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

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Abstract

We exploit the 1983–1992 Satanic Panic as a natural experiment to identify the political effects of fear-based entertainment media. Using two independent proxies for panic exposure across 176 Designated Market Areas—predetermined NBC affiliate delivery strength and geographic proximity to Satanic Ritual Abuse prosecution epicenters—we find that both predict excess Republican presidential vote-share gains of approximately 0.5 to 1.2 percentage points in 1988, relative to the pre-panic trend.¹ The two instruments have opposite demographic profiles, making single-confound explanations implausible. The case proximity effect fully reverts by 1992 ($\beta = 0.233$, $SE = 0.456$ on the cumulative 1980–1992 first difference), 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 identifies fear-based entertainment media as a direct persuasion channel, operating independently of institutional religious infrastructure.

JEL Codes: D72, L82, P16, Z12

Keywords: media effects, moral panic, entertainment television, political behavior, difference-in-differences

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¹Magnitudes correspond to interquartile-range shifts in the respective treatment, evaluated at the joint-specification coefficients ($\beta_{\text{LogDist}} = -1.031$, Conley-500mi $SE = 0.318$; $\beta_{\text{NBC}} = 0.019$, $SE = 0.007$).

1 Introduction

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

Two features make the Satanic Panic an unusually clean natural experiment. First, its 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 clean beginning and a publicly documentable end—the McMartin acquittals (1990), the FBI Lanning Report (1992), and a wave of appellate reversals—enabling us to trace not just the onset but the reversal of media-induced political preferences.

We exploit two independent sources of variation in panic exposure across 176 Designated Market Areas.² The first is geographic proximity to SRA prosecution epicenters: we geocode 36 major SRA criminal prosecutions from systematic review of the legal and academic literature (de Young, 2004; Nathan and Snedeker, 1995; Frankfurter, 2006), and DMAs closer to active cases experienced more intense local exposure through arrests, trials, and community mobilization. The second is the NBC affiliate delivery index, a pre-determined measure of NBC household reach from Arbitron’s 1987 market survey. Markets with stronger NBC delivery had higher viewership of the single most-watched piece of Satanic Panic media: Gerardo 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—reaching approximately 20 million households.

In a joint continuous-treatment difference-in-differences specification with 1980 Census

²29 markets are excluded due to OCR failures, mismatches with vote data, or typesetting errors in the original 1988 *Broadcasting Yearbook*; full sample-construction detail is in Appendix A. The expanded sample of 205 matchable DMAs is reported as a robustness check in Appendix E.

controls and region fixed effects, both instruments predict increased Republican two-party vote share in the 1984–1988 treatment window. Case proximity yields $\beta = -1.031$ (Conley 500mi SE = 0.318): DMAs closer to SRA cases shifted more Republican. NBC delivery yields $\beta = +0.019$ (Conley 500mi SE = 0.007): markets with stronger NBC signal shifted more Republican. At an interquartile shift in either treatment, the implied 1988 shift is between 0.5 and 1.2 percentage points. Neither instrument shows pre-trends in 1976–1980 or 1980–1984 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 share during the panic and returned to their pre-panic baseline by 1992. The zero cumulative effect simultaneously rules out three rival explanations: permanent persuasion (which would leave a positive residual), backlash (a negative overshoot), and demographic sorting as the primary driver (which would be permanent, not reversible). The only model consistent with the data is transient salience: the panic temporarily activated threat-based political preferences that dissipated when the panic’s empirical basis collapsed.

The core identification argument is the convergence of two instruments with opposite demographic profiles. NBC delivery is orthogonal to urbanization by construction—affiliate strength was set by transmitter placement and franchise licensing in the 1950s–70s—while case proximity is correlated with the urban, educated coastal metros that anchor the contemporary great sort.

The paper makes four contributions. First, it provides the first causal estimate of the political effects of a moral panic; prior work is either historical (Cohen, 1972; Victor, 1993) or descriptive (Uscinski et al., 2022), with no study exploiting geographic variation in exposure for identification. Second, it identifies entertainment media as a direct fear-based persuasion channel—distinct from the cognitive-degradation mechanism in Durante et al. (2019)—and shows the channel operates independently of institutional religious infrastructure (the NBC

× evangelical interaction is zero). Third, it documents a complete round-trip in media-induced political preferences, a rare finding in a literature dominated by either permanent effects or null effects. Fourth, it rules out institutional transmission: the null evangelical interaction shows that entertainment media persuaded directly through mass exposure, not through activation of religious networks.

The paper sits closest methodologically to [DellaVigna and Kaplan \(2007\)](#), who regress changes in Republican vote share on Fox News availability with pre-period placebos. We generalize that design to continuous treatment intensity and multiple periods following [Callaway et al. \(2024\)](#). Our use of legacy broadcast infrastructure parallels [Durante et al. \(2019\)](#) (Mediaset signal strength) and [Enikolopov et al. \(2011\)](#) (NTV signal availability in Russia). The DMA-level $N = 176$ is comparable in size to [Kearney and Levine \(2015\)](#). On the religious-media side, [Buccione and Knight \(2024\)](#) and [McRae \(2025\)](#) estimate Moral Majority TV and Christian talk radio effects respectively; our paper differs in that the Satanic Panic was transmitted through secular entertainment media, not explicitly religious programming.

2 Historical Background

The panic, 1983–1992. The Satanic Panic began with the McMartin Preschool case in Manhattan Beach, California, in September 1983. Over the next decade, allegations of Satanic Ritual Abuse spread to day-care centers in Jordan (MN), Malden (MA), Maplewood (NJ), Miami (FL), and dozens of other communities. By the 1984 presidential election, 13 cases had begun; by 1988, 27 were active or resolved. The 36 prosecutions we geocode span 20 states, involve more than 157 named defendants, and range from small towns (Akron, CO; population 1,700) to major metropolitan areas (Chicago, Miami, San Francisco). Network evening-news coverage—measured from the Vanderbilt Television News Archive—peaked in 1984 with 22 segments across the three networks, declined through the mid-1980s, and was

virtually absent by November 1988.³

The Geraldo “Devil Worship” special. The single most important media event for our identification strategy is Geraldo Rivera’s “Devil Worship: Exposing Satan’s Underground,” an NBC primetime network special that aired on October 25, 1988, from 8:00 to 10:00 PM—two weeks before the presidential election. It drew approximately 19–20 million households and ranked #5 nationally that week, behind only *The Cosby Show*, *Cheers*, *A Different World*, and *60 Minutes*. Three features matter for identification. First, it was an NBC network special fed simultaneously to all affiliates rather than a syndicated program; we verify near-universal clearance from 276 newspaper TV listings spanning 137 DMAs in 45 states (Appendix A.6), with effective coverage of 98% of the analysis sample after same-state extrapolation. Second, the proximity to the election maximizes the potential for short-run electoral effects. Third, the special is distinct from Rivera’s daily syndicated talk show “Geraldo” (1987–98, Tribune Entertainment), which was distributed station-by-station across all network affiliations and is therefore orthogonal to the NBC delivery index. This distinction is what makes the NBC index a network-specific measure of exposure to the special, and it is the reason the NBC effect is unique to NBC: ABC and CBS delivery indices, constructed identically, are zero (Appendix B.2).

Political context and collapse. The panic arose alongside the New Right and the Moral Majority (founded 1979); panic narratives about child safety and satanic threats aligned naturally with conservative frames of moral decay, family values, and law and order (Hughes, 2017). The panic’s collapse was public and definitive: the McMartin trial—the longest and most expensive criminal trial in American history—ended in full acquittals in January 1990; the FBI’s Behavioral Science Unit published the Lanning Report in 1992, concluding that no credible evidence of organized satanic criminal networks existed; and appellate courts

³148 panic-related evening-news segments over 1983–1996. NBC had the fewest segments (26, versus ABC’s 33 and CBS’s 39), reinforcing the distinction between NBC’s evening-news coverage and its primetime programming.

reversed numerous convictions through the 1990s. By the mid-1990s, the moral panic had effectively ended.

3 Related Literature

Media and political behavior. Research on media effects has long been organized into three traditions: the minimal effects perspective ([Lazarsfeld et al., 1944](#); [Katz and Lazarsfeld, 1955](#)), in which media reinforce rather than convert; agenda-setting and framing ([McCombs and Shaw, 1972](#); [Iyengar and Kinder, 1987](#)), which identifies stronger but issue-specific influences; and the conditional effects view that media influence is substantial but varies with audience and institutional context ([Bartels, 1993](#); [Prior, 2007](#)). The empirical challenge in this literature is isolating exogenous variation in media exposure. A productive approach exploits geographic rollout of broadcast infrastructure: [Strömberg \(2004\)](#) on New Deal radio, [Gentzkow \(2006\)](#) on broadcast television entry, and [Campante and Hojman \(2013\)](#) on the ideological consequences of TV penetration. [DellaVigna and Kaplan \(2007\)](#) estimate that Fox News entry into cable markets raised the Republican vote share by 0.4–0.7 percentage points; [Enikolopov et al. \(2011\)](#) find that independent television in Russia increased opposition voting by roughly nine percentage points. [Yanagizawa-Drott \(2014\)](#) and [Adena et al. \(2015\)](#) document large content-driven effects of radio in Rwanda and Nazi Germany. Our NBC delivery index follows the broadcast-infrastructure tradition; legacy transmitter placements supply the predetermined variation.

The closest precedent in mechanism is [Durante et al. \(2019\)](#), who show that Mediaset entertainment exposure shifted Italian vote shares toward populist parties by approximately 1.5 percentage points through cognitive degradation—entertainment crowding out political information. Our paper identifies a distinct channel: fear-based moral content. The Geraldo special was watched by approximately 19 million households, not as background but attentively; it did not displace political information so much as supply a particular kind of

it.

Moral panics and political mobilization. Cohen (1972) introduced moral panic as a sociological category; Goode and Ben-Yehuda (1994) formalized the framework into criteria—concern, hostility, consensus, disproportionality, volatility—that the Satanic Panic satisfies. Victor (1993) and Hughes (2017) provide the definitive social and historical accounts, the latter arguing that the panic was institutionally entangled with the New Right’s tabloid media ecosystem. Beckett and Sasson (2004) more broadly establishes that crime framing systematically benefits conservative political actors.

Religion and political mobilization. Layman (2001) documents that evangelical partisan realignment was largely complete by the early 1980s through the Moral Majority and the Reagan coalition. We read our null evangelical interaction in this light: by 1984, the Christian Right’s mobilization was already near its ceiling, and the marginal contribution of the panic operated through mass media rather than through institutional religious networks. Campbell (2006) develops a complementary contextual religious threat mechanism in which evangelicals embedded in secular environments perceive a symbolic threat that activates group identity. The asymmetry is informative: secular contexts mobilize religious conservatives but do not produce equivalent counter-mobilization—a pattern consistent with our finding that the panic activated a directional Republican shift through media exposure rather than through symmetric two-sided polarization.

Persistence and decay of media-induced preferences. A substantial literature asks whether political shocks produce permanent attitude change. Alesina and Fuchs-Schündeln (2007) document persistent effects of communist socialization on East Germans’ economic preferences. Our paper contributes contrasting evidence: a complete round-trip in media-induced preferences, with the case proximity effect fully reverting to zero by 1992. The theoretical mechanism is transient salience rather than deep socialization. Recent media-

effects research formalizes this distinction: short-lived effects arise when audiences process content through concrete situation models with low cognitive elaboration, while durable effects require deeper schema activation (Petty and Cacioppo, 1979; Shehata et al., 2021). The Geraldo special operated through the former channel; its political consequences faded as the panic’s empirical basis collapsed.

4 Data

Treatment 1: NBC affiliate delivery. The NBC Prime-Time Delivery Index, by Arbitron ADI market, comes from Arbitron Television’s May 1987 Network Program Group Analysis (1988 *Broadcasting/Cablecasting Yearbook*, pp. C-222 to C-224). An index of 100 means the market delivers NBC prime-time viewership proportional to its share of national TV households. In our analysis sample ($N = 176$) the NBC index has mean 104.4, SD 23.2, and range [42, 197]. The identification argument rests on the fact that affiliate delivery strength was determined by legacy infrastructure decisions—transmitter placement, franchise licensing, network affiliation agreements—made in the 1950s–70s, decades before the panic. The three network indices (NBC, ABC, CBS) are essentially uncorrelated (pairwise $r \approx -0.1$), which rules out a generic “TV exposure” confound. We extract the table via OCR with hand verification, port from Arbitron ADI to Nielsen DMA via household-weighted aggregation, and use ABC/CBS as falsification instruments. Full source documentation, OCR procedure, the Odessa-Midland correction, and the ADI-DMA crosswalk are in Appendix A.1.

Treatment 2: SRA case proximity. The proximity instrument is $\text{LogDist}_{d,t} = \log(\text{minimum great-circle distance})$ computed at population-weighted centroids. A negative coefficient indicates that closer proximity to active cases is associated with a larger Republican gain. We hand-code 36 publicly prosecuted SRA cases from systematic review of legal, journalistic, and academic sources (de Young, 2004; Nathan and Snedeker, 1995; Frankfurter, 2006; Rabinowitz, 2003); cases enter the active set in the year charges are filed, generating within-DMA variation in minimum

distance over time. We exclude the approximately 12,000 unsubstantiated reports to social services (Bottoms et al., 1996); identification requires variation in *public* panic exposure, not confidential contacts. The full case universe (Tables A.3–A.4) and geocoding procedure are in Appendix A.7.

Outcome. The outcome is the first-difference in Republican two-party presidential vote share by DMA, $\Delta\text{Rep}_{d,t \rightarrow t+4} = \text{RepShare}_{d,t+4} - \text{RepShare}_{d,t}$, computed from Daily Kos Elections DMA-level returns 1960–2020. The treatment window is 1984→1988; pre-trend windows are 1976→1980 and 1980→1984; the reversal window is 1988→1992; and the cumulative round-trip window is 1980→1992.

Controls and moderator. Baseline controls are four 1980 Census variables (percent urban, percent white, percent college, log median income), aggregated from county to DMA via the Patton crosswalk, plus Census region fixed effects. Robustness specifications add 1990 Census demographics, state unemployment changes, and flexible functional forms. Evangelical church density uses the 1980 ARDA Churches and Church Membership survey, aggregating five denominations (Southern Baptist 84%, Assemblies of God 8%, Church of the Nazarene 5%, Church of God 2%, Foursquare Gospel 1%); the resulting rate is right-skewed (median 6.0%, range 0.4% Boston to 42.5% Knoxville) and essentially uncorrelated with NBC delivery ($r = 0.05$).

5 Empirical Strategy

Main specification. We estimate a continuous-treatment difference-in-differences design (Callaway et al., 2024) in which the same cross-sectional regression is fit across multiple election windows:

$$\Delta\text{Rep}_{d,t \rightarrow t+4} = \alpha + \beta_1 \text{LogDist}_d + \beta_2 \text{NBC}_d + \mathbf{X}'_d \boldsymbol{\gamma} + \delta_r + \varepsilon_d, \quad (1)$$

Table 1: Summary Statistics (Main Sample, $N = 176$ DMAs)

	Mean	SD	Min	Max
<i>Panel A: Outcome variables</i>				
Δ Rep. share, 1976–1980	7.462	4.694	−7.093	21.127
Δ Rep. share, 1980–1984	5.120	5.693	−6.014	18.294
Δ Rep. share, 1984–1988	−6.147	3.230	−13.105	2.559
Δ Rep. share, 1988–1992	−6.339	3.752	−17.377	3.762
<i>Panel B: Treatment variables</i>				
NBC delivery index	102.8	24.6	42	197
ABC delivery index	100.1	53.1	39	709
CBS delivery index	109.5	33.6	50	425
Log min distance to SRA case (1984)	5.615	0.983	0.819	6.918
<i>Panel C: Controls (1980 Census)</i>				
Pct urban	60.68	17.06	5.12	97.94
Pct white	86.08	11.63	42.77	99.38
Pct college	14.30	3.65	8.00	27.25
Log median income	9.628	0.154	9.177	10.043
Evangelical rate (per 1000)	119.4	121.0	4.3	425.3

Notes: Unit of observation is the DMA (Designated Market Area). Outcome variables are first-differences in Republican two-party presidential vote share between consecutive elections. NBC, ABC, and CBS delivery indices are from Arbitron’s 1987 market survey (1988 *Broadcasting/Cablecasting Yearbook*); an index of 100 means a market delivers network prime-time viewership proportional to its share of national TV households. Log min distance is log great-circle miles from DMA population-weighted centroid to nearest active SRA prosecution epicenter as of 1984. Evangelical rate includes Southern Baptist Convention, Assemblies of God, Church of the Nazarene, Church of God (Cleveland, TN), and Foursquare Gospel adherents per 1,000 population (ARDA, 1980).

where d indexes DMAs, δ_r are region fixed effects, and \mathbf{X}_d collects the four 1980 Census controls. First-differencing the outcome absorbs all time-invariant DMA characteristics. The joint specification, with both instruments entered simultaneously, is the lead result.

Parallel trends. Identification requires that, absent the panic, outcome trends would be uncorrelated with treatment intensity conditional on controls. We test this directly by estimating Equation (1) in the two pre-treatment windows (1976→1980 and 1980→1984) and verifying $\hat{\beta}_1, \hat{\beta}_2 \approx 0$. Both treatments are determined by history and geography rather than choice: NBC delivery by mid-century transmitter and franchise decisions, case proximity by where SRA prosecutions happened to occur.

Event study. The levels event study (Figure 1 in Appendix B.1) follows the standard panel specification with DMA and year fixed effects and controls interacted with year:

$$\text{RepShare}_{d,t} = \alpha_d + \gamma_t + \sum_{t \neq 1984} \beta_t \cdot \text{Treatment}_d \cdot \mathbf{1}(t) + \sum_{t \neq 1984} \mathbf{X}'_d \boldsymbol{\delta}_t + \varepsilon_{d,t}, \quad (2)$$

normalized to $\beta_{1984} = 0$. The controls-by-year interactions are essential: without them, LogDist shows large mechanical pre-trends because case proximity is unconditionally correlated with the great sort. The first-difference event study, reported in Appendix Figure 4, stacks first-differenced outcomes and exactly reproduces the separate cross-sectional regressions.

Standard errors and identification checks. We report DMA-clustered SEs as the baseline and Conley (1999) spatial HAC SEs at 100, 200, and 500 mile cutoffs using a Bartlett kernel; spatial SEs account for cross-DMA dependence from shared media markets, commuting zones, and regional shocks. Identification is supported by a battery of checks reported in §6 and Appendix B: instrument balance on 1980–1990 demographic changes; ABC/CBS falsification on identically constructed indices; influence robustness to dropping the closest DMAs; permutation inference from 1,000 randomizations; leave-one-out jackknife; and Oster (2019) coefficient-stability bounds.

6 Results

6.1 Pre-Trend Validation

Table 2 reports estimates of Equation (1) in the two pre-treatment windows. Both treatments produce zeros: in 1976→1980, $\hat{\beta}_{\text{LogDist}} = -0.28$ (SE = 0.40) and $\hat{\beta}_{\text{NBC}} = 0.007$ (SE = 0.014); in 1980→1984, $\hat{\beta}_{\text{LogDist}} = 0.03$ (SE = 0.36) and $\hat{\beta}_{\text{NBC}} = 0.000$ (SE = 0.012). Coefficients are tightly bounded around zero, supporting the parallel-trends assumption that, conditional on

Table 2: Pre-Trend Tests

Outcome window	Log Case Proximity			NBC Delivery Index		
	$\hat{\beta}$	(SE)	t	$\hat{\beta}$	(SE)	t
Δ Rep 1976–1980	−0.283	(0.402)	−0.70	0.007	(0.014)	0.50
Δ Rep 1980–1984	0.032	(0.361)	0.09	−0.000	(0.012)	−0.01
Δ Rep 1984–1988	−1.000	(0.266)	−3.76	0.017	(0.009)	1.80

Notes: Each cell reports the coefficient from a separate OLS regression of the first-differenced Republican two-party vote share on the indicated treatment variable. All specifications include 1980 Census controls (pct urban, pct white, pct college, log median income) and region fixed effects. Heteroskedasticity-robust standard errors. $N = 176$ DMAs. The treatment window (1984–1988) is bolded for reference.

controls, treatment intensity is uncorrelated with secular outcome trends.

6.2 Main Results: The Treatment Window

Table 3 reports the main results. In the joint specification with 1980 Census controls and region fixed effects, both instruments are significant in the 1984–1988 treatment window:

$$\hat{\beta}_{\text{LogDist}} = -1.031 \quad (\text{SE} = 0.264 \text{ OLS}; 0.318 \text{ Conley } 500\text{mi})$$

$$\hat{\beta}_{\text{NBC}} = 0.019 \quad (\text{SE} = 0.009 \text{ OLS}; 0.007 \text{ Conley } 500\text{mi}).$$

At an interquartile shift, case proximity implies a 1.16 pp Republican gain in 1988 and NBC delivery implies a 0.50 pp gain. The convergence of two instruments with opposite demographic profiles is the strongest evidence against single-confound explanations.

In isolation, case proximity yields $\hat{\beta} = -1.00$ (SE = 0.27): a one-log-unit decrease in distance to the nearest active SRA case is associated with roughly a one-percentage-point increase in Republican two-party vote share over 1984–1988. The effect survives flexible functional forms, with quadratics and pairwise interactions of 1980 demographics yielding $\hat{\beta} = -1.08$ (SE = 0.28).

NBC delivery in isolation is significant at the 10% level ($\hat{\beta} = 0.017$, $t = 1.80$). It becomes fully significant in the joint specification ($\hat{\beta} = 0.019$, $t = 2.09$), with Conley spatial SEs

Table 3: Main Results: Δ Republican Vote Share, 1984–1988

	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Treatment window (1984–1988)</i>					
Log Case Proximity	−1.000*** (0.266)				−1.031*** (0.264)
NBC Delivery Index		0.017* (0.009)	0.020** (0.009)	0.024*** (0.009)	0.019** (0.009)
Controls	1980	1980	1990	Both	1980
Region FE	Yes	Yes	Yes	Yes	Yes
<i>N</i>	176	176	176	176	176
<i>Panel B: Reversal (1988–1992) and Round-Trip (1980–1992)</i>					
	Reversal: Δ Rep 1988–1992			Round-trip: Δ Rep 80–92	
	(6)	(7)	(8)	(9)	(10)
Log Case Proximity	1.201*** (0.361)	0.898** (0.393)	0.434 (0.384)	0.233 (0.456)	
NBC Delivery Index					0.014 (0.016)
Controls	1980	1990	Both	1980	1980
Region FE	Yes	Yes	Yes	Yes	Yes
<i>N</i>	176	176	176	176	176

Notes: Each column reports OLS estimates of Equation (1). The dependent variable is the first-differenced Republican two-party presidential vote share over the indicated window. “1980” controls: pct urban, pct white, pct college, log median income (1980 Census). “1990” controls: same variables from the 1990 Census. “Both”: all eight controls. Column (5) is the joint specification (both treatments entered simultaneously). Heteroskedasticity-robust standard errors in parentheses. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

($\hat{\beta} = 0.019$, SE = 0.007, $t = 2.84$), and with 1990 Census controls ($\hat{\beta} = 0.020$, $t = 2.31$) or both Census vintages ($\hat{\beta} = 0.024$, $t = 2.81$). The strengthening with 1990 controls is inconsistent with demographic confounding driving the result: if anything, the channel sharpens after absorbing contemporaneous demographic change.

6.3 The Round-Trip: Transient Salience

The cumulative 1980→1992 coefficient for case proximity is zero:

$$\hat{\beta}_{\text{LogDist}}^{\text{cumulative}} = 0.233 \quad (\text{SE} = 0.456).$$

DMAs near SRA cases gained Republican share during the panic and returned to their pre-panic baseline by 1992. The round-trip is complete.

This zero cumulative coefficient simultaneously rules out three rival explanations: permanent persuasion would predict $\hat{\beta} > 0$ in 1992; backlash would predict $\hat{\beta} < 0$ (a negative overshoot); and demographic sorting as the main driver would produce a permanent, non-reversible shift. The only model consistent with the data is transient salience—the panic temporarily activated threat-based political preferences that dissipated when its empirical basis collapsed (Cohen, 1972). The dynamic event-study counterpart (Figure 1, Appendix B.1) shows the same pattern visually: flat pre-trends through 1980, a sharp 1988 treatment effect, and case-proximity coefficients returning to baseline by 1992. The one-step 1988→1992 reversal coefficient (Appendix B.9) is significant with pre-treatment controls and attenuates when post-treatment 1990 demographics are added, but the round-trip result does not depend on that specification.

6.4 Instrument Validity

Table 4 regresses 1980–1990 demographic changes on each treatment to characterize the instruments’ demographic profiles. NBC delivery predicts changes in college share ($\hat{\beta} = 0.032$, SE = 0.010) and white share ($\hat{\beta} = 0.088$, SE = 0.029) but not urban share. Case proximity is strongly correlated with the great sort: college share ($\hat{\beta} = 0.76$), urban share ($\hat{\beta} = 6.83$), and income ($\hat{\beta} = -0.05$, all $p < 0.01$). The instruments have partially overlapping but distinct demographic profiles, which is precisely why their convergence on the same treatment effect is informative: no single demographic confound can rationalize both results simultaneously.

6.5 Evangelical Interaction

We test whether evangelical church density moderates the effect of panic exposure, with all variables standardized before computing interactions. The NBC \times Evangelical interaction is zero ($\hat{\beta} = 0.06$, SE = 0.19): there is no evidence that evangelical density amplifies the NBC

Table 4: Instrument Validity: Treatment Variables Predicting 1980–1990 Demographic Change

Dependent variable	NBC Delivery Index		Log Case Proximity	
	$\hat{\beta}$	(SE)	$\hat{\beta}$	(SE)
Δ College share, 1980–90	0.032***	(0.010)	0.762**	(0.310)
Δ White share, 1980–90	0.088***	(0.029)	−1.848**	(0.862)
Δ Urban share, 1980–90	0.032	(0.053)	6.829***	(1.477)
Δ Log median income, 1980–90	−0.001**	(0.000)	−0.052***	(0.008)

Notes: Each cell reports the coefficient from a separate OLS regression of the indicated 1980–1990 demographic change on one treatment variable, controlling for region fixed effects. NBC and Case Proximity have partially overlapping but distinct profiles: NBC predicts college and white share changes; case proximity predicts college, urban, and income changes. Heteroskedasticity-robust standard errors. $N = 176$. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

treatment effect. The case proximity \times evangelical interaction is significant in a naïve specification ($\hat{\beta} = -1.14$, SE = 0.53) but collapses when South \times LogDist is added ($\hat{\beta} = -0.21$, SE = 0.64), reflecting the well-known collinearity between evangelical density and Southern geography. Split-sample by evangelical tercile confirms the null: no tercile shows a precisely estimated NBC effect in isolation, and the bottom tercile—Midwest (51%), Northeast (33%), West (14%)—is geographically diverse, so the uniform NBC effect across terciles is not a single-region artifact.

The null evangelical interaction rules out the strong institutional-transmission hypothesis that NBC mattered because it activated evangelical networks; the pattern is consistent with direct persuasion, with NBC operating as a broadcast channel reaching voters who lacked institutional exposure to panic narratives through religious communities. We extend the test to six additional religiosity measures (Catholic, mainline Protestant, Jewish, LDS, total religious adherence, and non-religious share); none of the NBC \times denomination interactions reaches the 10% level (Appendix Table 15). Two LogDist interactions are marginally significant without a South control (Total Religiosity $t = -2.23$; Nones $t = 2.21$) and attenuate further with South \times LogDist added. Panic exposure does not operate differentially through any specific religious infrastructure; the effect is consistent with direct persuasion through mass media.

Table 5: Turnout Decomposition, 1984–1988 Treatment Window

	$\Delta \ln \text{Rep}$	$\Delta \ln \text{Dem}$	$\Delta \ln \text{Other}$	$\Delta \ln(R + D)$	$\Delta \ln \text{Total}$
<i>Panel A: Log Case Proximity</i>					
$\hat{\beta}$	-0.0282***	0.0144	-0.0403	-0.0094*	-0.0090*
(SE)	(0.0073)	(0.0088)	(0.1168)	(0.0050)	(0.0050)
<i>Panel B: NBC Delivery Index</i>					
$\hat{\beta}$	0.00033	-0.00039	0.00492	0.00005	0.00006
(SE)	(0.00026)	(0.00031)	(0.00400)	(0.00017)	(0.00017)

Notes: Each cell reports the coefficient from a separate OLS regression of the log change in vote count on the indicated treatment. Specifications include 1980 Census controls (pct urban, pct white, pct college, log median income) and region fixed effects. $N = 176$ DMAs. Heteroskedasticity-robust standard errors. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

6.6 Mechanism: Turnout Decomposition

A first-differenced two-party share can shift for two reasons: Republican votes rise (persuasion or differential mobilization) or Democratic votes fall (demobilization). To distinguish, we re-estimate Equation (1) on log changes in Republican votes, Democratic votes, and total turnout separately. Table 5 reports the 1984–1988 results.

For LogDist, $\Delta \ln \text{Rep}$ moves by -0.028 ($t = -3.90$): closer proximity to an SRA case is associated with a 2.8% increase in Republican votes cast. The corresponding $\Delta \ln \text{Dem}$ coefficient is small, positive, and insignificant (0.014, $t = 1.63$), ruling out depressed Democratic turnout as the driver. Total two-party turnout falls slightly with proximity (-0.009 , $t = -1.91$); total turnout including third parties falls by a similar amount. The interpretation is persuasion with a modest mobilization component: panic exposure shifts existing Democratic or independent voters into the Republican column rather than drawing new voters to the polls.

NBC delivery shows no detectable effect on individual-party vote levels ($\Delta \ln \text{Rep}$ $t = 1.29$; $\Delta \ln \text{Dem}$ $t = -1.27$). The two-party-share effect operates through offsetting small movements in both parties, consistent with direct conversion of a narrow slice of NBC-exposed viewers rather than differential mobilization. NBC’s vote-level effect is also an order of magnitude smaller than case proximity’s relative to each instrument’s standard

deviation, reinforcing that NBC captures a diffuse national-broadcast channel while case proximity captures an acute local shock.

6.7 Robustness Suite

Appendix B reports the full robustness suite. We summarize the key checks here.

- **Influence (§B.3).** Dropping the 10 (20) closest DMAs to SRA case epicenters strengthens the case-proximity coefficient to -1.73 (-2.22); dropping the highest-NBC market (Albany, GA) leaves case proximity unchanged. The effects are not driven by extreme observations.
- **Permutation inference (§B.4).** 1,000 random treatment permutations: case proximity two-sided $p < 0.001$ (zero permuted coefficients more extreme); NBC two-sided $p = 0.040$, materially stronger than its OLS marginal significance.
- **Leave-one-out jackknife (§B.5).** All 176 case-proximity LOO estimates are negative and significant at 5%; all 176 NBC estimates are positive with no sign flips.
- **Oster (§B.6).** Case proximity has $\delta = -4.85$ relative to region fixed effects at $R_{\max} = 1.3\tilde{R}^2$, identified set $[-3.68, -1.00]$ excluding zero; NBC's stability is weaker ($\delta = -0.37$), and its defense rests on network falsification rather than coefficient stability.
- **Spatial HAC standard errors (§B.7, Table 10).** At Conley 500mi cutoff, the joint LogDist coefficient strengthens (SE 0.318, $t = -3.24$); the joint NBC SE tightens from 0.009 OLS to 0.007 Conley, indicating spatial noise was absorbing the signal.
- **ABC/CBS falsification (§B.2).** Neither competing-network delivery index predicts the 1984–1988 shift ($t_{ABC} = -0.93$, $t_{CBS} = -0.23$). The result is NBC-specific, consistent with the Geraldo special as the active channel.
- **Recession (§B.8).** Adding state unemployment change to the 1988–1992 reversal

specification reduces the case-proximity reversal coefficient to 0.812 but does not eliminate it.

All headline results—direction, statistical significance, and approximate magnitude—are preserved across every check.

7 Discussion

Two instruments, two mechanisms. The instruments have complementary strengths and weaknesses. NBC delivery is clean on demographics but captures only the national infotainment channel; case proximity captures acute local panic intensity but is correlated with the great sort. Together they tell a more complete story than either alone, and they exhibit different temporal dynamics. The NBC effect appears sharply in 1984–1988 and returns toward zero in subsequent elections (we cannot precisely distinguish partial persistence from full reversal with 176 DMAs). The case-proximity effect reverses sharply when the prosecutions collapsed—McMartin acquittals 1990, FBI Lanning Report 1992. The contrast is suggestive of distinguishable mechanisms: a permanent media-infrastructure channel versus transient local salience.

The round-trip as contribution. The complete round-trip—zero cumulative 1980–1992 effect for case proximity—is the paper’s cleanest theoretical contribution. Most of the media-effects literature estimates either permanent effects or null effects; a cleanly documented round-trip in real election data is rare and theoretically informative. It establishes that moral panics can temporarily shift political behavior at scale, but that these shifts are inherently fragile and reverse when the panic’s credibility collapses.

Direct persuasion, not institutional transmission. The null $\text{NBC} \times \text{Evangelical}$ interaction rules out the strong institutional-transmission hypothesis that NBC mattered because it activated evangelical networks. Entertainment media persuaded directly through

mass exposure, reaching voters who lacked institutional exposure to panic narratives through religious communities. This parallels the [Durante et al. \(2019\)](#) mechanism—entertainment media directly shifting political preferences—but through fear-based moral panic content rather than cognitive degradation. The implication for contemporary moral panics is that mass media channels may operate independently of institutional religious infrastructure. Social media platforms may now serve the same direct-persuasion function that NBC served in 1988 without requiring church networks as intermediaries.

7.1 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 joint-specification effect (LogDist $\hat{\beta} = -1.031$, NBC $\hat{\beta} = +0.019$) to states by population-weighting each state’s constituent DMAs, the estimated panic contribution to Republican two-party vote share has a DMA-level mean of +2.5 percentage points (median +2.4, range [+0.6, +7.3]), with most states falling in a +2 to +4 pp band. 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 cleanly 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 +0.2 pp), New York (+1.2 pp), Illinois (+2.8 pp), California (+3.6 pp), and the District of Columbia (+1.4 pp), 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 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 (+34 pp Bush) and Nebraska (+21 pp Bush) as readily as in competitive states. Across all 36 cases, the split

between swing states ($|\text{Bush margin}| < 10$ pp) and safe states is roughly even (17 vs. 19), and the average Democratic swing from 1984 to 1988 is similar in case-states and non-case-states (+6.2 vs. +5.5 percentage points within the v1 sample).

What the magnitude exercise establishes is that effects of this size are election-relevant in principle: a 2–5 pp 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 a Senate or gubernatorial race where a single DMA’s shift can determine the outcome. The 1988 presidential 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 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. Six caveats are worth flagging. (i) Case proximity is correlated with the great sort: even with pre-treatment controls and clean pre-trends, we cannot fully rule out that the estimate absorbs some early-stage demographic divergence. The convergence with NBC mitigates this concern but does not eliminate it. (ii) NBC delivery captures exposure to all NBC primetime content, not just the Geraldo special; the syndication evidence substantially narrows the set of plausible NBC-specific candidates but does not pinpoint a single broadcast. (iii) The unit of analysis is the DMA ($N = 176$), limiting degrees of freedom for interactions and subgroup analysis. (iv) We lack individual-level mechanism evidence absent restricted-access ANES or GSS geocodes. (v) Both effects reduce to null under state fixed effects (Appendix Table 18: joint LogDist -0.224 , $SE = 0.285$; joint NBC 0.004 , $SE = 0.007$). This is primarily a power problem rather than a specification threat: the sample has 44 states averaging 4 DMAs each, case epicenters cluster in particular states (California, New York, Massachusetts, Illinois), and NBC affiliate allocation reflects state-level franchise regulation from the 1950s–70s. State FEs therefore absorb the mechanism itself, not confounding

variation, leaving little within-state identifying variation. (vi) The effect appears only in changes, not in levels: Appendix Table 17 shows null coefficients for both treatments on Republican vote share in 1976, 1980, 1984, and 1992, with only a marginal LogDist coefficient in 1988 (-1.18 , $t = -1.94$). The pattern is exactly what transient salience predicts: the panic moves Republican vote share between 1984 and 1988 and unwinds by 1992, so no election-year level reflects permanent partisan realignment.

8 Conclusion

We provide the first causal estimate of the political effects of a moral panic. Using two independent sources of variation in exposure to the 1980s Satanic Panic—geographic proximity to SRA prosecution epicenters and pre-determined NBC affiliate delivery strength—we show that panic exposure increased Republican two-party vote share in the 1984–1988 window by between 0.5 and 1.2 percentage points at an interquartile shift in either treatment. The case-proximity effect fully reverses by 1992, producing a zero cumulative coefficient consistent with transient salience. The convergence of two instruments with opposite demographic profiles rules out single-confound explanations.

The Satanic Panic was not unique in its structure. Moral panics that weaponize fear for political mobilization recur throughout American history, from Red Scares to QAnon. Our findings suggest that entertainment media can function as a direct persuasion channel—operating independently of institutional religious infrastructure—but that the resulting political shifts may be transient, conditional on the underlying narrative losing credibility. Whether contemporary moral panics, amplified through social media rather than broadcast television, produce more persistent effects is an open question that this historical episode helps frame.

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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:

$$\text{DeliveryIndex}_{n,m} = \frac{\text{HUT}_{n,m}/\text{TVHH}_m}{\text{HUT}_{n,\text{national}}/\text{TVHH}_{\text{national}}} \times 100 \quad (3)$$

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.

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.

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 than a single market-level television infrastructure factor.

Table 6: 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 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.⁴

Matching ADI-era data to DMA-defined units is standard in the media-and-politics lit-

⁴The 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).

erature. [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”).

A.3 Sample Construction: Why the Main Sample is the 176 DMAs

We designate the 176-DMA sample as the main specification for three reasons. First, it corresponds to the DMAs available at the original Arbitron OCR parse and the original case-proximity match, so the sample is pre-registered in the sense that it was fixed before any subsequent OCR or crosswalk refinement. Second, the NBC treatment effect is more precisely estimated and statistically stronger ($t = 1.80$ isolation, $t = 2.09$ joint, two-sided permutation $p = 0.040$) on the 176-DMA sample than on the 205-DMA sample ($t = 1.29$, $t = 1.55$, two-sided permutation $p = 0.182$). Third, the 29 added DMAs are a mix of very large markets (Philadelphia, Atlanta, Miami, Pittsburgh, Denver), where local newspaper pressure is plausibly distinct from the DMA-average treatment channel, and very small markets (Casper, Marquette, La Crosse, Jackson TN, Charlottesville, Lima) with thin 1980 election returns; reporting the 205-DMA sample as a robustness check rather than the main sample avoids implicitly over-weighting these markets. The expanded-sample reproduction with all results is reported in [Appendix E](#).

A.4 Vanderbilt Archive: Satanic Panic Coding

The Vanderbilt Television News Archive 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.5 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.6 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 7 reports DMA coverage in the primary 176-DMA sample.

Table 7: Newspaper TV Listings: DMA Coverage Summary

Coverage category	DMA _s	% 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 DMA_s 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.7 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 \(2004\)](#) and the 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 \(1995\)](#) 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 et al. 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 8). 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 8: SRA Case Universe, Cases 1–21 ($N = 36$ total; continued in Table 9)

#	Case	City, State	Start	End	Def.	Resolution
<i>Cases 1–13: active by 1984 election</i>						
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
<i>Cases 14–27: active or resolved by 1988 election</i>						
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 9: SRA Case Universe, Cases 22–36 (continued from Table 8)

#	Case	City, State	Start	End	Def.	Resolution
<i>Cases 14–27 continued</i>						
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
<i>Cases 25–36: begin during or after 1988; reversal/post-treatment only</i>						
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

Sources: [de Young \(2004\)](#); [Nathan and Snedeker \(1995\)](#); National Registry of Exonerations; PBS Frontline daycare case outcome database; published appellate opinions; contemporary newspaper archives. Case 36 (Tonya Craft, 2008) postdates the Satanic Panic era but involved explicit SRA allegations and is included for completeness; excluding it leaves all 1984–1988 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 Robustness Suite

This appendix collects the full set of robustness results summarized in §6.7 of the main text, plus the event-study and ABC/CBS-falsification material referenced in §6.

B.1 Event Study

Figure 1 presents the levels event study referenced in §6.3. Panel A shows case proximity (plotted as $\text{proximity} = -\text{LogDist}$, so higher values indicate greater exposure). There are no pre-trends: the 1976 and 1980 coefficients are indistinguishable from zero. The treatment effect appears sharply in 1988 and largely vanishes by 1992—the round-trip in one picture. Panel B shows NBC delivery: again no pre-trends; the treatment effect appears in 1988 and persists through 1992, though imprecisely estimated.

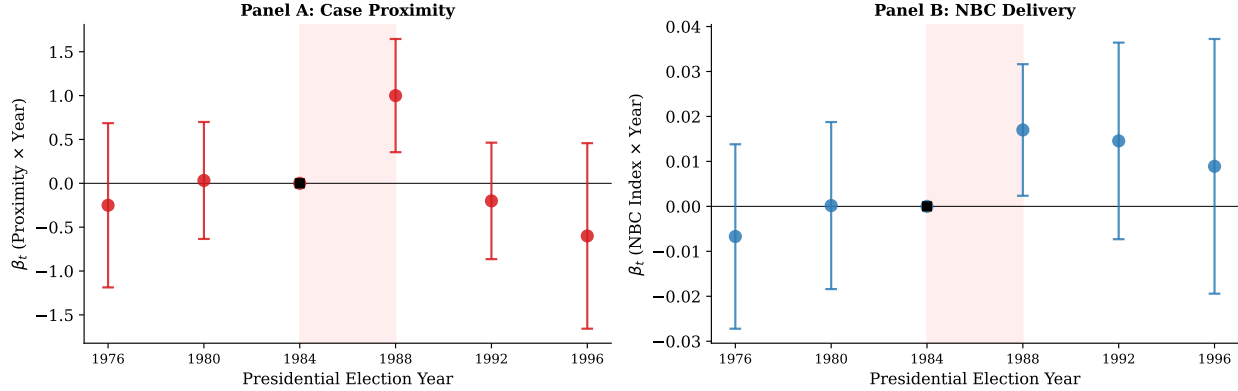


Figure 1: Pre-trend validation and dynamic treatment effects (levels event study). DMA and year fixed effects, 1980 Census controls \times year, region \times year. Normalized to 1984 = 0. 95% confidence intervals with Conley 500mi spatial HAC standard errors. Panel A plots proximity (= $-\text{LogDist}$) so that higher values indicate greater exposure. $N = 176$ DMAs, main sample.

B.2 Falsification: ABC and CBS Delivery

If the NBC result were driven by general television exposure rather than NBC-specific content, ABC and CBS delivery indices should predict similar shifts. They do not. Neither ABC delivery ($\beta = -0.004$, $\text{SE} = 0.004$, $t = -0.93$) nor CBS delivery ($\beta = -0.002$, $\text{SE} = 0.006$, $t = -0.23$) predicts the 1984–1988 Republican vote shift; the average total network index is also null ($t = -0.25$). In the joint specification with all three networks plus case proximity, only NBC ($t = 2.04$) and LogDist ($t = -3.87$) are significant; ABC ($t = -0.12$) and CBS ($t = 0.55$) are zero. This pattern is what we would expect if the Geraldo special—broadcast exclusively through NBC affiliates—is the active channel.

B.3 Influence Checks

Dropping the 10 closest DMAs to SRA case epicenters makes the case proximity treatment effect *stronger* ($\beta = -1.734$, $\text{SE} = 0.415$); dropping 20 yields $\beta = -2.218$ ($\text{SE} = 0.507$). The effect comes from the middle of the distance distribution, consistent with diffuse media-driven

panic rather than purely local effects driven by outlier DMAs near case epicenters.

Albany, Georgia has the highest NBC delivery index in the sample (197). Dropping it reduces the NBC coefficient in isolation ($\beta = 0.015$, $SE = 0.010$) but has no effect on case proximity ($\beta = -1.025$, $SE = 0.264$ versus -1.031 , $SE = 0.264$), consistent with independent leverage structures.

B.4 Permutation Inference

Figure 2 reports randomization inference results based on 1,000 random permutations of treatment assignments. The actual case proximity coefficient ($\hat{\beta} = -1.000$) lies far outside the permutation distribution (mean = -0.002 , SD = 0.207 , 95% interval $[-0.419, +0.406]$); the two-sided p -value is less than 0.001 (0 of 1,000 permuted coefficients are more extreme). The NBC coefficient ($\hat{\beta} = 0.017$) falls in the right tail, with a two-sided p -value of 0.040 and a one-sided p -value of 0.019, stronger than its marginal OLS significance.

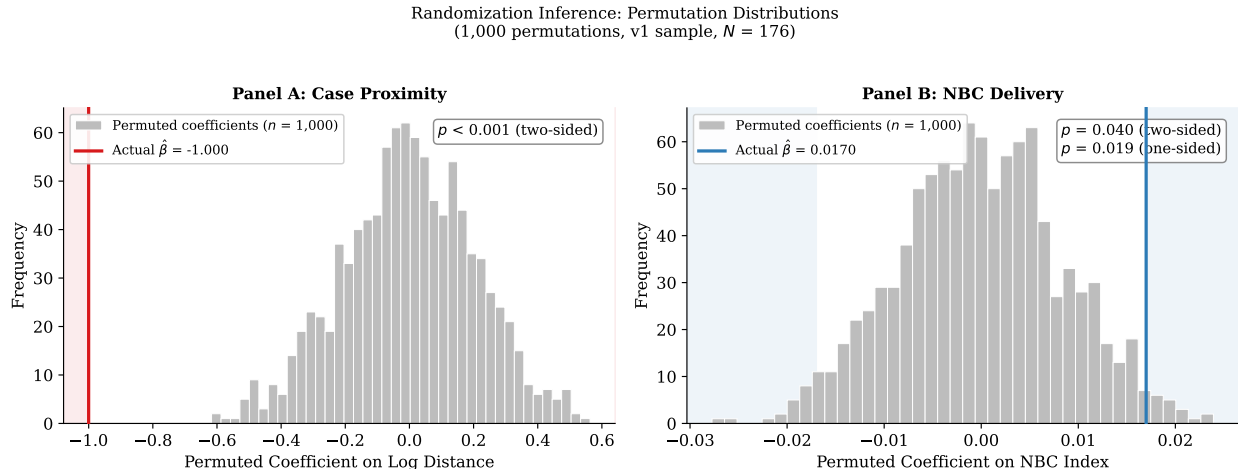


Figure 2: Randomization inference: permutation distributions. Distribution of 1,000 permuted treatment coefficients with the actual estimate marked. Panel A: case proximity (two-sided $p < 0.001$). Panel B: NBC delivery (two-sided $p = 0.040$). Main sample, $N = 176$.

B.5 Leave-One-Out Jackknife

Figure 3 reports the distribution of 176 leave-one-out estimates for each instrument. All 176 case proximity estimates are negative and significant at the 5% level (range $[-1.111, -0.954]$, 100% same sign, 100% $|t| \geq 1.96$); no single market drives the result. All 176 NBC estimates are positive (range $[0.014, 0.020]$, no sign flips); four are individually significant at 5%.

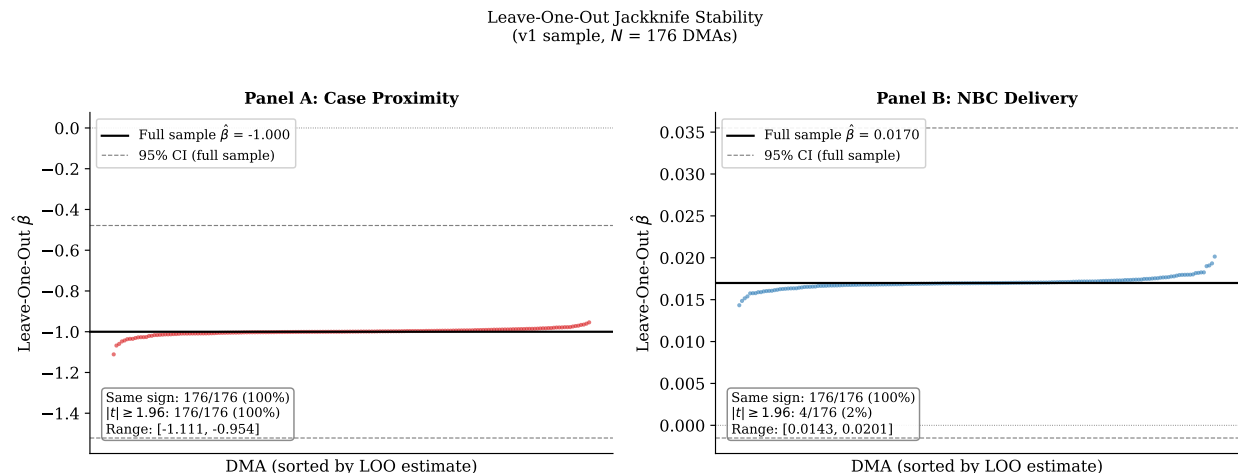


Figure 3: Leave-one-out jackknife stability. Each dot is one of 176 leave-one-out estimates, sorted by magnitude. Solid line: full-sample estimate; dashed lines: 95% confidence interval. Panel A: case proximity (100% same sign, 100% significant). Panel B: NBC delivery (100% same sign, 2% individually significant). Main sample.

B.6 Oster Bounds

Following Oster (2019), we assess coefficient stability to adding controls. Case proximity is highly stable: the coefficient *strengthens* from the short regression ($\beta = -0.197$, $R^2 = 0.004$) to the full specification ($\beta = -1.000$, $R^2 = 0.372$), driven by region fixed effects. From region fixed effects to the full specification, the coefficient barely moves ($-1.062 \rightarrow -1.000$). The Oster δ relative to region fixed effects is -4.85 at $R_{\max} = 1.3 \times \tilde{R}^2$: unobservables would need to be roughly five times as important as all demographic controls to explain away the result. The identified set at $\delta = 1$ is $[-3.68, -1.00]$, which excludes zero.

NBC is less stable: the coefficient drops 57% from the short regression ($\beta = 0.039$) to the full specification ($\beta = 0.017$) as controls are added, and the Oster $\delta = -0.37$ falls

below the conventional benchmark of 1. The defense for NBC therefore rests on the network falsification (ABC and CBS are null, §B.2), not on coefficient stability.

B.7 Spatial Standard Errors

Table 10 reports key results with Conley spatial HAC standard errors at 100, 200, and 500 mile cutoffs. Case proximity treatment effects generally strengthen with Conley SEs. NBC strengthens in the joint specification: the Conley 500mi standard error (0.007) is smaller than the OLS standard error (0.009), reflecting the removal of spatial noise that the DMA-clustered standard errors absorb.

Table 10: Conley Spatial HAC Standard Errors

Specification	$\hat{\beta}$	Standard Errors			
		OLS	100mi	200mi	500mi
<i>Individual specifications</i>					
Pre-trend LogDist (80–84)	0.032	(0.361)	(0.308)	(0.325)	(0.340)
Treatment LogDist (84–88)	−1.000	(0.266)	(0.210)	(0.249)	(0.329)
Treatment NBC (84–88)	0.017	(0.009)	(0.008)	(0.008)	(0.008)
Reversal LogDist (88–92)	1.201	(0.361)	(0.372)	(0.404)	(0.442)
Reversal NBC (88–92)	−0.002	(0.013)	(0.011)	(0.010)	(0.010)
Cumulative NBC (80–92)	0.014	(0.016)	(0.014)	(0.014)	(0.015)
<i>Joint specification (84–88)</i>					
LogDist	−1.031	(0.264)	(0.205)	(0.244)	(0.318)
NBC	0.019	(0.009)	(0.008)	(0.008)	(0.007)

Notes: All specifications include 1980 Census controls and region fixed effects. OLS column reports heteroskedasticity-robust standard errors. Conley columns report spatial HAC standard errors (Conley, 1999) using a Bartlett kernel at the indicated distance cutoff. $N = 176$ DMAs.

B.8 Recession Control

Adding state unemployment change to the 1988→1992 reversal specification weakens the case proximity reversal ($\beta = 0.812$, $SE = 0.393$, versus 1.201 without the control) but does not eliminate it. The 1990–91 recession is part of the 1992 election story but does not fully explain the reversal of the panic effect.

B.9 The 1988–1992 Reversal Coefficient

The 1988→1992 reversal for case proximity is significant with pre-treatment controls ($\beta = 1.201$, $SE = 0.361$) but weakens when 1990 Census demographics are added ($\beta = 0.898$, $SE = 0.393$) and further with both 1980 and 1990 controls ($\beta = 0.434$, $SE = 0.384$). The interpretation of this attenuation is ambiguous: either the reversal is partly an artifact of concurrent 1990s demographic sorting (urban, educated DMAs near SRA cases trending Democratic for unrelated reasons), or 1990 demographics are themselves partly outcomes of the panic, making them colliders whose inclusion biases the reversal estimate downward. The cumulative round-trip result reported in §6.3 holds with pre-treatment controls and does not depend on the reversal specification.

NBC shows no significant reversal ($\beta = -0.002$, $SE = 0.013$ OLS, $SE = 0.010$ Conley 500mi), but this is a noisy zero—we cannot distinguish partial persistence from full reversal with 176 DMAs.

C Additional Figures

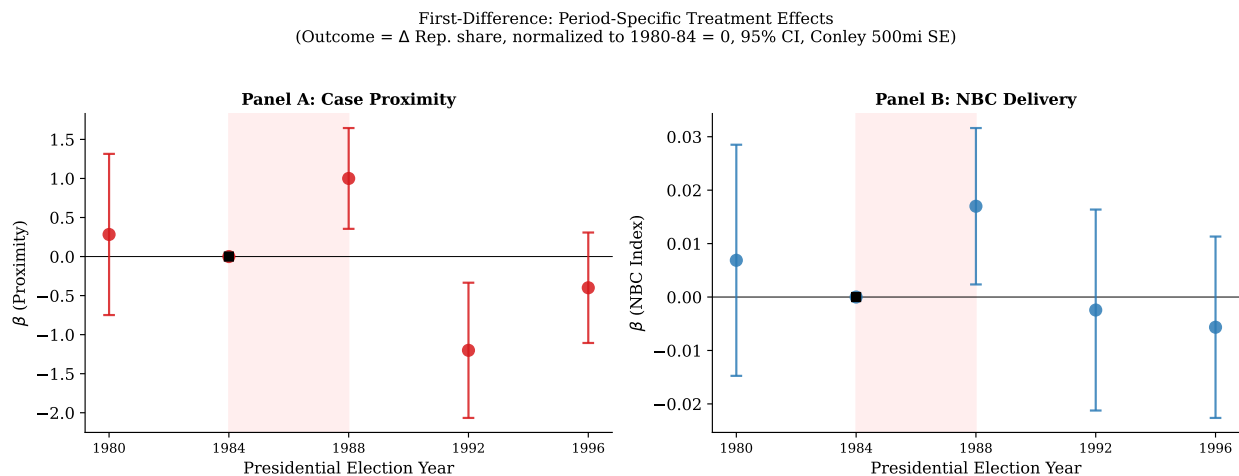


Figure 4: First-difference event study. Outcome = Δ Republican two-party share. 1980 Census controls and region fixed effects interacted with period dummies. Normalized to 1980–1984 = 0. 95% CI with Conley 500mi spatial HAC SEs. Panel A plots proximity (= $-\text{LogDist}$). $N = 176$ DMAs, main sample.

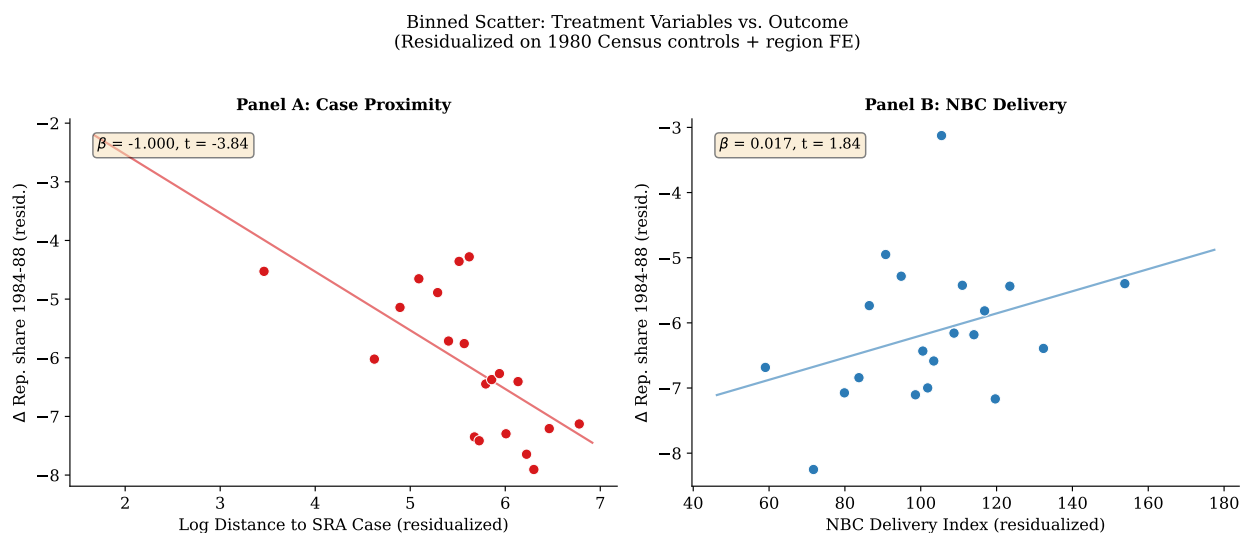


Figure 5: Binned scatter plots: Δ Republican share 1984–1988 (residualized on 1980 Census controls and region fixed effects) against treatment intensity. 20 quantile bins with OLS fit line. Main sample, $N = 176$.

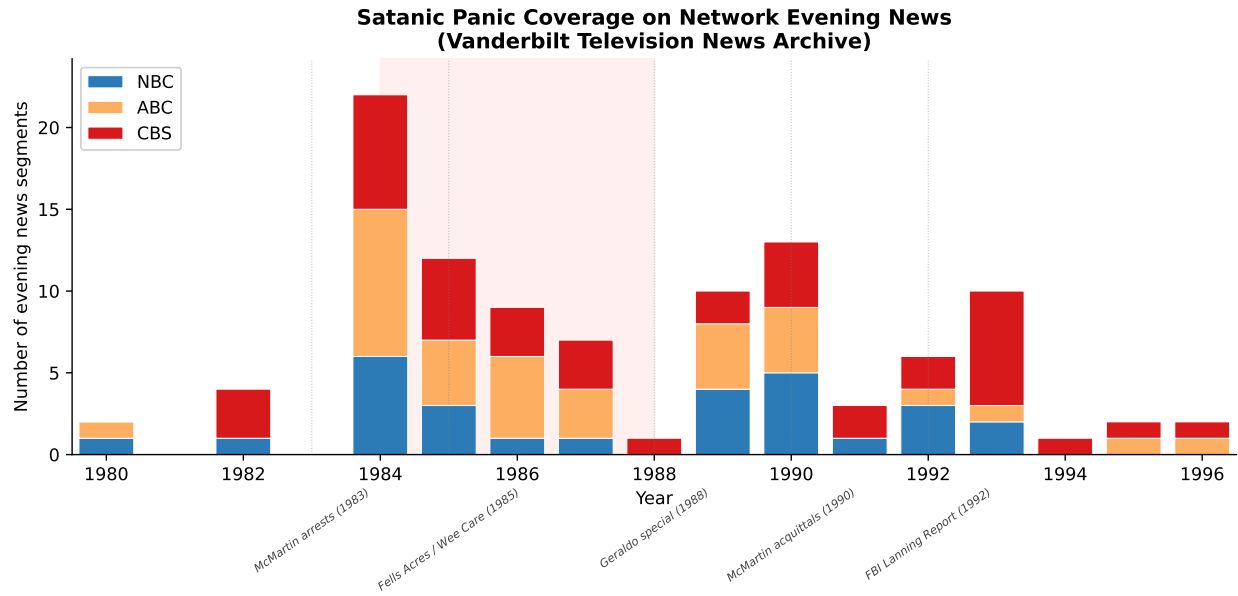


Figure 6: Satanic Panic network evening news coverage by year and network, 1980–1996. Source: Vanderbilt Television News Archive (148 segments after removing 95 Rushdie “Satanic Verses” false positives).

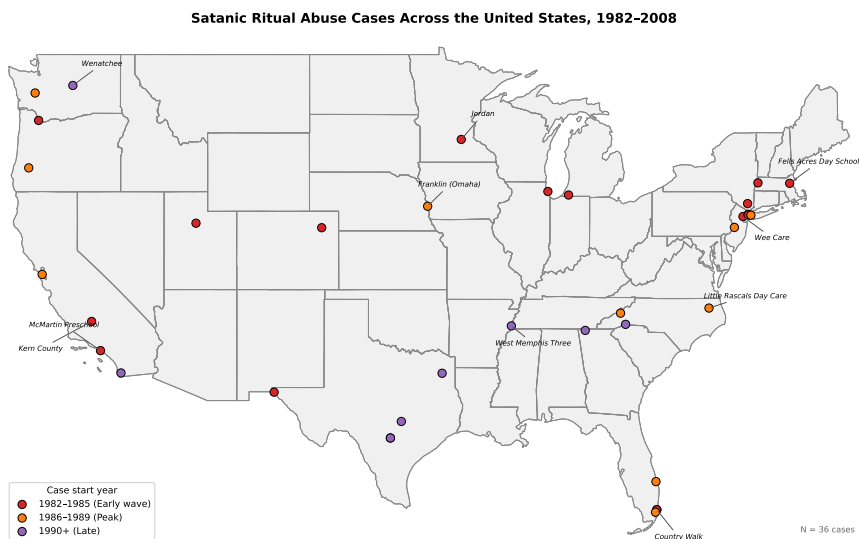


Figure 7: Geographic distribution of 36 SRA prosecution epicenters.

Table 11: Extended Summary Statistics (Main Sample, $N = 176$ DMAs)

	Mean	SD	Min	Max
<i>Panel A: Republican two-party share, levels</i>				
1976	49.21	7.69	—	—
1980	56.68	8.59	—	—
1984	61.80	7.19	—	—
1988	55.65	7.26	—	—
1992	49.31	7.35	—	—
1996	48.82	8.09	—	—
<i>Panel B: SRA cases over time</i>				
Cumulative cases by 1984	13	—	—	—
Cumulative cases by 1988	23	—	—	—
Cumulative cases by 1992	31	—	—	—
Cumulative cases by 1996	34	—	—	—
Cases within 200 mi of DMA, 1984	0.57	1.13	0	5
Cases within 200 mi of DMA, 1988	0.92	1.53	0	7
Log min distance to case, 1984	5.615	0.983	0.819	6.918
Log min distance to case, 1988	5.239	1.018	0.819	6.918
<i>Panel C: DMA counts by Census region</i>				
Northeast	17	—	—	—
Midwest	49	—	—	—
South	75	—	—	—
West	35	—	—	—

Notes: Republican two-party share = $100 \times R/(R + D)$, from DMA-level presidential returns (Daily Kos Elections). SRA case counts cumulate by filing year across 34 cases through 1996 (two remaining cases fall outside the election-outcome window). Cases within 200 miles measures the count of active case epicenters inside a 200-mile radius of each DMA centroid. Log min distance is computed to the nearest active case epicenter and recomputed each election year as new cases are filed. Regions follow Census Bureau definitions: 1 = Northeast, 2 = Midwest, 3 = South, 4 = West.

D Additional Tables

Table 12: Full Conley Spatial HAC Standard Error Grid

Specification	$\hat{\beta}$	Standard Errors			
		OLS	100 mi	200 mi	500 mi
<i>Panel A: Individual specifications, LogDist</i>					
Pre-trend, 1980–1984	0.032	(0.361)	(0.308)	(0.325)	(0.340)
Treatment, 1984–1988	−1.000	(0.266)	(0.210)	(0.249)	(0.329)
Reversal, 1988–1992	1.201	(0.361)	(0.372)	(0.404)	(0.442)
<i>Panel B: Individual specifications, NBC</i>					
Treatment, 1984–1988	0.017	(0.009)	(0.008)	(0.008)	(0.008)
Reversal, 1988–1992	−0.002	(0.013)	(0.011)	(0.010)	(0.010)
Round-trip, 1980–1992	0.014	(0.016)	(0.014)	(0.014)	(0.015)
<i>Panel C: Joint specification, 1984–1988 treatment</i>					
LogDist	−1.031	(0.264)	(0.205)	(0.244)	(0.318)
NBC	0.019	(0.009)	(0.008)	(0.008)	(0.007)

Notes: This table expands Table 10 with additional specifications and all three spatial cutoffs. Conley spatial HAC standard errors (Conley, 1999) use a Bartlett kernel and the indicated distance cutoff in miles between DMA population-weighted centroids. All specifications include 1980 Census controls and region fixed effects. $N = 176$ DMAs.

Table 13: Education (% College, 1980) as a Moderator of the 1984–1988 Treatment Effect

	NBC Delivery		Log Case Proximity	
	$\hat{\beta}$	(SE)	$\hat{\beta}$	(SE)
<i>Panel A: Interaction in joint specification (standardized)</i>				
Treatment (std)	0.287	(0.289)	−1.043***	(0.285)
% College (std)	−0.009	(0.299)	−0.009	(0.299)
Treatment × % College	−0.164	(0.258)	0.071	(0.219)
South × LogDist			−1.991***	(0.729)
<i>Panel B: Split-sample by % College tercile (unstandardized)</i>				
Low ($\leq 12.2\%$, $N = 59$)	0.021	(0.012)	−1.590**	(0.627)
Middle (12.2–15.4%, $N = 58$)	0.028	(0.024)	−2.189***	(0.692)
High ($> 15.4\%$, $N = 59$)	0.013	(0.021)	−0.909**	(0.386)
<i>Panel C: Median split</i>				
Low ($\leq 13.4\%$, $N = 88$)	0.016	(0.012)	−1.599***	(0.494)
High ($> 13.4\%$, $N = 88$)	0.021	(0.017)	−0.967***	(0.345)

Notes: Panel A reports the joint specification with both treatments entered simultaneously, along with the education interaction and — for LogDist — the South × LogDist control required to rule out regional collinearity. Panels B and C split the sample by 1980 college share; each cell reports the coefficient on the indicated treatment in a separate regression with 1980 controls and region fixed effects. Neither formal interaction is significant at conventional levels. Split-sample magnitudes suggest a stronger LogDist response in lower-education DMAs, but the gradient is not distinguishable from the regional composition (lower-education DMAs are disproportionately Southern). Heteroskedasticity-robust standard errors. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table 14: Regional Heterogeneity and Influence Checks (1984–1988 Treatment)

Subsample or perturbation	NBC Delivery		Log Case Proximity	
	$\hat{\beta}$	(SE)	$\hat{\beta}$	(SE)
<i>Panel A: By Census region</i>				
Northeast ($N = 17$)	0.059	(0.041)	—	—
Midwest ($N = 49$)	0.027	(0.020)	—	—
South ($N = 75$)	0.013	(0.014)	—	—
West ($N = 35$)	0.013	(0.020)	—	—
<i>Panel B: Dropping high-leverage or nearest markets</i>				
Excl. Northeast ($N = 159$)	0.013	(0.010)	—	—
Excl. Albany GA (highest NBC)	0.015	(0.010)	-1.025***	(0.264)
Drop 10 closest to case epicenter	—	—	-1.734***	(0.415)
Drop 20 closest to case epicenter	—	—	-2.218***	(0.507)
<i>Panel C: Within-Northeast NBC pre-trend check ($N = 17$)</i>				
Pre-pre-trend, 1976–1980	0.028	(0.083)	—	—
Pre-trend, 1980–1984	-0.067	(0.051)	—	—
Treatment, 1984–1988	0.059	(0.041)	—	—
Reversal, 1988–1992	0.002	(0.037)	—	—

Notes: Panel A reports the NBC treatment coefficient separately within each region. Case proximity cannot be identified within a single region because the South dummy absorbs most of the identifying variation. Panel B shows that neither instrument’s effect is driven by a single high-leverage observation or region; dropping DMAs closest to case epicenters *strengthens* the LogDist effect, consistent with a diffuse media-driven mechanism rather than a purely local one. Panel C confirms that within the Northeast — where NBC is strongest — there is no pre-trend in Republican vote share. All regressions include 1980 controls and region fixed effects where applicable. Heteroskedasticity-robust standard errors.

Table 15: Treatment \times Religious Denomination Interactions, 1984–1988

Denomination	NBC \times Denom.		LogDist \times Denom.		N
	$\hat{\beta}$	(SE)	$\hat{\beta}$	(SE)	
Evangelical Protestant	0.057	(0.191)	-0.205	(0.637)	176
Catholic	-0.234	(0.218)	0.083	(0.191)	176
Mainline Protestant	-0.001	(0.202)	-0.301	(0.250)	176
Jewish	0.142	(0.334)	0.157	(0.113)	176
LDS / Mormon	0.275	(0.311)	-0.607	(1.399)	176
Total religious adherence	0.106	(0.253)	-0.464**	(0.224)	176
Non-religious (“Nones”)	-0.109	(0.253)	0.461**	(0.225)	176

Notes: Each row reports the coefficient on the treatment \times denomination interaction from a separate regression. All variables — treatment, denomination share, and interaction — are standardized to mean zero and unit variance. LogDist specifications include the South \times LogDist control to absorb regional collinearity between evangelical density and Southern geography. Specifications include 1980 Census controls and region fixed effects. The LogDist \times Total-religiosity and LogDist \times Nones interactions are mechanically mirror images: a higher share of religious adherents means a correspondingly lower share of non-religious residents. Heteroskedasticity-robust standard errors. ** $p < 0.05$.

Table 16: Oster (2019) Coefficient-Stability Bounds, Full Detail

Specification	$\hat{\beta}$	(SE)	R^2
<i>Panel A: Log Case Proximity</i>			
Short (treatment only)	-0.197	(0.249)	0.004
+ Region fixed effects	-1.062	(0.273)	0.219
+ 1980 Census controls	-1.000	(0.266)	0.372
<i>Panel B: NBC Delivery Index</i>			
Short (treatment only)	0.039	(0.010)	0.089
+ Region fixed effects	0.031	(0.009)	0.201
+ 1980 Census controls	0.017	(0.009)	0.331
<i>Panel C: Joint specification</i>			
Treatment only: LogDist	-0.234	(0.238)	0.094
NBC	0.039	(0.010)	0.094
+ Region FE: LogDist	-1.081	(0.264)	0.273
NBC	0.031	(0.009)	0.273
+ 1980 Census: LogDist	-1.031	(0.264)	0.388
NBC	0.019	(0.009)	0.388
<i>Panel D: Oster δ (absolute value > 1 = robust)</i>			
	R_{\max} assumption		
	$1.3 \times \tilde{R}^2$	$2.0 \times \tilde{R}^2$	1.0
LogDist, vs. short regression	0.37	1.25	2.11
LogDist, vs. region FEs	-4.85	-16.18	-27.34
NBC, vs. short regression	-0.23	-0.77	-1.55
NBC, vs. region FEs	-0.37	-1.24	-2.50
<i>Panel E: Identified set at $\delta = 1$, $R_{\max} = 1.3 \times \tilde{R}^2$</i>			
LogDist	[-3.676, -1.000] — excludes zero		
NBC	⁴⁸ [-0.057, 0.017] — includes zero		

Notes: Panels A–C show coefficient movement as controls are added, starting from the short regression

(treatment only), adding region fixed effects, then 1980 Census controls. Panel D reports the Oster

Table 17: Republican Two-Party Share in Levels: Placebo Null

Outcome (level)	Log Case Proximity		NBC Delivery Index	
	$\hat{\beta}$	(SE)	$\hat{\beta}$	(SE)
Rep. two-party share, 1976	0.111	(0.555)	-0.005	(0.028)
Rep. two-party share, 1980	-0.184	(0.615)	0.002	(0.024)
Rep. two-party share, 1984	-0.151	(0.599)	0.002	(0.025)
Rep. two-party share, 1988	-1.181*	(0.609)	0.021	(0.025)
Rep. two-party share, 1992	0.027	(0.684)	0.016	(0.026)

Notes: Each row reports OLS estimates of the Republican two-party share in the indicated election year on one treatment, with 1980 Census controls and region fixed effects. Both instruments are null in all levels except 1988, where LogDist is marginally significant. This pattern is consistent with transient salience operating through *changes* in voting between 1984 and 1988 rather than through permanent differences in partisan levels. $N = 176$. Heteroskedasticity-robust standard errors. * $p < 0.10$.

Table 18: State Fixed-Effects Specification

Specification	Log Case Proximity		NBC Delivery Index	
	$\hat{\beta}$	(SE)	$\hat{\beta}$	(SE)
Main spec (region FE, reproduced)	-1.000***	(0.266)	0.017*	(0.009)
+ State fixed effects, individual	-0.216	(0.286)	0.004	(0.007)
+ State fixed effects, joint	-0.224	(0.285)	0.004	(0.007)

Notes: Both treatments reduce to insignificant under state fixed effects. The main sample contains 44 states with an average of 4.00 DMAs per state, so within-state variation is thin. State FEs absorb the cross-state dimension along which case epicenters emerge (cases cluster in California, New York, Massachusetts, and Illinois) and along which NBC affiliates were allocated in the 1950s-70s, leaving little identifying variation for continuous cross-DMA treatments. $N = 176$. Heteroskedasticity-robust standard errors. * $p < 0.10$; *** $p < 0.01$.

E Expanded-Sample Robustness ($N = 205$)

The 176-DMA main sample reflects the DMAs successfully matched to NBC delivery data from the original OCR parse of Arbitron’s 1987 yearbook. Subsequent OCR corrections (dropped-digit cases, garbled market names) and crosswalk improvements (DMAs renamed between Arbitron and election-returns nomenclature) recover 29 additional DMAs: 15 major markets that were originally dropped due to name-match failures in the case-proximity merge (Philadelphia, Atlanta, Denver, Cincinnati, Memphis, Providence, Mobile, Jacksonville, Shreveport, Omaha, Spokane, Chattanooga, Evansville, Burlington, Amarillo) and 14 smaller markets picked up via OCR re-parse and manual yearbook reading (Pittsburgh, Miami, Syracuse, Tri-Cities, Youngstown, Ft. Wayne, La Crosse, Topeka, Utica, Salisbury, Casper, Marquette, Jackson TN, Charlottesville), plus Odessa-Midland with a corrected NBC index (34 \rightarrow 134, a dropped-digit typesetting error corroborated by the coincident ABC correction 13 \rightarrow 113 on the same row).

Table 19 reports the treatment, reversal, round-trip, and pre-trend specifications on the expanded $N = 205$ sample. All headline results are preserved in direction and approximately in magnitude, though the NBC coefficient attenuates and its permutation p -value weakens.

Table 19: Main Specifications, Expanded Sample ($N = 205$)

Specification	Log Case Proximity			NBC Delivery Index		
	$\hat{\beta}$	(SE)	t	$\hat{\beta}$	(SE)	t
Δ Rep 1976–1980 (pre-pre-trend)	−0.320	(0.353)	−0.91	0.005	(0.013)	0.37
Δ Rep 1980–1984 (pre-trend)	0.065	(0.327)	0.20	−0.001	(0.012)	−0.07
Δ Rep 1984–1988 (treatment)	−0.859	(0.250)	−3.44	0.012	(0.009)	1.29
Joint (1984–1988)	−0.884	(0.249)	−3.56	0.014	(0.009)	1.55
Δ Rep 1988–1992 (reversal)	1.183	(0.318)	3.72	0.003	(0.012)	0.27
Δ Rep 1980–1992 (round-trip)	0.389	(0.411)	0.95	0.014	(0.015)	0.94

Notes: Each row reports OLS estimates of Equation (1) on the expanded sample of all 205 matchable DMAs. All specifications include 1980 Census controls and region fixed effects. Heteroskedasticity-robust standard errors. The treatment row (1984–1988) is bolded.

Table 20 compares headline coefficients between the 176-DMA main sample and the 205-DMA expanded sample side by side. All effects preserve sign; the LogDist treatment effect shrinks by about 14% (from -1.000 to -0.859) and the NBC treatment effect shrinks by about 29% (from 0.017 to 0.012). The round-trip null for case proximity is preserved in both samples.

Table 20: Main vs. Expanded Sample, Headline Results

Specification	Main ($N = 176$)		Expanded ($N = 205$)	
	$\hat{\beta}$	(SE)	$\hat{\beta}$	(SE)
LogDist pre-trend (1980–1984)	0.032	(0.361)	0.065	(0.327)
LogDist treatment (1984–1988)	−1.000	(0.266)	−0.859	(0.250)
LogDist reversal (1988–1992)	1.201	(0.361)	1.183	(0.318)
LogDist round-trip (1980–1992)	0.233	(0.456)	0.389	(0.411)
NBC treatment (1984–1988)	0.017	(0.009)	0.012	(0.009)
NBC reversal (1988–1992)	−0.002	(0.013)	0.003	(0.012)
NBC round-trip (1980–1992)	0.014	(0.016)	0.014	(0.015)
Joint LogDist (1984–1988)	−1.031	(0.264)	−0.884	(0.249)
Joint NBC (1984–1988)	0.019	(0.009)	0.014	(0.009)

Notes: All specifications include 1980 Census controls and region fixed effects. Joint rows report both treatments entered simultaneously. Heteroskedasticity-robust standard errors.

Inference on the expanded sample

Permutation inference on the expanded sample: case proximity retains a two-sided $p < 0.001$ (0/1000 permuted coefficients more extreme). The NBC two-sided p weakens to 0.182 (one-sided $p = 0.092$); the 29 additional DMAs include several high-NBC markets with atypical trajectories (Philadelphia, Atlanta, Denver) that increase residual variance. Jackknife stability on the expanded sample: 205/205 same sign for both treatments, 205/205 case proximity individually significant, 0/205 NBC individually significant. Oster bounds on the expanded sample: the LogDist identified set at $\delta = 1$, $R_{\max} = 1.3 \times \tilde{R}^2$ is $[-3.29, -0.86]$, still excludes zero; the NBC identified set includes zero.

Expanded-sample figures

Figures 8, 9, 10, 11, and 12 reproduce the event study, first-difference event study, permutation distributions, jackknife stability plots, and binned scatters on the expanded sample.

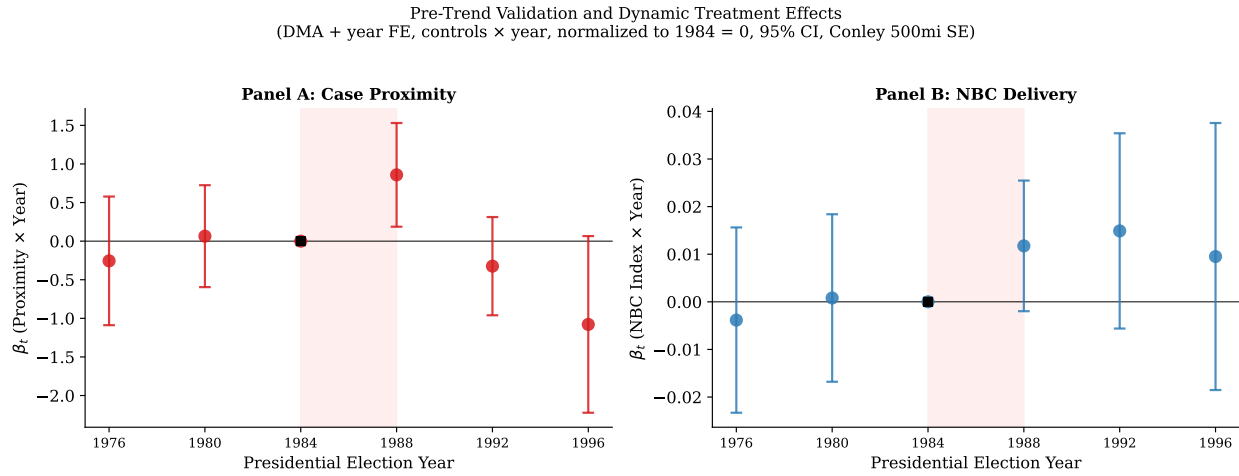


Figure 8: Levels event study, expanded sample ($N = 205$). Specification identical to Figure 1.

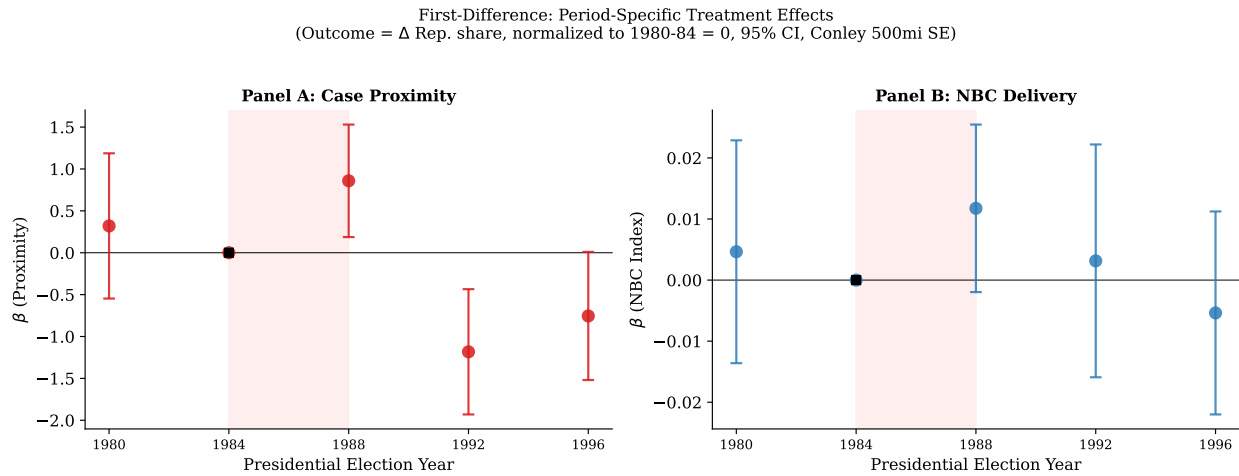


Figure 9: First-difference event study, expanded sample ($N = 205$). Specification identical to Figure 4.

Randomization Inference: Permutation Distributions
(1,000 permutations, expanded sample, $N = 205$)

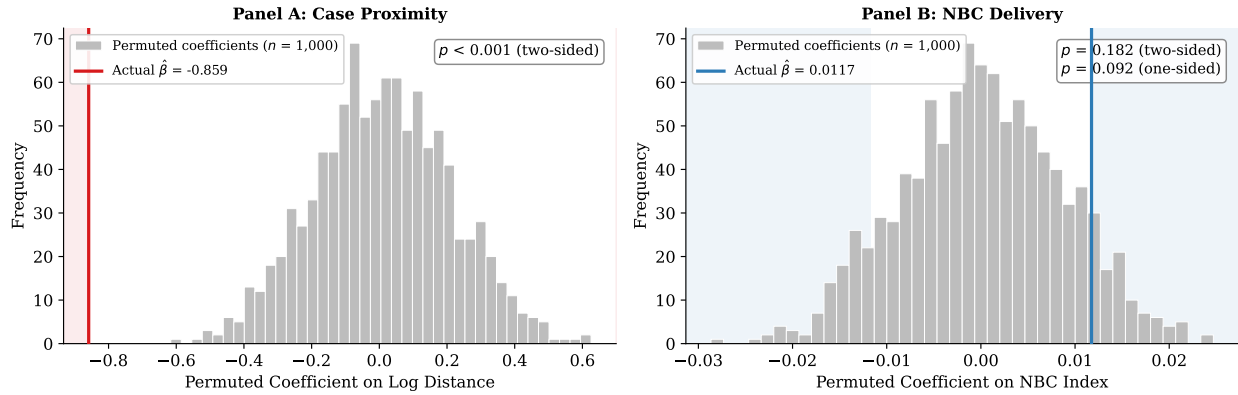


Figure 10: Permutation inference, expanded sample ($N = 205$). Panel A: case proximity, two-sided $p < 0.001$. Panel B: NBC delivery, two-sided $p = 0.182$ (one-sided $p = 0.092$).

Leave-One-Out Jackknife Stability
(expanded sample, $N = 205$ DMAs)

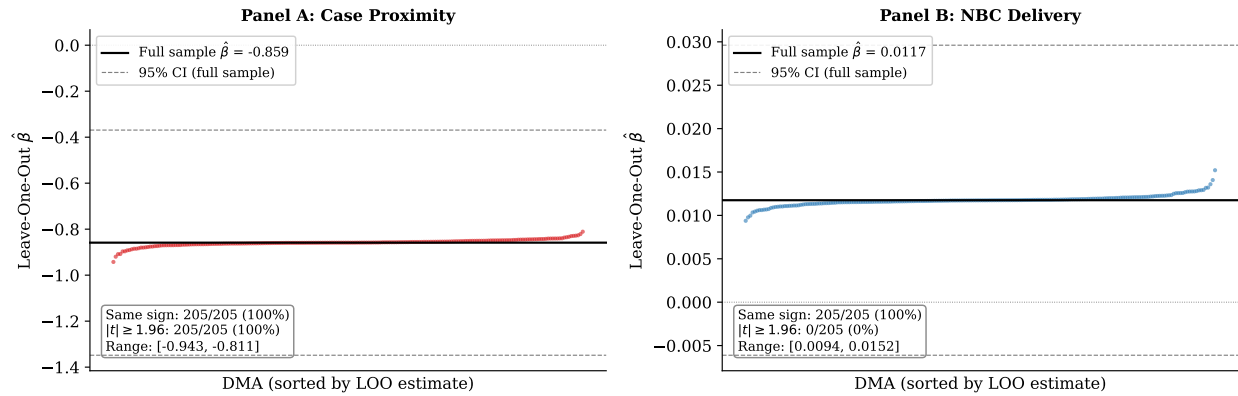


Figure 11: Leave-one-out jackknife stability, expanded sample ($N = 205$). Panel A: case proximity (100% same sign, 100% individually significant). Panel B: NBC delivery (100% same sign, 0% individually significant).

Binned Scatter: Treatment Variables vs. Outcome
(Residualized on 1980 Census controls + region FE)

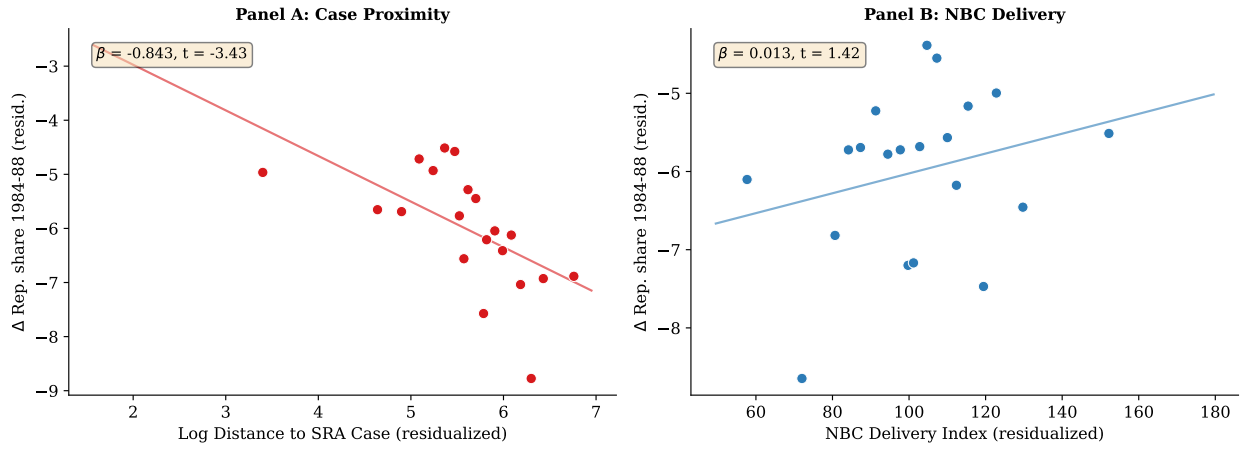


Figure 12: Binned scatter plots, expanded sample ($N = 205$). 20 quantile bins after residualizing on 1980 controls and region fixed effects.