From the Dial to the Aisle: The Effects of Talk Radio

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Abstract

I study how the deregulation of radio content in the U.S. transformed local politics and public health outcomes. The 1987 repeal of the FCC's Fairness Doctrine removed requirements for broadcasters to air contrasting viewpoints on political issues and triggered the expansion of conservative talk radio. I find that quasi-exogenous exposure to growth in conservative talk radio raised Republican vote shares across presidential, Senate, and House elections, and also increased "deaths of despair" (alcohol-, overdose-, and suicide-related mortality). These effects emerge in the early 1990s and persist decades later, underscoring how partisan media ecosystems can reshape both politics and public health.

JEL Codes: D72, L82, N32, N42, I12

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1 Introduction

Political polarization in the United States has risen markedly over the past four decades (Gentzkow, 2016). Since the late 1980s, ideological overlap between Democrats and Republicans has diminished considerably: by 2014, 92% of Republicans were positioned to the right of the median Democrat, and 94% of Democrats were left of the median Republican (Pew Research Center, 2014). Coinciding with this increase in political polarization has been a large decline in trust in non-political institutions, which itself has become polarized. Democrats now trust science, the press, labor, and higher education far more than Republicans, while Republicans express more confidence than Democrats in police, religion, business, and the military (Brady and Kent, 2022). These trends have spurred a substantial academic literature on the drivers of contemporary polarization, with particular focus on partisan media.[1] Yet one of the most widespread and ideologically uniform sources of political information—talk radio—has attracted comparatively less empirical attention. Despite reaching tens of millions of weekly listeners across the United States throughout the 1990s and 2000s, radio's role in shaping contemporary beliefs and polarization is not well understood.

In this paper, I exploit the 1987 repeal of the Federal Communication Commission's (FCC's) Fairness Doctrine, which lifted content-balancing requirements and opened public airwaves to explicitly partisan programming, to examine the effect of deregulating radio content on voting behavior and public health. I show that in the decade following the Fairness Doctrine's repeal, talk radio's popularity grew rapidly, and the number of stations devoted to talk radio nearly quintupled. This growth was driven almost entirely by conservative voices: by 1993, conservative radio host Rush Limbaugh's daily show (first nationally syndicated in 1988) was broadcast on 610 stations with a weekly audience of over 17 million Americans, and in 2010, eight of the ten most popular radio hosts were conservative, while none were liberal (Berry and Sobieraj, 2011; Rosenwald, 2021). Using newly-digitized radio

[1]For overviews of the literature, see DellaVigna and Gentzkow (2010); Prat and Strömberg (2013); Enikolopov and Petrova (2015); Zhuravskaya et al. (2020).

broadcasting data from Arbitron and leveraging variation across radio markets in pre-period talk radio market shares as an instrument for post-repeal talk radio growth, I show that counties more exposed to post-repeal conservative talk radio shift systematically rightward: Republican vote share increases in subsequent presidential, Senate, and House elections, and Republican representatives become more ideologically extreme. These political realignments coincide with increases in "deaths of despair" (suicides, drug overdoses, and alcohol-related mortality) in exposed areas. Complementary post-repeal survey evidence shows that individuals who rely principally on radio for political news express lower trust in medicine and psychiatric care, suggesting one mechanism through which media influence can extend beyond political persuasion and into public health.

The Fairness Doctrine was a rule introduced by the FCC in 1949 which stipulated that in order to receive a broadcast license, stations had to both "devote reasonable attention to the coverage of controversial issues of public importance" and "air contrasting sides of those issues" (Simmons, 1977). The doctrine has been called "the most successful episode of government censorship of the last half century," and its repeal—part of the sweeping deregulation of Ronald Reagan's presidency—fundamentally changed what content was provided on public airwaves (Matzko, 2020). Although popular commentary often assumes that radio hosts such as Rush Limbaugh and Sean Hannity, who rose to prominence after the doctrine's repeal, shaped voter behavior—and, more broadly, the direction of the Republican Party—rigorous empirical evidence on their effects remains limited (Edsall, 1994; Hemmer, 2022).[2]

To examine the effects of the Fairness Doctrine's repeal, I combine its nation-wide timing shock with cross-market variation in how much "room" there was for talk radio to expand. Specifically, I predict post-Doctrine growth in talk radio using the pre-repeal share of local

[2]On Limbaugh, Hemmer writes, "His style didn't just influence political broadcasting; it influenced an entire political party, so much so that, a quarter-century later, more politicians would sound like Rush Limbaugh than Ronald Reagan—and more lived in fear of crossing the radio host than of deviating from the former president's political legacy." Edsall writes, "Rush Limbaugh III has done more to shape the tone of national political discussion than any member of the House and Senate, than any cabinet level appointee, than the chairmen of both the Democratic and Republican parties or the anchors of the major network news broadcasts."

listening already devoted to talk formats: markets that had low talk radio penetration in 1980, yet similar overall radio audiences and station counts, experienced the largest inflows of new syndicated talk shows after content-balancing requirements were removed, where entry was easier with fewer existing talk competitors. Recent advances in satellite technology had also substantially lowered the costs of syndication, allowing AM stations unable to afford the costs of in-house talk radio production to cheaply broadcast syndicated content, converting the "specialized and geographically constricted field of talk radio into a national, syndicated marketplace, altering the competitive pattern of radio stations in hundreds of urban, suburban, and rural communities" (Viles, 1993; Edsall, 1994). I show that prior to 1987, relying on radio for political news was not predictive of an individual's party identification, indicating that baseline exposure was unrelated to partisan leanings. Using these pre-period talk radio market shares as an instrument for growth, I implement an instrumented difference-in-differences design using data from 152 Arbitron Radio Metros, comprising 576 counties and two-thirds of the United States population (Duflo, 2001).

In event-study analyses, I show that conservative talk radio had significant and lasting effects on political outcomes. Counties in the highest quartile of predicted talk radio growth shifted rightward by 4.1 percentage points in presidential elections, 3.9 percentage points in Senate elections, and 5.8 percentage points in House elections compared to the lowest quartile, effects that arise by 1992 and persist more than 30 years later. Importantly, none of the event studies exhibit pre-trends in elections between 1968 and 1987, and results are robust to the inclusion of an increasing number of flexible controls. I include various pre-period county-level measures—such as the rural population share and the share of adults with a college degree—interacted with year fixed effects to partial out secular trends in party composition unrelated to radio, and additionally add controls (also interacted with year fixed effects) for exposure to the "China shock" and NAFTA, two economic shocks that increased Republican Party affiliation (Autor et al., 2020; Choi et al., 2021). All specifications also include state-by-year fixed effects to isolate the effects of talk radio growth from potential state-level policies or other unobserved state-level variation over time.

In addition to political outcomes, I study how media-driven political ideology can shape public health, specifically focusing on "deaths of despair", which have risen in recent decades and reshaped overall United States mortality patterns in certain demographic groups (Case and Deaton, 2015, 2021). I find that counties in the highest quartile of predicted talk radio growth experienced an additional 2.8 deaths of despair per 100,000 people, a roughly 12.2 percent increase, which emerges by the mid-1990s and increases over time. Moreover, I provide descriptive evidence that talk radio listeners in the post-repeal era had less confidence in medical institutions, especially regarding treatment for mental illness: these individuals were significantly less willing to ever use psychiatric medication for depression, had lower beliefs about the efficacy of medication, and were less likely to recommend psychiatric care to others experiencing mental illness compared to consumers of other forms of media. These findings suggest that politically polarized local media environments can have profound and unintended consequences for population health.

This paper contributes to a large literature on the effects of media on political persuasion. Academic work in this area has found that exposure to partisan cable news in the United States (specifically Fox News) increases Republican vote share (e.g. DellaVigna and Kaplan 2007; Martin and Yurukoglu 2017), while other work has found comparable political persuasion effects of television content in other countries (e.g. Enikolopov et al. 2011; Durante et al. 2019). Studies of other traditional media sources have found effects on voter persuasion or turnout in print media (e.g. Gerber et al. 2009; Gentzkow et al. 2011) and radio (e.g. Wang 2021; Engist et al. 2024), while a newer literature has explored persuasion and mobilization in social media (for a review, see Zhuravskaya et al. (2020)). In particular, my paper is closely related to the subset of the literature studying the effects of radio more broadly, which has linked radio exposure across a variety of settings to changes in historical political and social outcomes. In the United States, the closest works are Wang (2021), who studies Father Charles Coughlin's populist radio show, which attracted as many as 30 million weekly listeners across the United States during the 1930s, and Engist et al. (2024), who study political radio in the era of the Fairness Doctrine from 1950-1970. Wang (2021) finds exposure to Coughlin's broadcasts decreased support for Franklin D. Roosevelt in the 1936 election and increased antisemitism, and Engist et al. (2024) find small conservative persuasive effects of radio in presidential elections. In other work, Strömberg (2004) finds that areas with more radio listeners received more relief funds in the New Deal, providing evidence for the link between radio listenership and political action. Finally, in work outside of economics that studies the influence of Rush Limbaugh specifically, Barker and Knight (2000) employ an observational study to note that listening to Rush Limbaugh is associated with holding similar beliefs as Limbaugh on issues he discusses on his radio show. However, they acknowledge the difficulty of making any credible causal claims about influence, given the selection involved with being a regular listener to the Rush Limbaugh show. This paper uses a natural experiment—the repeal of the Fairness Doctrine—to provide long-term causal evidence of the political effects of talk radio in a more contemporary setting, providing evidence that it is a meaningful contributor to increased political polarization seen in the United States today.

These findings are also situated within a growing body of work showing that media environments can reshape broader social outcomes, including incitement of violence and public health. A rich historical literature links incendiary or biased broadcasts to intergroup conflict and violence, and modern entertainment media has been shown to alter other social- and health-related behaviors.[3] More similar to this study, a growing descriptive literature links partisan environments and cultural upheaval to the rise in deaths of despair (Montez et al., 2022; Warraich et al., 2022; Oberlander, 2024), while other work has documented links between media exposure and risky health behaviors, particularly suicide (Gould et al., 2003; Niederkrotenthaler et al., 2010; Sisask and Värnik, 2012). Recognizing that access

[3]Regarding media and conflict, local screenings of *The Birth of a Nation* increased lynchings and Ku Klux Klan membership in the early-twentieth-century South (Ang, 2023); the extremist RTLM radio signal fuelled participation in the Rwandan genocide (Yanagizawa-Drott, 2014); BBC radio increased political violence in WWII Italy (Gagliarducci et al., 2020); cross-border Serbian radio increased anti-Serbian sentiment in Croatia (DellaVigna et al., 2014); and Nazi-controlled radio escalated antisemitic acts in pre-war Germany (Adena et al., 2015). Regarding modern entertainment media, television content affected religiosity in Poland (Grosfeld et al., 2024), the staggered roll-out of cable television in rural India reduced fertility and improved women's autonomy (Jensen and Oster, 2009), and exposure to Brazilian soap operas portraying small families likewise lowered fertility rates (Ferrara et al., 2012).

to information environments can influence these outcomes, the CDC now includes "access to mass media" among its 12 social determinants of health (CDC, 2025). While most of these studies are descriptive, recent work has shown that exposure to opioid marketing campaigns can both increase mortality and shift voting patterns (Arteaga and Barone, 2022). This paper builds on these insights by exploiting the deregulation-induced expansion of conservative talk radio to provide evidence that media-driven political realignments can translate into persistent increases in despair-related mortality, extending the documented social reach of mass communication from collective violence and demographic behavior to population health.

The rest of the paper is organized as follows. Section 2 provides a detailed background of the research setting, the Fairness Doctrine, and the rise of conservative talk radio. Section 3 describes the various data sources used throughout this project. Section 4 discusses the empirical strategy for causal identification in this paper, which centers around an instrumented difference-in-differences design. Section 5 discusses the paper's results, and Section 6 concludes.

2 Background

The Fairness Doctrine was a policy introduced by the FCC in 1949 which required all holders of broadcast licenses to devote time to contrasting views when discussing contentious matters deemed to be in the public interest. The doctrine was borne out of concern that the three main networks of their time—ABC, CBS, and NBC—could use their broadcasts, which were delivered over publicly-owned airwaves, to advance private interest, rather than serve their communities.

The doctrine had two basic elements: the first was that broadcast stations had to devote airtime to discussing matters of public interest, and the second was that they had to air contrasting views regarding these matters. Failure to comply with the doctrine could lead to the full revocation of one's broadcast license.

While the Fairness Doctrine is clear conceptually, how it worked in practice is a separate issue. The Supreme Court case *Red Lion Broadcasting Co. v. FCC* (1969) provides a helpful example. In this case, journalist Fred J. Cook, who had recently written a scathing book about senator Barry Goldwater, the Republican Party's 1964 nominee for president, was the topic of a broadcast by Billy James Hargis, host of the popular *Christian Crusade* radio station on WGCB in Red Lion, Pennsylvania. Over the course of a 15-minute-long segment, Hargis criticized the book and Cook himself, alleging that Cook was affiliated with Communists. Cook demanded free airtime on WGCB to respond to the allegations, which was permissible under the Fairness Doctrine. Red Lion Broadcasting rejected the request, and the FCC ruled that they had violated the Fairness Doctrine. Red Lion Broadcasting filed suit, arguing the Fairness Doctrine was a violation of their First Amendment rights as broadcasters. The issue was eventually elevated to the Supreme Court, who ruled unanimously in favor of the FCC, arguing that although broadcasters enjoyed free speech rights under the First Amendment, the FCC could partially restrict these rights to ensure the equitable and public interest use of public airwaves.

There are two main things to glean from the case: the first is that the Fairness Doctrine was enforced with genuine regulatory authority. The second—and perhaps more important—detail to note regards the monetary costs involved with discussing political issues over radio during this era. Beyond allowing for airtime to discuss opposing viewpoints to discuss contentious issues, it's important from a financial standpoint that this airtime was free. Whenever a listener heard a contentious point on a broadcast, they could call in and demand free airtime to explain an opposing side. All in all, this could add up to substantial free airtime given to anyone who wanted opposing viewpoints presented. This was explained by Rush Limbaugh himself in the excerpt below:

> *The way the Fairness Doctrine would work—and it's being set up this way—is professional complainers hear me—take any element of today's show—criticizing Harry Reid, Ted Kennedy. Within minutes the general managers of 600 radio stations would receive phone calls from MoveOn.org-type activists demanding that they get a chance to respond to what I said, and they might put 'em off for a while, but they'd keep calling and keep calling, and if the Fairness Doctrine were law, they would have to grant that, and then the station managers would say, 'To hell with this! We can't run a business this way. This is ridiculous. We're turning over*

the programming, literally, to people who aren't broadcasters. We're a business,' and so they just cancel all the, quote, unquote, controversial programming and they'd have to go back to, you know, doing things that nobody wanted to listen to, which is what happened when radio was regulated so much in the first place.

— Rush Limbaugh, *The Rush Limbaugh Show*, June 28, 2007

In this excerpt, Limbaugh specifically references the business incentives as being the limiting factor in facilitating a very restrained radio environment. Related to this were costs from the Fairness Doctrine's "balanced coverage" rule: airing three hours of partisan commentary obliged a station to provide (or purchase) an equal block for the opposing view—which was potentially much less popular—effectively doubling programming costs and using valuable airtime.[4] With the ease of demanding free airtime, the need for balanced coverage, and the worries of license revocation with non-compliance, discussing politically contentious matters was not a financially sensible decision for owners of broadcast licenses.

Overall, the issues posed by the Fairness Doctrine were salient to broadcasters. Limbaugh mentioned the doctrine in nearly 150 different episodes of his show, and personally attributed the resurgence of talk radio—and its conservative bent—to the repeal of the doctrine (Matzko, 2020). Similar to Limbaugh, Brian Rosenwald notes in his book *Talk Radio's America: How an Industry Took Over a Political Party That Took Over the United States* that it was the owners of radio stations who worried about broadcasting politically charged content, as "the lack of balance might land them in hot water with the FCC" (Rosenwald, 2019). The threat of regulatory compliance, combined with the prohibitive costs of ensuring balanced coverage, led to near-universal compliance with the doctrine.

When the Fairness Doctrine was in place, radio was dominated by music, and what little talk radio existed was primarily nonpolitical.[5] Rosenwald writes "The talk programming that flourished in a limited range of markets during the 1960s and 1970s sounded nothing

[4]In his essay "Host" for *The Atlantic*, David Foster Wallace notes, "Because of the Fairness Doctrine, talk stations had to hire and program symmetrically: if you had a three-hour program whose host's politics were on one side of the ideological spectrum, you had to have another long-form program whose host more or less spoke for the other side... The crucial connection with the F.D.'s repeal was not Rush's show but that show's syndicatability. A station could now purchase and air three daily hours of Limbaugh without being committed to programming another three hours of Sierra Club or Urban League or something" (Wallace, 2005).

[5]There are some exceptions to this rule, such as in Engist et al. (2024).

like what Limbaugh would bring to the masses... Before the revolution Limbaugh sparked, hosts came in all ideological stripes, and most kept their political views to themselves. New York star Barry Farber believed that most hosts in his era would 'fly down to the Amazon and get our head shrunk before it would occur to attack the President'" (Rosenwald, 2019).

The nature of radio in the United States changed substantially when the Fairness Doctrine was repealed. In 1985, during the Reagan administration, the FCC released a report stating that the doctrine both hurt the public interest and violated First Amendment rights. On August 4, 1987, the FCC repealed the Fairness Doctrine entirely. In June 1987, before the FCC decision was made, Congress attempted to bypass the FCC decision and codify the doctrine. The bill passed but was eventually vetoed by then-president Ronald Reagan.

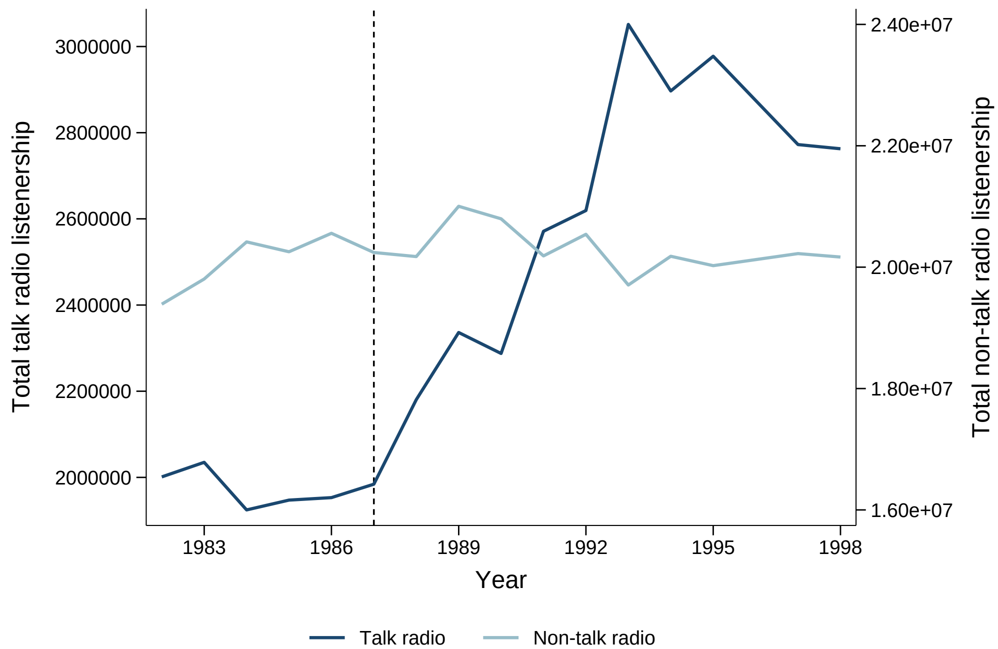
With the Fairness Doctrine no longer in place, radio became a much more permissive environment. Hosts were free to discuss issues without FCC requirements to provide fair or balanced coverage. This change, combined with recent advances in satellite technology that made national syndication cheaper and easier, led to a rapid rise in the popularity of talk radio. As shown in Figure 1, talk radio listenership grew over 38% between 1987 and 1998, while all other radio grew only 4%. The rise in popularity also coincides with the repeal of the Fairness Doctrine: in the five years before the repeal, talk radio listenership was flat or slightly decreasing, with a surge in popularity occurring in 1988, immediately after repeal.[6]

Beyond listenership, in the decade following radio deregulation, the number of talk radio stations grew substantially, shown in Figure 2. Similar to Figure 1, the number of talk radio stations was flat in the years prior to deregulation, with a steady increase happening after 1987. Ten years after repeal of the doctrine, the number of talk radio stations had grown from 127 to 633, a nearly five-fold increase, and the overall share of total radio stations devoted to talk radio had also grown considerably.

While the national trends show an immediate rise in talk radio popularity following deregulation, there was substantial geographic variation in its popularity. This is shown in

[6]The immediacy of the increase in listenership should not be surprising, given how quickly hosts reacted. For example, Rush Limbaugh's talk show on WABC-AM in New York City was first nationally syndicated on August 1, 1988, less than one year after the doctrine's repeal.

Figure 1: Talk radio vs. other radio growth over time

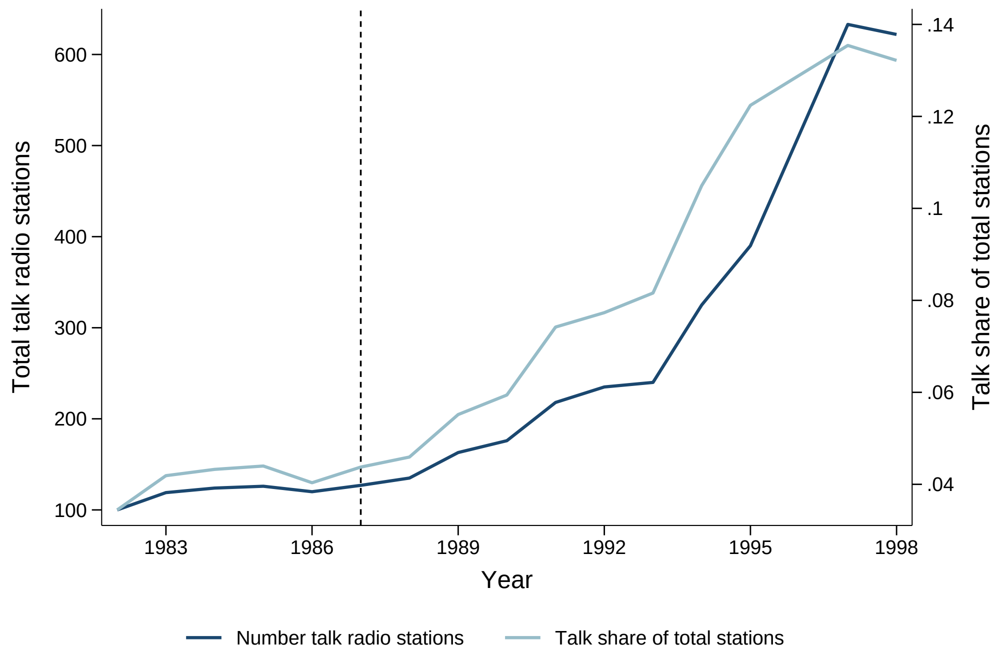


Notes: This figure plots growth in talk radio vs. growth in all other forms of radio from 1982 to 1998 among Arbitron-tracked markets. The left Y-axis plots total talk radio listenership over time, measured as the average total number of people listening to a station for at least 5 minutes within a 15-minute period (measured from 6 a.m. to midnight Monday-Friday). This measure is called average quarter-hour persons and is the standard measure of listenership in radio broadcasting. The right Y-axis contains the same measure for all forms of radio besides talk radio.

Appendix Figure A1, which displays the cross-market distribution of talk radio's share of total radio listenership over time. Between 1980 and 1987, the distributions are very similar, but talk radio's average share increases 66% by 1995. The distribution shifts to the right in the years following deregulation, but preserves a substantial amount of variation across markets in talk radio prominence.

Increases in listenership were driven by the introduction of new kinds of talk radio, which were predominantly political, often entertaining, and frequently openly conservative. The leader of this transition was Rush Limbaugh, the most popular conservative talk-show host of his era. Appendix Figure A2 shows the Pew Research Center's estimates of the most popular talk radio hosts in 2003, 2007, and 2010. As shown in the figure, Rush Limbaugh was consistently attracting upwards of 15 million unique weekly listeners throughout the

Figure 2: Growth in talk radio stations over time

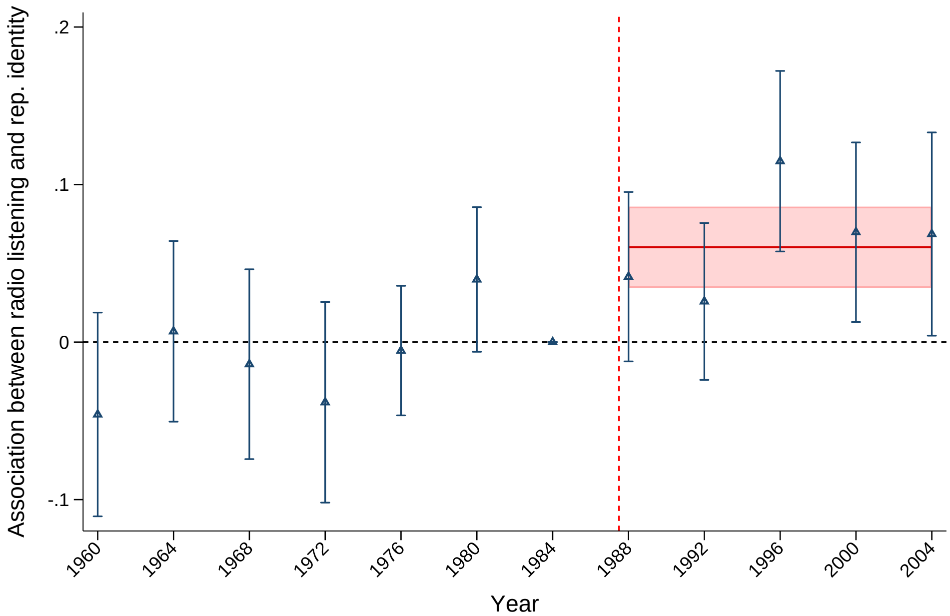


Notes: This figure plots growth in talk radio stations over time. The left Y-axis plots the total number of talk radio stations, while the right Y-axis plots the share of total stations (AM and FM) devoted to talk radio.

2000s, and 11 of the 16 most popular radio hosts were specifically political conservatives hosting conservative talk shows. Although the figure highlights popularity throughout the 2000s, conservative talk radio had already been established and growing for roughly a decade by that point. The Fairness Doctrine was repealed on August 4, 1987, and Rush Limbaugh's talk show on WABC-AM in New York City was first nationally syndicated on August 1, 1988, less than one year after the doctrine's repeal. In 1990, two years after Limbaugh's syndication, *The New York Times Magazine* wrote that "after only two years on the national dial, he has more listeners (about five million a week) than any other talk-show host and a list of stations (nearly 300) that grows every day" (Grossberger, 1990).

The contrast of this new form of talk radio is also evident when looking to the changing composition of radio listeners over time. Figure 3 shows how listening to political news on the radio related to identifying (or leaning) Republican, as estimated each survey year using data from the American National Election Studies (ANES). Before 1987—while the Fairness

Figure 3: Relationship between talk radio listening and Republican identity over time



Notes: This figure uses ANES data to show how the relationship between listening to political news on the radio and Republican beliefs has changed over time. The Y-axis variable is an indicator for whether the respondent identifies as a Republican, and includes Republican-leaners. The X-axis variable is an indicator for whether the respondent had heard about any political campaigns on the radio. The regression is estimated each year by interacting survey year indicators with the radio indicator. The regression controls for state, year, and state-by-year fixed effects. County identifiers are not available after 1996, but the estimates up to 1996 when using county fixed effects are nearly identical.

Doctrine was still enforced—this relationship hovers around zero and is never statistically significant. In the decade following repeal, however, the coefficient turns strongly positive and remains so for the rest of the sample, indicating those tuning in were increasingly Republican-leaning.

3 Data

Data for this paper come from a variety of different sources. The data sources can be broken out into three main categories: political data, media data, and health data. Each of these areas will be discussed in turn.

Political Data

The main political data used in this paper are presidential, Senate, and House voting outcomes from 1964-2022 that come from ICPSR's General Election Data for the United States and Dave Leip's Atlas of U.S. Elections (ICPSR, 2013; Leip, 2024). The ICPSR data contain election returns for 1964-1990, while the Atlas of U.S. Elections data contain election returns for 1992-2022. These data are available at the county level, which allows for a granular understanding of how the rise of conservative talk radio affected voting behavior in affected regions.

A second source of political data used in this paper comes from the Database on Ideology, Money in Politics, and Elections (DIME), which is a database of campaign finance data from 1979-2024. The data contain over 850 million itemized political contributions made by individuals to local, state, and federal elections. In addition, the data contain common-space DIME scores (CFscores), which are a measure of ideological position estimated from the distribution of contributions made (when estimating for individuals) or received (when estimating for candidates). These data allow for understanding how within-party ideological positioning was affected by the rise of conservative talk radio (Bonica, 2014, 2024).

A final source of political data used in this paper comes from the American National Election Studies (ANES), which is a series of national surveys of voters in the United States, dating back to 1948 (ANES, 2023). The ANES are used to trace out descriptive statistics of the relationship between radio listenership and conservative beliefs over time.

Media Data

The main media dataset used in this paper comes from *American Radio*, a series of extensive radio ratings reports published between 1975 and 2004 by media professional James Duncan. The ratings information was collected and published using data from Arbitron, the main consumer research company in the United States that collected radio listener data, and features an extensive look at various features of all local radio markets surveyed by Arbitron.

A key variable in this analysis is information on the number of stations in a radio market that were talk radio stations at a given point of time, as well as the total share of listening in that market that was devoted to these talk radio stations. All reports are available in PDF form on `https://www.worldradiohistory.com`. An example of the data contained in these reports in shown in Appendix Figure A3, which shows a page from the radio market report in Spring 1995 for Bakersfield, California. The report indicates the most popular stations in the Bakersfield area, as well as information on average weekly listening hours as well as the AM/FM breakdown of listening shares. The bottom-left of the page shows format-specific shares, both in terms of overall listening and in terms of number of stations, data that is instrumental for following analyses. The main Arbitron data used in this paper comes from the volume one of Duncan's *An American Radio Trilogy*, which tracks radio market-level trends from 1975 to 2004 (Duncan, 1987, 1995, 2004). While these specific data have not been used in any prior academic work, other issues of *American Radio* have been used in a series of papers in the industrial organization literature from the late 1990s to early 2000s (see Berry and Waldfogel 1996, 1999, 2001).

A limitation of these data is that the radio markets defined and tracked by Arbitron, then collected and formatted into *American Radio*, were generally large markets. Markets were defined using a proprietary measure called an Arbitron Radio Metro (ARM), which was a distinct collection of counties that closely resembled a Metropolitan Statistical Area (MSA). In 1980, Arbitron tracked and recorded detailed data for 152 of these areas, which had a median of three counties per area.[7] Appendix Figure A4 shows these areas, with different colors representing different market sizes.[8] Because the mapping of Arbitron Radio Metros to counties is proprietary and not identical to a mapping between MSAs and counties, I construct a new crosswalk of Arbitron Radio Metros to counties using the map above. The

[7]Arbitron tracked additional areas shown on the map, but Duncan's American Radio only provided extensive data for "Large" Arbitron markets, of which there were 152 in 1980.

[8]The color ordering is as follows, from largest to smallest radio markets: red, purple, yellow, green, blue. Areas not tracked skew much more rural than the average area included in the analysis. In all main analyses of this paper, the results are restricted to a balanced panel of portions of the United States who lived in areas that were tracked by Arbitron.

Table 1: Pre-deregulation county characteristics, by presence in Arbitron data

	Counties included in Arbitron data	Counties not included in Arbitron data	All counties
Demographics (1980)			
Population (in thousands)	258.621	30.293	72.217
Household income (in thousands)	21.617	17.819	20.317
Non-HS-grad share of population	30.913	38.675	33.534
College-grad share of population	18.070	12.536	16.202
Unemployed share	0.064	0.072	0.067
Manufacturing share of employment	0.223	0.224	0.224
Agricultural share of employment	0.020	0.086	0.043
NAFTA vulnerability	0.020	0.030	0.023
Urban share of population	0.820	0.219	0.614
Rural share of population	0.133	0.512	0.263
Poverty rate	0.088	0.112	0.096
Share receiving SSI benefits	0.239	0.293	0.258
Median age	30.220	30.527	30.325
White share of population	0.807	0.879	0.832
Political preference (1972-1980)			
Republican presidential two-party vote share	54.786	57.381	55.677
Republican Senate two-party vote share	45.030	45.263	45.110
Republican House two-party vote share	43.433	43.443	43.437
Number of counties	576	2774	3350

Notes: This table shows 1980 county characteristics by presence in Arbitron data. Demographic characteristics come from the 1980 Decennial Census, and NAFTA vulnerability comes from Choi et al. (2024). Political preference variables are estimated using ICPSR voting data and Dave Leip's Atlas.

Arbitron markets studied in this paper comprise 576 distinct counties representing 66% of the United States population. A histogram showing the average number of counties included in each Arbitron Radio Metro is provided in Appendix Figure A5.

Characteristics of areas in the United States that are vs. are not represented in the Arbitron data are shown in Table 1. All county characteristics are computed using pre-period (i.e. before the Fairness Doctrine was repealed) data from 1980 (1972-1980 in the case of voting outcomes). As shown in the table, the areas with coverage from Arbitron tend to be much larger in population, wealthier, more educated, and substantially more urban. Given that later analyses of the effects of radio will be restricted to counties tracked by Arbitron, the selection into the sample is important to take note of. With the large percentage of the United States covered in the data, the comparison of counties in the

Arbitron data to averages among all counties in the United States yields smaller differences. Additionally, to the extent that some of the largest growth in Republican Party affiliation in the United States over the past four decades has occurred in rural areas and talk radio became prominent in these areas, sample selection toward urban America may understate the true effects of talk radio on conservative sentiment and other outcomes.[9]

Health Data

A first source of health-related data comes from the General Social Survey (GSS), a biannual survey which collects information about the beliefs, attitudes, and practices of residents of the United States (NORC, 2025). The GSS cross-sectional cumulative data is used to show descriptive patterns about the relationship between main sources of information about event in the news (newspapers, internet, TV, radio, or other) and beliefs about mental health treatment and confidence in medical institutions.

The final source of data used in this paper is restricted-use multiple cause of death data provided by the National Center for Health Statistics (NCHS, 2023). The restricted-use version of the NCHS data has detailed descriptions of the universe of deaths occurring in the United States, including detailed geographic, demographic, and cause-of-death information. Specifically, the county of residence and county of occurrence for each deceased person is recorded, as well as their cause of death (recorded by either ICD-8, ICD-9, or ICD-10 codes depending on the year) and up to 20 additional related factors contributing to the death. The health data used in this paper extends from 1969 to 2021, allowing for an extensive look at pre-period trends in mortality and health behavior between more- and less-exposed areas. A core aim of this project is to argue that large political shifts can have downstream implications that extend far beyond politics. The NCHS data is crucial for being able to show how sudden political divergences across the United States can also affect public health.

[9] See Edsall (2023) and Wyatt (2013) for information about rural moves toward conservatism and rural engagement with talk radio.

4 Empirical Strategy

The primary concern with a naive approach to this analysis—comparing the evolution of outcomes among counties in the United States where conservative talk radio did vs. did not become popular—is the endogeneity of growth of conservative talk radio. While the sudden repeal of the Fairness Doctrine, the instigating factor allowing rapid entry of a novel and politically-charged brand of radio content, provides exogenous timing variation in radio content, this shock affected the entire United States. Simply comparing areas that did vs. did not experience growth in its aftermath ignores the fact that many of these areas that saw the largest growth likely had latent political attitudes that made them receptive to this new kind of political content.

With this in mind, this study employs an instrumented difference-in-differences approach (DDIV), where the instrument is pre-period share of talk radio in a local market. While the repeal of the Fairness Doctrine functions as a national-level shock which generates temporal variation in treatment, the instrument generates additional cross-sectional variation that, combined with the repeal of the Fairness Doctrine, can be exploited for identification. The availability of multiple periods of pre-shock data weakens the typical independence assumption of IV designs into one of parallel trends, which is tested in all following results (de Chaisemartin, 2010; Hudson et al., 2017).[10] Methodologically, the paper's estimation strategy is similar to Bartik-style designs, which use pre-period market shares combined with national-level shocks to predict local-area growth.

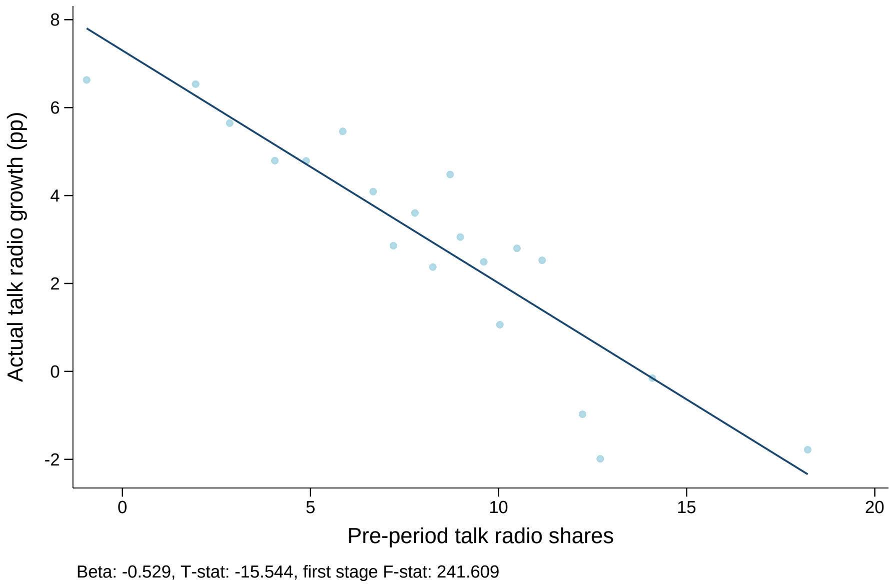
The first stage regression is:

$$TalkRadioGrowth_{m(c),1980-1995} = \delta + \mu\, TalkShare_{m(c),1980} + \sigma_{s(c)} + \xi \mathbf{X}_c + \nu_c \qquad (1)$$

Here, $TalkShare_{m(c),1980}$ refers the instrument, which is defined at the local Arbitron Radio Metro (ARM) level denoted by subscript m. $TalkRadioGrowth_{m(c),1980-1995}$ is the

[10]This relaxing of the conditional independence assumption is especially helpful in this setting, as researchers often think of industry shares as equilibrium objects, making exogeneity a difficult assumption to make.

Figure 4: First stage regression

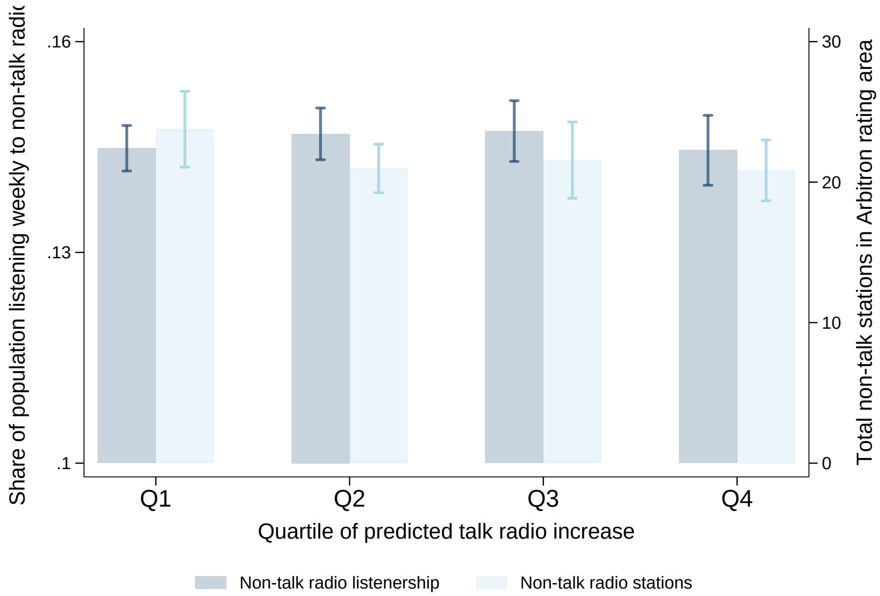


Notes: This figure shows the first stage relationship between the excluded instrument and actual talk radio growth between 1980 and 1995, conditional on state fixed effects and county-level controls for population density and rural share. The figure is a visual representation of the first stage regression in Equation 1.

change in local talk radio share over the 15-year period from 1980 to 1995. All shares and growth variables include talk radio on both the AM and FM bands. State fixed effects are denoted by $\sigma_{s(c)}$, and $\mathbf{X}_c$ contains 1980 county-level population density and rural share. ν_c is an idiosyncratic error term.

Figure 4 shows the results of the first stage regression, and a map of the United States displaying geographic variation in the first stage predicted values is shown in Appendix Figure A6. The relationship between the instrument and talk radio growth is strong and negative, with a first-stage F-statistic over 241. In other words, having less talk radio presence in 1980 is strongly predictive of large growth in talk radio in the following repeal of the Fairness Doctrine. Two main factors contribute to the negative relationship. First, areas where talk radio was well-established and prominent already meant new entrants faced much steeper competition when trying to enter these markets. Where radio markets were crowded and show lineups were well-known, there was less opportunity for new faces to emerge. Second, advances in satellite technology for program distribution in the early 1980s

Figure 5: Pre-period non-talk radio listenership and total non-talk stations



Notes: This figure shows: (1) in darker blue, the average share of people listening to non-talk radio weekly in 1987, and (2): in lighter blue, the average total number of non-talk radio stations available in each ARM in 1987. Both are split by quartile of predicted talk radio growth. Quartiles are constructed from the fitted values of talk-radio growth predicted only by the excluded instrument (Equation 1), after residualizing on all other first-stage controls. Higher quartiles imply larger predicted growth in talk radio market share.

had made national syndication of radio programming much easier and cheaper (Sterling and Kittross, 2001).[11] Before this technology was available, producing local talk radio was an expensive endeavor. Beyond needing an on-air host and producer, talk programs needed technical staff, someone to schedule and sequence segments, and additional personnel to screen incoming calls. Buying a syndicated show didn't necessarily fix these issues: because networks sent their programs over phone lines, they could only transmit one show at a time and buyers faced high line-rental fees. The arrival of satellite distribution changed this model by enabling networks to send multiple shows simultaneously at much lower costs, substantially easing the burden on affiliate stations (Rosenwald, 2015).

Because of the cost of production and difficulties with syndication, many small stations avoided producing their own talk shows, choosing instead to broadcast other, cheaper forms of content. The fact that these sometimes-prohibitive costs were specific to talk radio is

[11] This was spearheaded by the launch of Satellite Music Network in 1981, which was the first satellite delivered network to provide continuous music programming to stations.

Table 2: Pre-deregulation county characteristics, by quartile of predicted talk-radio change

	Quartile of instrument (Q4 = highest predicted growth)			
	1	2	3	4
Demographics (1980)				
Population (in thousands)	239.264	284.227	222.715	288.276
Household income (in thousands)	22.641	22.556	20.808	20.467
Non-HS-grad share of population	30.455	31.025	30.267	31.683
College-grad share of population	18.258	18.334	18.028	17.687
Unemployed share	0.060	0.065	0.061	0.068
Manufacturing share of employment	0.229	0.239	0.210	0.214
Agricultural share of employment	0.020	0.017	0.019	0.025
NAFTA vulnerability	0.022	0.018	0.019	0.021
Urban share of population	0.802	0.858	0.806	0.809
Rural share of population	0.140	0.105	0.144	0.144
Poverty rate	0.085	0.087	0.088	0.093
Share receiving SSI benefits	0.235	0.229	0.247	0.247
Median age	30.759	29.954	30.128	30.106
White share of population	0.791	0.799	0.816	0.820
Political preference (1972–1980)				
Republican presidential two-party vote share	55.032	54.481	56.050	53.944
Republican Senate two-party vote share	44.975	46.718	43.171	44.781
Republican House two-party vote share	43.386	42.333	47.583	41.483
Number of counties	144	144	144	144

Notes: 1980 county characteristics by quartile of predicted change in talk-radio market share (Equation 1). Quartiles are constructed from the fitted values of talk-radio growth predicted only by the excluded instrument, after residualizing on all other first-stage controls. Higher quartiles imply larger predicted growth.

important: Figure 5 shows that it's not the case that areas with little talk radio in the pre-period had little radio presence overall. In fact, the overall share of the population who was tuning into non-talk radio weekly, and the total number of non-talk stations broadcasting in the area, is very similar across markets. With the advent of new satellite technology, areas without prior talk radio could easily and cheaply broadcast syndicated content, making the cost of entry into new markets far cheaper than it was before. These two forces—lack of competition and cheap entry—allowed talk radio to flourish in new areas.

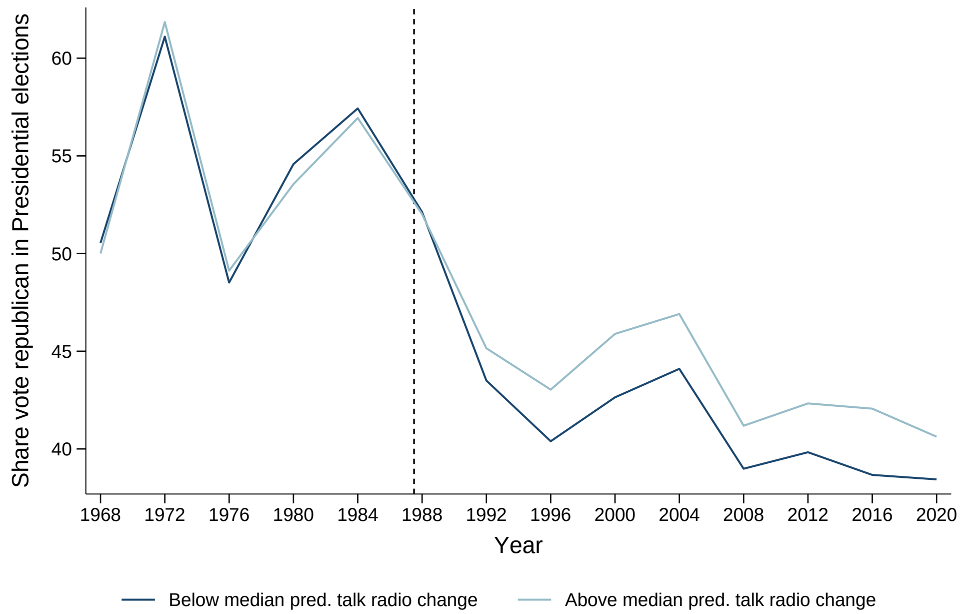
Table 2 provides more detail about the counties that were more or less exposed to the growth of talk radio. It provides demographic and political characteristics of these counties in the analysis sample during the pre-period separately by instrument quartile, estimated from residualized first stage values from Equation 1. Most demographic characteristics are estimated from 1980 data, while political preferences are estimated using averages from 1972-1980. As shown in the table, counties in the highest quartile of predicted talk radio growth do not differ sharply from those in the lowest quartile on most observable characteristics. Certain outcomes, such as population, high school graduation, and urban/rural share indicate a non-monotonic relationship across the instrument. The most exposed counties had slightly lower average household income and higher SSI recipiency than the least exposed counties, but differences overall across counties are generally very small. The overall similarity of these characteristics across instrument quartiles suggests that the instrument is unlikely to be confounded by other systematic factors, but later regression analyses will probe robustness to flexibly controlling for increasing numbers of these characteristics.

In the main following analyses, I use my measure of predicted talk radio growth from Equation 1 to estimate the following event study specification:

$$Y_{ct} = \alpha_c + \gamma_t + \sum_{\tilde{t} \neq t_0} \beta_t (\widehat{TalkRadioGrowth}_{m(c),1980-1995}) \times \mathbb{1}(t = \tilde{t}) + \lambda \mathbf{X}_{ct} + \theta_{s(c)t} + \varepsilon_{ct} \quad (2)$$

Where Y_{ct} is a given outcome in county c and year t, α_c are county fixed effects, γ_t are year fixed effects, and $\widehat{TalkRadioGrowth}_{m(c),1980-1995}$ is the predicted change in local area talk radio growth in radio metro l from 1980 to 1995 estimated from Equation 1, which is interacted with year fixed effects to trace out the dynamic evolution of talk radio's effects. $\mathbf{X}_{ct}$ are county-level controls that are interacted with year fixed effects to allow them to vary within areas over time, $\theta_{s(c)t}$ are state-by-year fixed effects, and ε_{ct} is an idiosyncratic error term. The omitted event, t_0, is defined to be the latest pre-period year available in the data, which will vary by outcome (e.g. presidential elections are once every four years). As opposed to the quartiles of predicted talk radio growth shown in Table 2, in the event study specification, predicted talk radio growth enters linearly, which allows for flexible covariate adjustments. The main specification includes flexible controls for rural share and population

Figure 6: Republican vote share in presidential elections, above/below median instrument



Notes: This figure shows raw trends in Republican vote share in presidential elections between counties with above- and below-median predicted changes in talk radio market share, from 1968 to 2022. Voting data comes from the ICPSR and Dave Leip's Atlas of elections. As before, quantiles come from the fitted-values of talk radio growth predicted only by the excluded instrument, residualizing on other first-stage controls.

density in $\mathbf{X}_{ct}$, with additional specifications including more controls.[12] Standard errors are clustered at the Arbitron Radio Metro level. All event-study and difference-in-differences regressions are estimated via two-stage least squares, where the first-stage and structural equations are fit jointly and the reported standard errors reflect first-stage estimation error.

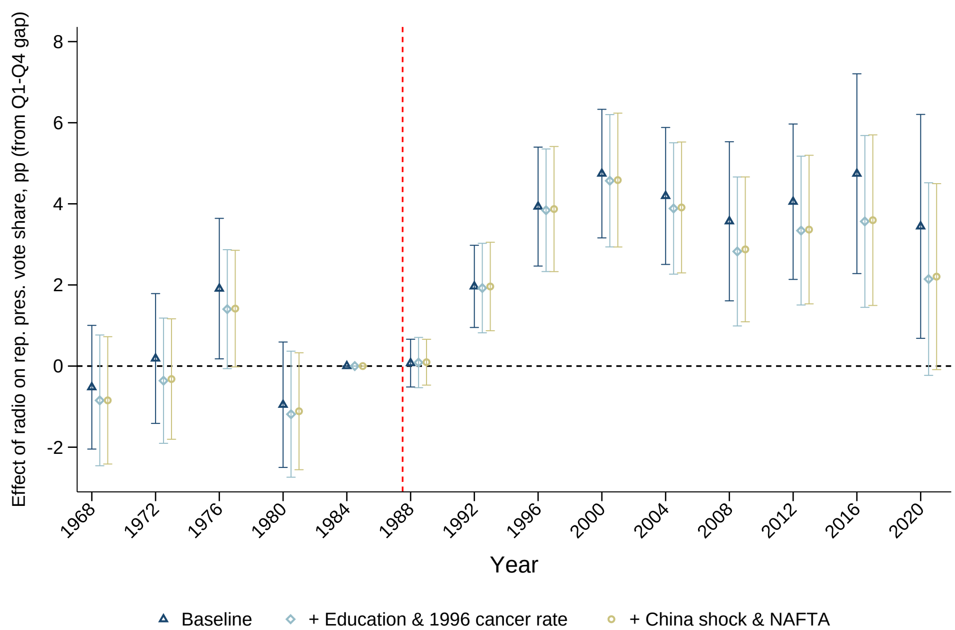
5 Results

Political results

Figure 6 shows raw trends that motivate the various event study results that will be described in this section. It plots the average Republican two-party vote share in presidential elections for counties above and below the median predicted change in talk radio (from Equation 1)

[12]The covariates included in $\mathbf{X}_{ct}$ are consistent across first- and second-stage regressions. Robustness checks which add additional controls also involve re-estimating Equation 1 using these same controls.

Figure 7: Republican vote share in presidential elections as a function of predicted talk radio increase



Notes: This figure shows the event study coefficients and 95% confidence intervals from estimation of Equation 2, where Y_{ct} is Republican two-party vote share in presidential elections. Three estimates are shown in different colors, starting with the baseline model (dark blue triangles). This specification estimates Equation 2 with state-by-year fixed effects, with $\mathbf{X}_{ct}$ containing interactions between 1980 rural share and year fixed effects and 1980 population density and year fixed effects; specification (2), shown in light blue diamonds, additionally controls for 1980 percent college educated interacted with year fixed effects and 1996 cancer rate interacted with year fixed effects; specification (3), shown in light brown circles, replicates specification (2) but with added controls for NAFTA exposure from Choi et al. (2024) interacted with year fixed effects and China shock exposure from Autor et al. (2019) interacted with year fixed effects. All specifications are weighted by 1980 county population, and standard errors are clustered at the Arbitron Radio Metro level.

from 1968 to 2022. As shown in the figure, areas above and below the median voted similarly in presidential elections both in trends and levels before the Fairness Doctrine was repealed in 1987, indicating that the instrument is not being driven by unobserved differences in political preferences. As soon as 1992, a gap emerges between these areas, which widens over the following decades and remains large as recently as the 2020 election.

While the raw data plot has the benefits of transparency and interpretability, more parametric event study figures can more easily show robustness to controls and specification choices. In Figure 7, I show the event study results for the effects of talk radio on two-party Republican vote share in presidential elections. The first series in Figure 7 plots the β_t

estimates from the baseline version of Equation 2 where I control for county, year, state-by-year fixed effects, and time-varying controls for county-level rural share and population density, estimated by interacting 1980 values for these variables with year fixed effects. As noted by Choi et al. (2021), the 1990s was an active moment for state policy experimentation. State-by-year fixed effects help capture the effects of these reforms or other unobserved state-level changes over time, so the remaining variation used to identify the effects of talk radio captures within-state differences across radio markets over time not driven by overall state trends.[13] The coefficient values in the period before the repeal of the Fairness Doctrine indicate no clear pre-trend, and become positive and significant starting with the 1992 presidential election and continue to be positive and significant for each election after.

In the second specification, I add in time-varying controls for 1980 county percent with a college education—as voters without a college education have increasingly turned toward the Republican Party in recent decades (Cohn, 2021)—and 1996 county-level cancer rate, which is used in Arteaga and Barone (2022) to predict exposure to opioid marketing and led to increased Republican vote shares. In the third series, I adapt the second specification to additionally include time-varying controls for NAFTA exposure, another policy change from the 1990s that led to affected voters turning away from the Democratic Party, as well as China shock exposure from Autor et al. (2013). Adding these controls has a mostly negligible impact on point estimates, but does depress them slightly: all post-period coefficients starting in 1992 stay statistically significant except for the 2020 election, which becomes marginally insignificant. I also re-estimate the event study computing confidence intervals from a block cluster bootstrap procedure, and show these results in Appendix Figure A7. The results are similar to the standard estimates: no pre-period coefficients are significant, and all coefficients from 1992 through 2016 are significant. Additionally, Appendix Figure A8 shows that the effect sizes seem to vary linearly with the instrument, indicating that results are not being driven by outliers. Results are also robust to the year in which the talk radio shares

[13]Choi et al. (2021) list the AFDC welfare waivers before the 1996 federal welfare reform act, Medicaid expansions, and EITC introductions and expansions as examples of some of the policies adopted on a state-by-state basis during the 1990s.

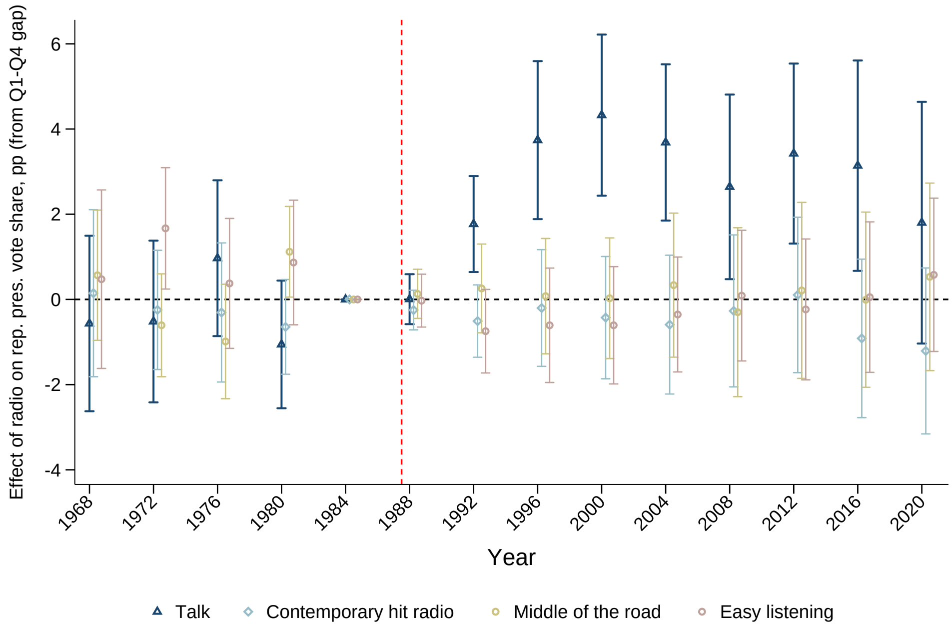
used to instrument for growth are estimated. When instead using more lagged shares from 1977 to instrument for growth from 1980 to 1995, results do not change. The estimated first stage and presidential event study results when using 1977 shares to instrument for growth are shown in Appendix Figure A9.

As a final check of sensitivity to inclusion of controls, I also employ a post-LASSO procedure for feature selection. To implement this, I run a LASSO regression of the predicted first stage values on all pre-period county characteristics available in the data, and use the features selected by the LASSO regression as controls in the event study. After inclusion of these additional variables, coefficient estimates remain very close to what is shown in the third series, indicating that it is unlikely results are being driven by unobservables. The post-LASSO event study and the full list of LASSO-selected controls are shown in Appendix Figure A10.

Because the measure of predicted talk radio growth enters the event-study regression linearly, it can be difficult to interpret the size of the coefficient estimates. To obtain an interpretable effect size I rescale the vertical axis by a measure of dispersion in the residualised instrument: specifically, the difference between the mean of the highest quartile (Q4) and the mean of the lowest quartile (Q1) of the first-stage fitted values, where those fitted values are generated only from the excluded instrument after partialing out all other first-stage controls. Multiplying each coefficient by this Q4-Q1 spread shows the effect of moving from a county in the bottom 25 percent of predicted talk-radio growth to one in the top 25 percent. With this scaling, the event study implies that talk-radio growth raised the Republican presidential two-party vote share by about 4 percentage points, an effect that is both statistically significant and substantively large.

To further probe robustness of the result, I re-estimate the event study using a series of placebo instruments. Here, I replicate the instrumental variables strategy, but replace the instrument with measures of predicted growth of various popular non-talk radio formats. I use three of the most popular radio formats in 1980, which were contemporary hit radio (CHR), middle-of-the-road radio (MOR), and easy listening. I then plot estimated event

Figure 8: Republican presidential vote share with placebo instruments

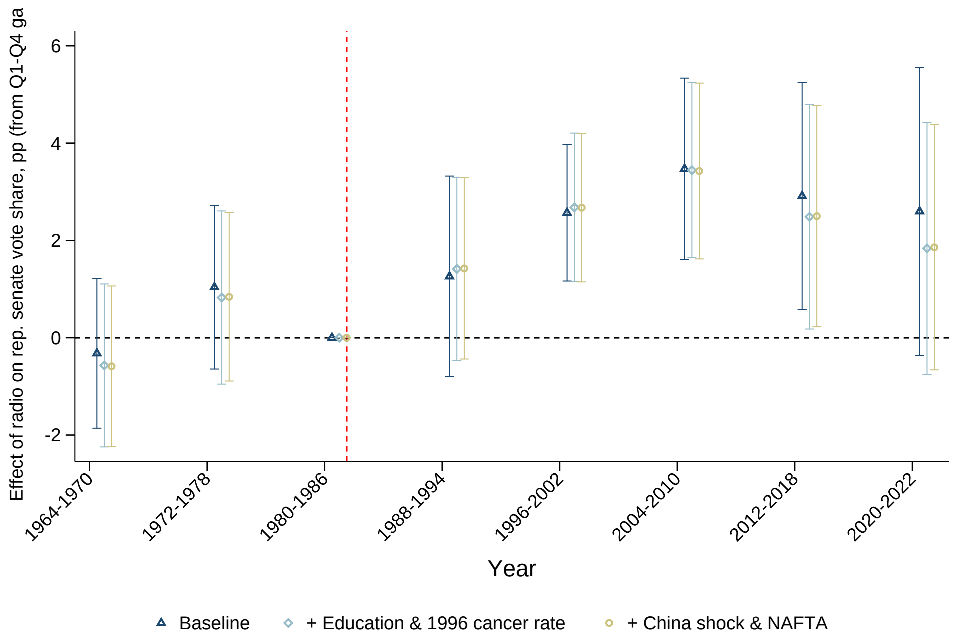


Notes: This figure plots event study coefficients and their associated 95% confidence intervals for estimation of Equation 2 for Republican two-party vote shares. Four series are shown, with the first series (dark blue triangles) showing the event study coefficients from the main analysis, and the other three series showing event study coefficients from estimation of Equation 2 using placebo instruments. The placebo instruments are predicted growth of three of the most popular radio formats in 1980: contemporary hit radio (CHR), middle-of-the-road radio (MOR), and easy listening. All specifications are weighted by 1980 county population, and standard errors are clustered at the Arbitron Radio Metro level. The controls used in each specification are the same as used in specification (3) of Figure 7, with additional controls for the pre-period shares of other radio formats interacted with year fixed effects to avoid mechanical correlations between radio shares.

study coefficients from instrumenting for growth in these formats alongside the event study coefficients from the main analysis. The results are shown in Figure 8. As shown in the figure, the coefficients from the placebo instruments are all economically and statistically negligible. This suggests that the results are not being driven by other unobserved factors that may have been correlated with radio growth, but rather by the repeal of the Fairness Doctrine and subsequent growth of talk radio.

Figure 9 shows that the large effects of talk radio on conservative voting were not limited to presidential elections. This figure shows the coefficient estimates when looking to Senate elections, with analogous specifications to Figure 7. Because Senate terms are for six years, election results are aggregated into six-year intervals. Such aggregation guarantees that each

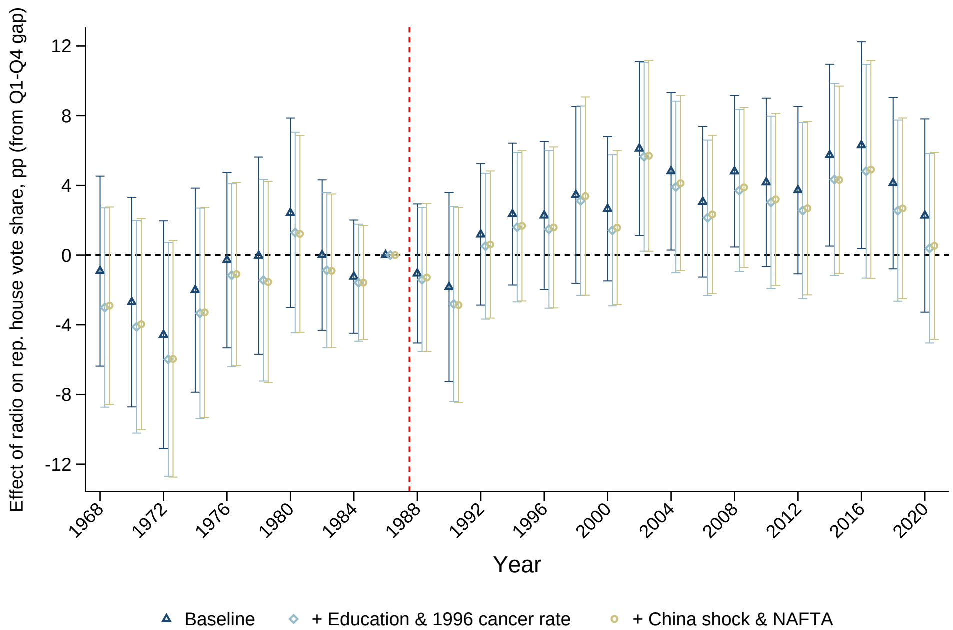
Figure 9: Republican vote share in Senate elections as a function of predicted talk radio increase



Notes: This figure is similar to Figure 7, replacing the outcome variable as Republican two-party vote share in Senate elections. Since Senate terms are for six years, the Senate election data are aggregated to 6-year intervals to ensure that the event study coefficients are each identified using at least one observation from each county in the sample. Three estimates are shown in different colors, starting with the baseline model (dark blue triangles). This specification estimates Equation 2 with state-by-year fixed effects, with $\mathbf{X}_{ct}$ containing interactions between 1980 rural share and year fixed effects and 1980 population density and year fixed effects; specification (2), shown in light blue diamonds, additionally controls for 1980 percent college educated interacted with year fixed effects and 1996 cancer rate interacted with year fixed effects; specification (3), shown in light brown circles, replicates specification (2) but with added controls for NAFTA exposure from Choi et al. (2024) interacted with year fixed effects and China shock exposure from Autor et al. (2019) interacted with year fixed effects. All specifications are weighted by 1980 county population, and standard errors are clustered at the Arbitron Radio Metro level.

event study coefficient is identified using the full panel of counties, as all counties must undergo at least one Senate election within a six-year period. The event study results are very comparable to the results from Figure 7. Here, the baseline specification indicates there are no noticeable pre-period differential trends, and the estimates are significant for all Senate elections between 1996 and 2018, becoming marginally insignificant for the 2020 and 2022 elections, with effect sizes slightly smaller but similar to those found in the analysis of presidential voting patterns. As before, additional controls beyond those included in the preferred specification have little effect on the estimates, but do depress them slightly in more recent years.

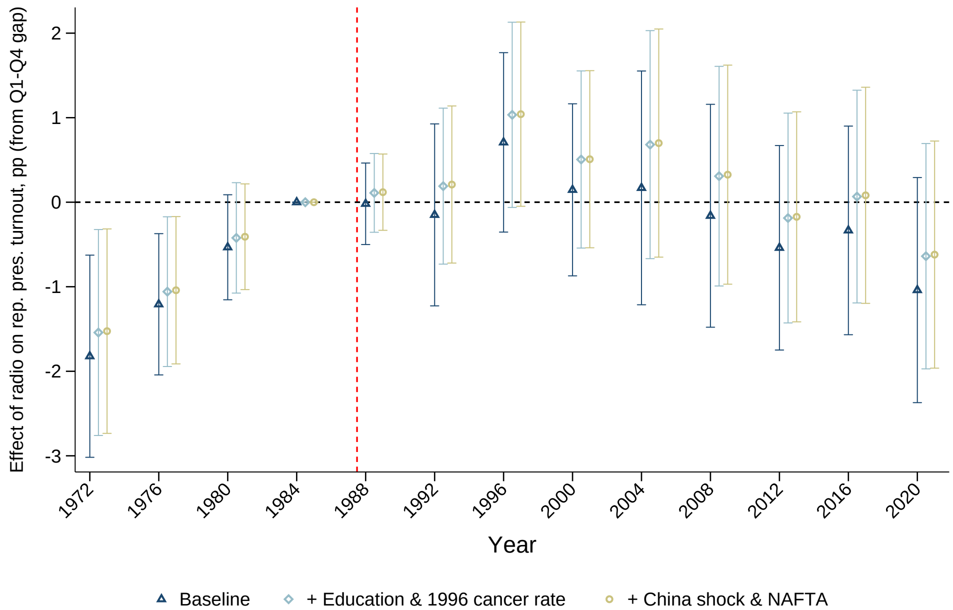
Figure 10: Republican vote share in House elections as a function of predicted talk radio increase



Notes: This figure replicates the previous event studies, using Republican two-party vote share in House elections as the outcome variable. Three estimates are shown in different colors, starting with the baseline model (dark blue triangles). This specification estimates Equation 2 with state-by-year fixed effects, with $\mathbf{X}_{ct}$ containing interactions between 1980 rural share and year fixed effects and 1980 population density and year fixed effects; specification (2), shown in light blue diamonds, additionally controls for 1980 percent college educated interacted with year fixed effects and 1996 cancer rate interacted with year fixed effects; specification (3), shown in light brown circles, replicates specification (2) but with added controls for NAFTA exposure from Choi et al. (2024) interacted with year fixed effects and China shock exposure from Autor et al. (2019) interacted with year fixed effects. All specifications are weighted by 1980 county population, and standard errors are clustered at the Arbitron Radio Metro level.

I replicate these analyses for House elections, shown in Figure 10. Because House terms are only two years, I estimate the event study coefficients here biannually from 1968-2020. The results are similar in magnitude and timing to the results from presidential and Senate elections—indicating a shift to the right—but with substantially larger standard errors. Across all specifications, the only statistically significant event study coefficient is for the 2002 election cycle. However, when running a differences-in-differences specification—essentially aggregating the event study coefficients—there is a significant and positive effect of talk radio on Republican vote share in House elections, comparable to those found in presidential and Senate elections. These results are included in Table 3. Overall, these analyses show that talk radio had a large and lasting influence on voting behavior in the United States that

Figure 11: Turnout in presidential elections as a function of predicted talk radio increase



Notes: This figure shows event study coefficients and 95% confidence intervals for estimation of equation (3) with turnout in presidential elections as the outcome variable, measured by total presidential votes cast divided by population 18 years or older. Three estimates are shown in different colors, starting with the baseline model (dark blue triangles). This specification estimates Equation 2 with state-by-year fixed effects, with $\mathbf{X}_{ct}$ containing interactions between 1980 rural share and year fixed effects and 1980 population density and year fixed effects; specification (2), shown in light blue diamonds, additionally controls for 1980 percent college educated interacted with year fixed effects and 1996 cancer rate interacted with year fixed effects; specification (3), shown in light brown circles, replicates specification (2) but with added controls for NAFTA exposure from Choi et al. (2024) interacted with year fixed effects and China shock exposure from Autor et al. (2019) interacted with year fixed effects. All specifications are weighted by 1980 county population, and standard errors are clustered at the Arbitron Radio Metro level.

continues into the current moment.

While the prior results show a clear shift toward voting for Republican candidates, it is not immediately obvious whether the shift was due to changes in political preferences (a persuasion channel) or increased turnout for existing Republican voters (a voter mobilization channel). To better understand these competing explanations, I study the effect of talk radio growth on overall voter turnout.

In Figure 11, I show the event study results for overall turnout in presidential elections. The results show no clear effect on overall voter turnout across all specifications, indicating that the persuasion channel seems to dominate in explaining political results. The results for turnout in Senate and House elections are shown in Appendix Figure A11, and are very similar to the results for presidential elections, indicating no clear effects on voter mobilization overall.

To show specific point estimates and place these various results alongside one another, I also estimate a difference-in-differences specification, given by Equation 3 below. The difference-in-differences specification is nearly identical to its event study counterpart in Equation 2, but here only one difference-in-differences coefficient is estimated.

$$Y_{ct} = \alpha_c + \gamma_t + \beta \, Talk\widehat{Radio}Growth_{m(c),1980-1995} \times \mathbf{1}[t > 1987] + \lambda \mathbf{X}_{ct} + \theta_{s(c)t} + \varepsilon_{ct} \tag{3}$$

The results from the difference-in-differences specification are shown in Table 3. The first three columns show the results for Republican two-party vote share in presidential, Senate, and House elections, while the last three columns show the results for election turnout in presidential, Senate, and House elections. Four specifications are shown with increasing numbers of controls, and additional specifications are given in Table A1. The difference-in-differences coefficients are the first value for each column/specification. Standard errors are in parentheses, and p-values are in brackets. The difference-in-differences estimate multiplied by the Q4-Q1 spread of the instrument (as in the event studies) are indicated by the fourth value (denoted with a $\sim$) for each column/specification. Overall, there are effects on two-party Republican vote share that range from 3.1 to 5.8 percentage points for presidential elections, 2.1 to 4.8 percentage points for Senate elections, and 4.1 to 7.5 percentage points for House elections, all of which are robust to the inclusion of additional controls. The turnout effects for all election types are both smaller in magnitude and mostly statistically insignificant. I also re-estimate the difference-in-differences results with varying controls using a block cluster bootstrap approach and show these results in Table A2. Results align very closely with the standard specifications, showing robust estimates for voting outcomes and statistically insignificant estimates for turnout.

While the results indicate effects of talk radio on changing the ideological preferences of the voting population, there is also evidence that the candidates themselves were changing.

Table 3: Difference-in-Differences Estimates for Political Outcomes

	Two-party Rep. vote share			Election turnout		
	President	Senate	House	President	Senate	House
County, Year,	1.039	0.854	1.336	0.295	0.251	0.228
State x Year FEs	(0.188)	(0.179)	(0.313)	(0.085)	(0.096)	(0.109)
	[0.000]	[0.000]	[0.000]	[0.001]	[0.009]	[0.038]
	∼5.798	∼4.765	∼7.460	∼1.645	∼1.403	∼1.272
+ Pop. density	0.922	0.737	1.209	0.180	0.129	0.127
	(0.197)	(0.163)	(0.316)	(0.118)	(0.134)	(0.142)
	[0.000]	[0.000]	[0.000]	[0.129]	[0.335]	[0.374]
	∼5.011	∼4.002	∼6.571	∼0.981	∼0.703	∼0.689
+ Pct. rural	0.569	0.400	0.760	0.131	0.095	0.086
	(0.131)	(0.126)	(0.244)	(0.102)	(0.121)	(0.130)
	[0.000]	[0.002]	[0.002]	[0.202]	[0.431]	[0.509]
	∼3.291	∼2.315	∼4.393	∼0.755	∼0.551	∼0.496
+ Pct. college	0.533	0.371	0.716	0.155	0.148	0.133
	(0.126)	(0.109)	(0.239)	(0.100)	(0.111)	(0.121)
	[0.000]	[0.001]	[0.003]	[0.124]	[0.186]	[0.274]
	∼3.104	∼2.162	∼4.173	∼0.903	∼0.860	∼0.777
Observations	7772	3899	14629	7008	3620	13773
Dep. var. mean	54.364	45.677	43.869	54.756	45.123	43.176
R-squared	0.895	0.909	0.694	0.937	0.934	0.883

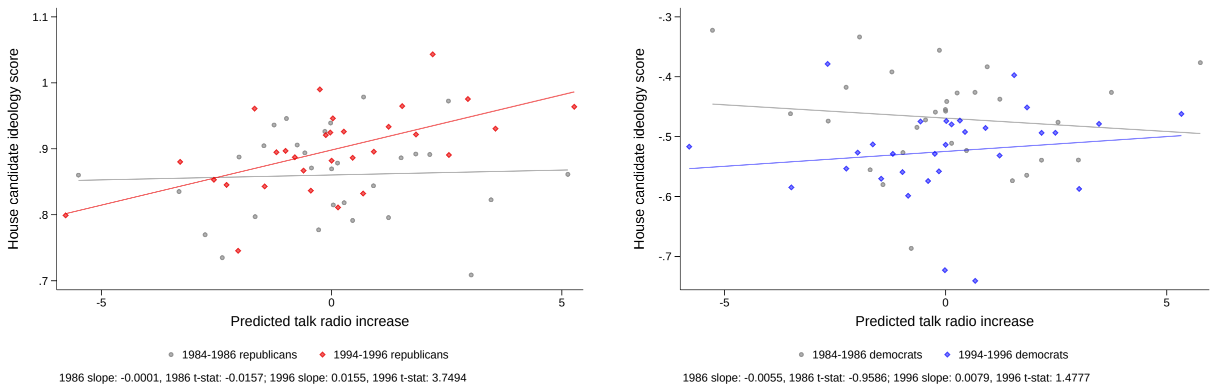
Notes: This table shows difference-in-differences estimates for the effects of talk radio on political outcomes, estimated from Equation 3. The first three columns show estimates for the effects of talk radio on Republican two-party vote share in presidential, Senate, and House elections, while the last three columns show estimates for the effects of talk radio on election turnout in presidential, Senate, and House elections. The difference-in-difference coefficients in the first value for each column/specification. Standard errors are in parentheses, and p-values are in brackets. The difference-in-differences estimate multiplied by the Q4-Q1 spread of the instrument (as in the event studies) are indicated by a tilde (∼). The baseline specification includes state-by-year fixed effects only. Other specifications progressively add 1980 values for the variable described interacted with year fixed effects as controls. Each additional specification is additive, and includes the prior specification's control variables. Additional (additive) specifications for this table are given in A1. The dependent variable means are calculated using the outcome for all pre-periods. The R-squared value refers to the value from the baseline specification. All specifications are weighted by 1980 county population, and standard errors are clustered at the Arbitron Radio Metro level.

Figure 12 shows the results of an analysis of candidate polarization, which is a measure of how extreme candidates are in their ideological positions, developed by Bonica (2014). I show within-party changes in ideology among candidates for the House of Representatives from 1986 to 1996, estimated from DIME contribution data. Figure 12a shows estimates of ideological change for Republican Party candidates, while Figure 12b shows estimates for Democratic Party candidates. The y-axis variable is the average change in CFscores for candidates from a given area. CFscores greater than 0 indicate Republican ideologies, while scores below 0 indicate Democratic ideologies. In this sense, an increasing CFscore indicates shifts towards conservatism over time (or toward greater conservatism if already conservative), while decreasing CFscores indicate the analogous effects for Democratic ideology. Here, in the pre-period, Republican candidates from areas with high vs. low predicted growth in talk radio had similar ideological positions, but Republican Party candidates from areas with higher predicted talk radio growth grew more conservative in the decade following repeal of the Fairness Doctrine (t-stat = 3.75). This effect is localized within Republican Party candidates: there is no appreciable difference in the ideological positions of Democratic Party candidates from areas with high vs. low predicted talk radio growth (t-stat = 1.48). This suggests that the effects of talk radio on political preferences were not limited to voters, but also extended to candidates themselves.

Health results

In this section, I document how exposure to talk radio and its resulting effects on political preferences led to notable increases in "deaths of despair"—mortality attributed to substance use (including alcohol and opioids) and suicide. This terminology, popularized by Case and Deaton (2015, 2021), has come to signify a set of behaviors and mortality risks linked to deteriorating economic and social conditions, particularly in working-class communities. While the original deaths of despair literature emphasized adverse economic trends, the results below suggest that changes in political messaging and media environments can be another powerful contributor to risky behaviors and poor health outcomes. Notably, the

Figure 12: House candidate changes in ideological polarization



(a) Republican candidates

(b) Democrat candidates

Notes: These figures show within-party changes in ideology among candidates for the House of Representatives from 1986 to 1996, estimated from DIME contribution data. Figure 12a shows estimates of ideological change for Republican Party candidates, while Figure 12b shows estimates for Democratic Party candidates. Geography is inferred from the zip code from which the candidate received the most individual contributions. The y-axis variable is the average change in CFscores for candidates from a given area. CFscores greater than 0 indicate republican ideologies, which scores below 0 indicate democratic ideologies. CFscores are estimated from campaign contributions as described in Bonica (2014). The x-axis variable is the predicted change in talk radio market share, estimated from the residualized excluded instrument in Equation 2.

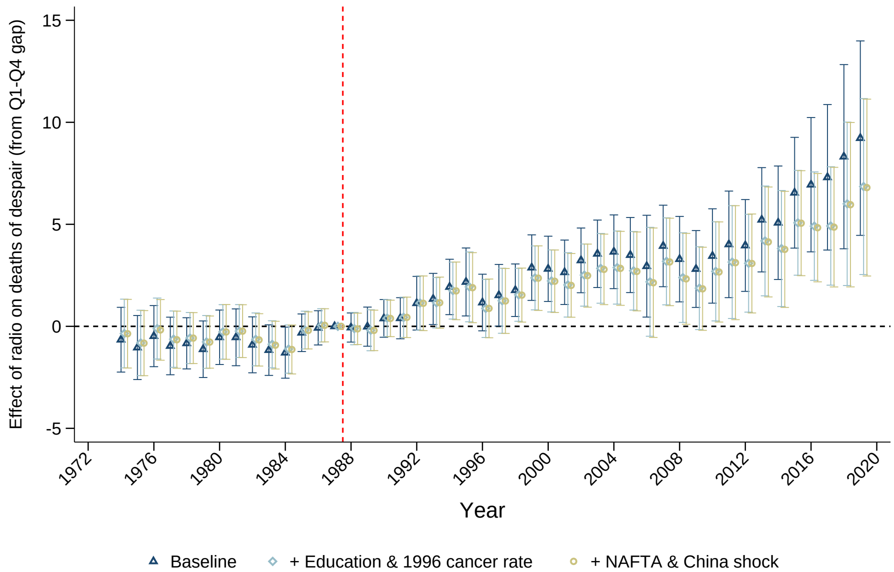
CDC itself recognizes "access to mass media" as one of its 12 social determinants of health (CDC, 2025), highlighting how information channels can shape broader health disparities. To shed light on potential mechanisms, I later present descriptive evidence from the GSS showing that radio listeners hold markedly different attitudes toward mental health care than audiences of other media.

A broad observational literature has linked increasing political polarization to health disparities, including deaths of despair. Oberlander (2024) notes that individual health outcomes are segregated along ideological lines, which they attribute to a variety of factors, including individual health behaviors, health provider ideological sorting, and health policy implementation. Specifically concerning deaths of despair, Warraich et al. (2022) find that the gap in overall mortality rates between counties won by Democrats versus Republicans widened substantially between 2001 and 2019, and they highlight that cause-specific mortality related to suicide and substance use has grown disproportionately in Republican-leaning areas. Similarly, Callaghan et al. (2024) find that Republicans are less likely than Democrats to use the new national suicide prevention hotline.

These observational studies underscore that broader politically-oriented cultural contexts can relate to individuals' health and well-being. However, they are not straightforward evidence of causality: political affiliation may reflect underlying socioeconomic conditions (job loss, poverty, insufficient healthcare access, and others) that also lead to higher mortality. A more recent causal literature has explored how media can have effects on outcomes beyond political ideology: Ang (2023) show that exposure to the 1915 film *The Birth of a Nation* had lasting impacts on local support for the KKK, and finds higher rates of hate crimes a century later, while Grosfeld et al. (2024) show that media access can substantially affect religiosity. Relatedly, recent findings around COVID-19 revealed that partisan environments can alter behavior, such as vaccine uptake and social distancing, thereby affecting mortality (Allcott et al., 2020; Wallace et al., 2023). This analysis provides new causal evidence in this vein, focusing on the sudden rise of conservative talk radio as an exogenous shift in local political messaging which led to lasting changes in political preferences.

Figure 13 plots event-study estimates for the principal measure of deaths of despair. Following Case and Deaton (2015), I categorize any given death as a death of despair if it involves alcohol, chronic liver disease, an overdose, or suicide. The outcome is age-adjusted rates of deaths of despair, where the rate is the number of occurrences per 100,000 people. As before, the effects are pre-multiplied by the difference in means of the residualized instrument between the highest and lowest quartile counties to be interpretable, so each coefficient in the figure represents the effect of moving from the bottom 25 percent to the top 25 percent of predicted talk radio growth, interacted with year indicators. Prior to the repeal of the Fairness Doctrine in 1987, there are no divergences, indicating no meaningful differences in mortality trends between areas more or less exposed to conservative talk radio. After deregulation, however, the two series begin to diverge, and by the mid-1990s, counties with higher predicted talk radio growth experience notably higher rates of deaths of despair. Over the sample period, these increases amount to 5 to 10 additional deaths of despair per 100,000

Figure 13: Event-study estimates of talk radio and deaths of despair

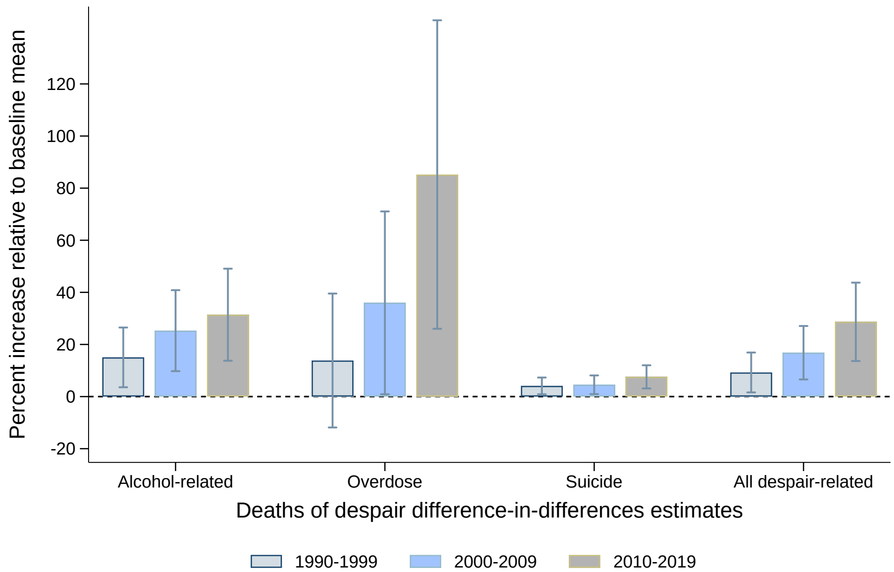


Notes: This figure shows the event study coefficients and 95% confidence intervals from estimation of Equation 2, where Y_{ct} is age-adjusted annual rate of deaths of despair (per 100,000). A death is categorized as a death of despair if it involved alcohol, chronic liver disease, an overdose, or suicide, following Case and Deaton (2015). Three estimates are shown in different colors, starting with the baseline model (dark blue triangles). This specification estimates Equation 2 with state-by-year fixed effects, with $\mathbf{X}_{ct}$ containing interactions between 1980 rural share and year fixed effects and 1980 population density and year fixed effects; specification (2), shown in light blue diamonds, additionally controls for 1980 percent college educated interacted with year fixed effects and 1996 cancer rate interacted with year fixed effects; specification (3), shown in light brown circles, replicates specification (2) but with added controls for NAFTA exposure from Choi et al. (2024) interacted with year fixed effects and China shock exposure from Autor et al. (2019) interacted with year fixed effects. All specifications are weighted by 1980 county population, and standard errors are clustered at the Arbitron Radio Metro level.

people relative to pre-repeal levels in more exposed counties, corresponding to a greater than 20% increase relative to baseline.

Although Figure 13 points to a pronounced post-1987 increase in overall deaths of despair, it does not distinguish among specific causes. In Figure 14, I disaggregate the results by category—overdose, alcohol-related (including chronic liver disease), and suicide—and present difference-in-differences estimates for three sub-periods (1990–1999, 2000–2009, and 2010–2019). Results are presented as percentage changes relative to baseline means. All despair-related deaths first appear in the 1990s and grow larger over time. By the 2010s, overdose mortality emerges as an increasingly important contributor to overall despair-related deaths. While these overdose deaths also have roots in changes to pharmaceutical access and

Figure 14: Difference-in-differences estimates by category of deaths of despair



Notes: This figure shows a decomposition of the deaths of despair result from Figure 13. Difference-in-differences estimates are shown for alcohol-related mortality, overdose mortality, and suicide mortality—which collectively comprise all deaths of despair—alongside overall despair related deaths. As in figure 13, outcomes are defined as age-adjusted annual rates. The differences-in-differences regression in Equation 3 is estimated for three different time periods (compared to the pre-period): 1990-1999, 2000-2009, and 2010-2019. The difference-in-differences regression includes as controls all control variables used in specification (1) of Figure 13. Outcomes on the y-axis are estimated as percent changes relative to their pre-period means. All specifications are weighted by 1980 county population, and standard errors are clustered at the Arbitron Radio Metro level. Event study versions of these outcomes (further disaggregating alcohol-related mortality to separate chronic liver disease) are provided in Appendix Figure A12.

marketing (e.g. Alpert et al. 2022; Arteaga and Barone 2022), the data here suggest that broader cultural and political mechanisms also contribute. Finally, suicide rates also exhibit a modest but persistent rise in more exposed counties, especially in the 2000s onward. Event study versions of each of these outcomes are provided in Appendix Figure A12.[14]

I aggregate these overall estimates into a standard difference-in-differences specification estimated in Equation 3 and show the results in Table 4. As with political outcomes, I also include additional controls in the differences-in-differences specification in Table A3, and show bootstrapped estimates for the deaths of despair measure in Table A4. In line with the event studies in Figure A12, Table 4 shows that the majority of despair-related deaths are

[14]The event studies in the Appendix further disaggregate alcohol-related mortality by excluding chronic liver disease and plotting it separately to show varying time paths of effects.

Table 4: Difference-in-Differences Estimates for Health-Related Outcomes

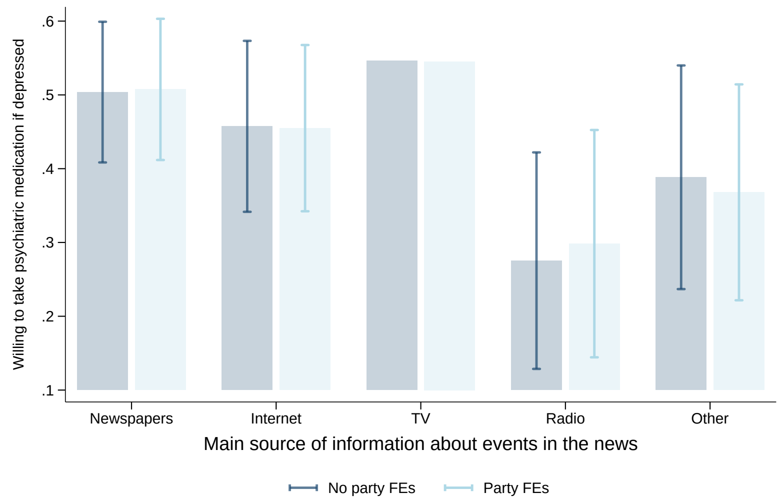
	Health-related Outcomes			
	Alcohol-related	Overdose	Suicide	All despair
County, Year,	0.399	0.126	0.218	0.706
State x Year FEs	(0.115)	(0.138)	(0.049)	(0.241)
	[0.001]	[0.365]	[0.000]	[0.004]
	$\sim$ 2.226	$\sim$ 0.701	$\sim$ 1.215	$\sim$ 3.940
+ Pop. density	0.360	0.295	0.140	0.771
	(0.110)	(0.129)	(0.040)	(0.223)
	[0.001]	[0.023]	[0.001]	[0.001]
	$\sim$ 1.957	$\sim$ 1.603	$\sim$ 0.760	$\sim$ 4.189
+ Pct. rural	0.327	0.282	0.112	0.698
	(0.103)	(0.124)	(0.038)	(0.211)
	[0.002]	[0.024]	[0.004]	[0.001]
	$\sim$ 1.889	$\sim$ 1.631	$\sim$ 0.645	$\sim$ 4.036
+ Pct. college	0.293	0.205	0.081	0.558
	(0.107)	(0.122)	(0.034)	(0.217)
	[0.007]	[0.095]	[0.018]	[0.011]
	$\sim$ 1.707	$\sim$ 1.196	$\sim$ 0.469	$\sim$ 3.249
Observations	25662	25662	25662	25662
Dep. var. mean	8.516	2.974	12.642	23.300
R-squared	0.781	0.846	0.643	0.852

Notes: This table shows difference-in-differences estimates for the effects of talk radio on deaths-of-despair outcomes, estimated from Equation 3. The first three columns show estimates for constituent parts of deaths of despair, while the fourth column shows estimates for their aggregation. The difference-in-difference coefficients in the first value for each column/specification. Standard errors are in parentheses, and p-values are in brackets. The difference-in-differences estimate multiplied by the Q4-Q1 spread of the instrument (as in the event studies) are indicated by a tilde ($\sim$). The baseline specification includes state-by-year fixed effects only. Other specifications progressively add 1980 values for the variable described interacted with year fixed effects as controls. Each additional specification is additive, and includes the prior specification's control variables. Additional (additive) specifications for this table are given in A1. The dependent variable means are calculated using the outcome for all pre-periods. The R-squared value refers to the value from the baseline specification. All specifications are weighted by 1980 county population, and standard errors are clustered at the Arbitron Radio Metro level.

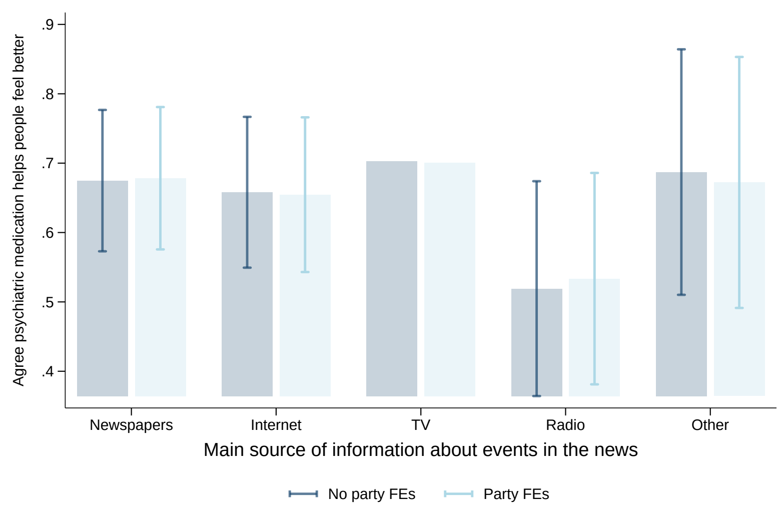
driven by alcohol-related mortality and overdose mortality, with increases in suicide being a statistically significant but small share of overall despair-related mortality increases.

Understanding why exposure to conservative talk radio might lead to higher rates of deaths of despair is a complex question. One possibility relates directly to the content of talk radio shows, which may downplay certain health risks or foster distrust in government

Figure 15: Willingness to use and beliefs about psychiatric medication



(a) Willingness to use medication if depressed



(b) Believe psychiatric medication improves peoples' lives

Notes: This figure shows descriptive patterns for the relationship between primary media sources and willingness to use / beliefs about the efficacy of psychiatric medication. Each outcome variable is a binary indicator for whether the respondent agreed with the given statement or question. The independent variable refers to main source of information the respondent uses for information about events in the news. The coefficients are estimated by regressing each outcome on main media source fixed effects, using television as the omitted category. Each regression includes fixed effects for year, 10-year age bins, sex, race, education level (aggregated into 8 categories), and region (9 categories). The specification shown in light blue also includes fixed effects for party identification, which has 8 categories (from strong Democrat to strong Republican, including other Party), while the specification in dark blue omits this control. Both variables are estimated in 2006—the only year of overlap with the main media source variable. Standard errors are clustered at the primary sampling unit level.

interventions, thereby altering health behaviors (Allcott et al., 2020). Alternatively, the influence of talk radio on political preferences could be the deeper driver: as individuals become more conservative in their outlook or voting patterns, they may become receptive to different public-health messages, or encounter policy environments less inclined toward robust social and mental-health supports. Another pathway is that talk radio often highlights economic grievances or cultural anxieties, and when these remain unresolved, individuals may turn to harmful coping behaviors—including substance use, alcohol abuse, or self-harm. Further research might disentangle whether it is the direct, day-to-day messaging of conservative radio programs or the broader realignment in political ideology (and corresponding policies) that more acutely shapes these health outcomes.

Consistent with the messaging channel, descriptive results indicate that individuals who used radio as their primary source of information about events in the news have markedly different views regarding mental health and mental health care than consumers of other forms of media. Using data from the GSS from after the repeal of the Fairness Doctrine, I regress a set of binary indicators concerning beliefs about various mental-health-related issues on fixed effects for the respondents' primary news source, including flexible controls for age, education, sex, race, region, and (in a second specification) political affiliation, with television (the dominant source of news at the time) as the omitted category.[15] As shown in Figure 15, individuals for whom radio is their primary media source exhibit a 27 percentage point lower willingness to use psychiatric medication for depression than television viewers, and are 18 percentage points less likely to believe psychiatric medication can improve peoples' lives. Additional results for differences by media source in beliefs about mental health are given in Table 5 and Table A5, which indicate that the aforementioned gap in willingness to use psychiatric medication does not stem from lower illness recognition: when presented with a vignette describing a symptomatic individual, radio and television audiences were comparably likely to indicate the person "has a mental illness", but radio listeners were significantly less likely to recommend seeing a psychiatrist or any mental-health professional. These results are all robust to including controls for party affiliation, which comprises 8 categories ranging from strong Democrat to strong Republican, indicating that the patterns

[15] Most specifications include only data from 2006, though some variables allow for estimation using a biannual sample from 2006 to 2018. Details are in the table notes for Table 5.

Table 5: Main Media Sources and Mental-Health–Related Beliefs

	No Party-ID FEs					+ Party-ID FEs				
Outcome (1=Agree)	Radio	Newspaper	Internet	Other	Cons./Obs.	Radio	Newspaper	Internet	Other	Cons./Obs.
A. Confidence in medical institutions										
Trust doctors' judgment	−0.073	−0.019	−0.062	−0.049	0.854	−0.089	−0.015	−0.053	−0.054	0.854
	(0.071)	(0.038)	(0.046)	(0.067)	909	(0.075)	(0.039)	(0.044)	(0.062)	906
Confident in medicine	−0.048	−0.018	−0.018	−0.107	0.407	−0.043	−0.016	−0.016	−0.098	0.405
	(0.030)	(0.021)	(0.020)	(0.031)	6,016	(0.030)	(0.021)	(0.020)	(0.031)	5,977
B. Willingness to take psychiatric medication										
Take b/c personal trouble	−0.101	0.026	−0.064	−0.089	0.310	−0.091	0.029	−0.063	−0.105	0.310
	(0.062)	(0.045)	(0.060)	(0.059)	897	(0.061)	(0.046)	(0.060)	(0.058)	894
Take b/c stress	−0.205	−0.064	−0.172	−0.107	0.519	−0.169	−0.055	−0.161	−0.118	0.513
	(0.078)	(0.048)	(0.058)	(0.074)	894	(0.078)	(0.047)	(0.056)	(0.074)	892
Take b/c depressed	−0.271	−0.042	−0.089	−0.158	0.546	−0.247	−0.038	−0.090	−0.177	0.545
	(0.075)	(0.049)	(0.059)	(0.077)	897	(0.079)	(0.049)	(0.057)	(0.075)	895
Take b/c fear	−0.092	−0.066	−0.083	0.025	0.688	−0.073	−0.059	−0.079	0.011	0.686
	(0.077)	(0.050)	(0.059)	(0.071)	896	(0.081)	(0.050)	(0.059)	(0.071)	894
C. Beliefs about psychiatric medication										
Harmful to body	0.094	0.000	−0.038	−0.017	0.275	0.084	0.000	−0.042	−0.010	0.275
	(0.088)	(0.048)	(0.050)	(0.082)	864	(0.088)	(0.048)	(0.050)	(0.079)	861
Helps people feel better	−0.184	−0.028	−0.045	−0.016	0.703	−0.167	−0.023	−0.046	−0.029	0.701
	(0.079)	(0.052)	(0.055)	(0.090)	870	(0.078)	(0.052)	(0.057)	(0.092)	867
D. Response to mental health vignettes (person X)										
X has mental illness	0.013	−0.010	0.013	0.070	0.612	0.004	−0.020	−0.001	0.066	0.618
	(0.078)	(0.049)	(0.060)	(0.085)	887	(0.079)	(0.050)	(0.062)	(0.081)	885
Recommend psychiatrist	−0.091	0.018	−0.021	0.018	0.874	−0.094	0.014	−0.029	0.005	0.878
	(0.070)	(0.030)	(0.045)	(0.048)	890	(0.069)	(0.030)	(0.044)	(0.052)	887
Recommend other MH	−0.136	−0.044	−0.044	−0.026	0.821	−0.134	−0.048	−0.054	−0.036	0.825
	(0.071)	(0.049)	(0.042)	(0.061)	887	(0.069)	(0.049)	(0.044)	(0.064)	884
E. Acceptability of suicide										
OK if: dishonor family	0.041	0.017	0.031	0.000	0.087	0.041	0.015	0.030	0.002	0.088
	(0.020)	(0.013)	(0.011)	(0.018)	5,842	(0.020)	(0.013)	(0.011)	(0.019)	5,801
OK if: tired of living	0.019	0.034	0.013	−0.002	0.180	0.019	0.030	0.012	−0.004	0.181
	(0.025)	(0.019)	(0.015)	(0.025)	5,786	(0.025)	(0.019)	(0.015)	(0.025)	5,744

Notes: This table shows descriptive patterns for the relationship between primary media sources, mental-health-related beliefs, and trust in medical institutions in the US. Each outcome variable is a binary indicator for whether the respondent agreed with the given statement or question. The independent variable refers to main source of information the respondent uses for information about events in the news. The coefficients are estimated by regressing each outcome on main media source fixed effects, using television as the omitted category. Each regression includes fixed effects for year, 10-year age bins, sex, race, education level (aggregated into 8 categories), and region (9 categories). The specification on the right side of the table also includes fixed effects for party identification, which has 8 categories (from strong Democrat to strong Republican, including other Party). Variables "Confident in medicine" and suicide-related variables are included in biannual surveys from 2006 to 2018. All other variables are estimated in 2006—the only year of overlap with the main media source variable. "Cons./Obs." refer to the baseline mean in the regression and the number of observations used to estimate the regression. Standard errors are clustered at the primary sampling unit level.

are not completely driven by party preferences. Although these patterns are purely descriptive, they are consistent with the notion that conservative radio may cultivate skepticism toward medical expertise and discourage help-seeking, thereby increasing vulnerability to despair-related mortality.

Irrespective of the specific channels, these findings show that partisan media extends well beyond influencing elections and shaping voter attitudes: it also carries significant consequences for population health. This conclusion aligns with a wider literature documenting that highly polarized information environments can exacerbate social and economic vulnerabilities. As Oberlander (2024) emphasizes, contemporary trends in mortality from despair cannot be understood solely in economic terms—cultural, political, and media factors play a crucial role. The repeal of the Fairness Doctrine in 1987—and the ensuing boom in conservative talk radio—creates a quasi-experimental setting that illuminates these dynamics. While existing descriptive work highlights that Republicans and Democrats differ in health outcomes, this analysis provides evidence of a causal link between changing political communication and rising despair-related mortality. More generally, these results underscore the interconnectedness of political and health systems: shifting the former can have profound, and sometimes unexpected, consequences for the latter.

6 Conclusion

This paper shows that deregulating radio content in 1987 had lasting effects on not only local political outcomes but also aspects of public health in the United States. By exploiting quasi-experimental variation in the rapid expansion of conservative talk radio following repeal of the Fairness Doctrine, I demonstrate a sizable and enduring rightward shift in voting behavior, beginning as early as the 1992 election and remaining apparent through subsequent decades. The effects I document also extend beyond the political sphere: counties that gained greater exposure to talk radio also experienced higher rates of mortality related to alcohol, drug overdoses, and suicide.

Overall, these findings underscore the capacity of partisan media to operate as a powerful channel for political persuasion, shaping ideological preferences and amplifying polarization. At the same time, the results speak to broader ways in which changes in political messaging can shape behavior outside the electoral arena, including decisions about substance use and mental health. From a policy perspective, these insights have implications for debates over media regulation and public-health interventions. The repeal of the Fairness Doctrine opened the door for hosts to air partisan views unencumbered, which in turn influenced voting patterns and downstream health outcomes. Policymakers interested in mitigating polarization and its health consequences may wish to consider the unintended effects of media deregulation, as well as options that bolster media literacy or expand mental-health resources in politically shifting communities.

In sum, the repeal of the Fairness Doctrine enabled a novel, conservative-leaning information ecosystem whose reach extended well beyond electoral success for Republican candidates. The findings indicate that media-induced ideological shifts can propagate into health-relevant behaviors. By linking political persuasion to rising deaths of despair, this study highlights that the realm of partisan media can have substantial long-run social costs, tying aspects of public health to the channels that shape political discourse.

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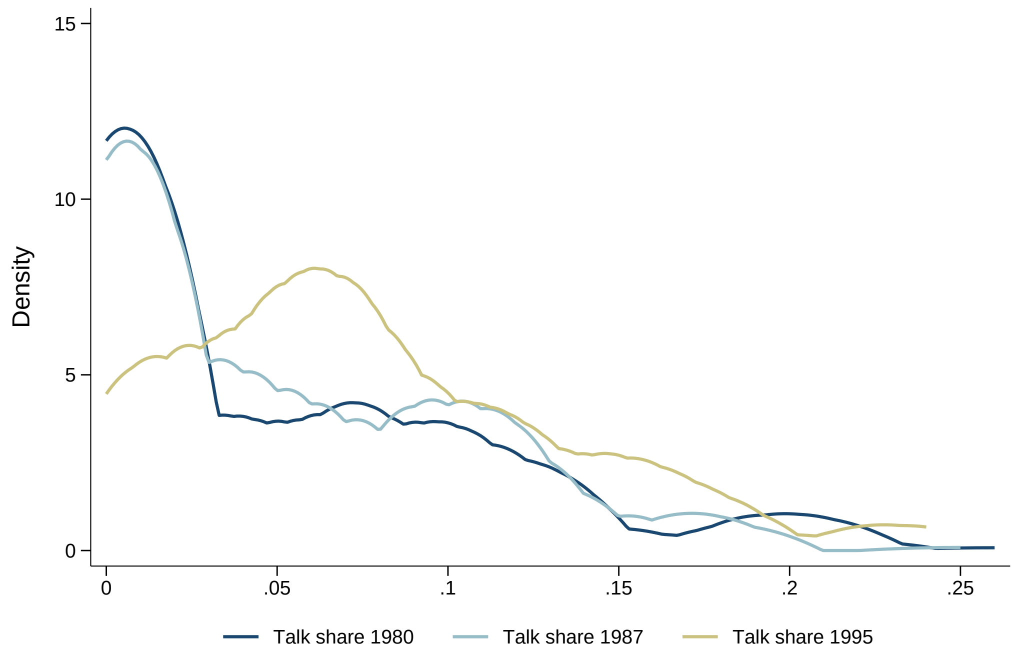
Appendix: Supplementary Figures and Tables Referenced in the Text

Appendix Figures

Figure A1: Changes in talk radio before/after repeal



Notes: This figure shows density plots of the distribution of talk radio market share across markets in 1980, 1987, and 1995. The dark blue line shows the distribution for 1980, the light blue line shows the distribution for 1987, and the light brown line shows the distribution for 1995.

Figure A2: Most popular radio shows

Top Talk Radio Hosts, Millions of Listeners (Weekly)

HOST	POLITICAL LEANING	2010	2007	2003
Rush Limbaugh	Conservative	15.0	13.5	14.5
Sean Hannity	Conservative	14.0	12.5	11.75
Glenn Beck	Conservative	9.0	5	*
Michael Savage	Conservative	9.0	8	7
Mark Levin	Conservative	8.5	4	*
Dave Ramsey	Financial Advice	8.5	4	*
Neal Boortz	Conservative	6.0	4	2.5
Laura Ingraham	Conservative	6.0	5	1.25
Jim Bohannon	Ind./Moderate	3.75	3.25	4
Jerry Doyle	Conservative	3.75	*	*
Mike Gallagher	Conservative	3.75	3.75	2.5
Michael Medved	Conservative	3.75	3.75	*
Doug Stephan	Ind./Moderate	3.75	3.25	2
Bill Bennett	Conservative	3.5	*	*
Clark Howard	Consumer Advice	3.5	*	*
George Noory	Supernatural, Paranormal	3.5	*	*

Source: *The State of the News Media, 2010*, Pew Project for Excellence in Journalism, at http://www.stateofthemedia.org/2010/audio_talk_radio.php#audio_toptalkhosts; and "The Top Talk Radio Audiences," *Talkers Magazine*, March, 2011, p. 22.

Note: * = Information unavailable or talk host not nationally broadcast.

Source: Pew Research Center

Figure A3: James Duncan's *American Radio*

BAKERSFIELD

Arbitron Rank/Pop (12+): 88/412,500
MSA Rank/Pop: 87/620,000
DMA # 121
Average Person Ratings: 15.8
Market TSL: 21.50 hours

Stations: 25/23
Diaries: 1315/314:1/51.7%
Sample Target: 1245
% Below Line: 2.1%
% Not Listed: 19.2%

1994 Revenue: $14,200,000
HH Inc.: $32,490
Retail $: 4.1 Bil
Pop per Station: 17,935
#1 Biller: KUZZ-F $3,000,000

12+ METRO		1/4/SHARE	WIN 95	FAL 94	SUM 94	SPG 94	4 Book Avg.	12+ TSA 1/4/CUME	12+ METRO CUME/CUME RATING	FAL 94	SPG 94	Revenue Level	
1. KUZZ-F	(C)	102/15.6	12.6	16.7	16.0	13.5	15.2	108/1326	1186/28.8	29.4	27.4	E	KUZZ-F
2. KKXX-F	(CHR)	68/10.4	10.9	8.7	7.4	10.4	9.4	68/958	936/22.7	21.3	24.9	C	KKXX-F
3. KERN	(N/T)	44/ 6.7	7.1	8.1	7.0	8.4	7.2	44/527	522/12.7	14.6	14.5	C	KERN
4. KRAB-F	(AOR)	40/ 6.1	9.3	7.8	8.2	7.5	7.9	40/710	699/16.9	17.6	17.6	C	KRAB-F
5. KERN-F	(O)	24/ 5.4	3.2	3.9	5.7	4.3	4.6	36/596	558/13.5	11.4	14.1	B	KERN-F
6. KGFM-F	(SAC)	30/ 4.6	5.8	4.1	5.1	6.6	4.9	30/509	491/11.9	10.3	14.7	B	KGFM-F
7. KIWI-F	(SP)	29/ 4.4	3.6	4.5	6.2	5.3	4.7	30/375	344/ 8.3	7.3	9.0	C	KIWI-F
8. KKBB-F	(CL AOR)	19/ 2.9	3.0	0.9	---	0.9	1.7	22/353	326/ 7.9	4.0	3.0		KKBB-F
9. KLLY-F	(AC)	18/ 2.8	4.9	3.3	4.7	3.7	3.9	19/410	394/ 9.6	9.4	10.2	B	KLLY-F
10. KNZR	(N/T)	17/ 2.6	2.4	1.1	1.9	2.6	2.0	18/362	348/ 8.4	5.0	8.3		KNZR
KWAC	(SP)	17/ 2.6	1.7	3.2	1.4	2.2	2.2	17/201	194/ 4.7	4.8	4.5	B	KWAC
12. KCWR	(C)	15/ 2.3	1.5	2.4	2.8	1.2	2.3	17/239	225/ 5.5	5.4	4.1	A	KCWR
13. KSUV-F	(SP)	13/ 2.0	3.3	4.4	3.2	3.6	3.2	13/272	262/ 6.4	7.1	7.5	B	KSUV-F
14. KTIE-F	(C)	11/ 1.7	2.0	2.1	1.6	1.4	1.9	11/280	263/ 6.4	7.0	7.0	A	KTIE-F
KCNQ-F	(C)	11/ 1.7	1.2	1.1	1.3	1.2	1.3	11/91	87/ 2.1	1.6	1.6		KCNQ-F

12+ FM SHARE (METRO): 78.75% (415 of 527) (SPG 94: 75.22%)

TEENS	18-34	18-49	25-49	25-54	35+	12+ AMD	12+ MID	12+ PMD	12+ EVE	OVERNIGHT
1 KKXX-F	1 KUZZ-F	1 KUZZ-F	1 KUZZ-F>	1 KUZZ-F>>	1 KUZZ-F>	1 KUZZ-F>	1 KUZZ-F>	1 KUZZ-F	1 KKXX-F	1 KUZZ-F>>
2 KUZZ-F<	2 KKXX-F>	2 KKXX-F>	2 KKXX-F	2 KKXX-F	2 KERN	2 KKXX-F	2 KERN	2 KKXX-F>	2 KUZZ-F>	2 KKXX-F
3 KRAB-F>	3 KRAB-F>	3 KRAB-F	3 KERN	3 KERN	3 KERN-F>	3 KERN	3 KKXX-F>	3 KERN-F<	3 KRAB-F>	KNZR
4	4 KIWI-F	4 KERN	4 KERN-F	4 KERN-F	4 KKXX-F	4 KRAB-F	4 KGFM-F<	4 KRAB-F	4	4
5	5 KGFM-F	5 KERN-F	5 KGFM-F	5 KGFM-F	5 KGFM-F	5 KERN-F	5 KERN-F	5 KERN	5	5
6	KKBB-F	6	KRAB-F	6	6	6	6	6	6	6

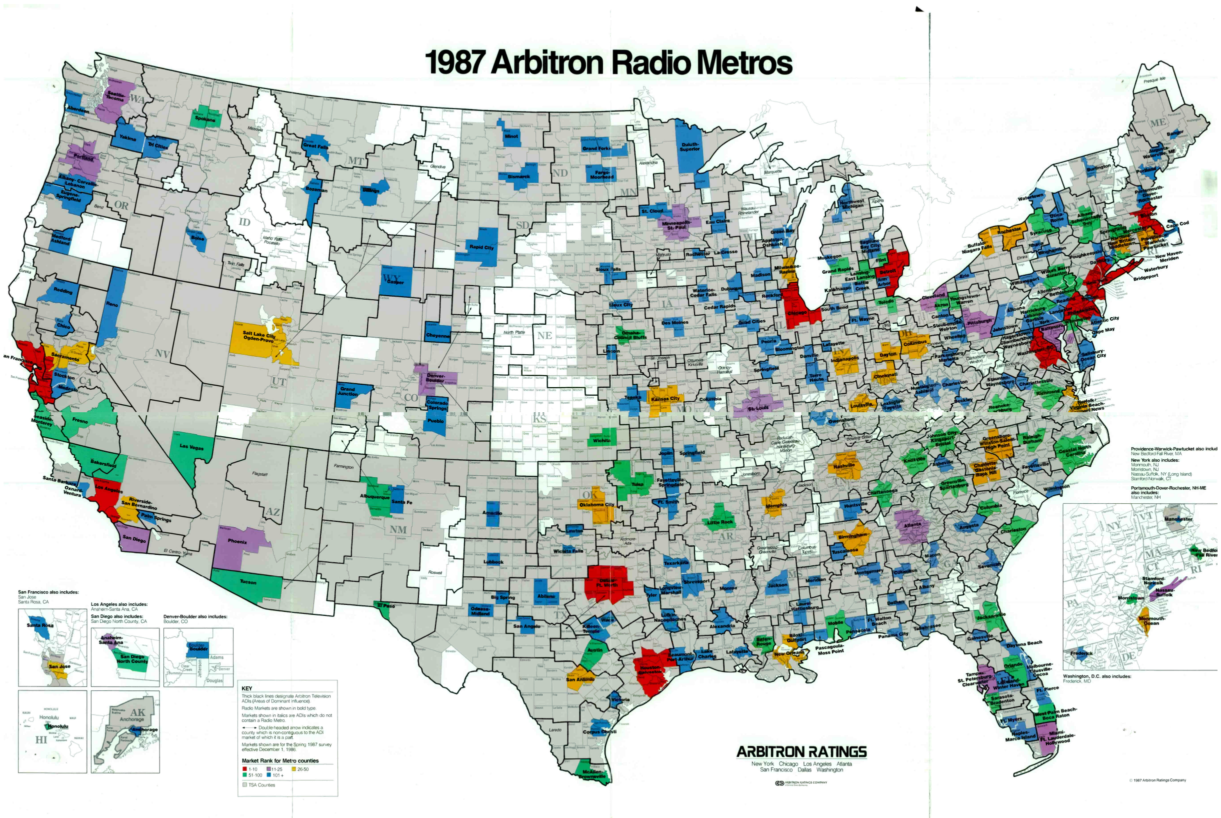
WOM 18-24	WOM 18-34	WOM 25-34	WOM 18-49	WOM 25-54	MEN 18-24	MEN 18-34	MEN 25-34	MEN 18-49	MEN 25-54
1 KKXX-F	1 KUZZ-F<	1 KUZZ-F	1 KUZZ-F<	1 KUZZ-F>	1 KUZZ-F	1 KUZZ-F	1 KUZZ-F	1 KUZZ-F>	1 KUZZ-F>
2 KUZZ-F	2 KKXX-F>	2 KKXX-F	2 KKXX-F>	2 KKXX-F<	KKXX-F	2 KKXX-F<	2 KKBB-F	2 KKXX-F<	2 KERN >
3 KRAB-F	3 KGFM-F	3 KGFM-F	3 KGFM-F>	3 KGFM-F>	3 KIWI-F	3 KRAB-F	3 KERN	3 KERN	3 KKBB-F
4	4 KRAB-F	4 KRAB-F	4 KERN-F	4 KERN-F	4 KRAB-F	4 KKBB-F	KRAB-F	4 KRAB-F	KKXX-F
5	5	KLLY-F	KRAB-F	5 KLLY-F	5	5 KERN	5 KKXX-F	5 KIWI-F	5 KRAB-F
6	6	6	6	6	6	KIWI-F	6	KKBB-F	KERN-F

FORMATS	AM	FM	TOTAL	%	SPG 94 %
AC		18 (1)	18 (1)	3.4	5.5
AOR/CLASSIC/NR		63 (3)	63 (3)	12.0	9.8
COUNTRY	15 (1)	124 (3)	139 (4)	26.4	21.7
NEWS/TALK	61 (2)		61 (2)	11.6	15.5
CHR		78 (2)	78 (2)	14.8	14.3
BLACK/URBAN					
SOFT AC/EZ		30 (1)	30 (1)	5.7	7.9
HISPANIC	22 (2)	47 (3)	69 (5)	13.1	16.7
OLDIES		35 (1)	35 (1)	6.6	5.7
FULL SVC/VARIETY					
STANDARDS	9 (2)	7 (1)	16 (3)	3.0	1.2
RELIG/GOSPEL		8 (1)	8 (1)	1.5	1.7
SPORTS	5 (1)		5 (1)	0.9	0.0
CLASSICAL					
JAZZ					
OTHERS/UNKNOWN		5 (1)	5 (1)	0.9	0.0
TOTALS	112 (8)	415 (17)	527 (25)		

OTHER RATED STATIONS				METRO CUME TSA TSA SHARE/RTG/AQH/CUME
KBID	(ST)	1350	BAKERSFIELD	1.1/1.7/ 7/ 72
KCHJ	(SP)	1010	DELANO	0.8/2.6/ 12/ 207
KGEO	(SPRTS)	1230	BAKERSFIELD	0.8/3.0/ 5/ 144
KHIS-F	(REL)	96.5	BAKERSFIELD	1.2/3.9/ 9/ 175
KYLD-F	(?)	104.3	SHAFTER	0.8/2.0/ 5/ 82
KVLI-AF	(ST)	104.5	LAKE ISABELLA	1.4/2.1/ 9/ 86
KZBA-F	(SP)	97.7	SHAFTER	0.8/2.6/ 6/ 124
KBOS-F	(CHR)	94.9	TULARE	1.5/4.5/ 53/ 781
KLOS-F	(AOR)	95.5	LOS ANGELES	0.6/2.0/ 23/ 287

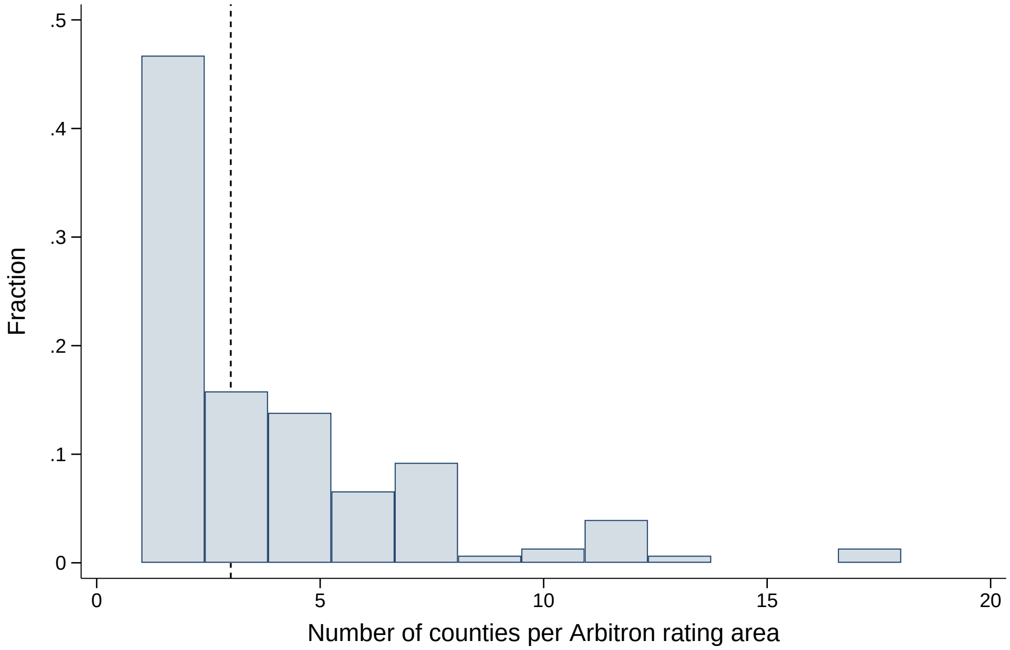
Source: James Duncan's *American Radio*, Spring 1995 Edition

Figure A4: 1987 Arbitron radio metro areas



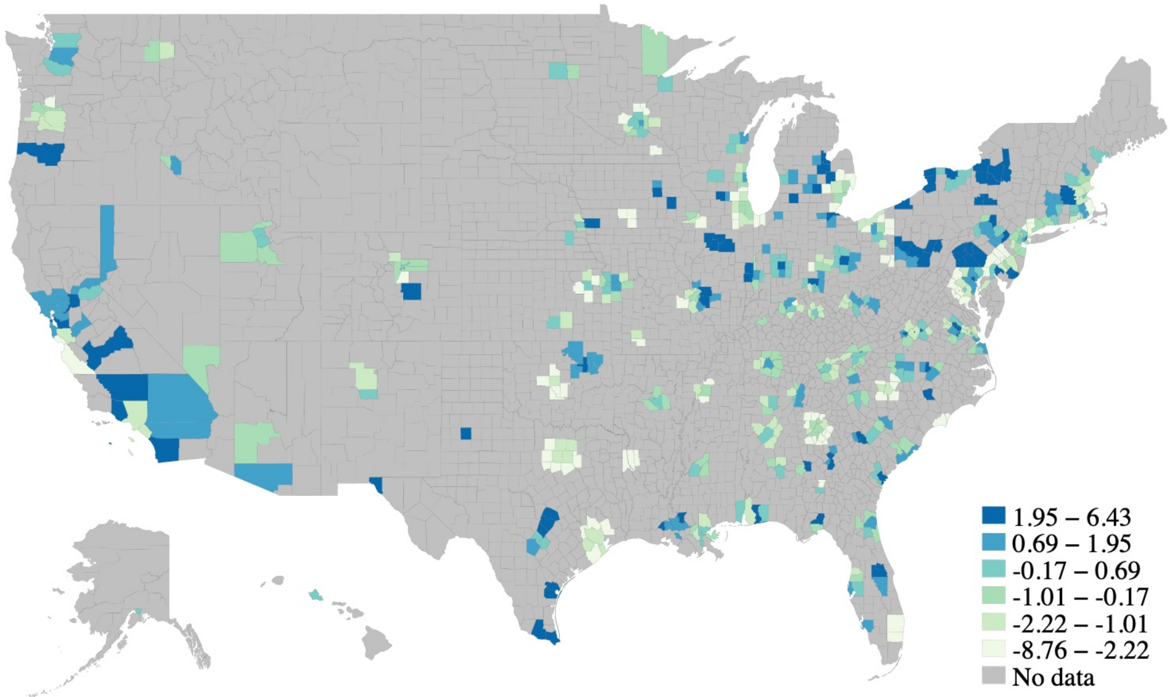
Source: Arbitron

Figure A5: Number of counties per Arbitron metro area



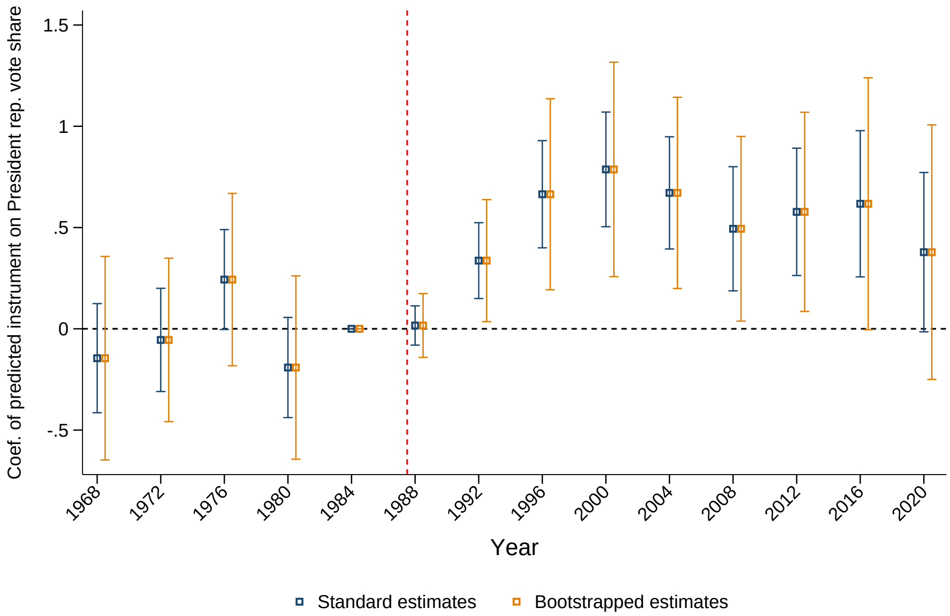
Notes: This plot shows a histogram of the number of counties included per Arbitron rating area. The median number of counties per area is 3, shown with the dashed black line.

Figure A6: Instrument variation



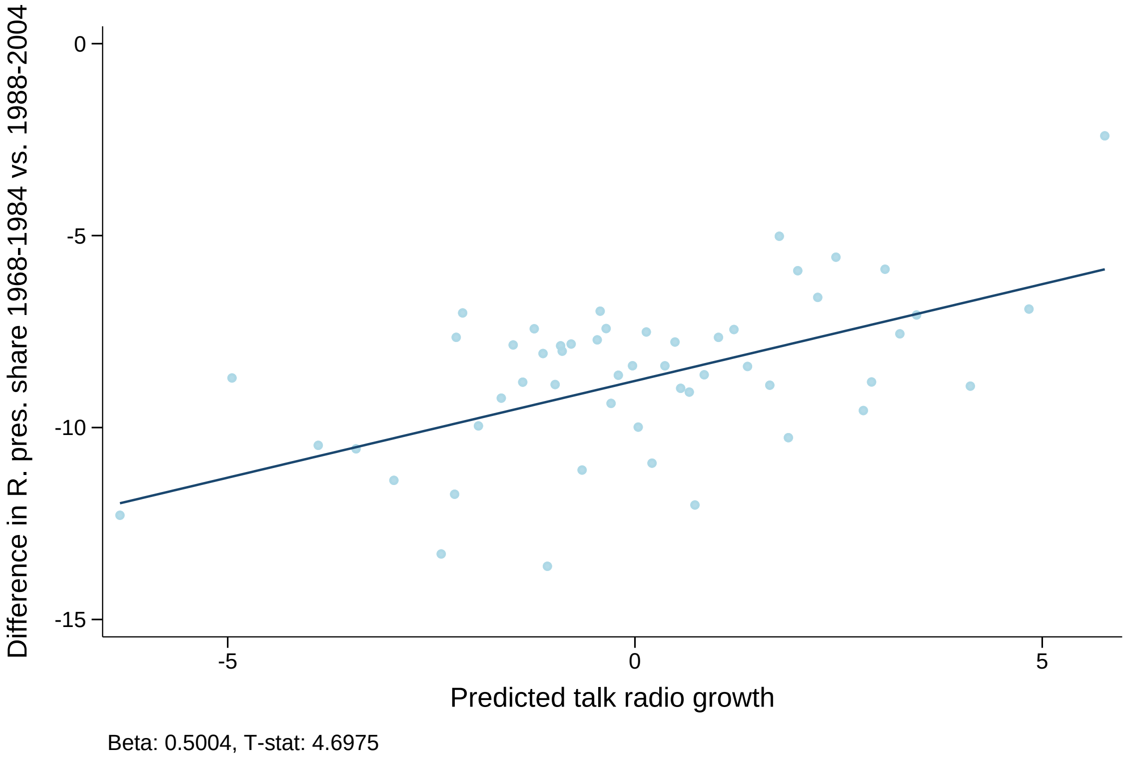
Notes: This plot shows a map of the US, with counties colored by the predicted change in talk radio market share. Darker colors indicate a larger predicted increase in talk radio share.

Figure A7: Event study Republican presidential vote share with bootstrapped CIs



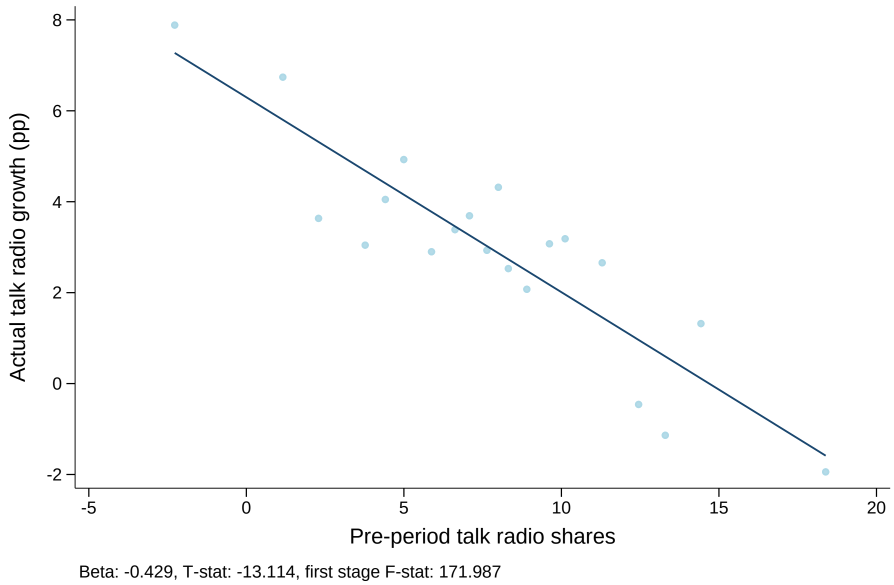
Notes: The above figure shows the comparison between standard estimates and bootstrapped estimates for the event study of presidential two-party Republican vote share in presidential elections. Each event study is estimated using specification (3) of Equation 2. The bootstrapped estimates are created using a block cluster bootstrap procedure with 1000 replications. Here, estimated coefficients are plotted instead of coefficients multiplied by the relevant Q4-Q1 gap as in other event studies.

Figure A8: Distribution of effect sizes for Republican presidential vote share

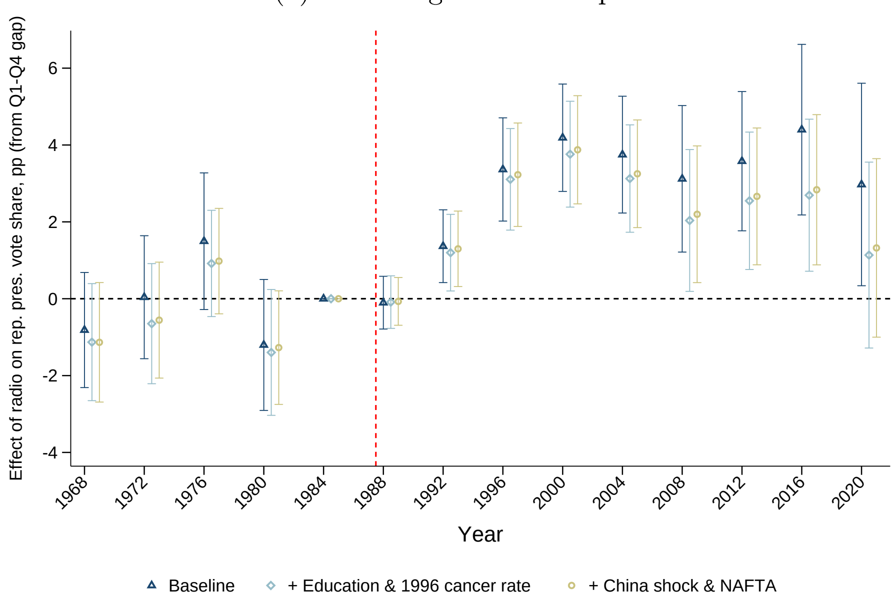


Notes: This figure shows the relationship between mean differences in two-party Republican vote share between the 1988–2004 elections and the 1968–1984 elections and the predicted talk radio growth instrument, residualized on first stage controls. The binned scatterplot and regression fit are weighted by 1980 county population and include state fixed effects. Standard errors are clustered at the Arbitron Radio Metro level.

Figure A9: First stage and presidential event studies, alternative instrument



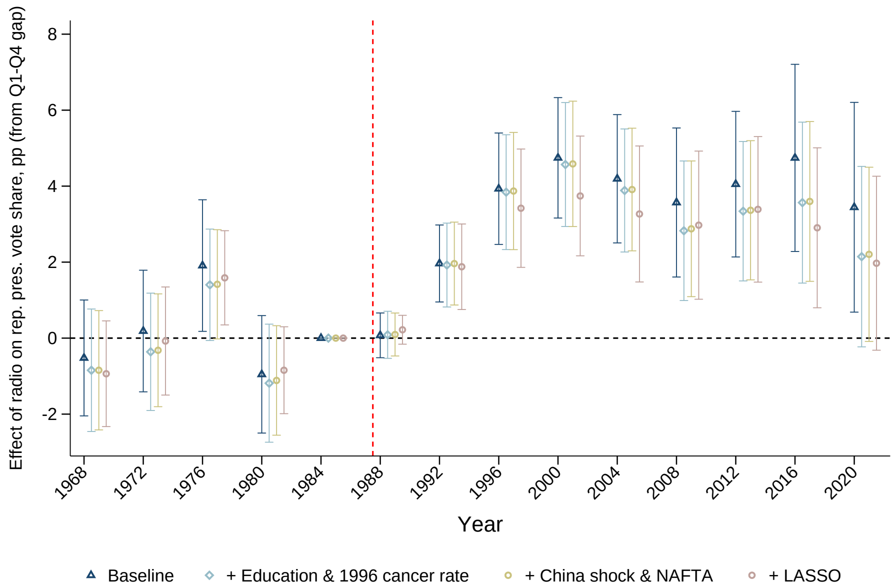
(a) First stage relationship



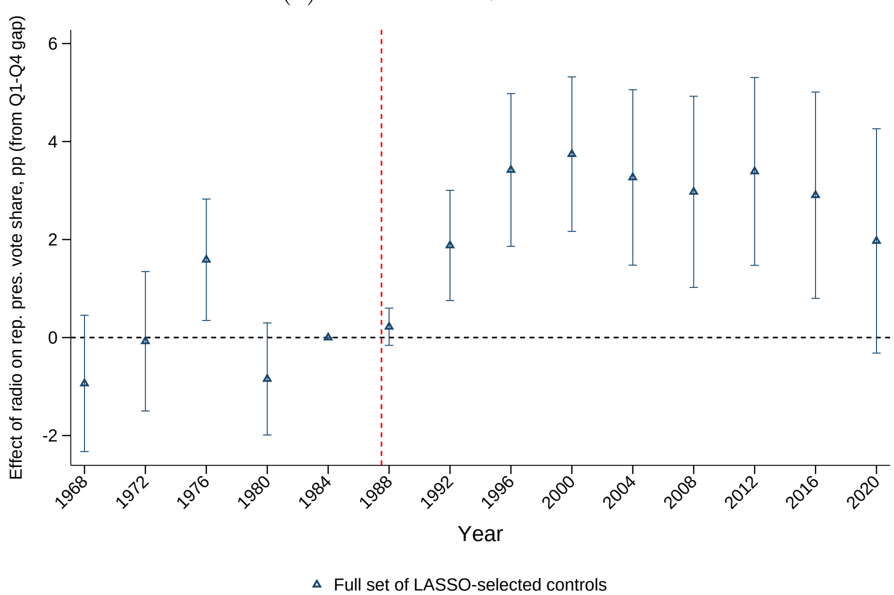
(b) Event study, two-party Republican vote share

Notes: These figures show the analogues to Figure 4 and Figure 7 using talk radio shares in 1977 to instrument for growth in the talk radio format between 1980 and 1995. Panel (a) shows that when using this further lagged instrument, the first stage F-statistic is 171.99, far above conventional weak-instrument thresholds. Panel (b) shows that the resulting event study for presidential vote shares closely mimics the analogous event study in the main text. Robustness to using further lagged shares as the instrument mitigates concerns of bias stemming from measurement error of talk shares in 1980, and show results are robust to the choice of baseline year for the excluded instrument.

Figure A10: Event study Republican presidential vote share with LASSO-selected controls



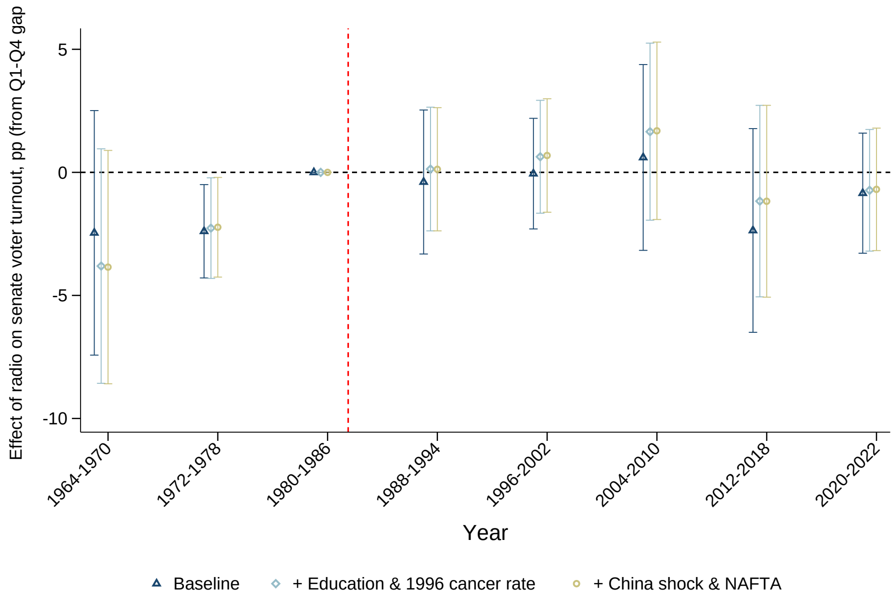
(a) Main series + LASSO



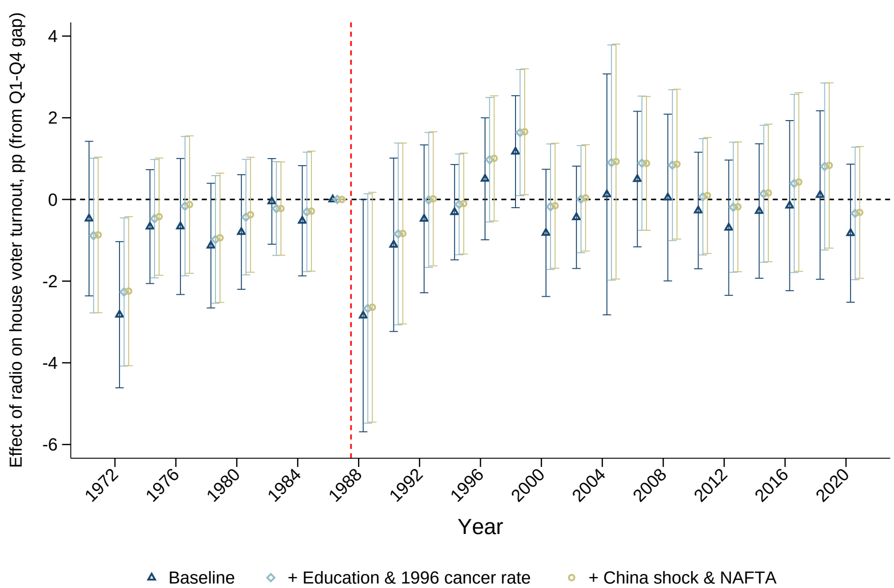
(b) LASSO series

Notes: This event study estimates Equation 2 for the two-party Republican vote share outcome, using a post-LASSO procedure for covariate selection to include in the regression as controls. First, a LASSO regression is run of the instrument on the full set of covariates in the data, and selected features are included as controls interacted with year fixed effects in the event study regression. The full set of LASSO-selected variables is: percent high-school graduates (1980), population (1970), total land area in square miles (1980), percent male age 35–44 (1980), percent male age 85+ (1980), median age of males (1980), percent Black (1980), percent Native American (1980), percent divorced (1980), percent of youth not enrolled in high school (1980), percent employed (1980), percent employed in construction (1980), percent employed in communications (1980), percent employed in wholesale/retail trade (1980), percent employed in entertainment services (1980), percent state-government workers (1980), percent self-employed (1980), percent of households with \$125–149k income (1980), percent occupied housing units (1980), median year housing built (1980), cancer prevalence (1996), unemployment rate (1980), and rural population share (1980).

Figure A11: Event studies of turnout in Senate and House elections



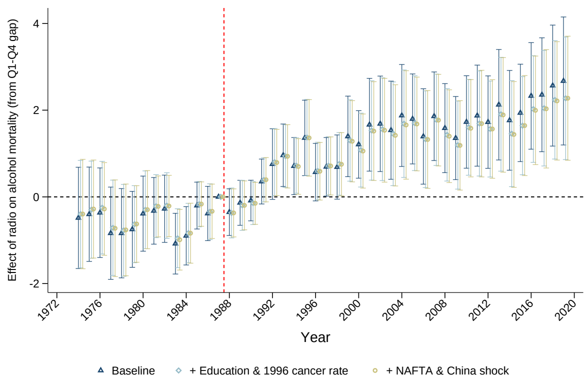
(a) Turnout in Senate elections



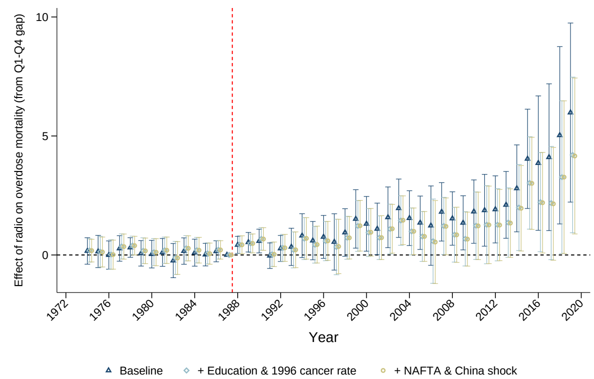
(b) Turnout in House elections

Notes: These figures replicate Figure 11 for Senate and House elections. For each panel, three estimates are shown in different colors, starting with the baseline model (dark blue triangles). This specification estimates Equation 2 with state-by-year fixed effects, with $\mathbf{X}_{ct}$ containing interactions between 1980 rural share and year fixed effects and 1980 population density and year fixed effects; specification (2), shown in light blue diamonds, additionally controls for 1980 percent college educated interacted with year fixed effects and 1996 cancer rate interacted with year fixed effects; specification (3), shown in light brown circles, replicates specification (2) but with added controls for NAFTA exposure from Choi et al. (2024) interacted with year fixed effects and China shock exposure from Autor et al. (2019) interacted with year fixed effects. All specifications are weighted by 1980 county population, and standard errors are clustered at the Arbitron Radio Metro level.

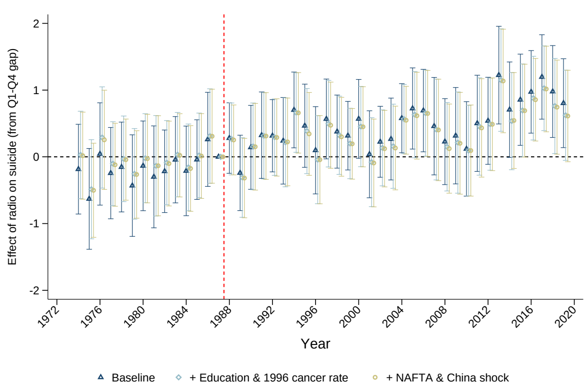
Figure A12: Event studies for components of deaths of despair



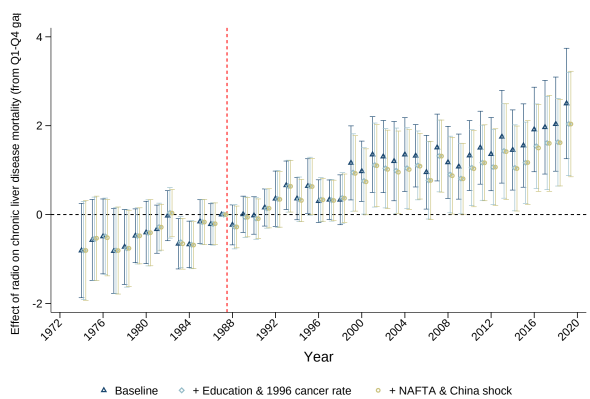
(a) Alcohol-related mortality



(b) Overdose mortality



(c) Suicide mortality



(d) Chronic liver disease mortality

Notes: These figures decompose Figure 13 into four categories of mortality: panel (a) shows alcohol-related mortality, panel (b) shows overdose mortality, panel (c) shows suicide mortality, and panel (d) shows chronic liver disease mortality. For each panel, three estimates are shown in different colors, starting with the baseline model (dark blue triangles). This specification estimates Equation 2 with state-by-year fixed effects, with $\mathbf{X}_{ct}$ containing interactions between 1980 rural share and year fixed effects and 1980 population density and year fixed effects; specification (2), shown in light blue diamonds, additionally controls for 1980 percent college educated interacted with year fixed effects and 1996 cancer rate interacted with year fixed effects; specification (3), shown in light brown circles, replicates specification (2) but with added controls for NAFTA exposure from Choi et al. (2024) interacted with year fixed effects and China shock exposure from Autor et al. (2019) interacted with year fixed effects. All specifications are weighted by 1980 county population, and standard errors are clustered at the Arbitron Radio Metro level.

Appendix Tables

Table A1: Additional Specifications, DiD Estimates for Political Outcomes

	Two-party Rep. vote share			**Election turnout**		
	President	Senate	House	President	Senate	House
+ Med. income	0.699	0.650	0.961	0.179	0.185	0.178
	(0.148)	(0.130)	(0.274)	(0.104)	(0.113)	(0.120)
	[0.000]	[0.000]	[0.001]	[0.086]	[0.103]	[0.138]
	∼4.229	∼3.934	∼5.821	∼1.084	∼1.119	∼1.079
+ China shock	0.697	0.651	0.956	0.183	0.187	0.180
	(0.150)	(0.129)	(0.277)	(0.106)	(0.114)	(0.120)
	[0.000]	[0.000]	[0.001]	[0.087]	[0.102]	[0.136]
	∼4.209	∼3.933	∼5.773	∼1.103	∼1.132	∼1.089
+ NAFTA	0.693	0.652	0.964	0.182	0.187	0.180
	(0.149)	(0.129)	(0.279)	(0.107)	(0.114)	(0.121)
	[0.000]	[0.000]	[0.001]	[0.089]	[0.103]	[0.138]
	∼4.152	∼3.903	∼5.772	∼1.092	∼1.122	∼1.081
+ Cancer rate	0.687	0.660	0.976	0.183	0.188	0.181
	(0.146)	(0.127)	(0.281)	(0.110)	(0.117)	(0.124)
	[0.000]	[0.000]	[0.001]	[0.099]	[0.111]	[0.146]
	∼4.111	∼3.944	∼5.835	∼1.094	∼1.122	∼1.082
Observations	7772	3899	14629	7008	3620	13773
Dep. var. mean	54.364	45.677	43.869	54.756	45.123	43.176
R-squared	0.895	0.909	0.694	0.937	0.934	0.883

Notes: This table extends the results from Table 3 for additional controls. The specifications include controls additively, so the first specification, which adds 1980 median income interacted with year fixed effects, is added in addition to the controls in the final specification of Table 3. The cancer rate control is the 1996 cancer rate, taken from Arteaga and Barone (2022) and the NAFTA and China shock controls are from Choi et al. (2024) and Autor et al. (2019), respectively.

Table A2: Bootstrapped DiD Estimates for Political Outcomes

	President	**Senate**	**House**
Panel A. Two-party Republican vote share			
County, Year,	1.039	0.854	1.336
State x Year FEs	[0.542, 1.857]	[0.443, 1.679]	[0.632, 2.790]
	∼5.798	∼4.765	∼7.460
+ Pct. rural & pop. density	0.569	0.400	0.760
	[0.177, 1.004]	[0.005, 0.796]	[0.186, 1.790]
	∼3.291	∼2.315	∼4.393
+ Pct. college & income	0.699	0.650	0.961
	[0.285, 1.219]	[0.353, 1.186]	[0.415, 2.060]
	∼4.229	∼3.934	∼5.821
+ NAFTA & China shock	0.693	0.652	0.964
	[0.258, 1.245]	[0.347, 1.251]	[0.305, 2.257]
	∼4.152	∼3.903	∼5.772
Panel B. Election turnout			
County, Year,	0.295	0.251	0.228
State x Year FEs	[0.083, 0.646]	[–0.026, 0.616]	[–0.121, 0.600]
	∼1.645	∼1.503	∼1.272
+ Pct. rural & pop. density	0.131	0.095	0.086
	[–0.113, 0.420]	[–0.227, 0.427]	[–0.355, 0.423]
	∼0.755	∼0.570	∼0.496
+ Pct. college & income	0.179	0.185	0.178
	[–0.069, 0.477]	[–0.083, 0.505]	[–0.165, 0.505]
	∼1.084	∼1.105	∼1.079
+ NAFTA & China shock	0.182	0.187	0.180
	[–0.071, 0.493]	[–0.086, 0.532]	[–0.151, 0.515]
	∼1.092	∼1.121	∼1.081

Notes: This table shows difference-in-differences estimates for political outcomes, where coefficient estimates and 95% confidence intervals are estimated using the block cluster bootstrap with 1000 replications. Estimated 95% confidence intervals are shown in brackets. The difference-in-differences estimate multiplied by the Q4-Q1 spread of the instrument (as in the event studies) are indicated by a tilde (∼). The control variables in each specification are identical to those in Figure 7. The first specification includes only state-by-year fixed effects; the second includes state-by-year fixed effects, with $\mathbf{X}_{ct}$ containing interactions between 1980 rural share and year fixed effects and 1980 population density and year fixed effects; the third specification additionally controls for 1980 percent college educated interacted with year fixed effects and 1996 cancer rate interacted with year fixed effects; and the fourth specification replicates specification the third but with added controls for NAFTA exposure from Choi et al. (2024) interacted with year fixed effects and China shock exposure from Autor et al. (2019) interacted with year fixed effects. All specifications are weighted by 1980 county population.

Table A3: Additional Specifications, DiD Estimates for Health-Related Outcomes

	Health-related Outcomes			
	Alcohol-related	Overdose	Suicide	All despair
+ Med. inc.	0.281	0.141	0.060	0.466
	(0.126)	(0.126)	(0.037)	(0.248)
	[0.027]	[0.264]	[0.102]	[0.063]
	$\sim$ 1.702	$\sim$ 0.855	$\sim$ 0.366	$\sim$ 2.821
+ China shock	0.287	0.133	0.064	0.467
	(0.128)	(0.128)	(0.037)	(0.252)
	[0.026]	[0.302]	[0.087]	[0.066]
	$\sim$ 1.735	$\sim$ 0.801	$\sim$ 0.386	$\sim$ 2.820
+ NAFTA	0.293	0.129	0.064	0.470
	(0.130)	(0.126)	(0.037)	(0.254)
	[0.026]	[0.308]	[0.087]	[0.066]
	$\sim$ 1.754	$\sim$ 0.772	$\sim$ 0.385	$\sim$ 2.812
+ Cancer rate	0.294	0.133	0.065	0.475
	(0.128)	(0.118)	(0.036)	(0.241)
	[0.024]	[0.261]	[0.074]	[0.051]
	$\sim$ 1.756	$\sim$ 0.796	$\sim$ 0.389	$\sim$ 2.840
Observations	25662	25662	25662	25662
Dep. var. mean	8.516	2.974	12.642	23.300
R-squared	0.781	0.846	0.643	0.852

Notes: This table extends the results from Table 4 for additional controls. The specifications include controls additively, as in Table 4.

Table A4: Bootstrapped DiD Estimates for Deaths of Despair

	Specification			
	(1)	(2)	(3)	(4)
Dep var: deaths of despair				
DiD estimate	0.706	0.698	0.466	0.475
Bootstrapped CI	[0.161, 1.551]	[0.200, 1.351]	[-0.094, 1.089]	[-0.025, 1.223]
Q4-Q1 rescaling	∼3.940	∼4.036	∼2.821	∼2.840

Notes: This table shows difference-in-differences estimates for age-adjusted annual rates of deaths of despair. Coefficients and 95% confidence intervals (shown in brackets) are computed using the block cluster bootstrap with 1000 replications. Each column indicates a different specification. Estimated 95% confidence intervals are shown in brackets. The difference-in-differences estimate multiplied by the Q4-Q1 spread of the instrument (as in the event studies) are indicated by a tilde (∼). Specification (1) includes state-by-year fixed effects, with $\mathbf{X}_{ct}$ containing interactions between 1980 rural share and year fixed effects and 1980 population density and year fixed effects; specification (2) additionally controls for 1980 percent college educated interacted with year fixed effects and 1980 county average household income interacted with year fixed effects; specification (3) adds controls for NAFTA exposure from Choi et al. (2024) interacted with year fixed effects, China shock exposure from Autor et al. (2019) interacted with year fixed effects, and 1996 cancer rate form Arteaga and Barone (2022) interacted with year fixed effects. All specifications are weighted by 1980 county population.

Table A5: Main Media Sources and Mental-Health–Related Beliefs

	No Party-ID FEs					**+ Party-ID FEs**				
Outcome (1=Agree)	Radio	Newspaper	Internet	Other	Cons./Obs.	Radio	Newspaper	Internet	Other	Cons./Obs.
A. Confidence in medical institutions										
Some confidence in medicine	0.017	−0.004	−0.010	0.008	0.502	0.013	−0.006	−0.012	0.004	0.503
	(0.032)	(0.022)	(0.020)	(0.037)	6,016	(0.032)	(0.022)	(0.020)	(0.037)	5,977
Hardly any confidence	0.031	0.022	0.029	0.099	0.091	0.030	0.022	0.028	0.094	0.091
	(0.023)	(0.014)	(0.012)	(0.027)	6,016	(0.023)	(0.014)	(0.012)	(0.027)	5,977
B. Willingness to take psychiatric medication										
Won't take b/c personal trouble	0.210	0.063	0.140	0.058	0.466	0.193	0.057	0.140	0.073	0.467
	(0.069)	(0.049)	(0.067)	(0.087)	897	(0.068)	(0.049)	(0.067)	(0.084)	894
Won't take b/c stress	0.079	0.065	0.141	0.152	0.299	0.046	0.054	0.131	0.170	0.304
	(0.073)	(0.046)	(0.054)	(0.080)	894	(0.073)	(0.046)	(0.052)	(0.078)	892
Won't take b/c depressed	0.139	0.076	0.062	0.191	0.291	0.124	0.070	0.058	0.202	0.293
	(0.081)	(0.048)	(0.056)	(0.080)	897	(0.078)	(0.049)	(0.055)	(0.077)	895
Won't take b/c fear	0.071	0.064	0.039	0.003	0.186	0.048	0.055	0.039	0.011	0.188
	(0.071)	(0.046)	(0.046)	(0.063)	896	(0.073)	(0.045)	(0.047)	(0.061)	894
C. Beliefs about psychiatric medication										
Interferes w/ daily act.	0.065	0.023	0.036	0.123	0.415	0.069	0.027	0.039	0.126	0.412
	(0.073)	(0.051)	(0.063)	(0.081)	869	(0.072)	(0.051)	(0.062)	(0.079)	866
Helps w/ stresses	−0.054	−0.064	−0.072	0.005	0.874	−0.050	−0.063	−0.075	−0.010	0.874
	(0.058)	(0.039)	(0.041)	(0.077)	884	(0.059)	(0.038)	(0.041)	(0.080)	882
Makes relations easier	−0.073	−0.119	−0.032	0.077	0.798	−0.073	−0.116	−0.036	0.059	0.799
	(0.074)	(0.044)	(0.052)	(0.072)	882	(0.073)	(0.044)	(0.051)	(0.074)	880
Controls symptoms	0.006	−0.062	−0.025	0.052	0.890	0.017	−0.057	−0.019	0.047	0.888
	(0.044)	(0.033)	(0.044)	(0.052)	881	(0.043)	(0.033)	(0.044)	(0.055)	879
E. Acceptability of suicide										
OK if: incurable disease	0.038	0.033	0.039	−0.021	0.612	0.035	0.026	0.035	−0.017	0.615
	(0.028)	(0.021)	(0.019)	(0.035)	5,737	(0.028)	(0.021)	(0.018)	(0.035)	5,700
OK if: bankrupt	0.018	0.016	0.029	−0.002	0.092	0.017	0.014	0.028	−0.001	0.093
	(0.018)	(0.013)	(0.012)	(0.019)	5,853	(0.018)	(0.013)	(0.012)	(0.019)	5,810

Notes: This table is identical to Table 5 for additional mental-health-related outcomes not included in the main text table. As before, each outcome variable is a binary indicator for whether the respondent agreed with the given statement or question. The independent variable refers to main source of information the respondent uses for information about events in the news. The coefficients are estimated by regressing each outcome on main media source fixed effects, using television as the omitted category. Each regression includes fixed effects for year, 10-year age bins, sex, race, education level (aggregated into 8 categories), and region (9 categories). The specification on the right side of the table also includes fixed effects for party identification, which has 8 categories (from strong democrat to strong republican, including other party). Panel A, "Confidence in medical institutions" and Panel E, "Acceptability of suicide" variables are included in biannual surveys from 2006 to 2018. All other variables are estimated in 2006—the only year of overlap with the main media source variable. "Cons./Obs." refer to the baseline mean in the regression and the number of observations used to estimate the regression. Standard errors are clustered at the primary sampling unit level.