents trends in religous attendance by subgroup. Attendance is measured as a binary indicator for whether the respondent attended worship once a week and the sample includes all age groups. In panel (a), a running average is used to deal with smaller sample sizes by race; thus the first and last year are ommitted. Panel (b) presents trends by gender, and panel (c) presents trends by rural/urban status.

Figure 4: State Levels of Attendance and Deaths of Despair, Late 1980s



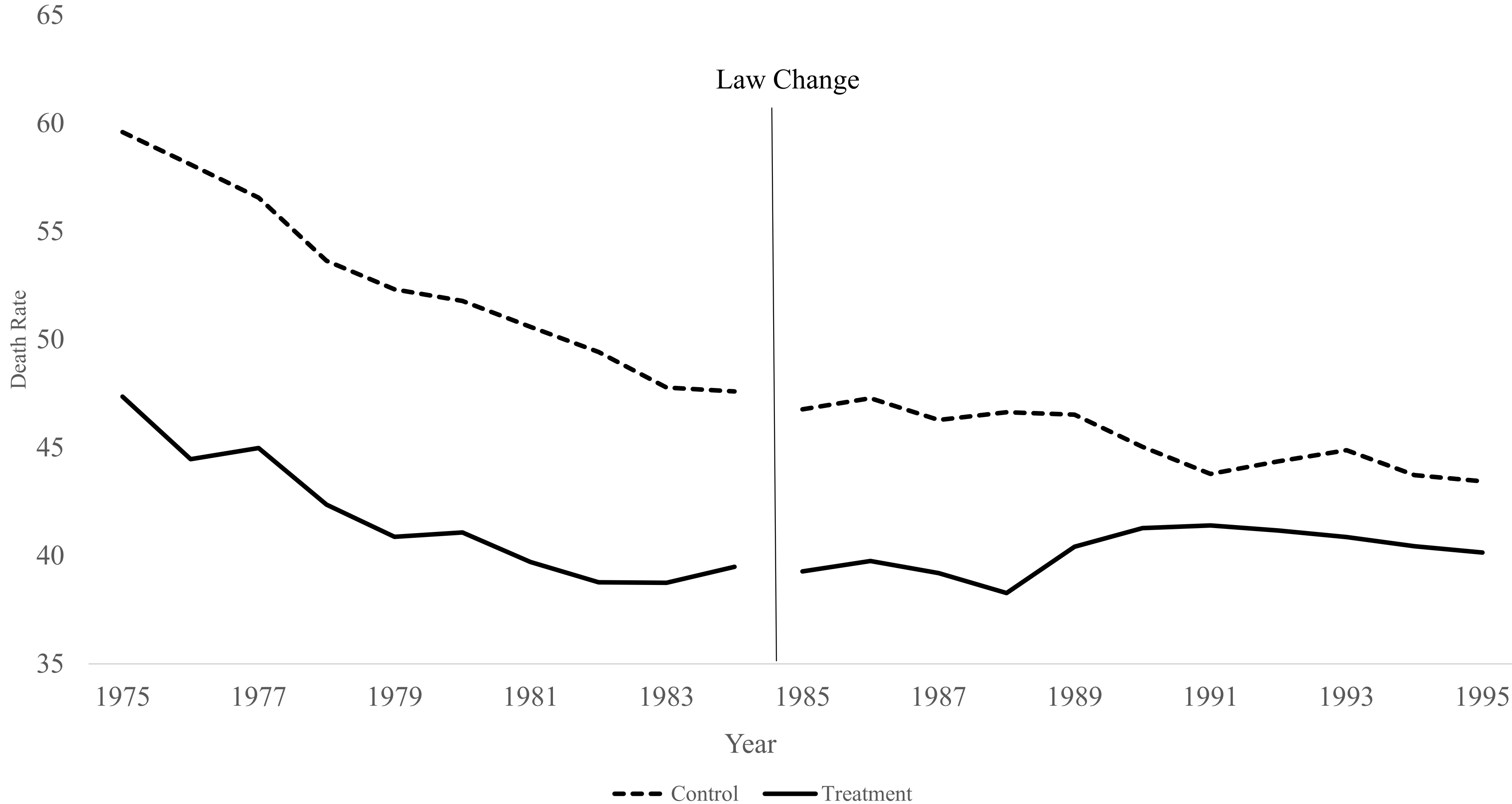
Notes: Each point corresponds to a state. The figure includes states featured both in the late 1980s (specifically, the 1986, 1987 or 1988 waves of the GSS) and in the 1998 or 2000 waves (for comparability with figure 5). The x axis shows the rate of religious attendance in the GSS survey in 1986, 1987, and 1988 years (years are combined since not all states are available in all years). The y axis shows the rate of deaths of despair (per 100,000) for each state over the same time period. Attendance is measured on a 9-point scale as described in Section 2.1. Each point on the index approximately represents a doubling in frequency, so that each point on the index is roughly akin to a log scale. We consider the average value of this attendance index across states. The dashed line represents the best fit line. The correlation is -0.443.

Figure 5: State Changes in Attendance and Deaths of Despair, 1986 to 2000



Notes: Each point corresponds to a state. The figure include states featured both in the late 1980s (specifically, the 1986, 1987 or 1988 waves of the GSS) and in the 1998 or 2000 waves. The x axis shows the change in the rate of religious attendance in the GSS survey: average attendance in the 1998-2000 period minus average attendance in 1986, 1987, and 1988. Attendance is measured on a 9-point scale as described in Section 2.1. Each point on the index approximately represents a doubling in frequency, so that each point on the index is roughly akin to a log scale. We consider the average value of this attendance index across states. The y axis shows the change in the rate of deaths of despair (per 100,000) for each state over the same time. The dashed line represents the best fit line.

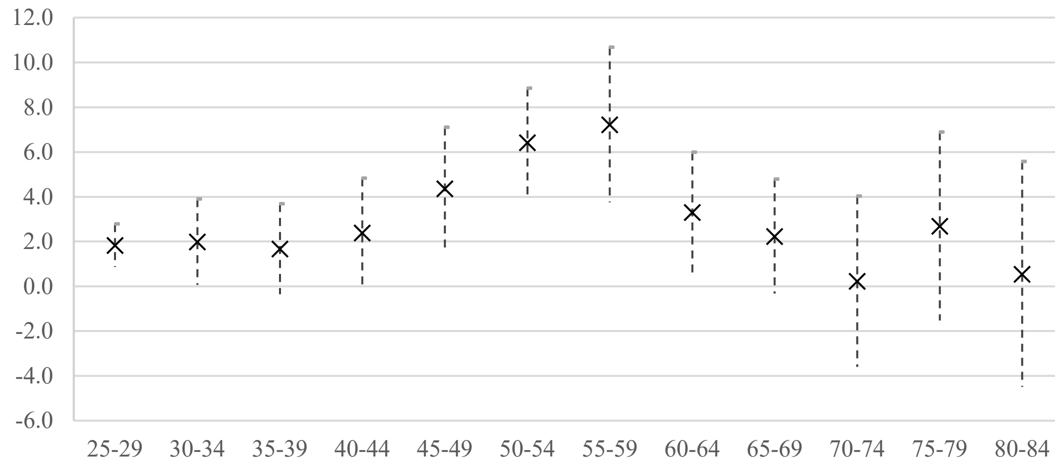
Figure 6: Deaths of Despair Trends in TX, SC, and MN



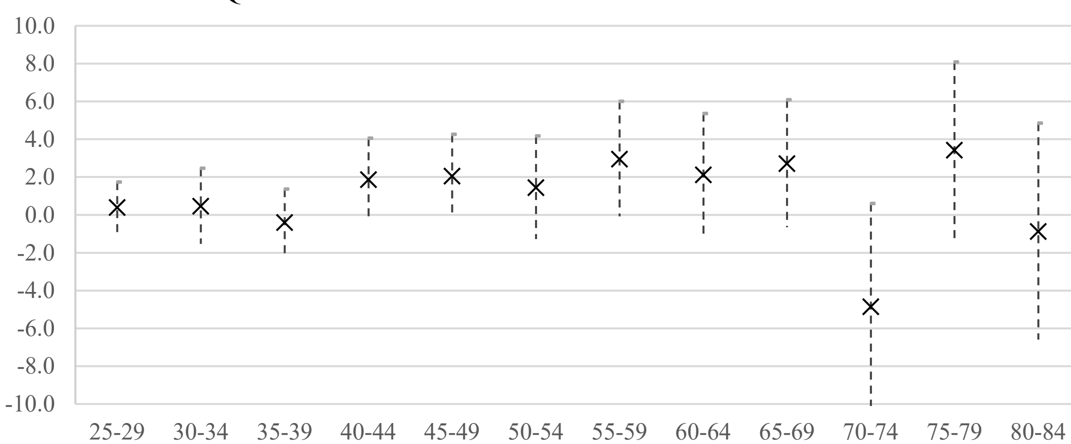
Notes: The figure presents patterns in deaths of despair over time for the treatment states of Texas, South Carolina and Minnesota, whose laws changed in 1985, relative to all the other states in the sample as the control states. The y axis represents the average rate of deaths of despair (per 100,000) and is weighted by the population of each state.

Figure 7: Effect of Blue Law Repeals for 5-Year Age Bins

Panel A: No Trends



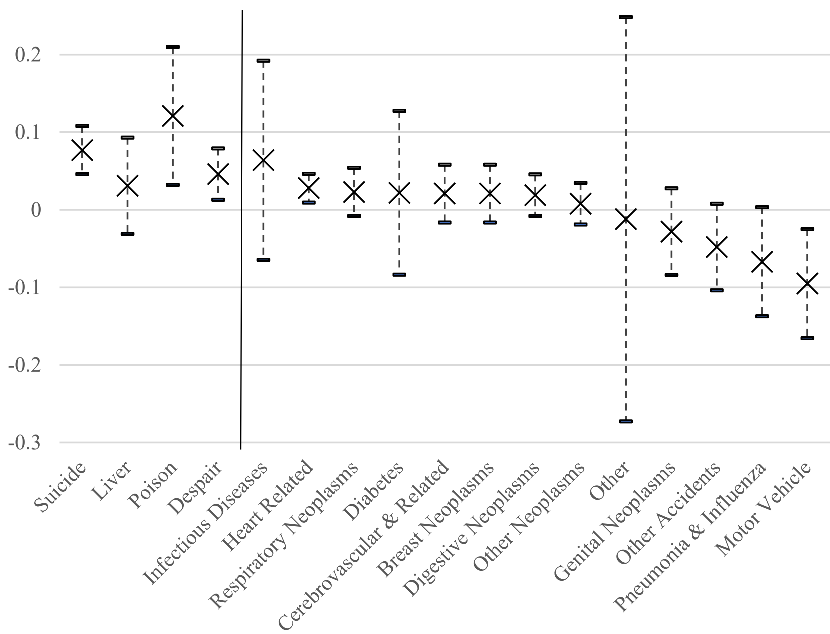
Panel B: Linear and Quadratic Trends



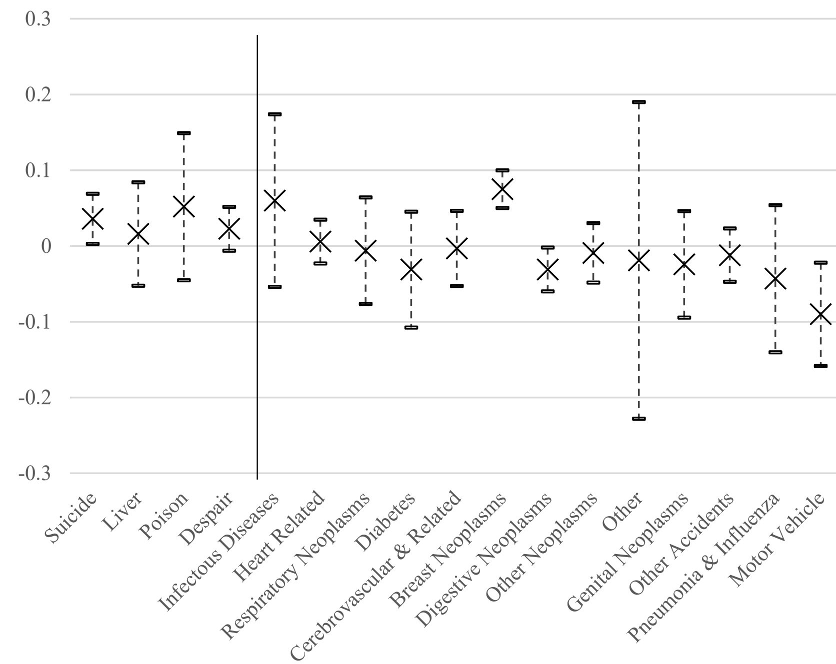
Notes: The figures show the coefficients and 95% confidence intervals from the estimation of equation (2) on each 5-year age bin separately. Each coefficient shows the effect of blue law repeals on deaths of despair. The regressions include the same controls as presented in table 3 (except for the age-group dummies, which cannot be included as they would be collinear with the constant term here). Panel (a) does not include trends, as in table 3, column (1), and panel (b) includes linear and quadratic state trends as in table 3, column (3).

Figure 8: Effect of Blue Law Repeals on Other Causes of Death

Panel A: Middle Aged



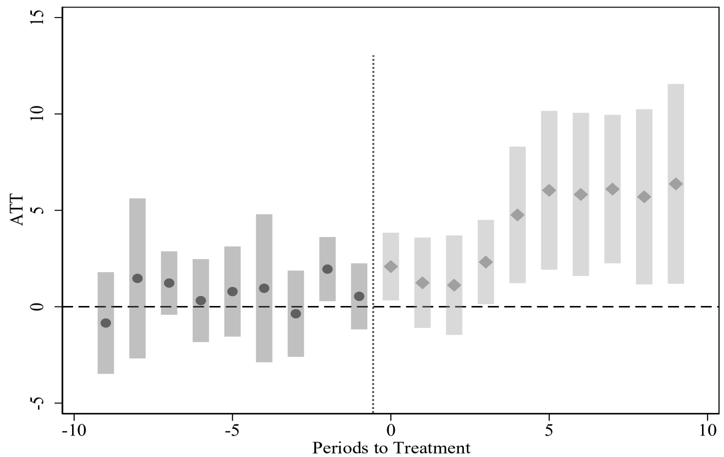
Panel B: All Ages



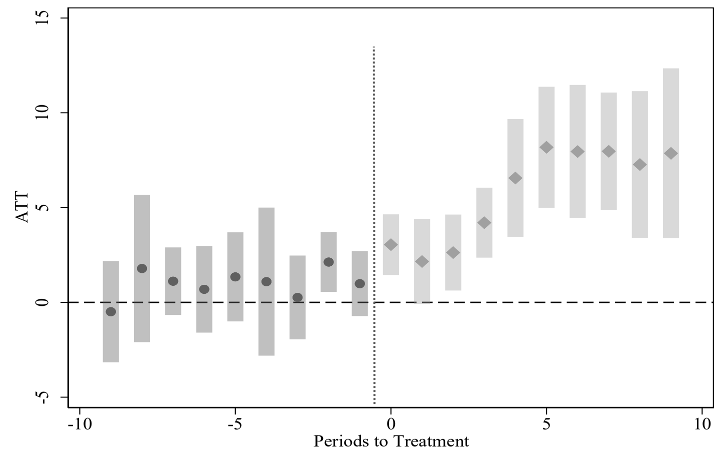
Notes: The figure shows the coefficients and 95% confidence intervals from 17 different regressions based on equation (2) using logged mortality rates for individuals 45-64 for separate causes of death. The results include linear and quadratic state trends and the same controls as in table 3. The "other cause" categories are based on the top 15 categories of mortality for the middle-aged that can be well-matched between ICD-8 and ICD-9 codes and are, respectively, in terms of ICD-9 codes: residual of infectious and parasitic diseases (001-009,020-088,098-139), acute myocardial infarction and other heart-related causes (390-398, 401-404, 410-429), malignant neoplasms of respiratory and intrathoracic organs (160-165), diabetes mellitus (250), cerebrovascular diseases and deaths related to arteries (430-438, 440-448), malignant neoplasm of breast (174-175), malignant neoplasms of digestive organs and peritoneum (150-159), other malignant neoplasms (140-149,170-173,190-203), symptoms, signs, and ill-defined conditions (780-799), malignant neoplasms of genital organs (179-187), all other accidents and adverse effects (E800-E807,E826-E949), pneumonia and influenza (480-487), and motor vehicle accidents (E81O-E825).

Figure 9: Robustness Test of Repeal Effects on Deaths of Despair Over Time

Panel A: All Control Groups



Panel B: Never-Adopters Only



Notes: The figures depict the ATT mortality effects before and after treatment (the repeal of blue laws) estimated using the outcome regression method proposed by Callaway and Sant'Anna (2021). Panel (a) uses all observations that have not repealed blue laws as control groups, and panel (b) uses only states that never adopted blue laws. The shaded areas represent 95% confidence intervals based on standard errors clustered at the state level. The overall ATT figure in panel (a) is 6.59 (se = 2.40), and in panel (b), it is 7.50 (2.18).

Table 1: Means and Sample Information

Blue Law Changes

Repealing State (Year)	FL (1969), IN (1977), MN (1985), ND (1991), OH (1973), PA (1978), SC (1985), SD (1977), TN (1981), TX (1985), UT (1973), VT (1982), VA (1975)

Blue Laws & GSS

Unit of Observation (N)	Individuals (20,279)		
States Included in Sample	AZ, CA, CO, FL, IA, IN, KS, MN, ND, OH, OR, PA, SC, SD, TN, TX, UT, VA, VT, WA, WY		
Years:	1973-2000		
Variable Means (*Std. Dev.*)			
Weekly Attendance Dummy	0.20	Married Dummy	0.56
	(0.40)		(0.50)
Weak Affiliation Dummy	0.42	Fraction State Ages 20 to 39	0.32
	(0.49)		(0.03)
Age	45	Fraction State Ages 40 to 64	0.26
	(18)		(0.02)
Female Dummy	0.56	Fraction State over 65	0.12
	(0.50)		(0.02)
White Dummy	0.87	Fraction State Male	0.49
	(0.34)		(0.01)
Black Dummy	0.10	Fraction State White	0.87
	(0.30)		(0.06)
Some College Dummy	0.24	Fraction State Black	0.10
	(0.43)		(0.06)
H.S. Education Dummy	0.32	Fraction State Other Race	0.03
	(0.47)		(0.03)
H.S. Dropout Dummy	0.24	Population (1,000)	11,800
	(0.43)		(8784)

Blue Laws & Mortality Data

Unit of Observation (N)	State x Age Group x Year x Race cells (19,630)		
States Included in Sample	AZ, CA, CO, FL, IA, ID, IN, KS, MN, ND, NM, NV, OH, OR, PA, SC, SD, TN, TX, UT, VA, VT, WA, WY		
Years	1969-2000		
Variable Means (*Std. dev.*)			
Mortality Rate: Suicide (per 100k)	15.8	Fraction State Male	0.49
	(6.20)		(0.01)
Mortality Rate: Liver (per 100k)	19.0	Fraction State White	0.87
	(17.57)		(0.06)
Mortality Rate: Poisoning (per 100k)	6.30	Fraction State Black	0.09
	(5.17)		(0.06)
Fraction State Ages 20 to 39	0.31	Fraction State Other Race	0.04
	(0.03)		(0.04)
Fraction State Ages 40 to 64	0.26	State Population (1,000)	12600
	(0.02)		(9284)
Fraction State over 65	0.12	Cell Size (1,000)	569
	(0.03)		(539)

Notes: Averages in the "Blue Laws & Mortality Data" section are taken from age x race x state x year cells where ages are combined into 5-year age bins; means are weighted by cell size.

Table 2: Effect of Blue Laws Repeal on Religiosity

	Measures of Low Religiosity		Measures of High Religiosity	
	Attend Once a Year or Less	Weak or No Religion	Attend Weekly	Strong Religion
	(1)	(2)	(3)	(4)
Ages 25-44				
Blue Law Repeals	0.0435	0.0247	-0.0707	-0.0298
	(0.0286)	(0.0419)	(0.0358)	(0.0401)
Ages 45-64				
Blue Law Repeals	0.0705	0.194	-0.0927	-0.111
	(0.0471)	(0.0476)	(0.0562)	(0.0624)
Ages 65 and Up				
Blue Law Repeals	0.0762	0.0787	-0.0498	-0.0905
	(0.0463)	(0.0354)	(0.0577)	(0.0609)
All Ages				
Blue Law Repeals	0.0618	0.0830	-0.0770	-0.0668
	(0.0168)	(0.0351)	(0.0193)	(0.0401)
Controls	Yes	Yes	Yes	Yes
State Trends	Linear + Quad	Linear + Quad	Linear + Quad	Linear + Quad

Notes: Each coefficient is from a separate regression of equation (1). State-clustered standard errors are in parentheses. The sample includes 20,279 individuals from the General Social Survey from 1973 to 1998. Controls include the fraction of the state population aged 20 to 39, 40 to 64, and over 65, the fraction male, the fraction white, the fraction black and the state population. Individual controls include age, age squared, gender, race, and dummies for educational attainment and for marital status. The first row uses only respondents aged 25-44 at the time of the survey, the second row uses those aged 45-64, and the third row uses those aged 65 and up. The outcome in column (1) is an indicator for whether an individual reports attending worship once a year or less. In column (2), the outcome is an indicator for whether a respondent's stated religious preference is "weak" or "none." Column (3) uses an indicator for whether an individual reports attending worship weekly, and the outcome in column (4) is an indicator for whether a respondent's stated religious preference is "strong."

Table 3: Effect of Blue Law Repeals on Deaths of Despair

	(1)	(2)	(3)
Ages 25-44			
Blue Law Repeals	1.858	-0.240	0.501
	(0.737)	(0.563)	(0.536)
Ages 45-64			
Blue Law Repeals	5.320	1.941	2.154
	(0.975)	(0.754)	(0.669)
Ages 65-84			
Blue Law Repeals	1.345	-1.047	0.210
	(1.386)	(1.021)	(0.980)
All Ages			
Blue Law Repeals	2.915	0.297	0.913
	(0.741)	(0.530)	(0.536)
Mortality Rate	Levels	Levels	Levels
State Trends	No	Linear	Linear + Quad
Controls	Yes	Yes	Yes

Notes: Each coefficient is from a separate regression of equation (2), where the outcome is the mortality rate due to deaths of despair (per 100,000). Standard errors clustered by state are in parentheses. The sample includes 24 states either without blue laws or with usable statewide blue laws (see text). The mean mortality rate is 28 per 100,000 for ages 25-44, 51 per 100,000 for ages 45-54, 58 per 100,000 for ages 65-84, and 41 per 100,000 for all ages. All regressions include dummies for each 5-year age bin, state dummies, and year dummies. Each regression is weighted by population.

Table 4: Effect of Blue Law Repeals by Cause of Death

	(1) All	(2) Liver	(3) Poisonings	(4) Suicide
Ages 25-44				
Blue Law Repeals	0.501	0.0279	0.233	0.240
	(0.536)	(0.268)	(0.351)	(0.300)
Ages 45-64				
Blue Law Repeals	2.154	0.745	0.208	1.201
	(0.669)	(0.608)	(0.208)	(0.234)
Ages 65-84				
Blue Law Repeals	0.210	0.386	-0.468	0.292
	(0.980)	(0.692)	(0.181)	(0.532)
All Ages				
Blue Law Repeals	0.913	0.259	0.101	0.552
	(0.536)	(0.351)	(0.196)	(0.264)
Mortality Rate	Levels	Levels	Levels	Levels
State Trends	Linear + Quad	Linear + Quad	Linear + Quad	Linear + Quad
Controls	Yes	Yes	Yes	Yes

Notes: Each coefficient is from a separate regression of equation (2), where the outcome is the mortality rate due to all deaths of despair (column 1), and, separately, liver disease (column 2), poisonings (column 3), or suicide (column 4) (per 100,000). The sample, controls and weighting are the same as in Table 3.

Table 5:
Robustness of Difference-in-Difference Estimates

	(1)	(2)	(3)	(4)	(5)
	First 3 Years of Repeal	First 10 Years of Repeal	3-Year Repeal Window	10-Year Repeal Window	No Early Adopters
Repeal	1.78	2.45	2.13	2.91	2.07
	(0.66)	(0.68)	(0.64)	(0.64)	(0.57)
Mortality Rate	Levels	Levels	Levels	Levels	Levels
State Trends	No	No	No	No	Linear + Quad
Controls	Yes	Yes	Yes	Yes	Yes

Notes: Table reports regression estimates of deaths of despair on blue law repeals for ages 45-64. Standard errors are clustered by state in parentheses. Column (1) includes only the first three years observed after a state repeals its blue laws, and column (2) includes only the first ten years observed after a state repeals its blue laws. (In both cases, never-repeal states are included in all years as controls). Column (3) further eliminates all but the three years prior to repeal in treatment states. The last column drops four states (FL, IA, KS, and WA) that repealed their blue laws prior to the sample period; omitting trends in the last column produces an estimate of 3.98 (0.72).

Table 6: Effect of Blue Law Repeals on Behavior and Beliefs

	At Bar (1)	Safe Drink (2)	Drunk (3)	Social w/ Neighbors (4)	Social Index (5)	Trust (6)	Confidence in Institutions (7)	Belief in Afterlife (8)	Pray (9)
Ages 25-44									
Blue Law Repeals	-0.00628	0.0420	-0.0456	-0.0214	-0.423	-0.0379	0.0273	-0.0170	0.0161
	(0.0366)	(0.0765)	(0.0651)	(0.0450)	(0.298)	(0.0408)	(0.0334)	(0.0317)	(0.116)
Ages 45-64									
Blue Law Repeals	-0.0606	0.0173	0.0850	-0.00903	0.360	-0.0464	-0.00268	-0.0956	-0.0580
	(0.0397)	(0.0636)	(0.0281)	(0.0634)	(0.951)	(0.0405)	(0.0472)	(0.0581)	(0.106)
Ages 65 and Up									
Blue Law Repeals	-0.0158	0.0908	0.0731	0.0390	-0.286	-0.0467	0.0190	-0.0937	0.193
	(0.0127)	(0.0648)	(0.0580)	(0.124)	(1.279)	(0.0807)	(0.0766)	(0.0743)	(0.286)
All Ages									
Blue Law Repeals	-0.0240	0.0413	0.0164	-0.0156	-0.386	-0.0392	0.0161	-0.0567	-0.00705
	(0.0215)	(0.0546)	(0.0455)	(0.0236)	(0.412)	(0.0192)	(0.0261)	(0.0273)	(0.0777)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Trends	Linear+Quad	Linear+Quad	Linear+Quad	Linear+Quad	Linear+Quad	Linear+Quad	Linear+Quad	Linear+Quad	Linear+Quad

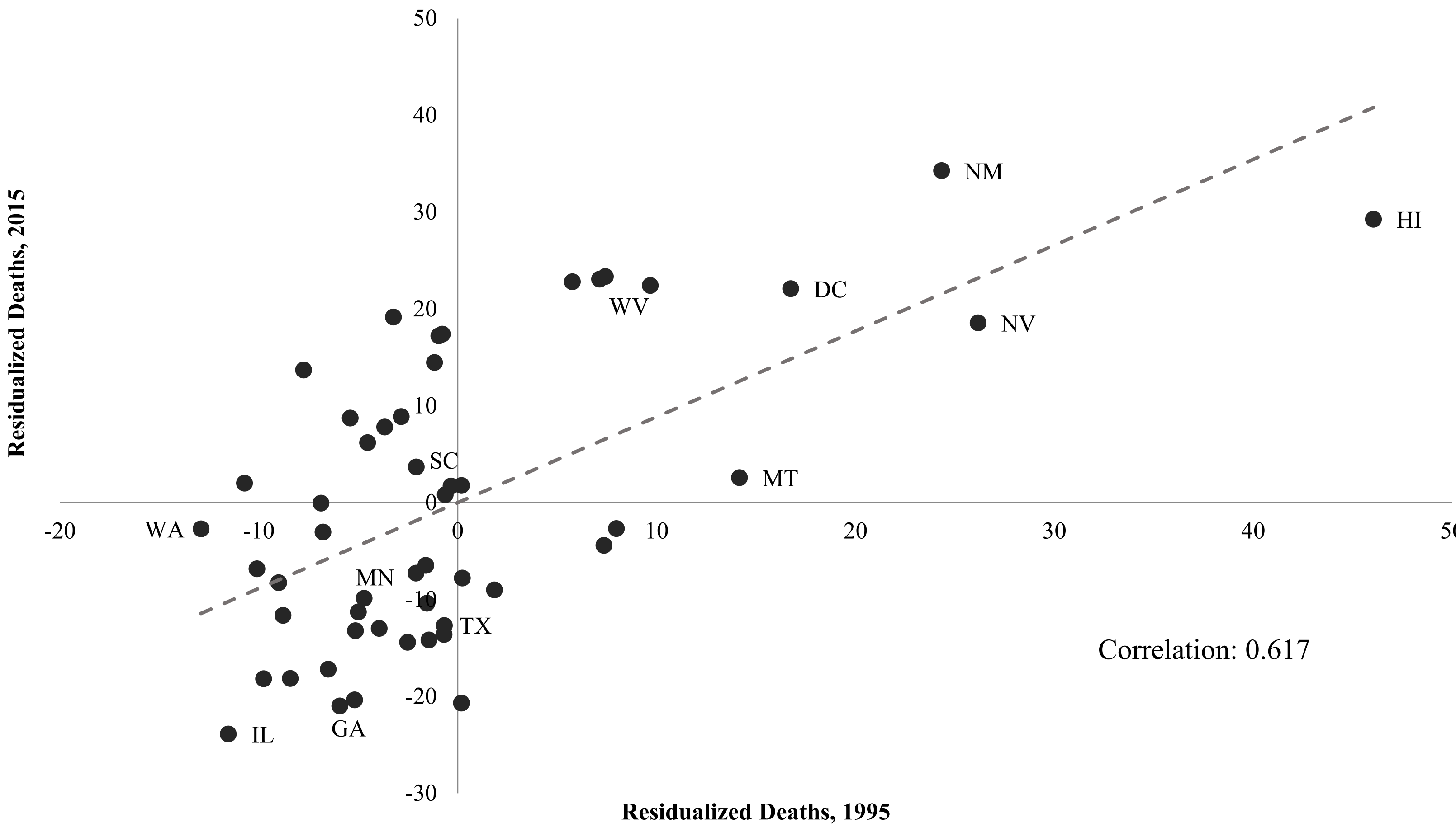
Notes: Each coefficient is from a separate regression of equation (1). State-clustered standard errors are in parentheses. The sample includes 20,279 individuals from the General Social Survey from 1973 to 1998. Controls include the fraction of the state population aged 20 to 39, 40 to 64, and over 65, the fraction male, the fraction white, the fraction black and the state population. Individual controls include age, age squared, gender, race, and dummies for educational attainment and for marital status. The first row uses only respondents aged 25-44 at the time of the survey, the second row uses those aged 45-64, and the third row uses those aged 65 and up. Column (1) is an indicator for whether the respondent spends the evening at a bar several times a week or more. In column (2), the outcome is an indicator for whether the respondent ever drinks but does not drink more than they should, while in column (3) the outcome is whether the respondent is sometimes "drink[s] more than they should". The outcome in column (4) is whether the respondent spends the evening with neighbors at several times a week (the median level of socialization). The outcome in column (5) is a social index which sums time spent with friends, neighbors, relatives, and socializing at a bar (each on a 0 to 6 point scale); the mean is 10 and the std dev. is 4. Column (6) is an index for trust, where "1" means the respondent stated most people can be trusted, "0.5" means the repondent stated that it depends and "0" means the respondent stated that you can't be too careful. Column (7) is an index that sums the confidence in thirteen institutions (e.g. religion, major companies, education), each on a zero to one scale. The outcome in column (8) is an indicator for whether the repondent believes there is life after death and in column (9) is an indicator for whether the respondent prays at least every day.

Table 7: Correlations in Community Characteristics in Rupasingha et al. (2006) and Religious Attendance

Characteristics with a Positive Correlation	Correlation	*Characteristics with a Negative Correlation*	Correlation
Religious Organizations	0.57	Voter Turnout 1988 Election	-0.14
Aggregate Number of All Associations	0.43	Physical Fitness Facilities	-0.14
Membership in Unclassified Organizations	0.33	Response Rate to 1990 Census	-0.15
Membership in sports and recreation clubs	0.31	Labor Organizations	-0.15
Business Associations	0.25	Turnout for 1992 Election	-0.15
Bowling Centers	0.13	Public Golf Courses	-0.21
Civic and Social Organizations	0.01	Number of not-for-profit organizations	-0.22
		Professional Organizations	-0.25
		Political Organizations	-0.25
		Sports Clubs, Managers, Promoters	-0.41

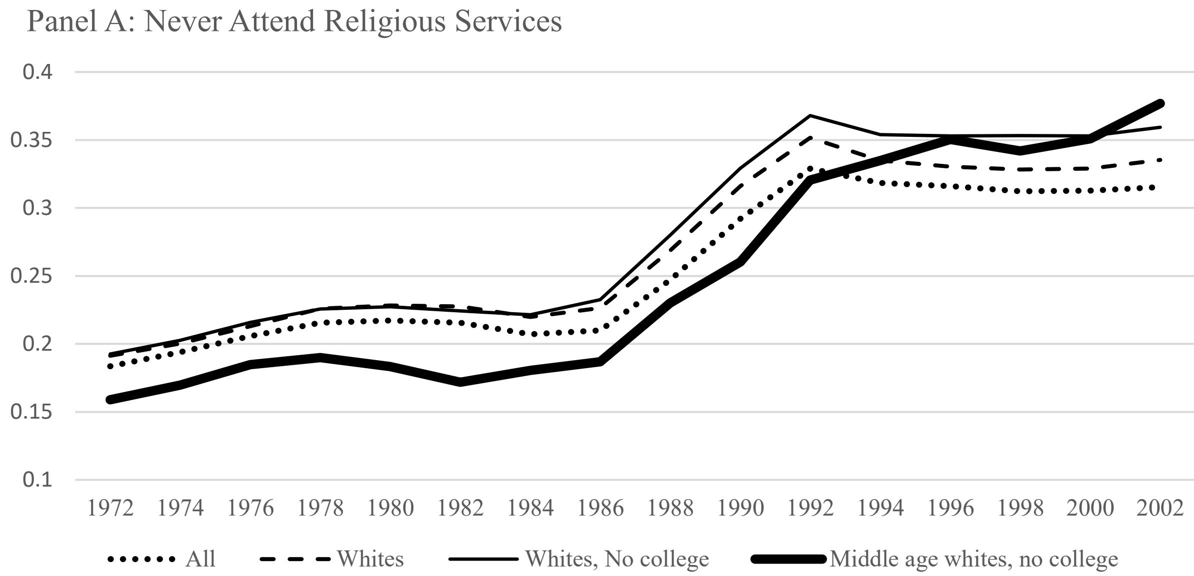
Notes: The community characteristics presented in the table are population-weighted state-level averages of the 1990 inputs used to create a social capital index in Rupasingha, Goetz, and Freshwater (2006); the data are available from the Northeast Regional Center for Rural Development. These characteristics are correlated with the state averages of the religious attendance index for the late 1980s used in figure 4.

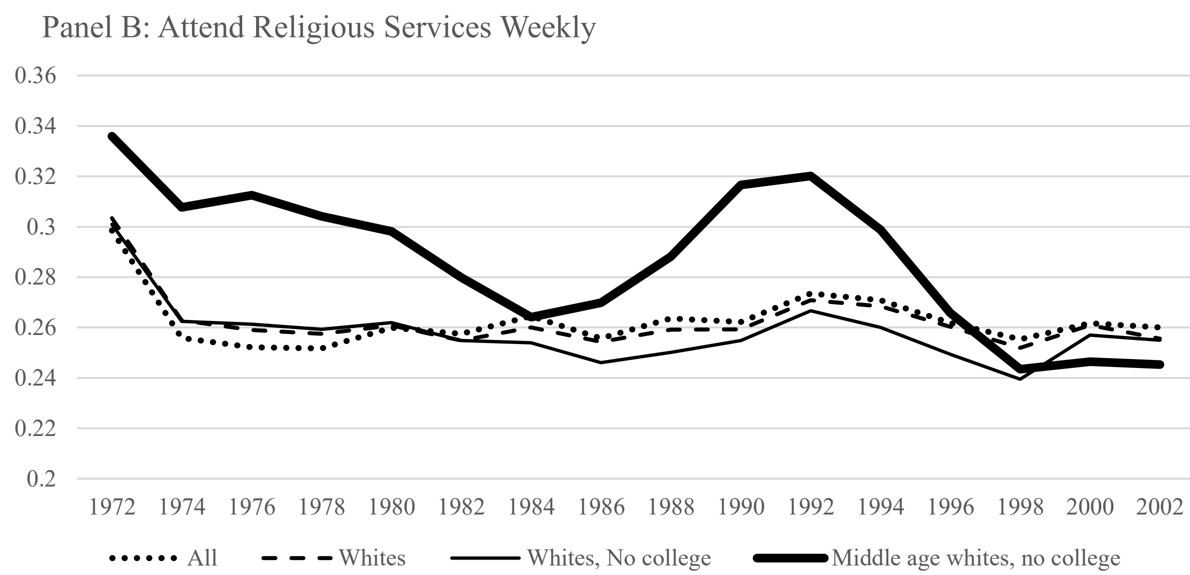
Figure A1: State-Level Correlation Between Deaths of Despair in 1995 and 2015



Notes: The figure presents a state-level scatterplot of the residualized deaths of despair mortality rate for white Americans aged 45 to 54 in 1995 against the same rate in 2015. We plot the residuals from a regression of the state-level mortality rates from 1995 and 2015 on state-level controls from 2015. These controls include the average unemployment rate, median household income, and the fraction of adults with less than a high school degree, high school degree, some college or associate degree, and bachelor's or graduate degree. We plot the line of best fit and report the correlation coefficient.

Appendix Figure A2: Religiosity in the ANES

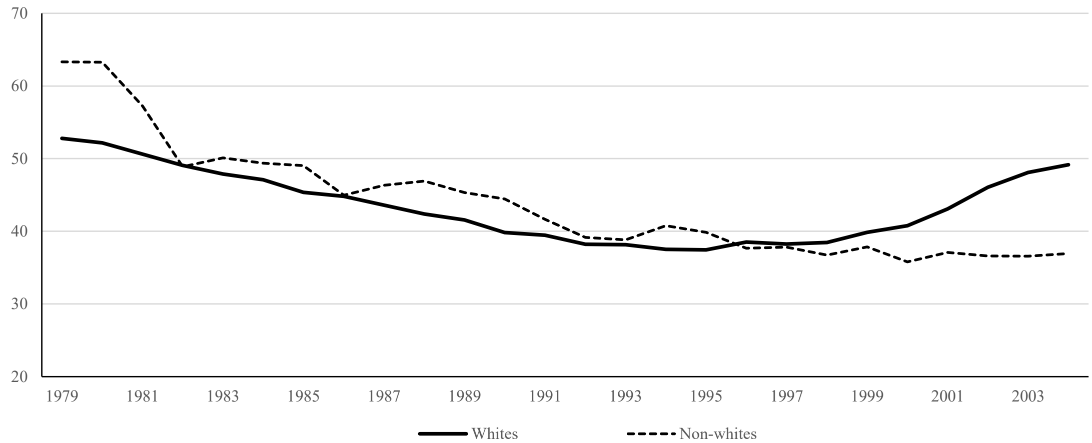




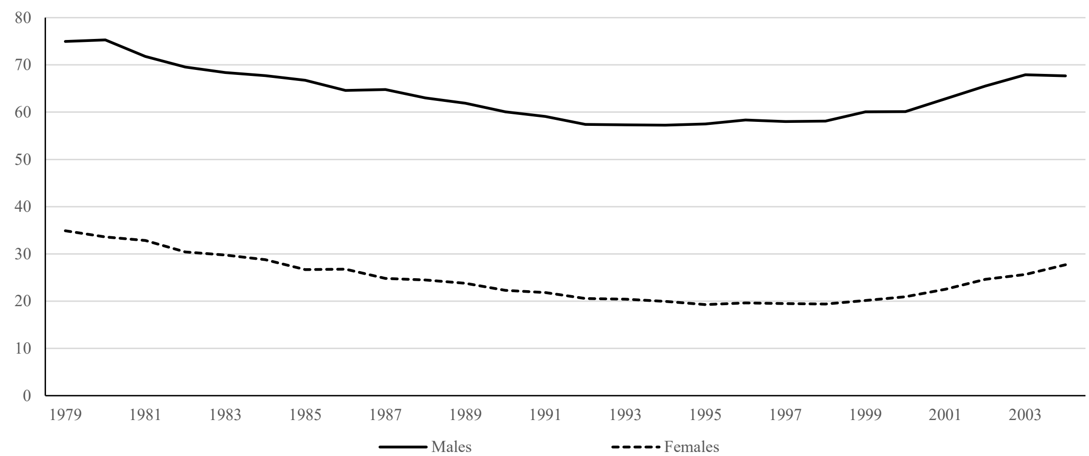
Notes: The figure presents, for different groups of respondents, measures of religious behavior found in the American National Election Studies (ANES) Cumulative Data File. Panel (a) plots the fraction of respondents who report never attending worship services. Panel (b) plots the fraction of respondents who attend worship services weekly. The label "no college" refers to those without a college degree, and "middle-aged" includes respondents aged 45-64. The attendance question in the ANES changed in 1990 (see Appendix A1).

Figure A3: Deaths of Despair by Demographic Group

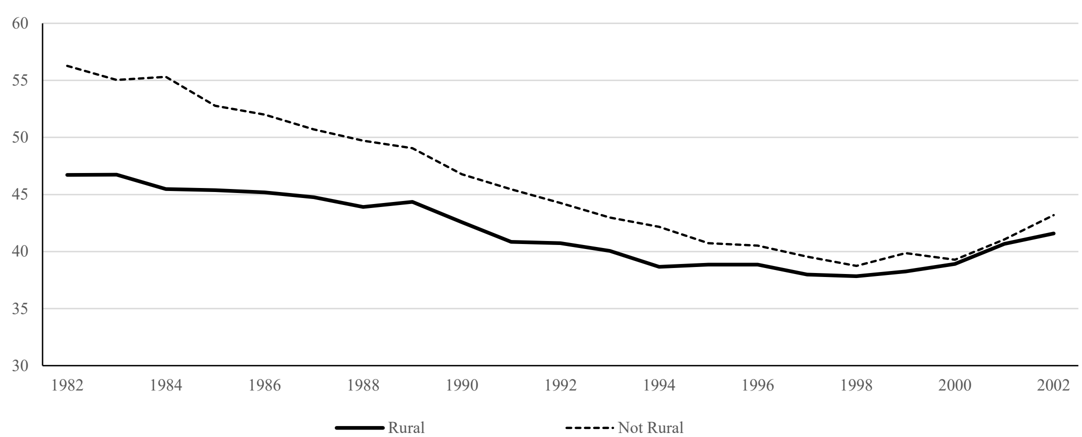
Panel A: By Race



Panel B: By Gender

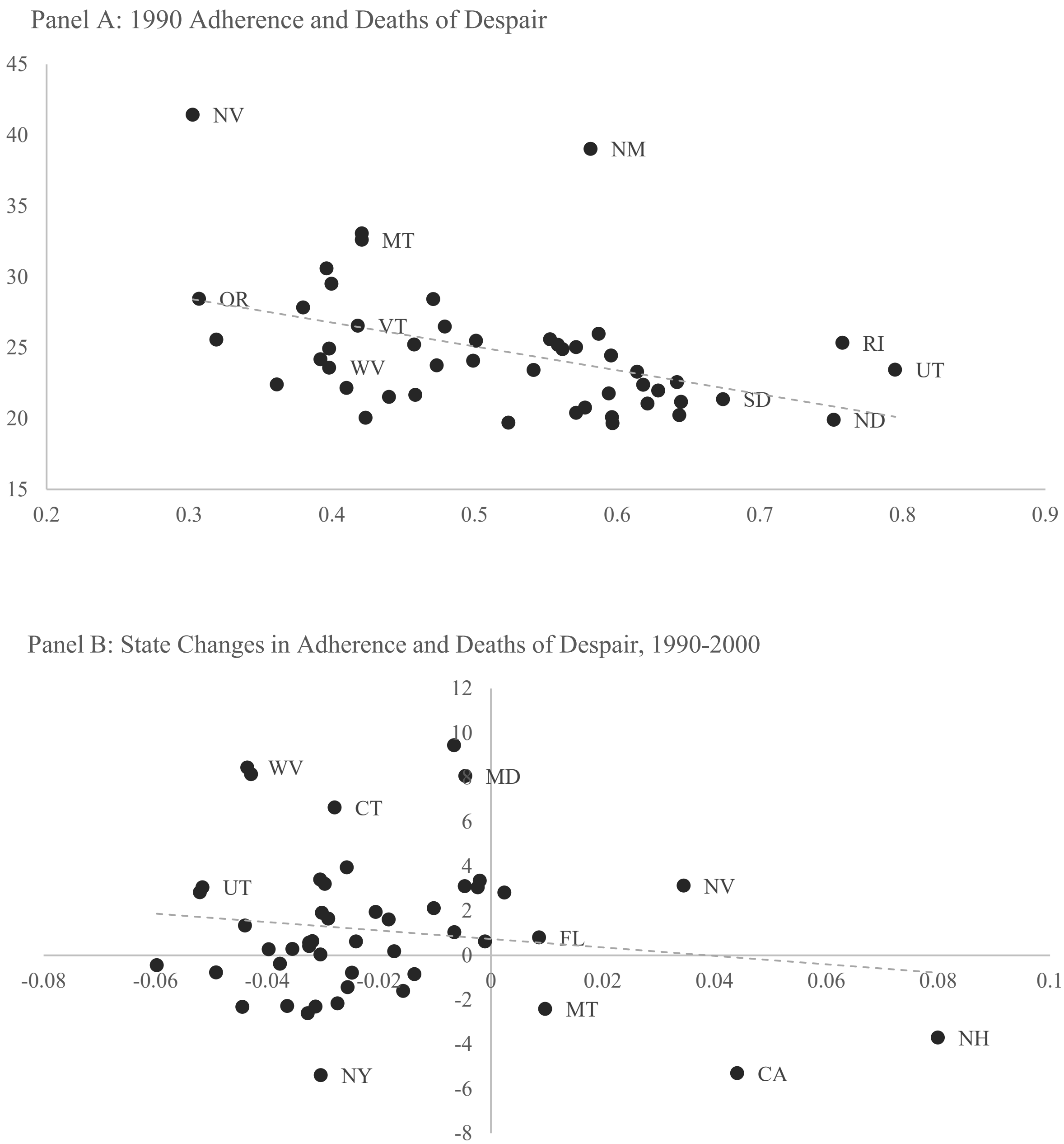


Panel C: By Rural Status



Notes: The figure presents trends in deaths of despair (per 100,000) by subgroup. All ages are included as in Figure 3, which presents analogous trends in weekly attendance. Panel (a) presents trends by race, panel (b) presents trends by gender, and panel (c) presents trends by rural/urban status.

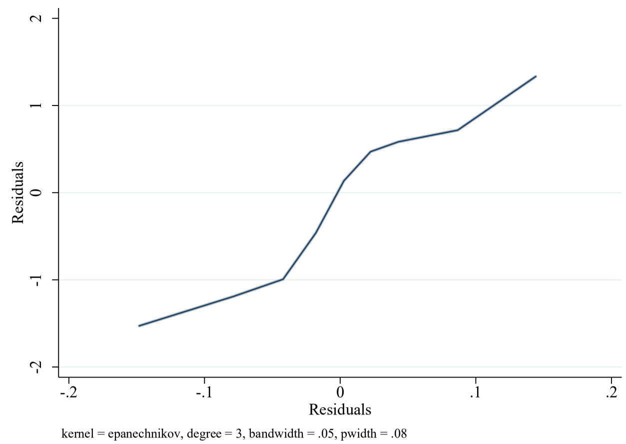
Appendix Figure A4: Adherence and Deaths of Despair in the LRCM



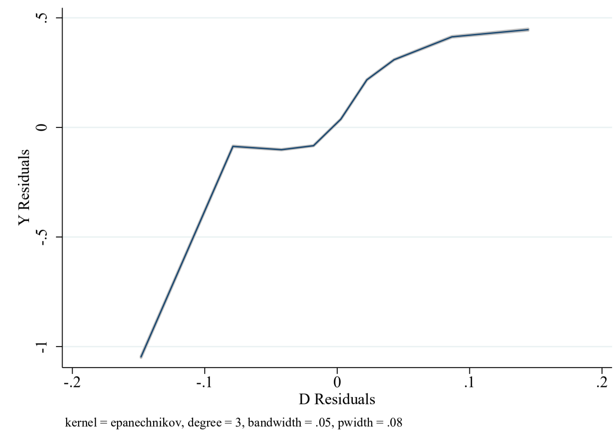
Notes: Figure plots the 1990 and 2000 waves of the Longitudinal Religious Congregations and Membership file. Panel A shows the correlation between state adherence rates in 1990 (on the x axis) and deaths of despair per 100,000 on the y axis. Panel B shows the 2000 - 1990 difference in adherence rates (x axis) and difference in deaths of despair per 100,000 (y axis). The dashed lines represent the best-fit lines.

Figure A5: Relating Regression Weights to Residualized Outcomes

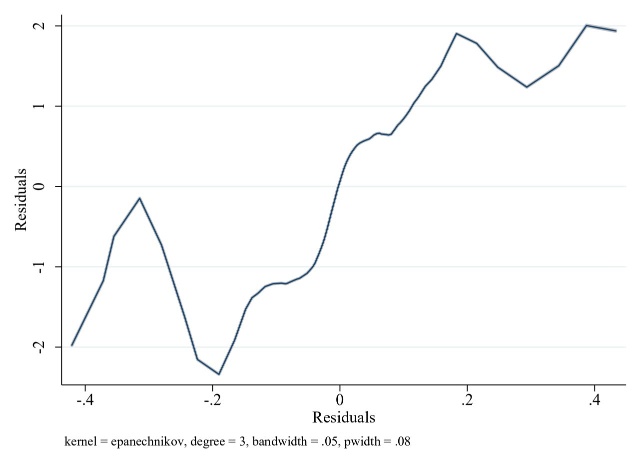
Panel A: No Trends--Deciles



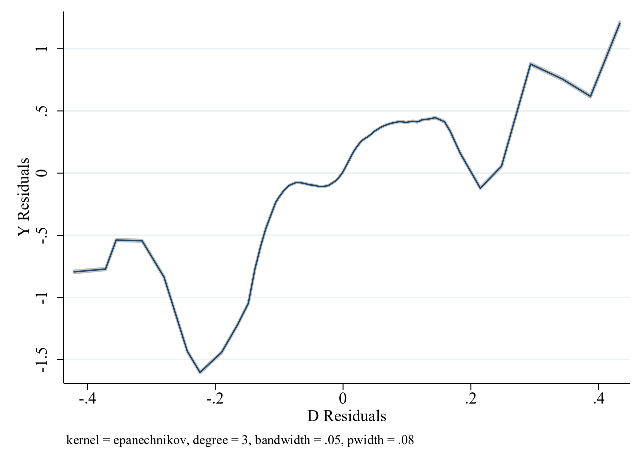
Panel C: Quadratic Trends--Deciles



Panel B: No Trends--Percentiles



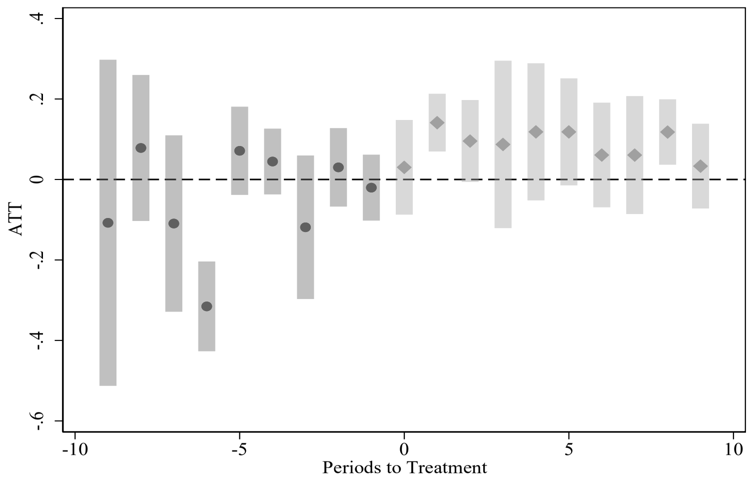
Panel D: Quadratic Trends--Percentiles



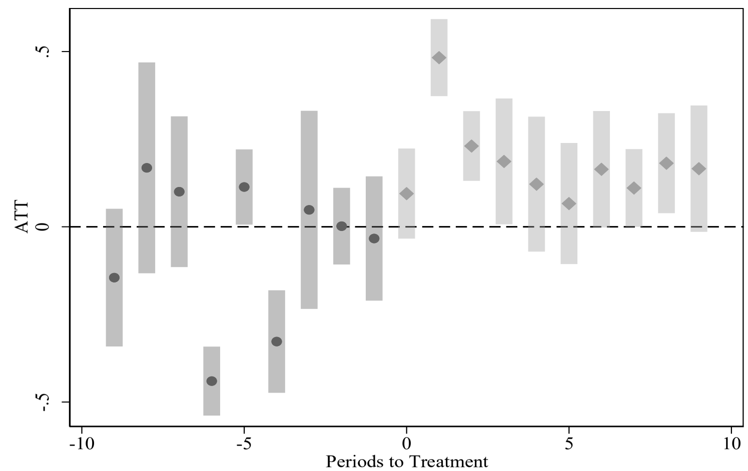
Notes: The figures show a nonlinear regression estimate of the residuals from regressing mortality due to deaths of despair on all other controls excluding blue law repeals and the residuals from regressing a indicator for blue law repeals on all other controls. Panels (a) and (b) are from regressions without state trends. Panels (c) and (d) include linear and quadratic trends. Panels (b) and (d) show estimates calculated at each percentile value of the x axis, while panels (a) and (c) use deciles (so that these figures have a smaller x-axis range and less noise in the tails). A regression of the mortality residuals on the blue law repeal residuals and the square of the blue law repeal residuals in the no-trends case produces coefficients of 5.5 (se = 0.98) and 4.6 (2.8), respectively; for the with-trends case, the coefficients are 2.15 (.67) and -1.77 (2.16).

Figure A6: Robust Effects on Religiosity Over Time

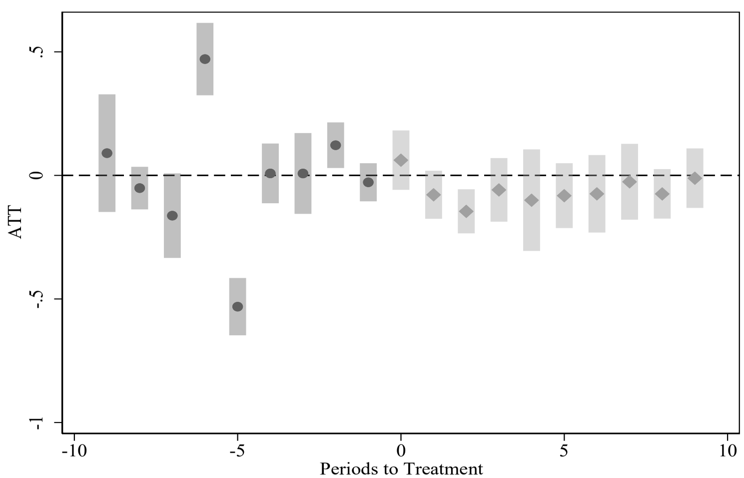
Panel A: Attend Religious Services Once a Year or Less



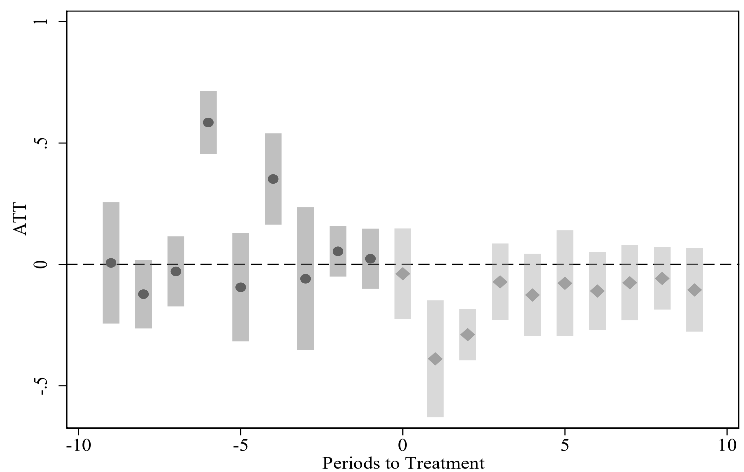
Panel B: Weak Religious Affiliation



Panel C: Attend Religious Services Weekly

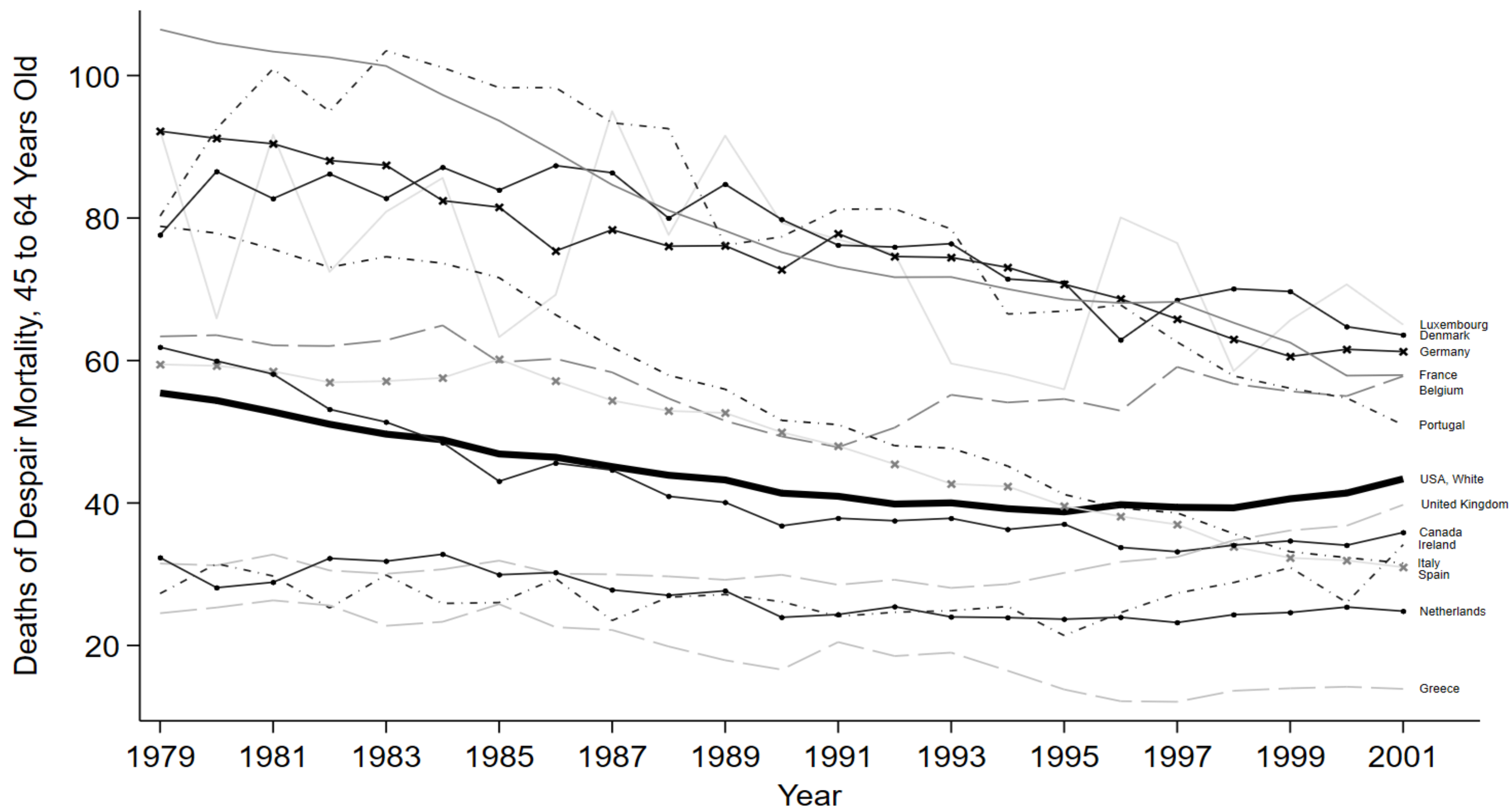


Panel D: Strong Religious Affiliation



Notes: The figure depicts ATT effects before and after treatment (the repeal of blue laws) estimated using the outcome regression method proposed by Callaway and Sant'Anna (2021). All observations that have not repealed blue laws are used as control groups. The shaded areas represent 95% confidence intervals based on standard errors clustered at the state level. The method here makes calculations assuming a repeated cross-section, as the GSS is a repeated cross-section.

Appendix Figure A7: International Rates of Deaths of Despair



Notes: The figure presents deaths of despair mortality rates per 100,000 amongst the middle-aged (45 to 64 year olds) for a collection of countries from 1979 to 2001. The U.S. rate is calculated using white Americans only; all other rates include all racial demographic groups. The mortality rate for white Americans is bolded. See Appendix Section A3 for information on data sources and mortality rate construction.

Table A1: Effect of Blue Law Repeals on Religiosity -- Extensions

	Attend Once a Year or Less (1)	Over Once a Yr, l.t. Weekly (2)	Weekly (3)	Over Weekly (4)	No Religion (5)	Weak or No Religion (6)	Somewhat Strong (7)	Strong (8)
Ages 25-44								
Blue Law Repeals	0.0435	0.0422	-0.0707	-0.0150	0.0208	0.0247	0.00697	-0.0298
	(0.0286)	(0.0291)	(0.0358)	(0.0177)	(0.0239)	(0.0419)	(0.0223)	(0.0401)
Ages 45-64								
Blue Law Repeals	0.0705	-0.000667	-0.0927	0.0229	0.0385	0.194	-0.0821	-0.111
	(0.0471)	(0.0582)	(0.0562)	(0.0278)	(0.0175)	(0.0476)	(0.0291)	(0.0624)
Ages 65 and Up								
Blue Law Repeals	0.0762	0.0925	-0.0498	-0.119	0.0384	0.0787	0.0143	-0.0905
	(0.0463)	(0.0435)	(0.0577)	(0.0397)	(0.0263)	(0.0354)	(0.0522)	(0.0609)
All Ages								
Blue Law Repeals	0.0618	0.0346	-0.0770	-0.0194	0.0271	0.0830	-0.0146	-0.0668
	(0.0168)	(0.0238)	(0.0193)	(0.0121)	(0.0140)	(0.0351)	(0.0211)	(0.0401)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Trends	Linear + Quad	Linear + Quad	Linear + Quad	Linear + Quad	Linear + Quad	Linear + Quad	Linear + Quad	Linear + Quad

Notes: Each coefficient is from a separate regression of equation (1). State-clustered standard errors are in parentheses. The sample includes 20,279 individuals from the General Social Survey from 1973 to 1998. Controls include the fraction of the state population aged 20 to 39, 40 to 64, and over 65, the fraction male, the fraction white, the fraction black and the state population. Individual controls include age, age squared, gender, race, and dummies for educational attainment and for marital status. The first row uses only respondents aged 25-44 at the time of the survey, the second row uses those aged 45-64, and the third row uses those aged 65 and up. The regressions in the first column are a dummy for whether an individual reports attending worship less than once a year (as in Table 2); in column (2), it is a dummy for those attending more than once a year but less than weekly (and zero otherwise), column (3) uses a dummy for weekly attendance (as in Table 2), and column (4) uses a dummy for more than weekly attendance. The next set of columns consider religious intensity. Column (5) uses a dummy for an individual's self-stated religious affiliation as "none." In column (6), we use a dummy indicating than an individual's self-stated religious affiliation is "not very strong" or "none." Column (7) uses a dummy indicating "somewhat strong" religious affiliation (other answers, including "none" are coded as zeros), and column (8) uses a dummy for "strong" religious affiliation.

Table A2: Effect of Blue Laws Repeal on Religiosity by Gender

	Measures of Low Religiosity		Measures of High Religiosity	
	Attend Once a Year or Less (1)	Weak or No Religion (2)	Attend Weekly (3)	Strong Religion (4)
Ages 25-44				
Blue Law Repeals × Female	0.00669	-0.0114	0.00286	0.0105
	(0.0288)	(0.0203)	(0.0192)	(0.0211)
Blue Law Repeals	0.0398	0.0310	-0.0723	-0.0356
	(0.0343)	(0.0458)	(0.0389)	(0.0428)
Ages 45-64				
Blue Law Repeals × Female	0.0197	0.0144	-0.0293	-0.00488
	(0.0263)	(0.0273)	(0.0255)	(0.0253)
Blue Law Repeals	0.0593	0.186	-0.0760	-0.108
	(0.0487)	(0.0468)	(0.0502)	(0.0603)
Ages 65 and Up				
Blue Law Repeals × Female	0.00982	0.0415	-0.0297	-0.0372
	(0.0225)	(0.0354)	(0.0288)	(0.0286)
Blue Law Repeals	0.0698	0.0512	-0.0304	-0.0659
	(0.0504)	(0.0341)	(0.0573)	(0.0597)
All Ages				
Blue Law Repeals × Female	0.00977	0.00101	-0.0116	0.00204
	(0.0164)	(0.0136)	(0.0143)	(0.0135)
Blue Law Repeals	0.0562	0.0825	-0.0703	-0.0680
	(0.0198)	(0.0363)	(0.0222)	(0.0404)
Controls	Yes	Yes	Yes	Yes
State Trends	Linear + Quad	Linear + Quad	Linear + Quad	Linear + Quad

Notes: State-clustered standard errors are in parentheses. The regressions redo the GSS results in table 2, but include an interaction of the repeal dummy with a dummy for gender (the regressions also include non-interacted dummies for gender). The outcome in column (1) is an indicator for whether an individual reports attending worship once a year or less. In column (2), the outcome is an indicator for whether a respondent's stated religious preference is "weak" or "none." Column (3) uses an indicator for whether an individual reports attending worship weekly, and the outcome in column (4) is an indicator for whether a respondent's stated religious preference is "strong."

Table A3: Effect of Blue Laws Repeal on Religiosity by Rural Status

	Measures of Low Religiosity		Measures of High Religiosity	
	Attend Once a Year or Less (1)	Weak or No Religion (2)	Attend Weekly (3)	Strong Religion (4)
Ages 25-44				
Blue Law Repeals × Rural	0.0772	0.0221	-0.0373	-0.0569
	(0.0531)	(0.0385)	(0.0361)	(0.0434)
Blue Law Repeals	0.0271	0.0176	-0.0618	-0.0192
	(0.0295)	(0.0432)	(0.0352)	(0.0381)
Ages 45-64				
Blue Law Repeals × Rural	-0.00266	-0.0430	-0.0119	0.0531
	(0.0763)	(0.0756)	(0.0570)	(0.0772)
Blue Law Repeals	0.0642	0.193	-0.0884	-0.111
	(0.0488)	(0.0515)	(0.0549)	(0.0605)
Ages 65 and Up				
Blue Law Repeals × Rural	-0.00436	-0.0277	-0.0258	-0.0482
	(0.0583)	(0.0327)	(0.0623)	(0.0642)
Blue Law Repeals	0.0737	0.0847	-0.0410	-0.0781
	(0.0516)	(0.0363)	(0.0582)	(0.0635)
All Ages				
Blue Law Repeals × Rural	0.0362	-0.00886	-0.0196	-0.0224
	(0.0564)	(0.0344)	(0.0403)	(0.0500)
Blue Law Repeals	0.0508	0.0801	-0.0705	-0.0597
	(0.0210)	(0.0384)	(0.0210)	(0.0416)
Controls	Yes	Yes	Yes	Yes
State Trends	Linear + Quad	Linear + Quad	Linear + Quad	Linear + Quad

Notes: State-clustered standard errors are in parentheses. The regressions redo the GSS results in table 2, but include an interaction of the repeal dummy with a dummy for rural residence (the regressions also include non-interacted rural-status dummies). The outcome in column (1) is an indicator for whether an individual reports attending worship once a year or less. In column (2), the outcome is an indicator for whether a respondent's stated religious preference is "weak" or "none." Column (3) uses an indicator for whether an individual reports attending worship weekly, and the outcome in column (4) is an indicator for whether a respondent's stated religious preference is "strong."

Table A4: Alternate Specifications for Effect of Blue Law Repeals on Deaths of Despair

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Ages 25-44										
Blue Law Repeals	-0.240	0.501	-0.0115	0.0178	-0.236	0.499	-0.059	0.683	-0.295	0.518
	(0.563)	(0.536)	(0.0152)	(0.0189)	(0.563)	(0.533)	(0.546)	(0.558)	(0.541)	(0.540)
Ages 45-64										
Blue Law Repeals	1.941	2.154	0.0338	0.0462	1.883	2.137	2.02	2.07	1.911	2.157
	(0.754)	(0.669)	(0.0125)	(0.0164)	(0.753)	(0.666)	(.742)	(0.627)	(0.763)	(0.673)
Ages 65-84										
Blue Law Repeals	-1.047	0.210	-0.0181	-0.00106	-1.064	0.169	-0.926	0.506	-1.076	0.176
	(1.021)	(0.980)	(0.0194)	(0.0180)	(1.005)	(0.974)	(0.995)	(0.983)	(1.015)	(0.984)
Mortality Rate	Levels	Levels	Logs	Logs	Levels	Levels	Levels	Levels	Levels	Levels
State Trends	Linear	Linear + Quad	Linear	Linear + Quad	Linear	Linear + Quad	Linear	Linear + Quad	Linear + Quad	Linear + Quad
Age Group Trends	No	No	No	No	Linear	Linear + Quad	No	No	Linear + Quad	Linear + Quad
Alternate Group FEs	No	No	No	No	No	No	No	No	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each coefficient is from a separate regression of equation (2), where the outcome is the mortality rate due to deaths of despair (per 100,000). Standard errors clustered by state are in parentheses. Regressions are weighted by population and include the same controls as in table 3. The first two columns reproduce the main estimates using state linear or linear and quadratic trends. Columns (3) and (4) use the natural logarithm of deaths as the dependent variable. Columns (5) and (6) add age group-specific trends. Columns (7) and (8) add controls for state per capita spending on Medicaid and Medicare. The last two columns repeat the specifications of columns (5) and (6), but here state fixed effects are replaced by state-by-age-bin-by-race fixed effects for each five-year age bin and for the white and nonwhite racial groups.

Table A5: Effect of Blue Law Repeals on Mortality by Race

White

	(1)	(2)	(3)	(4)	(5)
Ages 25-44					
Blue Law Repeals	-0.627	0.215	-0.139	0.312	0.0425
	(0.569)	(0.606)	(0.220)	(0.392)	(0.318)
Ages 45-64					
Blue Law Repeals	2.005	1.987	0.496	0.254	1.237
	(0.765)	(0.620)	(0.558)	(0.227)	(0.267)
Ages 65-84					
Blue Law Repeals	-0.978	0.559	0.439	-0.320	0.440
	(0.968)	(1.054)	(0.763)	(0.201)	(0.601)

Nonwhite

	(1)	(2)	(3)	(4)	(5)
Ages 25-44	2.674	2.447	1.084	-0.525	1.888
Blue Law Repeals	(2.048)	(1.600)	(1.162)	(0.929)	(0.697)
Ages 45-64	2.584	4.494	3.107	-0.139	1.527
Blue Law Repeals	(1.854)	(2.060)	(1.927)	(0.610)	(0.411)
Ages 65-84	-1.104	-2.385	-0.478	-1.836	-0.0721
Blue Law Repeals	(2.836)	(2.286)	(2.259)	(0.574)	(0.411)
Mortality Cause	All	All	Liver	Poison	Suicide
State Trends	Linear	Linear + Quad	Linear + Quad	Linear + Quad	Linear + Quad
Controls	Yes	Yes	Yes	Yes	Yes

Notes: Each coefficient is from a separate regression of equation (2), where the outcome is the mortality rate due to deaths of despair for white (top panel) and nonwhite (bottom panel) individuals per 100,000. The sample, controls and weighting are the same as in table 3.

Table A6: Effect of Blue Law Repeals on Mortality, Dropping Each State

Panel A: No Trends

No AZ	No CA	No CO	No FL	No IA	No ID	No IN	No KS	No MN	No ND	No NM	No NV
9.553	6.026	9.240	9.282	9.636	9.402	9.397	9.649	9.818	9.389	9.562	9.188
(2.230)	(1.483)	(2.252)	(2.539)	(2.197)	(2.207)	(2.253)	(2.178)	(2.241)	(2.224)	(2.219)	(2.248)
No OH	No OR	No PA	No SC	No SD	No TN	No TX	No UT	No VA	No VT	No WA	No WY
9.936	9.332	8.460	9.343	9.203	9.191	8.644	9.275	9.539	9.360	9.258	9.288
(2.227)	(2.256)	(2.068)	(2.341)	(2.221)	(2.316)	(2.677)	(2.204)	(2.235)	(2.201)	(2.346)	(2.229)

Panel B: Linear and Quadratic State Trends

No AZ	No CA	No CO	No FL	No IA	No ID	No IN	No KS	No MN	No ND	No NM	No NV
2.219	1.912	2.170	2.130	2.081	2.160	2.002	2.234	2.152	2.246	2.180	2.204
(0.684)	(0.843)	(0.686)	(0.658)	(0.679)	(0.666)	(0.702)	(0.652)	(0.756)	(0.644)	(0.678)	(0.673)
No OH	No OR	No PA	No SC	No SD	No TN	No TX	No UT	No VA	No VT	No WA	No WY
1.817	2.064	1.938	1.813	2.103	2.369	2.737	2.127	2.148	2.121	2.152	2.123
(0.774)	(0.680)	(0.812)	(0.650)	(0.676)	(0.763)	(0.565)	(0.681)	(0.706)	(0.684)	(0.623)	(0.675)

Notes: Each coefficient is from a separate regression of equation (2), where the outcome is mortality due to deaths of despair (per 100,000) for ages 45-64. Each regression omits one of the 24 states from the sample. Standard errors are clustered at the state level and given in parentheses. All regressions include right-hand-side controls, year dummies, and state dummies, and are weighted by population. Panel (a) omits state-specific trends, and panel (b) includes linear and quadratic trends.

Table A7: Effect of Blue Laws Repeal on Religiosity by Religious Tradition

	Measures of Low Religiosity		Measures of High Religiosity	
	Attend Once a Year or Less	Weak or No Religion	Attend Weekly	Strong Religion
	(1)	(2)	(3)	(4)
Ages 25-44				
Blue Law Repeals × Catholic	0.0146	-0.0638	0.0103	0.0362
	(0.0341)	(0.0220)	(0.0344)	(0.0287)
Blue Law Repeals	0.0340	0.0330	-0.0748	-0.0324
	(0.0303)	(0.0394)	(0.0383)	(0.0396)
Ages 45-64				
Blue Law Repeals × Catholic	0.0124	-0.0177	0.0163	0.0223
	(0.0276)	(0.0517)	(0.0328)	(0.0492)
Blue Law Repeals	0.0605	0.186	-0.113	-0.106
	(0.0381)	(0.0439)	(0.0510)	(0.0625)
Ages 65 and Up				
Blue Law Repeals × Catholic	-0.0119	-0.0447	0.0912	0.0275
	(0.0360)	(0.0360)	(0.0357)	(0.0385)
Blue Law Repeals	0.0594	0.0732	-0.0609	-0.0874
	(0.0422)	(0.0388)	(0.0559)	(0.0654)
All Ages				
Blue Law Repeals × Catholic	0.00970	-0.0519	0.0256	0.0367
	(0.0292)	(0.0258)	(0.0303)	(0.0267)
Blue Law Repeals	0.0510	0.0843	-0.0874	-0.0664
	(0.0177)	(0.0327)	(0.0211)	(0.0392)
Controls	Yes	Yes	Yes	Yes
State Trends	Linear + Quad	Linear + Quad	Linear + Quad	Linear + Quad

Notes: State-clustered standard errors are in parentheses. The regressions redo the GSS results in table 2, but include an interaction of the repeal dummy with a dummy for religious tradition (the regressions also include non-interacted religious tradition dummies). The outcome in column (1) is an indicator for whether an individual reports attending worship once a year or less. In column (2), the outcome is an indicator for whether a respondent's stated religious preference is "weak" or "none." Column (3) uses an indicator for whether an individual reports attending worship weekly, and the outcome in column (4) is an indicator for whether a respondent's stated religious preference is "strong."

Table A8: Trends in Religious Attendance; European Countries and the US

Year	France	Belgium	Netherlands	Germany	Italy	Lux.	Denmark	Ireland	Britain	N. Ireland	US
1973	19	38	33	22	48	48	5	91	16	59	28
1985	12	27	24	19	37	32	6	88	8	58	33
1994	11	27	28	16	41	22	3	77	12	54	27

Notes: The European data is taken from Table 3.5 in Norris and Inglehart (2004) and are calcualted from the Mannheim Eurobarometer Trend File 1970-1999; each column shows the percentage of respondents reporting attending religious services "several times a week" or "once a week." The US data are the author's calculations using the number of respondents reporting weekly or more than weekly attendance in the GSS. The definition of this variable and the use of single years of data differs slightly from the construction in figure 2 earlier; remaking figure 2 using these definitions of attendance and years produces similar results. Three countries in Norris and Ingelhart's table (Greece, Portugal, and Spain) lack data before 1980 and are ommitted here. The fraction of respondents with weekly or more attendance in Greece falls from 27 to 24 from 1985 to 1995; for Spain it is 47 to 36. Portugal lacks any data prior to 1988. Northern Ireland lacks data in 1973 and 1994; for this country the above table uses 1975 and 1992 instead.