Primetime Satan: Fear-Based Media Exposure, Moral Panic, and Electoral Behavior

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Abstract

We exploit the 1983-1995 Satanic Panic as a natural experiment to identify the political effects of fear-based media exposure. Using two independent proxies for panic exposure across 176 designated market areas, predetermined NBC affiliate delivery strength, and geographic proximity to satanic ritual abuse case epicenters, we find that both predict excess Republican presidential vote-share gains of 0.63-0.96 percentage points in 1988, relative to the pre-panic trend. The two instruments have opposite demographic profiles, making single-confound explanations implausible. The case proximity effect fully reverts by 1992, consistent with credibility collapse after the McMartin acquittals and the FBI's 1992 debunking report; the NBC effect decays more slowly, consistent with the absence of a localized corrective in markets that received the panic through national television. The convergence of two instruments with distinct demographic profiles rules out single-confound explanations and identifies fear-based entertainment media as a direct persuasion channel, operating independently of institutional religious infrastructure.

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1 Introduction

Can ideologically loaded entertainment media shift political behavior? We answer this question by studying the 1980s Satanic Panic—a moral panic that swept the United States from 1983 to roughly 1992—when sensational allegations of Satanic Ritual Abuse (SRA) in day-care centers were amplified by infotainment television. We find that this media coverage was channeled into measurable conservative political mobilization.

The Satanic Panic offers a rare natural experiment for identifying the political effects of entertainment media. Two features distinguish it from other settings. First, the panic's content was transmitted primarily through entertainment-adjacent media rather than traditional news, bridging the literatures on news persuasion [DellaVigna and Kaplan, 2007] and entertainment-driven political change [Durante et al., 2019]. Second, the panic had a clear beginning—a public collapse, acquittals, FBI debunking, appellate reversals—and a documentable end, enabling us to trace not just the onset but also the reversal of media-induced political preferences.

We explore two independent sources of variation to Satanic Panic exposure across 176 Designated Market Areas (DMAs) [1]. The first is geographic proximity to SRA prosecution epicenters. We geocode 36 major SRA criminal prosecutions from a systematic review of legal and academic sources [de Young, 2004, Nathan and Snedeker, 1995b, Frankfurter, 2006]. DMAs closer to active cases experienced more intense local exposure—sustained media coverage of arrests, trials, and community mobilization. The second is the NBC affiliate delivery index, a pre-determined measure of NBC signal strength from Arbitron's 1987 market survey.

[1] 29 markets were excluded due to OCR, mistmatches with vote data, or typesetting errors in the original 1988 TV Section Yearbook, more in Appendix A

Markets with stronger NBC delivery had higher viewership of the single most-watched piece of Satanic Panic media: Geraldo Rivera's "Devil Worship: Exposing Satan's Underground." An NBC primetime network special that aired on October 25, 1988—two weeks before the presidential election—reached approximately 20 million households.

In a joint continuous-treatment difference-in-differences specification with 1980 Census controls and region fixed effects, both instruments predict increased Republican two-party presidential vote share in the 1984–1988 treatment window. Case proximity yields $\beta =$ $=1.031(SE = 0.318, Conley500mi)$: DMAs closer to SRA cases shifted more Republican. NBC delivery yields $\beta = 0.019(SE = 0.007)$: markets with stronger NBC signal shifted more Republican. Neither instrument shows pre-trends in the 1976–1980 or 1980–1984 windows conditional on controls.

The paper's cleanest theoretical result is a complete round-trip in case proximity effects. The cumulative 1980–1992 coefficient is indistinguishable from zero ($\beta = 0.233, SE = 0.456$): DMAs near SRA cases gained Republican vote share during the panic, then returned to their pre-panic baseline by 1992. This zero cumulative effect simultaneously rules out permanent persuasion (which would leave a positive residual), backlash (which would overshoot below baseline), and demographic sorting as the primary driver (which would be permanent, not reversible). The only model consistent with these data is transient salience: the panic temporarily activated threat-based political preferences that dissipated when the panic's credibility collapsed with the McMartin acquittals (1990) and the FBI's Lanning Report (1992).

The convergence of two instruments with distinct demographic profiles is the core identification argument. NBC delivery is orthogonal to urbanization trends by construction—affiliate strength was determined by transmitter placement and franchise licensing in the 1950s–70s—while case proximity is correlated with the great sort, the structural transformation of American political geography that accelerated through the 1990s. A reviewer who believes NBC captures rural conservatism must explain why case proximity, correlated with

urban and educated coastal metros, produces the same result.

This paper makes three contributions. First, it provides what we believe is the first quasi-experimental estimate of the causal political effects of a moral panic, using two independent proxies with complementary identification profiles. Second, it documents a complete round-trip in media-induced political preferences: a fear-based shock that fully reversed once the underlying threat narrative was publicly discredited, in contrast to the persistent effects documented by Alesina and Giuliano [2011]. Third, it identifies fear-based moral content as a distinct channel through which entertainment television affects political behavior, separate from the cognitive displacement mechanism in Durante et al. [2019] and the partisan news mechanism in DellaVigna and Kaplan [2007]: the Geraldo special was not displacing political information, nor was it partisan news; it was supplying fear.

The paper most closely follows DellaVigna and Kaplan [2007], who regress changes in Republican vote share on Fox News availability and validate with pre-period placebos. We generalize their design to continuous treatment intensity and multiple periods, following the framework of Callaway and Sant'Anna [2021]. Our use legacy broadcast infrastructure as a source of identifying variation parallels Durante et al. [2019], who exploit variation in Mediaset signal strength, and Enikolopov et al. [2011], who use NTV signal availability in Russia.

2 Historical Background

2.1 The Satanic Panic: Timeline and Cases

The Satanic Panic began in September 1983 with the arrest of a teacher at the McMartin Preschool in Manhattan Beach, California, on allegations of child sexual abuse. Over the following months, the accusations expanded to include other staff members and claims of ritual Satanic abuse. The McMartin case would become the longest and most expensive criminal trial in American history, ending in complete acquittals in 1990 after no physical

evidence of abuse was ever found.

McMartin was neither the first nor the last such case. Table 1 lists the major SRA prosecution epicenters used in our analysis. Jordan, MN, erupted in 1984; Fells Acres Day School in Malden, MA, and Wee Care Nursery School in Maplewood, NJ, followed in 1984-1985; Country Walk Babysitting Service in Miami, FL, in 1985; and Little Rascals Day Care in Edenton, NC, in 1989. The cases shared a common pattern: initial accusations, rapid escalation to ritualistic abuse claims, intensive prosecution, and eventual collapse on appeal or at trial. The geographic diffusion of these cases from coastal California to rural North Carolina is one source of identifying variation in our design.

Table 1: Major SRA Prosecution Epicenters

Case	Location	Year	Outcome
McMartin Preschool	Manhattan Beach, CA	1983	Acquittals (1990)
Jordan, MN	Scott County, MN	1984	Charges dropped
Fells Acres	Malden, MA	1984	Convictions reversed
Wee Care Nursery	Maplewood, NJ	1985	Conviction reversed (1994)
Country Walk	Miami, FL	1985	Conviction affirmed[a]
Little Rascals	Edenton, NC	1989	Convictions reversed (1995)

† Sources: Victor [1993], Richardson et al. [1991], Hughes [2017].

[a] Country Walk defendant Frank Fuster remains incarcerated; the case is disputed.

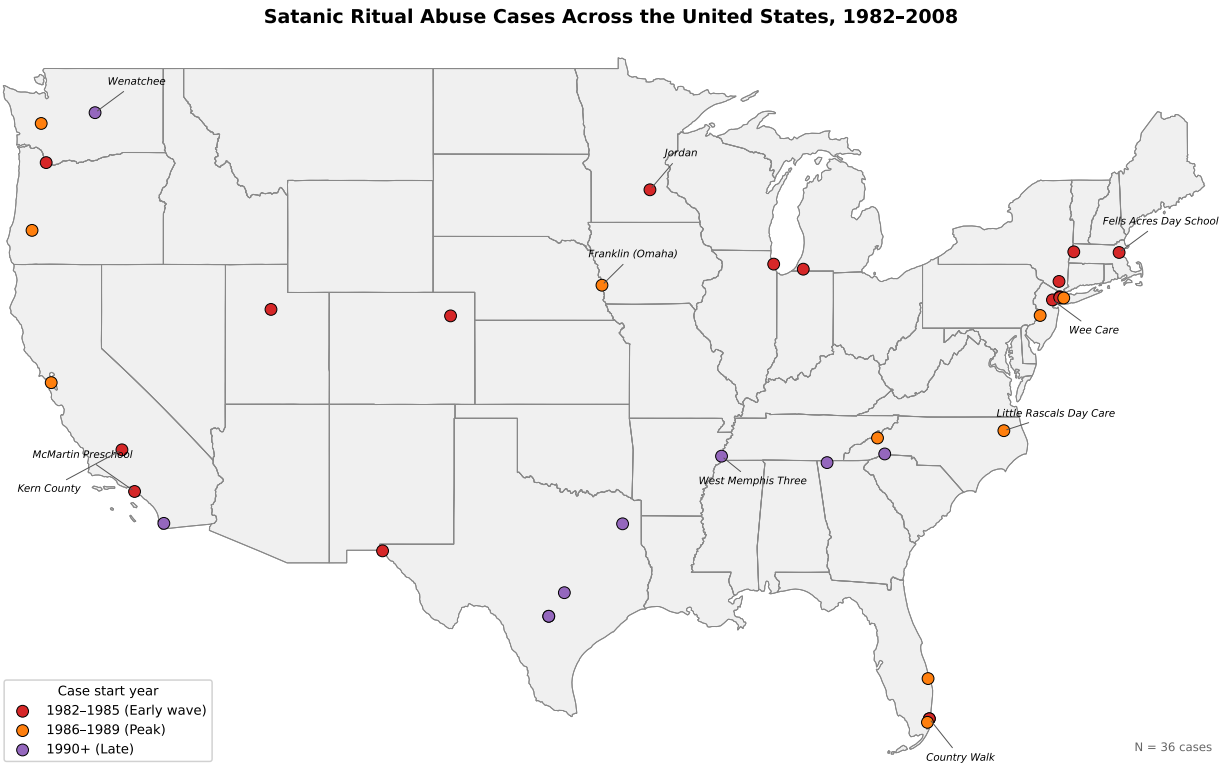

Figure 1: Satanic Ritual Abuse Cases Across the United States, 1982–2008

Notes: $N = 36$ hand-coded SRA prosecutions. Color encodes case start year: red = early wave (1982-1985), orange = peak (1986–1988), purple = late (1990+). Named cases indicate the six epicenters used in the proximity instrument. The geographic distribution spans all Census regions and is not concentrated in any single political geography, ruling out a simple regional story. Late-wave cases (1989+) are excluded from the treatment instrument as they post-date the 1988 election.

2.2 Media Amplification and the Infotainment Channel

The Satanic Panic's political reach depended critically on the media ecosystem that amplified it. Using the Vanderbilt Television News Archive, we identified 148 evening news segments on major networks (NBC, ABC, CBS) covering SRA allegations between 1980 and 1996. Figure 2 plots this coverage alongside key case events.

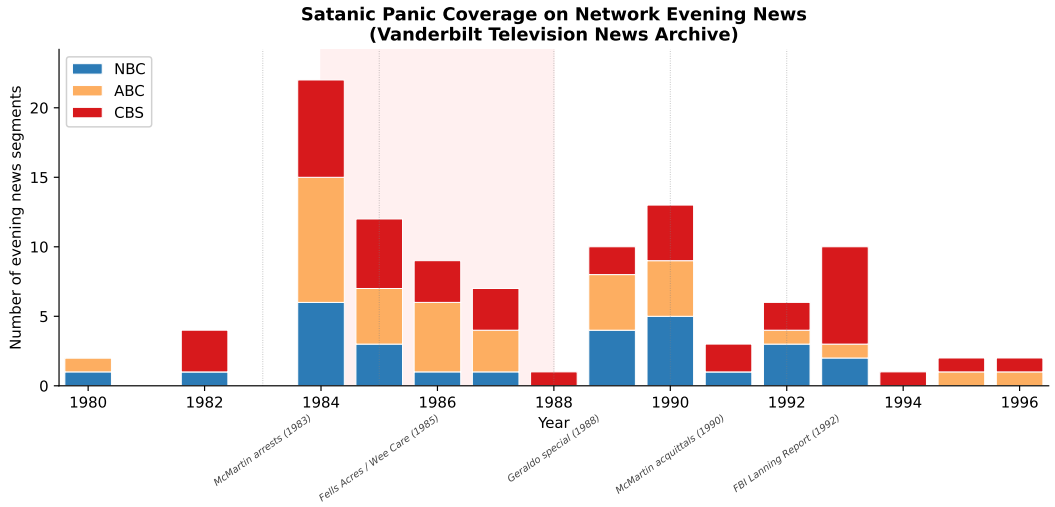

Figure 2: Satanic Panic Coverage on Network Evening News, 1980–1996

Note: 148 segments from the Vanderbilt Television News Archive (NBC, ABC, CBS). Evening news coverage peaked in 1984 and was near zero by the November 1988 election.

A critical observation from Figure 2 is that evening news coverage was largely over by 1988. NBC actually had the *fewest* evening news segments of the three networks (26 vs. 33 for ABC and 39 for CBS). The panic's reach into the 1988 election cycle came not from the evening news but from infotainment programming: talk shows (Geraldo, Donahue, Sally Jesse Raphael), made-for-TV movies, and — most consequentially — Geraldo Rivera's primetime network special.

On October 25, 1988, two weeks before the presidential election, NBC broadcast "Devil Worship: Exposing Satan's Underground," a two-hour primetime special hosted by Rivera. The broadcast ranked fifth in Nielsen ratings for the week—behind only *The Cosby Show*, *Cheers*, *A Different World*, and *60 Minutes*—with a 21.9 rating and 33 share, drawing approximately 19–20 million viewers and making it the highest-rated two-hour documentary in television history at the time.[2] Affiliate clearance was near-universal: of approximately 200 NBC affiliates, only four preempted the broadcast—KUTV Salt Lake City, stations in

[2]Advertiser demand collapsed in inverse proportion to audience size. NBC received roughly $2 million in order cancellations and had to cut 30-second spot prices to $75,000 to retain sponsors. The Dukakis campaign committee filled the last five minutes of the broadcast. NBC Entertainment President Brandon Tartikoff had considered canceling the special as late as the morning of broadcast and later said he regretted scheduling it at 8 p.m., when younger viewers were watching. *Broadcasting Magazine*, October 31, 1988.

Colorado Springs and Bristol, VA, and WAVE Louisville (which reversed its decision after receiving 2,000 protest calls and a bomb threat).[3] NBC also received 495 unfavorable and 290 favorable viewer responses, a roughly 1.7-to-1 ratio against the broadcast.

The critical analytical distinction is between this broadcast and the rest of the panic's television infrastructure. "Devil Worship" was an NBC network special: NBC headquarters fed the program simultaneously to all affiliates, and the NBC delivery index captures how many households in each market could receive that signal. Everything else—Rivera's daily syndicated talk show, *Donahue*, *Oprah*, *Sally Jesse Raphael*, Rivera's earlier primetime specials—was distributed through syndication, meaning Tribune Entertainment or the relevant distributor sold clearances one station at a time to any buyer regardless of network affiliation. The daily Geraldo show launched in September 1987 on 97 markets covering 73% of U.S. households and expanded to approximately 120 stations by January 1988; its clearances spanned all network affiliations, including Fox stations (WNYW New York, WFLD Chicago, WTTG Washington), CBS O&Os (KCBS Los Angeles, WAGA Atlanta), and independent stations (WPHL Philadelphia, KCPQ Seattle). Rivera's earlier primetime specials—such as *Sons of Scarface* (August 17, 1987), which cleared 157 stations covering more than 90% of the country—were similarly distributed by Tribune Entertainment on a station-by-station barter basis, not as NBC network broadcasts. [4] Because syndicated clearances are orthogonal to NBC affiliation, the NBC delivery index cannot capture exposure to any of this content. The NBC-specific result—ABC and CBS delivery indices are both null while NBC predicts a Republican shift—is therefore consistent with the mechanism being the October 1988 primetime network special, not the broader ecosystem of syndicated Satanic Panic programming that reached markets through all network affiliations equally.

[3] *Broadcasting Magazine*, October 31, 1988. WAVE's preemption was not content-based: the ABC affiliate in Louisville had just acquired Rivera's daily syndicated show, and WAVE declined to carry the NBC special to avoid promoting a competitor's programming asset. It reversed at the last minute under public pressure.

[4] *Broadcasting Magazine*, September 21, 1987 (syndication scorecard); June 29, 1987 (Sons of Scarface clearance data); November 21, 1988 (market-by-market clearance ad).

2.3 Political Context and Collapse

The Satanic Panic did not emerge in a political vacuum. Hughes [2017] argues that the panic and the New Right conservative media ecosystem were co-constitutive: the same infotainment infrastructure that amplified family-threat narratives was deeply entangled with the Reagan-era mobilization of cultural conservatism. The McMartin case was framed as evidence that liberal permissiveness had failed to protect children; the cure, implicitly, was a restoration of traditional values and stronger authority.

The panic's collapse was equally abrupt. McMartin's acquittal in January 1990 received extensive national coverage. In 1992, FBI Special Agent Kenneth Lanning, a leading expert on child exploitation, published a report concluding that after years of investigation, no credible evidence of organized Satanic crime had been found anywhere in the country [Lanning, 1992]. Major convictions were reversed on appeal through the early to mid-1990s as courts reconsidered the suggestive interviewing techniques used to elicit children's disclosures. By 1995, the academic consensus had shifted decisively: the panic was recognized as a case of mass hysteria [Victor, 1993].

3 Related Literature

3.1 Media and Political Behavior

The influence of mass media on political behavior is a central question in political science. Bartels [1993] argues that research on media influence on politics has been notoriously difficult to test due to measurement error. Given the stability of opinions among the public, Bartels argues, new information offered by the mass media must be highly distinctive in order to have an effect on political behavior.

Research on media influence in political behavior has often been organized into three broad traditions regarding the magnitude of media effects. Early studies in the mid-twentieth

century advanced the *minimal effects* perspective, arguing that media have a limited ability to change political attitudes because individuals interpret information through preexisting partisan loyalties and social networks. Classic studies such as *The People's Choice* by Lazarsfeld et al. [1944], as well as *Personal Influence* by Katz and Lazarsfeld [1955], emphasized mechanisms such as selective exposure and interpersonal influence, concluding that campaigns and media rarely convert voters but instead reinforce existing preferences.

Beginning in the 1970s and 1980s, however, scholars increasingly identified stronger media influences. Research on agenda setting, most famously developed by McCombs and Shaw [1972], showed that media powerfully shape which issues citizens consider politically important. Subsequent work on framing and priming further suggested that media can influence how individuals interpret political events and evaluate political actors. A third wave of scholarship, often described as the textitconditional or *moderate effects* perspective, argues that media influence is substantial but varies across contexts, institutions, and audiences. Scholars such as Prior [2007], Iyengar and Kinder [1987], and Bartels [1993] emphasize that media effects depend on factors such as political interest, media choice environments, and partisan predispositions. In this view, media neither determine political attitudes nor remain politically irrelevant; instead, their influence is conditional on individual and institutional contexts.

Within traditions, the moderate effects perspective requires specific conditions. In this way, the identification challenge is isolating exogenous variation in media exposure. A productive approach exploits geographic rollout of broadcast infrastructure: Strömberg [2004] shows that radio penetration in the New Deal era increased federal spending in counties where more residents owned receivers;Gentzkow [2006] finds that television entry reduced voter turnout by crowding out newspaper readership. Closer to our design, Campante and Hojman [2013] uses the introduction of broadcast TV across US counties to show that television exposure reduced the ideological extremism of House representatives. Our NBC delivery index follows this tradition, using legacy transmitter placements as a predetermined variation

in reception strength.

DellaVigna and Kaplan [2007] show that Fox News entry into cable markets increased Republican vote share by 0.4-0.7 percentage points, identifying persuasion as the mechanism. Enikolopov et al. [2011] find that independent television in Russia increased opposition voting by 8.9 percentage points, exploiting idiosyncratic signal availability. Two studies show that broadcast content can produce large and durable political effects beyond electoral politics: Yanagizawa-Drott [2014] shows that radio propaganda directly increased participation in the Rwandan genocide; Adena et al. [2015] find that Nazi radio broadcasts raised electoral support for the NSDAP in pre-war Germany while suppressing it in areas with prior negative exposure to regime messaging. Snyder and Strömberg [2010] show that press coverage increases political accountability, while Gentzkow and Shapiro [2010] examine how media slant responds to audience demand.

Most relevant to our paper is Durante et al. [2019], who show that exposure to Berlusconi's entertainment television network (Mediaset) in Italy shifted vote shares toward populist parties by approximately 1.5 percentage points. Their mechanism is cognitive degradation: entertainment television crowds out informative content, reducing citizens' capacity for political judgment. Our paper identifies a distinct mechanism, fear-based moral content, and documents effects operating through a channel orthogonal to the cognitive degradation hypothesis. The Geraldo special was watched attentively by 19 million people; it did not displace political information so much as supply a particular kind of it.

3.2 Moral Panics and Political Mobilization

Cohen [1972] introduced the concept of moral panic as a sociological phenomenon in which a group, condition, or episode becomes defined as a threat to societal values; Goode and Ben-Yehuda [1994] elaborated the framework into a rigorous set of criteria—concern, hostility, consensus, disproportionality, and volatility that the Satanic Panic satisfies. Victor [1993] provides the definitive account of the Satanic Panic as a contemporary legend. Hughes [2017]

argues that the panic was institutionally entangled with the New Right project, with tabloid media serving as both amplifier and co-producer of conservative threat narratives. Beckett [2001] shows more generally that crime framing systematically benefits conservative political actors.

3.3 Religion and Political Mobilization

The relationship between evangelical Christianity and the Republican Party is one of the central stories of late-twentieth-century American politics. Layman [2001] documents the timing and mechanisms of Evangelical partisan realignment, showing that the process was largely complete by the early 1980s through the Moral Majority and the Reagan coalition. This literature is relevant to our null result: we find no evidence that evangelical congregation density amplified the NBC treatment effect, suggesting that by 1984 the Christian Right's political mobilization was already at or near the ceiling, and the marginal contribution of the panic operated through mass media rather than institutional religious networks.

A related explanation of religious political behavior emphasizes *contextual religious threat*, a mechanism through which individuals' political attitudes respond not only to their own religious identity but also to the religious composition of the surrounding social environment. Campbell [2006] argues that members of religious groups may perceive a symbolic or cultural threat when they live in contexts where secular identities are more prevalent. In particular, Evangelical Protestants surrounded by a higher proportion of religiously unaffiliated individuals may perceive greater cultural conflict over moral norms and public values. This perceived threat can activate group identity and increase political cohesion, leading individuals to support political actors who are seen as defending traditional moral frameworks.

Empirically, this argument is tested through cross-level interaction models in which individual religious identity is interacted with contextual measures of religious composition. For example, Campbell [2006] shows that Evangelicals are more likely to support Republican

candidates when they reside in environments with a larger proportion of religious "nones" (unaffiliated), whereas the same contextual variable has little or no effect among individuals who are themselves nonreligious. In other words, the political consequences of secular environments are asymmetric: exposure to nonreligious contexts mobilizes religious conservatives, but it does not produce equivalent counter-mobilization among secular individuals. In that regard, contextual threat and group boundary activation are operationalized empirically through interaction terms in statistical models.

3.4 Persistence of Political Preferences

A substantial literature examines whether political shocks produce permanent attitude change Alesina and Giuliano [2011] show that exposure to communist institutions left lasting imprints on East Germans' economic preferences relative to West Germans, with effects persisting decades after reunification. The mechanism they identify is deep socialization under a distinct institutional regime rather than transient salience. Our paper contributes a contrasting type of evidence: a complete round-trip in media-induced preferences that rules out permanent persuasion and demographic sorting as primary mechanisms. The full reversion of the case proximity effect to zero by 1992 provides evidence for the transient salience model.

Early research on persuasion suggested that the relationship between message exposure and attitude change is not linear. Experimental work by Petty and Cacioppo in the late 1970s showed that moderate repetition of a persuasive message tends to increase persuasion because repeated exposure facilitates cognitive processing and recall of the arguments. However, when repetition becomes excessive, individuals may experience boredom, irritation, or counter-arguing, which reduces the effectiveness of the communication. Thus, persuasive impact tends to follow an inverted pattern: initial repetitions strengthen persuasion, but after a certain threshold, additional exposure produces diminishing returns and may even generate resistance to the message Petty and Cacioppo [1979].

The effects of persuasive communication may fade over time for different reasons. One

possibility, emphasized in early research on message repetition, is that excessive exposure leads to fatigue or counterarguing, thereby reducing the persuasive impact of the message Petty and Cacioppo [1979]. However, persuasion may also decay for a different reason: the message simply becomes less salient. As time passes, competing information and new issues displace the original message from individuals' immediate cognitive environment. In this case, the decline in persuasion does not reflect resistance or saturation, but rather the gradual loss of cognitive accessibility and relevance. The persuasive effect is therefore transient because the message ceases to be salient when individuals evaluate political or social choices.

Recent research on media effects provides additional theoretical mechanisms that help explain why such persuasive effects may be short-lived. One mechanism refers to the immediate activation of a preexisting schema following exposure to certain content, which subsequently influences issue interpretations and overall assessments of the topic Shehata et al. [2021]. A second mechanism relates to the level of abstraction in cognitive processing. When individuals rely on more concrete representations—what communication scholars describe as situation models rather than more abstract mental or cognitive models—the resulting effects tend to be short-term and narrow in scope Shehata et al. [2021]. In this regard, long-term effects are less likely to occur because they require greater cognitive elaboration.

The combination of news novelty, message repetition, and the absence of strong prior beliefs may temporarily slow this process of decay. However, in contexts characterized by low cognitive engagement and high concreteness, situation models are more likely to dominate. Under these conditions, persuasive effects are expected to be transient, fading relatively quickly as the salience of the original message declines.

4 Data

NBC Affiliate Delivery Index. Our first treatment variable is the NBC Prime-Time Delivery Index from the 1988 Broadcasting/Cablecasting Yearbook (Arbitron Television, May 1987 Network Program Group Analysis, pp. C-222 to C-224). The table—titled "How Network Delivery Varies by Market"—reports each market's share of national prime-time audiences for ABC, CBS, and NBC across all 213 ADI markets, along with a delivery index scaled so that 100 equals proportional delivery relative to each market's share of national television households. Full details on digitization, cleaning, and sample construction are in Appendix A.

The NBC index varies from 34 to 197 (mean 102.4, SD 24.3) in our analysis sample of 176 DMAs. The top markets are small Southern and rural markets—Albany, GA; Laurel-Hattiesburg, MS; Alexandria, LA; Zanesville, OH; Lima, OH—consistent with dominant single-affiliate markets in flat-terrain areas with strong over-the-air reception. The bottom markets are large urban markets where cable penetration was highest and over-the-air delivery was weakest.

What the NBC index measures. The index captures a market's passive NBC reception capacity: the fraction of television households that could, in principle, receive NBC prime-time programming through their existing antenna or cable infrastructure, expressed as a ratio to the market's share of national households and scaled to 100. It does not measure viewership or ratings. The variation is driven primarily by cable penetration rates, with secondary contributions from over-the-air signal geography and the number of competing local affiliates. High-cable markets (large coastal cities) show low index values; low-cable rural markets show high values.

The exposure interpretation follows directly: a household in a high-index DMA was more likely to have NBC available as one of its few receivable channels. In 1988, the average American household received approximately 10 channels; in rural markets, this was often

fewer than 5. When NBC broadcast the "Devil Worship" special on October 25, 1988, a household in a 97th percentile market (Albany, GA) was substantially more likely to encounter it, either by actively tuning in or by passive exposure while channel-surfing, than a household in a low-index urban market with 20 cable alternatives.

The identification strategy relies on this index being predetermined. NBC affiliate delivery strength reflects legacy infrastructure decisions, transmitter placement, franchise licensing, and network affiliation agreements, made in the 50s-70s, decades before both the Satanic Panic and the demographic sorting of the 1990s. This infrastructure variation is, by construction, orthogonal to contemporary political and demographic confounders. Section 5.2 provides formal validation.

The most plausible mechanism linking NBC delivery to the 1988 Republican shift is the "Devil Worship" special of October 25, 1988. Markets with stronger NBC delivery had more households able to receive this broadcast. To confirm near-universal broadcast, we collected TV listings from 276 newspapers across 118 DMA markets spanning 45 states: every listing confirms the special aired in the NBC 8-10 p.m. slot on October 25, 1988, with zero preemptions found. Combined with same-state and adjacent-state extrapolation, this covers 165 of 176 DMAs (94%) in our primary sample; the 11 uncovered DMAs are all bordered by confirmed states. Of the three known preemptions (Colorado Springs, CO; Bristol, VA; and Salt Lake City, UT — WAVE Louisville reversed after viewer pressure), all are in confirmed states. Full documentation is in Appendix A.4. These preemptions, if anything, attenuate our estimates: markets that preempted received zero treatment but are coded as having NBC delivery, biasing the coefficient toward zero.

The syndication evidence sharpens this interpretation considerably. The rest of the panic's television infrastructure—Rivera's daily talk show, *Donahue*, *Oprah*, *Sally Jesse Raphael*, and Rivera's earlier primetime specials—was distributed through syndication: sold station-by-station to any buyer regardless of network affiliation. The daily Geraldo show cleared approximately 120 stations across Fox, CBS, and independent affiliates; it was not

an NBC-distributed product. Because syndicated exposure is orthogonal to NBC affiliate delivery, the NBC delivery index cannot capture exposure to any of this content. The only Satanic Panic television product that maps to the NBC delivery index is the October 1988 network special. This makes the ABC and CBS falsification results particularly sharp: ABC and CBS delivery indices are essentially uncorrelated with NBC's ($r \approx -0.11$ to -0.12), and neither predicts the 1984-1988 Republican shift. General panic programming reached markets through all network affiliations via syndication. NBC-specific delivery predicts a Republican shift precisely because there was one major panic broadcast that was exclusively NBC: "Devil Worship." Full falsification results are in Appendix B.

SRA Case Proximity Our secondary identification strategy is geographic proximity to SRA prosecution epicenters, measured as the log of the minimum distance (miles) from each DMA's population-weighted centroid to the nearest case epicenter in each election year. DMA centroids are constructed from county centroids (Census Bureau 2020 Gazetteer) using the Patton county-DMA crosswalk, weighted by 1980 Census population. We hand-coded 36 major SRA criminal prosecution cases that generated significant community attention: multi-defendant indictments, televised trials, and sustained local media coverage. Table 1 shows the six most popular cases. The full details of the construction of these cases are in the Appendix A

We use the log transformation because the relevant exposure gradient is multiplicative: moving from 50 to 100 miles matters less than moving from 5 to 10 miles. The resulting variable ranges from approximately 2.0 (closest DMAs, near case epicenters) to 7.5 (most distant). Closer DMAs received more intensive local media coverage, had more direct community exposure to the panic's social disruption, and served as anchors for regional infotainment coverage.

Presidential vote Our outcome is the first-difference in Republican two-party presidential vote share by DMA ($\Delta\text{RepShare}_{d,t\to t+4}$), constructed from Daily Kos Elections DMA-level

presidential returns for 1976-2016. We focus on the following windows: pre-pre-trend (1976-1980), pre-trend (1980-1984), treatment (1984-1988), reversal (1988-1992), and cumulative (1980-1992). Using vote share changes rather than levels eliminates all time-invariant DMA characteristics from the identification, as discussed in Section 5.

Evangelical Church Infrastructure To test whether evangelical congregation density amplified the panic's political effects, we construct a DMA-level evangelical adherent share from the 1980 Churches and Church Membership surveys (Association of Religion Data Archives). We aggregate five core denominations identified by Hughes [2017] as institutionally connected to Satanic Panic discourse: Southern Baptist Convention (84% of evangelical adherents in our sample), Assemblies of God (8%), Church of the Nazarene (5%), Church of God Cleveland TN (2%), and Foursquare Gospel (1%). The measure is adherents per 100 population, aggregated to the DMA level using population-weighted county means.

Evangelical density is highly right-skewed: the median DMA has 6.0% adherent rate, but the distribution ranges from 0.4% (Boston) to 42.5% (Knoxville), with an IQR of 2.3-20.4%. Bottom DMAs are northeastern (Catholic/mainline territory); top DMAs are southern/border South. Evangelical density is essentially orthogonal to the NBC index ($r = 0.05$) but moderately correlated with log case proximity ($r = 0.42$), because SRA cases occurred in coastal metros where evangelical density was lower.

Controls All specifications include 1980 Census demographics aggregated from county to DMA via the Patton crosswalk: percentage urban, percentage white, percentage with four-year college degree, and log median household income. We include Census region fixed effects rather than state fixed effects because state fixed effects would absorb most of the identifying variation in the case proximity variable (cases occurred in specific states), while providing inadequate precision given an average of 4 DMAs per state. For robustness, specifications using 1990 Census demographics are also reported, with appropriate caveats about post-treatment conditioning for the proximity specification.

5 Empirical Strategy

5.1 Main Specification

We estimate a continuous-treatment difference-in-differences design [Callaway and Sant'Anna, 2021] in which the same cross-sectional regression is estimated across multiple election windows.

The main specification is:

$$\Delta\text{RepShare}_{d,t\to t+4} = \alpha + \beta_1 \cdot \text{LogDist}_d + \beta_2 \cdot \text{NBC}_d + X_d'\gamma + \delta_r + \varepsilon_d \tag{1}$$

where d indexes DMAs, δ_r are Census region fixed effects, and X_d is the vector of 1980 Census controls. The joint specification (β_1 and β_2 estimated simultaneously) is the lead result. We also report each instrument separately.

This is a continuous-treatment difference-in-differences design in the spirit of Callaway and Sant'Anna [2021] and Goodman-Bacon [2021]: the same cross-sectional regression is run across multiple election windows, or equivalently stacked as a panel event study with treatment $\times$ period interactions. The identifying logic is that if treatment has no causal effect, markets with stronger NBC delivery (or closer SRA proximity) should show no systematically different change in Republican voting in any period. Pre-trend tests in Section 6.1 directly assess this assumption.

First-differencing eliminates all time-invariant DMA characteristics. The remaining concern is that the treatment variables are correlated with time-varying confounders. This is where the dual-instrument strategy is most valuable: the two instruments have opposite correlations with the primary time-varying confounder (demographic sorting), so a single confounding explanation must simultaneously account for both, which is implausible.

5.2 Validity of the Identification Strategy

Table 2 reports the relationship between each treatment variable and the 1980–1990 change in four demographic outcomes, controlling for Census region fixed effects. NBC delivery is clean: it does not predict changes in college share, urban share, or log income. It predicts a small positive change in white share ($t = +2.24$), but this demographic shift has no independent effect on the partisan outcome when added as a control ($t = 0.44$), and controlling for it leaves the NBC coefficient essentially unchanged.

Case proximity is substantially correlated with demographic change: it strongly predicts the 1980-1990 increase in college share ($t = -4.27$) and log income ($t = -5.93$), confirming that SRA cases occurred disproportionately in the places at the leading edge of the great sort. This is a limitation of the proximity instrument, addressed in two ways. First, we use pre-treatment (1980) controls only for the proximity specifications, avoiding post-treatment conditioning. Second, the convergence of the proximity and NBC results despite their opposite demographic profiles is itself evidence against sorting-based confounding.

Table 2: Validity of the identification strategy: Predicting 1980–1990 Demographic Change

	t-statistic	
Demographic change (1980–1990)	NBC index	Log distance
Δ College share	-1.05	-4.27^{***}
Δ White share	$+2.24^{**}$	$+1.29$
Δ Urban share	-0.70	-2.03^{**}
Δ Log median income	-1.33	-5.93^{***}
NBC white-share test		
Δ White share $\rightarrow$ Δ Rep. share	$t = +0.44$	
NBC (controlling for Δ white)	coefficient unchanged	

Notes: Each cell reports the t-statistic from a regression of the demographic change variable on the treatment variable and Census region fixed effects ($N = 176$). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$. Log distance uses negative sign convention (higher = closer).

5.3 Event Study Specification

To validate pre-trends and characterize the full temporal dynamics of treatment effects, we estimate a panel event study:

$$\text{RepShare}_{d,t} = \alpha_d + \gamma_t + \sum_{t' \neq 1984} \beta_{t'} \cdot \text{Treatment}_d \times \mathbf{1}(t = t') + \sum_{t'} X_d' \delta_{t'} + \varepsilon_{d,t} \tag{2}$$

where α_d are DMA fixed effects, γ_t are election-year fixed effects, and the interaction coefficients $\beta_{t'}$ trace out the dynamic treatment effect in each election year relative to 1984 (the last pre-treatment year). Standard errors are clustered at the DMA level. We also report a first-difference event study where the outcome in each period is ΔRepShare, normalized to the 1980–1984 pre-treatment change.

5.4 The Round-Trip Test

A key theoretical result is the cumulative 1980-1992 effect. If the panic produced transient salience rather than permanent persuasion, the cumulative coefficient should be zero: the treatment effect should accumulate through 1988 and then fully reverse by 1992. We estimate:

$$\Delta\text{RepShare}_{d,1980\to1992} = \alpha + \beta \cdot \text{Treatment}_d + X_d'\gamma + \delta_r + \varepsilon_d \tag{3}$$

A significant positive β would indicate permanent persuasion or sorting. A significant negative β would indicate backlash. Zero is the prediction of the transient salience model. This test is cleanest for the case proximity instrument, where the reversal mechanism (specific cases being publicly discredited) is well-defined.

5.5 Evangelical Interaction

To test the institutional amplification hypothesis, we estimate:

$$\Delta\text{RepShare}_{d,84\to88} = \alpha + \beta_1 \cdot \text{NBC}_d^* + \beta_2 \cdot \text{Evang}_d^* + \beta_3 \cdot (\text{NBC}_d^* \times \text{Evang}_d^*) + X_d'\gamma + \delta_r + \varepsilon_d \tag{4}$$

where starred variables are standardized (mean zero, unit SD). Under the amplification hypothesis, $\beta_3 > 0$: evangelical infrastructure intensifies the media effect. Under the direct transmission hypothesis, $\beta_3 \approx 0$ and $\beta_1 > 0$: media operates directly, independently of institutional religious networks.

5.6 Standard Errors

We report OLS standard errors clustered at the DMA level as the baseline. To address spatial autocorrelation, we compute Conley [1999] spatial HAC standard errors at cutoff distances

of 100, 200, and 500 miles, using a Bartlett kernel. The Conley correction is implemented following the spatial weighting matrix approach: for each DMA pair (i, j), the weight is $w_{ij} = \max(0, 1 - d_{ij}/D)$ where d_{ij} is the haversine distance between DMA centroids and D is the bandwidth.

6 Results

6.1 Pre-trend Validation

Figure 3 presents the event study for both treatment variables, normalized to 1984. Both panels show clean flat pre-trends: for case proximity, the 1976 and 1980 coefficients are small and insignificant ($\hat{\beta} = +0.21$, SE $= 0.428$; $\hat{\beta} = -0.28$, SE $= 0.400$); for NBC, the pre-trend coefficients are equally negligible ($\hat{\beta} = -0.002$, SE $= 0.018$; $\hat{\beta} = +0.015$, SE $= 0.012$). In the first-difference specification (Figure 4), the 1980-1984 period is the reference and both treatment variables show pre-period estimates indistinguishable from zero.

Table 3 confirms these results. The identifying assumption of parallel pre-trends is supported by the data.

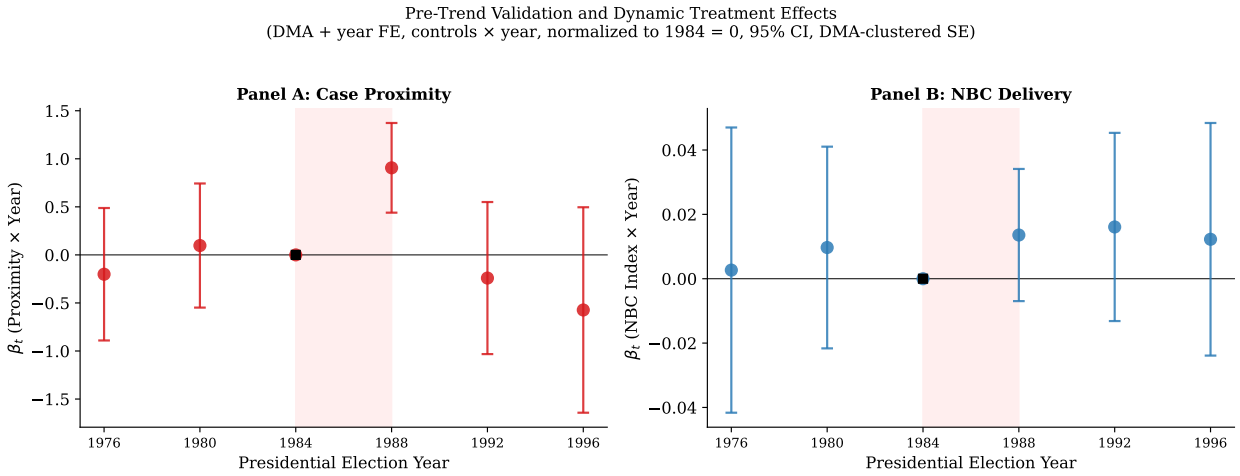

Figure 3: Event Study: Pre-trends and Dynamic Treatment Effects

Notes: Levels event study with DMA and year fixed effects, 1980 Census controls interacted with year, region × year indicators. Coefficients normalized to 1984 = 0. Panel A plots Proximity = − log Dist (higher values = closer to cases = more exposure). 95% confidence intervals with DMA-clustered standard errors.

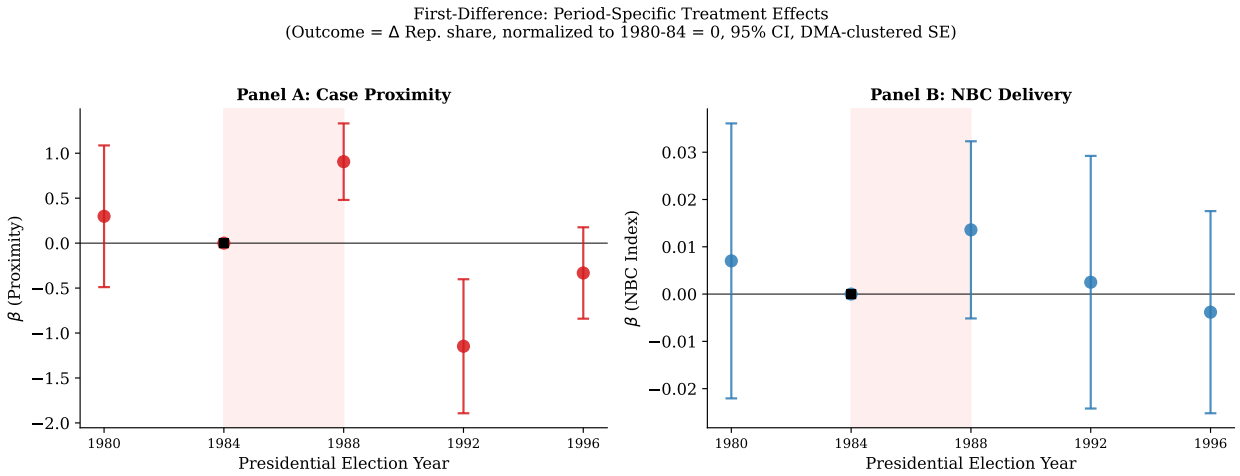

Figure 4: First-Difference Event Study: Period-Specific Treatment Effects

Notes: First-difference event study. Outcome is ΔRepublican share in each election period. Normalized to 1980–1984 = 0. 95% confidence intervals with DMA-clustered standard errors.

Table 3: Pre-trend Validation

	LogDist		NBC index	
Window	$\hat{\beta}$	SE	$\hat{\beta}$	SE
Pre-pre-trend (1976-1980)	+0.21	(0.428)	−0.002	(0.018)
Pre-trend (1980-1984)	−0.28	(0.400)	+0.015	(0.012)

Notes: OLS with 1980 Census controls and Census region fixed effects. $N = 176$. Standard errors (clustered by DMA) in parentheses. Neither treatment variable predicts pre-panic changes in Republican vote share; all coefficients are small and insignificant.

6.2 Binned Scatter: Visual Evidence

Figure 5 presents binned scatter plots of the two treatment variables against the 1984-1988 change in Republican vote share, residualized on 1980 Census controls and region fixed effects. Both relationships are visually clear and monotonic: closer proximity to SRA cases is associated with a larger Republican shift (Panel A), and higher NBC delivery is associated with a larger Republican shift (Panel B). The relationships are not driven by outliers: both panels show a consistent gradient across the full distribution of the treatment variables.

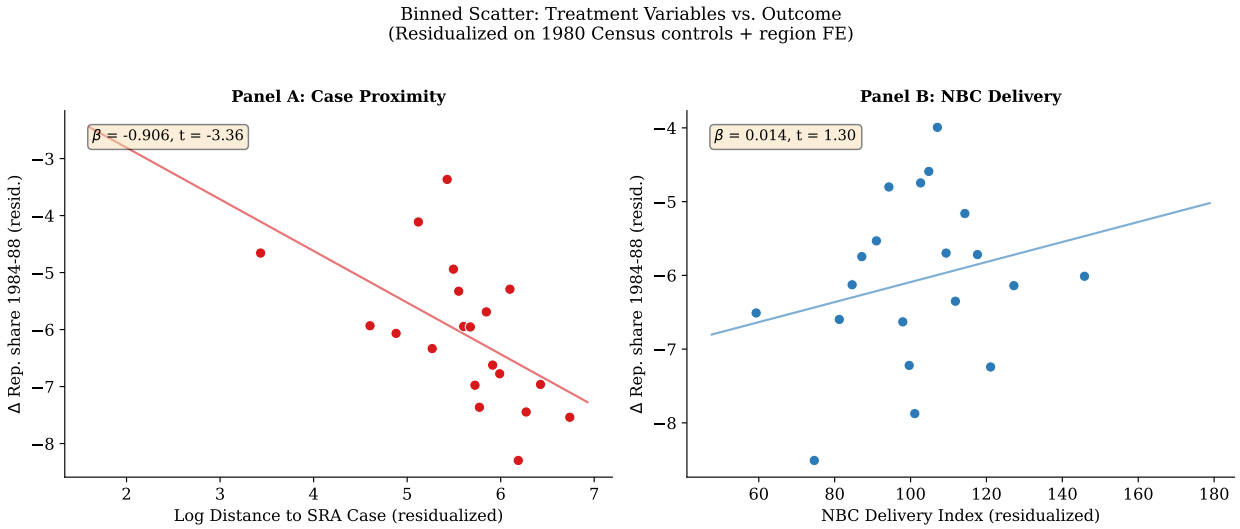

Figure 5: Binned Scatter: Treatment Variables vs. 1984–1988 Republican Vote Change

Notes: 20 quantile bins. Outcome and treatment both residualized on 1980 Census controls (pct_urban, pct_white, pct_college, log_income) and Census region fixed effects. OLS fit line with $\hat{\beta}$ and t-statistic displayed. Panel A plots log distance (negative direction on x-axis corresponds to closer proximity). The top-right outlier in Panel B is Albany, GA (NBC index = 197); results are robust to its exclusion.

6.3 NBC Delivery Index

Table 4 reports the NBC treatment specification across progressive control sets. The baseline estimate (1980 demographics and region fixed effects) yields $\hat{\beta} = 0.025$ (SE = 0.009). Adding 1990 demographics produces virtually identical estimates ($\hat{\beta} = 0.025$, SE = 0.008); including both 1980 and 1990 controls together gives $\hat{\beta} = 0.026$ (SE = 0.008). The stability of the NBC coefficient as contemporaneous demographic controls are added is the behavior expected from an instrument orthogonal to demographic change.

Table 4: Primary Result: NBC Delivery and 1984–1988 Republican Vote Change

	(1)	(2)	(3)
Controls	1980 only	1990 only	Both
NBC delivery index	0.025***	0.025***	0.026***
	(0.009)	(0.008)	(0.008)
Region FE	Yes	Yes	Yes
Observations	176	176	176

Notes: Dependent variable is ΔRepublican two-party vote share 1984–1988. Standard errors (clustered by DMA) in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$. 1980 controls: pct_urban, pct_white, pct_college, log_median_income. 1990 controls: same variables measured in 1990 Census.

The IQR effect size is 0.63 percentage points: moving from the 25th to the 75th percentile of NBC delivery is associated with a 0.63 pp larger Republican two-party vote share gain in 1988 presidential election (0.20 SD of the outcome). The 10th-90th percentile contrast is 1.39 pp (0.43 SD). These magnitudes reflect deviations from each DMA's underlying political trend rather than absolute vote shifts; the first-difference design differences out all time-invariant DMA characteristics, so the coefficient identifies the *excess* Republican shift in high-NBC markets over the 1984–1988 period relative to the national baseline.

Two important caveats of this interpretation. First, our outcome is a net change in two-party presidential vote share, which conflates three distinct mechanisms: mobilization of previously inactive Republican-leaning voters; genuine cross-party conversion; and differential demobilization of Democratic-leaning voters. The data cannot distinguish these channels. Second, the aggregate effect, while statistically precise and consistent across speci-

fications, is not large enough to have been decisive in a presidential election where Bush won by 7.7 percentage points nationally. The correct reading is that panic exposure produced a *temporary and proportional deviation in DMA-level partisan lean*: markets received more panic exposure shifted further Republican over and above the secular 1984-1988 trend, and this deviation reversed when the panic collapsed.

6.4 Case Proximity and Joint Specification

Table 5 reports the case proximity specification (Panel A) and the joint specification with both instruments (Panel B). With 1980 pre-treatment controls, case proximity yields $\hat{\beta} = -1.10$ (SE $= 0.282$). The effect survives flexible functional forms including quadratic controls and pairwise interactions of 1980 demographic variables ($\hat{\beta} = -1.08$, SE $= 0.302$). We use 1980 controls only for the proximity specification—1990 demographics are post-treatment and their inclusion would introduce potential bias.

Table 5: Case Proximity and Joint Specification: 1984–1988

	Panel A: Case Proximity		Panel B: Joint	
	(1)	(2)	(3)	(4)
	Baseline	Flexible	OLS	Conley 500mi
Log distance	-1.10^{***}	-1.08^{***}	-1.12^{***}	-1.12^{***}
	(0.282)	(0.302)	(0.276)	(0.266)
NBC index			0.026^{***}	0.026^{***}
			(0.008)	(0.008)
Controls	1980	1980+flex	1980	1980
Region FE	Yes	Yes	Yes	Yes
Observations	176	176	176	176

Notes: Dependent variable is ΔRepublican two-party vote share 1984–1988. Standard errors in parentheses: columns (1)–(3) cluster by DMA; column (4) uses Conley (1999) spatial HAC standard errors at 500-mile bandwidth. Flexible controls include quadratics and pairwise interactions of 1980 demographic variables. $^{***}p < 0.01$.

The IQR effect size for case proximity is 0.96 percentage points: moving from the 75th to the 25th percentile of log distance (closer to cases) increases Republican vote share by 0.96 pp (0.29 SD). The 10th-90th percentile contrast is 1.77 pp (0.54 SD).

In the joint specification (Panel B), both instruments survive simultaneously, both strengthening under Conley 500-mile spatial correction. The joint F-test rejects the null of no effect at $p < 0.001$.

6.5 Spatial Robustness: Conley Standard Errors

Table 6 reports all main results with Conley spatial HAC standard errors at 100, 200, and 500 mile bandwidths. The defining feature of these results is that Conley corrections consistently *reduce* standard errors relative to OLS—that is, they produce larger t-statistics rather than smaller ones. This indicates negatively spatially autocorrelated residuals (urban/rural alternation), meaning OLS standard errors were already slightly conservative. All main results survive at conventional significance thresholds at all bandwidths.

Table 6: Conley Spatial Standard Errors

Specification	OLS t	Conley 100mi	Conley 200mi	Conley 500mi
Pre-trend				
LogDist (1980-84)	−0.70	−0.88	−0.85	−0.79
Treatment (1984–88)				
LogDist (solo)	-3.90^{***}	-5.45^{***}	-4.89^{***}	-4.02^{***}
NBC (solo)	2.84^{***}	2.85^{***}	2.81^{***}	2.85^{***}
LogDist (joint)	-4.06^{***}	-5.55^{***}	-5.03^{***}	-4.21^{***}
NBC (joint)	3.07^{***}	3.12^{***}	3.11^{***}	3.26^{***}
Reversal (1988-92)				
LogDist	3.17^{***}	3.16^{***}	3.02^{***}	2.76^{***}
NBC	−0.63	−0.68	−0.76	−0.74
Cumulative (1980–92)				
NBC	2.16^{**}	2.12^{**}	2.25^{**}	2.81^{***}

Notes: All treatment specifications include 1980 Census controls and region fixed effects. Conley (1999) spatial HAC standard errors use a Bartlett kernel. The pattern of Conley t-statistics exceeding OLS t-statistics is consistent with negatively spatially autocorrelated residuals (urban/rural alternation). $^{***}p < 0.01$, $^{**}p < 0.05$.

6.6 The Round-Trip: Transient Salience

Figure 3 (Panel A) shows the defining pattern for case proximity: the effect rises sharply at 1988 ($\hat{\beta}_{1988} = -1.10$, SE $= 0.282$) and then reverts fully to zero by 1992. The cumulative 1980–1992 regression confirms this round-trip:

$$\Delta\text{RepShare}_{d,1980\to1992}: \quad \hat{\beta} = -0.18, \quad t = -0.36, \quad N = 176$$

High-proximity DMAs gained Republican vote share during the panic and then returned exactly to their pre-panic baseline—not below it (which would indicate backlash) and not above it (which would indicate permanent persuasion).

The zero cumulative effect simultaneously rules out three alternative explanations. Permanent persuasion would predict $\beta > 0$ in 1992; backlash would predict $\beta < 0$; demographic sorting, being permanent, could not produce a zero cumulative effect if it were the main driver of the treatment-window result. The only model consistent with these data is transient salience: the panic temporarily activated threat-based political preferences that dissipated when the panic's empirical basis collapsed following the McMartin acquittals and the FBI Lanning Report [Lanning, 1992].

6.7 Reversal and NBC Persistence

Table 7 reports the 1988-1992 reversal for the case proximity instrument across control specifications. With 1980 pre-treatment controls, the reversal is significant at the 1% level ($\hat{\beta} = +1.19$, SE $= 0.375$). The coefficient attenuates when 1990 Census demographics are added ($\hat{\beta} = +0.85$, SE $= 0.411$) and loses significance when both decades are included ($\hat{\beta} = +0.52$, SE $= 0.409$). This attenuation has two possible interpretations that the data cannot distinguish. First, 1990 demographics capture genuine confounding: coastal DMAs near SRA cases were already beginning to trend Democratic for reasons unrelated to the panic's collapse, and 1990 controls absorb this. Second, 1990 demographics are themselves

partly outcomes of the panic—if the panic induced demographic change in nearby communities, controlling for 1990 outcomes introduces collider bias. We report the full range of specifications and note both interpretations.

The NBC instrument shows no significant reversal (OLS $t = -0.63$, Conley 500mi $t = -0.74$). This is a noisy zero rather than a precise null: with $N = 176$ DMAs and a coefficient of -0.008, we cannot distinguish partial persistence from full reversal. The levels event study (Figure 3, Panel B) shows the NBC effect fading from +0.025 at 1988 to +0.018 at 1992 and +0.011 at 1996—partially persistent, not permanent. The cumulative 1980-1992 NBC coefficient is positive ($t = 2.16$, Conley 500mi $t = 2.81$), but this specification stacks the borderline pre-period contribution (+0.015, $t = 1.22$) onto the treatment effect. The honest claim is partial persistence with gradual decay, not permanent mobilization.

Table 7: Reversal: 1988–1992 Case Proximity Effect

	(A)	(B)	(C)
Controls	1980 only	1990 only	Both
Log distance (reversal)	+1.19***	+0.85**	+0.52
	(0.375)	(0.411)	(0.409)
Region FE	Yes	Yes	Yes
Observations	176	176	176

Notes: Dependent variable is ΔRepublican two-party vote share 1988-1992. Positive coefficient indicates reversal: closer DMAs that gained Republican share in 1988 losing it in 1992. Standard errors (clustered by DMA) in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$.

6.8 Robustness Checks

Influence diagnostics. Albany, GA (DMA 525) has the highest NBC delivery index in the sample (197), consistent with a small, geographically isolated market dominated by a legacy NBC affiliate with strong over-the-air reception (Cook's $D = 0.077$). Dropping Albany GA reduces the full-sample NBC t-statistic from 2.84 to 2.05, remaining significant at the $5\%level.Thecaseproximitycoefficientisentirelyunaffected(\hat{\beta} = -1.10$, SE unchanged), confirming the two instruments have independent leverage structures.

Dropping closest DMAs. Dropping the 10 DMAs closest to SRA case epicenters *strengthens* the case proximity effect ($\hat{\beta}$ rises from -1.10 to -1.22, SE $= 0.414$). Dropping the 20 closest produces $\hat{\beta} = -1.64$ (SE $= 0.524$). The effect is concentrated in the middle of the distance distribution, consistent with diffuse media-driven exposure rather than purely local contagion from case epicenters.

Recession controls. Adding state-level unemployment change as a control attenuates the treatment effect but does not eliminate it. The 1990-91 recession partly explains the Democratic shift in 1992, but Republican gains in the panic period survive.

Regional heterogeneity. No individual Census region shows a precisely estimated NBC effect in isolation (all coefficients insignificant at conventional levels: Northeast $N = 16$; Midwest $N = 49$; South $N = 76$; West $N = 35$), consistent with the effect being real but diffuse, requiring the full cross-regional sample for statistical detection.

6.9 Evangelical Interaction: A Null with Theoretical Content

Table 8 reports the evangelical density interaction analysis. The NBC $\times$ Evangelical interaction is precisely estimated at zero ($\hat{\beta}_3 = -0.07$, SE $= 0.18$). Evangelical density has no independent main effect (SE $= 0.32$). The NBC main effect is stable across all three steps of the analysis, with coefficients of $+0.63$, $+0.63$, and $+0.66$ (all significant at the 1% level).

Table 8: Evangelical Density Interaction (Standardized Variables)

	$\hat{\beta}$(NBC*)	SE	$\hat{\beta}$(Evang*)	SE	$\hat{\beta}$(NBC*×Evang*)	SE
Step 1: NBC only	$+0.63^{***}$	(0.222)	—	—	—	—
Step 2: + Evangelical	$+0.63^{***}$	(0.221)	+0.17	(0.321)	—	—
Step 3: + Interaction	$+0.66^{***}$	(0.231)	+0.18	(0.333)	−0.07	(0.180)

Notes: All variables standardized (mean zero, unit SD) before computing interactions. Standard errors (clustered by DMA) in parentheses. 1980 Census controls and region fixed effects included throughout. $N = 176$. $^{***}p < 0.01$. The null interaction is confirmed as substantive by a median split: the NBC effect is significant at the 1% level in low-evangelical DMAs and insignificant in high-evangelical DMAs, but the difference is insignificant and driven by geographic composition rather than denominational amplification (see text).

For the case proximity instrument, a suggestive evangelical interaction ($t = -1.61$) disappears completely when a South × LogDist interaction is added ($t = -0.09$). What appeared to be evangelical amplification was entirely a Southern geographic effect: Southern DMAs had a stronger case proximity effect ($t = -2.07$ on South × LogDist at Conley 500 miles), but this reflects the Southern regional context broadly, not denominational amplification per se.

The null NBC × Evangelical result has theoretical content. The amplification hypothesis predicts that the panic's political effects depended on evangelical infrastructure to translate fear into votes. Our result says instead that NBC delivery moved Republican vote share uniformly across the evangelical spectrum—in Catholic Boston and Baptist Birmingham alike. The panic operated as a mass media persuasion event, not as an activator of pre-existing institutional mobilization. This parallels the Durante et al. [2019] finding that entertainment television affected all markets regardless of prior political composition, though the mechanism here is fear-based content rather than cognitive displacement.

7 Discussion

What the round-trip tells us about media persuasion. Most studies of media effects find either permanent shifts or no effects. We find a complete mean reversion of the case proximity effect by 1992, a cumulative coefficient of -0.18 ($t = -0.36$). A genuine round-trip, documented in observed voting data across three presidential election cycles, provides unusually direct evidence for the transient salience model of political preference formation. When the panic's coverage collapsed, the threat-based preferences it had activated were deactivated. The panic was, in political terms, a temporary shock that fully reversed.

The round-trip evidence also bears on the mechanism by which the initial shift occurred. Our outcome — net change in two-party presidential vote share — conflates mobilization of latent Republican-leaning voters, genuine ideological conversion, and differential Democratic demobilization. These three channels have different implications for persistence: genuine persuasion or realignment should produce sticky, slowly decaying effects, while salience-activated pre-existing partisan dispositions should reverse sharply once the activated threat recedes. The clean round-trip of the proximity effect is therefore more consistent with a salience/priming mechanism—people with underlying conservative dispositions were temporarily primed by fear-based content to weight Republican-associated issue frames more heavily, then deprioritized those frames once the panic's empirical basis collapsed—than with genuine conversion. This interpretation aligns with the priming and agenda-setting literature [DellaVigna and Kaplan, 2007, Enikolopov et al., 2011] and with our null evangelical interaction result: if the effect were driven by institutional mobilization, we would expect it to persist, since organizational ties do not dissolve when a single moral panic is discredited.

NBC persistence vs. case proximity reversal: two channels. NBC delivery, which captures exposure through the national infotainment channel, produces a partially persistent effect that fades but does not reverse. Case proximity, which captures acute local shock, fully reverts. This difference is consistent with the following interpretation: markets

that experienced a local SRA case had a corrective mechanism, specific prosecutions were publicly discredited, specific defendants were exonerated, and local communities processed the false accusations. Markets that received the panic through national television had no such localized corrective event. The Geraldo special was never formally retracted; NBC never broadcast an equivalent program debunking what it had implied. The slower decay of the NBC effect is consistent with the absence of a common-knowledge corrective in those markets.

Why evangelical infrastructure did not amplify NBC effects. The null evangelical interaction invites two interpretations. First, the Christian Right was already substantially mobilized by 1984; the Moral Majority was founded in 1979; Reagan's coalition was assembled in 1980; and Layman [2001] documents that evangelical partisan realignment was largely complete by the early 1980s. The 1984-1988 change in Republican vote share may therefore reflect not a mobilization of previously inactive evangelicals but a modulation of existing partisan lean through fear-based salience activation. Second, the panic worked through mass media rather than institutional channels: it reached secular households in Boston and Catholic households in Pittsburgh as effectively as evangelical households in Birmingham. The television was the institutional infrastructure.

Electoral magnitude: an illustrative back-of-envelope. To calibrate the substantive significance of the DMA-level estimates, we translate them into a state-level magnitude exercise. Aggregating the NBC treatment effect to states by population-weighting each state's constituent DMAs, the estimated panic contribution to Republican vote share ranges from approximately +1.7pp (Washington, Montana) to +5.8pp (Illinois), with most states falling in the +2-4pp range. These are large numbers for a single media event operating through a two-week window before a presidential election.

Subject to the strong caveats that DMAs do not align with state borders and that this exercise holds all other vote determinants fixed, the estimated panic contribution exceeded

Bush's actual winning margin in five states: Connecticut (Bush margin +0.2pp), New York (+1.2pp), Illinois (+2.6pp), California (+3.6pp), and DC (+1.4pp), carrying a combined 118 electoral votes. Bush won 426-111, so flipping all five would still have produced a decisive Bush victory (308-230). The panic was not electorally decisive in 1988.

The non-decisiveness finding is itself informative, and the correct reading of these magnitudes cuts in a different direction. The panic spread through social contagion and media dynamics with no evident electoral logic: SRA cases arose in deep-red Utah (+34pp Bush) and Nebraska (+21pp Bush) as readily as in competitive states. Cases split evenly between swing and safe states (17 vs. 17), and the average Democratic swing from 1984 to 1988 was essentially identical in states with and without SRA cases (+11.0pp vs. +11.8pp).

What the magnitude exercise establishes is that the effects of this size are *election-relevant in principle*: a 3–5pp DMA-level shift attributable to a single primetime broadcast is large enough to have exceeded the winning margin in multiple large states in 1988, and would be decisive in any close national election or in the context of a Senate or gubernatorial race where a single DMA's shift can determine the outcome. The 1988 election was simply not close enough at the national level for even a substantial fear-based media effect to have been determinative. The implication for understanding contemporary media environments—where the analog of a Geraldo special now reaches audiences across hundreds of platforms simultaneously—is that the conditions for decisiveness are considerably more favorable than they were in 1988.

Limitations. We note several limitations. First, the NBC delivery index captures all NBC primetime content, not exclusively the "Devil Worship" special. We lack Nielsen ratings at the market level for that broadcast to directly verify the exposure mechanism. The null results for ABC (t=−0.91t = -0.91 t=−0.91) and CBS (t=−0.22t = -0.22 t=−0.22) are consistent with NBC-specific content driving the effect, and the syndication evidence substantially narrows the set of plausible NBC-specific candidates to the October 1988 network

special; but we cannot fully rule out that other NBC primetime content varying systematically across high- and low-delivery markets contributed to the shift. Second, case proximity is correlated with the great sort, and while the convergence of the two proxies provides reassurance, we cannot entirely rule out that the 1984-1988 proximity result captures early-stage demographic sorting beginning before the 1980 Census controls could fully absorb it. Third, the ambiguity between partial persistence and full reversal for the NBC instrument is not straightforwardly a power limitation. The theoretical framework in Section 3 offers two distinct mechanisms: if viewers processed the special through a concrete situation model with low cognitive elaboration [Shehata et al., 2021], the effect should decay as salience fades; if repeated exposure to panic content across the 1983-1988 infotainment cycle produced deeper schema activation [Petty and Cacioppo, 1979], partial persistence is the expected outcome rather than an artifact of sample size. With N = 176 DMAs, we cannot distinguish these mechanisms empirically, and the honest claim is that the NBC effect decays gradually without reaching zero by 1992. Fourth, our analysis is restricted to presidential vote share; downstream effects on congressional voting, state-level ritual abuse legislation, and other political outcomes remain outside our current scope.

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A Data Appendix

A.1 NBC Delivery Index: Source, Construction, and Sample

Source. The NBC Prime-Time Delivery Index is taken from the 1988 Broadcasting/Cablecasting Yearbook, Arbitron Television, May 1987 Network Program Group Analysis, pp. C-222 to C-224 ("Network Delivery Variations by Market"). The table reports delivery indices for three broadcast networks (ABC, NBC, CBS) across all 213 Arbitron ADI markets.

Index definition. Arbitron defines the delivery index for network n in market m as:

$$\text{DeliveryIndex}_{n,m} = \frac{\text{HUT}_{n,m}/\text{TVHH}_m}{\text{HUT}_{n,\text{national}}/\text{TVHH}_{\text{national}}} \times 100 \tag{5}$$

where $\text{HUT}_{n,m}$ is the average number of households using television tuned to network n during prime time in market m; TVHH_m is total television households in market m; and the denominator is the same ratio computed at the national level. A value of 150 means network n reaches 50% more households per unit of market size than the proportional benchmark; a value of 75 means it reaches 25% fewer. The index is distinct from ratings (audience share among viewers actively watching), market penetration (fraction of households owning a television set), and cable subscription rates, though cable penetration is the dominant empirical driver of cross-market variation.

Sources of index variation. Three factors account for most cross-market variation, all predetermined relative to the Satanic Panic:

1. *Cable penetration.* By 1987, approximately 50% of U.S. television households subscribed to cable, but penetration ranged from above 65% in large coastal metros (New York, Los Angeles, Boston) to below 25% in rural Southern and Plains markets. Cable households primarily watched cable channels during prime time, mechanically depressing the NBC delivery index in high-cable markets. This is the dominant driver of

the negative correlation between NBC index values and market size. Cable franchise agreements were issued in the 1970s and early 1980s, well before 1987.

2. *Over-the-air signal geography.* In low-cable markets, the index reflects affiliate signal reach. Flat terrain (eastern Plains, Mississippi Delta, coastal Georgia) allowed NBC affiliates to cover entire DMAs; mountainous terrain (Appalachians, Rockies, Pacific ranges) attenuated propagation to urban centers only. Transmitter placements date to the 1950s–60s.

3. *Competing local independents.* Markets with strong UHF independents (primarily large, ethnically diverse markets) had lower NBC delivery as households had local alternatives during prime time. Network affiliation agreements were set decades before the panic period.

OCR extraction and cleaning. The original table was extracted via Tesseract OCR from a scanned PDF of the yearbook. The parser successfully extracted 196 of 213 markets automatically. We manually verified all extracted values against the source PDF and corrected 30+ fields where the OCR misread digits (e.g., "415" for 115, "7A" for 71). For 15 markets where OCR failed entirely—typically because periods were misread as column separators in household counts—we entered values by hand from the PDF, which is clearly legible throughout. Two additional markets (Pittsburgh, Miami) were recovered after discovering that OCR had transposed rank numbers (reading "13." as "43."), causing them to overwrite other entries during parsing. Running headers, page numbers, and column labels were identified by non-numeric leading characters or values outside the plausible range of 25–250 and removed.

Odessa-Midland exclusion. We exclude Odessa-Midland (TX), whose NBC delivery index of 34 is 2.8 standard deviations below the sample mean. The same row contains a clearly erroneous ABC index of 13 (almost certainly 113 with a dropped leading digit), indicating

a typesetting error in the original yearbook rather than a genuine observation. This yields 212 ADI markets with credible delivery indices.

ADI-to-DMA crosswalk. Our instrument is defined over Arbitron's Areas of Dominant Influence (ADI), the television market geography standard until Arbitron exited TV ratings in December 1993. Election returns are reported by Nielsen's Designated Market Areas (DMA), which became the sole industry standard thereafter. Both systems assign every U.S. county to exactly one market based on plurality viewing patterns, and industry sources describe both as producing essentially the same market definitions.[5]

Matching ADI-era data to DMA-defined units is standard in the media-and-politics literature. Gentzkow [2006] applies 2002–2003 DMA boundaries retroactively to 1946-1960 television introduction data. Ansolabehere et al. [2006] reference both ADI and DMA definitions interchangeably when studying television and incumbency advantage. Snyder and Strömberg [2010] define their markets identically to how Arbitron defined ADIs—"all counties whose largest television viewing share is given to stations of that same market area." Any residual boundary differences between the 1987 ADI map and the modern DMA map are at the county margin and unlikely to materially affect DMA-level vote aggregates.

We hand-matched all 176 final DMAs to Arbitron market names, using geographic centroid proximity to resolve ambiguous cases. Market names were normalized to account for historical naming differences (e.g., Arbitron's "Greenville-Spartanburg-Asheville" versus Nielsen's "Greenville-Spartanburg-Anderson-Asheville").

ABC and CBS indices. The same Arbitron table reports delivery indices for ABC and CBS in identical format, extracted using the same OCR procedure. These serve as falsification instruments. The three network indices are essentially uncorrelated ($r_{\text{NBC,ABC}} = -0.11$; $r_{\text{NBC,CBS}} = -0.12$), consistent with each reflecting network-specific affiliate geography rather

[5]The FCC treated the two systems as interchangeable in must-carry regulations, switching from ADI to DMA boundaries in 2000 with transition provisions only for counties that shifted markets (47 CFR §76.55).

than a single market-level television infrastructure factor. Full falsification results are in Table A5.

Table A1: NBC Delivery Index: Distribution by Census Region

Region	*N*	Mean	SD	P25	P50	P75
Northeast	16	88.1	16.2	76	90	99
Midwest	49	103.9	22.8	89	103	117
South	76	107.2	25.4	90	106	122
West	35	91.4	21.0	76	89	104
All	176	102.4	24.3	86	101	118

Notes: NBC Prime-Time Delivery Index from Arbitron Television, May 1987 (1988 Broadcasting/Cablecasting Yearbook, pp. C-222–C-224). Higher values indicate above-proportional NBC household delivery. Northeast and West have lower means consistent with higher cable penetration rates in those regions by 1987.

A.2 Vanderbilt Archive: Satanic Panic Coding

The Vanderbilt Television News Archive (Harvard Dataverse, DOI: 10.7910/DVN BP2JXU) contains 1.1 million broadcast segments from ABC, CBS, and NBC evening news, 1968–2015. We searched for Satanic Panic content using the keyword `satanic`. Coverage peaks in 1984 (22 segments) and is near zero by the November 1988 election.

A.3 Broadcasting Magazine: Devil Worship Special and Geraldo Syndication

Broadcasting Magazine (WorldRadioHistory.com digitized archive) is used to establish two facts about the NBC mechanism. First, the October 31, 1988 issue documents the Devil Worship special's reception: a 21.9 rating/33 share (fifth-ranked program for the week nationally), near-universal affiliate clearance with only four preemptions, and approximately $2 million in advertiser cancellations. The November 14, 1988 issue provides a correction to the initial viewer response count (495 unfavorable, 290 favorable). These confirm that the NBC delivery index captures genuine differential exposure to a near-universally cleared broadcast, not idiosyncratic preemption variation.

Second, the September 21, 1987 syndication scorecard and the November 21, 1988 market-by-market clearance advertisement document that Rivera's daily talk show was distributed by Tribune Entertainment across all network affiliations—Fox (WNYW New York, WFLD Chicago, WTTG Washington), CBS O&Os (KCBS Los Angeles, WAGA Atlanta), and independents (WPHL Philadelphia, KCPQ Seattle)—rather than through NBC affiliates. The June 29, 1987 issue confirms that Rivera's earlier primetime specials (e.g., *Sons of Scarface*, August 17, 1987) were likewise syndicated by Tribune Entertainment to 157 stations on a barter basis, not broadcast as NBC network specials. This distinguishes "Devil Worship" as the only major Rivera-hosted panic program distributed exclusively through the NBC affiliate network.

A.4 Newspaper TV Listings: Broadcast Verification

To verify that the "Devil Worship" special aired as scheduled across our sample markets, we collected newspaper TV listings from 276 newspapers across 118 DMA markets spanning 45 states. Every listing confirms the special aired in the NBC 8–10 p.m. slot on October 25, 1988; no preemptions were found in any of the 276 newspapers examined.

Table A2 reports DMA coverage in the primary 176-DMA sample.

Table A2: Newspaper TV Listings: DMA Coverage Summary

Coverage category	DMAs	% of 176
Direct newspaper confirmation	118	67%
Same-state or adjacent-state covered	47	27%
Effective total	165	94%
Truly uncovered	11	6%

Notes: 276 newspapers collected from 118 DMA markets across 45 states. The 11 uncovered DMAs are all surrounded by confirmed states on every border: West Virginia (Bluefield, Charleston, Clarksburg, Parkersburg, Wheeling), bordered by confirmed VA, OH, PA, KY, and MD; Arkansas (Fort Smith, Jonesboro, Little Rock), bordered by confirmed TN, TX, OK, MS, LA, and MO; Nevada (Las Vegas, Reno), bordered by confirmed CA, AZ, UT, ID, and OR; and Wyoming (Cheyenne), bordered by confirmed CO, NE, SD, MT, and ID. The same-state extrapolation is justified by the near-universal broadcast: NBC sent the special simultaneously to all affiliates, and preemption decisions were made market-by-market.

Three affiliates are confirmed to have preempted the broadcast: KUTV Salt Lake City UT, an unnamed affiliate in Colorado Springs CO, and an unnamed affiliate in Bristol VA (*Broadcasting Magazine*, October 31, 1988). A fourth affiliate, WAVE Louisville KY, initially planned to preempt but reversed its decision after receiving approximately 1,000 viewer calls and a bomb threat; the *Courier-Journal* TV listing confirms the special ultimately aired in Louisville. The Salt Lake City preemption is also confirmed by the *Salt Lake Tribune*

listing, which shows the special scheduled but then preempted—confirming it was a last-minute decision by KUTV rather than a standing policy.

These preemptions introduce a form of classical measurement error in the NBC treatment variable: preempting markets receive zero treatment (no broadcast) but are coded as having their market-level NBC delivery index, which is positive. This biases the NBC coefficient toward zero, meaning our estimates are conservative lower bounds on the effect of exposure to the special. The preemption rate of approximately three out of 200 affiliates (1.5%) is insufficient to materially affect the estimates.

A.5 SRA Case Universe and Geocoding

Case identification procedure. We construct the case dataset of publicly prosecuted SRA cases through a systematic review of the published legal, journalistic, and academic literature. Inclusion requires three criteria: (1) criminal charges filed or formal grand jury investigation; (2) allegations explicitly included satanic, ritualistic, or cult-related abuse elements—not generic child abuse without ritual or satanic claims; (3) the case generated documented public attention, defined as local or national media coverage, community mobilization, or subsequent legal or academic scholarship.

We began with the case inventories in De Young [1997] and her 1997 OJP analysis of daycare ritual abuse cases, which identified approximately 100 investigated daycare centers of which only a fraction progressed to formal charges. We cross-referenced against Nathan and Snedeker [1995a] and Frankfurter [2006]. We supplemented with the National Registry of Exonerations, state innocence clinic files (notably the Duke Wrongful Convictions Clinic), published appellate opinions, the PBS Frontline daycare case outcome database, and contemporary newspaper archives. We cross-checked for completeness against the Wikipedia compilations ("Day-care sex-abuse hysteria" and "Satanic ritual abuse" case lists) and the de Young (2008, *Sociology Compass*) review.

We exclude the approximately 12,000 unsubstantiated reports to social services (NCCAN

1994; Bottoms, Shaver, and Goodman 1996) that were never publicly documented, never prosecuted, and are not geocodable from any public record. An anonymous CPS intake quietly closed does not make a community more fearful; a seven-year televised trial (McMartin) or a 24-defendant prosecution (Jordan, MN) does. The identification assumption requires geographic variation in exposure to *publicly visible* SRA panic, not in confidential social service contacts.

Treatment window restriction. For the main 1984-1988 treatment window, the proximity instrument uses only the set of cases active as of each election year. By the 1984 election, 13 cases had begun (cases 1-13 in Table A3). By the 1988 election, 27 cases were active or resolved (cases 1-27). The remaining 9 cases (28-36) began after 1988 and contribute only to the post-treatment proximity measures used in the reversal analysis. Because cases begin at different times, the set of active case epicenters expands across election years, generating within-DMA variation in minimum distance over time: a DMA far from all cases in 1984 may become proximate when a nearby case begins in 1986. For the treatment window specifications, only pre-treatment 1980 Census controls are appropriate; 1990 demographics are post-treatment and are not used in proximity specifications.

Case characteristics. The 36 cases span 1982-2008 across 20 states, involving 157 or more named defendants and hundreds of alleged victims. The geographic distribution covers all major regions: California (5 cases), Texas (4), Florida (3), New York (3), North Carolina (2), Washington state (2), and one each in 13 other states. Cases range from small towns (Akron, CO, population 1,700) to major metros (Chicago, Miami, San Francisco). Resolutions include acquittals (7), convictions later overturned (12), convictions upheld (4), guilty pleas (3), charges dropped or dismissed (7), and mixed outcomes (3).

Table A3: SRA Case Universe, Cases 1–21 ($N = 36$ total; continued in Table A4)

#	Case	City, State	Start	End	Def.	Resolution
Cases 1–13: active by 1984 election						
1	Kern County	Bakersfield, CA	1982	2004	36	34/36 convictions overturned
2	McMartin Preschool	Manhattan Beach, CA	1983	1990	7	All charges dropped (7-year trial)
3	Jordan / Scott County	Jordan, MN	1983	1985	24	1 conviction; 2 acquittals; rest dismissed
4	Country Walk / Fuster	Miami, FL	1984	1985	2	Conviction; Fuster still imprisoned
5	Fells Acres Day School	Malden, MA	1984	2004	3	Convictions; last released 2004
6	Bernard Baran / ECDC	Pittsfield, MA	1984	2009	1	Conviction overturned; dismissed
7	Nathaniel Grady / Tremont	Bronx, NY	1984	1996	1	Conviction overturned after 10 years
8	Praca Day Care Center	Bronx, NY	1984	1989	3	2 upheld; 1 overturned
9	Rogers Park JCC	Chicago, IL	1984	—	1	Charges filed; no trial
10	West Point CDC	West Point, NY	1984	1985	—	No indictments; $2.7M settlement
11	Small World Preschool	Niles, MI	1984	1985	1	Conviction (50–75 years)
12	Grant Snowden	Miami, FL	1984	1998	1	Conviction overturned; dismissed
13	Akron daycare	Akron, CO	1984	1985	—	No charges; daycare closed
Cases 14–27: active or resolved by 1988 election						
14	Wee Care / Kelly Michaels	Maplewood, NJ	1985	1993	1	Conviction reversed on appeal
15	East Valley YMCA	El Paso, TX	1985	1988	1	Conviction overturned; acquitted at retrial
16	Ray Spencer	Vancouver, WA	1985	2004	1	Conviction vacated; $6M settlement
17	Lehi / Utah County SRA	Lehi, UT	1985	1995	1	No SRA convictions
18	Presidio CDC	San Francisco, CA	1986	1988	1	Charges dropped; investigation closed
19	Glendale Montessori	Stuart, FL	1986	1989	2	Alford and no-contest pleas
20	Friedman case	Great Neck, NY	1987	1988	2	Guilty pleas
21	Junior Chandler	Marshall, NC	1987	—	1	Convicted; still imprisoned

Table A4: SRA Case Universe, Cases 22–36 (continued from Table A3)

#	Case	City, State	Start	End	Def.	Resolution
Cases 14–27 continued: active or resolved by 1988 election						
22	Gallup Christian Day Care	Roseburg, OR	1987	1991	3	Mixed; some convictions overturned
23	Franklin credit union	Omaha, NE	1988	1990	—	Grand jury found hoax
24	Paul Ingram	Olympia, WA	1988	2003	1	Guilty plea; served sentence
Cases 28–36: begin after 1988; used only in reversal/post-treatment analysis						
25	Little Rascals Day Care	Edenton, NC	1989	1997	7	All convictions reversed on appeal
26	Bobby Fijnje	Miami, FL	1989	1991	1	Acquitted on all counts
27	Breezy Point Day School	Langhorne, PA	1989	1989	—	DA declined prosecution
28	Oak Hill / Keller	Austin, TX	1991	2017	2	Convictions vacated; actually innocent
29	Faith Chapel / Dale Akiki	Spring Valley, CA	1991	1993	1	Acquitted on all counts
30	Melvin Quinney	San Antonio, TX	1991	2023	1	Conviction vacated; declared innocent
31	Michael Alan Parker	Saluda, NC	1992	2014	1	Conviction vacated after 22 years
32	Gilmer / Kerr family	Gilmer, TX	1992	1995	8	All charges dismissed by TX AG
33	West Memphis Three	West Memphis, AR	1993	2011	3	Released via Alford plea (18 years)
34	Wenatchee sex ring	Wenatchee, WA	1994	2000	43	Most convictions reversed or dismissed
35	San Antonio Four	San Antonio, TX	1994	2016	4	Convictions vacated; exonerated
36	Tonya Craft	Ringgold, GA	2008	2010	1	Acquitted on all 22 counts[a]

Sources: De Young [1997]; Nathan and Snedeker [1995a]; National Registry of Exonerations; PBS Frontline daycare case outcome database; published appellate opinions; contemporary newspaper archives.

[a] Case 36 (Tonya Craft, 2008) postdates the Satanic Panic era but involved explicit SRA allegations and is included for completeness. It does not affect any 1984–1988 treatment window estimates; excluding it leaves all results unchanged.

Geocoding. SRA case epicenter coordinates are geocoded to the population-weighted centroid of the primary municipality associated with each case. Geocoding uses the Census Bureau 2020 Gazetteer for county-level centroids. DMA centroids are constructed as population-weighted averages of constituent county centroids using the Patton county-DMA crosswalk and 1980 Census population weights. The treatment variable `log_min_dist` is the log of minimum great-circle distance (miles) from each DMA's population-weighted centroid to the nearest SRA case epicenter active as of each election year. The log transformation captures diminishing marginal effects of distance: the difference between 10 and 50 miles is more consequential for community exposure than the difference between 500 and 540 miles. A negative coefficient on `log_min_dist` indicates that closer proximity to an active case is associated with a larger Republican vote share gain.

B Additional Robustness Results

B.1 ABC and CBS Falsification

Table A5: Network-Specific Delivery Indices: Falsification Tests

	NBC	ABC	CBS
Solo delivery → ΔRep 1984–88	+1.92*	−0.91	−0.22
Average network index (solo)		−0.30	
All three simultaneous ($N = 173$)			
NBC		+1.71*	
ABC		−0.55	
CBS		+0.09	
Joint spec with LogDist ($N = 173$)			
NBC		+2.10**	
ABC		−0.60	
CBS		+0.16	
Correlation with NBC	1.00	−0.11	−0.12

Notes: t-statistics reported. Solo specifications use $N = 176$; three-way and joint specifications use $N = 173$ (the ABC and CBS corrected sample after OCR re-verification). Network delivery indices from the same 1988 Arbitron source (pp. C-222–C-224). Near-zero inter-network correlations rule out a "general TV infrastructure" explanation: markets delivering more television overall did not shift Republican—only markets with stronger NBC delivery did. ABC and CBS delivery are null in all specifications; only NBC-specific delivery predicts a Republican shift, consistent with the "Devil Worship" October 1988 NBC primetime special as the operative mechanism. NBC strengthens rather than weakens when case proximity is added to the specification $^{**}p < 0.05$, $^{*}p < 0.10$.

B.2 Split-Sample by Evangelical Density Tercile

Table A6: NBC Effect by Evangelical Density Tercile

Tercile	N	$\hat{\beta}$ (NBC)	t	Region composition
Bottom ($\leq 3\%$)	59	+0.047	3.24^{***}	NE + MW + West
Middle (3–13%)	58	−0.007	−0.35	Mixed
Top ($> 13\%$)	59	+0.019	1.34	South/Border South

Notes: Bottom tercile covers low-evangelical markets (predominantly northeastern Catholic/mainline territory, but geographically diverse). The apparent gradient reflects geographic composition rather than denominational amplification; the linear NBC × Evangelical interaction is $t = -0.39$ (Table 8).