ABSTRACT

Title of Dissertation: ESSAYS ON RACE
IN POLITICAL AND
PUBLIC INSTITUTIONS

William LeRoy
Doctor of Philosophy, 2025

Dissertation Directed by: Professor Ethan Kaplan
Department of Economics

This dissertation investigates the following features of liberal-democratic society in political and public institutions through the lens of race: the freedoms of speech and expression, the right to vote, and humane engagement with law enforcement.

The second chapter, "The Impact of Conservative Media on Southern Partisan Realignment", examines the most listened to pro-segregation radio broadcaster of the 1960s, Carl McIntire, to gauge conservative media's impact on Southern partisan realignment. I exploit irregular topographical contours as a source of exogenous variation in exposure to McIntire's political program. I incorporate these signal strength measures in a triple difference-in-differences design for estimation. A one standard deviation increase in predicted signal strength is associated with a 2.31 p.p. decline in Democratic vote share in the South. This effect corresponds to an 18% increase in overall realignment. His impact on realignment is driven by local ideological competition on preserving segregation.

The third chapter, "Voter Mobilization or Electoral Oversight?: Exploring Determinants of Black Re-Enfranchisement", analyzes Black political participation at the height of the Civil Rights Movement. While the Voting Rights Act of 1965 (VRA) is credited with the large-scale "re-enfranchisement" of Blacks following some 75 years of restrictive test devices, state-level registration rates call into question if the VRA alone is responsible for reigniting Black political participation. The political economy literature has conjectured that pre-VRA gains in black registration could be attributed to mobilization campaigns funded by the Voter Education Project (VEP). This chapter is the first to put this theory to the test. I estimate a triple difference-in-differences design that permits a statistical horse race between VEP, VRA, and VEP-by-VRA counties. Results indicate that the VEP was equally effective in bolstering Black turnout relative to the VRA from 1964 to 1980. Further suggestive evidence links pre-VRA gains to the VEP. White resistance to Black registration is predictive of long-run gains, but organization type is most associated with pre-VRA gains.

The fourth chapter, "Understanding Policing in the Aftermath of Gun Violence: Examining Investigatory Stops and Crime in Chicago", examines investigatory stops conducted by Chicago police officers following recent gun violence. I use a staggered difference-in-differences design to dynamically compare stops between neighboring police beats. In the immediate aftermath of gun violence, I find a rise in total stops that corresponds with a decline in index crime incidents and results in more seized weapons and/or contraband. Over the next month, officers continue to conduct additional stops, but these stops prove unproductive in yielding illicit goods, and seemingly have no impact on crime. Stops resulting in enforcement action persist three months after gun violence. Non-White stops in regions with considerable gang concentration account for much of the baseline rise in total stops.

The concluding chapter, "Insights into American Black Journalism: From the *Freedom's Journal* to the 21st Century", examines trends in Black print media from 1820 to 1997. Black media access accelerated in industrial Northern centers around the timing of the Great Migrations. Regional path dependency was deterministic of the rate of print publications over time: differences between smoothed South and Non-South rates never converged over this roughly 180 year window. Evidence suggests that Black authors wrote on a diverse set of subject matter. Finally, burgeoning literary rates in Black communities are surprisingly not correlated with the founding of new Black newspapers and periodicals.

ESSAYS ON RACE IN POLITICAL AND PUBLIC INSTITUTIONS

by

William LeRoy

Dissertation submitted to the Faculty of the Graduate School of the
University of Maryland, College Park in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
2025

Advisory Committee:
Professor Ethan Kaplan, Chair/Advisor
Professor Allan Drazen
Professor Peter Murrell
Professor John Wallis
Professor Patrick Wohlfarth

© Copyright by
William LeRoy
2025

Acknowledgments

The completion of this dissertation was only made possible with the collective support of professors, friends, and family.

First and foremost, I would like to thank my dissertation committee. I am forever indebted to my main advisor, Professor Ethan Kaplan, for generously dedicating time from his research to our biweekly meetings. These meetings were invaluable to my development as an economic researcher. Professor John Wallis profoundly shaped the way I read, think, and write about economic history. His reading group exposed me to a diverse range of texts that broadened my understanding of domestic and foreign institutions. Professor Allan Drazen helped refine my presentation skills with his insightful feedback in our Political Economy group, pushing me to communicate my research more clearly and effectively.

I would like to acknowledge the participants of the Political Economy group, the ECON 708 Microeconomics Workshop, my fellow graduate students, and the economics department administrators for their support. The Political Economy group provided a vital platform for feedback on works in progress. More advanced graduate students, such as Daniel Kolliner and Kartikeya Barta, led by example, setting a high standard and always offering guidance to less experienced researchers. I am especially grateful to my fellow cohort member Weizheng Lai, who has been a close confidant over the years. I thank Professors Peter Murrell and Patrick Wohlfarth for their willingness to serve as my fourth committee member and Dean's Representative, respectively.

I deeply appreciate the support of Graduate Student Coordinator, Vickie Fletcher, and Administrative Coordinator, Orpha Jewell. Their assistance in keeping track of program deadlines and submission dates for assistant professor positions was critical.

Finally, I would like to recognize the unwavering support of my family—my parents, Marvin and Cynthia; my brother, Michael; my sister-in-law, Hannah; and my grandmother, Barbara—who have been constant sources of encouragement, always willing to listen and serve as soundboards for my ideas. I dedicate this dissertation to my grandfather, Marvin, who passed away on August 24, 2020, during the week of first-year comprehension examinations. His memory and influence continue to inspire me every day.

Table of Contents

Acknowledgements ii

Table of Contents iv

List of Tables vii

List of Figures ix

List of Abbreviations xii

Chapter 1: Introduction 1

Chapter 2: The Impact of Conservative Media on Southern Partisan Realignment 5

2.1 Introduction . . . 5
2.2 Historical Background . . . 16
2.2.1 Right-Wing Media in the Civil Rights Era . . . 16
2.2.2 Carl McIntire's Media Empire . . . 18
2.3 Data . . . 21
2.3.1 Exposure to McIntire's *TCRH* . . . 21
2.3.2 Additional Data . . . 25
2.4 Empirical Design . . . 26
2.4.1 Identification Assumption . . . 26
2.4.2 Raw Data . . . 29
2.4.3 Baseline Specifications . . . 31
2.5 Results . . . 33
2.5.1 Baseline Estimates . . . 33
2.5.2 Pairwise Matching . . . 38
2.5.3 Robustness Exercises . . . 41
2.6 Mechanisms . . . 47
2.6.1 Ideological Competition on the Race Issue . . . 48
2.7 Conclusion . . . 56
2.8 Appendix . . . 58
2.8.1 Additional Tables and Figures . . . 58
2.8.2 Additional Primary Source Materials . . . 90
2.8.3 Additional Information on Calculating Signal Strength Measures . . . 109
2.8.4 Stylized Illustration of the Empirical Design . . . 113
2.8.5 Alternative Covariate & Fixed Effect Combinations . . . 115

2.8.6 Alternative Specification, Weighting, & Treatment Breakdown 119
2.8.7 Exploring Tennessee Counties in More Detail 122
2.8.8 Restricting the Sample to Confederate Counties 128
2.8.9 Is the Non-South Backlash Effect the Product of Regional Trends? 131
2.8.10 Additional Exercises Supporting Ethnic Competition Theory 134
2.8.11 Influence on Political Group Membership: John Birch Society & Americans for Democratic Action 139
2.8.12 Data Appendix 143

Chapter 3: Voter Mobilization or Electoral Oversight?: Exploring Determinants of Black Re-Enfranchisement 151
3.1 Introduction 151
3.2 Historical Background 157
3.3 Empirical Design 159
3.3.1 Data 159
3.3.2 Raw Trends 162
3.3.3 Main Specification 164
3.4 Results 167
3.4.1 Baseline Estimates 167
3.4.2 Robustness Exercises 169
3.5 Mechanisms 172
3.5.1 Intensive Margin in VEP Projects 172
3.5.2 "Big Five" Organizations 173
3.5.3 White Resistance 175
3.6 Conclusion 175
3.7 Appendix 177
3.7.1 Additional Tables and Figures 177
3.7.2 List of County-Level Voter Education Project Sites 190
3.7.3 Registration Data and Results 198

Chapter 4: Understanding Policing in the Aftermath of Gun Violence: Examining Investigatory Stops and Crime in Chicago 203
4.1 Introduction 203
4.2 Institutional Background & Empirical Design 208
4.2.1 Institutional Background 208
4.2.2 Empirical Design 209
4.3 Data Sources, Aggregate Trends, & Summary Statistics 214
4.3.1 Data Sources 214
4.3.2 Aggregate Trends 217
4.3.3 Summary Statistics 217
4.4 Main Specification, Baseline Results, & Heterogeneity 218
4.4.1 Main Specification 218
4.4.2 Baseline Results 220
4.4.3 Heterogeneity by Race 224
4.4.4 Heterogeneity by Gang Concentration 225

4.5 Conclusion . . . 228
4.6 Appendix . . . 232
4.6.1 Additional Tables and Figures . . . 232

Chapter 5: Insights into American Black Journalism: From the *Freedom's Journal* to the 21st Century 251
5.1 Introduction . . . 251
5.2 Data . . . 253
5.3 Results . . . 255
5.3.1 National, Regional, and Place-Level Trends . . . 255
5.3.2 Black Literacy Rates and Black Print Publications . . . 257
5.4 Conclusion . . . 259
5.5 Tables and Figures . . . 260

Bibliography 270

List of Tables

2.1 Covariate Balance Tests: Correlations Between McIntire's Predicted Signal Strength and Socio-Demographic Characteristics . . . 28
2.2 The Impact of McIntire's *TCRH* on Democratic Vote Share and Turnout . . . 34
2.3 How much did McIntire Contribute to Partisan Realignment? . . . 39
2.4 Differential Effects by Local Organization on the Race Issue: White Resistance (KKK) and Non-Discriminatory Public Amenities (Green Book Establishments) 53
2.A1 Comparing McIntire's *TCRH* Audience to Popular Programs . . . 63
2.A2 The Impact of McIntire's *TCRH* on Democratic Vote Share and Turnout: Pairwise Matching Minimizing Penalty Function using Cross Validation . . . 71
2.A3 The Impact of McIntire's *TCRH* on Democratic Vote Share and Turnout: Pairwise Alternative Matching ($\mu \geq 1$ and $\lambda \leq 0.5$) . . . 72
2.A4 The Impact of McIntire's *TCRH* on Democratic Vote Share: Exclude Southern States One-by-One . . . 78
2.A5 The Impact of McIntire's *TCRH* on Democratic Vote Share and Turnout: Assuming Survivorship of Stations with Missing Exit Dates . . . 80
2.A6 The Impact of McIntire's *TCRH* on Democratic Vote Share and Turnout: Expanded Definition of South . . . 81
2.A7 The Impact of McIntire's *TCRH* on Democratic Vote Share: Conley SEs . . . 83
2.A8 The Impact of *The Rush Limbaugh Show* on Democratic Vote Share and Turnout . . . 84
2.A9 Comparisons of Differential Effects by Local Organization on the Race Issue: White Resistance (KKK) and Non-Discriminatory Public Amenities (Green Book Establishments) . . . 86
2.A10 Differential Effects by Local Organization on the Race Issue: White Resistance (KKK) and Black Voter Mobilization Projects (VEP) . . . 88
2.A11 Comparisons of Differential Effects by Local Organization on the Race Issue: White Resistance (KKK) and Black Voter Mobilization Projects (VEP) . . . 89
2.A12 The Impact of McIntire's *TCRH* on Democratic Vote Share: Using Predicted Signal Strengths with Antenna Heights set to 100 Meters . . . 112
2.A13 Double Selection Lasso: The Impact of McIntire's *TCRH* on Democratic Vote Share . . . 118
2.A14 Alternative Specification and Weighting: The Impact of McIntire's *TCRH* on Democratic Vote Share . . . 120

2.A15 The Impact of McIntire's *TCRH* on Democratic Vote Share: Only Confederate Sample . . . 129
2.A16 The Impact of McIntire's *TCRH* on Democratic Vote Share: Vary Non-South Sample . . . 132
2.A17 The Impact of McIntire's *TCRH* on John Birch Society and Americans for Democratic Action Membership . . . 141

3.1 Summary Statistics - Turnout, Registration, and Socio-Demographics . . . 163
3.2 The Impact of the VEP and VRA by Percent Black on Log Turnout . . . 168
3.A1 Alternative Control Set: The Impact of the VEP and VRA by Percent Black on Log Turnout . . . 178
3.A2 Drop Future VEP: The Impact of the VEP and VRA by Percent Black on Log Turnout . . . 179
3.A3 Long DiD: The Impact of the VEP and VRA by Percent Black on Log Turnout 180
3.A4 Robust SEs and Wild Cluster Bootstrapped p-values: The Impact of the VEP and VRA by Percent Black on Log Turnout . . . 181
3.A5 Drop VEP Neighbors: The Impact of the VEP and VRA by Percent Black on Log Turnout . . . 183
3.A6 Total VEP Projects: The Impact of the VEP and VRA by Percent Black on Log Turnout . . . 185
3.A7 Big Five Organization: The Impact of the VEP and VRA by Percent Black on Log Turnout . . . 187
3.A8 Ku Klux Klan: The Impact of the VEP and VRA by Percent Black on Log Turnout . . . 189
3.A9 Raw Treatment Data: Counties with a VEP Mobilization Project, May 1962-1964 . . . 190
3.A10 The Impact of the VEP and VRA by Percent Black on Log Registration by Race . . . 202

4.1 Summary Statistics - Sample Beat-Weeks . . . 219
4.2 Responsiveness in Stops and Crime to Gun Violence . . . 223
4.3 Responsiveness in Stops to Gun Violence by Race . . . 226
4.4 Responsiveness in Stops and Crime to Gun Violence by Gang Concentration 229
4.A1 CPD-Identified Gang Membership in Chicago . . . 236

5.1 Top-25 Rankings of Number of Black Print Journalism by Place . . . 262
5.2 Top-25 Rankings of No. of Black Print Journalism by Subject . . . 264
5.3 The Impact of Black Literacy on the Creation of Black Print Publications per 100k, 1870-1930 . . . 267

List of Figures

2.1 Predicted ("Free-Space") Signal Strength of McIntire's *TCRH* 24
2.2 Examining the Raw Data: Democratic Vote Share & Signal Strengths 30
2.3 The Impact of of McIntire's *TCRH* on Democratic Vote Share: Event Study Plots 36
2.4 Organized White Resistence (KKK) and Non-Discriminatory Public Amenities (Green Book Establishments) in the South 50
2.A1 Regional Trends in Democratic Vote Share 58
2.A2 Visualizing Changes in Average Democratic Vote Share Pre- and Post-1964 . 59
2.A3 Regional Trends in Turnout 60
2.A4 Visualizing Changes in Average Turnout Pre- and Post-1964 61
2.A5 Right-Wing Media Organizations by Income 62
2.A6 Gallup Poll (9/1964) - Communists & Civil Rights Movement 64
2.A7 An FBI Letter Testifying to McIntire's Radio Persuasiveness 65
2.A8 Political Cartoons on the Civil Rights Movement in the *Christian Beacon* . . 66
2.A9 Stations' First and Last Year Airing McIntire's *TCRH* 67
2.A10 Stations Airing McIntire's *TCRH* in 1964 68
2.A11 Examining the Raw Data: Turnout & Signal Strengths 69
2.A12 The Impact of of McIntire's *TCRH* on Turnout: Event Study Plots 70
2.A13 Mean Ruggedness Distribution in the South 73
2.A14 Visualizing Southern Pre-1964 Average Democratic Vote Share and Changes in Average Democratic Vote Share Post 1964 74
2.A15 The Impact of of McIntire's *TCRH* on Democratic Vote Share and Turnout: Excluding Southern Counties in the Xth Percentile of Geographic Ruggedness 75
2.A16 Predicted (Free-Space) Signal Strength of McIntire's *TCRH* in the South . . 76
2.A17 The Impact of of McIntire's *TCRH* on Democratic Vote Share and Turnout: Exclude Southern States One-by-One 77
2.A18 Predicted (Free-Space) Signal Strength of McIntire's *TCRH* - Assuming Survivorship of Stations with Missing Exit Dates 79
2.A19 The Impact of of McIntire's *TCRH* on Democratic Vote Share: Distribution of Kernel Density Estimates from Randomization Inference 82
2.A20 Percent Black Distribution in the South 85
2.A21 Organized White Resistence (KKK) and Black Voter Mobilization Projects (VEP) in the South 87

2.A22 A Stylized Illustration of the Empirical Design . . . 114
2.A23 Alternative Covariate and Fixed Effect Combinations . . . 117
2.A24 Dropping Counties with Predicted Signal Strengths in Excess of Cutoff X . . . 121
2.A25 Mean Ruggedness Distribution in Tennessee . . . 124
2.A26 Visualizing Pre-1964 Average Democratic Vote Share and Changes in Average Democratic Vote Share Post 1964 . . . 125
2.A27 Predicted (Free-Space) Signal Strength of McIntire's *TCRH* in Tennessee . . . 126
2.A28 Dropping Tennessee Counties One-by-One by Signal Strength Rank (lowest to highest) . . . 127
2.A29 Re-estimating the DiD Effect over the Only Confederate Sample Dropping Tennessee Counties One-by-One by Signal Strength Rank (lowest to highest) 130
2.A30 Drop Non-South States One-by-One . . . 133
2.A31 First Four Random Klan-by-Green Book Assignments for Placebo Tests . . . 136
2.A32 Randomizing Klan-by-Green Book Assignments: Differential Placebo Effects by Local Organization on the Race Issue from 1000 Replications . . . 137
2.A33 Restricting the Sample by Pre-1964 Average Democratic Vote Share . . . 138
2.A34 McIntire's Influence on Americans for Democratic Action and John Birch Society Membership . . . 142

3.1 State-level Trends in Black Voter Registration . . . 153
3.2 Difference in Average Turnout Pre- and Post-1964 (Top) and Voter Education Projects, May 1962-1964 (bottom) . . . 161
3.3 Raw Trends in Log Turnout by Percent Black: Breakdowns by VEP, VRA, VEP-VRA, and Control . . . 165
3.4 Event Study: The Impact of the VEP and VRA by Percent Black on Log Turnout . . . 170
3.5 Mechanism Event Study . . . 174
3.A1 Voter Education Projects, May 1962-1970 . . . 177
3.A2 Neighboring Counties without Voter Education Projects, May 1962-1964 . . . 182
3.A3 Total Voter Education Projects, May 1962-1964 . . . 184
3.A4 Voter Education Projects with Big Five Organizations, May 1962-1964 . . . 186
3.A5 Voter Education Projects (May 1962-1970) and the Ku Klux Klan . . . 188
3.A6 Difference in Voter Registration Rates by Race: Non-White (Top) and White (Bottom) . . . 201

4.1 Illustration of Empirical Design: Beats 215 and 224 . . . 212
4.2 Raw Means in Stops by Treatment Status . . . 215
4.3 Responsiveness in Total Stops to Gun Violence . . . 221
4.A1 Breakdown of the Chicago Bureau of Patrol . . . 232
4.A2 Organizational Structure of the Chicago Police Department . . . 233
4.A3 Distribution of Maximum Pairwise Differences in Gun Victimizations . . . 234
4.A4 Distribution of Maximum Pairwise Differences in Gun Victimizations . . . 235
4.A5 Heat Map of Aggregate Stops . . . 237
4.A6 Heat Map of Gun Victimizations . . . 238
4.A7 Aggregate Trends in Stops and Gun Victimizations . . . 239

4.A8 Trimming the Sample by Maximum Pairwise Differences 240
4.A9 Police Beats only Receive Treatment or Serve as Controls 241
4.A10 Raw Means in Index Crime Incidents by Treatment Status 242
4.A11 Responsiveness in Stops by Type to Gun Violence 243
4.A12 Responsiveness in Crime by Type to Gun Violence 244
4.A13 Responsiveness in Stops by Race to Gun Violence 245
4.A14 Responsiveness in Wep/Contra Stops by Race to Gun Violence 246
4.A15 Gang Territory in Chicago 247
4.A16 Responsiveness in Total Stops by Gang Concentration to Gun Violence . . . 248
4.A17 Responsiveness in Wep/Contra Stops by Gang Concentration to Gun Violence 249
4.A18 Responsiveness in Total Crime by Gang Concentration to Gun Violence . . . 250

5.1 National Trends - Black Print Journalism 260
5.2 Regional Trends - Black Print Journalism 261
5.3 Superimposed Trends for Top-10 Places - Black Print Journalism 263
5.4 Superimposed Trends for Top-10 Subjects - Black Print Journalism 265
5.5 National and Regional Trends - Sample Black Literacy Rates 266
5.6 The Relationship between Black Literacy and Publications by Region 268
5.7 The Relationship between Black Literacy and Publications by Subject 269

List of Abbreviations

ADA	Americans for Democratic Action
CORE	Congress of Racial Equality
DDD	Triple Difference-in-Differences
DiD	Difference-in-Differences
FBI	Federal Bureau of Investigation
FCC	Federal Communications Commission
GB	Green Book Establishment
GOP	Grand Old Party (Republican Party)
GSS	General Social Survey
IRS	Internal Revenue Service
ITM	Irregular Terrain Model
JBS	John Birch Society
KKK	Ku Klux Klan
NAACP	National Association for the Advancement of Colored People
NUL	National Urban League
RNC	Republican National Committee
SCLC	Southern Christian Leadership Conference
SRC	Southern Regional Council
TCRH	*Twentieth Century Reformation Hour*
UAW	United Auto Workers
USCCR	U.S. Commission on Civil Rights
VEP	Voter Education Project
VRA	Voting Rights Act of 1965

Chapter 1: Introduction

A postwar liberal-democratic society is defined by political and public institutions that recognize the inalienable rights of its people. In the United States, these rights include, but are not limited to, an unabridged free press, unfettered access to voting polls, and a collective respect between law enforcement officials and local communities. These rights are protected through a collection of constitutional amendments, federal and state legislation, and Supreme Court jurisprudence.

This dissertation investigates these features of liberal-democratic society through the lens of race. In many ways, the development of American political and public institutions traces back to the peculiar institution of slavery, and the enduring struggle of the newly-freed Black population to reap the benefits of liberalism. This progression towards socio-political equality reached a tipping point during the Civil Rights Movement in the American South, which serves as the backdrop of the subsequent two chapters.

The battle over racial integration was a focal point of the Civil Rights Movement. The 1964 presidential contest featuring the sitting President, Lyndon Johnson, and Arizona Senator, Barry Goldwater, marked the first instance in recent memory where both federal party platforms embraced strong and opposing stances on racial integration. This watershed election is often attributed to the "Southernization" of the Republican Party (Kuziemko and Washington, 2018).

The second chapter, titled "The Impact of Conservative Media on Southern Partisan Realignment", argues that far-right radio personalities amplified this partisan transformation. Restricting attention to the most listened to conservative broadcaster of the decade, Carl McIntire, I find that his daily program contributed to nearly one-fifth of overall realignment. McIntire was most persuasive in Southern counties primed at the margin by added issue saliency tied to fierce competition between anti- and pro-integration organizations.

Calls for racial integration were not limited to public amenities. Political participation, especially in the American South, was defined by a lily-white registrar. When President Eisenhower sought re-election in 1956, as many as 80% of eligible Blacks remained unregistered. This statistic has its origin in legal barriers to the franchise, such as the all-white primary, literacy rates, and poll taxes, included in redrafted Southern state constitutions (circa 1890 to 1910) (Valelly, 2004). While Cascio and Washington (2013) attribute large turnout gains among eligible Black voters to the Voting Rights Act (VRA) of 1965, they suggest that a series of mobilization campaigns financed by the Voter Education Project (VEP) might also be a contributing factor. The third chapter, titled "Voter Mobilization or Electoral Oversight?: Exploring Determinants of Black Re-Enfranchisement" explores this possibility. I find suggestive evidence that pre-VRA gains in Black turnout are linked to VEP mobilization campaigns, particularly those organized by a "big five" organization (NAACP, SCLC, SNCC, CORE, or NUL). I also find that these mobilization campaigns were generally as effective as the VRA in expanding Black turnout from 1968 through 1980. White resistance (KKK chapters) to Black political participation seemingly generated unrealized gains in turnout.

While the 1960s was a decade of great social progress, Americans continue to question what a liberal-democratic society ought to be in the mid-21st century. An often-cited issue in

legacy media is how police officers interact with historically marginalized communities. The George Floyd incident, and the subsequent Black Lives Matter Movement, reignited debate over impending officer-civilian relations: what generates officer engagement with civilians?; how persistent is this engagement over time?; which communities are most affected?; and what claims can we make regarding social welfare? Chapter 4, titled "Understanding Policing in the Aftermath of Gun Violence: Examining Investigatory Stops and Crime in Chicago" considers these issues by analyzing discretionary-based policing outcomes (investigatory stops) following local shocks to gun violence. On the one hand, locally-targeted pedestrian and traffic stops in the aftermath of gun violence can increase confiscation rates of weapons and/or drugs, while also keeping crime rates in check. On the other hand, conducting more stops may yield no material benefit, disproportionately burden Black and Hispanic residents, and produce higher enforcement (arrest/citation) rates among these demographic groups. Apart from the week of shootings, I find that additional stops appear to lack material benefit and disproportionately target marginalized classes.

The second, third, and fourth chapters highlight various challenges to liberal democracy on the basis of race. These issues highlight the need for media representation "to petition the government for redress of grievances". Legacy media often denied Black journalists a forum to raise awareness for socio-political issues afflicting their communities. As a result, Black activists, e.g. Frederick Douglass, Ida B. Wells, W.E.B. Du Bois, Robert Abbott, and Robert Vann, launched their own print media outlets (Washburn, 2006). The fifth chapter, titled "Insights into American Black Journalism: From the *Freedom's Journal* to the 21st Century" tracks the rise of Black print media over nearly 180 years of American history. I find that industrial Northern cities around the timing of the Great Migrations were most responsible in precipitating Black

media access: smoothed Southern rates in print media never fully converge to Non-Southern rates from 1820 to 1997. Interestingly, expanded prospective consumer bases tied to rising literacy rates seemingly fail to capture the market entrance of upstart Black newspapers and periodicals.

This dissertation is dedicated to the historical and ongoing challenges to liberalism along racial lines in political and public institutions. While the future direction of our nation remains uncertain, the widely attributed adage that history tends to repeat itself often proves true. I can only hope that future readers are inspired by this work to preserve our liberal-democratic nature in pursuit of a more perfect union.

Chapter 2: The Impact of Conservative Media on Southern Partisan Realignment

> *"There was a moment when you could sit in your home or car, in downtown Dallas or suburban Philadelphia, tune in a single spot on the radio dial, and listen to anticommunist, anti-Civil Rights, anti-United Nations, anti-fluoridation, anti-Halloween (pagan holiday!), anti-mainline Protestant, anti-Earl Warren, anti-federal income tax, anti-Social Security, anti-JFK (or LBJ or Nixon), pro-Goldwater, and pro-Jesus programming all day long."*
>
> — Heather Hendershot, *What's Fair on the Air?*

2.1 Introduction

In the post-war era, media has not only served as a conduit between politicians and voters, but as an instrument to mold political beliefs and influence voting patterns. While researchers stress the importance of media in politics, its role in the "Southernization" of the Republican Party at the national level remains unexplored. This paper examines the impact of conservative media on Southern partisan realignment, defined as a shift in the 1960s from century's old one-party Democratic rule to (temporary) two-party competition.[1] The existing literature on partisan

[1]Figure 2.A1 plots regional differences in Democratic vote share between the South and Non-South from 1952 to 1980. In 1952, the average Southern county was 9.64 p.p. more aligned with the Democratic Party than the average non-Southern county. Southern counties' relative Democratic slant persisted in 1956 (7.35 p.p.) and 1960

realignment largely neglects the role of media. Instead, most research has attempted to resolve an ongoing debate between a race-based and economic-based narrative of realignment (Kuziemko and Washington, 2018).[2]

Advocates of the race-based narrative often draw from V.O. Key's *Southern Politics*, a text generally perceived as the backbone of Southern political scholarship (Kuziemko and Washington, 2015). Key (1949) chronicles the political development of the South with an emphasis on the planter elite's ability to mobilize a cohesive voting bloc against racial equality. He unmistakably identifies race as the pre-eminent political issue entering the second half of the century. Building on *Southern Politics*, Carmines and Stimson (1989) reason that Southern realignment was a consequence of racial issues emerging to the national forefront in the watershed 1964 presidential election. With exception in 1948, they claim that party platforms avoided hard-line stances on racial issues leading up to the Johnson-Goldwater divide.

Advocates of the economic-based narrative often point towards the South's gradual transition, preceding the Civil Rights movement, from an agrarian to an industrial economy as the principle catalyst of realignment (Shafer and Johnston, 2006). According to Wright (1986), minimum wage laws and unions put upward pressure on wages, which generated a shift from unskilled,

(1.15 p.p.) before a reversal in regional trends in 1964. The Johnson-Goldwater election signified a drastic shift in Southern voting, where former Confederate counties were 12.01 p.p. more likely to vote against the Democratic candidate. Nixon was the beneficiary of similar regional trends in 1968 (14.86 p.p.) and 1972 (11.38 p.p.). Southern realignment was momentarily suspended by Carter in 1976 and 1980. Figure 2.A2 visualizes the change in average Democratic vote share before (1952-1960) versus after (1964-1980) the 1964 presidential election. The Southeast trended most to the Republicans, the Northeast to the Democrats, and the West somewhere in between. For reference, I re-produce these figures for turnout per capita: see figure 2.A3 and 2.A4. The average Southern county experienced less political participation in each election, but the regional difference converged from 30.99 p.p. in 1952 to 7.67 p.p. in 1980.

[2]This literature often makes the distinction between "realignment" and "dealignment": the latter term merely indicates losses in Democratic Party identification, while the former is suggestive of subsequent gains in Republican Party identification. Kuziemko and Washington (2018) opt for "dealignment" over "realignment" since they use party identification as their main dependent variable. The current paper is interested in realized voting outcomes, which makes this distinction less meaningful. As a result, I use "realignment" throughout the paper.

labor-intensive production to mechanization. Shafer and Johnston (2006) argue that the per-capita income gains associated with industrialization formed right-leaning attitudes towards welfare that fully capture realignment. In a similar vein, Black and Black (1987) attribute realignment to the South's improved economic condition and stronger preferences towards a GOP platform consisting of tax cuts, economic growth, and polices aimed to rein in inflation.

Recent evidence favors the race-based narrative. Most notably, Kuziemko and Washington (2018) overcome a data-limitation bottleneck by combining Gallup Poll and GSS data on "a consistently worded question on racial views" surveyed from 1958 to 1980. They find that the white Southern exodus from the Democratic Party is solely attributed to conflicting racial attitudes. Yet, media's role in expanding partisan realignment remains unexplored. This paper aims to rectify this omission in the literature by raising the following questions: *to what extent did conservative media contribute to the decline of the Democratic South?; was conservative media's political impact limited to, or at least largely driven by, attitudes towards Jim Crow?; and was local ideological competition on the issue of racial integration, as proxied by the presence of pro- and anti-segregation organizations at the county-level, a determining feature of conservative media's political influence?*

There are two principal challenges to gauging right-wing media's impact on Southern realignment. First, media is expansive, not just in the number of operational outlets, but also in mediums (e.g. print, radio, television): it is unclear how to aggregate this information into a measure of media exposure. Second, even if measurement can be resolved, identifying a source of quasi-random variation to leverage for causal inference is a remaining issue.

With these concerns in mind, I restrict attention to the most listened to 1960s conservative radio broadcaster, Rev. Carl McIntire (Matzko, 2020b). McIntire launched a daily program,

Twentieth Century Reformation Hour (*TCRH*), at the height of McCarthyism to promote a far right-wing agenda consisting of the militant rollback of communism, laissez-faire economics, and American withdrawal from the United Nations (Ruotsila, 2015). As the Civil Rights movement entered the national conscience, McIntire used his platform to undermine the cause, which he viewed as a communist plot to expand the federal government's power (Ruotsila, 2015). In *What's Fair on the Air?*, Hendershot (2011) goes so far as to write: "Indeed, nowhere was protest against Civil Rights louder than on right-wing radio, especially on McIntire's *Twentieth Century Reformation Hour*." At the request of the RNC, McIntire endorsed Senator Goldwater's 1964 presidential bid. He used his radio program to further Goldwater's candidacy to an audience numbering 20 million "regular" listeners (Ruotsila, 2015).

Apart from his wide media footprint and strong pro-segregation slant, the available data on McIntire's program is conducive to an empirical design that permits causal inference. I pair data on radio stations carrying *TCRH* (Bazzi et al., 2023) with transmitter information (Corp., 1963) to quantify county-level exposure to the program leading up to the 1964 presidential contest.[3] Exposure is measured using Olken's modified Irregular Terrain Model (ITM) software. The software maps inputted transmitter information (e.g. power, frequency, and geocoordinates) to effective path loss measures determined endogenously by relative distance and exogenously by topographical irregularities: signals propagating through space from transmitters to receivers weaken with distance traveled and diffraction caused by non-smooth topography (e.g. mountains and the curvature of the earth). I subtract total effective path loss from transmission power to obtain predicted signal strengths at the county level. Then, I repeat this process isolating effective

[3]Most transmitter information is extracted from *Radio Annual Televion Yearbook (1963)*, but I also draw from additional sources to fill in missing information (Broadcasting, 1962, 1965, 1967, 1969, 1973; Corp., 1959).

path loss purely to the distance traveled channel. The latter measure is the predicted "free-space" signal strength, or the level of exposure absent topographical irregularities ("flat earth" exposure). By internalizing predicted "free-space" signal strengths constructed using only the endogenous determinant, the remaining variation in (overall) predicted signal strengths should be as good as random. I leverage this "leftover" exogenous variation for causal inference (Olken, 2009).

First, this paper estimates difference-in-differences (DiD) specifications to causally identify the impact of McIntire's program on voting outcomes: comparisons in voting outcomes pre- (1952-1960) and post-1964 (1964-1980) are drawn between counties with differences in predicted signal strengths generated by topographical irregularities. These specifications generate null results, but also smooth over clear regional trends in the raw data. In subsequent specifications, I permit regional differential effects (South vs. Non-South) to estimate the triple difference-in-differences (DDD) specification: comparisons are similar to before, but along regional lines. I find strong partisan realignment among the former Confederacy, and even a small backlash effect favoring the Democratic Party outside the South: a one standard deviation increase in exposure is associated with a 2.31 p.p. decline in Democratic vote share among Southern states, and a 0.87 p.p. gain elsewhere.

Event study dynamics show that the Southern decline is captured by an immediate, one-time drop in Democratic vote share in 1964 that persists at roughly the same magnitude up to the 1980 election. Since there remains a null effect on turnout in the DDD specifications, I rule out a potential mobilization effect that fully, or even partially, accounts for the Democratic declines in the South: in other words, McIntire's impact on vote share is indeed contributing to the more general partisan realignment phenomenon. I quantify his contribution by linking the baseline estimates to an 18% increase in realignment for the average Southern county.

I add robustness to these baseline results by estimating over an alternative specification that limits identifying variation to pairwise comparisons of counties that meet a set of matching criteria. In particular, I find all pairwise combinations, with replacement, of counties that would have had virtually the "same" level of exposure to McIntire's *TCRH* absent topographical irregularities, but due to these features, have vastly "different" predicted signal strengths. I use the parameter combination from cross validation techniques that minimizes the penalty function of this criteria. Estimating the alternative DDD specifications over this pairwise dataset yields similar results to the baseline case. These results also persist across a number of additional robustness exercises.

Evidence suggests that the baseline regional differentials are not simply driven by attitudes towards Jim Crow, but specifically ideological competition in the form of opposing local organizations on the race issue. Empirically, I decompose the Southern triple interaction into four different cases (pro-segregation organization, anti-segregation organization, neither organization, or both organizations), where each serves as a proxy for county-level temperature on segregation. I gather data on the third-wave of the Ku Klux Klan (KKK) (Ang, 2023) to capture pro-segregation organization; and data on Green Book establishments, or anti-discriminatory public amenities (Cook et al., 2022), for anti-segregation organization. I find that McIntire's realignment effect is almost entirely limited to Southern counties with the Klan and a Green Book establishment: a one standard deviation increase in exposure to his program is associated with a 5.37 p.p. decline in Democratic vote share. In the remaining Southern counties, including those with the Klan but no Green Book establishment, the baseline regional differential vanishes. These results hold when using pairwise comparisons, and when controlling for heterogeneous effects by the underlying black population.

For robustness, I repeat the exercises above using black voter mobilization projects instead

of Green Book establishments to represent anti-segregation organization. In particular, I restrict attention to the Voter Education Project (VEP): a sub-unit of the Southern Regional Council (SRC) based in Atlanta that raised and allocated funds to local chapters of the NAACP, CORE, SCLC, SNCC, and NUL, as well as smaller-scaled groups, to bolster Southern black suffrage (Faulkenbury, 2019). First, I digitize all mobilization projects through 1964 listed in Faulkenbury (2016): these projects allocated roughly $850,000 in funds ($8.78M in 2025 dollars) and resulted in nearly 700,000 black voter registrations (Faulkenbury, 2019). Then, I map all projects with sufficient information to the county level. Similar to the Green Book results, I find that only counties with the Klan and a black mobilization project are impacted by McIntire's *TCRH*. An additional standard deviation increase in these counties generates a 5.76 p.p. loss for the Democratic Party.

Additional robustness exercises add further support to local ideological competition as the leading mechanism. Randomly assigning Klan-by-Green Book status generates placebo results larger in magnitude than the actual results in only 6.7% of the replications. Moreover, consistent with local ideological competition, McIntire's Southern differential effect grows over sub-samples of increasingly competitive counties in average pre-1964 Democratic vote shares.

Since segregation was largely a regional issue that distinguished the South from the rest of the nation, it need not follow ex-ante that conservative media's realignment effect vary by county-level competition on the race issue. Nonetheless, calls to integrate the South jeopardized the existing status quo in some counties more than others: deep-South, upper-class whites were especially incentivized to preserve Jim Crow, while the Appalachia region, Texas, and Florida were comparatively less incentivized due to smaller black populations and/or diversified economies (Key, 1949). According to ethnic competition theory, efforts to integrate resistant counties should

heighten groups differences, increase issue salience, and generate group conflict (Cunningham, 2012, 2013). Rising issue salience can influence beliefs towards minority out-groups and political outcomes (Barrera et al., 2020; Bordalo et al., 2020; Colussi et al., 2021; Giavazzi et al., 2023). As a result, local ideological competition likely primed counties to differentially respond to pro-segregation messaging on the airwaves.

The main limitation of this paper is that I restrict the impact of conservative media to McIntire's program. Nonetheless, these results should be indicative of the aggregate effect for two reasons. First, conditional on airing far-right content, stations often programmed multiple political broadcasters (Hendershot, 2011). This means county-level exposure to McIntire internalizes a more general level of exposure to other conservative media figures. Second, McIntire's program constitutes a substantial share of conservative media, especially on radio, at the time of the 1964 presidential election. While data limitations prevent sweeping comparisons between McIntire's and his contemporaries' listenerships, annual income data is readily available and likely strongly correlated with audience size. Figure 2.A5 plots annual incomes for the top ten right-wing organizations by income in 1964.[4] By 1964, only the John Birch Society (JBS) rivals McIntire's *TCRH* in income, while both organizations individually accrue at least three times more than their contemporaries (e.g. William F. Buckley Jr.'s *National Review*, H.L. Hunt's *Life Line*, and Rev. Billy James Hargis' *Christian Crusade*). Since the JBS preferred bumper-stickers and billboards to radio, McIntire's *TCRH* was the clear-cut favorite for right-wing radio content (Hendershot, 2011).[5]

In fact, McIntire amassed a historically large listenership. In table 2.A1, I draw comparisons

[4] I gather this data by digitizing a September 1, 1964 report, *The Finances of the Right Wing* (Inc., 1964).

[5] In *What's Fair on the Air*, Hendershot writes: "The JBS only briefly experimented with radio, preferring more low-tech media projects such as bumper-sticker and billboard campaigns."

between McIntire's 20 million "regular" listeners and more recent political programs (Ruotsila, 2015). No current radio show matches McIntire's 20 million. The top five conservative talk radio shows by weekly listenership are the following: *The Sean Hannity Show* (4.40% (14.5M) of the total population); *The Mark Levin Show* (3.64% (12M) of the total population); *The Glenn Beck Program* (3.03% (10M) of the total population); *The Dan Bongino Show* (2.66% (8.75M) of the total population); and *The Mike Gallagher Show* (2.58% (8.5M) of the total population). The current audiences for Sunday morning political TV shows across major broadcasting corporations ($\approx 0.91\%$ (3M) of the total population) also pales in comparison. In terms of listenership, perhaps the best comparison to McIntire's *TCRH* is *The Rush Limbaugh Show*, which also plateaued around 20 million listeners in the 1990s. However, the per-capita comparisons are not particularly close: one in every nine Americans listened to McIntire compared to one in every twelve and a half for Limbaugh.[6]

To my knowledge, this paper is the first to empirically connect conservative media to Southern partisan realignment, and ultimately, the development of (temporary) two-party competition in the region. No other work of which I am aware has identified a Northern backlash effect to conservative media favoring the Democratic Party, or showed that conservative media's impact in the South is limited to counties with local ideological competition on the race issue. In fact, this paper is only the second empirical paper to examine the impact of this generation's conservative media figures on any political outcomes: Engist et al. (2024) find that conservative talk radio (Clarence Manion, Billy James Hargis, and Carl McIntire) generated an in-group

[6]Audience sizes for radio programs are sourced from Wikepedia: `https://en.wikipedia.org/wiki/List_of_most-listened-to_radio_programs`. The statistics for TV programs are gathered from Pew Research Center: `https://www.pewresearch.org/journalism/fact-sheet/network-news/`. For the conservative talk radio shows and Sunday morning political TV shows, I use the total population from the 2020 Census (329.5M) to calculate per capita audience measures; for McIntire, the total population from the 1960 Census (179.3M); and for Limbaugh, the total population from the 1990 Census (248.7M).

Protestant effect that mobilized an already existing Republican base fully responsible for declines in congressional Democratic vote share.[7] The authors neither connect conservative media to partisan realignment nor make any empirical claims concerning right-wing radio's unmistakable pro-segregation slant. In contrast, the current paper argues that it was conservative media's appeal to Jim Crow, not religion, that generated opposing presidential voting patterns strictly on regional lines and weakened Democratic footholds in the former Confederacy.[8]

This paper contributes to a diverse literature examining the impact of media on political economy. The most relevant papers among this literature study religious broadcasters, right-wing media, and the Civil Rights era. Apart from Engist et al. (2024), the current work most resembles Wang (2021a), who also restricts attention to an ecclesiastical broadcaster's (Father Coughlin's) impact on presidential elections. Father Coughlin characterizes an "Old Christian Right" defined by fascism, anti-semitism, and isolationism. In contrast, McIntire is a predecessor of the "New Christian Right" defined instead by anti-communism, and ultimately, a pro-segregation stance on racial issues.

By examining a 1960s conservative radio broadcaster, I further add to a literature on right-wing media investigating the staggered introduction of cable news (DellaVigna and Kaplan, 2007), "conservative slant" (Martin and Yurukoglu, 2017), local fiscal policy and preferences (Ash and Galletta, 2023), and agenda-driven film (Ang, 2023; Esposito et al., 2023). An additional paper closely related to this literature is Xiong (2021), who analyzes how Ronald Reagan's earlier years on apolitical television impacted his future vote share and campaign contributions.

[7]Similarly, Buccione and Knight (2024) find that Jerry Falwell's *Old Time Gospel Hour* realigned Evangelical voters to Ronald Reagan in 1980.

[8]There also exist a number of empirical design differences between the two papers. Engist et al. (2024) leverage the staggered introduction of stations at the county level, which treats media exposure as a binary and invites potential selection bias. In contrast, I use the modified ITM to causally identify on continuous, exogenous supply shocks in exposure to McIntire's *TCRH* that overcome this form of bias.

I also contribute to a literature investigating the role of media in the Civil Rights movement. While Wang (2021b) and Bernini (2023) explicitly analyze radio's impact on the movement, both papers focus on black radio programming: Wang (2021b) links this programming to black voter registration, NAACP chapters, state aid, and Civil Rights legislation, while Bernini (2023) identifies a connection to race riots.[9] Similar to Bernini (2023), Wasow (2020) explores how the dynamic between 1960s protesters and media influenced coverage.

This paper's emphasis on 1960s political radio also generalizes to a literature on historical media and politics. This literature includes the introduction of television in the United States (Gentzkow, 2006), state-independent television in the waning years of the Soviet Union (Enikolopov et al., 2011), radio and the spread of conflict in the Rwandan Genocide (Yanagizawa-Drott, 2014), radio following the Serbo-Croatian conflict (DellaVigna et al., 2014), radio during the Weimar and Nazi governments (Adena et al., 2015), and BBC radio's impact on Nazi resistance in Italy (Gagliarducci et al., 2020). For surveys on the literature, see DellaVigna and Gentzkow (2010), DellaVigna and La Ferrara (2015), and Enikolopov and Petrova (2015).[10]

This paper will proceed as follows. Section 2.2 provides historical background on right-wing media in the Civil Rights era and Carl McIntire's media empire. Section 2.3 describes county-level exposure to McIntire's *TCRH* and details additional data. Section 2.4 formalizes the empirical design, tests the identification assumption, explores the raw data, and outlines the baseline specifications. Section 2.5 includes baseline estimates, pairwise matching estimates, and a variety of robustness exercises that fail to invalidate these results. Section 2.6 considers local

[9] In a related paper, Wang (2022) finds that Black Southerners in 1960 were more likely to listen to radio than watch television.

[10] Recent work on media and politics has also focused on more modern media platforms: Gavazza et al. (2018) analyze broadband internet, local elections, and political policies in the United Kingdom; while Bursztyn et al. (2019) analyze social media, ethnic hate crimes, and xenophobia in Russia. For the literature on social media and internet, see Zhuravskaya et al. (2020).

ideological competition on segregation as a leading mechanism. Section 2.7 concludes.

2.2 Historical Background

2.2.1 Right-Wing Media in the Civil Rights Era

The origins of modern conservative media trace back to early non-interventionism. As war escalated in Europe, bipartisan commitment to neutrality gave way to demands for US involvement. Non-interventionists were ultimately shunned from party politics, and further ridiculed following American entrance into the global conflict. Disillusioned by establishment politics, a band of American First Committee members founded media outlets to launch a grassroots movement. In the post-war era, the forebears of right-wing media redirected their focus to opposing the United Nations, and ultimately, the spread of communism (Hemmer, 2016).[11]

Before the late 1950s, right-wing media was predominantly in print. In 1956, an estimated 310,000 individuals were subscribers to major right-wing weekly/monthly periodicals.[12] Radio airwaves were overwhelming concentrated in the hands of major broadcasting corporations with veto power over local programming that was too political or religious. The advent of television siphoned major broadcasting corporations' investment from radio, created a vacuum in the market, and lowered costs for airtime. Independent station owners, now constituting a majority of the radio industry, welcomed polarized broadcasters so long as they could generate cash flow. This market transformation unilaterally altered the relationship between media and far-right politics:

[11] Mainstream media's alleged claim of objective journalism was also central to the rise of right-wing media. Despite the industry standard of an impersonal, accurate reporting of the facts, many conservatives viewed mainstream journalists as mere left-wing ideologues who peddled their own talking points as truth. This perceived lack of industry representation motivated the birth of right-wing media outlets characterized by patently ideological reporting (Hemmer, 2016).

[12] This estimation is an upper-bound since it does not account for the possibility of individuals subscribing to multiple periodicals.

from near exclusion to mass outreach, twelve broadcasters were carried by at least 100 stations apiece in the early 1960s. Among this group, Carl McIntire's *TCRH* stood alone with some 20 million listeners (Matzko, 2020a).

The rise of right-wing radio was largely a Southern phenomenon that coincided with the peak of the Civil Rights movement. As a result, broadcasters leaned into pro-segregation discussion to bolster their audience and solicit financial contributions (Matzko, 2020a). They used their platform to suggest that the movement itself was a communist ruse to seize political influence (Hendershot, 2011). According to a September 1964 Gallup Poll, a majority of Southerners also held this position: 56.5% of respondents agreed that the Civil Rights movement was infiltrated by communists; 23.7% disagreed, and 19.8% were unsure (see figure 2.A6).[13] By championing segregation, Matzko (2020a) posits that right-wing broadcasters "eased the transition" of white Southerners to the Republican ranks during partisan realignment. He cites Truman's 1948 presidential platform as evidence that racial issues, absent right-wing radio, would not have politically transformed the South.

Right-wing broadcasters' political influence peaked in Barry Goldwater's primary victory in 1964. Growing weary of these broadcasters' influence, UAW organizer, Roy Reuther, sent Robert Kennedy a memo outlining strategy to limit their national outreach. Central to this strategy was using IRS audits to challenge right-wing outlets' tax exemption status and enforcement of the FCC's Fairness Doctrine (Matzko, 2020a). According to the Fairness Doctrine, stations must "serve the public interest", and permit alternative viewpoints to be expressed on air. The Kennedy and Johnson administrations wielded the doctrine not to strike media balance, but instead to

[13]The question from the Gallup poll reads: "And now, one more statement about the Communists: Most of the organizations pushing for Civil Rights have been infiltrated by the Communists and are now dominated by Communist trouble-makers. Do you agree with the statement or not?"

threaten station owners airing right-wing content with FCC fines and diminishing advertising revenue (Hendershot, 2011). These persistent attacks from left-wing insiders rendered this generation of far-right radio broadcasters a non-factor by the early 1970s. Gradual FCC turnover and deregulation, coupled with the rise of cable television, ushered in a new era of right-wing media featuring Jerry Falwell and Pat Robertson (Matzko, 2020a).[14]

2.2.2 Carl McIntire's Media Empire

In the early 1960s, Carl McIntire's *TCRH* garnered more listeners than any other right-wing radio show (Matzko, 2020a). Yet, his involvement in media and politics dates back at least to the mid-1930s. McIntire's principal interest in socio-political issues was motivated by the Federal Council of Church's attempt to redefine mainline Protestantism to better reflect an increasingly liberal, post-industrialized society. In opposition to the Council's efforts, McIntire launched a newspaper, the *Christian Beacon*, in 1936 with columns devoted to separatist fundamentalism, anticommunism, and decentralized government (anti-New Deal). Shortly thereafter, he founded the American Council of Churches (1941) and International Council of Churches (1948) to further promote right-wing politics (Ruotsila, 2015).

McIntire's rise to national prominence was directly related to the radio industry's evolving market conditions (Matzko, 2020a). Low-cost, accessible airwaves transformed *TCRH* from a locally-defined audience in the mid-1950s to tens of millions of listeners by the early 1960s

[14] In contrast to their predecessors, the "New Christian Right" were better organizers and political tacticians who ditched anticommunism for family values (Hendershot, 2011). Ronald Reagan's repeal of the Fairness Doctrine in 1987 generated a mass influx of conservative talk radio shows that built on Falwell's and Robertson's success (Matzko, 2020a). Most notably, deregulation catapulted Rush Limbaugh from relative obscurity in Sacramento to national syndication in 1988. With Limbaugh's 20 million listeners, the birth of Fox News in 1996 provided conservatives a cross-medium dual threat that defined much of the twenty-first century (Hemmer, 2016). See Amarasinghe and Raschky (2022) for an empirical analysis on Rush Limbaugh's impact on political outcomes.

(Matzko, 2020a; Ruotsila, 2015). With newfound national outreach, McIntire's entrance into Civil Rights discourse was marked by his on-air endorsement of Governor Barnett's attempt to bar James Meredith from the University of Mississippi in September 1962.[15] His efforts to preserve Jim Crow intensified leading up to the passage of the Civil Rights Act of 1964: McIntire would join the pro-segregation Mississippi State Sovereignty Commission and ridicule liberal pastors protesting with Martin Luther King Jr. in Selma and Birmingham (Ruotsila, 2015). He also used his platform to glorify staunch segregationist, Strom Thurmond, who was a personal friend and donor to the show (Matzko, 2020a; Ruotsila, 2015).[16]

McIntire's plea for preserving segregation, heard in roughly one-in-nine American households, helped shape public opinion on the race issue. In fact, one impassioned listener from Mississippi was so moved by the program's defense of Jim Crow that he wrote FBI Director Hoover on the perceived dangers associated with Southern integration. The following excerpt from the letter, dated March 21, 1965, recounts McIntire's on-air profession that black interests could ultimately disrupt white hegemony in the South:

> The negros [*sic*] never made this country what it is but they are going to try and ruin it if they are let go.
>
> I wish you would listen to Carl McIntire over the radio sometime. He lives in New Jersey. I've heard him really take up for you and that is the reason I think you could have lots of bearron [*sic*] on our government.
>
> Mr. Mcintire said the Negro wasent [*sic*] going to stop at taking Alabama & Miss.

[15] Certainly, McIntire mentioned the Civil Rights movement on-air prior to September 1962, just not consistently (Ruotsila, 2015).

[16] In bizarre fashion, McIntire even likened Thurmond's Senate filibuster of the Civil Rights bill to Moses guiding the Israelites in battle against the Amalekites (Matzko, 2020a).

[He] said they was [*sic*] let go they would take all of them.

I believe the Communist is backing them.[17]

McIntire's rebuke of the Civil Rights movement is evident in additional primary source material. Figure 2.A8 displays relevant political cartoons featured in the *Christian Beacon*. The first cartoon (panel a) likens the message of racial equality to the expansion of federal power: the illustration features an incarnate "Civil Rights 'Bill' ", armed with a "Federal Power Cudgel", molesting "Private Property". Underlying this political theme is the dehumanizing portrayal of blacks as apes who sexually assault white women. The subsequent cartoon (panel b) illustrates McIntire's belief that King's "so-called" nonviolent demonstrations often result in physical harm (Griffith Jr, 2009).

More primary source materials are included in subsection 2.8.2. I convert *TCRH* audio recordings to text files for the episodes airing immediately following the University of Mississippi riot. These episodes, from October 1-4, 1962, support Ruotsila's claim that McIntire used his platform to promote an anti-Civil Rights stance on issues such as school segregation and states' rights. Also included in this section are excerpts from FBI files connecting McIntire to the KKK: the Klan made documented efforts to invite him as a guest speaker, disseminate his writings, and cajole him into promoting their public events on his radio program.

McIntire eagerly threw his support and resources behind Goldwater's campaign in 1964. Dismayed by Nixon's nomination in 1960, McIntire finally saw a candidate on the national stage he could relate to politically: Goldwater was militantly anti-communist, anti-United Nations, and opposed to federal intervention to instigate racial change.[18] For this reason, McIntire devoted

[17] See figure 2.A7 to read the remainder of the declassified letter.

[18] McIntire could not see much difference between Nixon's and Kennedy's presidential platforms in 1960. He

airtime to promoting Goldwater's *The Conscience of a Conservative* to his 20 million listeners. His support for Goldwater also extended beyond radio: McIntire would publicly exalt Goldwater during the latter's campaign stop in New Jersey, and at the urging of the RNC, endorse the Arizona Senator by signing "a clergymen's petition" (Ruotsila, 2015).

By late 1964, McIntire was slowly stripped of his nation audience due to impending FCC threats: station owners preemptively dropped *TCRH* to avoid potential FCC fines and losses in advertisement revenue. Additional attacks from both the left and right permanently sullied his character: from the left, he was branded a racist, Klansman, and Nazi; from the right, he was tone-deaf and out of touch with an evolving Republican Party aiming to distance itself from former segregationists. For the remainder of his life, McIntire continued to jockey for political influence, but was often dismissed by those in power (Ruotsila, 2015).

2.3 Data

2.3.1 Exposure to McIntire's *TCRH*

In this paper, I identify McIntire's impact on presidential elections to understand the more general relationship between conservative media and Southern realignment. Since listenership data for McIntire's *TCRH* is limited to infrequently reported, nationally-aggregated statistics, I use county-level signal strength measures of the program leading up to the contentious 1964 presidential contest as a proxy. I follow a robust literature on political economy and media to obtain these measures. First, I gather extensive data on the stations carrying McIntire's program

disliked Nixon for his moderate politics and his relationship with Billy Graham, whose neoevangelical ideals ran counter to McIntire's separatist inclinations (Ruotsila, 2015). Similar to Goldwater, McIntire's pro-segregation stance was driven more by his aversion to expanded centralized power than any personal racial animus. Of course, he was also motivated by his large Southern viewership and ability to solicit financial contributions (Matzko, 2020a).

from Bazzi et al. (2023): the data includes station callsigns, transmitter geo-coordinates, and transmission power. Then, I scrape the *Radio Annual Television Yearbook (1963)* for radio frequencies and merge by callsign with the existing data.[19] After dropping a trivial number of stations with missing frequencies and/or zero wattage, 539 stations remain. I also employ GIS software to calculate county centroids, which serve as my receiver geo-coordinates.

The main data limitation is missing entries for stations' end dates airing McIntire's program. Figure 2.A9 displays stations' start and end dates in coverage of *TCRH*. Panel (a) shows a histogram and cumulative density function of station start dates: almost every station that carried the program (95.18%) was airing the content in 1964. Panel (b) displays a similar histogram for end dates that includes the 226 stations (41.93%) with missing entries: conditional on observable end dates, only 16 stations (5.11%) dropped the program prior to 1964. Since interest lies in quantifying McIntire's national footprint leading up to the 1964 presidential contest, I impute missing end date entries using the observable distribution. Panel (C) plots the histogram of imputed end dates with an overlaid cumulative density function. I only use stations with start (end) dates no later (earlier) than 1964 to calculate signal strength measures.[20] In subsection 2.5.3, I also show that the baseline results are robust to assuming full survivorship of stations with missing entries through 1964.

Using Olken's modified ITM software, I input the cleaned station data to calculate the effective path loss from electromagnetic signal propagation for each transmitter-receiver pair. There are two primary determinants of effective path loss: (1) the relative distance between transmitters and receivers; and (2) diffraction induced by irregular topographical contours (e.g.

[19] Since this yearbook only provides information on radio transmitters operating in 1963, not all stations carrying McIntire's program are included. I use a set of yearbooks around the timing of 1963 for stations not represented in *Radio Annual Television Yearbook (1963)* (Broadcasting, 1962, 1965, 1967, 1969, 1973; Corp., 1959).

[20] Figure 2.A10 plots the stations carrying the program that meet this criteria.

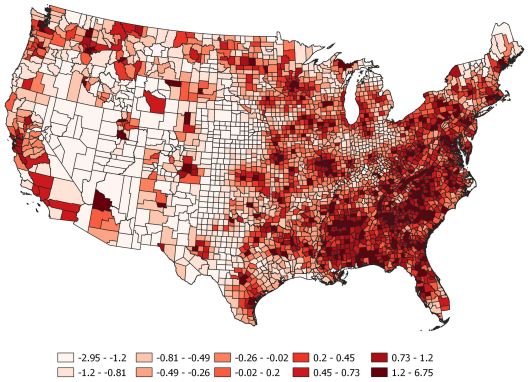
mountains) and the curvature of the earth (Olken, 2009). I difference out effective path loss from transmission power to obtain predicted signal strengths (in decibel-milliwatts). I follow equation (2.39) of Goldsmith (2005) for this calculation:

$$Signal_c = \frac{Power_c}{10^{\frac{EffectivePathLoss_c}{10}}}. \tag{2.1}$$

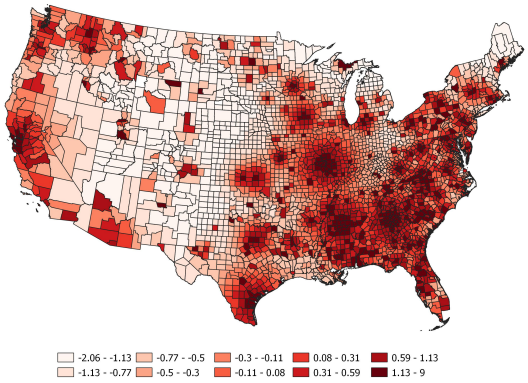
For each receiver, I use the maximum signal strength generated across the set of all transmitters. I subsequently standardize these values for estimation. In panel (a) of figure 2.1, I plot standardized predicted signal strengths at the county level. Counties shaded in dark (light) red have relatively stronger (weaker) signals. In general, exposure to McIntire's *TCRH* is greater in Southern counties east of the Mississippi River.

I repeat the process above using effective path loss solely attributed to distance. In other words, I re-calculate signal strengths using a counterfactual terrain with a perfectly smooth surface. Without diffraction, "free-space" effective path loss is proportional to the squared relative distance between transmitters and receivers (Olken, 2009). In panel (b) of figure 2.1, I plot standardized predicted "free-space" signal strengths. The proportionality rule produces a concentric pattern of a gradually declining signal moving away from "hot spots". Importantly, a side-by-side comparison of panels (a) and (b) highlights immediate differences between each type of signal strength. The spatial variation between predicted signal strength and predicted "free-space" signal strength is explained by diffraction. Since diffraction is induced by plausibly exogenous topographical irregularities, my empirical design depends on this "leftover" variation for causal inference.[21]

[21] In section 2.8.3, I derive equation (2.1) from Goldsmith's equation (2.39) in *Wireless Communications*. I also describe the application of Olken's modified ITM software in more detail.



(a) Predicted Signal Strength



(b) Predicted "Free-Space" Signal Strength

Figure 2.1: Predicted ("Free-Space") Signal Strength of McIntire's *TCRH*

This figure displays predicted signal strengths in panel (a) and predicted "free-space" signal strengths in panel (b) at the county level. Both measures are computed from inputting radio transmitter data into Olken's modified ITM software. Predicted signal strengths are calculated from differencing the effective path loss due to relative distance and irregular topographical features from transmission power. The "free-space" measures are calculated in a similar fashion, except effective path loss is restricted solely to relative distance. Hence, the spatial variation between each type of signal strength is attributed to topography, which is the source of plausibly exogenous variation I exploit for causal inference. See Olken (2009) for additional information on this empirical design.

2.3.2 Additional Data

My main dependent variable is county-level Democratic vote share from CQ Press Voting and Elections Collection. I am mostly interested in differences in vote share pre- and post-1964 given that the Johnson-Goldwater election was the first to be severely impacted by realignment. Hence, this paper defines the pre-period over the 1952-1960 elections, and the post-period over the 1964-1980 elections. From the same source, I gather total vote counts by election. I convert these counts into turnout per capita measures by dividing by the voting age population at the timing of the nearest decennial census. I use turnout per capita as a dependent variable to show that McIntire's impact on Democratic vote share is not completely, or even partially, driven by a mobilization effect: in other words, Democratic declines are due to voters realigning with the Republican Party as opposed to more conservative voters heading to the polls.

I collect additional data for covariates and robustness checks. This paper draws from the 1952 Survey of Churches and Church Membership, the 1960 Census (Haines, 2010), and the 1947-1977 County and City Data Book (Census, 2012) to source socio-demographic variables for covariate balance tests and regression controls. In the main specification with regional differentials, I "flexibly" (interacted by time and time-by-region) control for percent black (1960), percent high school (1960), percent Protestant (1952), percent unemployed (1960), and percent urban (1960). I gather average county-level elevation and ruggedness measures from Wang (2021a) to establish robustness against pre-existing correlations between geographic features and voting patterns. For an additional robustness exercise, I source station data on *The Rush Limbaugh Show* from Bazzi et al. (2023), and merge with FCC data to determine counties' exposure to the program using

Olken's software.[22]

This paper draws from the following data sources to explore potential mechanisms responsible for the baseline results: third-wave KKK chapters (Ang, 2023); anti-discriminatory public amenities in 1955 (Green Book establishments) (Cook et al., 2022); and black voter mobilization projects from May 1962 through 1964 (Faulkenbury, 2016). The data collection process for the last source requires digitization. Mobilization projects are also mapped to the county level for analysis. Projects defined at the state or regional (multi-county) level, as well as projects with missing information, are discarded.

2.4 Empirical Design

2.4.1 Identification Assumption

The primary challenge to identifying McIntire's influence on the American electorate is selection bias. First, station owners' decision to carry/drop *TCRH* could depend on their own political slant, and/or the political slant of the station's coverage zone: empirical evidence suggests that approximately one-fifth of the variation in media outlets' political slant is captured by variation in consumer slant (Gentzkow and Shapiro, 2010).[23] In this case, a county's relative proximity to stations airing the program would be an endogenous determinant of signal strength. Second, there could exist demand-driven bias due to right-wing consumers' greater likelihood of selecting into right-wing media content: empirical evidence suggests that voters with perfect ideological overlap with Fox News Channel have a 45% greater likelihood of selecting into the network relative to MSNBC (Martin and Yurukoglu, 2017). In this case, underlying political preferences

[22] The FCC data can be found here: `https://www.fcc.gov/media/radio/fm-query`.

[23] Gentzkow and Shapiro (2010) find that owners' political slant does not explain their station's slant.

would confound with my estimated impact of McIntire's program.

I use the modified ITM (and corresponding software) (Olken, 2009) to overcome these issues. As earlier mentioned, this model calculates effective path loss between station transmitters and receivers (county centroids) to determine predicted signal strengths. Critically, I isolate effective path loss to plausibly exogenous topographical irregularities and the curvature of the earth absent relative proximity to station transmitters. The effective path loss generated by these geographic features accounts for the differences between predicted signal strength and predicted "free-space" signal strength. Therefore, by internalizing the latter "free-space" measure, the remaining variation in predicted signal strength should be as good as random (Olken, 2009). This is my identification assumption. In theory, these supply-side shocks should be orthogonal to station owners' and consumers' decisions. See subsection 2.8.4 for a stylized illustration of the plausibly exogenous variation leveraged by the empirical design for estimation.

I conduct covariate balance tests for a wide range of socio-demographic variables to bolster support for the identification assumption and empirical design. The variables of interest span a diverse set of topics including breakdowns by population, gender, age, race, education, financial well-being, and religion. The covariates are captured cross-sectionally in 1960, with exception to percent radio (1950), percent church membership (1952), percent Catholic (1952), and percent Protestant (1952). Column (1) of Table 2.1 provides summary statistics for each variable. The average county was roughly one-tenth black, one-third urban, and one-third Protestant. Only around one-third of the population aged 25 years or older earned a high school degree. For each variable, I run univariate regressions on standardized predicted signal strength and cluster standard errors at the state level. Column (2) displays these results, while columns (3) and (4) report the corresponding p-values and R^2 statistics. The results show a statistically significant

Table 2.1: Covariate Balance Tests: Correlations Between McIntire's Predicted Signal Strength and Socio-Demographic Characteristics

	Mean (SD)	Uni. (Coef.)	Uni. (p-val.)	Uni. (R^2)	Mult. (Coef.)	Mult. (p-val.)	Mult. (R^2)
Log Total Pop.	9.934	0.425	0.000	0.075	0.203	0.002	0.305
	(1.222)	(0.063)			(0.063)		
Pct. Male	49.990	-0.471	0.000	0.048	-0.106	0.112	0.180
	(1.704)	(0.052)			(0.066)		
Pct. Age ≥ 65	10.621	-0.375	0.063	0.008	0.025	0.842	0.271
	(3.299)	(0.197)			(0.127)		
Pct. Black	9.806	5.403	0.000	0.069	0.933	0.195	0.519
	(16.235)	(1.102)			(0.710)		
Pct. Urban	32.063	4.357	0.000	0.015	2.488	0.038	0.139
	(28.375)	(0.919)			(1.162)		
Pct. Married no HH	2.119	0.520	0.000	0.091	0.065	0.172	0.512
	(1.361)	(0.068)			(0.047)		
Pct. High School ($\geq$ 25)	34.681	-2.977	0.000	0.045	-0.261	0.562	0.470
	(11.025)	(0.572)			(0.446)		
Pct. Unemployed	5.166	0.190	0.247	0.003	0.121	0.358	0.236
	(2.549)	(0.162)			(0.130)		
Log Med. Fam. Inc.	8.260	-0.009	0.694	0.000	0.014	0.587	0.196
	(0.513)	(0.023)			(0.025)		
Pct. Radio	92.462	-0.673	0.077	0.008	0.056	0.832	0.400
	(5.882)	(0.373)			(0.264)		
Pct. Church Mem.	45.250	-1.194	0.249	0.003	-1.737	0.150	0.252
	(18.004)	(1.022)			(1.189)		
Pct. Protestant	34.215	0.926	0.404	0.002	-1.352	0.092	0.291
	(16.392)	(1.100)			(0.785)		
Pct. Catholic	11.686	-1.333	0.224	0.004	-1.071	0.314	0.266
	(16.982)	(1.082)			(1.052)		

This table displays results from running covariate balance tests. Each row corresponds to a different covariate. The first column displays means and standard deviations; the second output from estimating univariate regressions between covariates and standardized predicted signal strength; the third and fourth p-values and R^2 measures from the univariate regressions; the fifth output from re-estimating univariate regressions while controlling for standardized "free-space" signal strength ($Free_c$) and state-by-year fixed effects (μ_{st}); and the sixth and seventh p-values and R^2 measures from the re-estimated regressions. I cluster standard errors at the state-level.

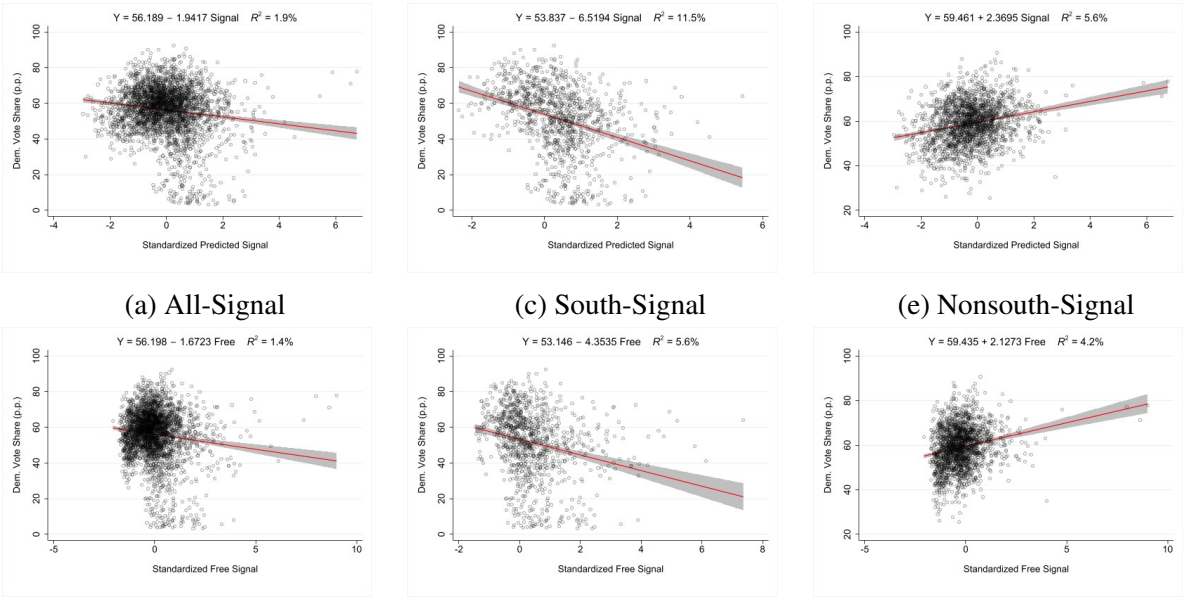
correlation between predicted signal strength and the log of total population, percent male, percent black, percent urban, percent of married couples without a household, and percent high school degree. However, once I control for standardized predicted "free-space" signal strength ($Free_c$) and state-by-year fixed effects (μ_{st}), only log of total population and percent urban remain significant: see columns (5)-(7) for these results. In other words, many of the pre-existing correlations are driven by relative proximity to transmitters and/or secular trends. The main specification includes county fixed effects to internalize these observed, as well as other unobserved, time invariant differences.[24]

2.4.2 Raw Data

Prior to formal estimation, I examine the raw data for suggestive evidence of a causal effect between McIntire's *TCRH* and Democratic vote share. Figure 2.2 displays six scatter plots of counties' Democratic vote shares (in 1964) and signal strengths with superimposed lines of best fit: each plot represents a different combination of region (U.S., Only South, Only Non-South) and signal strength type (predicted signal strength and predicted "free-space" signal strength). Panel (a) suggests that a one standard deviation increase in predicted signal strength is associated with a 1.94 p.p. decline in Democratic vote share in the typical U.S. county. However, panel (b) shows that most of this correlation is driven purely by the endogenous "free-space" variation that captures relative proximity to radio transmitters airing McIntire's program.

In panels (c) and (d), I restrict attention to Southern counties and find a 6.52 p.p. decline in vote share, where "free-space" variation yields only a 4.35 p.p. drop. Importantly, the 5.9

[24]These covariate balance tests are in the spirit of Wang (2021a). I mostly follow his choice of socio-demographic variables and mimic his tabular display of the results.



(a) All-Signal (c) South-Signal (e) Nonsouth-Signal

(b) All-Free (d) South-Free (f) Nonsouth-Free

Figure 2.2: Examining the Raw Data: Democratic Vote Share & Signal Strengths

This figure plots Democratic vote shares (1964) and signal strengths to McIntire's program. Each plot corresponds to a region (all U.S., South, Non-South) and signal strength type (predicted signal strength, predicted "free-space" signal strength) combination. Each scatter point denotes a Southern county's signal strength and vote share. I plot the lines of best fit (red) and corresponding 95% confidence intervals (gray). For each combination, I mathematically display the line of best fit and R^2 value at the top of each graph.

p.p. difference in R^2 values across specifications (11.5 % and 5.6 %) signals that there exists meaningful explanatory variation in exposure to McIntire's program excluding relative proximity to transmitters: in other words, variation in irregular topographical contours and the curvature of the earth and variation in relative proximity to transmitters are roughly equally explanatory. Unlike the entire U.S. sample, the raw data for Southern counties is highly suggestive of a causal link between McIntire's *TCRH* and Lyndon Johnson's vote share.

The remaining panels explore the raw data for Non-Southern counties. The positive sloping lines of best fit suggest a backlash effect to McIntire's pro-segregation messaging that runs counter to the strong Democratic losses in the South. These regional differentials highlight the need to permit South/Non-South interactions during formal estimation. The 2.37 p.p. Democratic gain is not as pronounced as the losses in the South, and also more indicative of a pure distance effect: the isolated "free-space" variation generates a 2.13 p.p. bump favoring Johnson.

Figure 2.A11 reproduces the six plots using turnout as the dependent variable. Irrespective of regional breakdown, the "free-space" variation is fully capturing the slope of the lines of best fit. This result serves as prima facie evidence against a mobilization effect.

2.4.3 Baseline Specifications

This paper is interested in causally identifying the impact of McIntire's *TCRH* on political participation and voting in presidential elections. I start by formalizing the main specification to estimate the average effect on a given U.S. county. This specification leverages differences in exposure, generated by the topography, between counties to capture differences, pre- and post-1964, in turnout and Democratic vote share. Since this specification smooths over regional

differentials, I later add a Southern interaction term to capture McIntire's impact in the South versus the Non-South. Since attitudes towards Jim Crow are largely shaped by regionality (South vs. Non-South), and the raw data yields regional effects in opposite directions, I designate the latter specification with the interaction term as the "preferred" specification.

First, I estimate the following specification over panel data covering the 1952-1980 elections to causally identify the impact of McIntire's *TCRH* on voting outcomes in a given U.S. county:

$$Y_{ct} = \alpha + \beta Post_t \cdot Signal_c + \gamma^{'} X_{ct} + \mu_{s(c)t} + \theta_c + \epsilon_{ct}, \tag{2.2}$$

where c denotes the county, t the election year, and s the state. The dependent variable (Y_{ct}) is either Democratic vote share or turnout per capita.[25]

Exposure to McIntire's program is captured by county-level standardized predicted signal strengths ($Signal_c$) and standardized predicted "free-space" signal strengths ($Free_c$). As earlier mentioned, the latter signal strength is calculated using an effective path loss measure solely attributed to relative proximity. Therefore, conditional on $Free_c$, the remaining variation in $Signal_c$ should be exogenously captured by irregular geographic features. I include $Post_t \cdot Free_c$ in X_{ct} for this reason, where $Post_t$ is a binary variable equal to one if the election year is 1964 or later. The coefficient on $Post \cdot Signal_c$ (β) is the parameter of interest and interpreted as follows: a one standard deviation increase in McIntire's predicted signal strength is associated with a β increase/decrease in the dependent variable. I control for the interactions by $Post_t$ of percent black (1960), percent high school (1960), percent Protestant (1952), percent unemployed (1960), and percent urban (1960) in X_{ct}. I control "flexibly" for these covariates to internalize

[25]The choice of temporal coverage is motivated by the political economy literature on the Civil Rights movement: panel data spans 1952 to 1980 in Cascio and Washington (2013); 1958 to 1980 in Kuziemko and Washington (2018); 1964 to 1980 in Bernini et al. (2023b); and 1956 to 1980 in Bernini et al. (2023a).

the county-level dynamics in socio-demographic features across time. State-by-year (μ_{st}) and county (θ_c) fixed effects are included to absorb secular trends at the state level and time invariant differences at the county level. Since I include the latter set of fixed effects, I do not control for "un-interacted" signal strengths or covariates: these measures would be collinear due to the fixed effects. The error term is ϵ_{ct}. I cluster standard errors at the state level.

Below, I add a Southern interaction term to equation (2.2) to measure regional differential effects in McIntire's exposure on vote share and turnout:

$$Y_{ct} = \alpha + \beta Post_t \cdot Signal_c + \xi Post_t \cdot Signal_c \cdot South_c + \gamma^{'} X_{ct} + \mu_{s(c)t} + \theta_c + \epsilon_{ct}, \quad (2.3)$$

where β now captures the Non-Southern effect and ξ the Southern differential. I also make the following additions to the regional specification: the triple interaction with predicted "free-space" signal strength ($Post_t \cdot Free_c \cdot South_c$) in X_{ct}; and post-by-South covariate interactions that permit the dynamics in socio-demographic features over both space and time.

2.5 Results

2.5.1 Baseline Estimates

Table 2.2 displays the results from estimating variations of equations (2.2) and (2.3) for Democratic vote share and turnout per capita. The table consists of two supercolumns, "Overall' and "Regional Differential", that correspond to each respective specification. Within each supercolumn, the first (latter) pair of columns reports results, with and without the covariate set, for Democratic

Table 2.2: The Impact of McIntire's *TCRH* on Democratic Vote Share and Turnout

	Overall				Regional Differential			
	(1) VS	(2) VS	(3) Turn	(4) Turn	(5) VS	(6) VS	(7) Turn	(8) Turn
Post $\cdot Signal$	0.045	0.150	0.210	-0.079	0.820**	0.871***	-0.153	-0.202
	(0.528)	(0.487)	(0.339)	(0.236)	(0.364)	(0.298)	(0.305)	(0.291)
Post $\cdot Signal \cdot South$					-2.198*	-2.313**	1.001	0.304
					(1.135)	(1.032)	(0.713)	(0.455)
County FEs	X	X	X	X	X	X	X	X
State-Year FEs	X	X	X	X	X	X	X	X
Free	X	X	X	X	X	X	X	X
Covariates		X		X		X		X
Mean	43.960	43.949	60.818	60.838	43.960	43.949	60.818	60.838
SD	15.257	15.229	16.041	15.994	15.257	15.229	16.041	15.994
Obs.	24,653	24,485	24,707	24,544	24,653	24,485	24,707	24,544

This table displays baseline results from estimating equations (2.2) and (2.3) across numerous specifications. The "Overall" supercolumn stores estimates from regressing equation (2.2). The first (second) pair of columns use Democratic vote share (turnout) as the dependent variable. Each regression internalizes county fixed effects, state-by-year fixed effects, and predicted "free-space' signal strengths. I re-estimate "flexibly" (post-period interactions) controlling for the set of covariates referenced in the main text: percent black (1960), percent high school (1960), percent Protestant (1952), percent unemployed (1960), and percent urban (1960). The "Regional Differential" supercolumn stores estimates from regressing equation (2.3). These columns are similar to the previous set of columns. The triple difference-in-differences regressions also include post-by-South covariate interactions and triple "free-space" interactions ($Post_t \cdot Free_c \cdot South_c$). I cluster standard errors at the state-level.

vote share (turnout per capita). Each regression includes predicted "free-space" signal strength measures, county fixed effects, and state-by-year fixed effects.

The impact of McIntire's *TCRH* is entirely driven by two-directional regional heterogeneity, where former Southern (Northern) Democratic (Republican) voters realign with the opposing party. The overall effect on political outcomes, not including the regional interaction term, is small, insignificant, and immune to the inclusion of covariates: a one standard deviation increase in predicted signal strength is associated with only a 0.15 p.p. increase in Democratic vote share, and a 0.08 p.p. decline in turnout per capita. In contrast, the preferred specification with regional differentials and covariates yields a 0.87 p.p. gain in Democratic vote share in the Non-South, and a 2.31 p.p. decline in the former Confederacy. The point estimates are virtually identical when excluding the covariate set. Since turnout per capita remains insignificant across regions, I reject mobilization as a sole, or even contributing, factor of the observed effect on Democratic vote share: conservative media was, at least in part, responsible for restoring (temporary) two-party competition in the South after a century of Democratic rule.

I update equations (2.2) and (2.3) to permit event study dynamics across presidential elections: the $Post_t$ term is replaced with binary election year terms, where the 1960 presidential election is omitted as the base case. I re-estimate over these specifications and plot the results in figure 2.3. Panel (a) displays the event study dynamics without regional interaction terms. The balanced pre-trends over the 1952 and 1956 elections suggest that the identification assumption is satisfied. In line with the null average effect over 1964 to 1980, no single election induces a statistically significant effect on Democratic vote share. Panel (b) of the same figure displays results by region: the light (dark) blue estimates correspond to the double (triple) interaction in equation (2.3). Both sets of pre-trends are balanced. The Southern differential (DDD) exhibits a marked

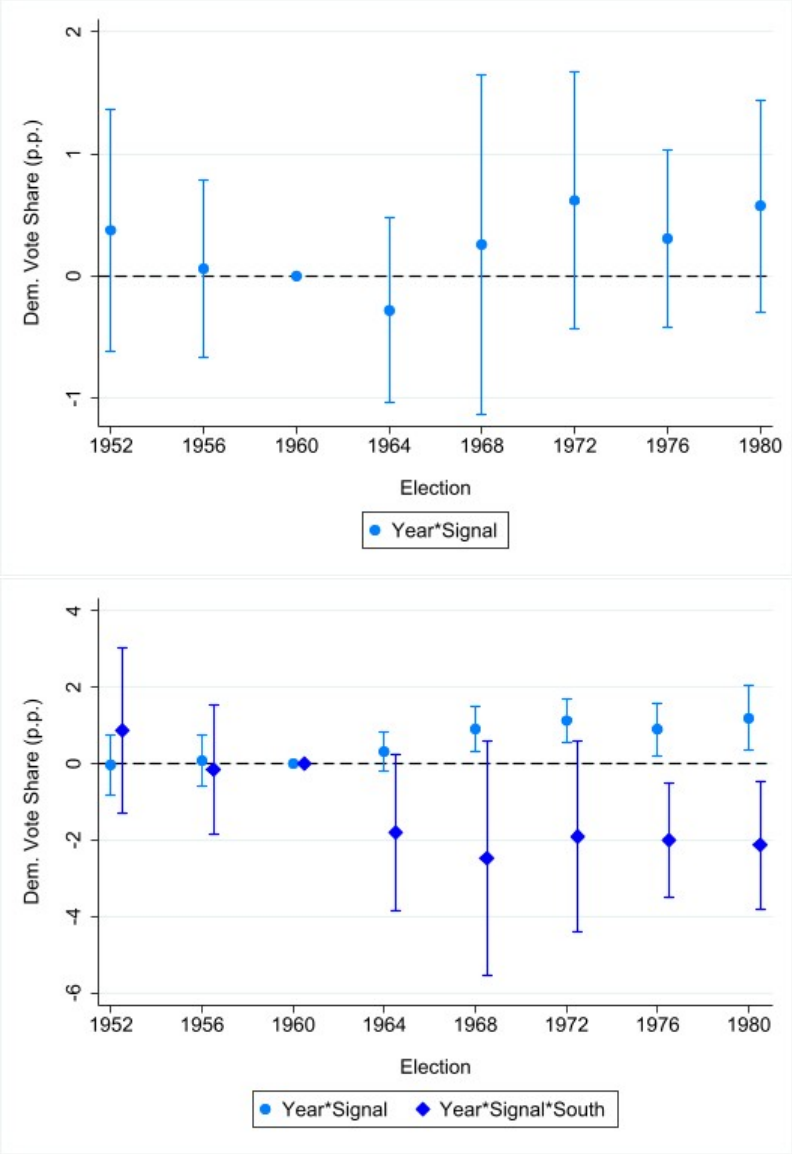


Figure 2.3: The Impact of of McIntire's *TCRH* on Democratic Vote Share: Event Study Plots

This figure displays event study plots from estimating equations (2.2) (top plot) and (2.3) (bottom plot) with Democratic vote share as the dependent variable. Post-period interactions with signal strength measures and covariates are replaced with election year interactions. Interactions with the 1960 election year binary term are omitted to avoid multicollinearity. The dynamic DiD (DDD) results are in light (dark) blue. Standard errors are clustered at the state level.

drop of roughly 2 p.p. in Johnson's 1964 presidential vote share that more or less persists at the same magnitude through the 1980 contest. Despite these large magnitudes, limited statistical power only leads to 10% significance in 1964, and insignificance in 1968 (p-val = 0.11) and 1972 (p-val = 0.13). However, both point estimates are significant at the 5% level in 1976 and 1980. The double interaction (DiD) moves opposite the regional interaction term beginning in 1968, and hovers around the 1 p.p. mark through the Carter-Reagan election. In both cases, the one-time shock at peak Civil Rights salience, and subsequent flat-lined persistence, serve as prima facie evidence in favor of the race issue as the principal catalyst of McIntire's baseline effect.[26]

I compare McIntire's baseline effect to cumulative partisan realignment to gauge conservative media's contribution to the end of one-party rule in the South. I begin by identifying the magnitude of overall realignment without accounting for exposure to McIntire's *TCRH*. Column (1) of table 2.3 stores results from the primitive regression of Democratic vote share on $South_c$ and $Post_t \cdot South_c$: Democratic nominees experienced an average Southern regional bump of 13.27 p.p. prior to the 1964 presidential contest; this relationship reversed course to an 11.54 p.p. disadvantage over the 1964 to 1980 elections. I find a similar 12.44 p.p. decline from re-estimating with the fixed effects and covariates from earlier. Importantly, these realignment estimates are in the ballpark of Kuziemko and Washington (2018), who identify anywhere from a 13.1 p.p. to 14.8 p.p. effect across similar specifications.[27]

[26]For completeness, figure 2.A12 displays event study results using turnout as the dependent variable. Consistent with earlier, conservative media exposure fails to generate a non-zero impact on turnout in any post-period election.

[27]For reference, the range of estimates cited from Kuziemko and Washington (2018) corresponds to columns (1), (3), and (7) of their Table 1. The likely reason for small differences in realignment estimates between this paper and Kuziemko and Washington (2018) is that they restrict attention to party re-identification of Southern whites. Since I focus on vote share, I also capture a re-enfranchised class of Southern blacks who were likely to vote for Democratic candidates. These voters would put downward pressure on realignment. This racial distinction is likely more important than differences between using party identification and vote share. Even if many "dealigned" voters subsequently failed to re-identify with the Republican Party, they still likely voted for non-Democratic candidates (Republican, Third-party, etc.). Since this paper's Democratic vote share measures are relative to overall vote share, not major-party vote share, whether voters subsequently identify as Republicans is unimportant.

After identifying overall realignment, I include signal strength terms: the updated specification is now simply equation (2.3) with a $Post_t \cdot South_c$ term to capture realignment absent exposure to McIntire's program. Estimation reproduces McIntire's baseline effect and leaves the coefficient on $Post_t \cdot South_c$ mostly unaffected (-12.85 p.p.): see column (3). I divide McIntire's Southern differential effect by the estimated coefficient on $Post_t \cdot South_c$, to identify the percent increase in realignment from a one standard deviation increase in predicted signal strength:

$$Media\ Realign.\ Contribution = \frac{\beta}{\delta}, \tag{2.4}$$

where δ is the parameter on the pure regional effect, and standard errors are calculated using the delta method. All else equal, a one standard deviation increase in exposure generates an 18% increase in realignment relative to Southern counties with no exposure.

2.5.2 Pairwise Matching

This paper replicates the baseline estimates using pre-processed matched data to establish robustness. I leverage pairwise variation between counties with vastly "different" levels of exposure to McIntire's program that would have otherwise had roughly the "same" exposure given a flat, smooth-surfaced counterfactual earth. In other words, identifiable variation is limited to sharp discontinuities in predicted signal strengths between counties that are almost entirely driven by topographical irregularities.

Table 2.3: How much did McIntire Contribute to Partisan Realignment?

	(1)	(2)	(3)
South	13.270***		
	(3.388)		
Post $\cdot South$	-11.541***	-12.438**	-12.849**
	(3.661)	(6.004)	(5.700)
Post $\cdot Signal$			0.871***
			(0.298)
Post $\cdot Signal \cdot South$			-2.313**
			(1.032)
Media Realign. Contribution			0.180*
			(0.109)
County FEs		X	X
State-Year FEs		X	X
Free			X
Covariates		X	X
Mean	43.960	43.949	43.949
SD	15.257	15.229	15.229
Obs.	24,653	24,485	24,485

This table displays baseline results from estimating a series of specifications building up to equation (2.3) with a $Post_t \cdot South_c$ term. Column (1) stores results from regressing Democratic vote share on region ($South_c$) and post-by-regional ($Post_t \cdot South_c$) binary terms. Column (2) re-estimates after including county fixed effects, state-by-year fixed effects, and the set of covariate interactions (post and post-by-South). The covariates are percent black (1960), percent high school (1960), percent Protestant (1952), percent unemployed (1960), and percent urban (1960). Column (3) estimates a modified version of equation (2.3) with a $Post_t \cdot South_c$ term. This column also includes an estimate of the media realignment effect given by equation (2.4). The standard errors on this effect are calculated using the delta method. I cluster standard errors at the state-level.

In the spirit of Durante et al. (2019) and Wang (2021a), these constraints are formalized in the following equation:

$$\mathcal{C} = \begin{cases} |Signal_c - Signal_{c'}| \geq \mu \\ \\ |Free_c - Free_{c'}| \leq \lambda \end{cases} . \tag{2.5}$$

Without loss of generality, consider two distinct counties belonging to the same state, c and $c^{'}$. These counties form a pair in the data only if their absolute difference in predicted signal strengths is at least as large as μ, and their absolute difference in predicted "free-space" signal strengths is no more than λ. The parameter set (μ, λ) establishes the cut-offs for how "different" or "similar" the absolute differences must be for inclusion.

One challenge in constructing a pairwise dataset for estimation is that the constraint set is a function of μ and λ: the modeler's choice of parameters impacts the pairwise comparisons included for estimation. For this reason, I use a data-driven cross-validation procedure that identifies the optimal (μ, λ) ordered pair that minimizes the following penalty function:

$$\begin{aligned} P(\mu, \lambda) = \max\{0, \mu - |Signal_c - Signal_{c'}|\}^{\gamma} + \\ \max\{|Free_c - Free_{c'}| - \lambda, 0\}^{\gamma}. \end{aligned} \tag{2.6}$$

The function "penalizes" county pairs that do not meet the criteria by how much they sit below or above the requirements, where each penalty is raised to the power γ. I optimize over the dataset with all eligible county-pair combinations, with replacement, belonging to the same state. The parameter spaces are restricted to $p(50) \geq \mu \geq p(75)$ and $p(25) \leq \lambda \leq p(50)$, where $p(X)$ is the

X^{th} percentile in the data. This choice is motivated by match quality and statistical power: the lower-bound (upper-bound) for μ (λ) sets a minimum quality benchmark; and the upper-bound (lower-bound) for μ (λ) ensures pairs remain for regression analysis. For both the linear and quadratic penalty functions, the optimal parameter combination is $(0.65, 0.54)$.[28] I only estimate over county pairs that meet these thresholds.

I re-estimate (2.2) and (2.3) after adding pairwise fixed effects and two-way clustering standard errors at the state and pair level. I also include regression weights to account for unequal representation of counties in the pairwise sample. Table 2.A2 displays these results, which are more or less the same as the baseline estimates. For added robustness, I consider more restrictive parameter combinations ($\mu \in \{0.75, 1, 1.25\}$ and $\lambda \in \{0.25, 0.5\}$) than the cross validation selection. These parameter choices also more or less reproduce the baseline estimates.[29]

2.5.3 Robustness Exercises

This subsection explores numerous robustness exercises. First, I establish robustness across covariate sets, fixed effects, specifications, weighting, and treatment breakdowns. Second, I fail to invalidate the identification assumption against the threat of geographic confounders with Democratic vote share. Third, this paper shows no one Southern state is driving the baseline estimates, that these results more or less hold restricting attention to within-South comparisons, and that the Non-South "backlash" effect is insensitive to the exclusion of certain regions and states. Finally, the following miscellaneous exercises are also conducted: re-estimating assuming full survivorship of station transmitters with missing end dates; re-estimating using a more inclusive

[28] Both of these parameter values correspond to $p(50)$.

[29] See table 2.A3 for results corresponding to $\mu \geq 1$ and $\lambda \leq 0.5$. Output from the other parameter combinations is available upon request.

definition of South, randomization inference, and placebo tests substituting McIntire's signal strengths with Rush Limbaugh's in 2020. Many of these exercises are only previewed in the main text, and formally outlined in appendices.

Alternative Covariates, Fixed Effects, Specification, Weighting, & Treatment Breakdowns

Subsection 2.8.5 shows that the baseline results are unaffected by the exclusion of post-by-South covariate interactions, county fixed effects, percent urban, percent black, and all other covariates. Omitting state-by-year fixed effects more than doubles McIntire's impact, which reflects the importance of controlling for statewide trends at the timing of regional partisan realignment. Excluding predicted "free-space" signal strengths pushes McIntire's impact on realignment to zero. This result indicates the importance of isolating a confounding, endogenous distance effect with an exogenous irregular topographical effect. The Southern differential effect also persists when implementing double-selection lasso linear regressions as a data-driven approach to choosing control sets.

Re-estimating over a two-period variant of equation (2.3), à la Cascio and Washington (2013) and Bernini et al. (2023b), produces results similar to the baseline case. While weighting by total population yields null estimates, these results are sensitive to outlier counties: excluding counties with a population greater than half-a-million people reproduces significant effects. Weighting by the log of total population, or controlling for this measure, likewise generate similar results. This paper also finds that incrementally dropping counties with relatively low exposure to McIntire's *TCRH* amplifies his impact on Southern realignment. This result serves as evidence against the baseline results merely capturing the general realignment effect absent any conservative media impact. See subsection 2.8.6 for more information.

Terrain Ruggedness & Identification Assumption

The empirical design rests on the assumption that the remaining variation in predicted signal strength from internalizing relative proximity to transmitters is plausibly exogenous: in other words, variation in exposure to McIntire's *TCRH* induced by irregular contours and the curvature of the earth should be as good as random. Key (1949) challenges this assumption by insisting that Southern historical voting patterns are indeed correlated with terrain ruggedness, or variation in elevation. He claims that the most rebellious counties to the traditional one-party Democratic rule in the South were generally situated in mountainous regions with infertile soil, such as Appalachia North Carolina and Tennessee.[30] If the rugged terrain in these counties generates relatively low predicted signal strengths, and these counties partially realign opposite the rest of the region with the Democratic Party, then this confounding effect could account for the baseline results.

A visual inspection of the Southern distributions of county-level mean ruggedness (figure 2.A13), pre-1964 average Democratic vote share, and the change in average Democratic vote shares pre and post 1964 (figure 2.A14) call into question the identification assumption. The middle of rugged Appalachia, stretching from Eastern Tennessee upward to Northern Virginia, experienced strong Republican slant pre 1964, and post-1964 movement to the Democratic Party. As a result, I test for potential confounding by re-estimating equation (2.3) excluding Southern counties above the $p(X)$ percentile in ruggedness. Figure 2.A15 displays these results. Added

[30]The Highlands and Piedmont Plateau regions in Western North Carolina were far more likely to trend Republican relative to the Coastal Plain and Tidewater regions to the east: this geographic divide usually produced a Senator from each respective region. A similar pattern developed in Tennessee, where eastern counties favored the GOP. To a lesser extent, elevation factored into other states' politics. For instance, Mississippi experienced habitual, albeit waning, infighting between western "delta planters" and eastern "rednecks of the hills": the former were more inclined to back the Thurmond-Wright Dixiecrat ticket in 1948. There were also Republican leanings in Appalachia Virginia and Alabama, along with Arkansas' counties in the Ozark Mountains (Key, 1949).

ruggedness does not expand the baseline differential effect, and if anything, variation in elevation might even have a dampening effect on Democratic vote share once counties in the 80th percentile or higher are included for estimation.

While these results assuage concerns over a geographic confounder driving the baseline results, they raise an additional question regarding the estimates moving opposite of expectation. Figure 2.A16 shows that the overwhelming majority of rugged Southern counties actually have predicted signal strengths at least as large as the national average. This is explained by large clusters of station transmitters airing the content in the region outweighing the dampening exposure effect induced by signal diffraction. Therefore, the signal was likely sufficiently large for these counties moving opposite the regional realignment effect, placing upward pressure on the estimates.

Omitting Southern States One-by-One, Only Former Confederacy Sample, & Non-South "Backlash" Effect

One measurement concern is that the baseline results are driven by a single Southern state. In figure 2.A17, I re-estimate the event study variant of equation (2.3) dropping each state one-by-one.[31] Clearly, conservative media's impact on realignment is indicative of the region at-large, and not limited to a single state. If anything, removing Tennessee only heightens McIntire's effect: his impact improves to a 3.70 p.p. Republican realignment in 1968, and each election between 1968-1980 becomes individually significant at the 5% level. This result is tied to the earlier discussion on terrain, but also a cluster of central counties experiencing heightened realignment with low exposure: see subsection 2.8.7 for an in-depth analysis of Tennessee.

In subsection 2.8.8, I re-estimate equation (2.2) only including former Confederate states.

[31] See table 2.A4 for the average effect excluding Southern states one-by-one.

While the preferred specification yields an insignificant estimate of -1.44 p.p., this result is sensitive to a few Southern counties. I later show that omitting Tennessee's 95 counties generates a significant 2.28 p.p. drop, while omitting Tennessee's 26 counties with exposure to McIntire's *TCRH* below the national average yields a similar 2.00 p.p. decline. In fact, 5% significance for the Southern differential term is achieved by dropping the state's 15 counties with the weakest signal strengths.

The baseline results for the Non-South suggest McIntire produced a "backlash" effect among this group of listeners. Subsection 2.8.9 establishes that this effect is not simply a by-product of general Non-South realignment favoring the Democratic Party. The Northeast and Midwest regions exhibited the greatest post-1964 movement away from the Republican Party. I find that limiting the Non-South sample to the Northeast, or dropping these states, has no impact on the results. Moreover, limiting this sample to states bordering the former Confederacy, or excluding these states, likewise has little difference on the estimates. Dropping Non-South states one-by-one fails to identify an influential state driving the results.

Miscellaneous Exercises

The main challenge to identifying McIntire's county-level exposure in 1964 is incomplete coverage dates in the station data. In subsection 2.3.1, I draw from the observed distribution of end dates in coverage to impute missing values. I will show that assuming full survivorship of stations with missing end dates, as opposed to using imputations from the the observable distribution, yields virtually identical results. Figure 2.A18 plots the re-calculated predicted ("free-space") signal strengths assuming full survivorship: these plots bear a strong resemblance to those with imputed end dates. Given the positive correlation between these exposure measures,

it follows that a re-estimation of equation (2.3) produces virtually the same results under full survivorship: a one standard deviation increase in McIntire's predicted signal strength is associated with a 0.82 p.p. rise in Democratic vote share in the Non-South, and a 2.16 p.p. decline in the former Confederacy.[32]

While the literature often defines the South as the 11 former Confederate states, the Census Bureau includes Delaware, Kentucky, Maryland, Oklahoma, and West Virginia to the region. I redefine the South as these 16 states, and re-estimate equation (2.3), to establish robustness across different classifications. The estimates are statistically indistinguishable from the baseline results: the Southern differential is slightly dampened to a 1.94 p.p. decline, while the Non-South experiences a modest bump to a 0.92 p.p. gain (see table 2.A6). Therefore, conservative media's effectiveness in converting Southern voters to the Republican Party is orthogonal to the inclusion of nearby states outside the former Confederacy.

Additional robustness exercises strengthen this paper's causal claims. First, I show that the baseline results are statistically significant at any reasonable level using the p-value from randomization inference (1000 replications and within-state permutations). Figure 2.A19 displays the corresponding kernel density estimates. Second, I test for significance using spatially-correlated standard errors (Conley, 1999).[33] I test for spatial correlations at each 50 km interval between 50 km and 350 km, inclusive. Irrespective of distance, the DiD is 1% significant, while the Southern DDD is 5% significant for all but the 100 km and 150 km cases (10%): see table 2.A7. Finally, as a placebo test, I show that substituting McIntire's signal strength measures with Limbaugh's (in 2020) generates null results stored in table 2.A8.

[32] Table 2.A5 displays these results.

[33] Following Wang (2021a), I use the *acreg* command developed by Colella et al. (2020) in *STATA*.

While the modified ITM software is favored in the economics literature for its accessibility and user-friendliness, it is better suited for line-of-sight signal propagation, which is more characteristic of FM radio. Despite McIntire's program primarily airing on AM radio, the software should still provide rough estimates of signal strength. Moving forward, I plan to restrict my analysis to FM stations, conduct sensitivity analyses to correct for underestimated AM radio signal strength, and explore software better suited to AM radio, e.g. Millington's Method, for added robustness.

2.6 Mechanisms

The like-minded sensibilities and shared circumstances of the American South lead many to paint the region in sweeping generalities. So prevalent is this outlook on Southern life that Cash (1941) devotes the opening lines of his *The Mind of the South* to highlight this tendency:

> "There exists among us by ordinary - both North and South - a profound conviction that the South is another land, sharply differentiated from the rest of the American nation, and exhibiting within itself a remarkable homogeneity."

While Cash (1941) endorses the existence of Southern regional identity, he likewise acknowledges the presence of "many Souths" with distinct features that coalesce into "one South". No more does this statement hold true than on the race issue. The hallmark of the South was its enduring commitment to Jim Crow. A century removed from civil war, segregation continued to bind the former Confederacy. At the same time, regional heterogeneity shaped varying attitudes towards Jim Crow by disproportionately incentivizing counties to resist calls for integration.

Convergence towards racial equality was most pressing for upper-class whites fighting to preserve their socio-political standing among sizeable black populations. Figure 2.A20 displays

these "Black Belt" counties, extending from the Mississippi Delta, through Central Alabama and Georgia, to the South Carolina seaboard. Historically, "Black Belt" planters wielded unrivaled influence over the rest of the South, and were most faithful to the Democratic Party.[34] Other counties were less incentivized to defend Jim Crow. As earlier mentioned, mountainous counties were sheltered from large-scale slavery, racially homogeneous favoring whites, and hence less threatened by potential integration. Certain statewide trends also minimized the need for segregation. For instance, Texas's declining black population and diversified economy (cattle, oil, etc.), and Florida's high levels of urbanicity and immigration, left little reason to fixate on preserving race relations (Key, 1949).

This section investigates McIntire's political impact in the context of the race issue. Subsection 2.6.1 explores Southern differential effects by attitudes towards Jim Crow. This process involves decomposing the Southern triple interaction term into cases that proxy for county-level temperature on segregation. These results will show if McIntire's realignment effect is driven by racial attitudes, particularly counties with local ideological competition.

2.6.1 Ideological Competition on the Race Issue

There exist a number of barriers to measuring attitudes towards Jim Crow across Southern counties: racial prejudice is difficult to detect in observational data; and 1960s survey data on race relations lacks sufficiently large sample sizes and granular geographic identifiers for empirical

[34] For instance, with greater slave interests, they dragged dissenting regions into the Civil War: Northern Alabama, Eastern Tennessee, and North Carolina were embroiled in the conflict despite their initial reservations to fight for the cause. The "Black Belt" was also comparatively more inclined to tolerate Alfred Smith in 1928 despite his Catholicism and anti-Prohibition stance. In fact, the only instance between Reconstruction and the 1964 election that disrupted the Democratic stronghold in the Black Belt was Truman's radical inclusion of Civil Rights equality in the 1948 party platform. This insertion pushed a relatively greater share of "Black Belt" counties to the Thurmond-Wright Dixiecrat ticket (Key, 1949).

analysis. As a result, I use county-level distributions of pro- and anti-segregation organization to proxy for local racial attitudes. In the data, KKK chapters represent pro-segregation organization, and Green Book establishments anti-segregation organization.

I update equation (2.3) by permitting the triple interaction term ($Post_t \cdot Signal_c \cdot South_c$) to vary by organizational type: pro- and anti-organization (KKK-GB), only pro-organization (KKK-No GB), only anti-organization (No KKK-GB), and no organization (No KKK-No GB). Figure 2.4 displays these organizational breakdowns in the South: counties with the Klan and without a Green Book establishment are in red; the reverse case is in blue; and counties with both (neither) are in purple (white). Formally, I replace the DDD term and coefficient with the following decomposition:

$$\sum_k \sum_g \xi^{k,g} \mathbf{1}_c(k,g) \cdot \Big\{ Post_t \cdot Signal_c \cdot South_c \Big\}, \tag{2.7}$$

where $k \in \{0,1\}$ denotes Klan presence, and $g \in \{0,1\}$ a Green Book establishment. The indicator term, $\mathbf{1}_c(k,g)$, equals one if the (k,g) Klan-by-Green Book combination is realized in county c. I take the double summation to include all four combinations.

The choice to consider pro- and anti-segregation organizations in tandem, as opposed to in isolation, is motivated by ethnic competition theory. This theory argues that racial cleavages are more salient in the face of economic or political competition that disrupts the existing status quo. A robust sociological literature identifies ethnic competition theory as a catalyst of group conflict: cleavages in close (distant) proximity place greater (less) emphasis on group differences, experience more (less) racial competition, and incur more (less) risk of conflict (Cunningham, 2012). Drawing from this theory, pro-segregation attitudes would be amplified by competition

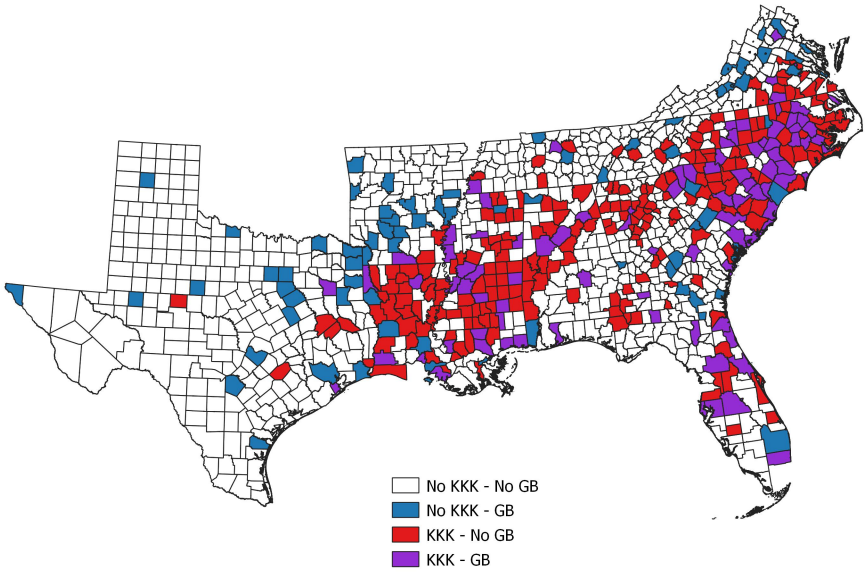


Figure 2.4: Organized White Resistence (KKK) and Non-Discriminatory Public Amenities (Green Book Establishments) in the South

This figure displays Southern counties by KKK and Green Book establishment (1955) presence. Counties with neither are displayed in white; only a Green Book establishment in blue; only the Klan in red; and both in purple.

from local anti-segregation organization, and vice versa. In fact, Cunningham (2013) even attributes the Klan's recruitment success to leveraging white communities with the strongest negative out-group attitudes sowed from racial competition. Consistent with Key (1949), he argues that these communities were incentivized by an intimate connection between social standing and segregation.[35]

Table 2.4 displays results from estimating the updated equation (2.3) with the decomposition. The table consists of two panels: the first shows the DiD term ($Post_t \cdot Signal_c$), the second the decomposed triple interaction terms stored in equation (2.7). In columns (1) and (2), I start by measuring pro- and anti-organizational effects separately, i.e. (KKK-No KKK) and (GB-No GB), for comparative purposes. In both cases, McIntire's Southern regional differential is largely insensitive to the Klan and Green Book distinctions: the regional differential with (without) the Klan is -2.01 p.p. (-2.34 p.p.); and with (without) a Green Book establishment -2.70 p.p. (-2.17 p.p.). These results are comparable in magnitude to the baseline regional differential, but mostly insignificant due to a loss of statistical power.

Column (3) of the same table displays results from considering both organizational types in tandem. In Southern counties with the Klan and a Green Book establishment, a standard deviation increase in exposure to McIntire's *TCRH* is associated with a 5.37 p.p. decline in Democratic vote share. The remaining Klan counties absent an anti-discriminatory public amenity experience a statistically insignificant regional differential (-0.91 p.p.). In other words, these counties are statistically indistinguishable from Northern counties in their response to pro-segregation media. The remaining Green Book counties absent the Klan also generate a similar coefficient (-1.2 p.p.)

[35]For more information on ethnic competition theory, consult the following sources cited in Cunningham's research: Barth (1969), Hannan (1979), Olzak (1992), Soule and Dyke (1999).

and interpretation. Finally, counties without pro- and anti-segregation organization experience a Democratic decline (-2.39 p.p.) that is nearly identical to the baseline case, but only significant at the 10% level.

The remaining two columns in table 2.4 serve as robustness exercises. Column (4) stores the results from re-estimating the decomposition terms with pairwise fixed effects. The estimates are similar to before, with exception to Klan-by-Green Book counties, which experience an even greater shift (-8.66 p.p.) away from the Democratic Party. One concern is that these results are not reflective of attitudes towards Jim Crow, but simply the underlying distribution of Southern blacks. For column (5), I substitute pairwise fixed effects with an additional Southern differential term by percent black. The pro- and anti-organization estimate (-8.95 p.p.) is similar to the corresponding pairwise estimate, while the remaining combinations all yield relatively larger point estimates: (KKK-No GB) -3.96 p.p. (10% significant); (No KKK-GB) -3.58 p.p. (5% significant); and (No KKK- No GB) -4.25 p.p. (5% significant). Across all five specifications, the estimates for counties outside the former Confederacy hover between 0.86 and 0.90 (1% significant).

Up to this point, this paper has statistically tested each decomposed Southern differential term relative to zero. In table 2.A9, I test for statistical differences between KKK-by-Green Book counties and the remaining organizational types: the first row compares KKK-by-Green Book counties to the remaining counties with the Klan; the second to the remaining counties with Green Book establishments; and the third to counties without the Klan and anti-discriminatory public amenities. Column (1) displays the differences between estimates stored in column (3) of table 2.A9. The -4.47 p.p. difference between Klan counties with and without Green Book establishments is significant at the 5% level; the -4.16 p.p. difference with only Green Book

Table 2.4: Differential Effects by Local Organization on the Race Issue: White Resistance (KKK) and Non-Discriminatory Public Amenities (Green Book Establishments)

	(1)	(2)	(3)	(4)	(5)
Panel A: Non-South					
Post $\cdot Signal$	0.871***	0.858***	0.858***	0.904***	0.858***
	(0.298)	(0.296)	(0.296)	(0.301)	(0.296)
Panel B: Southern Decomposition (Post ·Signal · South)					
$\times KKK$	-2.014				
	(1.252)				
$\times NoKKK$	-2.338*				
	(1.279)				
$\times GB$		-2.701			
		(1.625)			
$\times NoGB$		-2.173*			
		(1.144)			
$\times KKK \cdot GB$			-5.373***	-8.664***	-8.949***
			(1.650)	(2.031)	(2.422)
$\times KKK \cdot NoGB$			-0.908	-1.281	-3.963*
			(1.410)	(1.460)	(2.299)
$\times NoKKK \cdot GB$			-1.212	-0.772	-3.576**
			(1.301)	(1.455)	(1.674)
$\times NoKKK \cdot NoGB$			-2.385*	-2.442*	-4.247**
			(1.415)	(1.368)	(1.896)
County FEs	X	X	X	X	X
State-Year FEs	X	X	X	X	X
Pair FEs				X	
Free	X	X	X	X	X
Covariates	X	X	X	X	X
Black Heterogeneity					X
Mean	43.949	43.983	43.983	43.951	43.983
SD	15.229	15.229	15.229	15.359	15.229
Obs.	24,485	24,094	24,094	5,501,526	24,094

This table displays results from estimating equation (2.3) after replacing the triple difference-in-differences term ($Post_t \cdot Signal_c \cdot South_c$) with the decomposition in equation (2.7). Panel (a) stores estimates of the difference-in-differences coefficient scaling ($Post_t \cdot Signal_c$); panel (b) estimates of the coefficients scaling the decomposition terms. Each regression internalizes county fixed effects, state-by-year fixed effects, predicted "free-space' signal strengths, and a set of "flexible" (post and post-by-South interactions) covariates. This set includes percent black (1960), percent high school (1960), percent Protestant (1952), percent unemployed (1960), and percent urban (1960). Columns (1) and (2) include decompositions by KKK and Green Book establishments separately. Column (3) includes all four interaction terms. Column (4) displays results from estimating over the pairwise dataset and including pairwise fixed effects. Column (5) controls for heterogeneity in the Southern differential by percent black (($Post_t \cdot Signal_c \cdot South_c \cdot Black_c$)). I cluster standard errors at the state-level with exception to two-way clustering at the state and pair level in column (4).

counties is significant at the 10% level; and the -2.99 p.p. difference with counties with neither organization is insignificant ($p = 0.14$). The differences with pairwise fixed effects (column (4)) and black heterogeneity terms (column (5)) are each larger in magnitude and significant at 5% or better.

These results are largely in line with ethnic competition theory. McIntire's success rested in his ability to leverage pro-segregation voters conditional on local exposure to anti-segregation organization. Absent competition, Southern voters favoring Jim Crow were no more responsive to conservative media than their Northern counterparts. Hence, the added issue salience tied to local ideological competition appears necessary to conservative media's ability to impact partisan realignment. This finding is largely consistent with a literature that connects issue salience to attitudes towards out-groups (minorities) and political outcomes (Barrera et al., 2020; Bordalo et al., 2020; Colussi et al., 2021; Giavazzi et al., 2023).[36] One feature of the data seemingly unexplained by ethnic competition theory is the persistence in the baseline regional differential in counties without pro- and anti-segregation organization. However, the lack of organization, in and of itself, could generate ideological competition in counties without a firm slant on the race issue: in other words, without a strong pro- or anti-segregation foothold, these contested counties could have experienced greater issue salience.

For robustness, I repeat the previous exercises substituting Green Book establishments with VEP-funded black voter mobilization projects to capture anti-segregation organization. The Voter Education Project was the brainchild of Attorney General Kennedy, the Department of Justice, and Civil Rights activists and philanthropists. Their objective was to expand black

[36]Consistent with most the literature, these cited papers focus on immigrants as the out-group. See Alesina and Tabellini (2024) for a comprehensive review.

suffrage by supplying funds mostly to "Big Five" (NAACP, CORE, SCLC, SNCC, and NUL) local chapters. Despite their differences, these organizations were motivated by expanded cash flow to unite under the VEP-umbrella (Faulkenbury, 2019). As a rule of thumb, the VEP operated in either counties hostile to expanded black suffrage or counties with politically detached black populations (Watters and Cleghorn, 1967). Figure 2.A21 overlays VEP mobilization projects (May 1962-1964) and KKK chapters in the South. I display results from estimating the Southern differential term flexibly by KKK-by-VEP combinations. Table 2.A10 displays these results. The 5.76 p.p. drop in counties with the Klan and a VEP project is comparable to the 5.37 p.p. drop experienced in KKK-by-Green Book counties. Klan counties without a black mobilization project have a statistically zero regional differential. These results survive the inclusion of pairwise fixed effects and a black heterogeneity term. See table 2.A11 for tests of statistical differences across decomposition terms. In general, these findings are consistent with earlier results.

Subsection 2.8.10 includes further robustness exercises supporting ethnic competition theory. I show that only 67 of 1000 randomizations of KKK-by-Green Book assignment to Southern counties generate a stronger McIntire realignment effect than the actual 5.37 p.p. decline. This result highlights that the collection of counties with pro- and anti-organization on the race issue is not an arbitrary designation. Consistent with local ideological competition, this paper also finds that politically competitive counties in the pre-period (1952-1960) exhibited more realignment from conservative media exposure. Finally, subsection 2.8.11 offers suggestive evidence that McIntire's *TCRH* shifted membership of the far-right John Birch Society to the South.

2.7 Conclusion

Far removed from a Democratic South, it is convenient for many contemporary observers of American politics to write-off the GOP's presidential dominance below the Mason-Dixon line as a fait accompli. Yet, today's South is only two to three generations removed from being a century's old bastion of the Democratic Party. This abrupt political transformation has generated decades of research that mostly focuses on pinpointing the ideological drivers of Southern Democratic declines (Kuziemko and Washington, 2018). Surprisingly, this literature falls short of causally linking conservative media to the end of one-party rule in the South. This paper aims to empirically establish this relationship. I set out to show that conservative media's call to preserve segregation is responsible for converting former Democrats to the Republican ranks. While exploring this channel, I test if conservative media discriminately impacts pro-segregation voters who face local ideological competition on the race issue.

The sheer number of conservative broadcasters, operating across a variety of platforms, renders capturing media's comprehensive role in realigning the South virtually impossible. Instead, this paper analyzes radio's most popular right-leaning broadcaster of the 1960s, Carl McIntire (Matzko, 2020b). Sparked by the integration of the University of Mississippi, McIntire's *Twentieth Century Reformation Hour* commanded one-in-nine Americans to disavow any federal imposition of racial equality (Matzko, 2020a; Ruotsila, 2015). McIntire excited his listenership with fear-mongering tactics that linked integration to an unrecognizable South, forwarded by social upheaval, under black control. His rejection of Civil Rights was so resolute that the political media literature dubs *TCRH* as the token example of pro-segregation radio (Hendershot, 2011).

I use readily available station data (Bazzi et al., 2023), and modified ITM software (Olken,

2009), to measure the county-level predicted ("free-space") signal strengths of McIntire's *TCRH*. By isolating "free-space" coverage, this paper leverages variation attributed to exogenous features to identify McIntire's impact on partisan realignment. While estimating the DiD specification yields a null baseline effect, pronounced regional patterns in the raw data support the inclusion of a regional differential term. Estimating over the preferred DDD specification generates regional effects, unequal in magnitude, and opposite in direction: a one standard deviation increase in coverage generates a 2.31 p.p. loss (0.87 p.p. gain) in Democratic vote share in the South (Non-South). These Southern declines expanded partisan realignment by 18% relative to non-exposed counties in the region. These results persist when limiting identifying variation to county pairs with nearly identical "free-space" coverage, but "sufficiently" different predicted coverage. These results are also not driven by pre-existing correlations between Democratic vote share and geographic features (Key, 1949): if anything, I show that ruggedness works opposite the observed regional baseline effects.

This paper links the Southern differential baseline effect to attitudes towards Jim Crow. I update the DDD specification to flexibly estimate the Southern differential by county-level presence of pro and/or anti-segregation organization. A literature on ethnic competition theory motivates the choice to consider interactive effects by organizational type. Estimates across a number of specifications point towards local ideological competition on the race issue as the main driver of regional differences. McIntire's program has the strongest realignment effect in Southern counties with both organizational types, and to a lesser extent, neither organizational type. When the ideological slant of Southern counties is uncontested, either firmly for or against segregation, McIntire's regional differential goes to zero. These results hold irrespective of using Green Book establishments or black mobilization projects to capture anti-segregation organization.

2.8 Appendix

2.8.1 Additional Tables and Figures

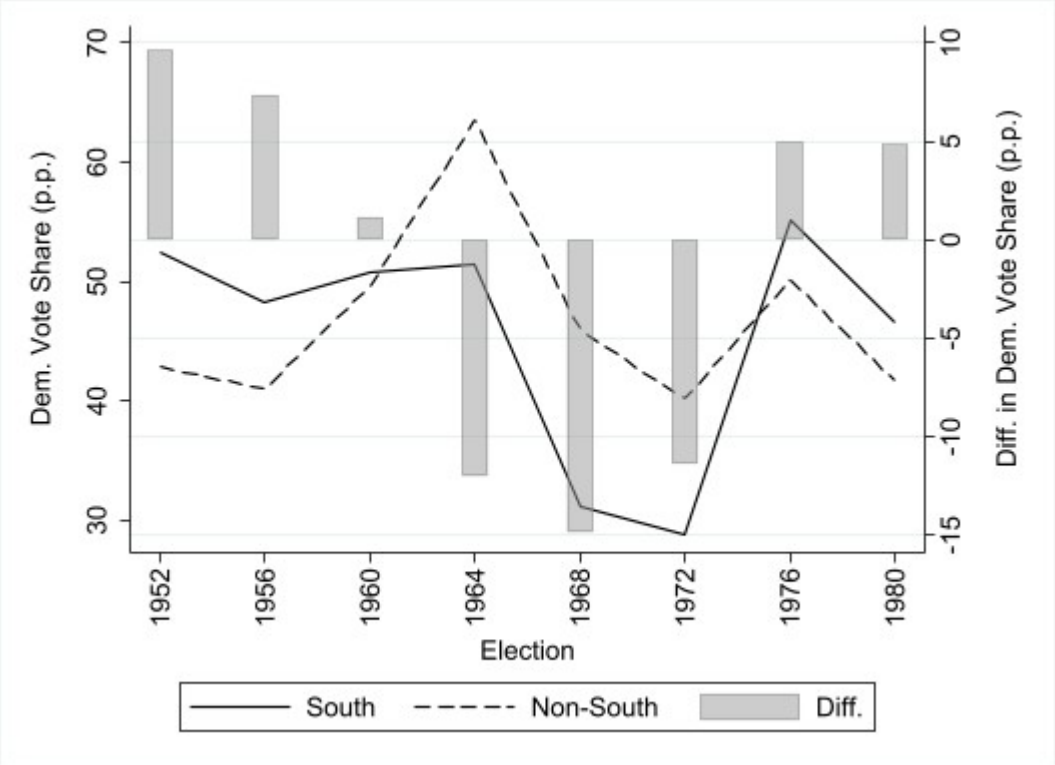


Figure 2.A1: Regional Trends in Democratic Vote Share

This figure displays average regional trends in Democratic vote shares from 1952-1980. The solid line measures vote shares for the average Southern county; the dashed line for the average non-Southern county. These trends correspond to the leftward y-axis. The gray bars measure the regional difference in Democratic vote shares between the average Southern and non-Southern counties across elections. These differences correspond to the rightward y-axis.

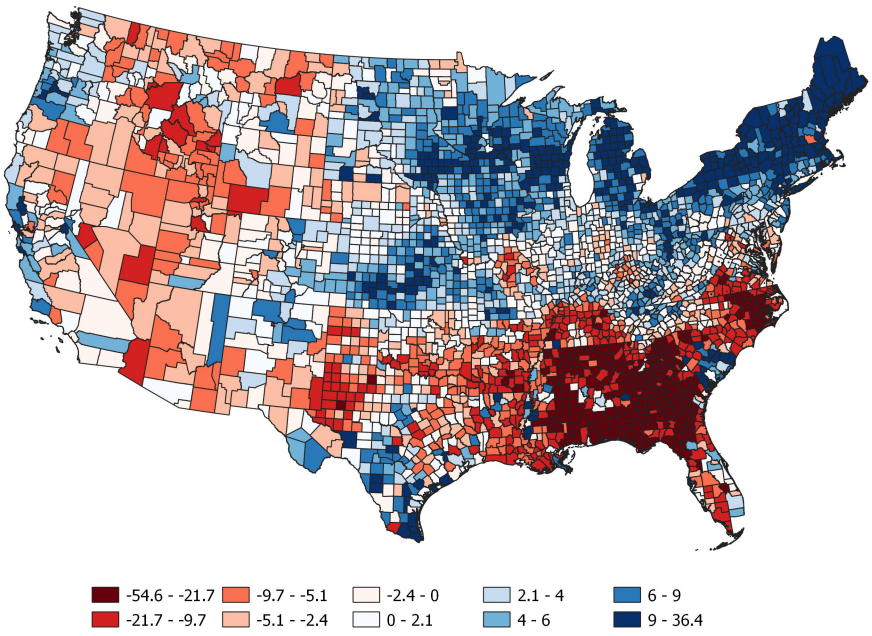


Figure 2.A2: Visualizing Changes in Average Democratic Vote Share Pre- and Post-1964

This figure displays county-level changes in average Democratic vote shares pre- and post-1964. The pre-period average is over the 1952-1960 presidential elections, while the post-period is over 1964-1980. Counties in red (blue) experience average Republican (Democratic) net gains. Darker shades of red (blue) signal larger Republican (Democratic) partisan swings that correspond to the legend below.

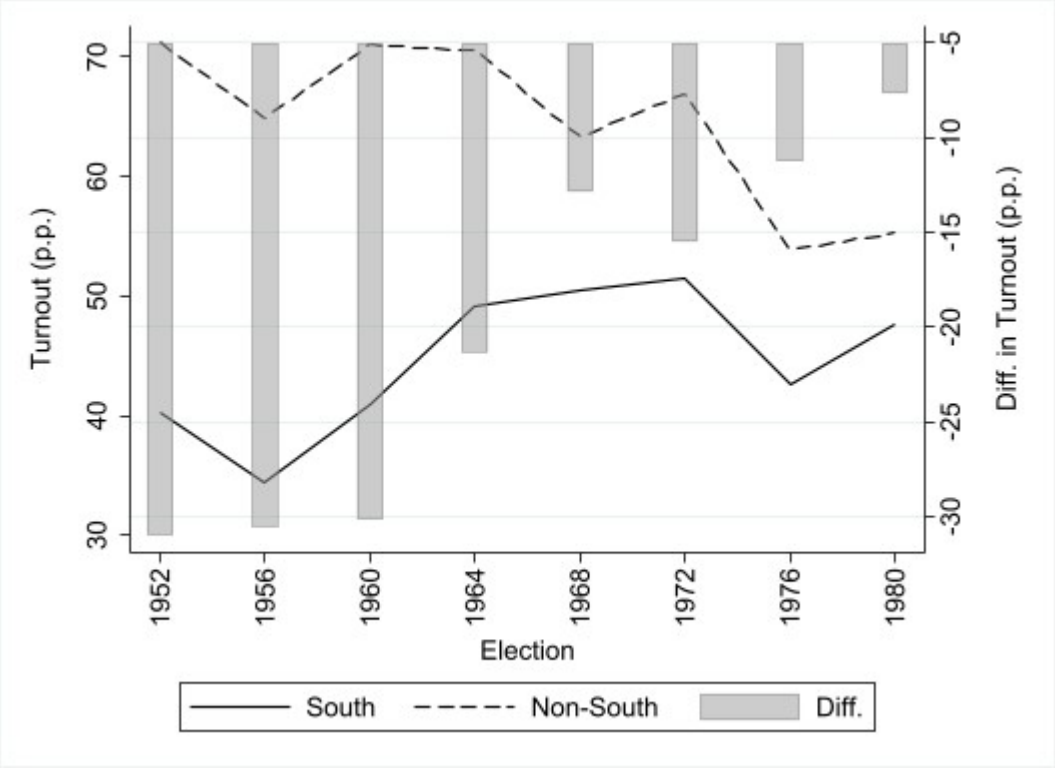


Figure 2.A3: Regional Trends in Turnout

This figure displays average regional trends in turnout from 1952-1980. The solid line measures turnout for the average Southern county; the dashed line for the average non-Southern county. These trends correspond to the leftward y-axis. The gray bars measure the regional difference in turnout between the average Southern and non-Southern counties across elections. These differences correspond to the rightward y-axis.

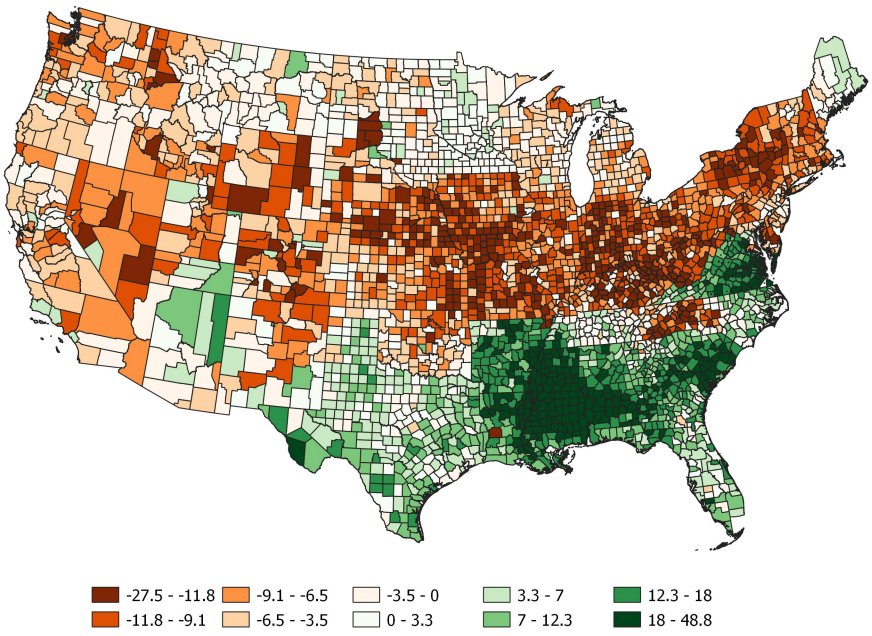


Figure 2.A4: Visualizing Changes in Average Turnout Pre- and Post-1964

This figure displays county-level changes in average turnout pre- and post-1964. The pre-period average is over the 1952-1960 presidential elections, while the post-period is over 1964-1980. Counties in green (orange) experience net gains (losses). Darker shades of green (orange) signal larger positive (negative) swings that correspond to the legend below.

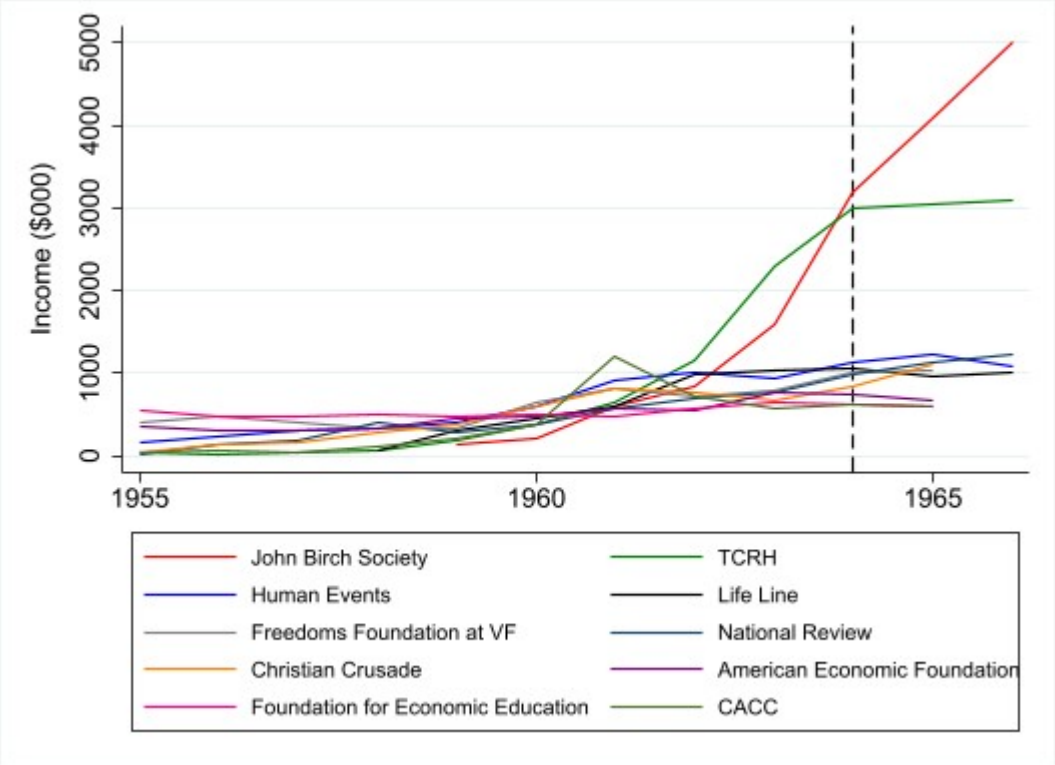


Figure 2.A5: Right-Wing Media Organizations by Income

This figure displays trends in annual income from 1955-1966 for each of the top ten right-wing media organizations by 1964 income. Income is measured in thousands on the y-axis. Each trend line corresponds to the organization name in the legend. Carl McIntire's *Twentieth Century Reformation Hour (TCRH)* is in green.

Table 2.A1: Comparing McIntire's *TCRH* Audience to Popular Programs

Show	Audience	Time Coverage
Panel A: Talk Radio		
The Dan Bongino Show	8.75M/week	Present (?)
The Glenn Beck Program	10M/week	Present (?)
The Mark Levin Show	12M/week	Present (?)
The Mike Gallagher Show	8.5M/week	Present (?)
The Rush Limbaugh Show	20M/week	1990s (peak)
The Sean Hannity Show	14.5M/week	Present (?)
Panel B: Political Talk Shows (TV)		
Face the Nation (CBS)	3M/show	2016-2022
Fox News Sunday (Fox)	1.5M/show	2016-2022
Meet the Press (NBC)	3M/show	2016-2022
This Week (ABC)	3M/show	2016-2022
Panel C: Radio Evangelists		
Charles Coughlin's Program	30M/week	mid-1930s (peak)
Carl McIntire's *TCRH*	20M (regular)	mid-1960s (peak)

This table displays audience sizes for popular political programs. Panel (a) consists of talk radio shows, namely the five most currently listened to right-wing programs, as well as *The Rush Limbaugh Show*; Panel (b) consists of the weekly Sunday morning TV political talk shows on major broadcasting networks; and Panel (c) consists of two radio evangelists in Charles Coughlin and Carl McIntire. Each audience size is a cross-sectional measurement corresponding to the show's peak audience (indicated in parentheses), or a particular instance in time. Question marks in parentheses signal a degree of uncertainty regarding the time horizon corresponding to the audience size.

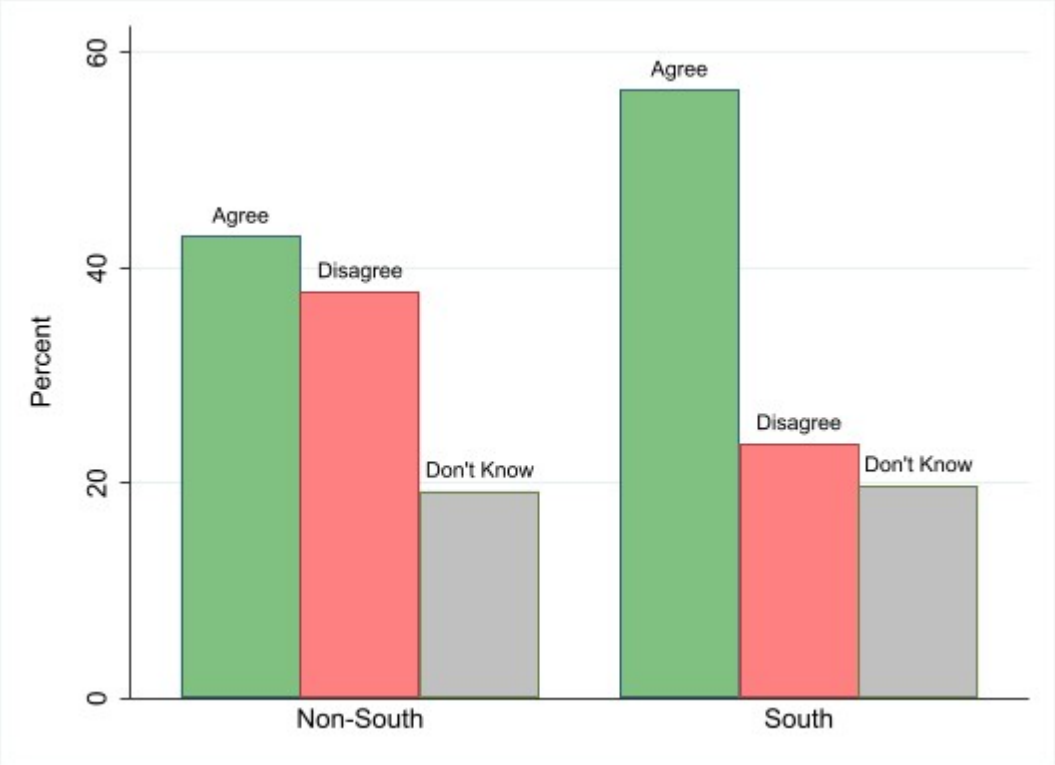
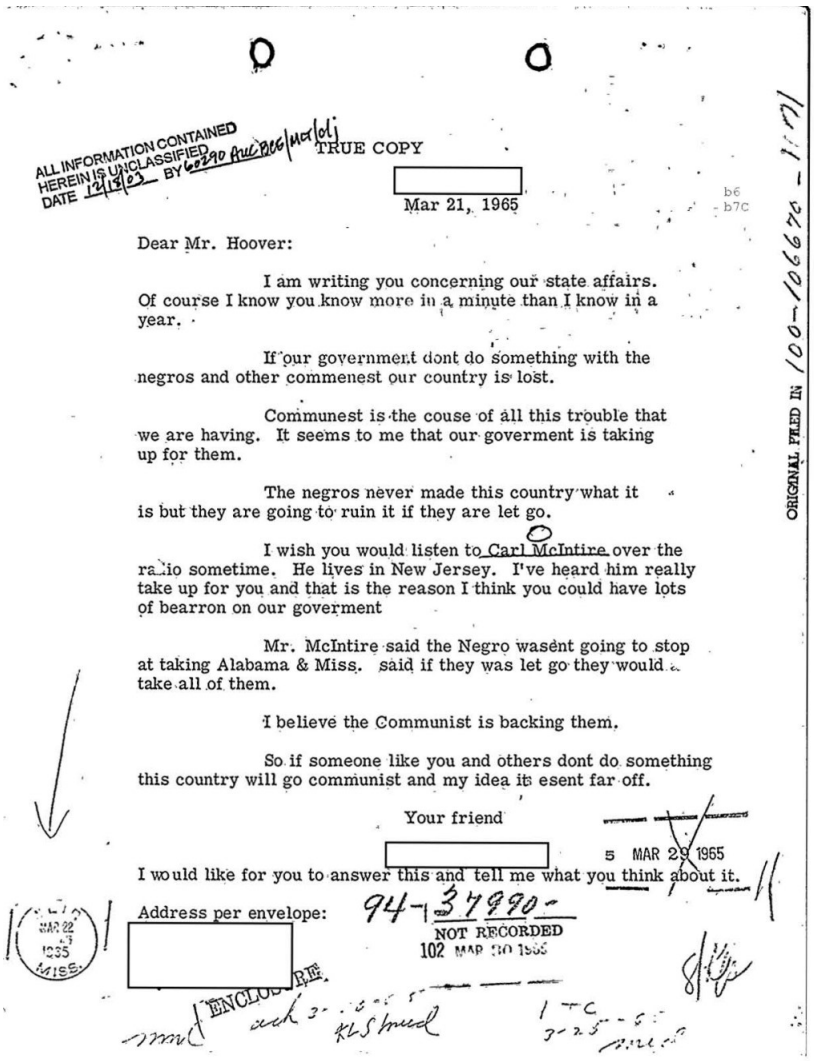


Figure 2.A6: Gallup Poll (9/1964) - Communists & Civil Rights Movement

This figure displays survey results for the following September 1964 Gallup Poll question: "And now, one more statement about the Communists: Most of the organizations pushing for Civil Rights have been infiltrated by the Communists and are now dominated by Communist trouble-makers. Do you agree with the statement or not?". I display results by region: Non-South and South. The y-axis measures the percent of respondents agreeing, disagreeing, or falling somewhere in between.



ALL INFORMATION CONTAINED HEREIN IS UNCLASSIFIED DATE 12/19/03 BY 60290 AUC/BCG/MCF/dlj

TRUE COPY

[redacted]

Mar 21, 1965

b6
b7C

Dear Mr. Hoover:

I am writing you concerning our state affairs. Of course I know you know more in a minute than I know in a year.

If our government dont do something with the negros and other commenest our country is lost.

Communest is the couse of all this trouble that we are having. It seems to me that our goverment is taking up for them.

The negros never made this country what it is but they are going to ruin it if they are let go.

I wish you would listen to Carl McIntire over the radio sometime. He lives in New Jersey. I've heard him really take up for you and that is the reason I think you could have lots of bearron on our goverment

Mr. McIntire said the Negro wasent going to stop at taking Alabama & Miss. said if they was let go they would take all of them.

I believe the Communist is backing them.

So if someone like you and others dont do something this country will go communist and my idea its esent far off.

Your friend

[redacted]

5 MAR 29 1965

I would like for you to answer this and tell me what you think about it.

Address per envelope:

[redacted]

94-37990-
NOT RECORDED
102 MAR 30 1965

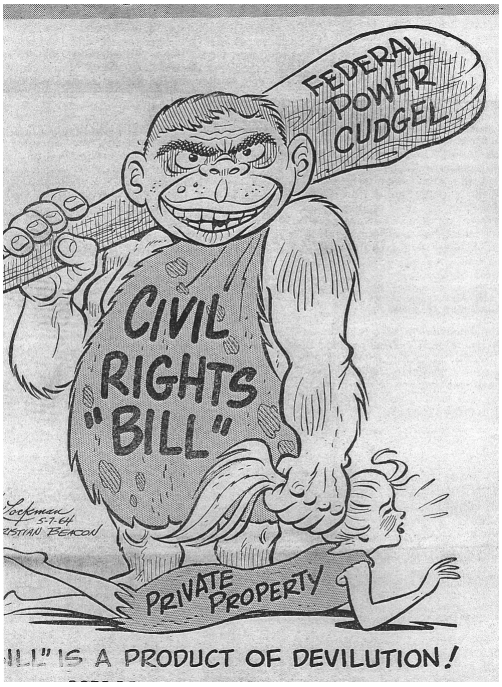
MAR 22 1235 MISS.

ENCLOS RE

ORIGINAL FILED IN 100-106670 - 1[illegible]

Figure 2.A7: An FBI Letter Testifying to McIntire's Radio Persuasiveness

This figure displays a declassified FBI letter sent from an unidentified listener of McIntire's radio program to FBI Director Hoover. The letter highlights McIntire's use of fear-mongering tactics to shape public opinion on federal calls for integration.



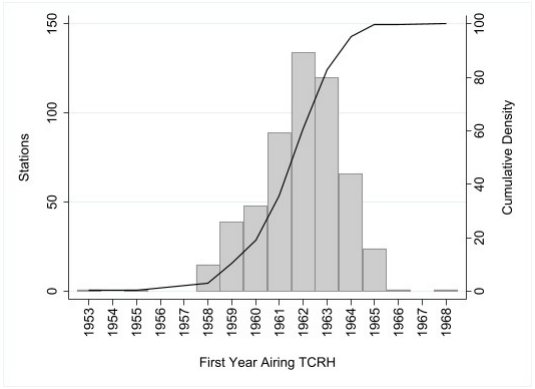
(a) May 7, 1964



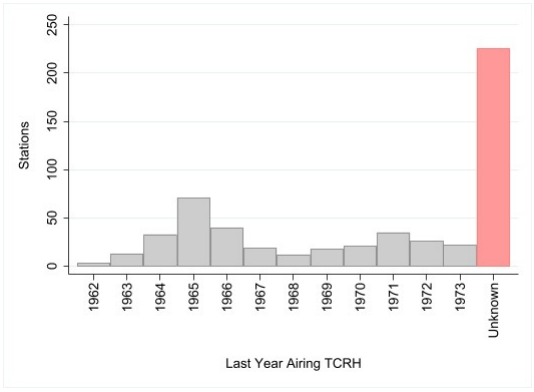
(b) June 11, 1964

Figure 2.A8: Political Cartoons on the Civil Rights Movement in the *Christian Beacon*

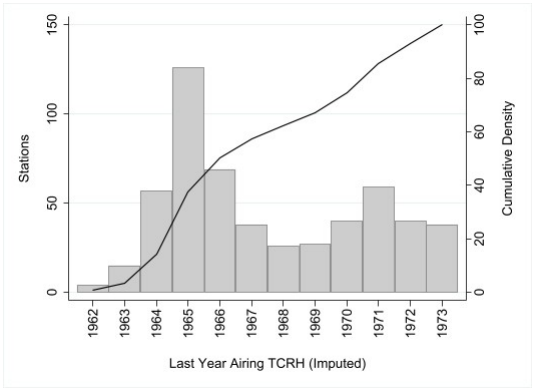
This figure displays political cartoons with Civil Rights messaging featured in Carl McIntire's *Christian Beacon*. Panel (a) reduces the Civil Rights Bill of 1964 to a mere federal power play to seize private property while perpetuating racial stereotypes. Panel (b) links Martin Luther King's Civil Rights activism to physical damage and bodily harm. See Griffith Jr (2009) for additional information on the cartoons.



(a) Start Date



(b) End Date



(c) End Date (Imputed)

Figure 2.A9: Stations' First and Last Year Airing McIntire's *TCRH*

This figure displays histograms and cumulative density functions of the start and end dates in radio station coverage of McIntire's program. Panel (a) corresponds to start dates. Panel (b) displays end dates, including a count of missing observations (in red). Panel (c) also displays end dates, but after imputing missing values using the observable distribution.

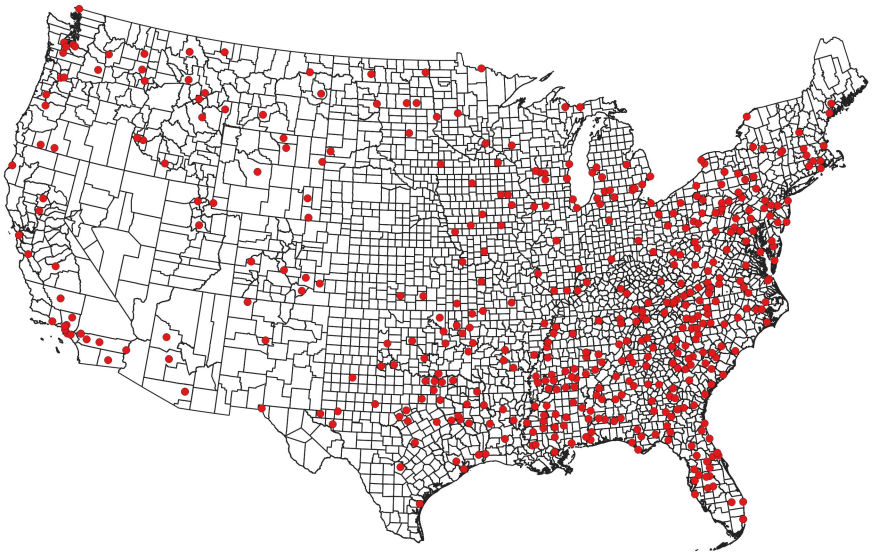
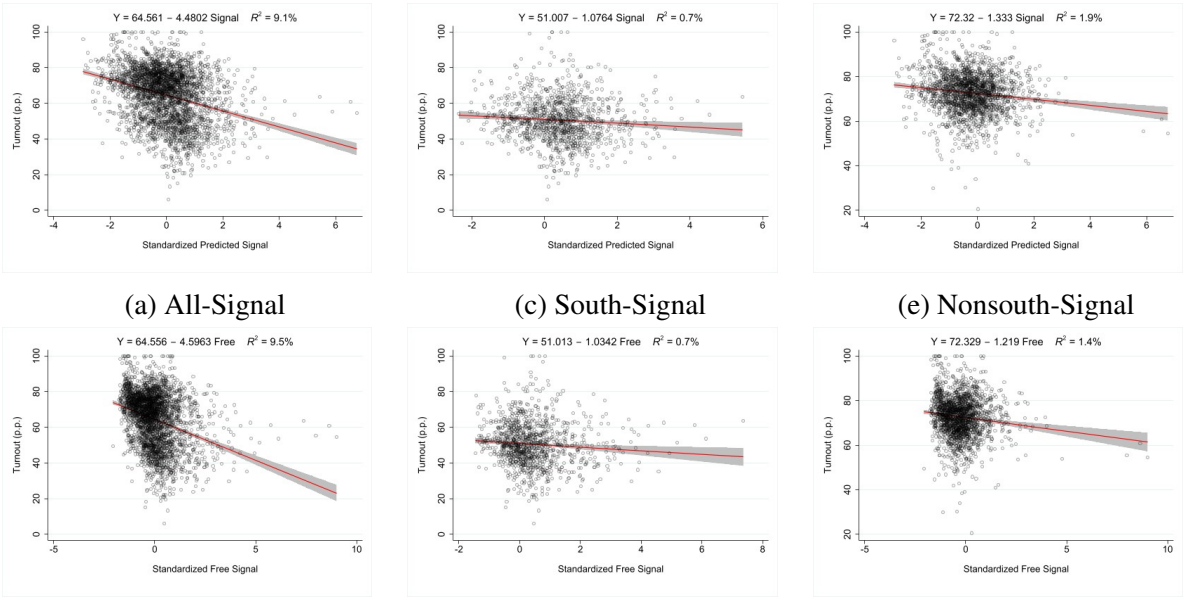


Figure 2.A10: Stations Airing McIntire's *TCRH* in 1964

This figure plots stations airing McIntire's *TCRH* in 1964. Imputed end dates are used in place of missing values.



(a) All-Signal (c) South-Signal (e) Nonsouth-Signal

(b) All-Free (d) South-Free (f) Nonsouth-Free

Figure 2.A11: Examining the Raw Data: Turnout & Signal Strengths

This figure plots turnout and signal strengths to McIntire's program. Each plot corresponds to a region (all U.S., South, Non-South) and signal strength type (predicted signal strength, predicted "free-space" signal strength) combination. Each scatter point denotes a Southern county's signal strength and turnout. I plot the lines of best fit (red) and corresponding 95% confidence intervals (gray). For each combination, I mathematically display the line of best fit and R^2 value at the top of each graph.

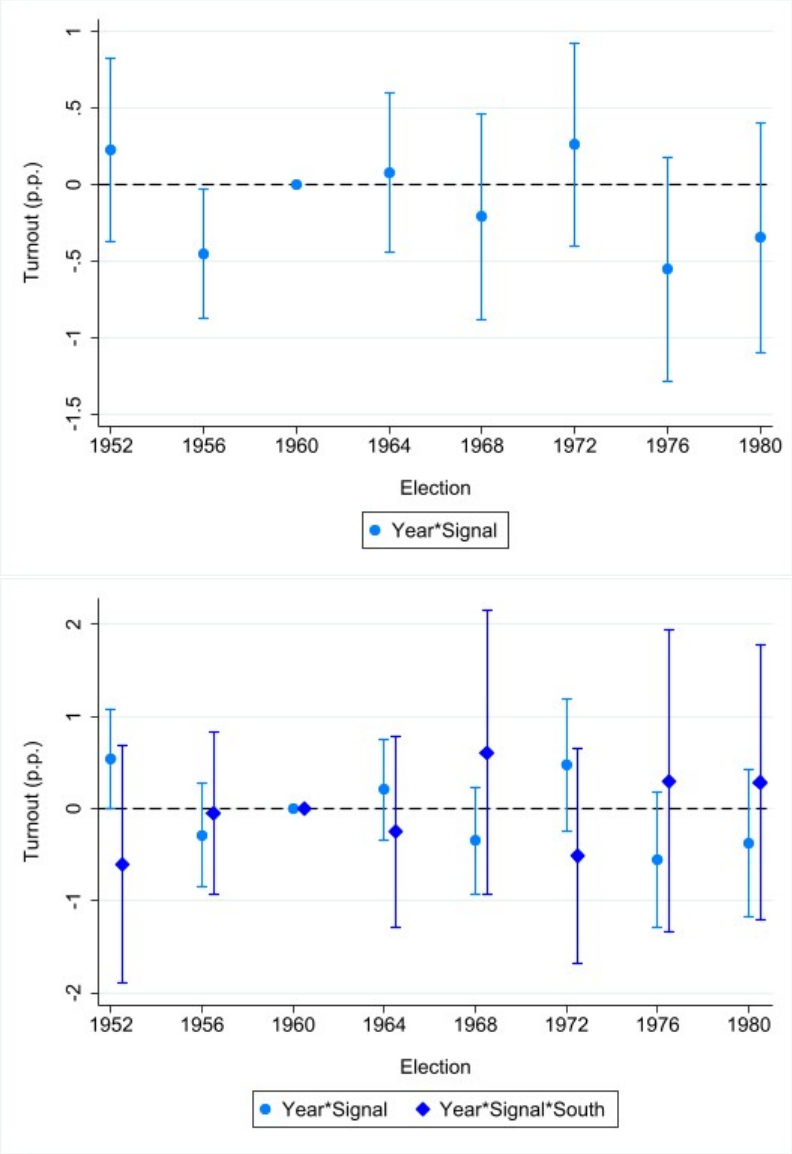


Figure 2.A12: The Impact of of McIntire's *TCRH* on Turnout: Event Study Plots

This figure displays event study plots from estimating equations (2.2) (top plot) and (2.3) (bottom plot) with turnout as the dependent variable. Post-period interactions with signal strength measures and covariates are replaced with election year interactions. Interactions with the 1960 election year binary term are omitted to avoid multicollinearity. The dynamic DiD (DDD) results are in light (dark) blue. Standard errors are clustered at the state level.

Table 2.A2: The Impact of McIntire's *TCRH* on Democratic Vote Share and Turnout: Pairwise Matching Minimizing Penalty Function using Cross Validation

	Overall				Regional Differential			
	(1) VS	(2) VS	(3) Turn	(4) Turn	(5) VS	(6) VS	(7) Turn	(8) Turn
Post $\cdot Signal$	-0.008	0.099	0.174	-0.132	0.855**	0.912***	-0.180	-0.249
	(0.541)	(0.501)	(0.346)	(0.244)	(0.373)	(0.302)	(0.303)	(0.289)
Post $\cdot Signal \cdot South$					-2.454**	-2.569**	0.915	0.305
					(1.134)	(1.040)	(0.741)	(0.464)
County FEs	X	X	X	X	X	X	X	X
State-Year FEs	X	X	X	X	X	X	X	X
Pair FEs	X	X	X	X	X	X	X	X
Free	X	X	X	X	X	X	X	X
Covariates		X		X		X		X
Mean	43.926	43.915	60.816	60.834	43.926	43.915	60.816	60.834
SD	15.386	15.360	16.096	16.054	15.386	15.360	16.096	16.054
Obs.	5,627,558	5,590,134	5,640,188	5,603,964	5,627,558	5,590,134	5,640,188	5,603,964

This table displays results from estimating equations (2.2) and (2.3) over the pairwise dataset constructed using cross validation techniques. The "Overall" supercolumn stores estimates from regressing equation (2.2). The first (second) pair of columns use Democratic vote share (turnout) as the dependent variable. Each regression internalizes county fixed effects, state-by-year fixed effects, pair fixed effects, and predicted "free-space' signal strengths. I re-estimate "flexibly" (post-period interactions) controlling for the set of covariates referenced in the main text: percent black (1960), percent high school (1960), percent Protestant (1952), percent unemployed (1960), and percent urban (1960). The "Regional Differential" supercolumn stores estimates from regressing equation (2.3). These columns are similar to the previous set of columns. The triple difference-in-differences regressions also include post-by-South covariate interactions and triple "free-space" interactions ($Post_t \cdot Free_c \cdot South_c$). Frequency weights are applied to account for unequal county representation in pairs. I two-way cluster standard errors at the state and pair level.

Table 2.A3: The Impact of McIntire's *TCRH* on Democratic Vote Share and Turnout: Pairwise Alternative Matching ($\mu \geq 1$ and $\lambda \leq 0.5$)

	Overall				Regional Differential			
	(1) VS	(2) VS	(3) Turn	(4) Turn	(5) VS	(6) VS	(7) Turn	(8) Turn
Post $\cdot Signal$	-0.078	0.074	0.244	-0.117	0.806**	0.852***	-0.140	-0.241
	(0.550)	(0.497)	(0.342)	(0.244)	(0.367)	(0.295)	(0.312)	(0.294)
Post $\cdot Signal \cdot South$					-2.520**	-2.433**	1.002	0.353
					(1.133)	(1.052)	(0.711)	(0.453)
County FEs	X	X	X	X	X	X	X	X
State-Year FEs	X	X	X	X	X	X	X	X
Pair FEs	X	X	X	X	X	X	X	X
Free	X	X	X	X	X	X	X	X
Covariates		X		X		X		X
Mean	44.001	43.992	61.067	61.087	44.001	43.992	61.067	61.087
SD	15.439	15.419	16.008	15.954	15.439	15.419	16.008	15.954
Obs.	4,344,452	4,313,588	4,352,452	4,322,668	4,344,452	4,313,588	4,352,452	4,322,668

This table displays results from estimating equations (2.2) and (2.3) over the pairwise dataset constructed using the following parameter combination: $\mu \geq 1$ and $\lambda \leq 0.5$. The "Overall" supercolumn stores estimates from regressing equation (2.2). The first (second) pair of columns use Democratic vote share (turnout) as the dependent variable. Each regression internalizes county fixed effects, state-by-year fixed effects, pair fixed effects, and predicted "free-space' signal strengths. I re-estimate "flexibly" (post-period interactions) controlling for the set of covariates referenced in the main text: percent black (1960), percent high school (1960), percent Protestant (1952), percent unemployed (1960), and percent urban (1960). The "Regional Differential" supercolumn stores estimates from regressing equation (2.3). These columns are similar to the previous set of columns. The triple difference-in-differences regressions also include post-by-South covariate interactions and triple "free-space" interactions ($Post_t \cdot Free_c \cdot South_c$). Frequency weights are applied to account for unequal county representation in pairs. I two-way cluster standard errors at the state and pair level.

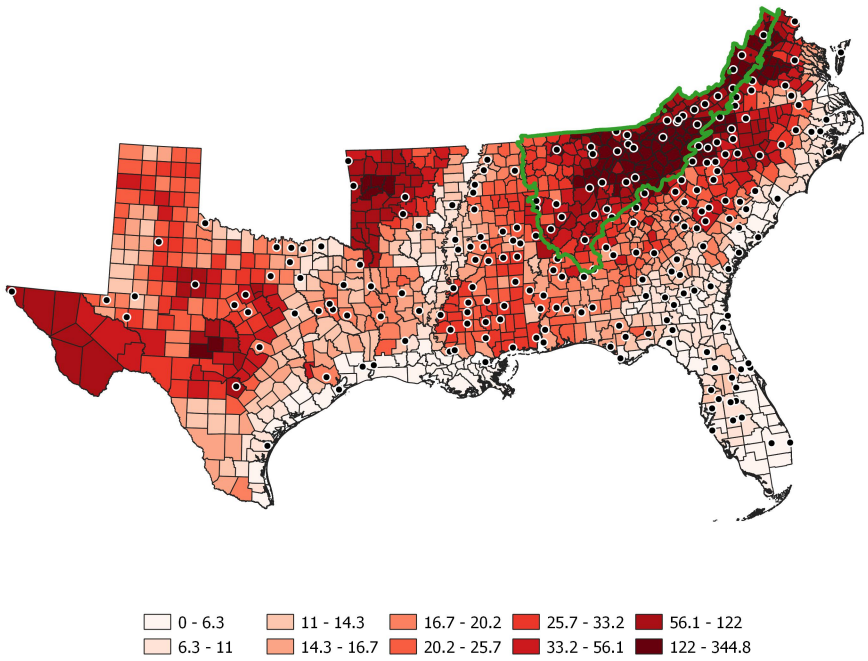
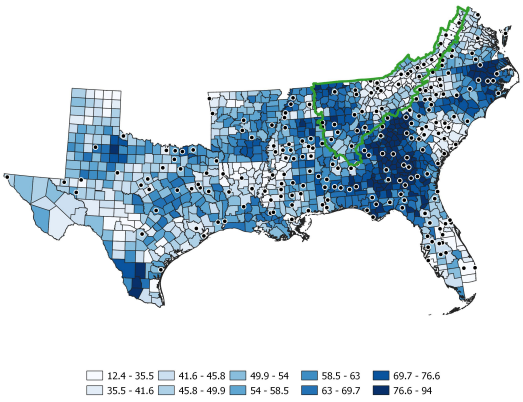
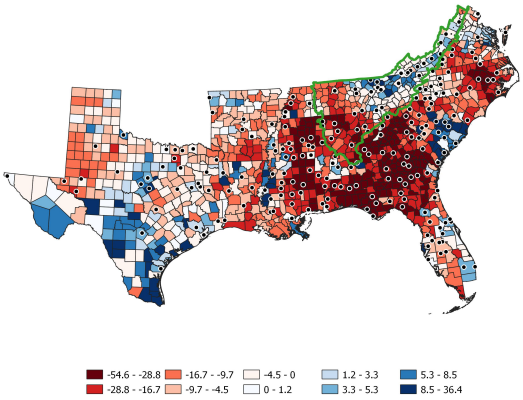


Figure 2.A13: Mean Ruggedness Distribution in the South

This figure displays a heat map distribution of mean ruggedness at the county level. The Southern Appalachia region is defined by the green boundary. Station transmitters airing McIntire's *TCRH* in 1964 are represented by black bullet points.



(a) Pre-1964 Avg. Dem. Vote Share



(b) Changes in Avg. Dem. Vote Share

Figure 2.A14: Visualizing Southern Pre-1964 Average Democratic Vote Share and Changes in Average Democratic Vote Share Post 1964

Panel (a) of this figure displays average Democratic vote shares over the 1952 to 1960 presidential elections in the South. Darker shades of blue signal larger average Democratic slant. Panel (b) displays changes in average Democratic vote shares pre- and post-1964 in the South. The pre-period average is over the 1952-1960 presidential elections, while the post-period is over 1964-1980. Counties in red (blue) experience average Republican (Democratic) net gains. Darker shades of red (blue) signal larger Republican (Democratic) partisan swings that correspond to the legend below. The Southern Appalachia region is defined by the green boundary. For each plot, station transmitters airing McIntire's *TCRH* in 1964 are represented by black bullet points.

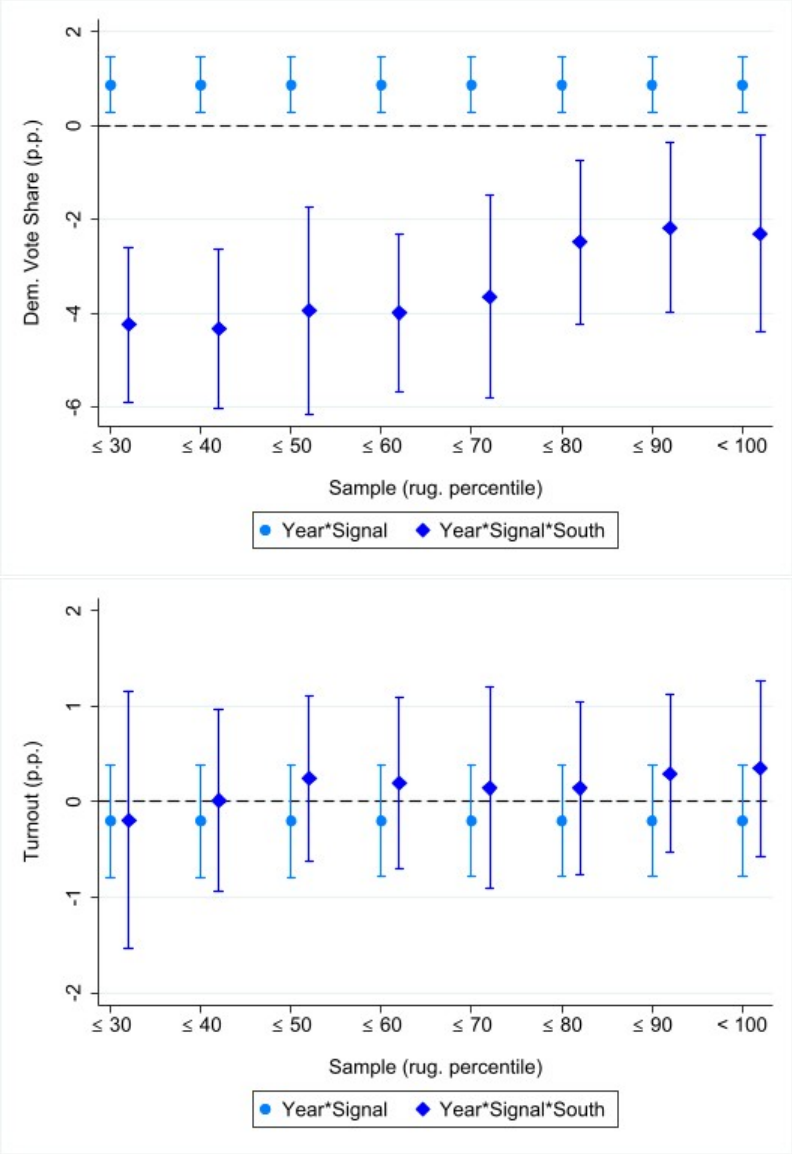
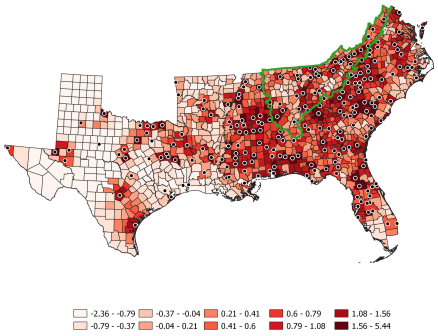
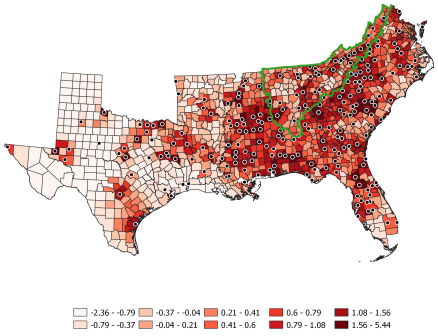


Figure 2.A15: The Impact of of McIntire's *TCRH* on Democratic Vote Share and Turnout: Excluding Southern Counties in the Xth Percentile of Geographic Ruggedness

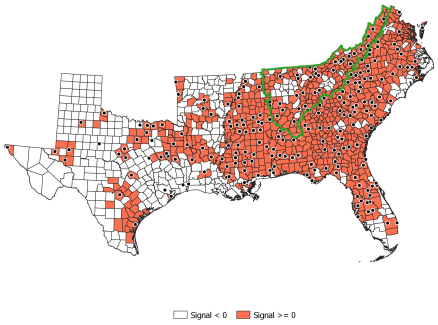
This figure displays event study plots from estimating equation (2.3) over counties with a mean ruggedness in excess of the Xth percentile. The top (bottom) panel uses Democratic vote share (turnout) as the dependent variables. Post-period interactions with signal strength measures and covariates are replaced with election year interactions. Interactions with the 1960 election year binary term are omitted to avoid multicollinearity. The dynamic DiD (DDD) results are in light (dark) blue. Standard errors are clustered at the state level.



(a) Predicted Signal Strength



(b) Predicted Free-Space Signal Space



(c) Predicted Signal Strength $\geq, <$ National Average

Figure 2.A16: Predicted (Free-Space) Signal Strength of McIntire's *TCRH* in the South

This figure displays predicted signal strengths in panel (a) and predicted "free-space" signal strengths in panel (b) in the South. Both measures are computed from inputting radio transmitter data into Olken's modified ITM software. Predicted signal strengths are calculated from differencing the effective path loss due to relative distance and irregular topographical features from transmission power. The "free-space" measures are calculated in a similar fashion, except effective path loss is restricted solely to relative distance. Hence, the spatial variation between each type of signal strength is attributed to topography, which is the source of plausibly exogenous variation I exploit for causal inference. See Olken (2009) for additional information on this empirical design. Panel (c) highlights counties with predicted signal strengths at least as large as the national average in orange. The Southern Appalachia region is defined by the green boundary. For each plot, station transmitters airing McIntire's *TCRH* in 1964 are represented by black bullet points.

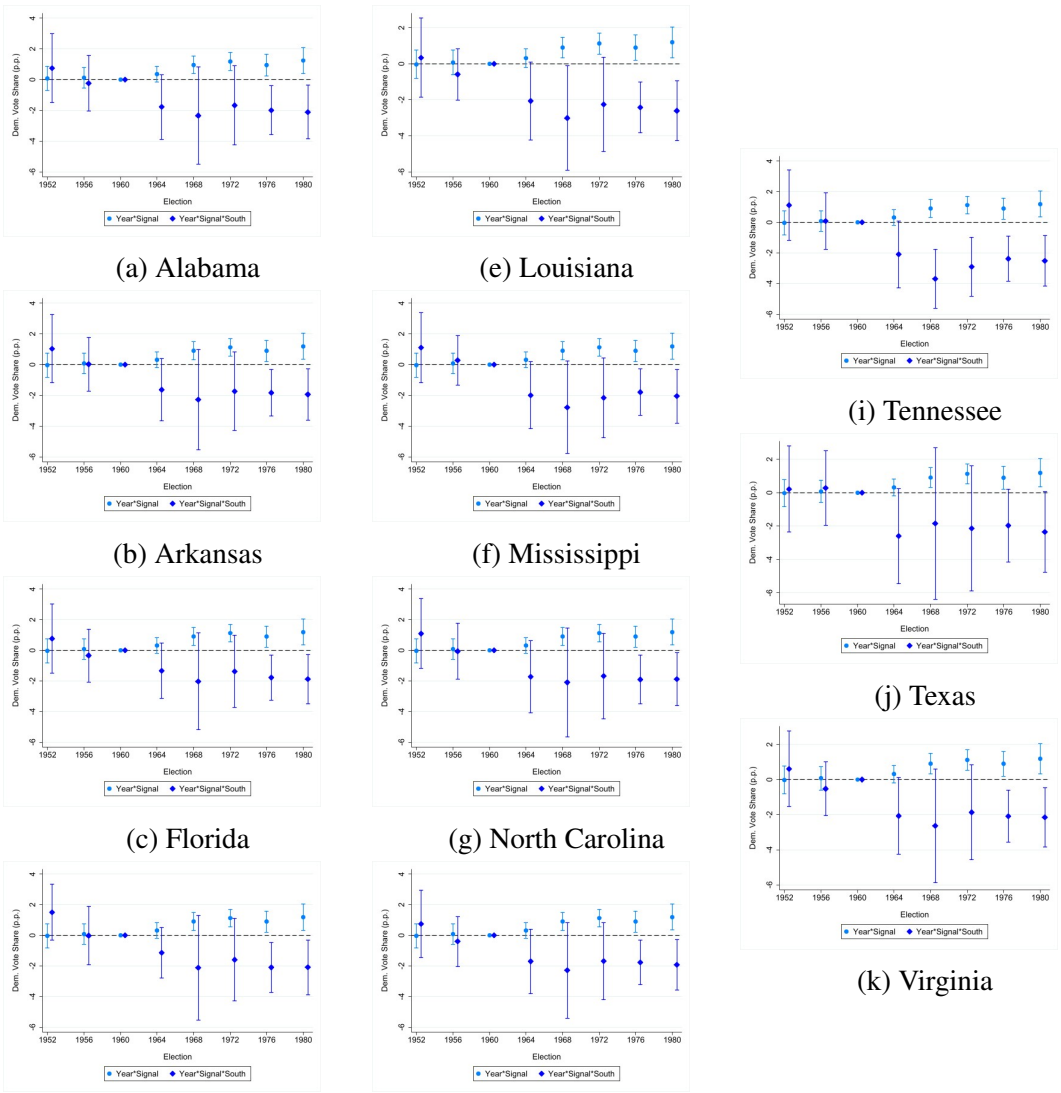


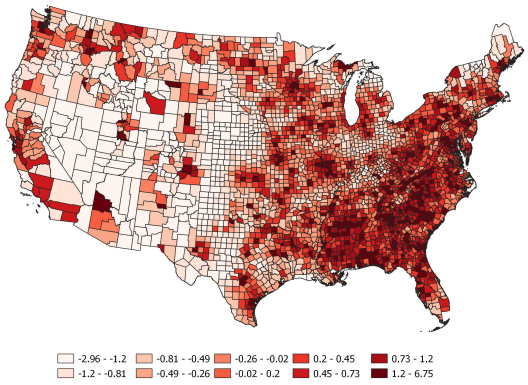
Figure 2.A17: The Impact of of McIntire's *TCRH* on Democratic Vote Share and Turnout: Exclude Southern States One-by-One

This figure displays event study plots from estimating equation (2.3) with Democratic vote share as the dependent variable. Each plot corresponds to omitting a single Southern state. Post-period interactions with signal strength measures and covariates are replaced with election year interactions. Interactions with the 1960 election year binary term are omitted to avoid multicollinearity. The dynamic DiD (DDD) results are in light (dark) blue. Standard errors are clustered at the state level.

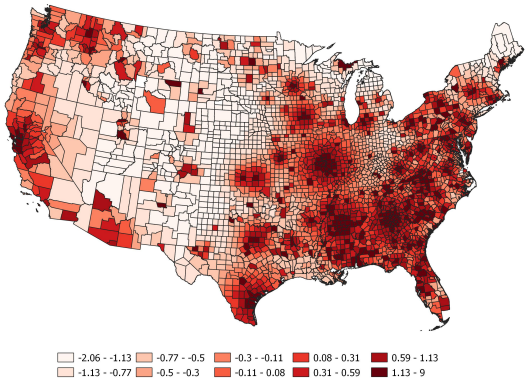
Table 2.A4: The Impact of McIntire's *TCRH* on Democratic Vote Share: Exclude Southern States One-by-One

	(1)
Omit Alabama	-2.147**
	(1.063)
Omit Arkansas	-2.250**
	(1.081)
Omit Florida	-1.844*
	(0.976)
Omit Georgia	-2.320*
	(1.187)
Omit Louisiana	-2.404**
	(1.069)
Omit Mississippi	-2.633**
	(1.032)
Omit North Carolina	-2.209*
	(1.147)
Omit South Carolina	-2.002*
	(1.008)
Omit Tennessee	-3.148***
	(0.774)
Omit Texas	-2.327
	(1.498)
Omit Virginia	-2.204*
	(1.110)
County FEs	X
State-Year FEs	X
Free	X
Covariates	X

This table displays results from estimating equation (2.3) with Democratic vote share as the dependent variable. Each row corresponds to separate regression output from omitting a single Southern state one-by-one. Only the Southern differential effect (DDD) is stored in the table. Each regression internalizes county fixed effects, state-by-year fixed effects, predicted "free-space' signal strengths, and a "flexible" (post-period and post-by-South interactions) set of covariates referenced in the main text: percent black (1960), percent high school (1960), percent Protestant (1952), percent unemployed (1960), and percent urban (1960). I cluster standard errors at the state-level.



(a) Predicted Signal Strength



(b) Predicted (Free-Space) Signal Space

Figure 2.A18: Predicted (Free-Space) Signal Strength of McIntire's *TCRH* - Assuming Survivorship of Stations with Missing Exit Dates

This figure displays recalculated signal strength measures assuming full survivorship of stations with missing end dates (as opposed to using imputed values). Predicted signal strengths are displayed in panel (a), and predicted "free-space" signal strengths in panel (b), at the county level. Both measures are computed from inputting radio transmitter data into Olken's modified ITM software. Predicted signal strengths are calculated from differencing the effective path loss due to relative distance and irregular topographical features from transmission power. The "free-space" measures are calculated in a similar fashion, except effective path loss is restricted solely to relative distance. Hence, the spatial variation between each type of signal strength is attributed to topography, which is the source of plausibly exogenous variation I exploit for causal inference. See Olken (2009) for additional information on this empirical design.

Table 2.A5: The Impact of McIntire's *TCRH* on Democratic Vote Share and Turnout: Assuming Survivorship of Stations with Missing Exit Dates

	Overall				Regional Differential			
	(1) VS	(2) VS	(3) Turn	(4) Turn	(5) VS	(6) VS	(7) Turn	(8) Turn
Post $\cdot Signal$	0.055	0.161	0.210	-0.091	0.820**	0.871***	-0.153	-0.202
	(0.522)	(0.480)	(0.339)	(0.238)	(0.364)	(0.298)	(0.305)	(0.290)
Post $\cdot Signal \cdot South$					-2.162*	-2.280**	1.000	0.263
					(1.122)	(1.023)	(0.711)	(0.459)
County FEs	X	X	X	X	X	X	X	X
State-Year FEs	X	X	X	X	X	X	X	X
Free	X	X	X	X	X	X	X	X
Covariates		X		X		X		X
Mean	43.960	43.949	60.818	60.838	43.960	43.949	60.818	60.838
SD	15.257	15.229	16.041	15.994	15.257	15.229	16.041	15.994
Obs.	24,653	24,485	24,707	24,544	24,653	24,485	24,707	24,544

This table displays results from estimating equations (2.2) and (2.3) using recalculated signal strength measures assuming full survivorship of stations with missing end dates (as opposed to using imputed values). The "Overall" supercolumn stores estimates from regressing equation (2.2). The first (second) pair of columns use Democratic vote share (turnout) as the dependent variable. Each regression internalizes county fixed effects, state-by-year fixed effects, and predicted "free-space' signal strengths. I re-estimate "flexibly" (post-period interactions) controlling for the set of covariates referenced in the main text: percent black (1960), percent high school (1960), percent Protestant (1952), percent unemployed (1960), and percent urban (1960). The "Regional Differential" supercolumn stores estimates from regressing equation (2.3). These columns are similar to the previous set of columns. The triple difference-in-differences regressions also include post-by-South covariate interactions and triple "free-space" interactions ($Post_t \cdot Free_c \cdot South_c$). I cluster standard errors at the state-level.

Table 2.A6: The Impact of McIntire's *TCRH* on Democratic Vote Share and Turnout: Expanded Definition of South

	(1) VS	(2) VS	(3) Turn	(4) Turn
Post $\cdot Signal$	0.810**	0.918***	-0.004	-0.091
	(0.396)	(0.336)	(0.217)	(0.232)
Post $\cdot Signal \cdot South$	-1.681	-1.943**	0.452	0.002
	(1.053)	(0.906)	(0.740)	(0.485)
County FEs	X	X	X	X
State-Year FEs	X	X	X	X
Free	X	X	X	X
Covariates		X		X
Mean	43.960	43.949	60.818	60.838
SD	15.257	15.229	16.041	15.994
Obs.	24,653	24,485	24,707	24,544

This table displays results from estimating equation (2.3) using an expanded definition of "South" consistent with the Census Bureau. The following states are added to the region: Delaware, Kentucky, Maryland, Oklahoma, and West Virginia. The first (second) pair of columns use Democratic vote share (turnout) as the dependent variable. Each regression internalizes county fixed effects, state-by-year fixed effects, and predicted "free-space' signal strengths. I re-estimate "flexibly" (post and post-by-South interactions) controlling for the set of covariates referenced in the main text: percent black (1960), percent high school (1960), percent Protestant (1952), percent unemployed (1960), and percent urban (1960). I cluster standard errors at the state-level.

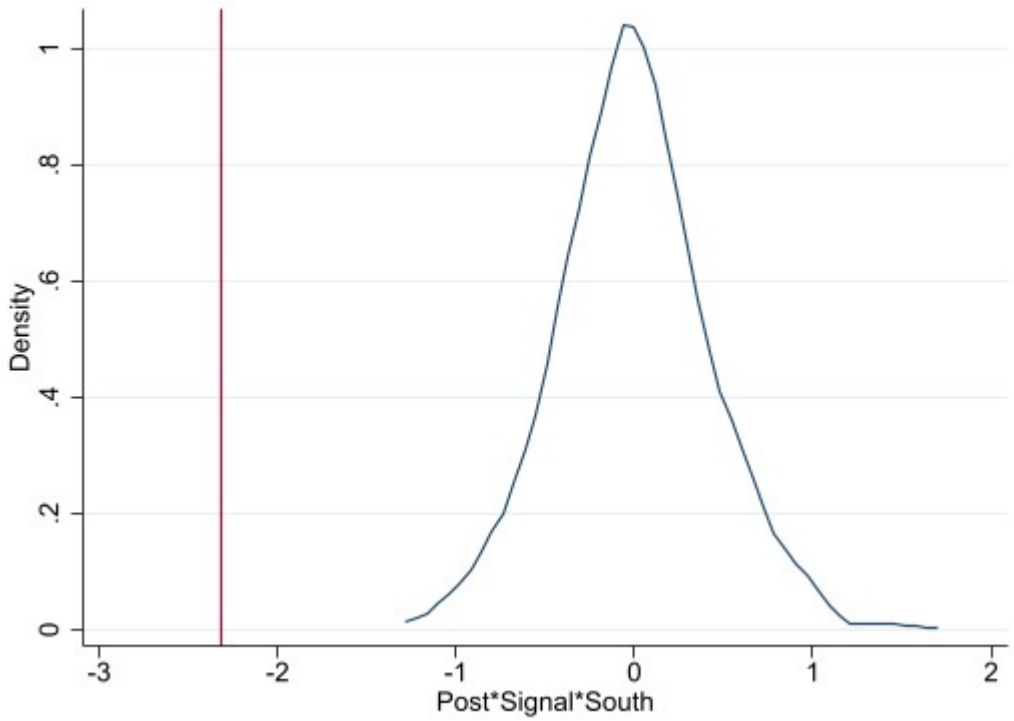


Figure 2.A19: The Impact of of McIntire's *TCRH* on Democratic Vote Share: Distribution of Kernel Density Estimates from Randomization Inference

This figure displays the distribution of kernel density estimates from randomization inference (1000 permutations and within-state permutations) of the coefficient on $Post_t \cdot Signal_c \cdot South_c$. The baseline regional differential (red vertical line) is included for comparative purposes.

Table 2.A7: The Impact of McIntire's *TCRH* on Democratic Vote Share: Conley SEs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	50 KM	100 KM	150 KM	200 KM	250 KM	300 KM	350 KM
Post $\cdot Signal$	0.871***	0.871***	0.871***	0.871***	0.871***	0.871***	0.871***
	(0.230)	(0.272)	(0.280)	(0.268)	(0.241)	(0.221)	(0.217)
Post $\cdot Signal \cdot South$	-2.313**	-2.313*	-2.313*	-2.313**	-2.313**	-2.313**	-2.313**
	(0.946)	(1.231)	(1.219)	(1.157)	(1.110)	(1.044)	(1.037)
County FEs	X	X	X	X	X	X	X
State-Year FEs	X	X	X	X	X	X	X
Free	X	X	X	X	X	X	X
Covariates	X	X	X	X	X	X	X
Mean	43.960	43.960	43.960	43.960	43.960	43.960	43.960
SD	15.257	15.257	15.257	15.257	15.257	15.257	15.257
Obs.	24,485	24,485	24,485	24,485	24,485	24,485	24,485

This table displays results from estimating equation (2.3) using Conley standard errors to account for potential spatial correlation. Each column denotes the distance (in kilometers) of spatial correlation permitted. Each regression internalizes county fixed effects, state-by-year fixed effects, predicted "free-space' signal strengths, and a "flexible" (post and post-by-South interactions) set of covariates referenced in the main text: percent black (1960), percent high school (1960), percent Protestant (1952), percent unemployed (1960), and percent urban (1960).

Table 2.A8: The Impact of *The Rush Limbaugh Show* on Democratic Vote Share and Turnout

	(1) VS	(2) VS	(3) Turn	(4) Turn
Post $\cdot Signal$	0.312	0.303	-0.269	-0.302
	(0.224)	(0.245)	(0.258)	(0.246)
Post $\cdot Signal \cdot South$	-0.116	0.129	-0.975	0.298
	(1.156)	(1.084)	(0.670)	(0.430)
County FEs	X	X	X	X
State-Year FEs	X	X	X	X
Free	X	X	X	X
Covariates		X		X
Mean	43.950	43.949	60.824	60.841
SD	15.256	15.231	16.035	15.995
Obs.	24,629	24,477	24,687	24,536

This table displays results from estimating equation (2.3) after substituting McIntire's signal strength measures for Limbaugh's. The first (second) pair of columns use Democratic vote share (turnout) as the dependent variable. Each regression internalizes county fixed effects, state-by-year fixed effects, and predicted "free-space' signal strengths. I re-estimate "flexibly" (post and post-by-South interactions) controlling for the set of covariates referenced in the main text: percent black (1960), percent high school (1960), percent Protestant (1952), percent unemployed (1960), and percent urban (1960). I cluster standard errors at the state-level.

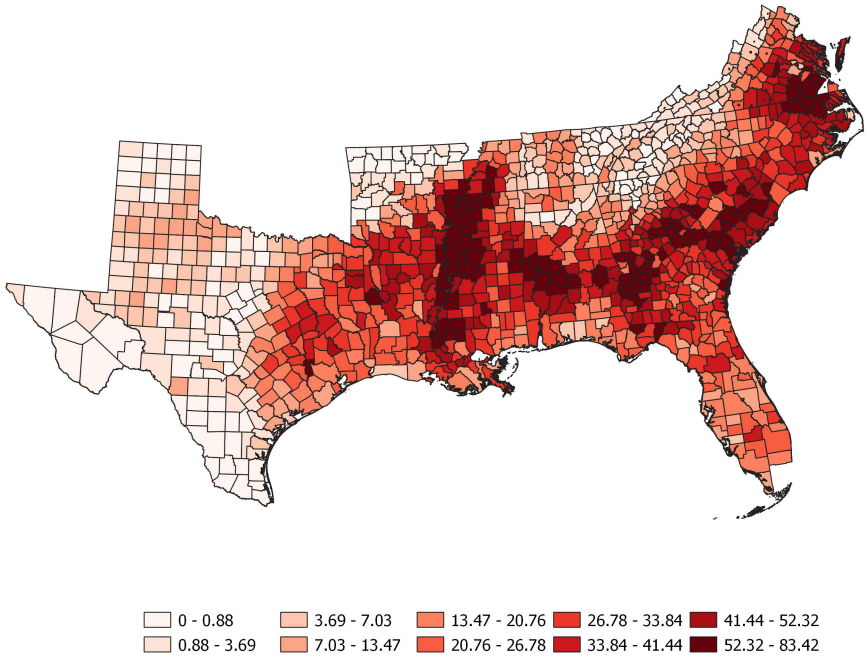


Figure 2.A20: Percent Black Distribution in the South

This figure displays a heat map distribution of percent black at the county level.

Table 2.A9: Comparisons of Differential Effects by Local Organization on the Race Issue: White Resistance (KKK) and Non-Discriminatory Public Amenities (Green Book Establishments)

	(1)	(2)	(3)
$South(\times KKK \cdot GB) - South(\times KKK \cdot NoGB)$	-4.465**	-7.383***	-4.986**
	(2.135)	(2.340)	(2.096)
	[0.042]	[0.003]	[0.021]
$South(\times KKK \cdot GB) - South(\times NoKKK \cdot GB)$	-4.161*	-7.892***	-5.373**
	(2.137)	(2.837)	(2.385)
	[0.058]	[0.008]	[0.029]
$South(\times KKK \cdot GB) - South(\times NoKKK \cdot NoGB)$	-2.988	-6.222***	-4.702**
	(1.975)	(2.220)	(2.137)
	[0.137]	[0.007]	[0.033]
County FEs	X	X	X
State-Year FEs	X	X	X
Pair FEs		X	
Free	X	X	X
Covariates	X	X	X
Black Heterogeneity			X
Mean	43.983	43.951	43.983
SD	15.229	15.359	15.229
Obs.	24,094	5,501,526	24,094

This table displays differences in Southern decomposition terms stored in table 2.4. Each row compares McIntire's Southern differential effect in Klan and Green Book counties to the remaining three regional effects. Column (1) corresponds to estimating equation (2.3) after substituting $Post_t \cdot Signal_c \cdot South_c$ with the decomposition terms in equation (2.7). Column (2) corresponds to estimating over the pairwise dataset and including pairwise fixed effects. Column (3) controls for heterogeneity in the Southern differential by percent black ($(Post_t \cdot Signal_c \cdot South_c \cdot Black_c)$). Standard errors are in parentheses, and p-values are in brackets. I cluster standard errors at the state-level with exception to two-way clustering at the state and pair level in column (2).

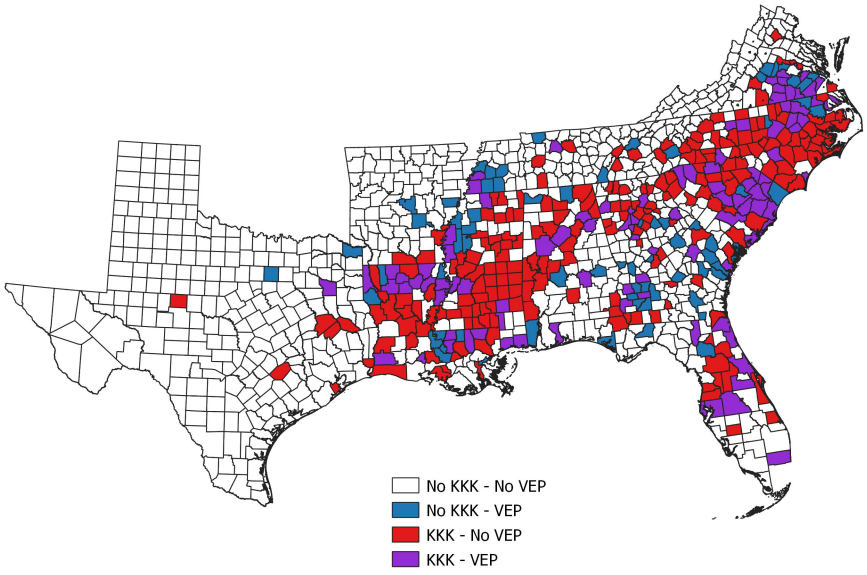


Figure 2.A21: Organized White Resistence (KKK) and Black Voter Mobilization Projects (VEP) in the South

This figure displays Southern counties by KKK and VEP presence. Counties with neither are displayed in white; only a VEP mobilization project in blue; only the Klan in red; and both in purple.

Table 2.A10: Differential Effects by Local Organization on the Race Issue: White Resistance (KKK) and Black Voter Mobilization Projects (VEP)

	(1)	(2)	(3)	(4)
Panel A: Non-South				
Post $\cdot Signal$	0.871***	0.871***	0.912***	0.871***
	(0.298)	(0.298)	(0.302)	(0.298)
Panel B: Southern Decomposition (Post ·Signal · South)				
$\times VEP$	-4.197*			
	(2.168)			
$\times NoVEP$	-1.831			
	(1.123)			
$\times KKK \cdot VEP$		-5.759***	-6.694***	-11.193***
		(2.006)	(1.924)	(2.336)
$\times KKK \cdot NoVEP$		-0.329	-1.588	-3.806
		(1.455)	(1.754)	(2.538)
$\times NoKKK \cdot VEP$		-4.173	-4.304	-10.496*
		(5.275)	(5.120)	(6.149)
$\times NoKKK \cdot NoVEP$		-1.925	-2.043	-4.053**
		(1.322)	(1.307)	(1.780)
County FEs	X	X	X	X
State-Year FEs	X	X	X	X
Pair FEs			X	
Free	X	X	X	X
Covariates	X	X	X	X
Black Heterogeneity				X
Mean	43.949	43.949	43.915	43.949
SD	15.229	15.229	15.360	15.229
Obs.	24,485	24,485	5,590,134	24,485

This table displays results from estimating equation (2.3) after replacing the triple difference-in-differences term ($Post_t \cdot Signal_c \cdot South_c$) with the decomposition in equation (2.7) (substitute VEP for Green Book). Panel (a) stores estimates of the difference-in-differences coefficient scaling ($Post_t \cdot Signal_c$); panel (b) estimates of the coefficients scaling the decomposition terms. Each regression internalizes county fixed effects, state-by-year fixed effects, predicted "free-space' signal strengths, and a set of "flexible" (post and post-by-South interactions) covariates. This set includes percent black (1960), percent high school (1960), percent Protestant (1952), percent unemployed (1960), and percent urban (1960). Columns (1) and (2) include decompositions by KKK and VEP mobilization projects separately. Column (3) includes all four interaction terms. Column (4) displays results from estimating over the pairwise dataset and including pairwise fixed effects. Column (5) controls for heterogeneity in the Southern differential by percent black (($Post_t \cdot Signal_c \cdot South_c \cdot Black_c$)). I cluster standard errors at the state-level with exception to two-way clustering at the state and pair level in column (4).

Table 2.A11: Comparisons of Differential Effects by Local Organization on the Race Issue: White Resistance (KKK) and Black Voter Mobilization Projects (VEP)

	(1)	(2)	(3)
$South(\times KKK \cdot VEP) - South(\times KKK \cdot NoVEP)$	-5.429*	-5.106*	-7.387***
	(2.797)	(2.968)	(2.618)
	[0.058]	[0.092]	[0.007]
$South(\times KKK \cdot VEP) - South(\times NoKKK \cdot VEP)$	-1.586	-2.390	-0.696
	(6.330)	(6.021)	(6.742)
	[0.803]	[0.693]	[0.918]
$South(\times KKK \cdot VEP) - South(\times NoKKK \cdot NoVEP)$	-3.834	-4.651**	-7.139***
	(2.353)	(2.259)	(2.322)
	[0.110]	[0.045]	[0.004]
County FEs	X	X	X
State-Year FEs	X	X	X
Pair FEs		X	
Free	X	X	X
Covariates	X	X	X
Black Heterogeneity			X
Mean	43.949	43.915	43.949
SD	15.229	15.360	15.229
Obs.	24,485	5,590,134	24,485

This table displays differences in Southern decomposition terms stored in table 2.A10. Each row compares McIntire's Southern differential effect in Klan and VEP mobilization project counties to the remaining three regional effects. Column (1) corresponds to estimating equation (2.3) after substituting $Post_t \cdot Signal_c \cdot South_c$ with the decomposition terms in equation (2.7). Column (2) corresponds to estimating over the pairwise dataset and including pairwise fixed effects. Column (3) controls for heterogeneity in the Southern differential by percent black ($(Post_t \cdot Signal_c \cdot South_c \cdot Black_c)$). Standard errors are in parentheses, and p-values are in brackets. I cluster standard errors at the state-level with exception to two-way clustering at the state and pair level in column (2).

2.8.2 Additional Primary Source Materials

TCRH Transcripts following the University of Mississippi Riot

Despite the *Brown v. Board (1954)* verdict, public education in Alabama, Mississippi, and South Carolina was segregated through the 1961 academic year. James Meredith attempted to reverse this trend in Fall 1962 by integrating Mississippi's flagship public university. His efforts were met with resistance from high-ranking officials such as Governor Ross Barnett and General Edwin Walker. Violence ensued September 30, 1962. President Kennedy responded by mobilizing the National Guard in Oxford (Scheips, 2005). Ruotsila (2015) credits this incident as the catalyst of McIntire's growing interest in Civil Rights discourse. Below, I provide excerpts (10/1/1962 - 10/4/1962) from *TCRH* transcripts generated from audio files shared by the Princeton Theological Seminary to support this claim.

The transcripts emphasize McIntire's constitutional arguments in favor of states' rights pertaining to the race issue. On October 1, he discusses the relevancy of *Brown v. Board (1954)* to Meredith's enrollment. McIntire rails against "judicial decree" supplanting the legislative process, uplifts the 9th and 10th amendments, and argues that Meredith's case of "token integration" does not fit the scope of the *Brown v. Board (1954)* verdict. On October 2, McIntire continues to prop up states' rights while claiming that Governor Barnett ought to have "equal time" to address his fellow Mississippians compared to President Kennedy. On October 3, he details General Walker's arrest. McIntire also points out a supposed double-standard between Walker and Martin Luther King, Jr.: both protest and instigate disorder, but the former is "examined for his mental consistency" while the latter is "a great hero". On October 4, McIntire outlines CORE's efforts

"to launch economic warfare against Mississippi". He also mentions the NAACP, SNCC, and SCLC.[37]

Excerpt from October 1, 1962

> I'd like to express my opinion on this situation down Mississippi. I know many of you people are wondering just what Dr. McIntire does think about this whole development and I would like to say to you people that my sympathies in this struggle are with the governor of Mississippi and with the state's rights principle that is involved in this present conflict. My problem in discussing this is simply that we have a delayed program and by the time this broadcast gets down into the south, some of you, a lot of other things may have happened but I heard the president's speech when he delivered it to the nation. Amen, Charlie and I sat down and listened to the whole thing together and my understanding of what the president said simply begged the great issue that is at stake so far as the Constitution of the United States is concerned. The putting of just one Negro on the campus and Mississippi isn't integration and the great question that's before the people of the United States is the Constitution of the United States of America. Now let me go back and express my opinion as I have in times passed about the development that brought on this terrible confusion among ourselves as a free people. This goes right back to the Supreme Court of the United States. That's where it belongs. It's decision of May the 17th 1954. Do you know that on May 16th 1954, the day before that decision was made. It was perfectly constitutional. It was perfectly lawful. It was perfectly proper in

[37] The conversion from audio to text for the show on October 4, 1962 resulted in no punctuation. This paper does not attempt to add punctuation to the converted file. As a rule of thumb, only minor edits were made to the text output, e.g. "Karl" to "Carl".

this country to have separate but equal facilities in the question of segregation. That was indeed the law of the land as the Supreme Court had interpreted the Constitution and ratified that position repeatedly. In other words, the Supreme Court recognized the right of sovereign states to handle this question and under that right some of our states had integration. Some of the states had integration and segregation at the same time and then some of the states particularly the ones down in the south simply had segregation. This was an exercise of a right which is reserved to the states in the 9th and the 10th amendments of the Constitution of the United States. Nine justices sitting on the Supreme Court of the United States changed all of this by an opinion which they delivered on May the 17th 1954. Within 24 hours all the states through the South were in violation of the Constitution and furthermore the people of the South had nothing to say about this change whatsoever. You know in our country we're supposed to make the laws on the basis of the will of the people. We elect legislators, we elect congressmen. They go down and they make the laws in behalf of the people. The Supreme Court is not a law making body and what we had at this particular instance was a case when the Supreme Court became a legislative body in effect. It changed the Constitution of the United States simply by its own decree and by its own fire. And what we have at stake at this great moment beloved isn't the question just of judicial orders being carried out by marshals. But we have here in the overall picture the illustration of how a great constitutional change has taken place in this country by judicial interpretation. And the states have lost certain rights which they previously had and the federal government has become supreme and all powerful in this field. And the states are literally helpless in the presence of this

grasp of power which the Supreme Court has taken to the federal government. Now maybe some of you people don't agree with me but I see it that way and I have a right to express my opinion in these regards here in the United States of America. And furthermore I am afraid of the Supreme Court of the United States. Once that door has been opened up in this way as we see it at this particular point to change a social pattern in this nation by judicial fight once that has been done that same procedure can be done to change policies and positions of the Supreme Court on other matters in days past and it could involve before we get through the fundamental extremists you know. And furthermore it could involve that the Supreme Court in some way would decree that we were a part of the world government. If they can take away state rights by judicial decree in this matter they may be able by a judicial decree to put us in a world government. We have an open door right there at the gate of the Supreme Court of the United States which is a very dangerous thing and which way as freedom loving Americans have to watch and should be deeply concerned about. Of course the Supreme Court could reverse itself again and decree that separate but equal were the proper provisions of the Constitution and if that were to take place it would require of course a change in the justices of the present Supreme Court and it might take a good many years for a thing like that to take place. But my opinion of what's happening right now is that we have been thrown into confusion by the Supreme Court legislating and seeking to change a social pattern which had been established as a result of the court's interpretation of the Constitution. And if our social patterns are to be changed in this nation they should be changed by the people through proper legislative channels and not by judicial decree. Amen. Now folks if

you agree with what Carl McIntire has said right now I wish to sit down and write me a letter and tell me so. I really do. I wish you'd let me know. I just like to know I don't think that the great bulk of the American people are in favor of what's happening down there in Mississippi. I just don't think they are. I know I'm not in favor of it and furthermore this idea that you put one Negro on a university campus isn't integration. My goodness that's not what the Supreme Court has decreed. A token integration isn't what the Supreme Court was trying to impose upon the South and the American people read the decision of May the 17th 1954. They intended that there would be complete and total integration of the races in all the schools and that there would be no deviation from this idea otherwise there would be discrimination and feelings of inferiority on the part of the Negro race in this particular. If you agree with what I said I'd appreciate it very much if you'd write me and tell me so. And you know when the President and the nation gets himself all tied up and the knot that is tied up in right now and we're told that we must honor the law. We've got to do we've got to carry out this decree of the Supreme Court. We get ourselves all tied up in this knot and the President gathers these troops and these police power to overwhelm sovereign state.

Excerpt from October 2, 1962

And since the whole country is upset and everybody's wondering what to think and what to do in regard to Mississippi and the problems that we have there, I decided that I'd just read the Constitution to you because I'm of the opinion that the root of this whole problem is a constitutional crisis of the first order in this nation. And I

think the good people down in Mississippi are just as good Americans as you are. And I think the good people that are struggling over this issue are just as loyal to our country as anybody else could be, but we are caught in a conflict over the Constitution of the United States. That's where the battle line actually is. Now we have ten amendments to this Constitution, and I want to read to you first the eighth or rather than ninth amendment and then the tenth amendment, just as they are. This is the ninth amendment. The enumeration in the Constitution of certain rights shall not be construed to deny or disparage others retained by the people. Now that is utterly, utterly magnificent. There are certain rights enumerated, freedom of speech, the free exercise of religion, the right of assemblage to petition the government for redress of grievances. They are all enumerated, but our fathers were wise enough to say that after they had enumerated these things it didn't imply that those that aren't mentioned in any way don't belong to the people. And the great reservoir of authority in this land is the people. That's what that Constitution said. We are the people and it is a government of the people by the people and for the people. That's the undergirding principle of that ninth amendment. Now the tenth amendment, procedures follows. The powers not delegated to the United States by the Constitution, nor prohibited by it to the states, are reserved to the states respectfully or to the people. In other words, our federal government is made up only of delegated powers. They're restricted, they're limited. Our founding fathers were afraid of a powerful centralized federal government. That's right. They were afraid of. Furthermore, we read the powers not delegated to the United States, nor prohibited to the states by it. In other words, they did place certain prohibitions upon the state in order to delegate powers to the federal

government. That's what they did. But the passage goes on to say, our reserves to the states respectfully or to the people. In other words, the great reservoir of power and authority in the governing and the operation of the lives of the people of this nation belong to the states. That's the way this country was set up. We have sovereign states that join together to establish a federal government with specifically delegated authority and powers written into a Constitution. Now, for I just thank God for this with all my heart, because this has given to this blessed nation of ours the liberty which we enjoy. And when we started this broadcast, you know, I said one of the things we were going to stand up for was states rights, because I've been against the powerful centralization of authority in Washington, because I know that when you get it there, it's going to impinge upon the liberties of the people back in the grassroots. We know that's what's happening. We're experiencing it all the time. For tomorrow, I said we were going to stand for separation of church and state. I said we were going to stand against the development of a powerful one-world church and a powerful one-world government. Beloved, I think that the secret and the heritage which we have of liberty belongs to the people and to the local churches in this blessed land of ours. And I have by the grace and the help of God undertaken a broadcast that will champion these concepts of individualism and of state's rights. Yes, amen. Now, under the rights of the states, as we've had them here in this blessed land of ours, the province of education, the responsibility for education has always been in the hands of the people and of the states. We have our local school boards, and then we have our state institutions in the field of education. The federal government has not operated in that field, and thank God it has. Furthermore, we've been opposed to

all these undertakings and the projects whereby we would receive federal aid for the schools. Because I know that old Solomon knew what he was talking about when he said in the book of Proverbs that a man's gift make a throne for him. And the minute the federal government reaches down and begins to give this aid and that aid and some other aid and the more aid, the first thing you know, there's a lot of room in there for the one who's been doing the giving. And it's right at that point that we have resisted and I have stood against all these proposals to introduce federal aid or federal assistance into the whole field of education. It should remain in the hands of the states where the constitution placed it. Amen. Now if that is the case, and that's the way in which this Republic of ours has developed in its responsibilities of education, I must say to you people that it is a strange spectacle indeed today to see federal marshals and federal troops taking over the possession of a campus of a state university and proceeding by the force, by the police power of a federal government to place upon a state institution certain demands and certain restrictions for the conduct of that education. It's a strange spectacle to see it, but that's what we're witnessing at the present time. And I have approached this whole question from the standpoint of what the constitution says and about the liberty and the rights of the state in this field of education. You know our state, our states have been free and the development of their education to have all manner of school. They've had a school, they can have a school for women and some of them do. They can have a school for boys if they want to. They can have a school for the blind. They can have a school for agriculture. They can have a school for, well, for the Indians. They can have a school for the Negroes. They can have a school for the white people. Listen

folks, in the field of education in the state area, we've got all manners, state schools, and we've been free to develop this type of a thing here in this country. And right now beloved, we are witnessing a shifting and evolution, a modulation over into the area where the federal government is coming in and beginning to exercise power in the field of education. And it ought not to be under the constitution of the United States of America. That's right. Amen. We have a mighty constitutional crisis at the present moment in this country, and I'm thankful that I'm still free in this country to talk to you people as I see it because I think our liberty is involved in it. Folks, I don't believe that you can divide up freedom. And when you begin to take away the states rights, it's going to affect our religious rights. The minute you get into this area of denying human liberty and centralizing power in Washington, the churches are going to be caught up in the cauldron before you get through. I see that and I believe it. And I believe it will all not. If we're going to preserve religious liberty and the free exercise of religion in this country, we've got to protect the state's rights and the reserved rights which are guaranteed in this constitution to the individual in this republic. Amen. Well, let me go just a step further away. I'm just discussing some of these things, and this will be healthy, and I hope a lot of you people agree with him. As a matter of fact, I'd really like to know just how many of you people listening really agree with the position which we're seeking to maintain on this matter. I'd like to know. And I'd like to have a lot of you people sit down and write me and tell me that you agree with us and that you stand where we stand in this great fight to preserve our lives. And this is the right to preserve our state's rights and these reserved rights which the constitution guarantees to the people in our states. Well, yesterday, of

course, I discussed the Supreme Court decision in May 17, 1954 and pointed out that the Supreme Court made a radical change in its own activity, and in effect became a legislative body. Today, I'd like to point out that what the Supreme Court did in effect was amend the constitution by judicial action and not by the processes of amendment which are established in the constitution itself. Now, once the Supreme Court interprets the constitution, that's its function. Once that interpretation has been made and has been established in the practice and the social pattern of our country, if that is to be changed, it is not to be changed by the Supreme Court. It is to be changed by the amendment processes which the constitution provides for the changing of the constitution of the United States. And if we're going to change our social patterns, if we're going to take away states' rights, it should be done by the amendment process and not by judicial nullification or judicial interpretation. Amen. Now, right on, therefore, those of us who believe in the Democratic processes and believe in the freedom of speech and believe in the power of persuasion, and who believe that we, as a free people, are concerned and have a right to go to the ballot box and in the secret ballot cast our vote for whom we please before God, we are the ones who should change the social patterns of this nation by legislation and by the will of the people and not by judicial fight. Amen. That's right. I'd like to say this at this particular point, I was terribly disappointed that the networks did not give the governor of Louisiana an opportunity to have equal time to the president. I really was. I know a lot of people were. Now, I understand and I fully agree that when the president steps up to exercise his position as president and the chief executive in the areas of the military and other decisions which he has to make, he can report them

to the American people and he has that privilege and right and we want him to do it, but listen folks, the speech which he made to us all involved argument and involve debate on his part dealing with this question and it involved an appeal to the people of the Mississippi. And when it involved those elements, the governor of the state of Mississippi should have been given equal time to answer that appeal and to deal with the arguments which the president was presenting to the country. And I think that this country right now ought to have a good statement of the case of the people in Mississippi so far as the Constitution is concerned. I think that should be given to this country and we as a free people have to face up to these great constitutional issues which are involved. This is our Constitution folks. This is our country. We're the ones who elect the president and he's the one who appoints the judges, justices of the Supreme Court. This is aligned with the people of the rule and if the people are going to rule by majority vote in our decisions and in our elections we've got to have a full airing of these various issues when they come before this nation. Amen, that's right. Now they've arrested General Walker down there and I have an open letter here that General Walker sent to the president on September 26th about Cuba that came to the border.

Excerpt from October 3, 1962

Senator Army Major General Edwin A. Walker, charging he is being held as a political prisoner, was ordered yesterday to undergo psychiatric examination at the US Hospital for federal prisoners. Attorneys for Mr. Walker arrested Monday for his part in the University of Mississippi integration crisis have told the former general to

give prison officials and doctors only his name, rank and serial number, such as any American soldier would do if captured by the enemy. Well, if he does that, they're sure to say he's crazy. That's all he's going to talk about. Mr. Walker was charged in Oxford, Mississippi with rebellion, insurrection and seditious conspiracy. He was flown here to the hospital prison last night. One of these three attorneys, Clyde Watts of Oklahoma City himself a retired general, said he would seek a writ of habeas corpus or an injunction to bar doctors from performing any surgery or administering any medical treatment to Mr. Walker. US Attorney Russell Millen said the order committing Mr. Walker for psychiatric study was issued by the US District Judge Claude Clayton of the Northern District of Mississippi. Mr. Watts told Mr. Millen he already had the $100,000 bond necessary to free Mr. Walker, but Mr. Millen said the question of bond is now immaterial. Mr. Millen said under the terms of the order, Mr. Walker could be kept at the hospital, quote, for such reasonable time as the medical center may feel it necessary to complete the study. Mr. Millen said such an examination normally requires from 60 to 90 days. So you see what apparently is in store for Walker at the hands of the federal government. And then the story continues. Mr. Watts said Mr. Walker was committed yesterday on the basis of a statement by Dr. Charles E. Smith, Staff Psychiatrist of the US Bureau of Prisons. The attorney said he had asked Dr. Smith if he had seen Mr. Walker and the psychiatrist, but he said he had not. Mr. Watts said he talked to Mr. Walker for almost an hour. This is the attorney you see for Walker earlier yesterday and found him clear, lucid, undisturbed, and in complete possession of all mental faculties. Mr. Watts said Mr. Walker denied all the charges made against him. Mr. Watts said Mr. Walker in fact

had urged that any protest in Oxford be made peacefully. At no time did Walker make any threats to federal officer. Mr. Watts said that no time did he go with him 30 feet of an officer. He told them the real war was in Cuba and not in Mississippi. I ask all citizens of the United States to examine what happened today when General Walker raised his voice. Mr. Watts said in a statement, he believes he is being held as a political prisoner. This can now happen to any person who disagrees with the government. Then the story goes on to say that he could be committed for 39 years in prison and fined a total of $40,000. What concerns me is that he's taken down there in this struggle and the implication clearly is now that there's something wrong with his brain or with his sanity and all of that is spread all over the world concerning General Walker. Now I just want to say to you people everywhere that I'm afraid that you've got an instance here of the way in which the rightists are going to be treated. We've been told by the psychiatrists that they had a convention recently in St. Louis that all the rightists needed to have their heads examined and they were serious about it. We've been told by some of these other professors from Columbia University and elsewhere that the rightists are not properly orientated and that they are unbalanced mentally. That type of thing is being said. It's a weapon that's being used by the leftists to discredit the conservatives in their battle for freedom. Then we have these mental health programs. I guess possibly they'd get ready to send us off to Alaska or somewhere else. But anybody who gets up and talks like this and begins to manifest some vigor of protest they think he's crazy. I might say this that if Martin Luther King as he has done repeatedly down there in the south gets into some sort of mob action, he turns out to be a great hero with his picture on the front page of some

national magazine. But when Walker goes in and manifests his feelings in regard to a protest, not insurrection, why he's accused of insurrection and conspiracy and hailed off to a hospital to be examined for his mental consistency or something else. I'll tell you folks, it's a pretty one sided affair that we're facing at the present moment and I am going to step right up to this whole issue because it's not only Walker, it's Carl McIntire right now.

Excerpt from October 4, 1962

now in regard to the developments down in Mississippi that we've been discussing from the standpoint of the Constitution of the United States and do you know what I think I ought to do I think I ought to give everybody a copy of the Constitution of the United States yeah man something like that would help us right now is nothing else to clarify the understanding of the Constitution and the rights that we have there but here's a story in the Wall Street Journal of New York City for October 3rd Persuading industry to avoid Mississippi is program of militant integrationist group in a modest third story office near New York City's Wall Street's financial district 1,000 miles from Strife-Torn Oxford Mississippi a militant integrationist organization is preparing to launch economic warfare against Mississippi Presiding over the planning is tall 42 year old James Farmer National Director of the Congress of Racial Equality which claims 49,000 members core will try to dissuade corporations from building factories in Mississippi in an effort to undermine the state's industrialization program if companies defy the organization their products may be boycotted in Washington it will urge the federal government to cancel all construction on any

facilities that might be put to segregated use then the story comes down and it lists the various groups that are lining up behind this program of boycotting Mississippi the National Association for the Advancement of Colored People which also has headquarters in New York believes the events in Oxford will shock many normally complacent Negroes into joining the 360,000 member organization quote this type of thing gets them mad says John a more sell assistant to Roy Wilkins the NAACP executive secretary it always results in an increased level of interest on the part of Negro sometimes I wonder what we would do without allies like Mississippi governor Ross Barnett in keeping this thing alive now isn't that interesting the way they twist this thing around actually it is the NAACP that's behind Meredith and now they're behind the young woman that we are told is going to register so the NAACP is the organization that brings this thing to a head in this provocative manner and then they turn around and say that as a result of all the provocation they're getting Negroes mad around the country and they're increasing increasing the membership of their own organization it's a nice way to get new members at least from their standpoint now may I go on just a little further and read you this ranged on the integration side with core and NAACP are such organizations as the Southern Christian leadership conference based in Atlanta and headed by the Reverend Martin Luther King jr the student non-violent coordinating committee also operating from Atlanta and such biracial or sympathetic groups as the American Civil Liberties Union the urban league the American Friends Service Committee a Quaker organization and the Anti-Defamation League of Benite Berythe and also the Southern Regional Council and Atlantic Group the story in the Wall Street Journal continues at some length to

explain that the next state on which this whole combination of organizations is going to concentrate is South Carolina now I ask you people those of you who followed my discussion of the constitutional questions that were raised in which we've sought to clarify for a lot of you people concerning states rights to write me if you approved of the views which I had I want to thank you people because the letters have really began to come in here and I thank God that there are many many of you people who look at the Constitution in the way in which we've sought to expound it and the way in which we believe is the historic position that every American should take do you know folks a lot of people have the idea that the Constitution is something that the courts look after they love it the Constitution is something you need to look after the Constitution is something that every citizen of this nation should understand and not only should he understand it but he should have the great principles of it written in his very nature is very being and in his heart and particularly these first ten amendments which is the Bill of Rights which our fathers gave to you and me and this isn't something that we should leave to the courts this is something which should be left to the people of this country who are the ones who amend this Constitution and who are the ones who should look after this Constitution and see that it's honored and that it is protected as a matter of fact the Constitution because of the people requires that our congressmen and our officers in our government take an oath to defend and maintain and protect the Constitution of the United States of America

FBI Files Establishing McIntire's Influence on, and Connection with, the KKK

I use KKK organization to proxy for pro-segregation attitudes at the county level. Below, I list a collection of excerpts from McIntire's FBI files to demonstrate his influence on, and connection with, the Klan. These excerpts outline the Klan's efforts to retain McIntire as a public speaker, distribute his print materials, and encourage attendance at his public speeches. They also implore McIntire to use his media footprint to back an Independence Day (1964) "March on Congress". Note that redacted information will be labeled as follows: "*Redacted*".

> *Redacted* advised that the National Ku Klux Klan (157-168) held a two day meeting in Atlanta, on May 5 and 6, 1962, at 631 Peeples Street S.W. At the session on 5/5/1962 *Redacted Redacted* stated that the Negroes in Atlanta expected to integrate all facilities in connection with the NAACP facility in July. *Redacted* explained that in order to offset the NAACP convention, it would be necessary to have topnotch speakers for the events to take place. He indicated that an effort was being made to obtain Dr. Carl McIntyre of New Jersey as a speaker.
>
> *Redacted* adviced that at the weekly meeting of the Little Rock Klavern of the Association of Arkansas Klans of the Ku Klux Klan (157-92) on 12/27/62, pamphlets were passed out which were written by Carl McIntire, editor of the "Christian Beacon". The pamphlets included those entitled: "False Prophets", "Bankrupt United Nations", and "Why Christians Should Fight Communism".
>
> *Redacted* advised in connection with information concerning weekly meetings of the Association of Arkansas Klans of the Ku Klux Klan (157-92) that on 5/6/63 there was a long discussion of Anti-Communist and Pro-Segregationist literature which

has been distributed by Reverend Billy James Hargis, Carl McIntyre, Wayne Poucher and former General Edwin A. Walker.

Redacted advised on 11/21/63 that a letter had been received from the State Grand Dragon advising that the next state meeting of the Association of South Carolina Klans (105-38068) would be held at West Columbia on 12/1/63. The letter also cited as important the "Prayer Petition" being circulated by Dr. Carl McIntire and the Twentieth Century Reformation Hour, Collingswood, N.J. Members were urged to write Dr. McIntire for blank petitions.

Redacted furnished a copy of a letter from the Grand Dragon to all Klaverns of the Association of South Carolina Klans (105-38068), dated 4/3/64. The letter announced a state meeting to be held on 4/12/64 in West Columbia, and it also urged members to attend one or both of "the great patriotic meetings this coming week". One meeting was an address of Billy James Hargis at Greenville, S.C. on 4/9/64, and the other of Dr. Carl McIntire at Charlotte, N.C. on 4/10/64.

Redacted advised that a National Meeting of the National Knights of the Ku Klux Klan (157-168) was held on March 7-8, 1964, Tucker, GA. On 3/8/64 there was a discussion concerning a march on Congress on 7/4/64. *Redacted* explained that the march was set for 7/4/64 in order to have time to raise money and to advertise. He said he tried to organize a march on Washington for 2/2/64 but did not have the time or finances. He said that Billy James Hargis and Carl McIntyre were going to announce the march on Congress on their radio programs and they would support the march.

Redacted advised that a National Meeting of the National Knights of the Ku Klux Klan (157-168) was held on June 6,7, 1964 at Tucker, GA. At the meeting on 6/7/64 *Redacted* of the Association of South Carolina Klans, Columbia, S.C., made a motion that every state ask their members to write Carl McIntire and Billy Gene Hargess asking them to announce on their programs the march on Washington on 7/4/64.

The following references in the file captioned "Committee of One Million Caucasions to March on Congress July 4, 1964" (157-1586) contain information concerning Dr. Carl McIntire. This information related mainly to efforts of the Ku Klux Klan to obtain the support of Dr. McIntire in promoting and advertising the proposed March on Congress. According to information furnished by informants of the *Redacted Redacted* Offices, at meetings of the KKK in Georgia and South Carolina, members were urged to attend a patriotic meeting in Charlotte, N.C. on 4/10/64 at which Dr. McIntire was the featured speaker, and at the meeting in Charlotte an official of the Association of South Carolina Klans of the KKK contacted McIntire to enlist his aid for the proposed "march". It was noted that in private conversation Dr. McIntire had advised the KKK oficial that *Redacted* was in the process of organizing a "Segregationist Youth Movement".

It was also noted that an account of the meeting at which McIntire spoke was carried in the "Charlotte Observer" on 4/11/64, did not indicate that McIntire had mentioned the March on Congress.

2.8.3 Additional Information on Calculating Signal Strength Measures

Deriving Equation (2.1) from Goldsmith's Equation (2.39)

Begin with Goldsmith's equation (2.39):

$$P_r = P_t K \left[\frac{d_0}{d}\right]^{\gamma},$$

where P_r is received power, P_t transmitted power, K a unitless constant, d_0 a reference distance, d transmission distance, and γ the path-loss exponent.

Convert each side of the equation to decibels, i.e. take log base 10 of each side scaled by a multiple of 10 (The "10" cancels out on both sides.):

$$log_{10}P_r = log_{10}\Big(P_t K \left[\frac{d_0}{d}\right]^{\gamma}\Big).$$

Distribute the log base 10 on the right-hand side. Flip $\frac{d_0}{d}$ and move exponent γ in front of the log operator:

$$log_{10}P_r = log_{10}P_t + log_{10}K - \gamma log_{10}\tfrac{d}{d_0}.$$

Multiple each side by $\frac{10}{10}$. The term $10\gamma log_{10}\frac{d}{d_0}$ is the measure for path loss. Re-arrange such that path loss is expressed in terms of the remaining variables:

$$-\tfrac{PathLoss}{10} = \Big(log_{10}P_r - log_{10}P_t - log_{10}K\Big) = log_{10}\Big(\tfrac{P_r K}{P_t}\Big).$$

Take the exponential function base 10 of each side:

$$10^{-\frac{PathLoss}{10}} = 10^{log_{10}\left(\frac{P_r K}{P_t}\right)} = \tfrac{P_r K}{P_t}.$$

Solve for P_r:

$$P_r = \frac{P_t}{K\left(10^{\frac{PathLoss}{10}}\right)}.$$

Applying my notation, received power P_r at the county level is $Signal_c$, transmitted power P_t at the county level is $Power_c$, and path loss at the county level (for predicted signal strength) is $EffectivePathLoss_c$:

$$Signal_c = \frac{Power_c}{K\left(10^{\frac{EffectivePathLoss_c}{10}}\right)}.$$

This is equation (2.1) with exception to the unitless constant K in the denominator. The modified ITM software does not directly measure K, but instead internalizes determinants of K while calculating path loss. In other words, effective path loss "absorbs" the constant. I will discuss the software inputs in more detail below. This feature of the software yields equation (2.1):

$$Signal_c = \frac{Power_c}{\left(10^{\frac{EffectivePathLoss_c}{10}}\right)}.$$

A Detailed Explanation of the Application of Olken's modified ITM Software

Four tab-delimited text files are required to run the software: locations.txt, channels.txt, transmitters.txt, and receivers.txt. The locations text file includes city name, latitude, and longitude; the channels text file channel number and frequency (mhz), where both entries must be integers; the transmitters file station name, city name, channel number, and power (kw); and the receivers file latitude and longitude, where county-level centroids are used as geocoordinates.

Note that while the readme file includes height (m) as an input in transmitters.txt, the test example included with the software does not include this variable. For this reason, I do not include it either. Many of the antenna heights of transmitters airing McIntire's program are missing in the television and radio yearbooks scraped for data construction. Instead, antenna

height is assumed uniform across stations and set to the default parameter value in the ITM software. This information is described in more detail below.

Using these four text files, I generate a query file to be processed by the application "itm-olken". I copy the path of the query file and paste in the "Query File" tab. I preload the "globe.dat" file provided with the software as the "Terrrain Handler" selection. Additional parameter selections are left at their default levels: "Surface Refractivity" 360 N-units, "Dielectric Constant of Ground" 15.0, and "Conductivity of Ground" 0.005 S/m. The "Radio Climate" is set to "Equatorial" and "Polarization" to "Vertical". Finally, the "Antenna Heights" are set to 10 m.

In deriving equation (2.1) from Goldsmith's equation (2.39), I claim that the unitless constant K is internalized by the effective loss calculation. This claim is supported by the parameter terms factoring into these calculations. Antenna height, surface refractivity, dielectric constant of ground, and radio climate all influence the value of K.

Running the application yields a query output file with distance (km), terrain delta height, free space loss (dB), and an n x m effective loss table with entries varying by reliability and confidence levels. For estimation, I use the first listed effective loss measure in the output file. From a visual inspection of the data, reliability and confidence levels have a small influence on the results.

One concern is that antenna heights are only assumed to be 10 meters. This value appears quite low from visually inspecting the non-missing heights of transmitters in operation during the 1960s. For robustness, I recompute signal strength terms for uniform antenna heights of 100 meters. Table 2.A12 displays results from re-estimating equation (2.3). I continue to find a statistically significant Southern differential term for democratic vote share that is not explained by a mobilization effect.

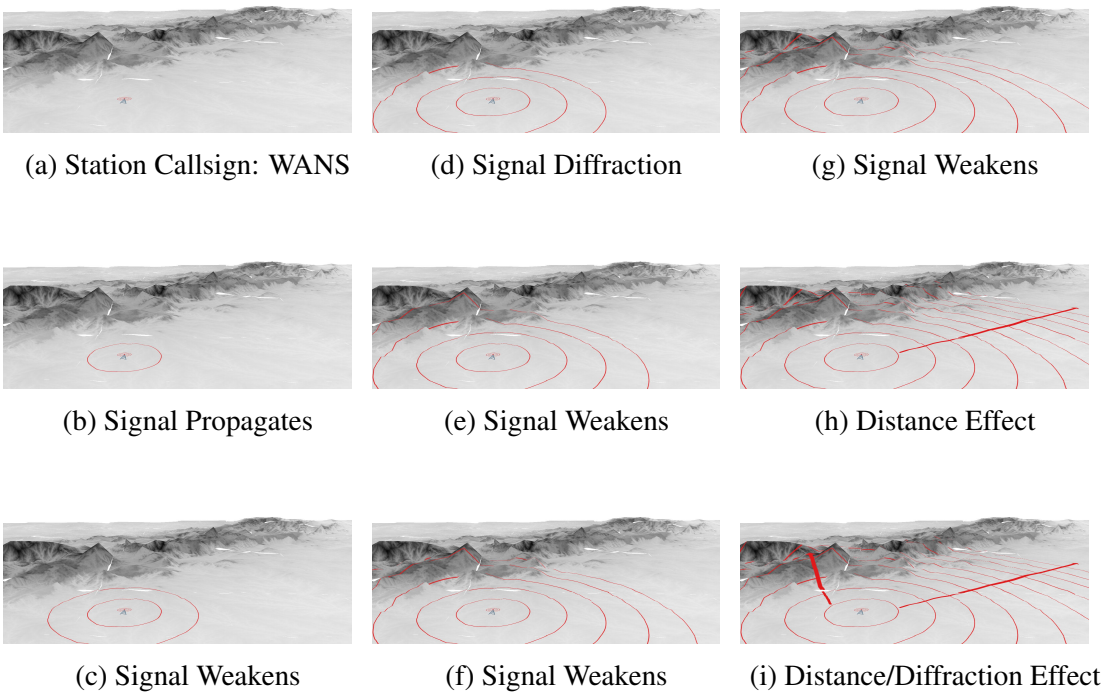
Table 2.A12: The Impact of McIntire's *TCRH* on Democratic Vote Share: Using Predicted Signal Strengths with Antenna Heights set to 100 Meters

	(1) VS	(2) VS	(3) Turn	(4) Turn
Post $\cdot Signal$	1.361***	1.458***	-0.170	-0.246
	(0.425)	(0.334)	(0.271)	(0.263)
Post $\cdot Signal \cdot South$	-2.799*	-3.185**	0.406	-0.067
	(1.650)	(1.463)	(0.831)	(0.512)
County FEs	X	X	X	X
State-Year FEs	X	X	X	X
Free	X	X	X	X
Covariates		X		X
Mean	43.960	43.949	60.818	60.838
SD	15.257	15.229	16.041	15.994
Obs.	24,653	24,485	24,707	24,544

This table displays results from re-estimating equation (2.3) after measuring signal strengths with antenna heights set to 100 meters. The first (second) pair of columns use Democratic vote share (turnout) as the dependent variable. Each regression internalizes county fixed effects, state-by-year fixed effects, and predicted "free-space' signal strengths. I re-estimate "flexibly" (post-period interactions) controlling for the set of covariates referenced in the main text: percent black (1960), percent high school (1960), percent Protestant (1952), percent unemployed (1960), and percent urban (1960). The triple difference-in-differences regressions also include post-by-South covariate interactions and triple "free-space" interactions ($Post_t \cdot Free_c \cdot South_c$). I cluster standard errors at the state-level.

2.8.4 Stylized Illustration of the Empirical Design

Figure 2.A22 is a stylized illustration of the empirical design. This figure consists of nine panels and visualizes the source of plausibly exogenous variation leveraged for causal inference. For ease of exposition, I restrict attention to a single station (WANS) in Anderson County, South Carolina located outside the foothills of the Appalachian Region. This station is represented by the transmitter marker in panel (a). In subsequent panels, the signal propagates through space, as represented by the expansion of red circular rings surrounding the station transmitter. In panels (b) and (c), the signal weakens with the distance traveled. As the signal approached the foothills in panel (d), it diffracts off the mountain ranges' surface, and further weakens. The signal strength continues to decline from these two sources in panels (e)-(g). The arrow drawn from the signal's source in panel (h) intersects counties with gradually weakening signals mostly due to distance. In contrast, the second arrow added in panel (i) intersects counties with gradually weakening signals due to both distance *and* diffraction. Counties intersecting the furthest visible ring from the source are roughly equidistant to the transmitter. However, counties fitting this description on opposite sides of the Appalachian Mountains will nonetheless have different levels of media exposure due to diffraction. This paper leverages differences in signal strengths induced by this plausibly exogenous source.



(a) Station Callsign: WANS (d) Signal Diffraction (g) Signal Weakens

(b) Signal Propagates (e) Signal Weakens (h) Distance Effect

(c) Signal Weakens (f) Signal Weakens (i) Distance/Diffraction Effect

Figure 2.A22: A Stylized Illustration of the Empirical Design

This figure is a stylized illustration of the plausibly exogenous variation in McIntire's signal strength leveraged for causal inference. The figure restricts attention to a single station outside the Appalachian Region for ease of exposition. Each panel depicts the propagation of the signal through space from its source (transmitter): this effect is visually represented by the expanding red rings surrounding the source. The directional arrows in panel (i) capture stark differences in signal coverage. One arrow diffracts off the mountain ranges' surface, while the other arrow travels along a comparatively smoother path. As a result, equidistant counties to the source situated on opposite sides of the Appalachian mountains have variation in signal strengths due to diffraction. The identification assumption is that variation in media exposure induced by irregular topographical contours is as good as random.

2.8.5 Alternative Covariate & Fixed Effect Combinations

In the main text, I control for county fixed effects, state-by-year fixed effects, and predicted "free-space" signal strengths. I show that adding a "flexible" (post-period and post-by-South interactions) set of covariates does not meaningfully impact the results. However, it remains to be demonstrated how the estimates respond to different fixed effect and covariate combinations. This section will establish robustness across these alternative control sets.

Figure 2.A23 re-estimates equation (2.3) following the exclusion of a given set of covariates and/or fixed effects listed on the y-axis. Results highlight that omitting regional differentials across covariates, percent black, percent urban, and all covariates with exception to percent black and urban, more or less yield the baseline estimates. Removing predicted "free-space" signal strength pushes the Southern differential to zero. In other words, permitting confounding between a "pure distance" effect and an irregular topographical effect eliminates McIntire's contribution to partisan realignment. This result emphasizes the need to include this term for the empirical design to be exogenous. Dropping county-level fixed effects does not alter the estimates, but dropping the state-by-year effects significantly amplifies the measured effect. Therefore, the results are not sensitive to time-invariant differences across counties, but otherwise pick up region-wide partisan realignment without state-by-year fixed effects.

One additional concern is that the set of covariates internalized for estimation is dependent on modeler's choice. Perhaps other covariates besides the following should be included: percent black (1960), percent high school (1960), percent Protestant (1952), percent unemployed (1960), and percent urban (1960). To assuage these concerns, I re-estimate equation (2.3) using double-selection lasso linear regressions. These regressions implement a data-driven, selection method to

determine the control set. Selections are made over all covariates, and their regional interactions, included in table 2.1, as well as state-by-year fixed effects.[38] A plugin iterative formula and Bayesian information criterion (BIC) are used as selection methods. Both methods select all state-by-year fixed effects, while the plugin method also selects percent high school, log of total population (1960), percent male (1960), percent unemployed (1960), percent urban (1960), percent urban by South (1960), percent Protestant (1952), and percent Catholic (1952). Both selection methods show that McIntire had a statistically significant impact on realignment.

[38] County fixed effects are excluded in the interest of limiting computing time.

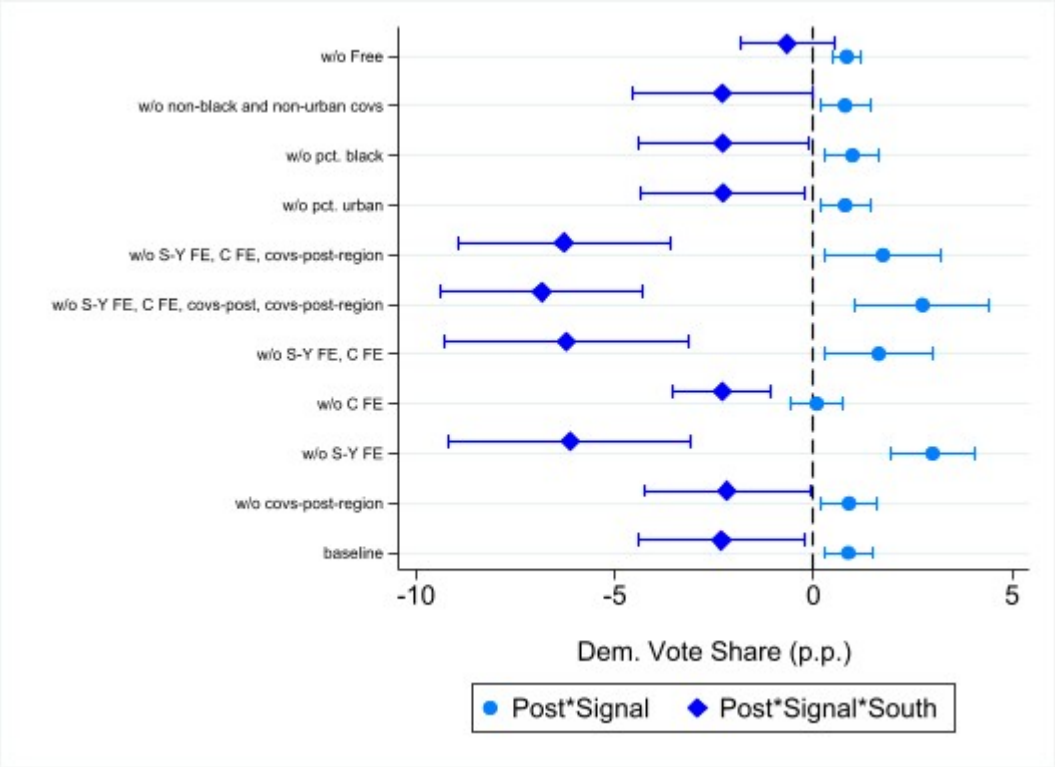


Figure 2.A23: Alternative Covariate and Fixed Effect Combinations

This figure displays baseline results from estimating equation (2.3) excluding designated components of the preferred specification: county fixed effects, state-by-year fixed effects, predicted "free-space' signal strengths, and a "flexible" (post-period and post-by-South interactions) set of covariates referenced in the main text: percent black (1960), percent high school (1960), percent Protestant (1952), percent unemployed (1960), and percent urban (1960). The y-axis lists exclusions from this set of fixed effects and covariates. The baseline results are included for reference. I cluster standard errors at the state-level. The DiD (DDD) results are in light (dark) blue.

Table 2.A13: Double Selection Lasso: The Impact of McIntire's *TCRH* on Democratic Vote Share

	(1)	(2)
Post $\cdot Signal$	-0.208	0.475
	(0.364)	(0.493)
Post $\cdot Signal \cdot South$	-1.642**	-2.824***
	(0.770)	(0.850)
Free	X	X
Select. Method	plugin	bic
No. Covs.	362	362
No. Covs Selected	341	333
Mean	43.961	43.961
SD	15.226	15.226
Obs.	24,469	24,469

This table displays results from estimating equation (2.3) using double-selection lasso linear regressions. A plugin iterative formula and Bayesian information criterion (BIC) are implemented to choose from a set of potential covariates and fixed effects: this set consists on all covariates, and their regional interactions, included in table 2.1, as well as state-by-year fixed effects. For each case, all state-by-year fixed effects are selected. The plugin methods also controls for percent high school, log of total population (1960), percent male (1960), percent unemployed (1960), percent urban (1960), percent urban by South (1960), percent Protestant (1952), and percent Catholic (1952). County fixed effects are omitted from estimation. I cluster standard errors at the state-level.

2.8.6 Alternative Specification, Weighting, & Treatment Breakdown

Following Cascio and Washington (2013) and Bernini et al. (2023b), I re-estimate equation (2.3) restricting attention to the 1960 and 1980 presidential elections. The long-run triple DDD should be consistent with the average triple DDD from the baseline case. Estimates stored in the first column of table 2.A14 are similar to the baseline results: a one standard deviation increase in predicted signal strength generates a 2.13 p.p. drop in Democratic vote share in the average Southern county, and a 1.2 p.p. gain elsewhere.

The baseline results do not weight county observations by population. The remaining columns in table 2.A14 explore a number of weighting cases. First, I weight by total population in 1960. The regional effects go to zero, but this movement in the point estimate is driven by outlier counties. Both dropping counties in excess of 500,000 people, and weighting by the log of total population, restore significance.[39] Instead of weighting, controlling for the log of total population leaves the baseline results virtually unaffected.

One concern is that this paper is merely picking up regional trends in presidential voting instead of a legitimate media realignment effect. While I add state-by-year fixed effects to safeguard against this threat, there might still be reason for concern. Therefore, I re-estimate equation (2.3) for counties with predicted signal strengths below a certain threshold. If the baseline results truly reflect McIntire's influence, then the estimates should grow in magnitude as counties with higher signal strengths are added for analysis. Figure 2.A24 displays these results. Clearly, there is a downward trend in Democratic vote share for Southern counties as exposure to McIntire's *TCRH* increases.

[39] For log of total population, I round to the nearest integer since weights are restricted to the set of integers.

Table 2.A14: Alternative Specification and Weighting: The Impact of McIntire's *TCRH* on Democratic Vote Share

	(1)	(2)	(3)	(4)	(5)
	Two-Period	Weight (Pop.)	Weight (Pop. $\leq$ 500k)	Weight (log(Pop.))	Control log(Pop.)
Post $\cdot Signal$	1.192***	-0.064	0.757**	0.852***	0.847***
	(0.423)	(0.404)	(0.356)	(0.286)	(0.301)
Post $\cdot Signal \cdot South$	-2.129**	-0.656	-2.840**	-2.264**	-2.326**
	(0.836)	(1.456)	(1.395)	(1.056)	(1.043)
County FEs	X	X	X	X	X
State-Year FEs	X	X	X	X	X
Free	X	X	X	X	X
Covariates	X	X	X	X	X
Mean	44.009	46.541	44.213	44.064	43.949
SD	13.484	12.991	13.353	15.071	15.229
Obs.	6,138	1.41e+09	891963820	243,623	24,485

This table displays baseline results from estimating equation (2.3) using either two-periods (1960 and 1980) or a series of weights. Each regression internalizes county fixed effects, state-by-year fixed effects, predicted "free-space' signal strengths, and a "flexible" set (post-period and post-by-South interactions) of covariates referenced in the main text: percent black (1960), percent high school (1960), percent Protestant (1952), percent unemployed (1960), and percent urban (1960). The first column drops all presidential elections except the 1960 and 1980 contests; the second weights by total population; the third also weights by total population, but restricts the sample to counties with no more than 500,000 people; the fourth weights by the log of total population rounded to the nearest integer, and the fifth instead controls for this term. I cluster standard errors at the state-level.

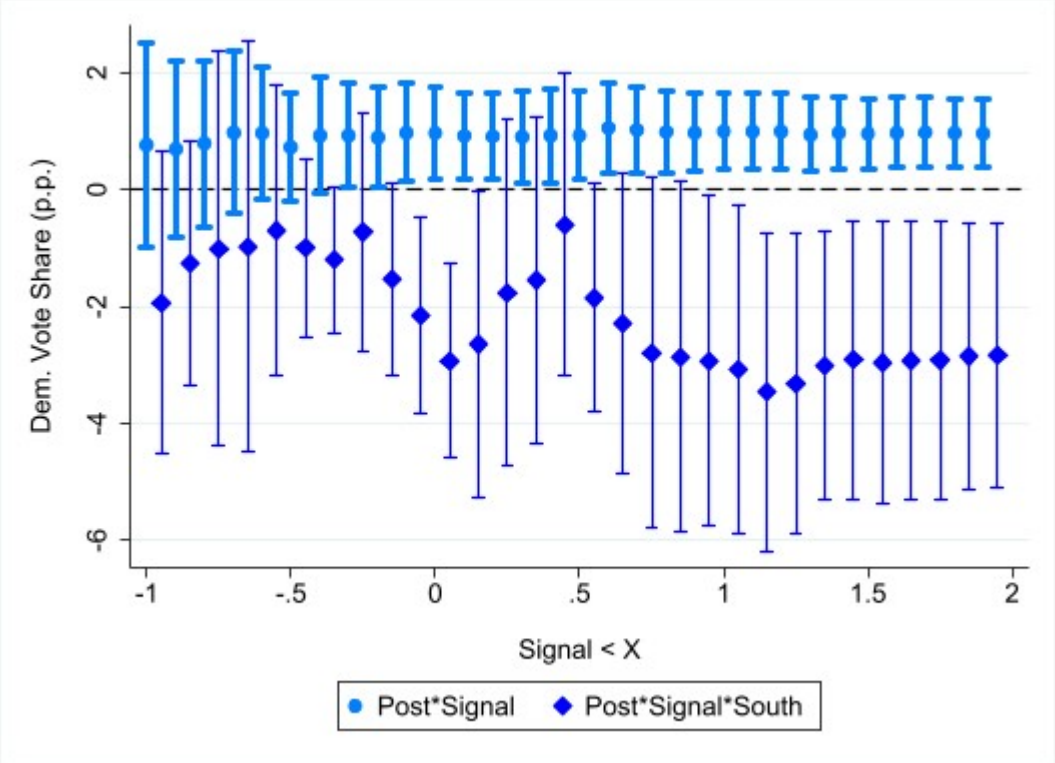


Figure 2.A24: Dropping Counties with Predicted Signal Strengths in Excess of Cutoff X

This figure displays baseline results from estimating equation (2.3) excluding counties with predicted signal strengths in excess of cutoff X. The x-axis measures different signal cutoffs. Each regression internalizes county fixed effects, state-by-year fixed effects, predicted "free-space" signal strengths, and a "flexible" (post-period and post-by-South interactions) set of covariates referenced in the main text: percent black (1960), percent high school (1960), percent Protestant (1952), percent unemployed (1960), and percent urban (1960). I cluster standard errors at the state-level. The DiD (DDD) results are in light (dark) blue.

2.8.7 Exploring Tennessee Counties in More Detail

The baseline results from omitting Tennessee amplify average partisan realignment by nearly a full percentage point: excluding these counties, a one standard deviation increase in predicted signal strength is associated with a 3.148 p.p. decline in Democratic vote share. This result is considerably larger in magnitude than the next closest average effect from omitting states one-by-one (Mississippi, 2.633 p.p.): see table 2.A4. Event study dynamics reveal that Tennessee accounts for four of the ten largest declines in Democratic vote share by election from excluding Southern states one-by-one: rank = 1, 1968 election, 3.696 p.p.; rank = 3, 1972 election, 2.907 p.p.; rank = 8, 1980 election, 2.524 p.p.; rank = 10, 1976 election, 2.380 p.p.: see figure 2.A17. Dropping Tennessee counties also adds precision to the estimates. This section explores why omitting Tennessee severely strengthens the baseline realignment effect.

Much of the analysis on terrain ruggedness from the main text is applicable to the amplified effect from dropping Tennessee. I reproduce the figures from this analysis solely for these counties: figure 2.A25 displays the ruggedness distribution, figure 2.A26 Democratic vote shares, and figure 2.A27 signal strengths. Clearly, the most rugged counties leaned towards the Republican Party prior to 1964 and realigned in subsequent elections. Since these counties actually experienced a level of exposure above the national average, Tennessee counties put downward pressure on the baseline results. Hence, omitting the state has an amplifying effect.

While the terrain ruggedness explanation fits the case of Tennessee, it is also descriptive of nearby North Carolina and Virginia. Why then does excluding counties in the Volunteer state have a comparatively larger magnifying effect? One reason might be the state's central counties. These counties tracked heavily Democratic prior to the Johnson-Goldwater contest, and exhibited the

largest realignment effect to the Republican Party. However, their signal exposure was relatively weak in 1964: even the easternmost counties in the heart of Appalachia had larger predicted signal strengths of McIntire's *TCRH*. As a result, these counties likely further dampen the baseline estimates: in other words, omitting these counties could generate even more upward pressure on McIntire's realignment effect. I test this hypothesis by re-estimating equation (2.3) excluding Tennessee counties one-by-one by predicted signal strength rank (lowest to highest). Figure 2.A28 displays these results. In line with expectations, the Southern differential approaches 3 p.p. in favor of the Republican Party as the 26 counties in Tennessee with exposure below the national average (in gray) are discarded.

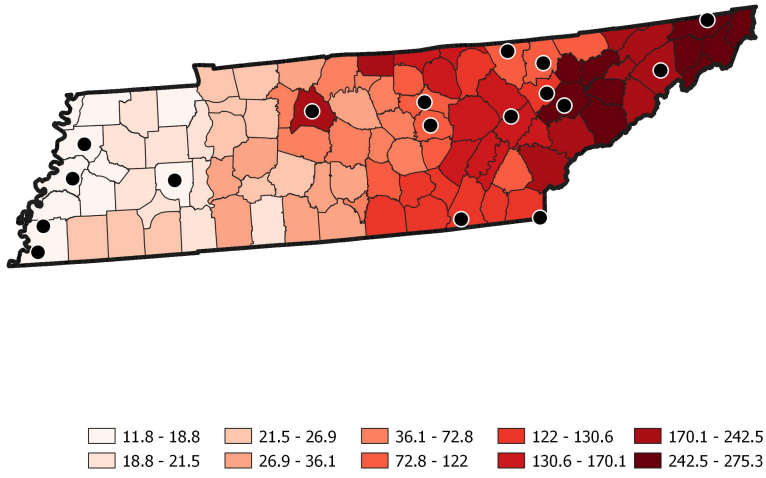
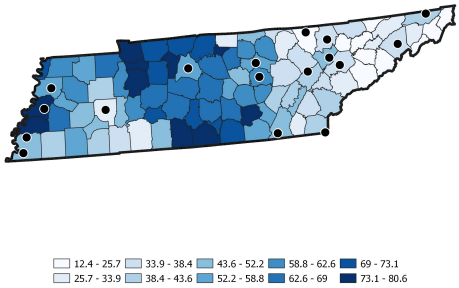
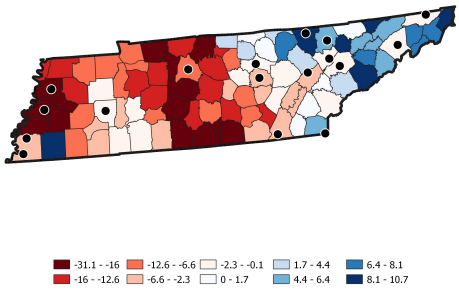


Figure 2.A25: Mean Ruggedness Distribution in Tennessee

This figure displays a heat map distribution of mean ruggedness for counties in Tennessee. Station transmitters airing McIntire's *TCRH* in 1964 are represented by black bullet points.



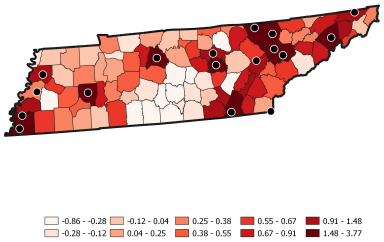
(a) [Pre-1964 Avg. Dem. Vote Share



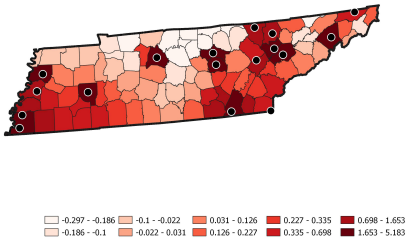
(b) Changes in Avg. Dem. Vote Share

Figure 2.A26: Visualizing Pre-1964 Average Democratic Vote Share and Changes in Average Democratic Vote Share Post 1964

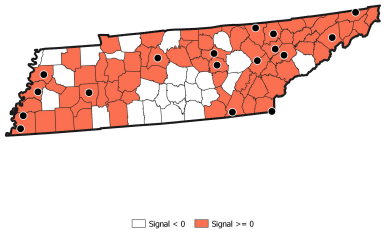
Panel (a) of this figure displays average Democratic vote shares over the 1952 to 1960 presidential elections. Darker shades of blue signal larger average Democratic slant. Panel (b) displays changes in average Democratic vote shares pre- and post-1964 for Tennessee counties. The pre-period average is over the 1952-1960 presidential elections, while the post-period is over 1964-1980. Counties in red (blue) experience average Republican (Democratic) net gains. Darker shades of red (blue) signal larger Republican (Democratic) partisan swings that correspond to the legend below. For each plot, station transmitters airing McIntire's *TCRH* in 1964 are represented by black bullet points.



(a) Predicted Signal Strength



(b) Predicted Free-Space Signal Space



(c) Predicted Signal Strength $\geq, <$ National Average

Figure 2.A27: Predicted (Free-Space) Signal Strength of McIntire's *TCRH* in Tennessee

This figure displays predicted signal strengths in panel (a) and predicted "free-space" signal strengths in panel (b) for counties in Tennessee. Both measures are computed from inputting radio transmitter data into Olken's modified ITM software. Predicted signal strengths are calculated from differencing the effective path loss due to relative distance and irregular topographical features from transmission power. The "free-space" measures are calculated in a similar fashion, except effective path loss is restricted solely to relative distance. Hence, the spatial variation between each type of signal strength is attributed to topography, which is the source of plausibly exogenous variation I exploit for causal inference. See Olken (2009) for additional information on this empirical design. Panel (c) highlights counties with predicted signal strengths at least as large as the national average in orange. For each plot, station transmitters airing McIntire's *TCRH* in 1964 are represented by black bullet points.

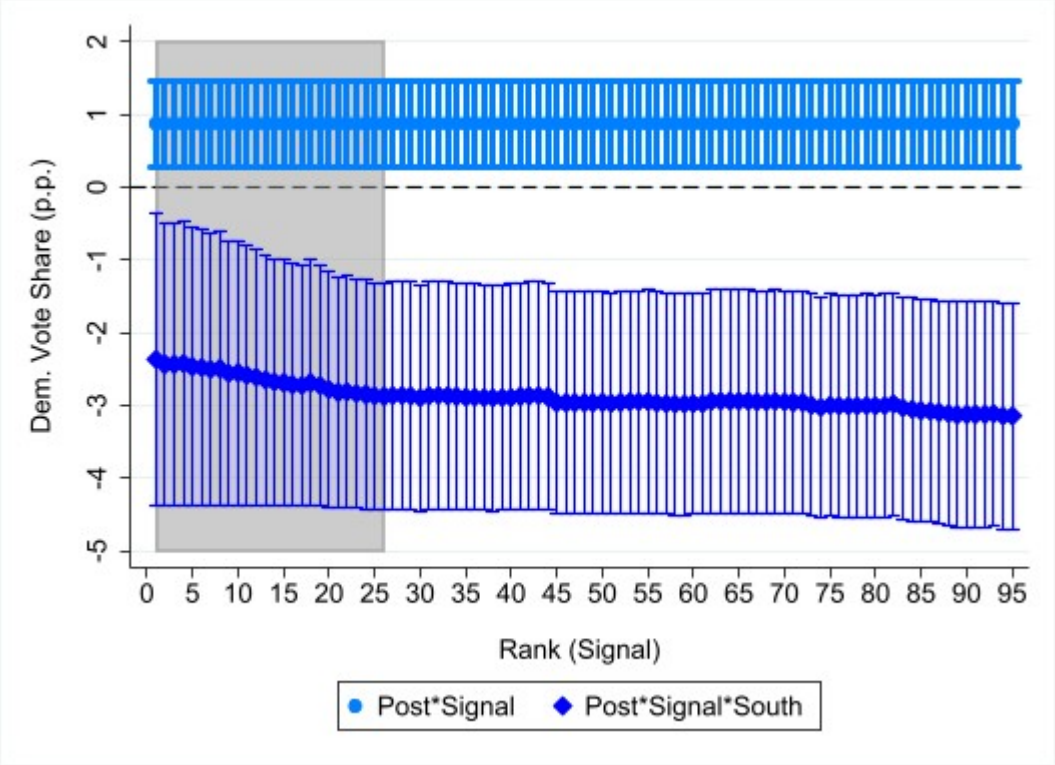


Figure 2.A28: Dropping Tennessee Counties One-by-One by Signal Strength Rank (lowest to highest)

This figure displays baseline results from estimating equation (2.3) excluding Tennessee counties by predicted signal strength rank one-by-one (lowest to highest). The x-axis measures ranks for all 95 counties. The gray bar gives the ranks with predicted signal strengths below the national average. Each regression internalizes county fixed effects, state-by-year fixed effects, predicted "free-space' signal strengths, and a "flexible" (post-period and post-by-South interactions) set of covariates referenced in the main text: percent black (1960), percent high school (1960), percent Protestant (1952), percent unemployed (1960), and percent urban (1960). I cluster standard errors at the state-level. The DiD (DDD) results are in light (dark) blue.

2.8.8 Restricting the Sample to Confederate Counties

The baseline results suggest McIntire's *TCRH* expanded partisan realignment in the South, and generated a backlash effect outside the former Confederacy. However, the statistical significance of the Southern regional differential might merely reflect the opposite-directional effect outside the South. I test for this possibility by re-estimating equation (2.2) over the former Confederacy. Table 2.A15 stores these results in the first two columns: the former (latter) excludes (includes) covariates. Estimates associated with the preferred specification show an insignificant 1.44 p.p. drop in Democratic vote share. While this is a discouraging result, it is also highly sensitive to the inclusion (or exclusion) of Tennessee. The next couple columns in the same table display results from dropping Tennessee counties. These estimates are larger in magnitude, a 2.28 p.p. decline, and significant at the 5% level.

The remaining columns highlight that this "Tennessee effect" is really confined within a few select counties. Omitting the 26 (of 95) counties in the state with exposure below the national average likewise yields a 2.00 p.p. drop at the 5% significance level. Figure 2.A29 re-estimates equation (2.2) excluding Tennessee counties one-by-one (lowest to highest) by predicted signal strength rank. Dropping the nine counties with the lowest exposure in the state, while retaining all other Southern counties, yields a Southern differential effect that is statistically significant at the 10% level. Discarding the lowest 15 counties generates 5% significance. Therefore, outside of a few influential Tennessee counties, McIntire's *TCRH* differentially expanded partisan realignment within the South. In other words, the preferred regional differential estimates in the main text are indeed reflective of media-generated Southern realignment, and not opposite-directional estimates in the Non-South.

Table 2.A15: The Impact of McIntire's *TCRH* on Democratic Vote Share: Only Confederate Sample

	All		Omit TN		Omit TN if Signal < 0	
	(1)	(2)	(3)	(4)	(5)	(6)
Post $\cdot Signal$	-1.378	-1.443	-2.117**	-2.277**	-1.920**	-2.003**
	(1.112)	(1.023)	(0.858)	(0.743)	(0.837)	(0.743)
County FEs	X	X	X	X	X	X
State-Year FEs	X	X	X	X	X	X
Free	X	X	X	X	X	X
Covariates		X		X		X
Mean	47.832	47.837	47.911	47.914	47.677	47.680
SD	18.480	18.468	18.484	18.466	18.450	18.436
Obs.	8,973	8,861	8,213	8,117	8,765	8,653

This table displays baseline results from estimating equation (2.2) exclusively over counties in the former Confederacy. The first two counties estimate over the entire region, the next two omit Tennessee counties, and the final two exclude Tennessee counties with predicted signal strengths below the national average (26 counties). Each regression internalizes county fixed effects, state-by-year fixed effects, and predicted "free-space' signal strengths. I re-estimate "flexibly" (post-period interactions) controlling for the set of covariates referenced in the main text: percent black (1960), percent high school (1960), percent Protestant (1952), percent unemployed (1960), and percent urban (1960). I cluster standard errors at the state-level.

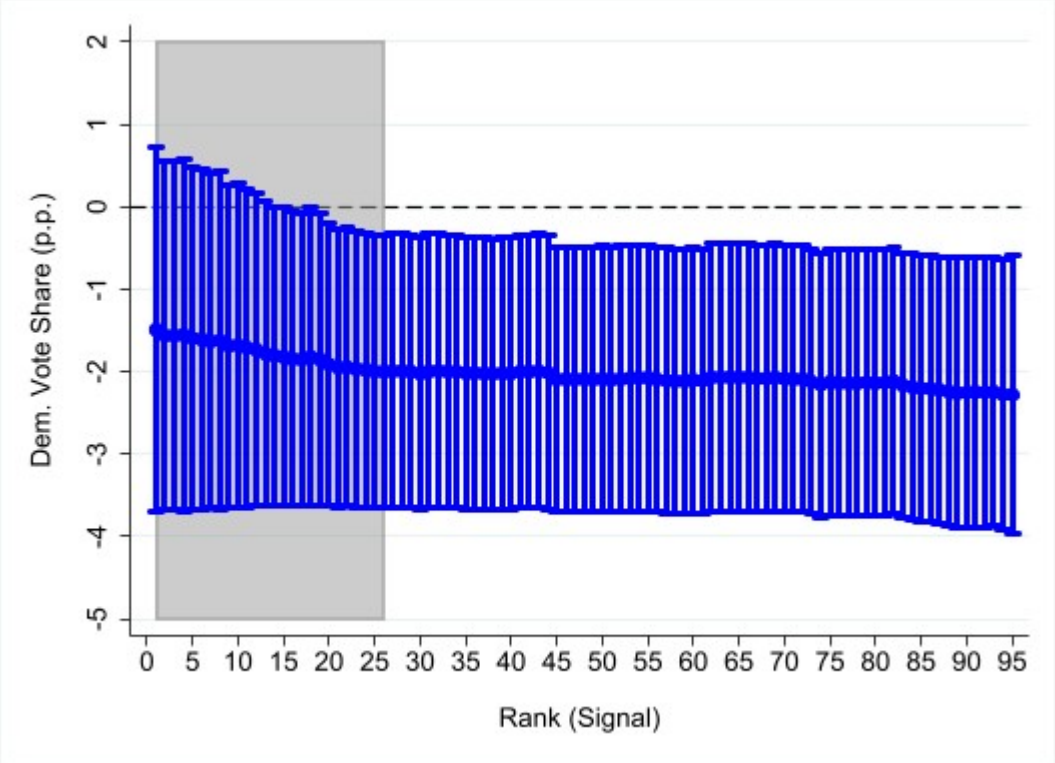


Figure 2.A29: Re-estimating the DiD Effect over the Only Confederate Sample Dropping Tennessee Counties One-by-One by Signal Strength Rank (lowest to highest)

This figure displays baseline results from estimating equation (2.2) over the Confederate only sample excluding Tennessee counties by predicted signal strength rank one-by-one (lowest to highest). The x-axis measures ranks for all 95 counties. The gray bar gives the ranks with predicted signal strengths below the national average. Each regression internalizes county fixed effects, state-by-year fixed effects, predicted "free-space' signal strengths, and a "flexible" (post-period) set of covariates referenced in the main text: percent black (1960), percent high school (1960), percent Protestant (1952), percent unemployed (1960), and percent urban (1960). I cluster standard errors at the state-level.

2.8.9 Is the Non-South Backlash Effect the Product of Regional Trends?

This paper finds that counties outside the South respond to conservative media by realigning with the Democratic Party post 1964 (0.87 p.p. effect). These counties, particularly in the Northeast and Midwest, experienced positive changes in average Democratic vote shares around the Johnson-Goldwater contest. Hence, one potential concern is that I am merely picking up these regional trends instead of a legitimate pro-segregation messaging effect over the airwaves. State-by-year fixed effects should aid in mitigating this concern, but the overall threat could persist beyond controlling for secular trends. In this section, I aim to add robustness to the existence of a Non-South backlash effect.

If the Non-South effect is actually tied to McIntire's *TCRH*, then it should not be driven by a particular region or state outside the former Confederacy. In other words, it should be descriptive of the collection of states as a whole. Since the Northeast strongly realigned with the Democrats post-1964, I establish that the results are not sensitive to this region. First, this paper restricts the Non-South sample only to Northeastern states: CT, ME, MA, NH, NJ, NY, PA, RI, VT.[40] Table 2.A16 displays these results in column (1). Both the DiD and DDD estimates are nearly identical to the baseline results stored in table 2.2. I also show in the subsequent column that dropping these counties does not generate meaningfully different results relative to the baseline case.

For even better comparisons, I only keep Non-South counties such that they belong to states that border the former Confederacy: KY, MD, MO, NM, OK, WV. The Non-South effect is reduced, but still 72.41% the size of the baseline estimate. The Southern differential term also remains above a 2 p.p. decline in Democratic vote share. Dropping these states yields results that

[40]Northeastern states are identified using the Census Bureau regional designation: see `https://www.census.gov/library/stories/state-by-state/northeast-region.html`.

are closer in line with the baseline case. See columns (3) and (4).

While regional trends appear unable to capture the Non-South backlash effect, I also rule out a single state driving the result. Figure 2.A30 displays estimates dropping each Non-South state one-by-one, where the omitted state is listed on the y-axis. Clearly, no one state is responsible for either effect.

Table 2.A16: The Impact of McIntire's *TCRH* on Democratic Vote Share: Vary Non-South Sample

	(1)	(2)	(3)	(4)
Post $\cdot Signal$	0.789	0.823**	0.629	0.763**
	(0.504)	(0.331)	(0.848)	(0.325)
Post $\cdot Signal \cdot South$	-2.231*	-2.266**	-2.071	-2.206**
	(1.122)	(1.043)	(1.316)	(1.042)
Non-South Sample	Only Northeast	Drop Northeast	Only Borders Confederacy	Drop Borders Confederacy
County FEs	X	X	X	X
State-Year FEs	X	X	X	X
Free	X	X	X	X
Covariates	X	X	X	X
Mean	46.882	44.098	47.381	43.594
SD	17.836	15.367	17.353	15.396
Obs.	10,597	22,749	12,229	21,117

This table displays baseline results from estimating equation (2.3) following different variations of the Non-South sample. Column (1) limits this sample to the Northeast (CT, ME, MA, NH, NJ, NY, PA, RI, VT); column (2) omits these states; column (3) restricts the Non-South to states that border the former Confederacy (KY, MD, MO, NM, OK, WV); and column (4) drops these states. Each regression internalizes county fixed effects, state-by-year fixed effects, predicted "free-space' signal strengths, and a "flexible" set (post-period and post-by-South interactions) of covariates referenced in the main text: percent black (1960), percent high school (1960), percent Protestant (1952), percent unemployed (1960), and percent urban (1960). I cluster standard errors at the state-level.

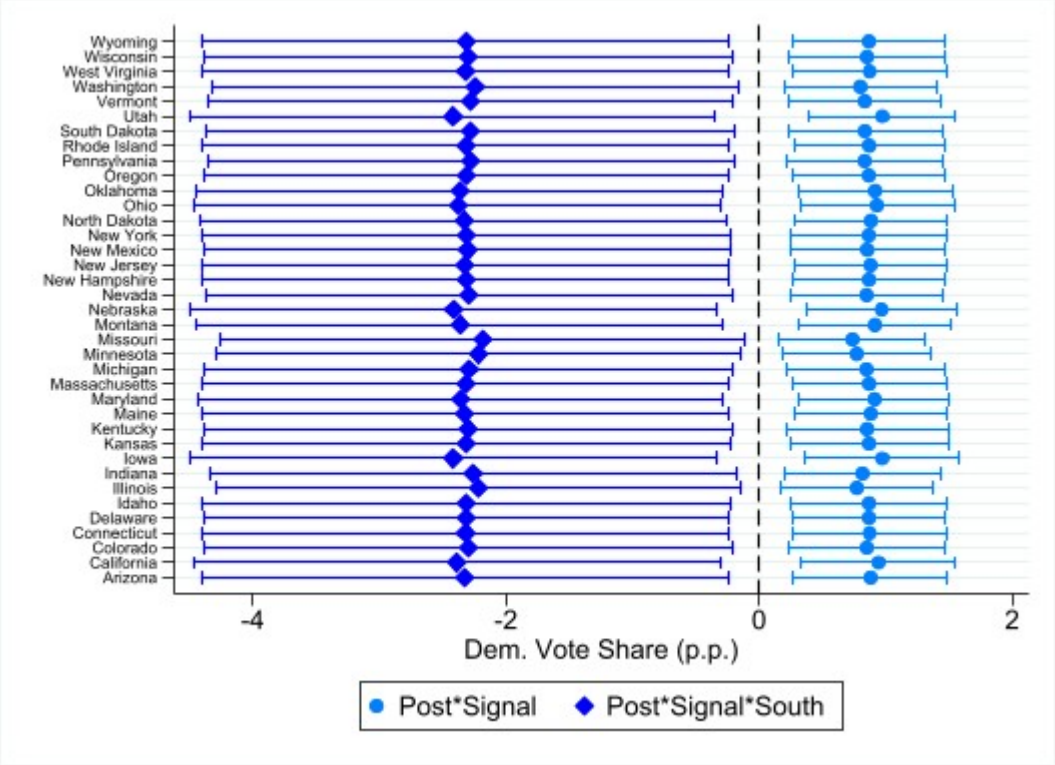


Figure 2.A30: Drop Non-South States One-by-One

This figure displays baseline results from estimating equation (2.3) omitting Non-South states one-by-one. The y-axis denotes excluded states, and the x-axis the estimated impact of a one standard deviation increase in predicted signal strength on Democratic vote share. Each regression internalizes county fixed effects, state-by-year fixed effects, predicted "free-space" signal strengths, and a "flexible" set (post-period and post-by-South interactions) of covariates referenced in the main text: percent black (1960), percent high school (1960), percent Protestant (1952), percent unemployed (1960), and percent urban (1960). The dynamic DiD (DDD) results are in light (dark) blue. Standard errors are clustered at the state level.

2.8.10 Additional Exercises Supporting Ethnic Competition Theory

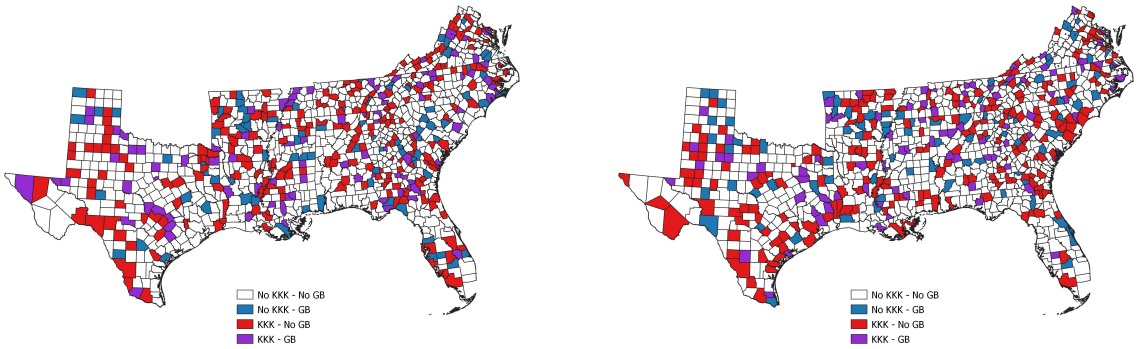
I cite ethnic competition theory as the basis for exploring decomposition effects of the Southern differential by interactions of pro- and anti-segregation organization at the county level. Ku Klux Klan chapters and Green Book establishments serve as the preferred proxies for these organizations. By generating these four decomposition cases (KKK-GB, KKK-No GB, No KKK-GB, and No KKK-No GB) of a triple (regional) interaction term, one might argue that I have "cherry-picked" an effect. This section shows that this is not the case.

First, I conduct a series of placebo tests to show that the decomposition estimates capture more than statistical noise. I randomly assign each of the four combinations across the South, while preserving their actual representation: 92 KKK-GB, 220 KKK-No GB, 85 No KKK-GB, and 710 No KKK-No GB. For expositional purposes, figure 2.A31 displays the first four of 1000 randomizations. In contrast to the actual distribution (figure 2.4), these distributions exhibit no clear spatial pattern.

This paper re-estimates each decomposition effect (equation (2.7)) for each randomization. Figure 2.A32 displays these results. Each plot corresponds to one of four decompositions. The estimates from each placebo test are sorted and displayed, in gray, by rank. I superimpose the actual decomposition effects, in dark blue, for comparative purposes. Results indicate that only 6.7% of these tests produce KKK-GB decomposition effects with larger declines in Democratic vote shares. None of the other decomposition effect stand out among the placebo results: the actual estimates do not sit within the bottom or top 10% of the placebo estimate distributions. These results serve as evidence in favor of ethnic competition theory as a leading mechanism.

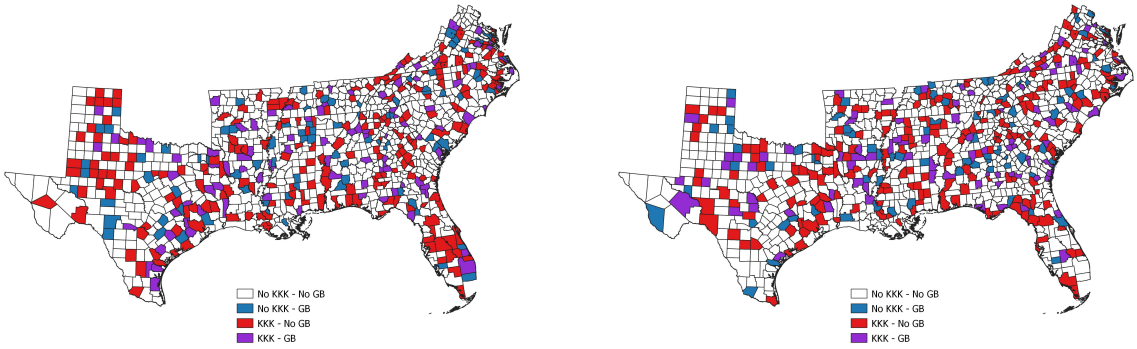
If local ideological competition on the race issue is responsible for conservative media's

realignment effect, then competition should also matter in vote share. I calculate each county's average pre-1964 (1952-1960) Democratic vote share in presidential elections. Then, I take the absolute value of this calculation subtracted by 50 for average margins, e.g. +1 Republican or +4 Democrat. I re-estimate equation (2.3) for counties at or below a given margin. Figure 2.A33 plot (a) displays these results conditional on each margin (50 through 1). For the most part, the Southern differential effect grows in magnitude, away from the Democratic Party, as Southern counties become more competitive. This downward trend rebounds around a margin of plus or minus 10, but apart from plus or minus 3, these estimates are still at or below those including the least competitive counties below the Mason-Dixon line. Since these results consider absolute margins, I further distinguish between close margins favoring the Democratic or Republican parties in plot (b). Much of the downward trend appears to be captured by close counties favoring Republican presidential nominees. For reference, I include plot (c) to illustrate trends by region in the number of counties meeting a given margin restriction.



(a) Randomization 1

(c) Randomization 3

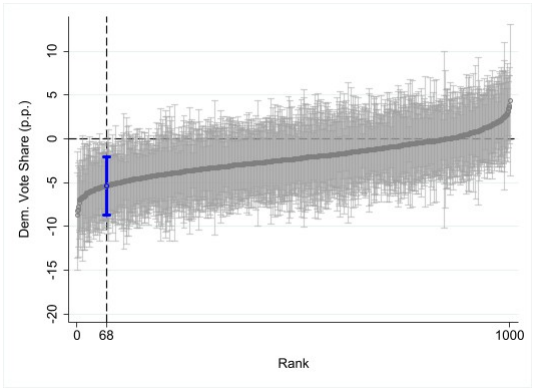


(b) Randomization 2

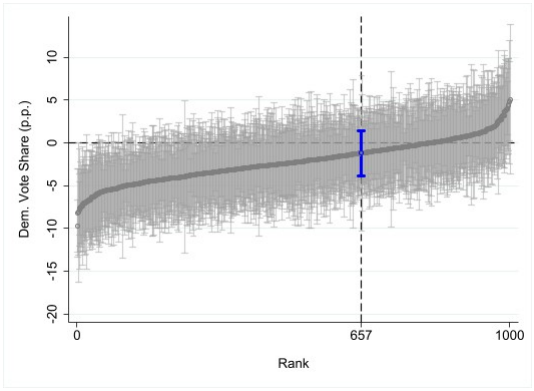
(d) Randomization 4

Figure 2.A31: First Four Random Klan-by-Green Book Assignments for Placebo Tests

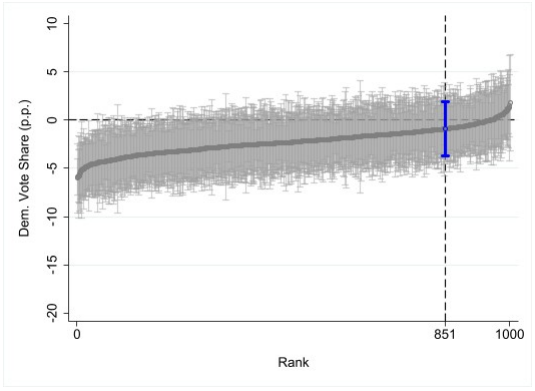
This figure displays the first four random assignments of Klan-by-Green Book designation. Counties assigned KKK-GB are in purple, KKK-No GB in red, No KKK-GB in blue, and No KKK-No GB in white. I generate 1000 replications of random assignment for regression analysis to compare to the actual results.



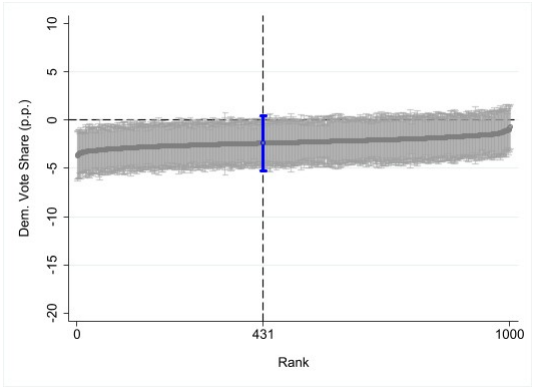
(a) KKK-GB



(c) No KKK-GB



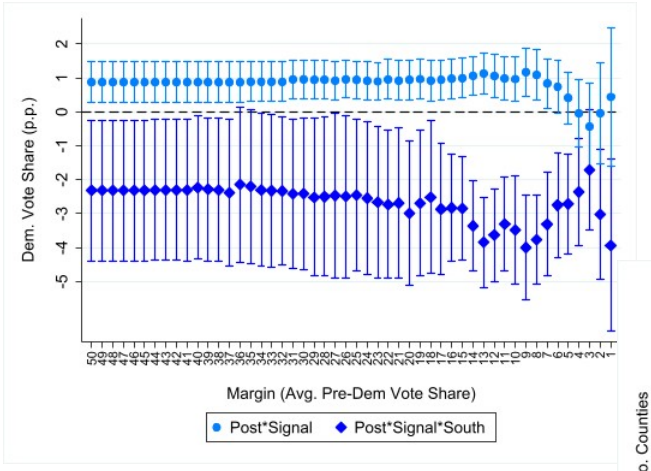
(b) KKK-No GB



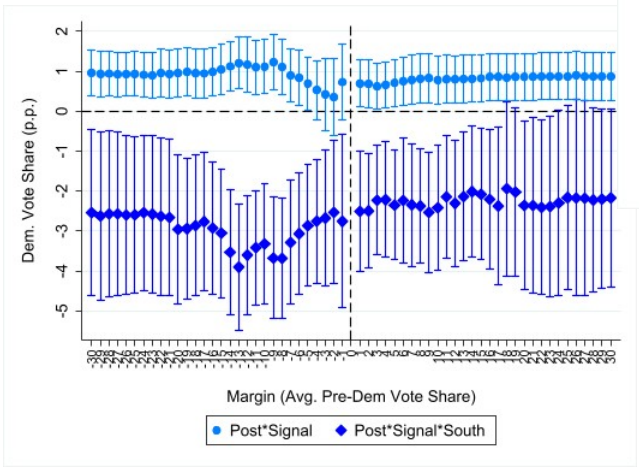
(d) No KKK-No GB

Figure 2.A32: Randomizing Klan-by-Green Book Assignments: Differential Placebo Effects by Local Organization on the Race Issue from 1000 Replications

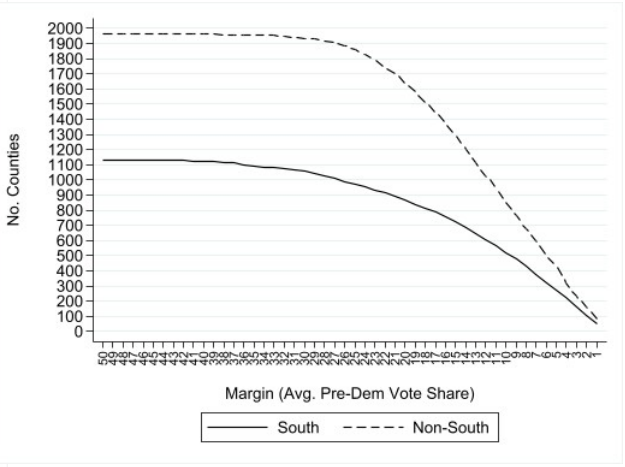
This figure displays results from 1000 randomizations of Klan-by-Green Book Assignments to Southern counties. For each randomization, I re-estimate equation (2.7) and store the results for each decomposition of the Southern differential: KKK-GB, KKK-No GB, No KKK-GB, No KKK-No GB. Within decompositions, I rank the estimates from lowest (1) to highest (1000) and display the results in the corresponding plot. The actual results are superimposed for comparative purposes in blue. For example, only 67 replications (6.7%) generate a larger effect in magnitude in counties with the Klan and at least one Green Book establishment relative to the actual estimate (-5.373).



(a) Regional Differential Effects by Absolute Margin



(b) Regional Differential Effects by Dem/Rep Margin



(c) No. Counties per Margin

Figure 2.A33: Restricting the Sample by Pre-1964 Average Democratic Vote Share

Panel (a) of this figure displays results from estimating equation (2.3) over subsets of counties with absolute margins in Democratic vote share below threshold "X". Panel (b) plots results distinguishing between Democratic/Republican margins, where negative values correspond to counties marginally leaning Republican. Each regression internalizes county fixed effects, state-by-year fixed effects, predicted "free-space' signal strengths, and a "flexible" set (post-period and post-by-South interactions) of covariates referenced in the main text: percent black (1960), percent high school (1960), percent Protestant (1952), percent unemployed (1960), and percent urban (1960). The dynamic DiD (DDD) results are in light (dark) blue. Standard errors are clustered at the state level. Panel (c) plots trends in the number of counties by region meeting certain absolute margin thresholds for reference.

2.8.11 Influence on Political Group Membership: John Birch Society & Americans for Democratic Action

While McIntire proved most effective in instigating partisan realignment in areas with pro- and anti-segregation organization, it remains to be shown that he influenced political group membership. This section uses 1965 survey data assembled by Frederick W. Grupp, Jr. on the John Birch Society (JBS) and Americans for Democratic Action (ADA) organizations to provide suggestive evidence in favor of a conservative media membership effect. McIntire used his radio platform to promote the JBS, and even had correspondence with the group's founder, Robert Welch (Ruotsila, 2015). It is no wonder that McIntire saw in the JBS an ally in the fight to preserve segregation due to their shared perception of the movement's communist orientation. In *Birchers*, Dallek (2023) writes as follows:

> Birchers' denunciation of Civil Rights as a Red plot synced with the most rabid foes of segregation in the Jim Crow South. If communists controlled the African American and white northern agitators who marched and went on "freedom rides" and "sat in" at lunch counters, then white opponents of Civil Rights could wave off the movement as alien and un-American, a hotbed of trickery and treason.

The data consists of survey responses from 650 JBS and 769 ADA members collected via mail questionnaires administered by the organizations. While respondents likely exceed average income and education measures, the ADA and JBS claim the sample to be geographically representative of total membership (Grupp, 1969). Critical for estimation, the data tracks the start date of membership for each organization. These dates range from 1957-1965, but due to low

JBS membership in the sample before 1960, i.e. 1 member in 1957, 5 in 1958, and 12 in 1959, I restrict attention to 1960-1965. Since the smallest geographic identifier in the data is member state, I collapse equations (2.2) and (2.3) to the state level using total population weights. State-by-year fixed effects are replaced by state fixed effects and year fixed effects, while South-by-year fixed effects are also included for the regional differential specification. My dependent variable of interest is a state's year-by-year share of JBS or ADA members from 1960-1965. This share only reflects new membership gains, and not losses from current membership counts.

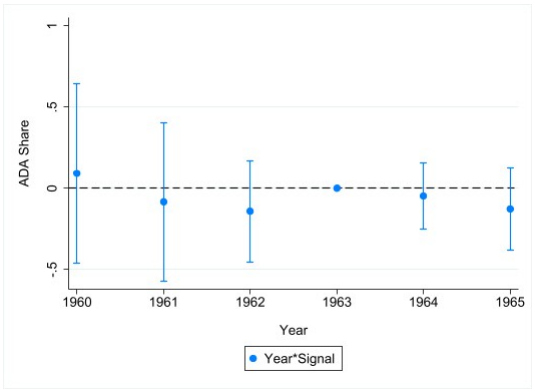
Table 2.A17 displays results from estimating variants of the overall and regional differential specifications. The first (latter) four columns provide results for the former (latter) specification. For each case, I estimate using JBS and ADA state shares with and without covariates. Consistent with Democratic vote share, the overall specification yields null results. In contrast, when permitting regional effects, McIntire appears to have shifted the distribution of the John Birch Society to the South. This paper finds that a one standard deviation increase in his predicted signal strength generates a 7.54 p.p. increase in a state's JBS membership share. McIntire might also have generated a comparatively smaller shift in ADA membership to the region. In this case, a one standard deviation increase is linked to a 0.77 p.p. gain in the average Southern state's share, albeit at the 10% significance level.

Figure 2.A34 displays the event study dynamics of these estimates, where 1963 is omitted for comparative purposes. One concern is the visible pre-trends in Southern states' JBS share prior to 1964. While this raises questions on internal validity, the estimates are at the least opposite in sign. This issue, along with collapsing to the state level, are why I consider these results as "suggestive".

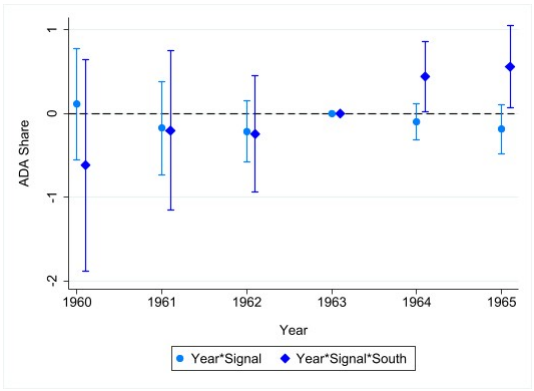
Table 2.A17: The Impact of McIntire's *TCRH* on John Birch Society and Americans for Democratic Action Membership

	Overall				Regional Differential			
	(1) JBS	(2) JBS	(3) ADA	(4) ADA	(5) JBS	(6) JBS	(7) ADA	(8) ADA
Post $\cdot Signal$	0.495	0.802	-0.153	-0.054	-0.108	0.092	-0.190	-0.074
	(0.672)	(0.826)	(0.131)	(0.154)	(0.222)	(0.380)	(0.149)	(0.172)
Post $\cdot Signal \cdot South$					9.308***	7.537***	0.368	0.767*
					(2.394)	(1.440)	(0.540)	(0.454)
South-Year FEs					X	X	X	X
State FEs	X	X	X	X	X	X	X	X
Year FEs	X	X	X	X	X	X	X	X
Free	X	X	X	X	X	X	X	X
Covariates		X		X		X		X
Mean	2.128	2.128	2.128	2.128	2.128	2.128	2.128	2.128
SD	3.939	3.939	4.219	4.219	3.939	3.939	4.219	4.219
Obs.	282	282	282	282	282	282	282	282

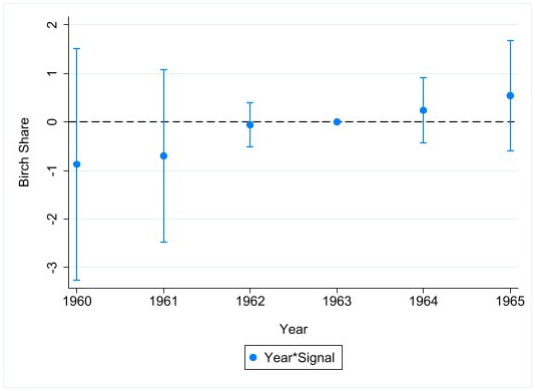
This table displays baseline results from estimating equations (2.2) and (2.3) across numerous specifications. The "Overall" supercolumn stores estimates from regressing equation (2.2). The first (second) pair of columns use shares of John Birch Society (Americans for Democratic Action) members as the dependent variable. Each regression internalizes county fixed effects, state fixed effects, year fixed effects, and predicted "free-space' signal strengths. I re-estimate "flexibly" (post-period interactions) controlling for the set of covariates referenced in the main text: percent black (1960), percent high school (1960), percent Protestant (1952), percent unemployed (1960), and percent urban (1960). The "Regional Differential" supercolumn stores estimates from regressing equation (2.3). These columns are similar to the previous set of columns. The triple difference-in-differences regressions also include post-by-South covariate interactions, South-by-year fixed effects, and triple "free-space" interactions ($Post_t \cdot Free_c \cdot South_c$). I cluster standard errors at the state-level.



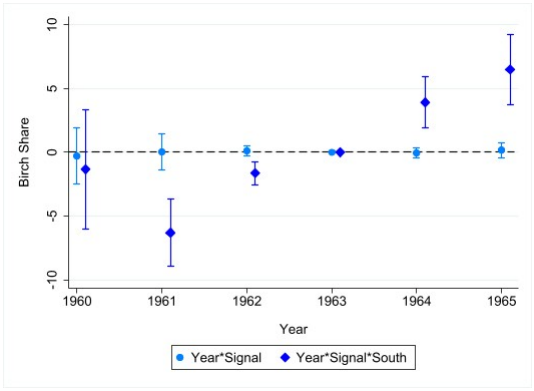
(a) Overall - ADA



(c) Regional Differential - ADA



(b) Overall - JBS



(d) Regional Differential - JBS

Figure 2.A34: McIntire's Influence on Americans for Democratic Action and John Birch Society Membership

This figure displays event study plots from estimating equations (2.2) (plots (a) and (b)) and (2.3) (plots (c) and (d)) with shares of John Birch Society (JBS) or Americans for Democratic Action (ADA) members as the dependent variable. Post-period interactions with signal strength measures and covariates are replaced with year-by-year interactions. Interactions with 1963 membership are omitted to avoid multicollinearity. The dynamic DiD (DDD) results are in light (dark) blue. Standard errors are clustered at the state level.

2.8.12 Data Appendix

Outcome Variables

Democratic Vote Share: The county-level share of votes cast for the Democratic presidential nominee measured for each election between 1952 to 1980, inclusive. The share is calculated using total votes, i.e. including those cast for third-party candidates. George Wallace's third-party bid for the presidency in 1968 is not included in Democratic vote share. Since Lyndon Johnson did not appear on Alabama ballots in 1964, Democratic vote shares are missing for these county-by-election years. Data for Democratic vote share construction is gathered from CQ Press Voting and Elections Collection.

Turnout per Capita: The county-level total number of votes cast divided by the voting age population (VAP) recorded by the closest Decennial Census. Raw total vote counts are gathered directly from CQ Press Voting and Elections Collection. VAPs are constructed using the 1950 census, 1960 census, 1970 census (Haines, 2010), and 1980 census (NHGIS). Turnout per capita in 1952 uses VAP (21+) from 1950 census; 1956 VAP (21+) from 1960 census; 1960 VAP (21+) from 1960 census; 1964 VAP (21+) from 1960 census; 1968 VAP (21+) from 1970 census; 1972 VAP (21+) from 1970 census; 1976 VAP (18+) from 1980 census; and 1980 VAP (18+) from 1980 census. Note that the VAP was lowered from 21 to 18 in 1971, but since I use the 1970 census for the 1968 and 1972 VAPs, I elect to use 21+ throughout for consistency.

John Birch Society (JBS) & Americans for Democratic Action (ADA) Membership Shares: The state-level share of JBS/ADA members year-by-year from 1960 to 1965. Each organization collected data via mail questionnaires in 1965. The JBS/ADA claim the sample to be geographically

representative of their overall membership distribution (Grupp, 1969). The data records a respondent's first year of membership. I use this information to identify JBS/ADA membership growth, which does not take into account group exit due to data limitations. West Virginia is dropped since the state does not account for any sample membership. I accessed the data through the Inter-university Consortium for Political and Social Research (ICPSR 7346).[41]

Signal Strength Measures & ITM Input Variables

Station Transmitter Information in the 1960s: Transmitter frequency and geocoordinates digitized mostly from *Radio Annual Television Yearbook (1963)* (Corp., 1963), but also from *Radio Annual Television Yearbook (1959)* (Corp., 1959), various editions of *Broadcasting: The Businessweekly of Television and Radio* (Broadcasting, 1962, 1965, 1967, 1969), and *Broadcasting: The Businessweekly of Broadcasting and Allied Arts* (Broadcasting, 1973).

Stations Airing McIntire's *TCRH*: This data source identifies stations carrying McIntire's radio program, the callsigns of the stations, start date of coverage, end date of coverage, and corresponding station transmitter power. I merge the station data with the transmitter information scraped from yearbooks using state name, city name, and station callsign. A trivial number of stations without frequency values or zero wattage are discarded: 539 stations remain. I use the start and end dates to determine coverage to McIntire in 1964. One issue is missing values in end dates (226/539 missing). This paper uses the known distribution of end dates to impute these missing values. Stations with actual start dates and actual/imputed end dates including 1964 are considered. Note that most stations with identifiable end dates aired the program in 1964 (all except 16 - 5.11%). Alternatively, I consider "full survivorship" of these stations as a robustness

[41] See the following link for data access: `https://www.icpsr.umich.edu/web/ICPSR/studies/7346/datadocumentation#`.

exercises. This data is collected from Bazzi et al. (2023).

County Centroids: The county-level geocoordinates corresponding to each geographic midpoint. I calculate these measurements using GIS software. They are used as "receiver geocoordinates" for each county's signal strength measures.

Predicted Signal Strength: The county-level standardized predicted signal strength of McIntire's *TCRH* calculated using Olken's ITM software. Input variables are listed above. The program internalizes these values, as well as a "globe" pre-load embedded in the software, to determine the effective path loss corresponding to each station transmitter-receiver pair. For this type of signal strength, path loss is generated by theoretically exogeneous diffraction caused by irregular topography (e.g. mountains, hills, etc.) and an endogeneous distance effect. Path loss is subtracted from power to identify signal strengths for all pairs. For each county, I maximize over these signal strengths. The maximum is the predicted signal strengths. Importantly, I restrict this process to stations airing McIntire's *TCRH* in 1964. See Olken (2009) for a more detailed explanation.

Predicted "Free-Space" Signal Strength: The description above follows here with one exception: in this case, effective path loss is limited only to the endogenous distance effect. In other words, the "free-space" measure yields the signal strength of a county in the absence of any topographical irregularities, i.e. in a counterfactual "flat earth".

Federal Communications Commission (FCC) Station Transmitter Information in 2020: Transmitter frequency and power, city, state, and type (AM/FM) collected from the FCC.[42]

Stations Airing *The Rush Limbaugh Show*: This data source identifies stations (callsigns) carrying *The Rush Limbaugh Show* as of 2020, city name, state name, and station transmitter

[42]See the following link for access to AM radio station transmitter data: `https://www.fcc.gov/media/radio/am-query`.

geocoordinates. I merge the station data with the transmitter information sourced from the FCC using station callsigns, type (AM/FM), and state name. About 68% of the stations are macthed. For the remaining non-matched stations, I fill in missing power and frequency values piece-by-piece. I draw from a number of sources for this information, e.g. radio-locator (`https://radio-locator.com/`), radiostationusa (`https://radiostationusa.fm/`), and Wikepedia. There remain 726 stations with non-missing input information for ITM software. I repeat the procedure outlined above for both types of signal strength measures for this program. This data is also collected from Bazzi et al. (2023).

County Centroids

Log Total Pop.: The county-level log of total population in 1960. Constructed from taking the log of the variable defined "totpop" in the 1960 Census (Haines, 2010).

Pct. Male: The county-level percent of total population that is male. Constructed from the fraction of variables defined "mtot" and "totpop" in the 1960 Census (Haines, 2010).

Pct. Age $\geq$ 65: The county-level percent of total population aged 65 years or older. This age demographic is calculated by summing over counts of men and women falling withing certain age brackets: in the 1960 Census, these variables are "m6569", "f6569", "m7074", "f7074", "m7579", "f7579", "m8084", "f8084", "m850", "f850". This summation is divided by the variable "totpop" in the 1960 Census (Haines, 2010).

Pct. Black: The county-level percent of total population that is black. Constructed by dividing the sum of variables defined "negmtot" (black men) and "negftot" (black women) over "totpop" (total population) in the 1960 Census (Haines, 2010).

Pct. Urban: The county-level percent of total population residing in urban communities in

1960. Corresponds directly to the variable defined "CC00040" in the County and City Data Book [United States] Consolidated File: County Data, 1947-1977 (ICPSR 7736).

Pct. Married no HH: The county-level percent of married couples who do not own a house. Corresponds directly to the variable defined "pctnohh" in the 1960 census (Haines, 2010).

Pct. High School ($\geq$ 25): The county-level percent of total population aged at least 25 with a high school degree in 1960. Corresponds directly to the variable defined "CC00127" in the County and City Data Book [United States] Consolidated File: County Data, 1947-1977 (ICPSR 7736).

Pct. Unemployed: The county-level percent of the civilian labor force that is unemployed in 1960. Corresponds directly to the variable defined "CC00166" in the County and City Data Book [United States] Consolidated File: County Data, 1947-1977 (ICPSR 7736).

Log. Med. Fam. Inc.: The county-level log of median family income in 1960. Constructed from taking the log of the variable defined "CC00243" in the County and City Data Book [United States] Consolidated File: County Data, 1947-1977 (ICPSR 7736). Note that 15 counties had values of "0". These cases were replaced with \$10 to take log transformations.

Pct. Radio: The county-level percent of occupied units with a radio in 1950. Corresponds directly to the variable defined "CC00317" in the County and City Data Book [United States] Consolidated File: County Data, 1947-1977 (ICPSR 7736).

Pct. Church Mem.: The county-level percent of total population who are church members in 1952. Corresponds directly to the variable defined "pctchmem" in the 1952 Survey of Churches and Church Membership (ICPSR 2896) (Haines, 2010). Note that 59 counties have percents in excess of 100. These cases were replaced with 100.

Pct. Protestant: The county-level percent of church members who are Protestant in 1952.

Corresponds directly to the variable defined "pctprot" in the 1952 Survey of Churches and Church Membership (ICPSR 2896) (Haines, 2010). Note that 43 counties have percents in excess of 100. These cases were replaced with 100.

Pct. Catholic: The county-level percent of church members who are Roman Catholic in 1952. Corresponds directly to the variable defined "pctrcath" in the 1952 Survey of Churches and Church Membership (ICPSR 2896) (Haines, 2010). Note that 44 counties have percents in excess of 100. These cases were replaced with 100.

Mean Ruggedness: The county-level average variation in elevation in 1930. Collected from Wang's replication files (Wang, 2021a). Wang (2021a) constructed these measures using the National Elevation Database (US Geological Survey).

Miscellaneous

Ku Klux Klan (KKK): A county-level binary variable equal to 1 for counties with a third-wave (1960s) KKK klavern (or chapter). Collected from Ang's replication files (Ang, 2023). Ang (2023) explains that the data is derived from House Un-American Activities Committee reports.

Green Book Establishments (GB): A county-level binary variable equal to 1 for counties with at least one green book establishment, or non-discriminatory public amenity, in 1955. Collected from Cook et al.'s replication files (Cook et al., 2022). The year 1955 was selected since it is the latest year in their sample, or the closest to this paper's period of interest. Cook et al. (2022) gather their data from digitizing Green Books from New York Public Library's Digital Collections.

Voter Education Projects (VEP): A county-level binary variable equal to 1 for counties with a black mobilization project funded by the VEP from May 1962 through 1964. Faulkenbury

(2016) lists these projects in tables. I digitize this information, and map projects (if necessary) to the county level. State-wide and region-wide (multi-county) mobilization projects are dropped. Projects with missing information are also dropped.

Right-Wing Media Organizations by Income: Annual income from 1955-1966 for each of the top ten right-wing media organizations by 1964 income. From highest to lowest income ($000) in 1964, these organizations are as follows: John Birch Society ($3200), Twentieth Century Reformation Hour ($3000), Human Events ($1141), Life Line ($1045.3), Freedoms Foundation at Valley Forge ($996.5), National Review ($988), Christian Crusade ($834.2), American Economic Foundation ($740.9), Foundation for Economic Education ($623.2), and Christian Anti-Communism Crusade ($616.5). This data was gathered from Group Research Inc.'s September 1964 report, *The Finances of the Right Wing*. Note that there are two tables with different incomes for 1963: Table 3 covers 1963 income and Table 5 1963-1965 income. I use the latter table for 1963 measures.

Audience Sizes of Popular Media Programs: Audience sizes for the five largest conservative talk radio shows are sourced from Wikepedia (`https://en.wikipedia.org/wiki/List_of_most-listened-to_radio_programs`). Viewership data on Sunday morning political talk shows is from Pew Research (`https://www.pewresearch.org/journalism/fact-sheet/network-news/`). Ruotsila (2015) and Wang (2021a) cite McIntire's *TCRH* and Father Coughlin's audience sizes.

Gallup Poll (9/1964): Counts of survey responses "Agree", "Disagree', and "Don't Know" divided by the total number of respondents. Calculations are made for each region (South and Non-South). The Gallup Poll question of interest is as follows: "And now, one more statement about the Communists: Most of the organizations pushing for Civil Rights have been infiltrated

by the Communists and are now dominated by Communist trouble-makers. Do you agree with the statement or not?". Survey data is sourced from Q41 of Roper #31092366.

Qualitative Sources

TCRH Audio Recordings: Audio files of McIntire's broadcast of *TCRH*. I convert these files into transcripts to justify Ruotsila's claim that the University of Mississippi riot sparked McIntire's interest in the Civil Rights movement. Excerpts of transcripts immediately following the incident, i.e. October 1-4, 1962, are stored in an earlier appendix. These files were shared by the Princeton Theological Seminary.

McIntire's FBI Files: A collection of documents, e.g. letters, newpaper clippings, etc., on McIntire assembled by the FBI. The March 21, 1965 letter referenced in the main text is from this source. Excertps from this source demonstrating McIntire's influence on the Ku Klux Klan are stored in an earlier appendix. I thank Bobby Griffith.

Christian Beacon Cartoons: Political cartoons printed in Mcintire's newspaper pertaining to the Civil Rights movement. The cartoon connecting the Civil Rights Bill of 1964 to a relinquishment of private property, as well as the suggestive cartoon implicating Martin Luther King, Jr. as an agitator of violence are from Griffith Jr (2009).

Chapter 3: Voter Mobilization or Electoral Oversight?: Exploring Determinants of Black Re-Enfranchisement

"We were very pleased with the manner in which VEP operated in the past years, and we found this quite helpful to our total movement."

— Martin Luther King Jr. to Vernon Jordan (December 28, 1965)

3.1 Introduction

Senator Edward Kennedy ranked the Voting Rights Act of 1965 (VRA) among the greatest achievements of any Congressional body in United States history (Wright, 2013). The Act is often cited as the principal catalyst of black "re-enfranchisement" following some 75 years of effective disenfranchisement in Southern states with restrictive test devices (Valelly, 2004). Most notably, Section 4 of the VRA outlawed literacy tests in low-turnout counties, and permitted the assignment of federal examiners to oversee black registration, while Section 5 mandated these counties seek federal "preclearance" prior to instituting changes in electoral procedure (Wright, 2013). Collectively, these measures are credited with increasing black registration by 19% (64%) in the Peripheral (Deep) South from 1964 to 1968 (Valelly, 2004).

Despite the VRA's influence, pre-trends in state-level black registration rates suggest that

gains in black participation might be (partially) attributed to a different source. Figure 3.1 plots state-level trends in black registration from 1940 to 2006. From 1940 to 1962, there are only modest increases in registration across states. These trends likely reflect the Supreme Court's *Smith v. Allwright (1944)* verdict banning the all-white primary (Watters and Cleghorn, 1967). The abrupt increase in 1962 registration, three years prior to the VRA, has come to define black political participation across many Southern states for the remainder of the twentieth century. In fact, each state experienced at least a 15% increase in black registration from 1962 to 1964, and six of the 11 states realized more than a 50% hike. Compared to 1964 to 1966, all but Alabama, Louisiana, and Mississippi experienced larger percent increases in registration in the immediate period preceding the VRA. These raw trends beg the following question: *Besides the VRA, what is responsible for reigniting black political participation?*

Cascio and Washington (2013), who find similar trends from regressing log turnout on percent black at the county level, suggest that the pre-VRA gains in black participation could be tied to a series of mobilization campaigns conducted by the Voter Education Project (VEP). The VEP was created in 1961 as a sub-unit of the Southern Regional Council with the aim to bolster the black vote by soliciting wealthy philanthropists to fund local chapters of civil rights organizations, e.g. NAACP, SCLC, SNCC, CORE, and NUL. Attorney General Robert Kennedy, along with the Department of Justice, promoted the founding of the VEP as a means to encourage political participation over direct action. The civil rights organizations, incentivized by access to funds, obliged by setting aside their differences and uniting under the VEP-umbrella. From March 1962 through 1964, the VEP oversaw 129 mobilization projects totaling roughly $850,000 ($8.78M in 2025 dollars) in expenditures (Faulkenbury, 2019). To my knowledge, no existing work has tested Cascio and Washington's claim tying the VEP to positive shocks in black

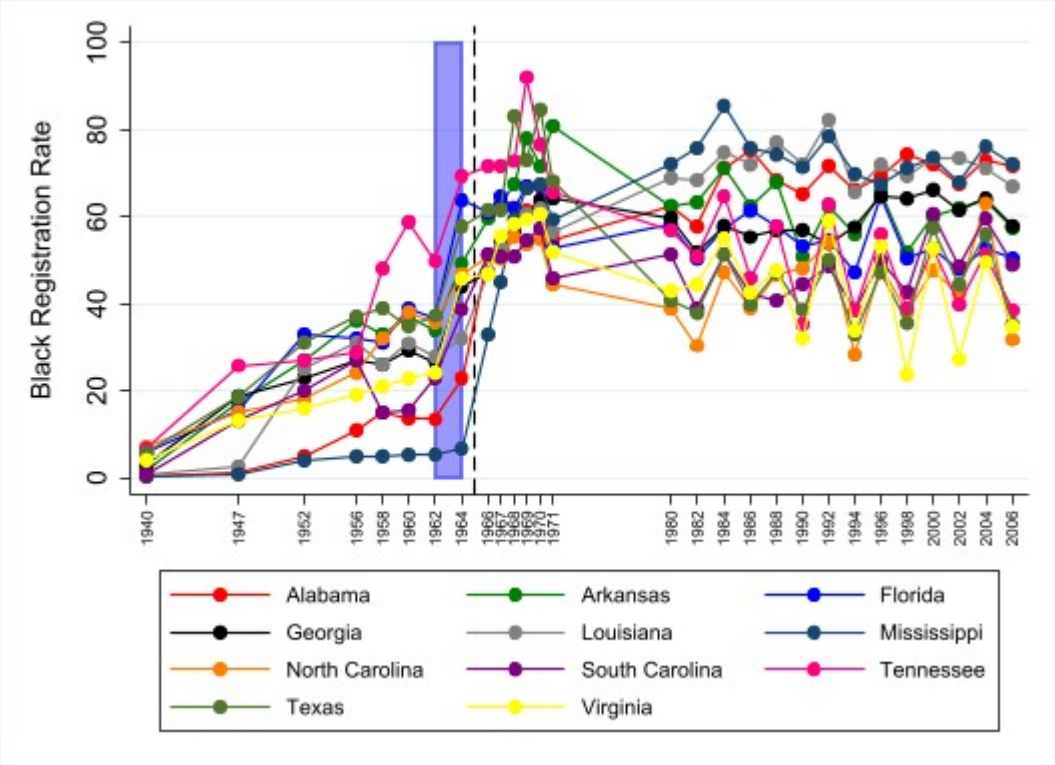


Figure 3.1: State-level Trends in Black Voter Registration

This figure displays statewide trends in black registration rates spanning 1940 to 2006. Data is obtained from scraping Garrow (1978) (1940-1971) and Bullock and Gaddie (2014) (1980-2006). This figure is a near-replication of Figure 6.1 in Wright (2013) with exception to excluding registration rates from 1976-1980. The shaded region covers the Voter Education Projects first mobilization campaign across the South. The dashed line corresponds to the timing of the passage of the Voting Rights Act of 1965.

registration.[1] The current paper aims to fill this gap in the literature.

In this paper, I employ a triple difference-in-differences design to determine the impact of the first-wave (1962-1964) of the VEP, conditional on VRA "coverage" status, on black political participation. VEP projects are identified using Faulkenbury (2016), and subsequently digitized and mapped to the county level. One principal challenge to estimation is that registration data is scant, while data on turnout is available but not by race. In the spirit of Cascio and Washington (2013) and Bernini et al. (2023b), I overcome this issue by using log turnout as the dependent variable, and identifying the treatment effect on the slope of percent black. The design's underlying assumption is that counties with larger black shares are more affected by mobilization drives and electoral oversight targeting this demographic. The main specification will estimate how a percentage point increase in percent black impacts VEP, VRA, or VEP-by-VRA counties' presidential turnout relative to the same percentage increase in counties without the VEP and VRA.

Baseline results reveal that the Voter Education Project was indeed successful in increasing black political participation. A one percentage point increase in percent black in VEP counties is associated with a 0.55% average rise in presidential turnout over the 1964 to 1980 elections. This mobilization effect is virtually identical to the 0.57% boost among counties with the VRA but without a VEP project. The joint VEP and VRA effect of 0.74% shows that, conditional on mobilization (electoral oversight), electoral oversight (mobilization) yields diminishing returns. In other words, mobilization was all the more important in counties without VRA coverage. Event study dynamics suggest that the VEP was responsible for pre-VRA gains in black political

[1] Valelly (2004) writes: "On the ground, the registration across the South of voting-age African Americans jumped 48 percent from 1962 to 1964, from approximately 29 percent of voting-age black southerners to approximately 43 percent. Strictly speaking, the causal role of the VEP in the *regional* increase is still uncertain and has not been econometrically modeled."

participation: a percentage point increase in percent black generates a 0.33% increase (10% significant) in turnout for the 1964 presidential contest.

Since the Voter Education Project targeted counties with strong white resistance, as well as urban counties with politically detached black populations, the baseline results could suffer from selection bias (Watters and Cleghorn, 1967). These concerns are evident in a simple comparison of summary statistics by treatment status. While VEP and non-VEP counties are roughly similar across economic conditions (log population (1960), percent unskilled labor (1960), percent unemployed (1960), percent family poverty (1960), and cotton suitability), the average VEP county is 15.30 percentage points more urban, and the backdrop to 4.66 more pro-Black protests ranging from 1960 to 1964. As a result, I control for differential treatment effects across these measures, among other covariates, to mitigate potential selection issues that could contaminate the results. For robustness, I also limit the control set to counties with their first VEP mobilization project between 1966 to 1970. Hence, I compare VEP, VRA, and VEP-by-VRA counties to future VEP counties. If anything, the estimates from this robustness exercise show that the baseline results might underestimate the true effect of mobilization: VEP counties experience a 0.90% increase, VRA counties a 0.73% increase, and VEP-by-VRA counties a 1.07% increase. The observed mobilization effects also persist across the following robustness checks: two-period, long-form difference-in-differences specification; robust and wild cluster bootstrapped standard errors; omitting future VEP counties; and omitting neighboring VEP counties.

I explore a number of potential mechanisms that could be responsible for the VEP's positive impact on turnout. First, I consider mobilization campaigns at the intensive margin by separating treated counties by the number of VEP projects launched from March 1962 through 1964. Conditional on treatment assignment, 71.26% of counties had a single project, 21.84% two projects, and the

remaining 6.90% three or more projects. Estimating over counties by number of mobilization projects reveals that the intensive margin is not the driver of the baseline results. Second, this paper explores if the presence of a "big five" organization (NAACP, SCLC, SNCC, CORE, or NUL) matters relative to a purely local grassroots organization, e.g. the Tuskegee Civic Association. More than four out of every five treated counties had at least one mobilization project coordinated by a "big five" organization. Regression results highlight that both organization types were effective in generating black political participation, but "big five" organizations were especially successful (0.35% versus 0.66% effects). Finally, I test if white resistance to black registration, proxied by Ku Klux Klan chapters at the county level, dampened the returns from mobilization projects: 57.47% of treated counties had Klan representation. Estimates suggest that white resistance served as a prohibitive barrier to black registration. Without the Klan, mobilization projects yielded a 0.72% bump in turnout for each percentage point increase in percent black. Among these three mechanisms, event study dynamics reveal that organizational type is most explanatory of pre-VRA gains in turnout.

To my knowledge, this is the first paper tying mobilization campaigns to gains in black registration preceding, as well as following, the passage of the Voting Rights Act of 1965. By showing that VEP projects further amplified turnout in VRA coverage counties, I add to the literature on black re-enfranchisement (Ang, 2019; Cascio and Washington, 2013; Fresh, 2018). Closely related papers link positive shocks in black political participation to greater representation, voting on civil rights legislation, and electoral rules (Bernini et al., 2023b; Schuit and Rogowski, 2017; Trebbi et al., 2008).[2] This paper also adds to a literature on strict voter identification laws that attributes null results to counterbalancing mobilization efforts by the

[2]Bernini et al. (2024) provide an excellent literature review on the Voting Rights Act of 1965.

Democratic Party (Cantoni and Pons, 2021; Valentino and Neuner, 2017): I lend support to this supposition by finding that mobilization yields positive turnout returns even in the absence of preventative measures, i.e. electoral oversight. Finally, I contribute to a robust literature on mobilization campaigns (Gerber and Green, 2017).

This paper will proceed as follows. Section 3.2 offers historical background on black enfranchisement and the Voter Education Project. Section 3.3 describes the data, outlines the empirical design, examines the raw data, and introduces the main specification. Section 3.4 displays the baseline estimates and establishes robustness. Section 3.5 test for heterogeneity on the intensive margin, organizational type, and white resistance to black mobilization. Finally, section 3.6 concludes.

3.2 Historical Background

The antebellum era saw limited black political participation: a mere three states (ME, TN, VT) recognized black suffrage at the timing of the Constitution's ratification, and only five states (ME, MA, NH, RI, VT) as civil war neared its conclusion in 1865. The next couple years marked the first period in United States history with sizeable black registration: Congressional legislation resulted in four of every five eligible blacks registered. The ratification of the fourteenth and fifteenth amendments intended to protect the voting rights of this newly enfranchised class. Black voting ultimately resulted in black political representation in the Reconstruction era.

Democratic control of Congress, futile extralegal efforts to limit the black vote, and Republicans' emphasis on pandering to Western white voters over Southern black voters contributed to the creation of prohibitive legal measures to erase earlier gains in black registration. From roughly

1890 to 1910, Southern states implemented barriers such as grandfather clauses, the all-white primary, literacy tests, and poll taxes to achieve effective black disenfranchisement. The women's suffrage movement of the 1910s and 1920s failed to make any considerable advancements along racial lines. The 1940s marked the first substantial improvements for black suffrage of the century: the Supreme Court's banning of the all-white primary coupled with NAACP registration drives generated growth in black registration rates. The Court's ruling in *Brown v. Board (1954)* that "separate was no longer equal" reignited white resistance to black registration. Gains in black political participation slowed as a result. By 1956, as many as four of every five eligible blacks remained unregistered (Valelly, 2004).[3]

Black civil rights organizations were vocal critics of rampant disenfranchisement throughout the South. The Southern Regional Council began routinely publishing reports outlining voting inequities across racial lines; A. Philip Randolph, Roy Wilkins, and Martin Luther King Jr.'s "Prayer Pilgrimage for Freedom" (May 17, 1957) generated added issue salience on a national level; and the SCLC's "Crusade for Citizenship" (1958), although an abject failure, served as a template for future large-scale Southern mobilization campaigns. According to Faulkenbury (2019), these developments set the stage for the Voter Education Project.

The founding of the VEP required multiple parties to unite under the common goal of registering eligible black voters. In the midst of the Freedom Rides, the Kennedy administration and Department of Justice were keen on shifting black activists away from direct action in favor of political participation. For the Kennedy's, the latter recourse would not only limit bloodshed, such as the recent bus bombing in Anniston, Alabama, but also increase Democratic vote share throughout the region. Robert Kennedy, among others, pitched the idea for an organization

[3]This historical background has drown solely from Valelly (2004) up to this point.

responsible for launching local mobilization campaigns to the "big five" civil rights groups and wealthy donors. The NAACP, SCLC, SNCC, CORE, and NUL were enticed by access to funds, along with a shared vision that voting rights would be a long-run determinant of black political influence. Collectively, they formed the Voter Education Project, named to mimic their parent organization's (SRC) research focus to retain tax-exemption status.

The first VEP campaign from 1962 to 1964 is believed to have registered some 688,000 blacks by funding 129 projects totaling $855,836.59 ($8,900,388.81 in $2024) in expenditures. The current paper aims to causally tie these projects to statewide trends in black registration. The VRA's passage in 1965 established federal oversight in covered Southern counties, but left many blacks unregistered. This continued disenfranchisement motivated the second VEP campaign from 1966 to 1969, which included around 409 projects and $580,065 ($5,622,852.92 in $2024) in expenditures. Conservative lawmakers severely weakened the VEP's funding capabilities through the Tax Reform Act of 1969. The VEP never attained its previous success and officially ceased operation in 1992 (Faulkenbury, 2019).[4]

3.3 Empirical Design

3.3.1 Data

Mobilization projects funded by the VEP are listed in appendix tables of Faulkenbury (2016). I digitize these tables where each row corresponds to a project. To my knowledge, this source contains all mobilization projects from May 1962 through 1970. Following digitization, I map each project to the county level for regression analysis. There are a number of complications

[4]The information on the VEP in this historical background is drawn solely from Faulkenbury (2019).

involved with this mapping. First, projects are defined at different geographic levels. Some are defined regionally, such as the Southeastern Georgia Crusade for Voters, while others are defined at the city level, such as Atlanta, Georgia. I am forced to drop regional and statewide projects. Projects at the county level require no mapping. Finally, projects at the city level are aggregated to their respective county. Since some cities partially cross into multiple county borders, I attempt to use the county with the most overlap. For example, Atlanta is mapped to Fulton County even though about 10% of the city belongs to DeKalb County. Robustness exercises will remove concern over potential spillover effects into neighboring counties.[5]

Panel (a) of figure 3.2 plots projects from the VEP's first campaign spanning May 1962 through 1964. Counties with at least one mobilization project are in orange. Projects are dispersed throughout the former Confederacy with discernible clusters around the Mississippi River, southwestern Georgia, central South Carolina, and the North Carolina-Virginia border. Most of these projects belong in future VRA coverage zones outlined in black. Panel (b) of the same figure plots county-level differences in average turnout before (1952-1960) and after (1964-1980) the mobilization campaigns. Counties in green (orange) experience net increases (decreases) in turnout, where color intensity represents the magnitude of gains (losses). Most counties saw at least modest gains in political participation with exception to the Appalachia region. There is noticeable overlap between counties with VEP projects and counties with large turnout gains.

Table 3.1 displays summary statistics by VEP status. Panel (a) includes data on turnout and registration. The turnout data is sourced from CQ Press Voting and Elections Collection. Registration data is scraped from United States Civil Rights Commission Reports (USCCR, 1959,

[5]Strictly speaking, I do not explicitly compute intersections between city and county boundaries. Instead, I reach these determinations through Google search results. The raw data tracks the web sources used to map cities to counties.

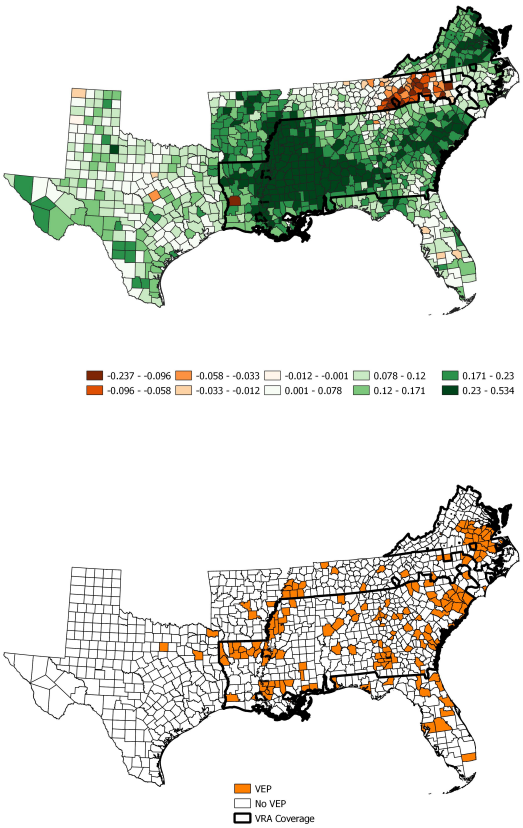


Figure 3.2: Difference in Average Turnout Pre- and Post-1964 (Top) and Voter Education Projects, May 1962-1964 (bottom)

The top figure displays differences in average turnout rates pre- (1952-1960) and post-1964 (1964-1980). Green (Orange) counties experience net gains (losses) in turnout, where color intensity corresponds to the magnitudes of gains (losses). The bottom figure plots counties with mobilization campaigns (in orange) conducted by the Voter Education Project from May 1962 through 1964. Counties in bolded regions are covered under the Voting Rights Act of 1965.

1961, 1968). I combine the 1959 and 1961 reports to obtain a rough estimate of county-level registration in 1960. Only states with non-missing 1959/1961 and 1968 registration data are counted towards registration summary statistics (AL, FL, GA, LA, MS, NC, SC).[6] VEP and non-VEP counties are fairly similar across these measures: differences in averages by treatment status are within a standard deviation.

Panel (b) includes socio-demographic information. The log population and economic covariates are gathered from the County and City Data Book (1947-1977); the cotton suitability index from Hornbeck and Naidu (2014); and the pro- and anti-black protest counts from Bernini et al. (2023b).[7] VEP and non-VEP counties share similar economic circumstances, but vary by urbanicity and pro-black protests. Assuming pro-black protests are more common in the face of greater white resistance, these differences are consistent with the VEP's selection process of counties to mobilize (Watters and Cleghorn, 1967). Empirically, I will tackle potential selection bias by internalizing differential treatment effects along these dimensions.

3.3.2 Raw Trends

Prior to formal estimation, I consider raw trends by type: only VEP counties, only VRA counties, VEP-by-VRA counties, and control counties. A county is labeled a "VEP county" if its first mobilization project was conducted between 1962 and 1964, inclusively. In other words,

[6] All counties in Arkansas are represented in the combined 1959/1961 report, but not in the 1968 report. The earlier combined report includes Tennessee, with exception to 32 counties with missing data for both white and non-white registration. Tennessee was omitted from the 1968 report. Texas falls in the same category as Tennessee, only with 24 missing counties in the 1959/1961 report. Virginian counties are almost completely represented in the earlier combined report (130/133), but entirely excluded from the 1968 report. While all the counties in the seven states listed in the main text are included for summary statistics, some have missing information. One county in Georgia (Spalding) did not record white registration in 1959/1961. Every county in Mississippi also has missing white registration data in the earlier combined report. Three counties in North Carolina (Davidson, Orange, Yadkin) did not record registration by race in 1968. Finally, one county in Florida (Dade) recorded complete registration data by race, but is not included due to being dropped during data construction.

[7] Bernini et al. (2023b) derive these counts using the Dynamics of Collective Action Dataset.

Table 3.1: Summary Statistics - Turnout, Registration, and Socio-Demographics

	VEP		No VEP	
	(1) Mean	(2) SD	(3) Mean	(4) SD
Panel A: Turnout & Registration				
Avg. Turnout, Pre-1964	33.252	9.674	41.389	14.683
Avg. Turnout, Post-1964	50.947	7.401	54.906	9.032
Diff. in Avg. Turnout	17.695	9.094	13.517	11.454
White Registration, 1960	74.470	17.141	86.601	14.807
Non-White Registration, 1960	25.194	20.189	34.102	25.730
Registration Racial Diff., 1960	-49.276	25.120	-52.499	26.740
White Registration, 1968	84.776	14.822	91.751	13.098
Non-White Registration, 1968	53.846	17.980	56.073	22.705
Registration Racial Diff., 1968	-30.930	20.677	-35.679	22.699
Panel B: Socio-Demographics				
(log) Population, 1960	10.698	1.166	9.758	0.904
Urban, 1960	41.485	28.538	26.185	25.079
Unskilled, 1960	29.661	9.513	26.699	8.425
Unemployed, 1960	4.967	1.586	5.050	2.075
Family Poverty, 1960	45.213	15.596	46.571	14.057
Cotton Suitability	0.381	0.275	0.433	0.406
Pro-Black County, 1960-1964	4.834	11.530	0.175	1.132
Anti-Black County, 1960-1964	0.981	3.824	0.057	0.521
Obs.	157	157	857	857

This table displays summary statistics for counties with(out) VEP mobilization. The first panel stores outcome variables on turnout and registration by race, where the latter measure is only available for counties in AL, FL, GA, LA, MS, NC, and SC. The second panel displays covariates on population, urbanicity, a series of economic conditions, and pro- and anti-black protest counts.

only counties with at least one campaign prior to the passage of the VRA. I follow Cascio and Washington (2013) by estimating over the following specification for each election year-by-type:

$$ln(Y_c) = \beta Black_t + \gamma^{'}\mathbf{X}_c + \mu_{s(c)} + \epsilon_c, \tag{3.1}$$

where $ln(Y_c)$ is log turnout, $Black_t$ a county's percent black in 1960, $\mathbf{X}_c$ a set of covariates, and $\mu_{s(c)}$ state-level fixed effects. My choice of covariates is motivated by Bernini et al. (2023b): log population (1960), percent urban (1960), percent unskilled (1960), percent unemployed (1960), Hornbeck and Naidu's cotton suitability index, and counts of pro- and anti-black protests (1960-1964). Standard errors are clustered at the state level.

Figure 3.3 plots estimated trend lines by type: green-circles are only VEP; orange diamonds are only VRA; purple-triangles VEP-by-VRA; and pink-plus signs control counties. Dashed vertical lines mark the first presidential elections following mobilization projects (1964) and the Voting Rights Act (1968). The trends suggest that mobilization and electoral oversight geared towards black voters improved turnout rates. However, pre-trends for the VEP and VRA call into question the causality behind this claim. Control counties also experience a bump in turnout by the slope of percent black, but this increase is comparatively modest and within the confidence intervals of pre-period estimates. These results signal the need to internalize time invariant differences across counties and general temporal trends.

3.3.3 Main Specification

This paper aims to identify the source of gains in Southern black registration rates immediately preceding and following the passage of the Voting Rights Act of 1965. The political economics

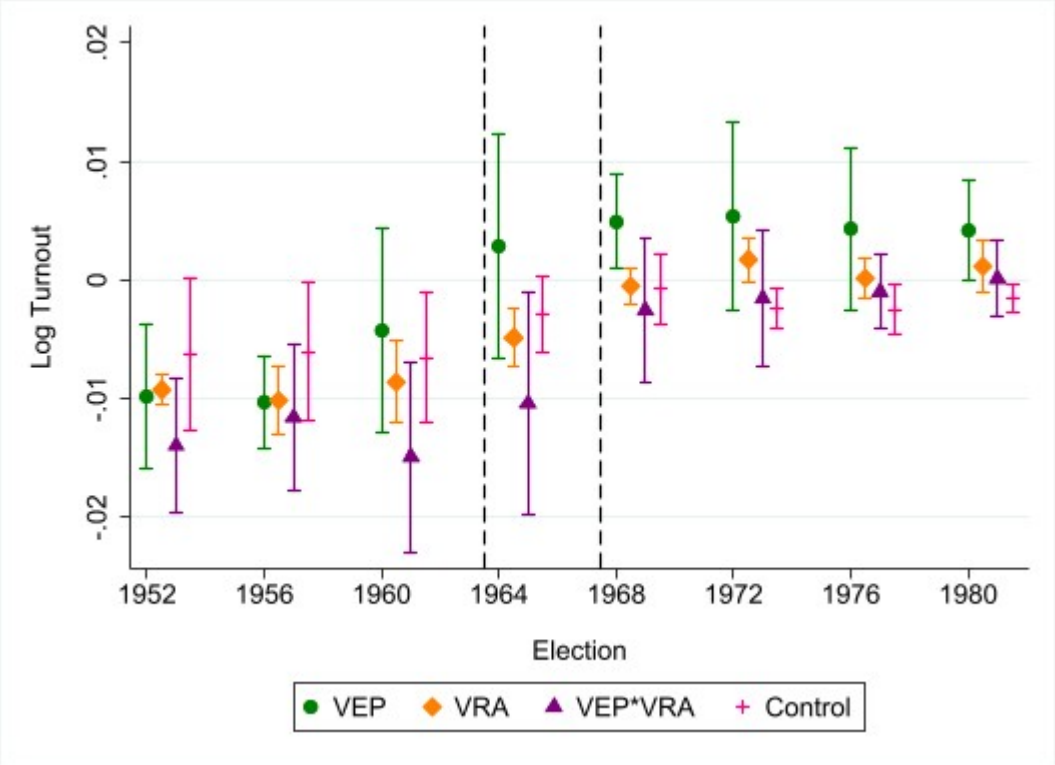


Figure 3.3: Raw Trends in Log Turnout by Percent Black: Breakdowns by VEP, VRA, VEP-VRA, and Control

This figure displays raw trends by type (VEP, VRA, VEP-by-VRA, Control) from estimating equation (3.1) for each election year-by-type. The dashed lines correspond to the timing of the first presidential elections following VEP mobilization projects and the VRA.

literature suggests that VEP mobilization projects are explanatory of rising black registration (Cascio and Washington, 2013). To my knowledge, I am the first to put this theory to the test. One present data difficulty is that registration rates by race at the county level are mostly unavailable.[8] As a result, I follow Cascio and Washington (2013) and Bernini et al. (2023b) by using log turnout and identifying along the slope of percent black. Intuitively, black mobilization campaigns should disproportionately affect counties with larger black shares. I modify the cited papers' main specification by permitting a statistical horse race between the VEP and the VRA. This paper estimates the following specification over a balanced panel ranging from 1952 to 1980:

$$ln(Y_{ct}) = \lambda Post_t Black_c + \sum_g \delta_g Post_t Black_c \times Type\{g\}_c + \gamma^{'}\mathbf{X}_c + \mu_{s(c)t} + \sigma_c + \epsilon_{ct}, \quad (3.2)$$

where $ln(Y_{ct})$, $Black_c$, and $\mathbf{X}_c$ are defined as before, $Post_t$ is a binary equal to one for presidential elections in 1964 and beyond, $Type\{g\}_c$ denotes whether county c has a VEP project between 1962-1964, is covered by the VRA, or both, and $\mu_{s(c)t}$ and σ_c are state-by-election year and county fixed effects. The omitted type is the set of counties without VEP mobilization and VRA coverage. The same covariates used for raw trends are included in the main specification. However, I now interact each covariate by $Post_t$, a post-by-vep term, and a post-by-vra term. Temporal interactions internalize differential trends in static (1960) covariates. Moreover, interactions by time and VEP/VRA absorb heterogeneous treatment effects in mobilization/electoral oversight

[8] In subsection 3.7.3, I take an alternative empirical approach using registration data from around the 1960 and 1968 elections as the dependent variable. There is evidence of large registration gains among Non-Whites, but small sample sizes prevent statistically significant claims.

by each covariate. These terms should isolate a pure effect by percent black from other correlated VEP/VRA effects along other covariate dimensions. Inclusion of these terms is motivated by Cascio and Washington (2013) and Bernini et al. (2023b). As before, I cluster standard errors at the state level.

3.4 Results

3.4.1 Baseline Estimates

Table 3.2 displays results from estimating variants of equation (3.2). The first couple columns estimate the effect of VEP mobilization and VRA oversight separately, controlling only for county and state-by-election year fixed effects. In these baseline specifications, VEP counties do not see a rise in turnout by percent black relative to non-VEP counties, but VRA counties see a 0.48% marginal gain compared to non-VRA counties. Column (3) displays results from a statistical horse race between the VEP and VRA internalizing only time invariant county features and secular statewide trends. In this case, only VEP (VRA) counties experience a statistically significant 0.33% (0.50%) rise. Interestingly, the interaction effect between the VEP and VRA wipes out the non-interacted VEP effect. This result suggests that mobilization gains were only made outside of VRA coverage zones. Adding covariates interacted by the post-period binary yield similar results. Column (5) displays results from subsequently controlling for covariate interactions by time (post-period) and VEP/VRA status. These results correspond to estimating the main specification. Only VEP or VRA counties see similar boosts in turnout (0.55% and 0.57%), but there is a diminishing cumulative effect for VEP-by-VRA counties of 0.38%. In other words, pre-VRA mobilization projects launched by the VEP appears equally effective in

Table 3.2: The Impact of the VEP and VRA by Percent Black on Log Turnout

	(1)	(2)	(3)	(4)	(5)
Post $\cdot Black$	0.0073***	0.0048***	0.0044***	0.0046***	0.0042***
	(0.0012)	(0.0009)	(0.0008)	(0.0008)	(0.0007)
Post $\cdot Black \cdot VEP$	0.0007		0.0033**	0.0026*	0.0055**
	(0.0005)		(0.0011)	(0.0014)	(0.0019)
Post $\cdot Black \cdot VRA$		0.0048***	0.0050***	0.0049***	0.0057***
		(0.0013)	(0.0012)	(0.0013)	(0.0014)
Post $\cdot Black \cdot VEP \cdot VRA$			-0.0032**	-0.0027*	-0.0038**
			(0.0011)	(0.0013)	(0.0013)
County FEs	X	X	X	X	X
State-Year FEs	X	X	X	X	X
Covs				X	X
Covs-VEP					X
Covs-VRA					X
Mean	-0.760	-0.760	-0.760	-0.760	-0.760
SD	0.319	0.319	0.319	0.309	0.309
Obs.	9,024	9,024	9,024	8,112	8,112

This table displays regression output from estimating different variants of equation (3.2). The first two columns consider VEP and VRA effects in isolation, while only controlling for county and state-by-year fixed effects. Column (3) adds an interaction effect between the VEP and VRA; column (4) adds the set of covariates referenced in the main text interacted by a post-period term; and column (5) adds a further set of covariate interactions by VEP and VRA status. The last column stores results from estimating over the main specification.

increasing black turnout relative to the VRA. The diminishing joint effect further suggests that conditional on mobilization (electoral oversight), electoral oversight (mobilization) has a lower marginal return.

The triple difference-in-differences results signal the importance of the VEP in bolstering black turnout, but it remains to be shown that the VEP actually had a pre-VRA effect. I amend equation (3.2) by replacing $Post_t$ with election year binary terms to capture dynamic effects.[9] Interactions with the 1960 election year are omitted to avoid multi-collinearity. Figure 3.4 displays results from estimating the dynamic specification. Estimates corresponding to election year-by-black interactions with VEP (green-circle), VRA (orange-diamond), and VEP-by-VRA (purple-triange) are appropriately labeled in the legend. Since leads are fairly balanced for each type, earlier concerns about pre-trends in the raw data are mitigated. Clearly, there is a rise in 1964 turnout by percent black in only VEP counties, albeit at the 10% significance level, that predates an analogous rise for only VRA counties in the next presidential election cycle. The earlier gains in turnout attributed to the VEP appear limited to future non-VRA counties. Dynamic effects for only VEP or VRA counties are similar across the post-period elections.

3.4.2 Robustness Exercises

The baseline results persist across a wide-range of robustness exercises. First, I show that the results are not specific to the baseline control set: I restrict the control set to counties with future VEP projects (1966-1970). Figure 3.A1 displays these counties in green. Visually, the

[9] This update takes the following form:

$$ln(Y_{ct}) = \sum_{t \neq 1960} \lambda_t \mathbf{1}\{t\} Black_c + \sum_{g} \sum_{t \neq 1960} \delta_{tg} \mathbf{1}\{t\} Black_c \times Type\{g\}_c + \gamma^{'} \mathbf{X}_c + \mu_{s(c)t} + \sigma_c + \epsilon_{ct}, \quad (3.3)$$

where $\mathbf{1}\{t\}$ is a binary term for election year t.

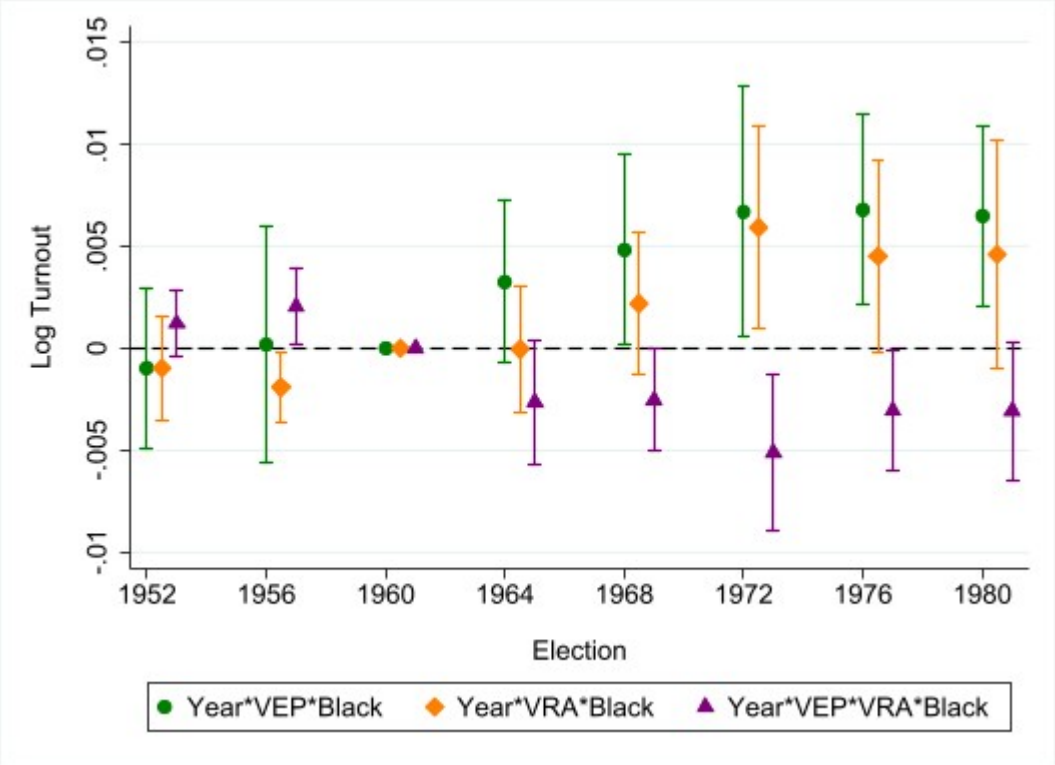


Figure 3.4: Event Study: The Impact of the VEP and VRA by Percent Black on Log Turnout

This figure displays estimates from estimating the dyanamic variant of equation (3.2), i.e. equation (3.3). Estimates represented by green-circles correspond to only VEP counties, orange-diamonds only VRA counties, and purple-triangles VEP-by-VRA counties.

VEP targeted counties in close proximity to sites of their first campaign. Results from estimating over this restricted subset show an even stronger VEP and VRA effect on turnout. The preferred specification yields VEP and VRA gains of 0.90% and 0.73%: see table 3.A1. I further show that dropping future VEP counties yields nearly identical estimates compared to the baseline results: see table 3.A2.

One concern is that the baseline results are dependent on the modeler's choice of temporal coverage. In line with Cascio and Washington (2013) and Bernini et al. (2023b), I estimate over the two-period, long-form difference-in-differences variant of equation (3.2) for added robustness: only election years 1960 and 1980 are included, county fixed effects are omitted, and state-by-election year fixed effects are substituted with state fixed effects. Estimation yields similar VEP (0.65%) and VRA (0.46%) effects.

In addition, I demonstrate that the statistical significance of the baseline results is not dependent on the type of standard error. While clustering at the state level absorbs within-state correlation across counties, having only 11 states in the data calls into question the appropriateness of this choice. Table 3.A4 replicates the baseline results using robust standard errors (in parenthesis), and clustered standard errors as before (in brackets), but with wild cluster bootstrapped p-values to account for few clusters. Both only VEP or VRA effects are significant at the 5% level. However, the VEP-by-VRA joint effect is no longer significant using wild cluster bootstrapped p-values. This finding suggests that there might not be diminishing returns from mobilization (electoral oversight) following electoral oversight (mobilization) after all.

Finally, this paper shows that data processing, namely mapping cities to counties, is not driving the baseline results. As earlier mentioned, cities often overlap county lines, but empirical analysis requires a mapping to a single county. In the data, I attempt to map cities to counties with

the largest boundary overlap. I re-estimate equation (3.2) after dropping neighboring counties: see figure 3.A2 for a visualization of these discarded counties. Dropping these counties should mitigate a potential spillover effect contaminating the baseline results. Table 3.A5 displays these estimates, which are virtually the same as the baseline results.

3.5 Mechanisms

In this section, I aim to pinpoint the drivers of the VEP's success in bolstering black turnout. Three candidate mechanisms will be explored. First, I will consider the number of mobilization projects conducted in counties: *how much does mobilization intensity matter?* Second, this paper will analyze mobilization projects by organizational leaders: *does "Big Five" organization matter relative to pure grassroots organization?*. Finally, I will consider differential levels of white resistance to black political participation: *does greater (lesser) resistance depress (amplify) mobilization returns?*

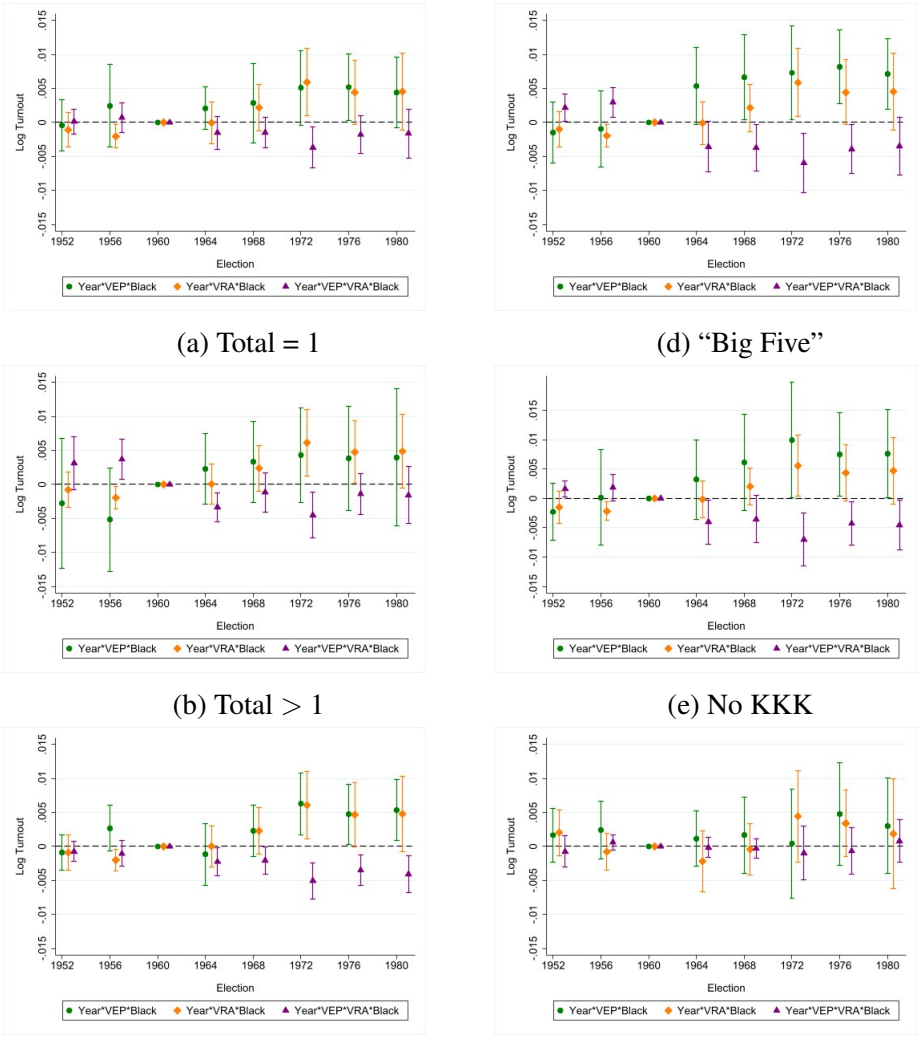
3.5.1 Intensive Margin in VEP Projects

The baseline results treat mobilization as a binary variable, but the VEP actually targeted some counties more than others. While tracking VEP expenditures to the county level would be a first-best approach to quantify the intensive margin, data limitations prevent this level of analysis. Instead, I determine treatment intensity by the number of VEP projects from 1962 through 1964. Figure 3.A3 displays VEP counties as before, but distinguishes between counties with one (light orange) and more than one (dark orange) project: the former group consists of roughly 70% of treated counties. I re-estimate equation (3.2) distinguishing between these two treatment intensity

breakdowns. These results are stored in table 3.A6. Counties with multiple mobilization projects appear to see greater turnout returns, but neither breakdown yields statistically significant results: however, the estimate for multiple treatment counties is roughly the same as the baseline estimate. Event study results for each case exhibit similar dynamic trends, with neither case seemingly capturing pre-VRA gains: see figure 3.5. Treatment intensity may have a small impact on the baseline results, but does not appear to be the leading mechanism.

3.5.2 "Big Five" Organizations

The VEP awarded grant funds for mobilization projects to "big five" and grassroots organizations alike. Most counties that had at least one mobilization project had some level of "big five" organization: see figure 3.A4 for a visualization. As before, I re-estimate equation (3.2) separating treated counties by "big five" and grassroots organization. Counties with multiple VEP projects from 1962 to 1964 are coded as "big five" if at least one project was spearheaded by the NAACP, SCLC, SNCC, CORE, or NUL. Table 3.A7 shows that mobilization campaigns with "big five" organizations at the helm were nearly twice as effective. Counties with national civil rights organizations saw a 0.66% rise in turnout compared to only a 0.33% gain in the remaining counties. I explore dynamic estimates to see if organization leadership is responsible for pre-VRA gains. Figure 3.5 plots these results. Unlike treatment intensity, "big five" leadership appears explanatory of rising black registration in 1964. Yet, local organizations prove equally effective in the post-VRA election cycles.



(a) Total = 1

(d) "Big Five"

(b) Total $>$ 1

(e) No KKK

(c) No "Big Five"

(f) KKK

Figure 3.5: Mechanism Event Study

This figure displays estimates from estimating the dyanamic variant of equation (3.2), i.e. equation (3.3), by different cases. Plots (a) and (b) consider treatment intensity; plots (c) and (d) "big five" (NAACP, SNCC, SCLC, CORE, and NUL) organization status, and plots (e) and (f) KKK presence. Estimates represented by green-circles correspond to only VEP counties, orange-diamonds only VRA counties, and purple-triangles VEP-by-VRA counties.

3.5.3 White Resistance

White resistance to social and political integration undoubtedly characterized the South during the Civil Rights era. As earlier stated, reaction to the *Brown v. Board* verdict negatively impacted black registration rates (Valelly, 2004). Consistent with Key (1949), Cunningham (2013) claims a prime determinant of KKK membership was varying underlying incentive structures to preserve segregation. This paper explores the importance of white resistance by using KKK presence as a proxy for anti-black attitudes. I draw from Ang (2019) for data on third-wave KKK chapters at the county level. Figure 3.A5 plots KKK counties by treatment status: over half of the VEP counties have at least one Klan chapter. Results stored in table 3.A8 reveal that mobilization proved ineffective in the face of organized white resistance. Only mobilized counties without the Klan experienced rising turnout rates by percent black (0.72%). Interestingly, these gains were not realized in 1964: event study dynamics displayed in figure 3.5 show that this distinction is not all that meaningful in capturing pre-VRA gains in black political participation.

3.6 Conclusion

The Voting Rights Act of 1965 improved black turnout rates, political representation, and voting on civil rights-related bills (Ang, 2019; Bernini et al., 2023b; Cascio and Washington, 2013; Fresh, 2018; Schuit and Rogowski, 2017). Yet, pre-VRA statewide trends in black registration suggest another source contributed to reintegrating blacks into the political process. Cascio and Washington (2013) conjecture that mobilization campaigns conducted by the Voter Education Project are responsible for pre-VRA gains. Valelly (2004) makes a similar claim while stating that no one has empirically demonstrated this relationship. The goal of this paper is tie the VEP

to black registration closely before, as well as after, the passage of the Voting Rights Act of 1965.

While registration data by race is limited at the county-level over the temporal window, turnout measures and percent black shares are readily available. As a result, I use overall turnout as the dependent variable and identify along the slope of percent black (Bernini et al., 2023b; Cascio and Washington, 2013). This design rests on the assumption that counties with larger black shares are more likely impacted by black mobilization projects. First, I estimate the triple interaction term by VEP, VRA, and VEP-by-VRA type to determine the average rise in turnout by percent black, pre- and post-1964, due to mobilization and/or electoral oversight. Second, I replace the post-period binary with a set of election-year binaries to estimate dynamic effects. In particular, interest lies in the 1964 presidential contest that predates the passage of the VRA.

This paper finds that mobilization campaigns outside eventual VRA coverage zones, relative to included counties without pre-VRA mobilization, were equally effective in registering blacks from 1964 to 1980. Only VEP (VRA) counties saw a 0.55% (0.57%) increase relative to non-VEP and non-VRA counties. Event study dynamics provide suggestive evidence that the VEP was indeed responsible for pre-VRA black registration: mobilization generates a 0.33% rise in 1964 turnout for every percentage point increase in percent black at the 10% level of significance. These results are robust to choice of control groups, temporal coverage, standard error type, and the inclusion/exclusion of future VEP and neighboring VEP counties. I set out to test three potential mechanisms driving the baseline results: treatment intensity (number of mobilization projects), organizational leadership ("big five" or grassroots), and white resistance (presence of KKK chapters). Comparatively, treatment intensity appears the least important of the three. Instead, "big five" organization is most explanatory of pre-VRA gains, while white resistance better captures post-VRA gains.

3.7 Appendix

3.7.1 Additional Tables and Figures

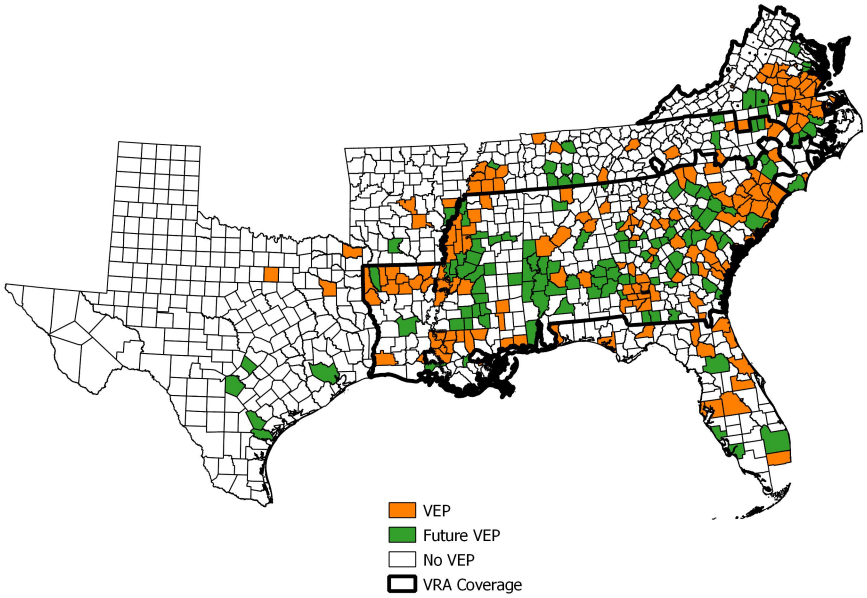


Figure 3.A1: Voter Education Projects, May 1962-1970

This figure plots counties with mobilization campaigns (in orange) conducted by the Voter Education Project from May 1962 through 1970. Counties with a project conducted between May 1962 through 1964 are in orange; remaining counties with a project between 1966 and 1970 are in green. Counties in bolded regions are covered under the Voting Rights Act of 1965.

Table 3.A1: Alternative Control Set: The Impact of the VEP and VRA by Percent Black on Log Turnout

	(1)	(2)	(3)	(4)	(5)
Post $\cdot Black$	0.0091***	0.0055***	0.0046***	0.0039**	0.0019
	(0.0011)	(0.0013)	(0.0007)	(0.0014)	(0.0015)
Post $\cdot Black \cdot VEP$	-0.0001		0.0040***	0.0038***	0.0090***
	(0.0004)		(0.0008)	(0.0011)	(0.0026)
Post $\cdot Black \cdot VRA$		0.0049**	0.0059***	0.0060***	0.0073***
		(0.0017)	(0.0015)	(0.0016)	(0.0017)
Post $\cdot Black \cdot VEP \cdot VRA$			-0.0044***	-0.0043***	-0.0056***
			(0.0009)	(0.0008)	(0.0016)
County FEs	X	X	X	X	X
State-Year FEs	X	X	X	X	X
Covs				X	X
Covs-VEP					X
Covs-VRA					X
Mean	-0.878	-0.878	-0.878	-0.881	-0.881
SD	0.375	0.375	0.375	0.376	0.376
Obs.	2,392	2,392	2,392	2,128	2,128

This table displays regression output from estimating different variants of equation (3.2). The first two columns consider VEP and VRA effects in isolation, while only controlling for county and state-by-year fixed effects. Column (3) adds an interaction effect between the VEP and VRA; column (4) adds the set of covariates referenced in the main text interacted by a post-period term; and column (5) adds a further set of covariate interactions by VEP and VRA status. The last column stores results from estimating over the main specification. The control set is restricted to counties with future VEP projects (1966-1970).

Table 3.A2: Drop Future VEP: The Impact of the VEP and VRA by Percent Black on Log Turnout

	(1)	(2)	(3)	(4)	(5)
Post $\cdot Black$	0.0063***	0.0049***	0.0042***	0.0046***	0.0041***
	(0.0011)	(0.0010)	(0.0009)	(0.0009)	(0.0009)
Post $\cdot Black \cdot VEP$	0.0015**		0.0034**	0.0028*	0.0053**
	(0.0006)		(0.0012)	(0.0015)	(0.0019)
Post $\cdot Black \cdot VRA$		0.0041***	0.0043***	0.0041***	0.0052***
		(0.0012)	(0.0011)	(0.0012)	(0.0013)
Post $\cdot Black \cdot VEP \cdot VRA$			-0.0028**	-0.0023	-0.0033**
			(0.0012)	(0.0014)	(0.0014)
County FEs	X	X	X	X	X
State-Year FEs	X	X	X	X	X
Covs				X	X
Covs-VEP					X
Covs-VRA					X
Mean	-0.745	-0.745	-0.745	-0.745	-0.745
SD	0.303	0.303	0.303	0.293	0.293
Obs.	8,024	8,024	8,024	7,240	7,240

This table displays regression output from estimating different variants of equation (3.2). The first two columns consider VEP and VRA effects in isolation, while only controlling for county and state-by-year fixed effects. Column (3) adds an interaction effect between the VEP and VRA; column (4) adds the set of covariates referenced in the main text interacted by a post-period term; and column (5) adds a further set of covariate interactions by VEP and VRA status. The last column stores results from estimating over the main specification. Counties with future VEP projects are omitted.

Table 3.A3: Long DiD: The Impact of the VEP and VRA by Percent Black on Log Turnout

	(1)	(2)	(3)	(4)	(5)
Post $\cdot Black$	0.0081***	0.0056**	0.0050**	0.0054**	0.0050**
	(0.0015)	(0.0019)	(0.0018)	(0.0017)	(0.0016)
Post $\cdot Black \cdot VEP$	0.0009		0.0024	0.0018	0.0065***
	(0.0009)		(0.0016)	(0.0018)	(0.0020)
Post $\cdot Black \cdot VRA$		0.0040	0.0044*	0.0040	0.0046*
		(0.0022)	(0.0022)	(0.0022)	(0.0025)
Post $\cdot Black \cdot VEP \cdot VRA$			-0.0020	-0.0014	-0.0031*
			(0.0018)	(0.0019)	(0.0015)
County FEs	X	X	X	X	X
State FEs	X	X	X	X	X
Covs				X	X
Covs-VEP					X
Covs-VRA					X
Mean	0.330	0.330	0.330	0.328	0.328
SD	0.323	0.323	0.323	0.327	0.327
Obs.	1,128	1,128	1,128	1,014	1,014

This table displays regression output from estimating different variants of the two-period (1960,1980), long-form DiD specification. The first two columns consider VEP and VRA effects in isolation, while only controlling for county and state-by-year fixed effects. Column (3) adds an interaction effect between the VEP and VRA; column (4) adds the set of covariates referenced in the main text interacted by a post-period term; and column (5) adds a further set of covariate interactions by VEP and VRA status. The last column stores results from estimating over the main specification.

Table 3.A4: Robust SEs and Wild Cluster Bootstrapped p-values: The Impact of the VEP and VRA by Percent Black on Log Turnout

	(1)	(2)	(3)	(4)	(5)
Post $\cdot Black$	0.0073	0.0048	0.0044	0.0046	0.0042
	(0.0002)***	(0.0002)***	(0.0002)***	(0.0002)***	(0.0003)***
	[0.0012]***	[0.0009]***	[0.0008]***	[0.0008]***	[0.0007]***
Post $\cdot Black \cdot VEP$	0.0007		0.0033	0.0026	0.0055
	(0.0002)***		(0.0005)***	(0.0005)***	(0.0011)***
	[0.0005]		[0.0011]	[0.0014]	[0.0019]**
Post $\cdot Black \cdot VRA$		0.0048	0.0050	0.0049	0.0057
		(0.0002)***	(0.0002)***	(0.0003)***	(0.0003)***
		[0.0013]***	[0.0012]***	[0.0013]***	[0.0014]***
Post $\cdot Black \cdot VEP \cdot VRA$			-0.0032	-0.0027	-0.0038
			(0.0005)***	(0.0006)***	(0.0006)***
			[0.0011]	[0.0013]	[0.0013]
County FEs	X	X	X	X	X
State-Year FEs	X	X	X	X	X
Covs				X	X
Covs-VEP					X
Covs-VRA					X
Mean	-0.760	-0.760	-0.760	-0.760	-0.760
SD	0.319	0.319	0.319	0.309	0.309
Obs.	9,024	9,024	9,024	8,112	8,112

This table displays regression output from estimating different variants of equation (3.2). The first two columns consider VEP and VRA effects in isolation, while only controlling for county and state-by-year fixed effects. Column (3) adds an interaction effect between the VEP and VRA; column (4) adds the set of covariates referenced in the main text interacted by a post-period term; and column (5) adds a further set of covariate interactions by VEP and VRA status. The last column stores results from estimating over the main specification. Robust standard errors are in parenthesis; clustered standard errors (state-level) are in brackets with corresponding wild cluster bootstrapped p-values.

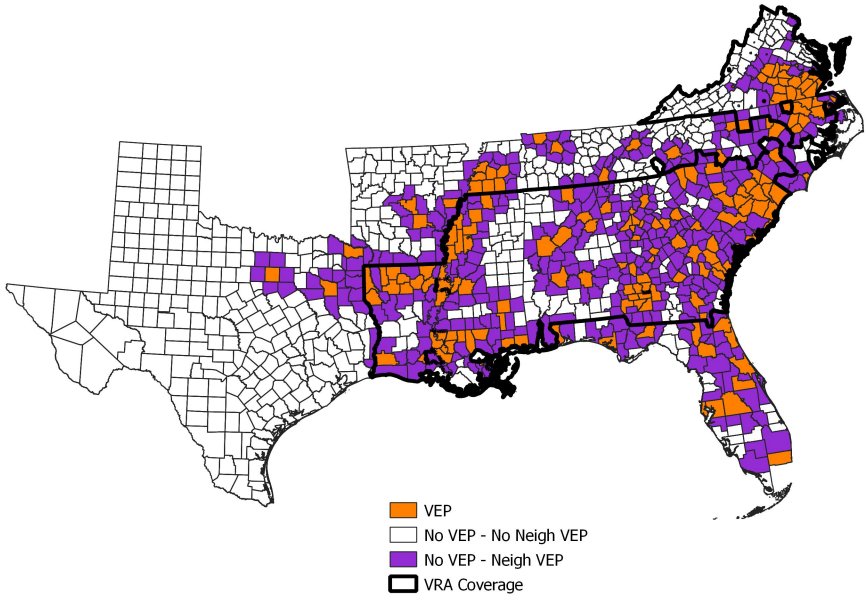


Figure 3.A2: Neighboring Counties without Voter Education Projects, May 1962-1964

This figure plots counties with mobilization campaigns (in orange) conducted by the Voter Education Project from May 1962 through 1964. Neighboring counties without a VEP project are in purple. Counties in bolded regions are covered under the Voting Rights Act of 1965.

Table 3.A5: Drop VEP Neighbors: The Impact of the VEP and VRA by Percent Black on Log Turnout

	(1)	(2)	(3)	(4)	(5)
Post $\cdot Black$	0.0054***	0.0045***	0.0033***	0.0035***	0.0029**
	(0.0014)	(0.0013)	(0.0010)	(0.0011)	(0.0009)
Post $\cdot Black \cdot VEP$	0.0023**		0.0042**	0.0040**	0.0066***
	(0.0010)		(0.0014)	(0.0015)	(0.0021)
Post $\cdot Black \cdot VRA$		0.0046**	0.0049***	0.0046***	0.0056***
		(0.0016)	(0.0014)	(0.0015)	(0.0017)
Post $\cdot Black \cdot VEP \cdot VRA$			-0.0031**	-0.0030*	-0.0038**
			(0.0013)	(0.0013)	(0.0014)
County FEs	X	X	X	X	X
State-Year FEs	X	X	X	X	X
Covs				X	X
Covs-VEP					X
Covs-VRA					X
Mean	-0.749	-0.749	-0.749	-0.750	-0.750
SD	0.297	0.297	0.297	0.297	0.297
Obs.	5,784	5,784	5,784	5,296	5,296

This table displays regression output from estimating different variants of equation (3.2). The first two columns consider VEP and VRA effects in isolation, while only controlling for county and state-by-year fixed effects. Column (3) adds an interaction effect between the VEP and VRA; column (4) adds the set of covariates referenced in the main text interacted by a post-period term; and column (5) adds a further set of covariate interactions by VEP and VRA status. The last column stores results from estimating over the main specification. Neighboring counties without VEP projects are omitted.

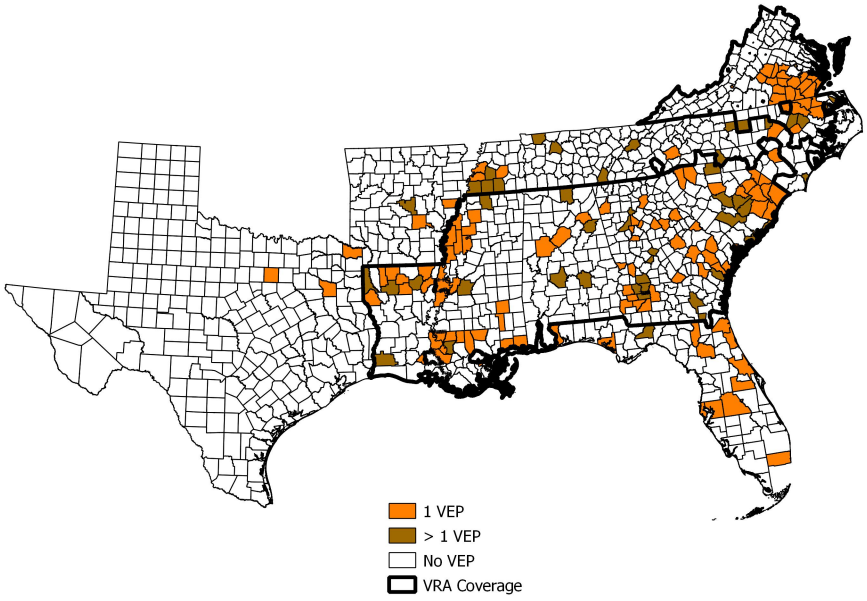


Figure 3.A3: Total Voter Education Projects, May 1962-1964

This figure plots counties with mobilization campaigns (in orange) conducted by the Voter Education Project from May 1962 through 1964. Counties with (more than) one project are in dark (light) orange. Counties in bolded regions are covered under the Voting Rights Act of 1965.

Table 3.A6: Total VEP Projects: The Impact of the VEP and VRA by Percent Black on Log Turnout

	Treat (Total = 1)			Treat (Total $>$ 1)		
	(1)	(2)	(3)	(4)	(5)	(6)
Post $\cdot Black$	0.0044***	0.0045***	0.0042***	0.0044***	0.0044***	0.0041***
	(0.0008)	(0.0008)	(0.0007)	(0.0009)	(0.0008)	(0.0007)
Post $\cdot Black \cdot VEP$	0.0014	0.0006	0.0029	0.0056***	0.0050**	0.0054
	(0.0010)	(0.0013)	(0.0016)	(0.0013)	(0.0016)	(0.0034)
Post $\cdot Black \cdot VRA$	0.0050***	0.0048***	0.0055***	0.0052***	0.0051***	0.0058***
	(0.0013)	(0.0013)	(0.0014)	(0.0012)	(0.0012)	(0.0013)
Post $\cdot Black \cdot VEP \cdot VRA$	-0.0015	-0.0008	-0.0017	-0.0050***	-0.0046**	-0.0042**
	(0.0011)	(0.0013)	(0.0011)	(0.0012)	(0.0015)	(0.0017)
County FEs	X	X	X	X	X	X
State-Year FEs	X	X	X	X	X	X
Covs		X	X		X	X
Covs-VEP			X			X
Covs-VRA			X			X
Mean	-0.755	-0.754	-0.754	-0.747	-0.745	-0.745
SD	0.316	0.306	0.306	0.310	0.297	0.297
Obs.	8,624	7,760	7,760	8,032	7,208	7,208

This table displays regression output from estimating different variants of equation (3.2). The first supercolumn restricted treated counties to only one VEP project; the second to more than one VEP project. Within each supercolumn, the first column controls only for county and state-by-year fixed effects; the second adds the set of covariates referenced in the main text interacted by a post-period term; and the third adds a further set of covariate interactions by VEP and VRA status. The third column stores results from estimating over the main specification.

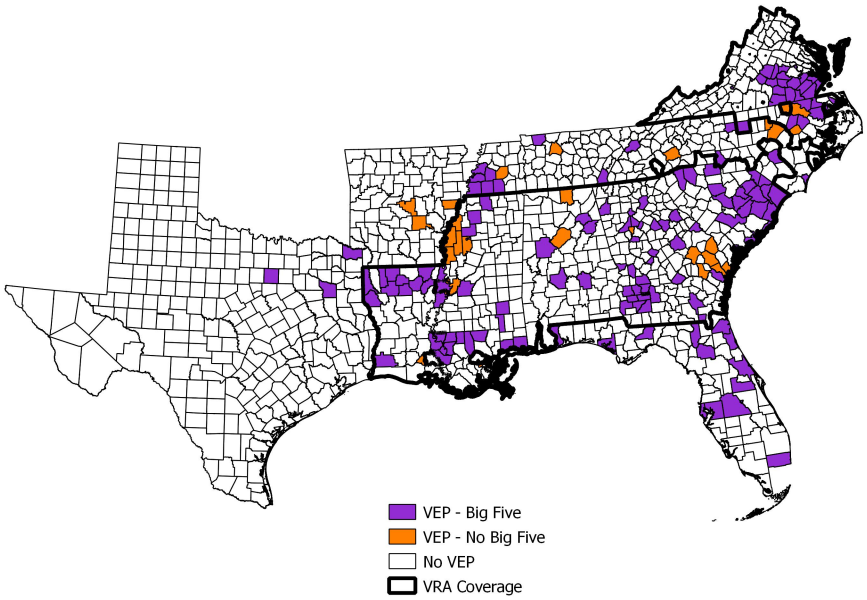


Figure 3.A4: Voter Education Projects with Big Five Organizations, May 1962-1964

This figure plots counties with mobilization campaigns conducted by the Voter Education Project from May 1962 through 1964. Counties with at least one project operated by a "big five" (NAACP, SNCC, SCLC, CORE, and NUL) organization are in purple; remaining counties with a project are in orange. Counties in bolded regions are covered under the Voting Rights Act of 1965.

Table 3.A7: Big Five Organization: The Impact of the VEP and VRA by Percent Black on Log Turnout

	Treat (No Big Five)			Treat (Big Five)		
	(1)	(2)	(3)	(4)	(5)	(6)
Post $\cdot Black$	0.0043***	0.0044***	0.0042***	0.0045***	0.0046***	0.0042***
	(0.0009)	(0.0008)	(0.0007)	(0.0008)	(0.0008)	(0.0007)
Post $\cdot Black \cdot VEP$	0.0030***	0.0018*	0.0035***	0.0035*	0.0029	0.0066**
	(0.0008)	(0.0008)	(0.0010)	(0.0016)	(0.0020)	(0.0026)
Post $\cdot Black \cdot VRA$	0.0051***	0.0050***	0.0057***	0.0051***	0.0049***	0.0056***
	(0.0012)	(0.0012)	(0.0013)	(0.0012)	(0.0013)	(0.0014)
Post $\cdot Black \cdot VEP \cdot VRA$	-0.0028**	-0.0016	-0.0023***	-0.0035*	-0.0031	-0.0047**
	(0.0010)	(0.0009)	(0.0007)	(0.0016)	(0.0019)	(0.0017)
County FEs	X	X	X	X	X	X
State-Year FEs	X	X	X	X	X	X
Covs		X	X		X	X
Covs-VEP			X			X
Covs-VRA			X			X
Mean	-0.744	-0.743	-0.743	-0.757	-0.756	-0.756
SD	0.311	0.298	0.298	0.316	0.305	0.305
Obs.	7,856	7,080	7,080	8,800	7,888	7,888

This table displays regression output from estimating different variants of equation (3.2). The first supercolumn restricted treated counties with no "big five" (NAACP, SNCC, SCLC, CORE, and NUL) project; the second with at least one "big five" project. Within each supercolumn, the first column controls only for county and state-by-year fixed effects; the second adds the set of covariates referenced in the main text interacted by a post-period term; and the third adds a further set of covariate interactions by VEP and VRA status. The third column stores results from estimating over the main specification.

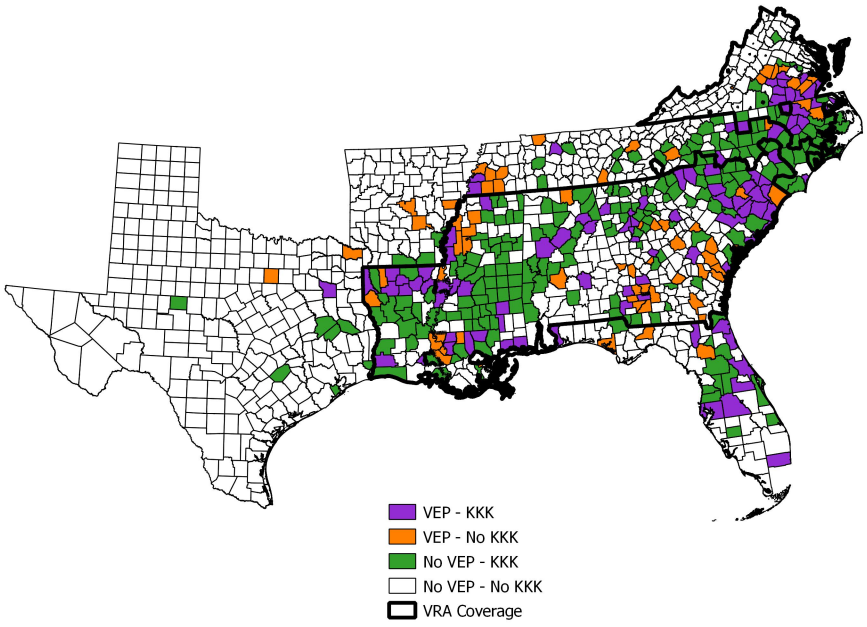


Figure 3.A5: Voter Education Projects (May 1962-1970) and the Ku Klux Klan

This figure plots counties with mobilization campaigns conducted by the Voter Education Project from May 1962 through 1964. Counties with a VEP project and a KKK chapter are in purple; counties with a VEP project and without a KKK chapter are in orange; counties with the KKK but without a VEP project are in green; and remaining counties without the Klan and a VEP project are in white. Counties in bolded regions are covered under the Voting Rights Act of 1965.

Table 3.A8: Ku Klux Klan: The Impact of the VEP and VRA by Percent Black on Log Turnout

	No KKK			KKK		
	(1)	(2)	(3)	(4)	(5)	(6)
Post $\cdot Black$	0.0041***	0.0042***	0.0040***	0.0056***	0.0065***	0.0050***
	(0.0008)	(0.0008)	(0.0008)	(0.0012)	(0.0011)	(0.0010)
Post $\cdot Black \cdot VEP$	0.0041***	0.0035*	0.0072**	0.0027***	0.0015	0.0028
	(0.0013)	(0.0017)	(0.0028)	(0.0005)	(0.0012)	(0.0017)
Post $\cdot Black \cdot VRA$	0.0053***	0.0051***	0.0054***	0.0047**	0.0045**	0.0064***
	(0.0013)	(0.0014)	(0.0015)	(0.0016)	(0.0015)	(0.0014)
Post $\cdot Black \cdot VEP \cdot VRA$	-0.0042***	-0.0036*	-0.0052**	-0.0026***	-0.0016	-0.0020**
	(0.0013)	(0.0016)	(0.0017)	(0.0007)	(0.0010)	(0.0007)
County FEs	X	X	X	X	X	X
State-Year FEs	X	X	X	X	X	X
Covs		X	X		X	X
Covs-VEP			X			X
Covs-VRA			X			X
Mean	-0.739	-0.739	-0.739	-0.815	-0.812	-0.812
SD	0.307	0.294	0.294	0.340	0.337	0.337
Obs.	6,488	5,768	5,768	2,536	2,344	2,344

This table displays regression output from estimating different variants of equation (3.2). The first supercolumn restricted treated counties without the KKK; the second to those with the KKK. Within each supercolumn, the first column controls only for county and state-by-year fixed effects; the second adds the set of covariates referenced in the main text interacted by a post-period term; and the third adds a further set of covariate interactions by VEP and VRA status. The third column stores results from estimating over the main specification.

3.7.2 List of County-Level Voter Education Project Sites

Table 3.A9: Raw Treatment Data: Counties with a VEP Mobilization Project, May 1962-1964

County (ICPSRFIP)	Big Five?	Total Mobilization Projects
1015	1	2
1047	1	2
1055	1	1
1073	0	1
1089	0	2
1101	1	3
1125	1	1
5069	0	1
5077	0	1
5119	0	2
12001	1	1
12005	1	1
12011	1	1
12023	1	1
12031	1	1
12033	1	1
12057	1	1

12073	1	2
12095	1	1
12103	1	1
12105	1	1
12109	1	1
12127	1	1
13007	1	1
13021	1	3
13029	0	1
13031	0	1
13037	1	1
13051	0	1
13063	0	1
13089	1	1
13095	1	2
13099	1	1
13107	0	1
13115	1	1
13121	1	4
13133	1	1
13139	1	1
13177	1	2

13179	1	2
13197	1	1
13205	1	1
13211	1	1
13215	1	1
13225	1	2
13243	1	1
13245	1	1
13247	1	1
13251	0	1
13255	1	1
13261	1	2
13267	0	1
13271	1	1
13273	1	2
13299	1	2
13303	1	1
13317	1	1
13321	1	1
22013	1	2
22017	1	3
22019	1	2

22027	1	1
22031	1	1
22033	1	4
22035	1	1
22037	1	1
22047	1	1
22049	1	1
22055	0	1
22061	1	1
22065	1	1
22067	1	1
22071	0	1
22073	1	2
22077	1	1
22083	1	1
22091	1	1
22105	1	1
22107	1	1
22117	1	1
22119	1	1
22121	1	1
22125	1	1

28011	0	1
28027	0	1
28035	1	1
28047	1	1
28049	1	5
28059	1	1
28067	1	1
28083	0	1
28093	1	2
28107	1	1
28133	0	1
28135	1	1
28149	0	1
28151	0	1
37015	1	1
37021	0	1
37041	1	3
37051	1	1
37063	0	2
37065	1	2
37067	1	2
37081	1	2

37083	0	1
37091	1	1
37119	1	2
37127	1	2
37129	1	2
37131	1	1
37139	1	2
37183	0	1
37185	0	1
37195	0	1
45013	1	1
45019	1	2
45027	1	2
45031	1	1
45033	1	1
45039	1	1
45041	1	1
45043	1	1
45045	1	1
45051	1	1
45061	1	1
45067	1	1

45069	1	1
45075	1	2
45079	1	3
45083	1	1
45085	1	2
45089	1	1
45091	1	2
47037	0	3
47047	1	2
47065	1	3
47069	1	2
47075	1	2
47093	1	3
47097	1	1
47113	0	1
47125	1	2
47157	1	2
47167	1	1
48037	1	1
48423	1	1
48439	1	1
51007	1	1

51011	1	1
51013	1	1
51025	1	1
51029	1	1
51049	1	1
51053	1	1
51081	1	1
51093	1	1
51111	1	1
51117	1	1
51135	1	1
51145	1	1
51147	1	1
51149	1	1
51175	1	1
51181	1	1
51183	1	1
51630	1	1
51670	1	1
51680	1	2
51730	1	2
51740	1	2

51760	1	6
51770	1	1
51800	1	2
Total	.8390805	1.396552

This table displays raw data on county-level VEP mobilization projects spanning May 1962 through 1964. Counties are identified by ICPSRFIP codes in the first column. The second column is a binary variable for whether at least one project was operated by a "Big Five" organization (NAACP, SNCC, SCLC, CORE, and NUL). The third column provides counts for mobilization projects in the temporal window. The final row yields averages for "Big Five" organization and total projects.

3.7.3 Registration Data and Results

One the shortcomings of this paper is the inability to observe direct registration gains generated by VEP mobilization projects. This data restriction also limits Cascio and Washington (2013) and Bernini et al. (2023b). I follow the empirical approach of these papers in the main text by using log turnout as the dependent variable and identifying on the slope of percent black at the county level. In this appendix section, I scrape a series of primary source materials to obtain registration rates by race around the timing of the 1960 and 1968 presidential elections (USCCR, 1959, 1961, 1968). See table 3.1 in the main text for summary statistics on registration rates. Using these rates as dependent variables, I estimate over an altered specification that no longer identifies on the slope of percent black.

Figure 3.A6 displays differences between 1960 and 1968 registration rates by race for all states with available county-level data. Counties in green (orange) experienced net gains (losses), where color intensity corresponds to the magnitude of gains (losses). Apart from a few cases in the Peripheral South, most counties experienced at least marginal gains in Non-White registration. In particular, registration rates increased considerably throughout Mississippi for this voting demographic. In contrast, White registration increased in some counties and decreased in others over the two presidential election cycles. Georgia and North Carolina saw registration declines, while the remaining states in the sample mostly experienced gains.

I update the main specification using registration rates by race as the dependent variable. Since racial breakdowns are available, my parameters of interest are simple $Post_t$ by type (VEP, VRA, VEP-VRA) interaction terms. I continue controlling for differential treatment effects by percent black, and include the covariates listed in the main text. The following is the updated specification with registration:

$$ln(R_{ct}^{race}) = \lambda Post_t + \sum_g \delta_g Post_t \times Type\{g\}_c + \gamma^{'}\mathbf{X}_c + \mu_{s(c)t} + \sigma_c + \epsilon_{ct}, \tag{3.A1}$$

where $ln(R_{ct}^{race})$ is the log registration rate by race in county c and election year t. The specification is otherwise identical to equation (3.2) in the main text apart from post-period and post-period interaction terms not including percent black: $\lambda Post_t + \sum_g \delta_g Post_t \times Type\{g\}_c$.

Table 3.A10 displays results from estimating equation (3.A1). Each supercolumn corresponds to Non-White or White, where columns vary by covariate terms and control groups. Preferred estimates for Non-White registration are stored in columns (3) and (4). Using the baseline

control set, VEP projects correspond to a 201% increase in registration. Restricting comparisons to future VEP project counties only amplifies this return (269%). Surprisingly, there appears to be a registration decline in only VRA counties, but this effect vanishes using the future VEP control set. Moreover, there is seemingly no interaction effect between mobilization and electoral oversight. Standard errors are too large to attain statistical significance throughout. White registration appears unaffected by VEP mobilization. Future work should aim to develop a balanced panel (1952-1980) of registration data by race to improve this empirical analysis.

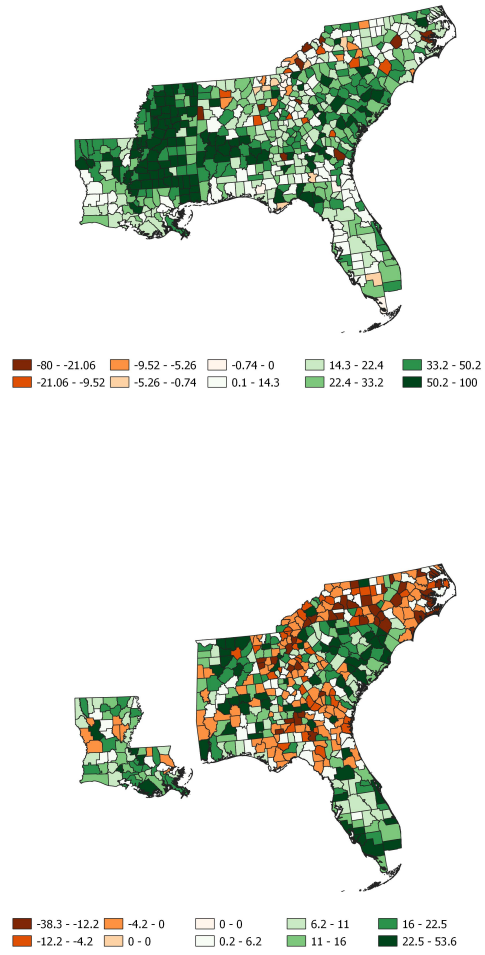


Figure 3.A6: Difference in Voter Registration Rates by Race: Non-White (Top) and White (Bottom)

The top figure displays differences in Non-White registration rates between 1960 and 1968. Green (Orange) counties experience net gains (losses) in turnout, where color intensity corresponds to the magnitudes of gains (losses). The bottom figure plots the same differences, only for White registration.

Table 3.A10: The Impact of the VEP and VRA by Percent Black on Log Registration by Race

	Non-White				White			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post $\cdot VEP$	0.3587	0.4695	2.0119	2.6886	-0.0078	-0.0302	-0.4238	-0.2586
	(0.2629)	(0.6438)	(2.4080)	(3.6145)	(0.1307)	(0.1566)	(0.3716)	(0.3562)
Post $\cdot VRA$	-0.5570**	-0.4448	-2.6954	0.7754	-0.1032**	-0.0351	0.4267*	0.2234
	(0.1806)	(0.4753)	(2.6678)	(1.9343)	(0.0264)	(0.1937)	(0.1929)	(0.6518)
Post $\cdot VEP \cdot VRA$	-0.2594	-0.4744	0.0914	-0.1627	-0.0195	-0.0717	0.0492	-0.3634
	(0.2825)	(0.7281)	(0.3822)	(0.9971)	(0.0779)	(0.2166)	(0.1812)	(0.2654)
Post $\cdot Black$	0.0134***	0.0054	0.0171***	-0.0187	-0.0015	0.0030*	-0.0026***	-0.0018
	(0.0031)	(0.0102)	(0.0031)	(0.0193)	(0.0012)	(0.0012)	(0.0003)	(0.0062)
Post $\cdot Black \cdot VEP$	-0.0115	-0.0115	0.0008	0.0318	0.0003	-0.0018	0.0044	-0.0039
	(0.0076)	(0.0175)	(0.0109)	(0.0211)	(0.0032)	(0.0035)	(0.0051)	(0.0085)
Post $\cdot Black \cdot VRA$	0.0094	0.0123	0.0045	0.0316	0.0016	-0.0027	0.0028***	-0.0003
	(0.0055)	(0.0118)	(0.0055)	(0.0221)	(0.0013)	(0.0051)	(0.0007)	(0.0055)
Post $\cdot Black \cdot VEP \cdot VRA$	0.0098	0.0120	0.0058	-0.0128	0.0016	0.0058	0.0012	0.0141*
	(0.0089)	(0.0163)	(0.0084)	(0.0259)	(0.0020)	(0.0055)	(0.0036)	(0.0064)
Control Set	All	Future VEP	All	Future VEP	All	Future VEP	All	Future VEP
County FEs	X	X	X	X	X	X	X	X
State-Year FEs	X	X	X	X	X	X	X	X
Covs	X	X	X	X	X	X	X	X
Covs-VEP			X	X			X	X
Covs-VRA			X	X			X	X
Mean	-1.236	-1.373	-1.236	-1.373	-0.159	-0.215	-0.159	-0.215
SD	1.209	1.265	1.209	1.265	0.205	0.233	0.205	0.233
Obs.	966	406	966	406	864	358	864	358

This table displays regression output from estimating different variants of equation (3.2). The first supercolumn uses Non-White registration rates; the second White registration rates. Within each supercolumn, odd columns use the baseline control set, while even columns the alternative future VEP set. The first two columns control for county and state-by-year fixed effects and a set of covariates referenced in the main text interacted by a post-period term. The remaining two columns further control for the set of covariate interactions by VEP and VRA status.

Chapter 4: Understanding Policing in the Aftermath of Gun Violence: Examining Investigatory Stops and Crime in Chicago

"And I don't want to suggest that we're ever going to eliminate all misunderstanding or stereotypes between police officers and minority communities. It's certainly not going to happen overnight. And it's especially tough because there's more crime in these communities, which means that the police are interacting with them more than they are in some fancy neighborhoods."

— President Barack Obama, 122nd Annual IACP Conference (October 27, 2015)

[*NOTE: This is the preprint version of Journal of Public Economics 234 (2024): 105117. See LeRoy (2024) for the published version.*]

4.1 Introduction

In the United States, gun violence claims the lives of tens of thousands of victims each year: in 2021, nearly 21,000 Americans were victims of fatal gun violence (Gramlich, 2023). Estimates suggest that an additional 36,000 victims survive assault incidents involving firearms annually (EFSGV, 2020). Despite this alarming magnitude, the role police officers play in the aftermath of gun violence to improve neighborhood safety has surprisingly received little

attention. This omission is crucial given that the distributional consequences of gun violence are disproportionately borne by historically marginalized neighborhoods: one-half of the nation's fatal gun victims reside in the most impoverished, racially segregated neighborhoods spanning just 127 cities (Edmund, 2022).

In theory, a spike in gun victimizations could trigger a rise in other, potentially dangerous, crime incidents. Locally-defined shocks to gun violence could also serve as a signal of the unknown, true distribution of deadly weapons and/or contraband throughout cities. For these reasons, officers might respond to locally-defined gun violence by not only increasing their presence in these neighborhoods to deter crime, but by also conducting more investigatory stops to confiscate weapons and/or contraband in an effort to limit future gun violence. In this case, increased levels of proactive policing would benefit neighborhoods regularly afflicted by gun violence. On the other hand, responding to gun violence in this fashion need not produce societal benefits. If crime is unaffected by an increased police presence, and conducting additional investigatory stops fails to produce weapons and/or contraband, then this response seemingly offers little societal gain. In fact, in this case, conducting additional investigatory stops may even generate societal losses: more police-civilian interactions could increase petty arrest and misdemeanor rates, which could be interpreted as a cost incurred by residents of some of the most vulnerable neighborhoods in the country.

In this paper, I aim to shed light on the aforementioned, and seemingly ambiguous, welfare implications surrounding this form of policing in the wake of gun violence. I restrict attention to the universe of all gun violence, fatal or non-fatal, over roughly a six-year window in Chicago. The unit of analysis is police beats, or spatially-defined units consisting of officers in the Chicago Police Department. I estimate police responsiveness in stops to gun violence by employing

a staggered difference-in-differences design that restricts attention to pairwise comparisons of contiguous police beats. Within each pair, the timing of treatment is the instance where the greatest relative difference in gun victimizations between police beats is realized: the beat with more gun victimizations is assigned the binary treatment. By restricting attention to pairwise comparisons, I reduce potential bias from comparing beats that are non-randomly different in ways that are correlated with the evolution of stops. Moreover, by leveraging the largest relative differences in gun victimizations, I circumvent a number of identification issues associated with using a continuous measure of gun victimizations to capture treatment intensity. This empirical design draws from a collection of papers belonging to different literatures (Boone et al., 2021; Cantoni, 2020; Dube et al., 2010, 2016; East et al., 2023; Huang, 2008; Spenkuch and Toniatti, 2018).

I find that police officers indeed conduct more stops in beats with more gun victimizations: in weeks with the largest pairwise differences in gun victimizations, officers conduct roughly an additional 1.6 stops in treated beats relative to control beats. Importantly, 0.2 of the 1.6 additional stops yield weapons and/or contraband, meaning that police officers are in fact productive in seizing illicit goods. Evidence suggests that the additional stops may also have a crime reductive effect: at the time of treatment, there are 0.9 fewer index (violent and property) crime incidents. In contrast to these short-run results, the long run tells a different story. Over the next month, police officers continue to make additional investigatory stops, but these stops neither yield weapons and/or contraband nor reduce crime incidents. Beyond one month, there is no statistically significant increase in total stops, but stops conditional on arrest or the issuance of misdemeanor citations remain significant as far as three months following "large" shocks to gun victimizations.

A breakdown of stops conditional on race reveals that Non-Whites account for 89% of the

initial baseline increase in investigatory stops. Non-white stops more or less continue to account for this share over the next month despite not yielding weapons and/or contraband. In contrast, over this time, additional White stops conditional on yielding illicit goods are statistically positive at the 10% level. On average, only Hispanics continue to be stopped more in treated beats relative to control beats two months post treatment. For these reasons, I argue that Non-Whites bear the largest distributional consequences from this form of policing.

Finally, I examine police responsiveness to gun violence restricting attention to pairs of police beats by gang concentration. Gang concentration is calculated by dividing gang area over total area, where the former area is measured by digitizing the territorial boundaries of the most prevalent gangs in the city using a map generated by the Chicago Police Department. For analysis, I restrict attention to pairs of beats sitting below and above the 25% gang concentration threshold: roughly two-thirds of pairs belong to the former group. I find that pairs with larger gang concentrations experience 3.4 times more investigatory stops at the time of treatment, but that these stops prove ineffective in yielding weapons and/or contraband and fail to limit total crime incidents. Meanwhile, pairs with smaller gang concentrations experience a rise in stops (10% significant) that produces illicit goods (10% significant) and is associated with a decline in total crime incidents. Following the initial week of treatment, officers seemingly have no impact on crime, and fail to seize weapons and/or contraband, irrespective of gang concentration breakdown.

I interpret these results as evidence that responding to gun violence in the short run by conducting investigatory stops can be an effective way to limit crime, seize deadly weapons, and remove contraband from vulnerable neighborhoods. Heterogeneity exercises suggest that the short-run benefits from increased proactive policing are driven by regions in Chicago without

gang hot-spots. The long-run results call into question if this form of policing confers societal benefits beyond the initial week of treatment. In fact, the persistent enforcement action three months post treatment could even be interpreted as a societal cost given that these stops do not yield weapons and/or contraband, and appear ineffective in reducing total crime incidents. In other words, in the absence of discovering illicit goods or curtailing crime, persistent enforcement action could merely reflect an "overpolicing" of petty offenses. This possibility is especially alarming due to Non-Whites, particularly Hispanics, overwhelmingly accounting for the persistence in the treatment effect. Nonetheless, I cannot completely rule out the prospect that conducting additional stops in the long run provides some gain to society: the counterfactual, or out-of-equilibrium, level of total crime incidents absent police officers conducting stops is unobservable. Therefore, crime could conceivably increase without additional, direct police-civilian interactions.

While the relationship between crime and police presence has been thoroughly examined, relatively less is known about the relationship between gun violence and investigatory stops (Chalfin et al., 2022; Chalfin and McCrary, 2018; Cheng and Long, 2018; Di Tella and Schargrodsky, 2004; Draca et al., 2011; Evans and Owens, 2007; Kirchmaier et al., 2019; Klick and Tabarrok, 2005; Levitt, 1997, 2002; Mastrobuoni, 2019; Mello, 2019; Vollaard and Hamed, 2012; Weisburd, 2021; Weisburst, 2018). In general, most papers examining the latter relationship restrict attention to isolated incidents of police brutality that garner substantial media scrutiny, protest, and reform (Cheng and Long, 2022; Devi and Fryer Jr, 2020; Morgan and Pally, 2016; Shi, 2009; Shjarback et al., 2017).[1] These papers identify a decline in discretionary policing activities, i.e."de-policing", in the aftermath of police brutality. In contrast, I restrict attention to civilian-perpetrated gun

[1]Some papers included in this literature examine the more general relationship between gun violence and proactive policing, which can include investigatory stops, but also other "self-initiated police activities" (Devi and Fryer Jr, 2020). This literature is not limited to gun violence, but rather includes many other forms of police brutality.

victimizations, which often fail to receive the same level of media salience, but are much more common in major cities. Importantly, these victimizations actually induce the opposite response from police officers: as earlier mentioned, officers conduct more stops in beats up to one month post treatment. To my knowledge, this paper is the first to identify this phenomenon, along with the aforementioned short-run benefits, potential long-run costs, and distributional consequences by race.

This paper will proceed as follows: section 4.2 offers an institutional background on the Chicago Police Department and formally introduces the empirical design for estimation; section 4.3 highlights the data sources, analyzes aggregate trends in stops and gun violence, and displays summary statistics; section 4.4 includes the main specification, baseline estimates, robustness exercises, and heterogeneity exercises by race and gang concentration; and section 4.5 concludes.

4.2 Institutional Background & Empirical Design

4.2.1 Institutional Background

Chicago is home to the second largest police department in the country, with just shy of 12,000 full-time active officers and a 2020 budget of $1.76 billion (Kershner, 2020). The department consists of a Special Functions Division and Patrol Bureau Areas that serve approximately 2.75 million people spanning 228 square miles (Bureau, 2021). The Special Functions Division is divided into a number of sections (Airport Law Enforcement, Public Transportation, and Traffic) and units (Mounted, SWAT, Marine/Helicopter, Troubled Building, and Canine) that address specific, citywide concerns. There are three patrol bureau areas (North, South, and Central), which are further broken down into 22 police districts, and 277 police beats. Police districts are

comprised of police beats with watches, tactical teams, and community relations offices (G01-02-03).[2] For the purposes of this paper, I am only interested in the Patrol Bureau.[3] Figure 4.A1 is a visual breakdown of the Patrol Bureau.

Districts in the Chicago Police Department are under the supervision of district commanders. Commanders are in charge of assigned department employees, and responsible for curtailing crime and disorder within their district boundaries (S03-03-01). Apart from district assignments, officers are also given beat assignments, which typically correspond to spatially-defined units represented in figure 4.A2. Usually, beats are not in excess of one-square mile (Ba et al., 2021a). While officers are expected to operate within their assigned beats, some circumstances may call for them to cross beat boundaries (Tendian, NA). For instance, 911 call dispatchers may assign officers to gun calls in different beats. In rare circumstances, officers may even assist with calls in different districts (G03-01-01).

4.2.2 Empirical Design

One problem with estimating police responsiveness in investigatory stops to gun violence is that police beats across Chicago are undoubtedly different across various dimensions, such as crime rates, in non-random ways. Therefore, comparing the number of stops in beats with many shootings relative to the number in all remaining beats with few shootings would likely violate the parallel trends assumption: the underlying differences across beats that generate differential trends in stops over time would bias the estimates. I restrict variation to pairs of contiguous beats for estimation to circumvent this identification concern. In theory, neighboring beats should be

[2]Parenthetical citations of this form correspond to Directives of the Chicago Police Department. They can be accessed here: `http://directives.chicagopolice.org/?msclkid=b8dd8d99cf6c11ec9a38b66ea64b835c`.

[3]In other words, stops made by officers in the Special Functions Division are not considered.

"similar" across determinants of investigatory stops.

Empirically, identifying on pairwise variation in gun violence is in the spirit of Dube et al. (2010), among others (Boone et al., 2021; Cantoni, 2020; Dube et al., 2010, 2016; Huang, 2008; Spenkuch and Toniatti, 2018). Dube et al. (2010) aim to determine how minimum wage impacts earnings and employment. By estimating on pairwise variation across contiguous counties, they draw comparisons across counties with roughly the same socio-demographic features. In a similar vein, I use neighboring police beats as control units for clean estimation.[4]

An additional concern for clean estimation is that police beats frequently experience gun violence, and in some cases, beats experience multiple gun victimizations in a given week. This data feature poses a couple of identification problems. If I assign treatment status to police beats with at least one gun victimization, then there could be instances where both police beats in a pair are "treated". Beats could also be assigned the treatment on multiple occasions across the nearly six-year temporal window of interest (first week of 2016 to the 39th week of 2021). Instead, I define a binary treatment where the timing of treatment corresponds to the calendar week denoting the largest difference in gun victimizations between contiguous police beats. Within each pair, the police beat with the greater (lower) number of gun victimizations is assigned the treatment (control). Therefore, this paper leverages the set of largest pairwise differences in gun victimizations.

This treatment construction is in the spirit of East et al. (2023), who exploit state-level

[4]I differ from this literature by not leveraging pairwise variation to exploit policy differences, and by extension, differences in treatment intensities across police beats. In contrast, Dube et al. (2010) restrict attention to not just any pair of contiguous counties, but specifically counties belonging to different states. As a result, they exploit differences in the timing of state-level minimum wage hikes for estimation. This feature does not apply to the current paper: in other words, I restrict attention to pairwise variation solely for the purpose of identifying clean control beats. The localized nature of policing and crime is apt for drawing comparisons across neighboring police beats.

"discrete jumps" in prenatal Medicaid eligibility to identify the effect on multigenerational health outcomes. In their set-up, states with "discrete jumps" are assigned a binary treatment, while states with level trends in eligibility serve as controls. Much like how each state has some level of Medicaid eligibility, virtually every police beat in my sample has gun violence at some point in time. Hence, I mimic East et al. (2023) by identifying on the largest pairwise differences in gun victimizations.

Figure 4.1 displays an example of the pairwise comparisons used for estimation. District boundaries are in bold, and beats are labeled by their corresponding department numbers. For ease of exposition, restrict attention to a single pair of beats, numbered 215 and 224, highlighted in grey. For this pair, the largest difference in gun victimizations is realized the 33rd week of 2021: beat 224 experiences five gun victimizations, while beat 215 does not experience any gun violence. As a result, I assign treatment (control) status to beat 224 (215) at this time. This procedure more generally applies to all sample pairs with the following exceptions: I discard pairs without any differences in gun victimizations at the weekly-level; and in cases where the largest difference is achieved multiple times over the sample window, I use the first instance as the timing of treatment. There are implications surrounding the number of pairs containing a given beat. Following Dube et al. (2010), if a beat belongs to "P" pairs, then it will appear "PT" times in the data, where "T" is the number of relative weeks in the data. In other words, the data consists of a balanced panel of all beat-pair combinations plus or minus 12 relative weeks around the timing of treatment.[5] One problem with this approach is that it induces correlation in the errors: pairwise variation will be correlated across pairs since most beats appear in more than

[5]Strictly speaking, the data is balanced for beat-pair combinations with a full set of leads and lags. I also include beat-pairs that do not meet this criteria.

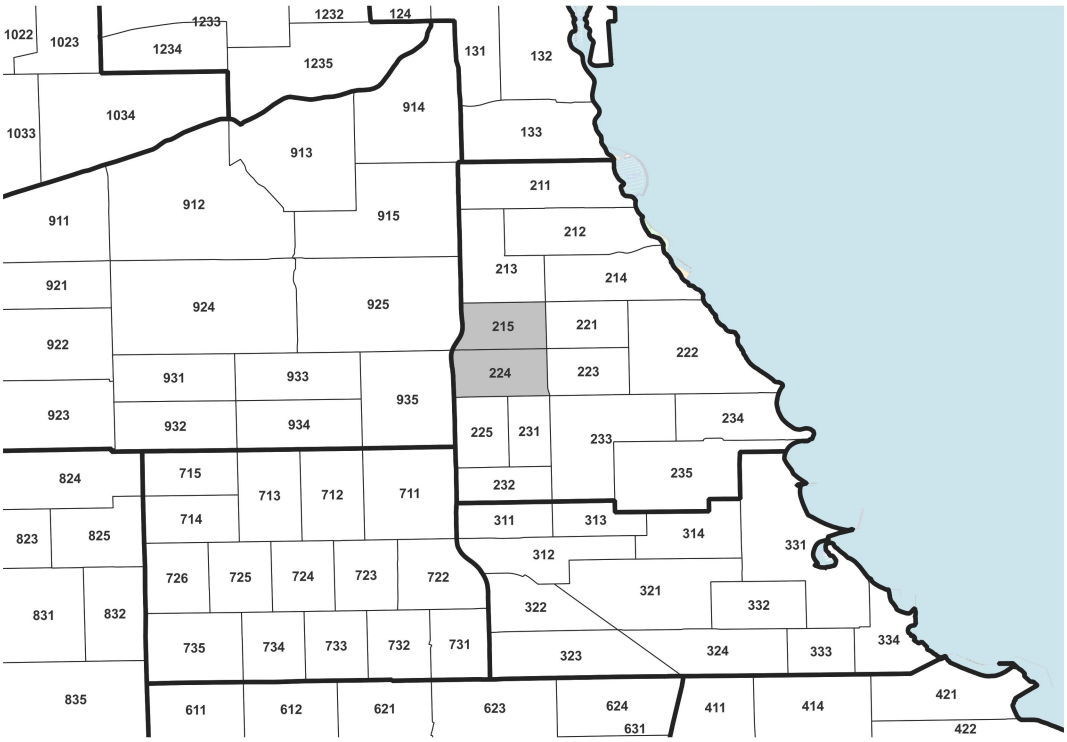


Figure 4.1: Illustration of Empirical Design: Beats 215 and 224

This figure illustrates a single pair of contiguous beats, numbered 215 and 224, highlighted in grey to demonstrate the type of pairwise comparisons used for estimation. Beat (District) boundaries are light (bold). In the data, I locate the calendar week when the largest pairwise difference in gun victimizations is first realized. For this pair, the largest difference occurs during the 33rd week of 2021: beat 224 (215) experiences five (zero) gun victimizations. As a result, this week denotes the initial assignment of treatment to beat 224, while beat 215 serves as a control unit.

one pair. In the spirit of Dube et al. (2010), I two-way cluster at the pair and district-year levels to resolve this issue (Cameron et al., 2011).[6]

Figure 4.A3 displays the distribution of maximum pairwise differences in gun victimizations leveraged for estimation. The x-axis measures the size of the shocks, the leftward y-axis the density for each maximum pairwise difference, and the rightward y-axis the cumulative density. The middle 50% of shocks have maximum pairwise differences between three and six gun victimizations. The distribution is skewed rightward: the top 4% of shocks correspond to double-digit maximum pairwise differences. I regress gun victimizations on my treatment leads and lags, controlling for beat and pair-week fixed effects, to further examine this "first-stage" relationship. Figure 4.A4 displays these results. In relative week zero, the average treated beat experiences 5.1 more gun victimizations than its pairwise comparison beat. There is virtually no difference in gun victimizations between the average treated and control beats surrounding the timing of treatment, meaning that there are no correlated, and potentially confounding, shocks.

Figure 4.2 shows the raw means in stops 12 weeks pre and post the timing of treatment. The solid (dashed) trend line corresponds to the average treated (control) police beat. This "naive" analysis suggests a positive treatment effect: on average, treated beats experience 1.3 additional stops during the initial week of treatment while control beats are seemingly unaffected. This analysis also bolsters support for the empirical design. The relatively smooth trend line for control beats assuages any spillover concerns related to using neighboring beats as control units. Also, even though treated beats experience roughly 1.5 more stops relative to control beats prior to treatment, both treated and control trend lines are flat over this time. This result is consistent with

[6]Since there are only 22 police districts, there are not enough districts to cluster on an "uninteracted" district term: this number sits well below the "42" designation established in Angrist and Pischke (2008).

the parallel trends assumption.

4.3 Data Sources, Aggregate Trends, & Summary Statistics

4.3.1 Data Sources

I use Investigatory Stop Reports (ISR) from the Chicago Police Department to measure investigatory stops in the city.[7] In August 2015, the ACLU of Illinois and the Chicago Police Department reached an agreement to protect civilians' fourth amendment rights against unreasonable search and seizure. Not long after, the department began making stop data available to the public (ACLU, 2022). Each row of the raw data consists of an individual pedestrian or traffic stop.[8] The location of each stop is reported at various levels, such as police beats and districts, but also down to the street block-level. The timing of each stop is recorded down to the hour. Numerous data points capture the physical features of the people stopped, such as age, sex, race, height, weight, etc.. The data also identifies if officers conduct pat downs and/or searches, if weapons and/or contraband are discovered, and if stops result in arrest or the issuance of misdemeanor citations. All in all, Devi and Fryer Jr (2020) refer to the data set as " ... one of the most comprehensive data sets on contacts with civilians" (pg. 26).[9]

This paper employs the Chicago Data Portal's Special Access-Only Version of the Violence Reduction - Victims data set to measure gun victimizations.[10] The data include both fatal and non-

[7]The data can be accessed here: `https://home.chicagopolice.org/statistics-data/isr-data/`.

[8]I include both types of investigatory stops in the cleaned data set. While ACLU (2019) and Hausman and Kronick (2021) argue that the 2015 agreement between the ACLU and CPD generated a shift from pedestrian to traffic stops, this change in composition is internalized by the set of pair-week fixed effects in the main specification.

[9]An official investigatory stop report can be accessed here: `http://directives.chicagopolice.org/forms/CPD-11.910.pdf`. The department transfers information from these reports to CSV files for data analysis.

[10]Access to this data requires creating an account with the Chicago Data Portal.

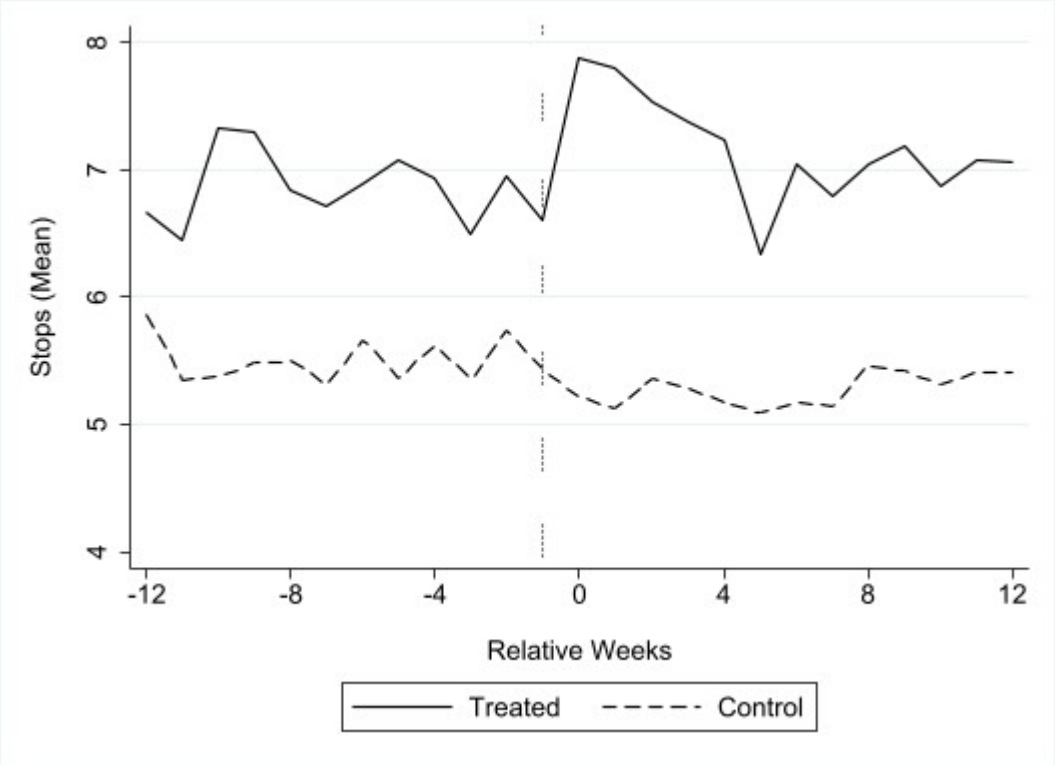


Figure 4.2: Raw Means in Stops by Treatment Status

This figure displays the raw means in stops by treatment status for the 12 relative weeks around the initial timing of treatment. The solid (dashed) trend line captures the evolution in mean stops for the average treated (control) police beat.

fatal gun victimizations, where each row of the raw data corresponds to an individual victimization. Similar to the stop data, locations of victimizations are reported down to the street block-level, but also include police beat and district. The timing of victimizations is recorded down to the minute of each day.

I gather crime data from the Chicago Data Portal to measure various breakdowns of crime incidents.[11] Crime incidents are reported at various spatial levels, such as police districts, police beats, and street-blocks. The data include Illinois Uniform Crime Reporting (IUCR) codes to distinguish between index and non-index crime.[12] Index crimes mostly consist of violent and property crimes, while non-index crimes are less severe in nature.[13] Importantly, I difference out gun victimizations from beat-week counts of total and index crime incidents to avoid capturing a mechanical effect: this paper aims to determine if police responsiveness to "large" shocks to gun victimizations leads to more confiscated illicit goods and fewer crime incidents; since a gun victimization itself is a crime incident, I restrict attention to all "other" types of crime.

Finally, I digitize a department generated "2018 - Gang Territorial Boundaries" map for the largest gangs in Chicago to calculate beat-level measures of gang concentration.[14] Beat-level measures of gang concentration capture the fraction of a beat's area denoted as gang territory.

[11]The data can be accessed here: `https://data.cityofchicago.org/Public-Safety/Crimes-2001-to-Present/ijzp-q8t2`.

[12]A dictionary of IUCR codes can be accessed here: `https://data.cityofchicago.org/Public-Safety/Chicago-Police-Department-Illinois-Uniform-Crime-R/c7ck-438e`.

[13]The crime data does not include homicides.

[14]I use data on gang membership from the Chicago Police Department (accessed via ProPublica) to identify the largest gangs in the city. The data can be accessed here: `https://www.propublica.org/datastore/dataset/chicago-police-clear-gang-data`. For digitization, I only consider gangs accounting for at least 3% of all gang members in the city. The following gangs meet this criteria: Gangster Disciples (29.3%), Latin Kings (10.4%), Black P. Stones (8.0%), Black Disciples (4.9%), Conservative Vice Lords (4.3%), and Travelling Vice Lords (4.1%). One notable omission is Four Corner Hustlers, which accounts for 6.9% of all gang members in Chicago, since I am unable to locate its territorial boundaries. See Table 4.A1 for a list of all Chicago-based gangs accounting for at least 1% of the city's gang members. The gang map used to identify territorial boundaries can be accessed here: `https://clearmap-chicagopd.hub.arcgis.com/`. While the department has released maps from other years ranging from 2007 to 2021, I use the 2018 map because it falls roughly in the middle of the sample window. This choice should be innocuous given that gang boundaries are relatively static from 2016 to 2021.

For sample construction, I use a police beat shapefile from the Chicago Data Portal to form pairs of contiguous beats. After collapsing patrol bureau stops, gun victimizations, crime, and gang concentrations to the beat-week-level, I merge with the file identifying the pairs. Next, I trim the data set to plus or minus 12 relative weeks around the timing of treatment, where calendar time ranges from the first week of 2016 to the 39th week of 2021.

4.3.2 Aggregate Trends

Figures 4.A5 and 4.A6 are heat maps of the aggregate number of investigatory stops and gun victimizations in the sample. There exists a clear, positive spatial relationship between these two variables. South and West Chicago are areas most prone to stops and gun violence, while both are less common in North Chicago. Figure 4.A7 captures citywide aggregate trends in investigatory stops and gun victimizations. In general, these trends positively covary over time.

4.3.3 Summary Statistics

Table 4.1 displays summary statistics for the main variables used for analysis. The unit of observation is the beat-week. The table consists of four panels: stops conditional on type, stops conditional on race, crime by type, and gun victimizations. On average, a given police beat experiences 5.9 investigatory stops per week: 2.1 of these stops involve pat downs; 1.2 search; 1.4 lead to arrests or misdemeanor citations; and 0.6 produce weapons and/or contraband. Non-Whites account for the overwhelming majority of stops: 4.1 of the 5.9 stops consist of Blacks; 1.2 Hispanics; and only 0.5 Whites. In terms of crime, there are 17.1 crime incidents in a given police beat per week: 6.9 of these incidents are index crimes. A typical beat experiences roughly

1.2 victimizations, fatal or non-fatal, per month. Finally, in terms of dispersion, columns (2)-(4) suggest that there is considerable variation across beat-weeks in stops, crime, and gun victimizations. This data feature is consistent with the spatial and temporal aggregate trends explored in the previous subsection (4.3.2).

4.4 Main Specification, Baseline Results, & Heterogeneity

4.4.1 Main Specification

For estimation, I employ a dynamic model with 12 leads and lags to identify how "large" shocks to gun victimizations impact the number of stops conducted by police officers in subsequent weeks. Formally, I estimate the following specification:

$$Y_{bpt} = \alpha + \sum_{r=-12, r\neq -1}^{12} \beta_r \mathbf{1}\{t - d_p^* = r\} + \gamma_b + \theta_{pt} + \nu_{bpt}, \tag{4.1}$$

where Y_{bpt} is the number of investigatory stops conducted in beat b, pair p, and week t; $\mathbf{1}\{t - d_p^* = r\}$ is a set of binary variables equal to one for treated beats when calendar week t is within r relative weeks of the largest pairwise difference in gun victimizations being realized (d_p^*); γ_b are beat fixed effects; θ_{pt} are pair-week fixed effects; and ν_{bpt} is the error term. My parameters of interest are the β_r terms scaling the binary variables $\mathbf{1}\{t - d_p^* = r\}$. These binary variables equal zero for control beats. I omit the first lead for comparative purposes. Without loss of generality, β_0 captures the average level increase in relative week zero, equally weighted across all pairs, in investigatory stops from "large" shocks to gun victimizations relative to the preceding week. I two-way cluster standard errors at the pair and district-year levels.[15]

[15] Subsequent regression specifications will build on this set-up. Note that substituting beat fixed effects (γ_b) for

Table 4.1: Summary Statistics - Sample Beat-Weeks

	Mean	SD	Min	Max
Panel A: Stops by Type				
Total	5.872	6.464	0	67
Pat Downs	2.050	2.896	0	36
Search	1.121	1.738	0	20
Enforce	1.379	1.970	0	20
Wep/Contra	0.593	1.123	0	12
Panel B: Stops by Race				
Black	4.095	5.857	0	67
Hispanic	1.245	2.652	0	40
Non-White	5.409	6.303	0	67
White	0.463	0.996	0	12
Panel C: Crime by Type				
Total	17.070	8.184	0	171
Index	6.865	4.261	0	136
Non-Index	10.198	5.762	0	70
Panel D: Gun Victimizations				
Total	0.311	0.844	0	16

This table displays summary statistics for beat-week observations in the data. The table consists of four panels: stops by type, stops by race, crime by type, and gun victimizations. Rows within the first panel correspond to different types of stops conditional on officer procedure or certain outcomes: the first row is total investigatory stops; the second stops conditional on pat downs; the third search; the fourth arrest or the issuance of misdemeanor citations; and the fifth seizing weapons and/or contraband. Rows within the second panel correspond to stops conditional on the race of the person stopped: the first row is stops conditional on being Black; the second Hispanic; the third Non-White; and the fourth White. Rows within the third panel correspond to different types of crime incidents: the first row is total crime incidents; the second index crime incidents; and the third non-index crime incidents. Finally, the fourth panel consists of a single row corresponding to gun victimizations, fatal or non-fatal. The table consists of four columns, each corresponding to a measure of central tendency or dispersion.

4.4.2 Baseline Results

I estimate equation (4.1) where Y_{bpt} measures total investigatory stops. Figure 4.3 displays the event study dynamics. Since all 11 pre-trends are statistically zero, I fail to reject the parallel trends assumption. Clearly, police officers conduct more stops in beats that experience "large" shocks to gun victimizations: at the initial time of treatment, there are 1.6 additional investigatory stops in these beats. One week post treatment, this response from officers more or less persists in magnitude, before steadily declining to roughly one additional investigatory stop over the next few weeks. Beyond one month, the treatment effect returns to pre-shock levels. Column (1) of table 4.2 aggregates these lead and lag effects into bins around the timing of treatment.

I conduct robustness exercises that reaffirm the positive baseline effect in total stops. As earlier mentioned, there is considerable heterogeneity in the size of gun victimization shocks across pairs, particularly in the the upper-tail of the distribution. Figure 4.A8 shows that the treatment effect dynamics are roughly the same even after trimming the sample at either tail of the distribution. The lone exception is in relative week zero, where the baseline rise in total stops is dampened after discarding pairs with maximum pairwise differences in excess of eight. Nonetheless, there is still an average increase of 0.9 additional stops for treated beats in this sub-sample that is statistically significant at the 10% level.

The empirical design permits beats to receive the treatment in some pairs and serve as controls in other pairs. I show that this data feature is not driving the baseline results by re-estimating over a subset of the data where this is no longer the case. In the main data set, beats belong to multiple contiguous pairs for estimation. Now, for each beat, I restrict attention to

pair-beat fixed effects (γ_{pb}) yields similar baseline results.

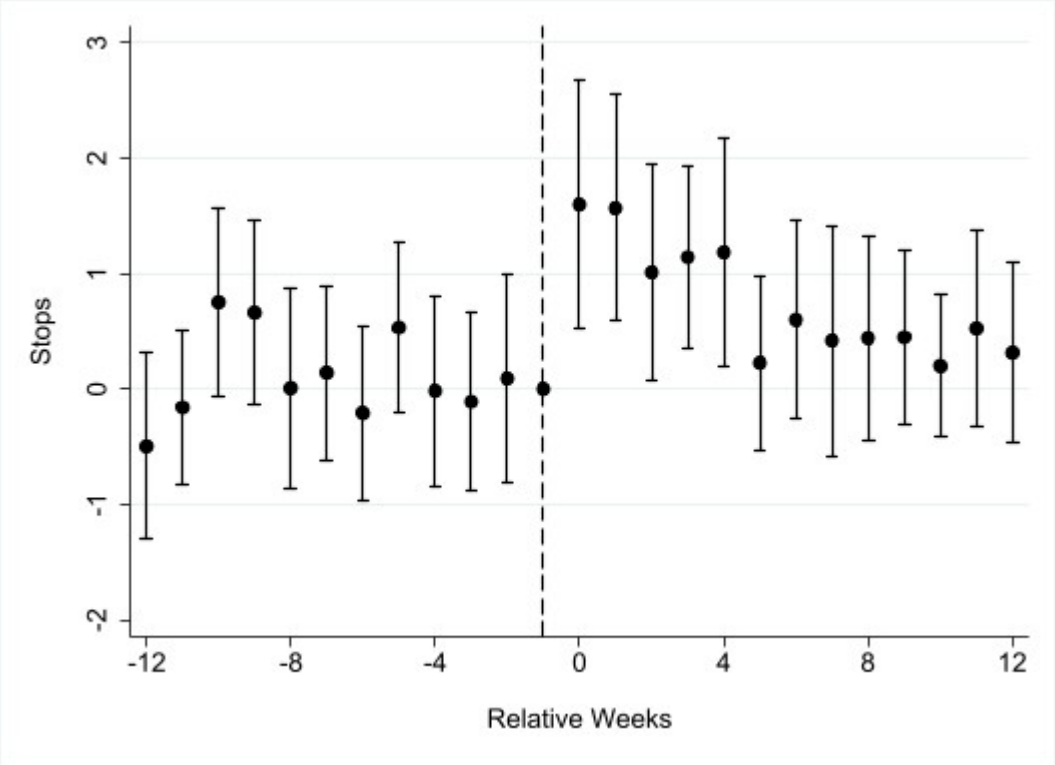


Figure 4.3: Responsiveness in Total Stops to Gun Violence

This figure displays the dynamic treatment effects from estimating equation (4.1), where Y_{bpt} measures total investigatory stops. I omit the first lead for comparative purposes, and denote this reference point with a dashed vertical line.

the pair that yields the "largest" shock to gun victimizations.[16] The remaining subset consists of beats that are only assigned the treatment or only serve as controls. Figure 4.A9 displays the results from re-estimating over this subset. These results satisfy the parallel trends assumption and exhibit similar treatment effect dynamics over time.

While police officers conduct additional investigatory stops in beats up to one month post treatment, it remains unknown if this response to gun violence is in vein: do these additional stops yield weapons and/or contraband, and are they associated with fewer crime incidents? I explore this question in detail below.

I re-estimate equation (4.1) replacing total investigatory stops with stops conditional on pat downs, search, enforcement action, or the discovery of weapons and/or contraband as the dependent variables (Y_{bpt}). Columns (2)-(5) of table 4.2 display aggregated lead and lag estimates for each type of stops. In the immediate week of treatment, I find that the additional stops conducted by police officers recover illicit goods: 0.2 of the 1.6 additional stops produce weapons and/or contraband. I also re-estimate equation (4.1) replacing total investigatory stops with total crime incidents, index crime incidents, or non-index crime incidents as the dependent variables (Y_{bpt}). Columns (6)-(8) of the same table displays aggregated lead and lag estimates for each type of crime incidents. In relative week zero, there are 0.9 fewer index crime incidents in treated beats compared to pre-shock levels. In theory, the decline in index crime could merely reflect officers displacing violent and property crimes from treated to control beats. In figure 4.A10, I plot the raw means of index crime incidents by treatment status to rule out this possibility. The decline in index crime incidents is captured by treated beats, while control beats exhibit little variation in incidents at the time of treatment. I interpret this finding as suggestive evidence that

[16] Ties are broken by using the earliest shock.

higher

Table 4.2: Responsiveness in Stops and Crime to Gun Violence

	Stops by Type					Crime by Type		
	Total	Pat Downs	Search	Enforce	Wep/Contra	Total	Index	Non-Index
Lead_12-Lead_9	0.762	0.045	-0.307	0.184	-0.244	-0.624	-0.311	-0.294
	(1.081)	(0.456)	(0.247)	(0.404)	(0.266)	(1.206)	(0.739)	(0.778)
Lead_8-Lead_5	0.477	0.255	-0.065	0.530	0.004	0.208	0.406	-0.193
	(0.948)	(0.450)	(0.303)	(0.394)	(0.208)	(0.985)	(0.639)	(0.777)
Lead_4-Lead_2	-0.033	-0.021	0.028	0.397	0.137	1.038	-0.035	1.084*
	(0.868)	(0.390)	(0.321)	(0.275)	(0.172)	(0.770)	(0.565)	(0.616)
Lag_0	1.598***	0.681***	0.583***	0.714***	0.227**	-0.710	-0.911**	0.378
	(0.543)	(0.260)	(0.182)	(0.237)	(0.114)	(0.549)	(0.401)	(0.354)
Lag_1-Lag_4	4.908***	0.885*	0.850**	1.275***	0.241	0.842	0.409	0.453
	(1.328)	(0.526)	(0.330)	(0.405)	(0.192)	(1.031)	(0.573)	(0.864)
Lag_5-Lag_8	1.694	-0.110	0.181	0.946**	0.197	0.923	0.157	0.799
	(1.277)	(0.561)	(0.369)	(0.437)	(0.224)	(1.103)	(0.694)	(0.851)
Lag_9-Lag_12	1.491	-0.365	0.527	0.870**	0.094	-0.794	-0.337	-0.439
	(0.967)	(0.406)	(0.338)	(0.383)	(0.216)	(0.939)	(0.587)	(0.739)
Beat FEs	X	X	X	X	X	X	X	X
Pair-Date FEs	X	X	X	X	X	X	X	X
Cluster	P-DY	P-DY	P-DY	P-DY	P-DY	P-DY	P-DY	P-DY
Obs	24,530	24,530	24,530	24,530	24,530	24,530	24,530	24,530

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This table displays aggregated lead and lag treatment effects from estimating equation (4.1) using various breakdowns of stops and crime as the dependent variables Y_{bpt}. The table consists of two supercolumns: stops by type and crime by type. The first supercolumn consists of five columns corresponding to different types of stops conditional on officer procedure or certain outcomes: the first is total investigatory stops; the second stops conditional on pat downs; the third search; the fourth arrest or the issuance of misdemeanor citations; and the fifth seizing weapons and/or contraband. The second supercolumn consists of three columns corresponding to different types of crime incidents: the first is total crime incidents; the second index crime incidents; and the third non-index crime incidents. For the immediate week of treatment, the treatment effect is given by row "Lag_0". The remaining rows group leads and lags into four week bins, with exception to row "Lead_4-Lead_2", since the first lead is omitted for comparative purposes.

rates of proactive policing, in this case conducting more investigatory stops, can have a crime reductive effect.[17]

Over the next month post treatment, police officers conduct 4.9 additional investigatory stops in treated beats, but these stops prove ineffective in yielding weapons and/or contraband, and seemingly have no impact on crime. The societal benefits conferred in the short run are

[17] See figures 4.A11 and 4.A12 for dynamic event study estimates for each type of stops and crime incidents.

evidently non-existent over this time. However, since the counterfactual crime rate absent any additional investigatory stops is unobservable, I cannot definitively rule out the possibility that these stops are beneficial: crime incidents could have otherwise increased in treated beats without more direct police intervention. Beyond a month, the effect on total stops is statistically indistinguishable from zero, but stops conditional on arrest or the issuance of misdemeanor citations remain statistically positive for as long as three months following "large" shocks to gun victimizations. Cumulatively, police officers arrest or issue misdemeanor citations to 3.8 additional people in the average treated beat three months post treatment. Since these stops do not recover weapons and/or contraband, and have no discernible impact on crime incidents, this finding suggests that petty offenses could be driving the persistence in enforcement action.

4.4.3 Heterogeneity by Race

This paper has yet to consider the distributional consequences of police responsiveness to gun victimizations along racial lines. This distinction is critical given that investigatory stops nationwide overwhelming involve police officers stopping Black people: according to the most comprehensive data set on stops, municipal officers' annual per-capita stop rate for Blacks is 43% greater than the corresponding measure for Whites (Pierson et al., 2020).[18] In general, the city of Chicago mirrors this national trend. While Chicago is nearly equally represented by Blacks, Hispanics, and Whites, 8.8 Blacks and 2.7 Hispanics are stopped for every White stopped (Bureau, 2021). Non-Whites account for 92.1% of total stops in the sample.

[18]The annual per-capita stop rate for Blacks (Whites) is 0.2 (0.14). Restricting attention to state agencies produces the same results: in this case, the annual per-capita stops rate for Blacks (Whites) is 0.1 (0.07), which still gives a measure for Blacks that is 43% greater than Whites. The data include roughly 95 million stops conducted between 2011-2018 by state patrol officers in 21 states and police officers in 35 cities. The data are made publicly available by The Stanford Open Policing Project (`https://openpolicing.stanford.edu/data/`).

I re-estimate equation (4.1) replacing total investigatory stops with stops conditional on race as the dependent variables (Y_{bpt}). Columns (1)-(4) of table 4.3 display aggregated lead and lag estimates for stops conditional on race. In the immediate week of treatment, Non-Whites account for 1.4 of the 1.6 additional investigatory stops in treated beats, where Blacks alone account for 1.1 of the stops. Responding to gun victimizations in this manner results in 6.0 more Black stops for every additional White stop, which is roughly in line with the summary statistics. To gauge the productiveness of stops along racial lines, I re-estimate equation (4.1) again, this time conditioning on both race and whether stops yield weapons and/or contraband. Columns (5)-(8) of the same table display the corresponding aggregated lead and lag estimates. There is suggestive evidence that both Non-White and White stops are effective in recovering weapons and/or contraband in relative week zero.[19]

One month post treatment, there are more stops conditional on each race, but only White stops continue to yield illicit goods at any reasonable level of significance. Only Hispanic stops remain significant into the second month post treatment: 1.5 more Hispanics are stopped in the average treated beat during this month. Holistically, I interpret these results as evidence that Non-Whites bear the largest distributional consequences from police officers conducting stops in the aftermath of gun victimizations.

4.4.4 Heterogeneity by Gang Concentration

Apart from racial heterogeneity, I also consider spatial variation in Chicago gang concentration given the intimate relationship between gang members and guns: past research has identified

[19] See figures 4.A13 and 4.A14 for dynamic event study estimates for stops conditional on race and stops conditional on both race and productiveness.

Table 4.3: Responsiveness in Stops to Gun Violence by Race

	Stops by Race				Wep/Contra Stops by Race			
	Black	Hispanic	Non-White	White	Black	Hispanic	Non-White	White
Lead_12-Lead_9	-0.257	1.021**	0.697	0.065	-0.230	-0.066	-0.308	0.064
	(0.868)	(0.487)	(1.057)	(0.221)	(0.232)	(0.084)	(0.266)	(0.041)
Lead_8-Lead_5	-0.142	0.389	0.349	0.128	0.057	-0.144	-0.040	0.044
	(0.791)	(0.441)	(0.909)	(0.194)	(0.161)	(0.091)	(0.202)	(0.038)
Lead_4-Lead_2	0.063	0.035	0.027	-0.060	0.117	0.005	0.124	0.013
	(0.782)	(0.283)	(0.847)	(0.141)	(0.150)	(0.069)	(0.167)	(0.030)
Lag_0	1.057**	0.394**	1.423***	0.175**	0.134	0.046	0.195*	0.032*
	(0.486)	(0.188)	(0.522)	(0.086)	(0.103)	(0.032)	(0.115)	(0.019)
Lag_1-Lag_4	2.260**	1.877***	4.296***	0.612***	0.051	0.105	0.157	0.084*
	(1.095)	(0.600)	(1.279)	(0.195)	(0.158)	(0.086)	(0.181)	(0.047)
Lag_5-Lag_8	0.006	1.499**	1.535	0.159	0.157	0.038	0.210	-0.012
	(1.011)	(0.615)	(1.249)	(0.167)	(0.177)	(0.107)	(0.220)	(0.036)
Lag_9-Lag_12	0.334	0.826*	1.140	0.351*	-0.017	0.058	0.056	0.038
	(0.782)	(0.449)	(0.911)	(0.211)	(0.193)	(0.102)	(0.219)	(0.036)
Beat FEs	X	X	X	X	X	X	X	X
Pair-Date FEs	X	X	X	X	X	X	X	X
Cluster	P-DY	P-DY	P-DY	P-DY	P-DY	P-DY	P-DY	P-DY
Obs 24,530	24,530	24,530	24,530	24,530	24,530	24,530	24,530	24,530

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This table displays aggregated lead and lag treatment effects from estimating equation (4.1) using various breakdowns of stops as the dependent variables Y_{bpt}. The table consists of two supercolumns: total stops by race and weapons and/or contraband stops by race. The first supercolumn consists of four columns corresponding to stops conditional on the race of the person stopped: the first is stops conditional on being Black; the second Hispanic; the third Non-White; and the fourth White. The second supercolumn consists of the same four columns, but instead restricts attention to stops conditional on yielding weapons and/or contraband. For the immediate week of treatment, the treatment effect is given by row "Lag_0". The remaining rows group leads and lags into four week bins, with exception to row "Lead_4-Lead_2", since the first lead is omitted for comparative purposes.

Chicago gang members as chief operators of illicit firearm markets (Lopez and Haskins, 2021). I aim to shed light on the following questions: (1) How much of the baseline police response to gun violence is driven by beats with substantial gang territory?; (2) Does the productiveness of these additional stops in confiscating weapons and/or contraband, or reducing crime incidents, vary by gang presence?; and (3) Do breakdowns by gang concentration lead to similar long-run findings relative to the baseline results?

As earlier mentioned in subsection 4.3.1, I set out to address these questions by digitizing a gang territorial map created by the Chicago Police Department: see figure 4.A15. I use this information to determine the fraction of each police beat's area consisting of gang territory, and then calculate averages at the pair-level. For example, consider the pair of police beats numbered 334 and 421. Roughly 24% of the former beat's area and 45% of the latter beat's area consist of gang territory, which yields a pair-level average of 34.5%. I re-estimate equation (4.1) using three different dependent variables: Y_{bpt} measures total investigatory stops, stops conditional on yielding weapons and/or contraband, or total crime incidents. For each dependent variable, I estimate over two sets of pairs: the first set consists of all pairs strictly below a 25% average gang concentration (approximately two-thirds of pairs), while the second consists of all remaining pairs in the sample. Table 4.4 displays aggregated lead and lag estimates for each set of pairs: columns (1)-(2) display the results for total investigatory stops; columns (3)-(4) stops conditional on recovering weapons and/or contraband; and columns (5)-(6) total crime incidents.[20]

The initial baseline increase in total investigatory stops is driven by police officers conducting more stops in gang-concentrated areas of the city. In pairs with at least a 25% average gang

[20]See figures 4.A16, 4.A17, and 4.A18 for dynamic event study estimates for total stops by average gang concentration, stops yielding weapons and/or contraband by average gang concentration, and total crime incidents by average gang concentration.

concentration, there are 2.8 additional stops at the time of treatment. Stops conditional on yielding weapons and/or contraband are statistically zero, and total crime incidents remain at pre-shock levels. In contrast, the remaining pairs experience only 0.8 additional stops immediately following "large" shocks to gun victimizations (10% significant). However, these stops produce weapons and/or contraband at the 10% level of significance, and are associated with 1.3 fewer total crime incidents. These findings indicate that regions with smaller gang concentrations account for the short-run gains in the baseline results.

One month post treatment, gang-concentrated pairs continue to disproportionately account for the baseline rise in total investigatory stops. These additional stops fail to produce illicit goods, and have no measurable impact on total crime incidents. In pairs with minimal gang presence, the immediate gains are no longer realized: seized weapons and/or contraband and total crime incidents are not statistically different than pre-shock levels. A breakdown by gang concentration gives reason to doubt that conducting additional stops beyond the immediate week of treatment confers societal benefits. This interpretation is consistent with the baseline results.

4.5 Conclusion

Each year, tens of thousands of Americans from the nation's most impoverished, racially segregated neighborhoods are victims of gun violence (Edmund, 2022; EFSGV, 2020; Gramlich, 2023). Yet, it remains largely unknown if police officers' response to gun violence improves neighborhood safety. This omission from the existing literature is critical given the seemingly ambiguous welfare implications tied to conducting additional investigatory stops in locally-defined regions with "large" shocks to gun victimizations.

Table 4.4: Responsiveness in Stops and Crime to Gun Violence by Gang Concentration

	Stops		Wep/Contra		Crime	
	$< 25\%$	$\geq 25\%$	$< 25\%$	$\geq 25\%$	$< 25\%$	$\geq 25\%$
Lead_12-Lead_9	1.934	-1.063	0.072	-0.924	-0.109	-2.490
	(1.261)	(2.154)	(0.260)	(0.557)	(1.437)	(2.151)
Lead_8-Lead_5	0.844	0.001	-0.028	-0.063	0.656	-1.490
	(1.228)	(1.665)	(0.226)	(0.396)	(1.312)	(1.726)
Lead_4-Lead_2	0.106	-0.130	-0.019	0.325	1.518*	-0.457
	(1.059)	(1.693)	(0.194)	(0.308)	(0.889)	(1.349)
Lag_0	0.840*	2.841***	0.180*	0.275	-1.296**	0.005
	(0.488)	(1.019)	(0.107)	(0.206)	(0.650)	(0.852)
Lag_1-Lag_4	4.022**	6.400***	0.155	0.285	1.023	-0.108
	(1.568)	(2.225)	(0.231)	(0.326)	(1.349)	(1.641)
Lag_5-Lag_8	2.788*	0.007	0.188	0.120	0.654	0.753
	(1.444)	(2.196)	(0.251)	(0.388)	(1.404)	(1.725)
Lag_9-Lag_12	2.158*	0.607	-0.115	0.357	-1.890*	0.127
	(1.211)	(1.538)	(0.249)	(0.384)	(1.034)	(1.751)
Beat FEs	X	X	X	X	X	X
Pair-Date FEs	X	X	X	X	X	X
Cluster	P-DY	P-DY	P-DY	P-DY	P-DY	P-DY
Obs	15,282	9,246	15,282	9,246	15,282	9,246

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

This table displays aggregated lead and lag treatment effects from estimating equation (4.1) using various breakdowns of stops and crime as the dependent variables Y_{bpt}. For each dependent variable, I restrict attention to subsets by average pairwise gang concentration. The table consists of three supercolumns: total investigatory stops, stops conditional on seizing weapons and/or contraband, and total crime incidents. Columns corresponds to a subset of data by average pairwise gang concentration: the first includes only pairs with less than a 25% average, and the second includes all remaining pairs. For the immediate week of treatment, the treatment effect is given by row "Lag_0". The remaining rows group leads and lags into four week bins, with exception to row "Lead_4-Lead_2", since the first lead is omitted for comparative purposes.

On the one hand, police officers responding to gun violence by conducting additional investigatory stops could be socially beneficial. In the event of positively correlated shocks to other types of crime, conducting additional stops could keep crime in check. If these stops are effective in seizing weapons and/or contraband, then they could further improve neighborhood safety. On the other hand, additional stops could have no impact on crime incidents, and prove ineffective in seizing weapons and/or contraband. In this case, there is seemingly little gain from conducting additional stops. In fact, if these stops fail to yield illicit goods, yet continue to result in enforcement action, then they could merely reflect an increased policing of petty offenses. This result could be interpreted as a cost disproportionately incurred by historically marginalized neighborhoods most vulnerable to gun violence.

This paper aims to resolve the theoretical welfare ambiguities tied to policing in the aftermath of gun violence. Restricting attention to roughly a six-year window in Chicago, I employ a staggered difference-in-differences design that exploits "large" relative shocks to gun victimizations between contiguous police beats. I show that police officers not only conduct more stops in treated beats, but that they continue to do so one month post treatment. In the immediate week of treatment, these additional stops correspond to fewer index crime incidents, and remove weapons and/or contraband from Chicago neighborhoods. Beyond relative week zero, index crime returns to pre-shock levels, and additional stops continue to result in enforcement action despite proving ineffective in seizing illicit goods. These findings serve as suggestive evidence of short-run benefits, and potential long-run costs, from this form of policing in the wake of gun violence.

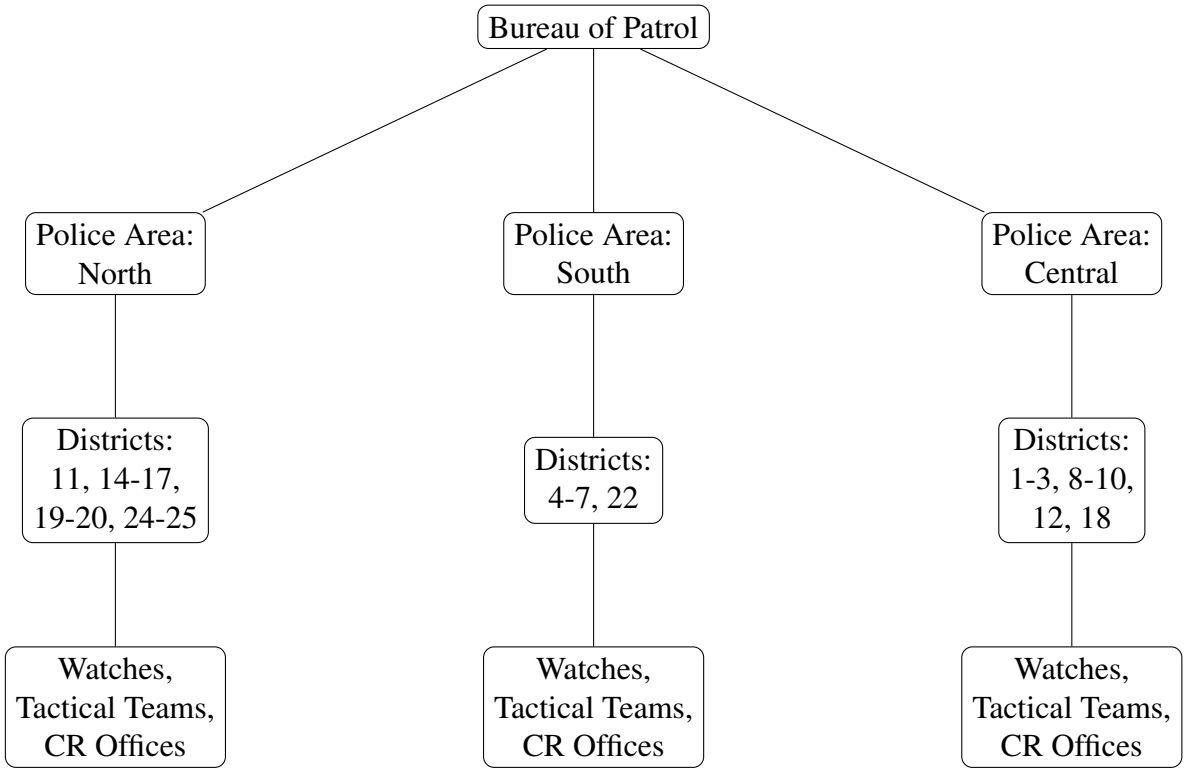
After identifying these baseline results, I set out to determine the distributional consequences and potential mechanisms tied to increased police responsiveness in stops. A breakdown of stops conditional on race reveals that Non-White stops overwhelmingly account for the baseline

rise in total stops, but only White stops produce weapons and/or contraband beyond the initial week of treatment at any reasonable level of significance. Only Hispanics face a treatment effect that persists into a second month. Hence, Non-Whites disproportionately bear the distributional consequences of officers conducting more stops. A breakdown of stops conditional on gang presence reveals that treated police beats with larger gang concentrations experience more investigatory stops. However, regions without gang hot-spots appear to benefit the most in the short run. Even a breakdown of Chicago by gang concentration fails to reveal welfare gains in beats beyond the immediate week of treatment.

4.6 Appendix

4.6.1 Additional Tables and Figures

Figure 4.A1: Breakdown of the Chicago Bureau of Patrol



This diagram is a visual representation of the patrol bureau in the Chicago Police Department. The bureau consists of three police areas, 22 police districts, and 277 police beats with watches, tactical teams, and community relations offices (G01-02-03).

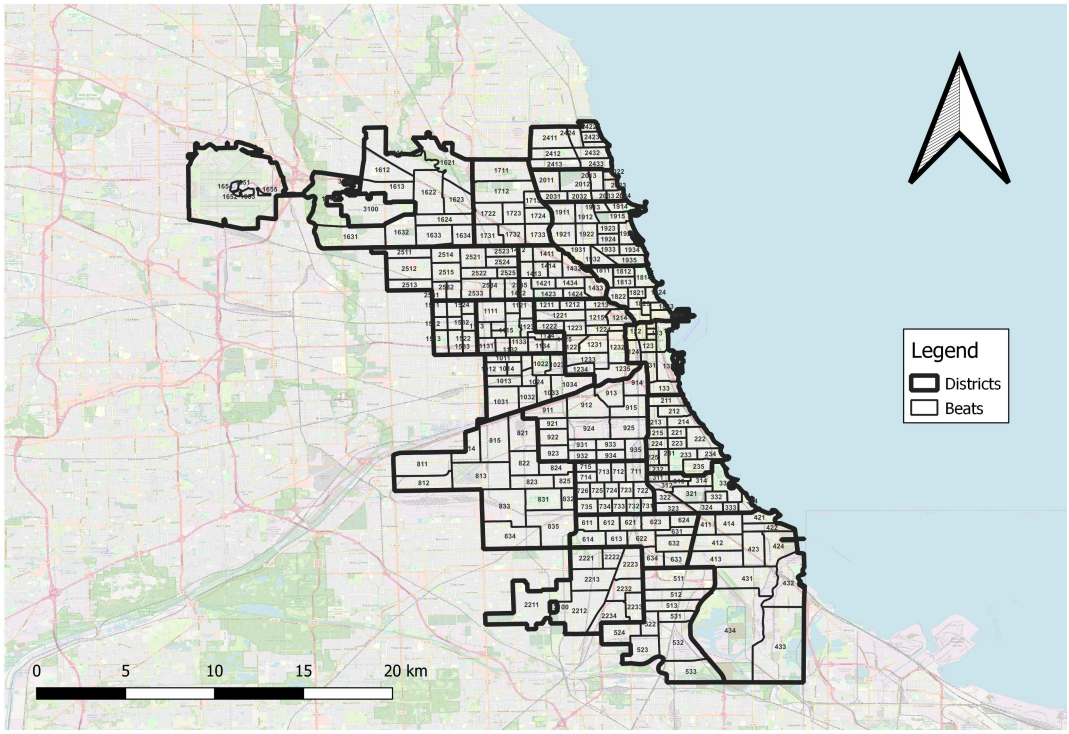


Figure 4.A2: Organizational Structure of the Chicago Police Department

This figure is a visual breakdown of the Chicago Police Department into police districts and beats. The darker (lighter) boundaries correspond to districts (beats). In total, there are 22 districts and 277 beats.

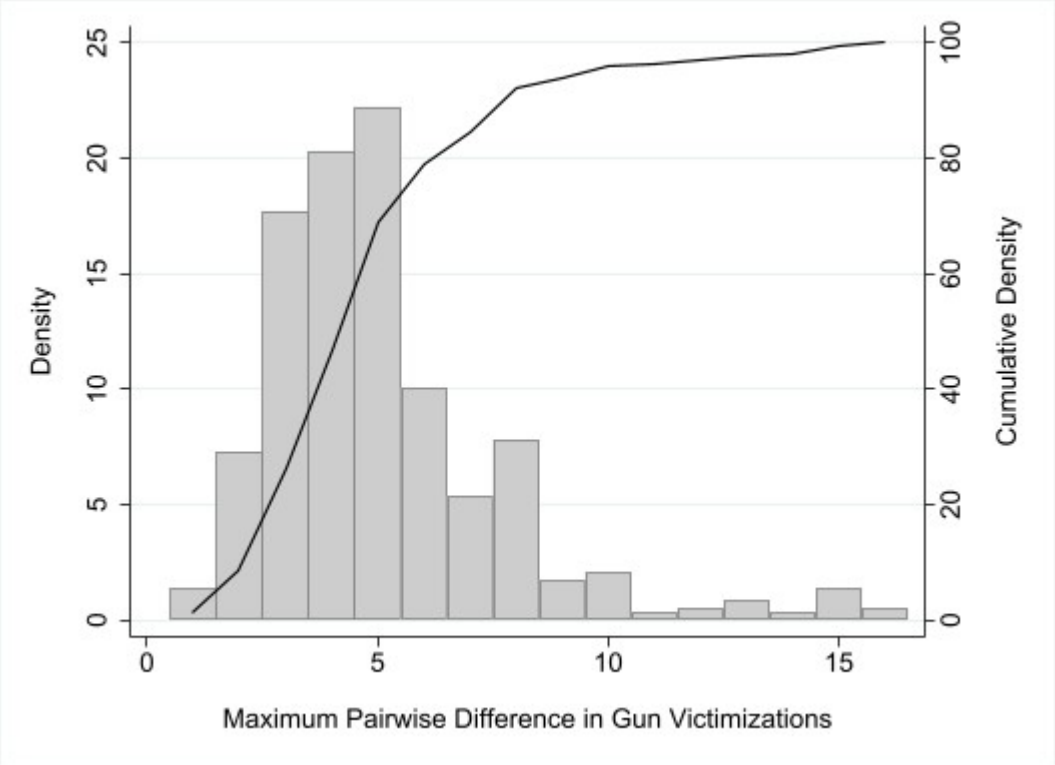


Figure 4.A3: Distribution of Maximum Pairwise Differences in Gun Victimizations

This figure displays the distribution of maximum pairwise differences in gun victimizations. The x-axis measures the size of the shocks, and the leftward (rightward) y-axis the (cumulative) density corresponding to each shock size.

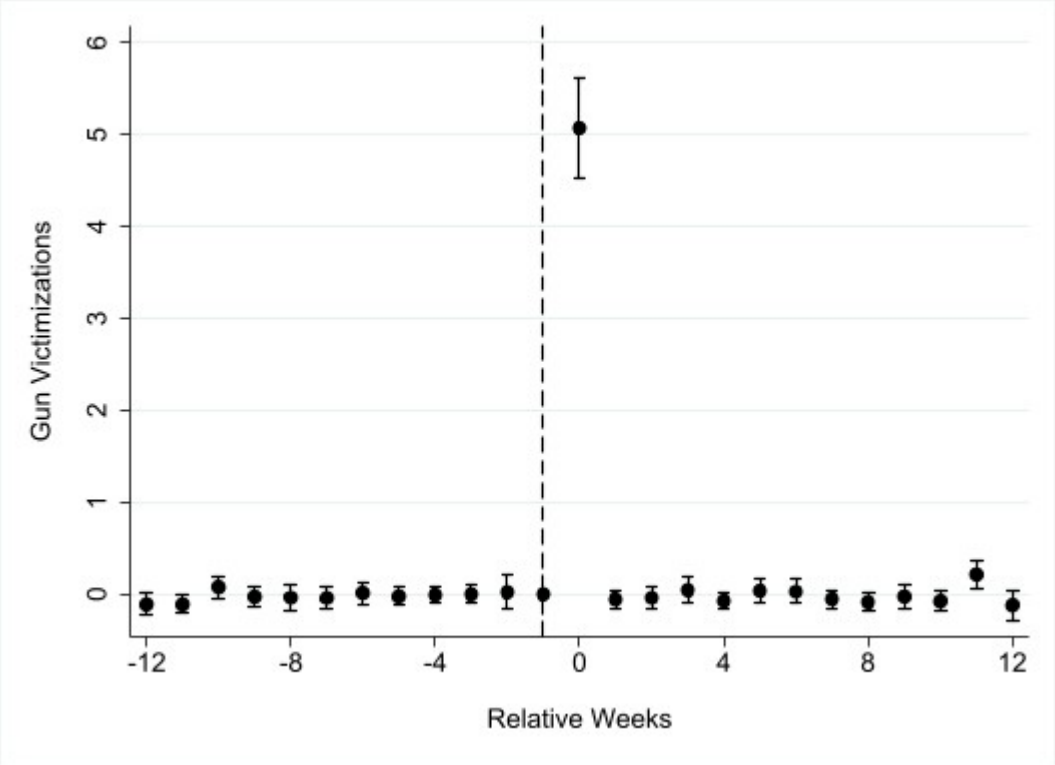


Figure 4.A4: Distribution of Maximum Pairwise Differences in Gun Victimizations

This figure shows the dynamic effects from regressing gun victimizations on the treatment leads and lags. I control for beat and pair-week fixed effects, and two-way cluster at the pair and district-year levels. I omit the first lead for comparative purposes, and denote this reference point with a dashed vertical line.

Table 4.A1: CPD-Identified Gang Membership in Chicago

Gang Name	Membership	Percent of Total Gang Membership
Gangster Disciples	37,113	29.29
Latin Kings	13,209	10.42
Black P. Stones	10,144	8.01
Four Corner Hustlers	8,752	6.91
Black Disciples	6,216	4.91
Conservative Vice Lords	5,470	4.32
Travelling Vice Lords	5,180	4.09
Maniac Latin Disciples	3,567	2.82
Satan Disciples	3,523	2.78
Two Six	3,138	2.48
Vice Lords	2,708	2.14
New Breed	1,998	1.58
Black Gangsters	1,905	1.50
Spanish Cobras	1,867	1.47
Mafia Insane Vice Lords	1,784	1.41
Unknown Vice Lords	1,666	1.31
Imperial Gangsters	1,407	1.11
Black Souls	1,394	1.10
112 remaining gangs	-	$<1\%$

This table displays gang membership counts for each Chicago-based gang accounting for more than 1% of the city's total gang population. The remaining 112 identified gangs are grouped together in the bottom row. I use the "Chicago Police Department Data on Gang Members" data set (accessed via ProPublica) to create this table. For digitization, I consider any gang sitting above the horizontal line separating "Travelling Vice Lords" and "Maniac Latin Disciples": the 3.00% threshold of total gang membership. One exception is Four Corner Hustlers since I am unable to identify their territory using the "2018 Gang Territorial Boundaries" map created by the Chicago Police Department.

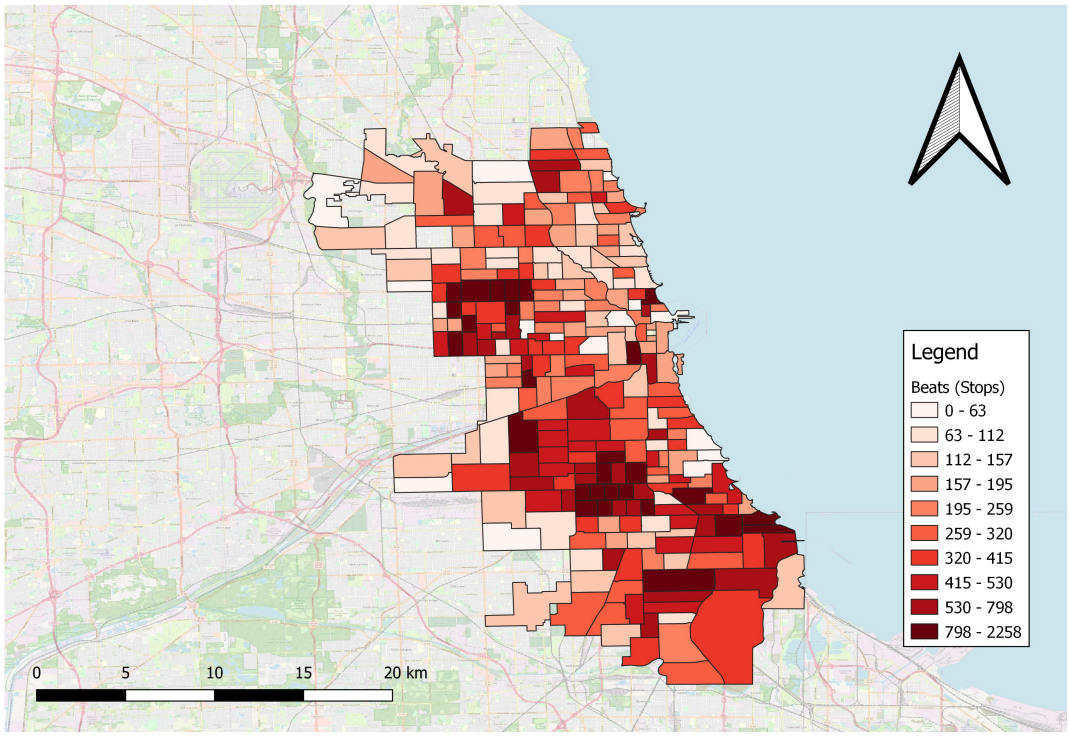


Figure 4.A5: Heat Map of Aggregate Stops

This figure illustrates the aggregate spatial variation in total investigatory stops in the sample at the beat-level. The legend on the right-hand side of the figure provides a color-coding scheme that corresponds to a range of aggregate stops: darker shades of red correspond to police beats with relatively more stops.

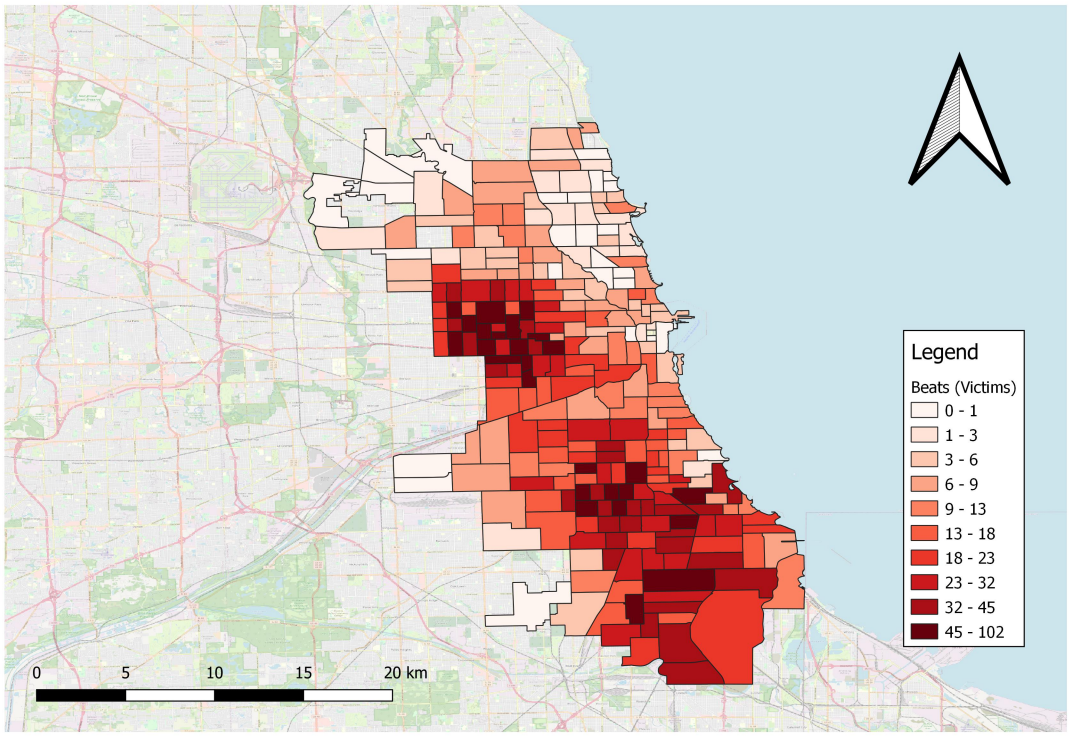


Figure 4.A6: Heat Map of Gun Victimizations

This figure displays the aggregate spatial variation in total gun victimizations, fatal or non-fatal, in the sample at the beat-level. The legend on the right-hand side of the figure provides a color-coding scheme that corresponds to a range of aggregate victimizations: darker shades of red correspond to police beats with relatively more victimizations.

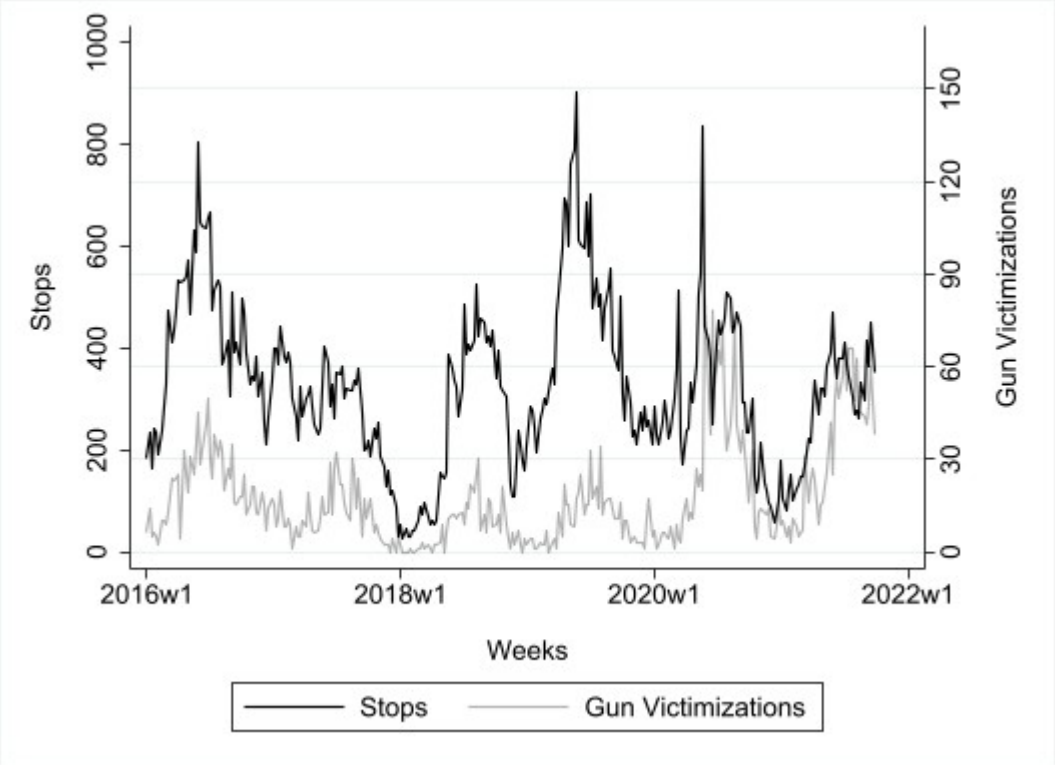
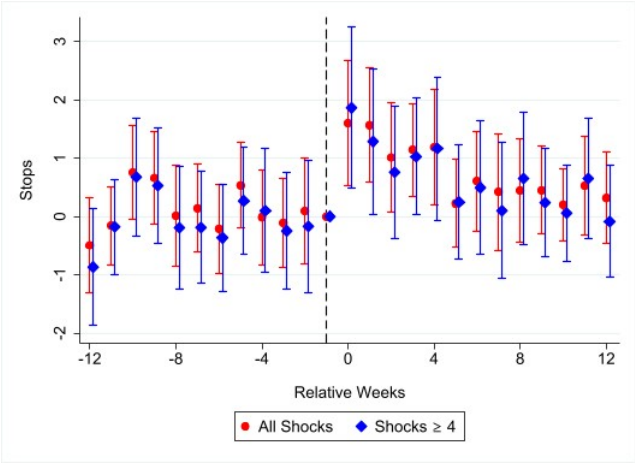
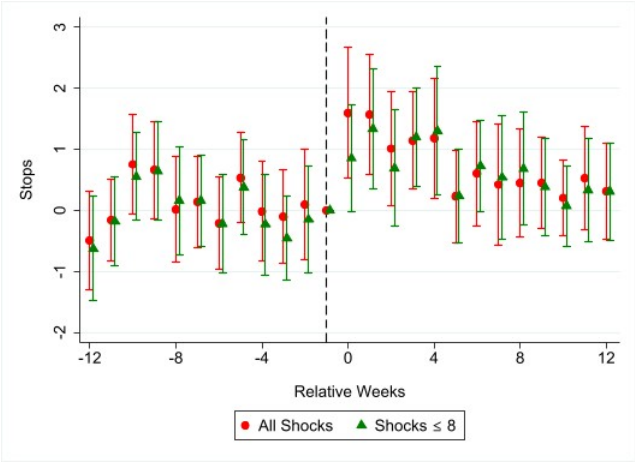


Figure 4.A7: Aggregate Trends in Stops and Gun Victimizations

This figure displays the aggregate temporal variation in total investigatory stops and gun victimizations, fatal or non-fatal, in the sample. The dark (light) trend lines correspond to the evolution of stops (gun victimizations). The leftward (rightward) y-axis measures stops (gun victimizations).



(a) All Shocks vs. Shocks ≥ 4



(b) All Shocks vs. Shocks ≤ 8

Figure 4.A8: Trimming the Sample by Maximum Pairwise Differences

This figure displays the dynamic treatment effects from estimating equation (4.1), where Y_{bpt} measures total investigatory stops, over a subset of data excluding either the lower- or upper-tail of the distribution of maximum pairwise differences in gun victimizations. In panel (a), I compare the baseline results (red-circle) to the results from re-estimating equation (4.1) over the subset of pairs with maximum pairwise differences in gun victimizations at least equal to four (blue-diamond). In panel (b), I amend this criteria to pairs with maximum pairwise differences no larger than eight (green-triangle).

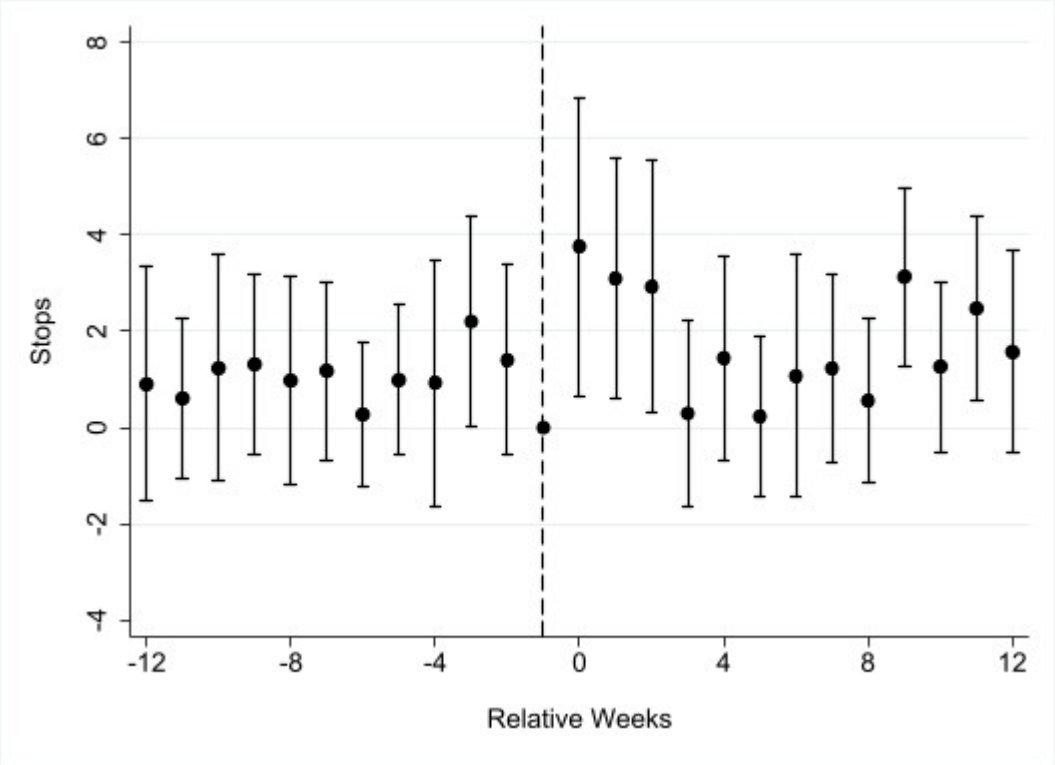


Figure 4.A9: Police Beats only Receive Treatment or Serve as Controls

This figure displays the dynamic treatment effects from estimating equation (4.1), where Y_{bpt} measures total investigatory stops, over a subset of data where beats are only assigned the treatment or only serve as controls. For each beat, I restrict attention to the pair that represents the "largest" shock to gun victimizations. After making these restrictions, beats are only assigned the treatment or only serve as controls. I omit the first lead for comparative purposes, and denote this reference point with a dashed vertical line.

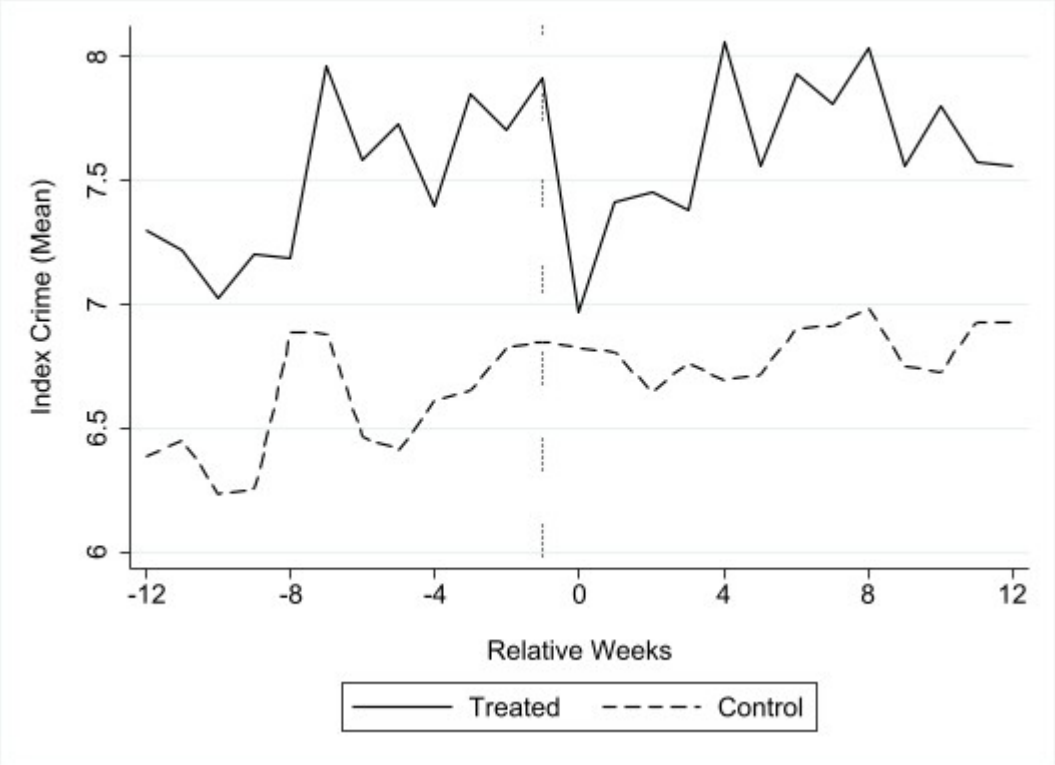
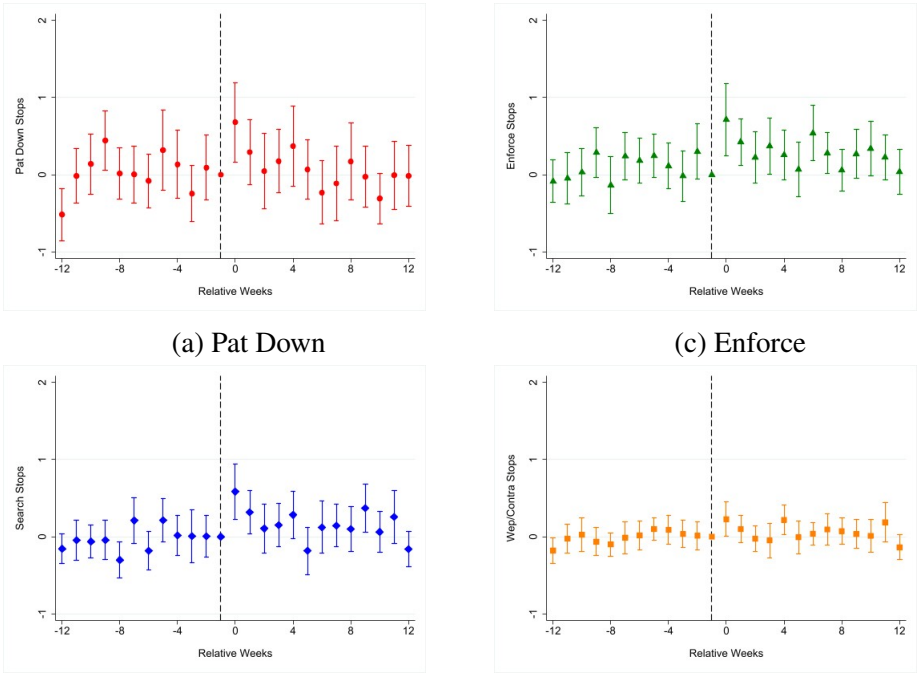


Figure 4.A10: Raw Means in Index Crime Incidents by Treatment Status

This figure displays the raw means in index crime incidents by treatment status for the 12 relative weeks around the initial timing of treatment. The solid (dashed) trend line captures the evolution in mean index crime incidents for the average treated (control) police beat.

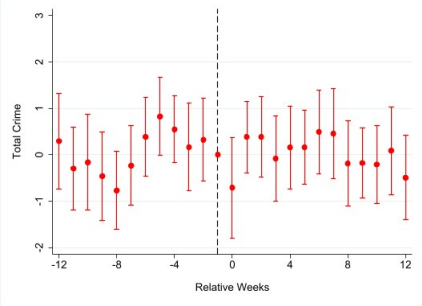


(a) Pat Down (c) Enforce

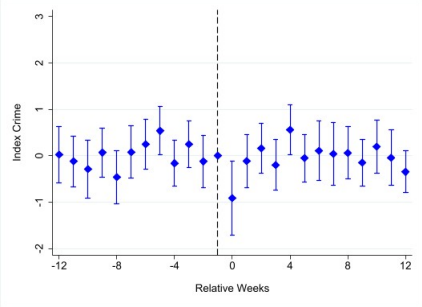
(b) Search (d) Wep/Contra

Figure 4.A11: Responsiveness in Stops by Type to Gun Violence

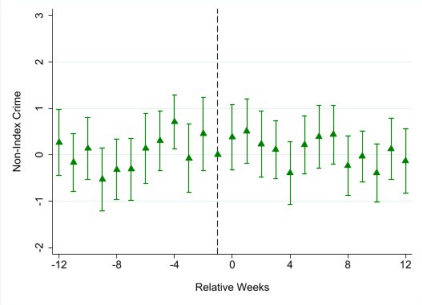
This figure displays the dynamic treatment effects from estimating equation (4.1), where Y_{bpt} measures investigatory stops conditional on pat downs (red-circle), search (blue-diamond), enforcement action (green-triangle), or seizing weapons and/or contraband (orange-square). I omit the first lead for comparative purposes, and denote this reference point with a dashed vertical line.



(a) Total Crime



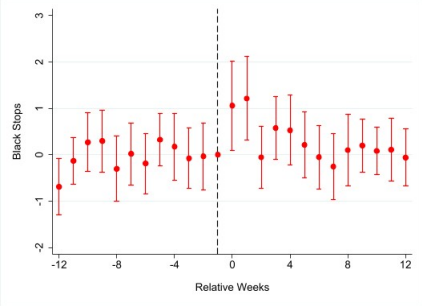
(b) Index Crime



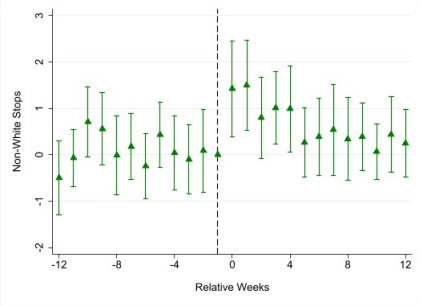
(c) Non-Index Crime

Figure 4.A12: Responsiveness in Crime by Type to Gun Violence

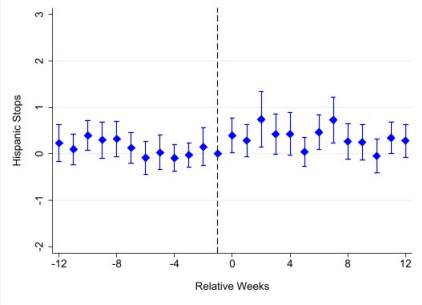
This figure displays the dynamic treatment effects from estimating equation (4.1), where Y_{bpt} measures total crime incidents (red-circle), index crime incidents (blue-diamond), or non-index crime incidents (green-triangle). I omit the first lead for comparative purposes, and denote this reference point with a dashed vertical line.



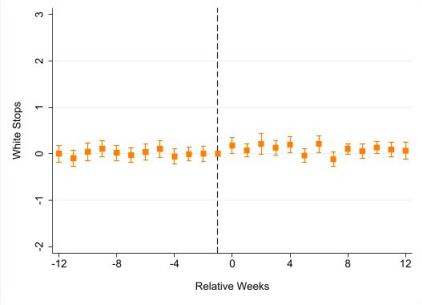
(a) Black Stops



(c) Non-White Stops



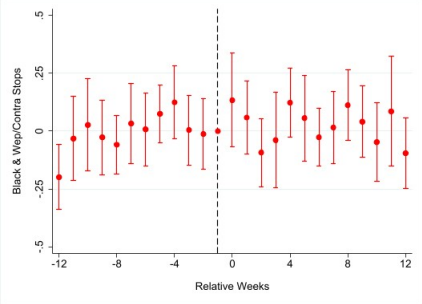
(b) Hispanic Stops



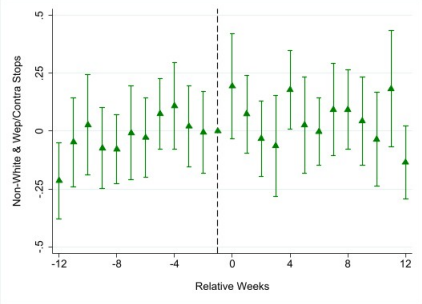
(d) White Stops

Figure 4.A13: Responsiveness in Stops by Race to Gun Violence

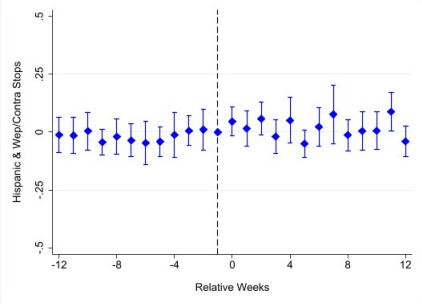
This figure displays the dynamic treatment effects from estimating equation (4.1), where Y_{bpt} measures investigatory stops conditional on the person stopped being Black (red-circle), Hispanic (blue-diamond), Non-White (green-triangle), or White (orange-square). I omit the first lead for comparative purposes, and denote this reference point with a dashed vertical line.



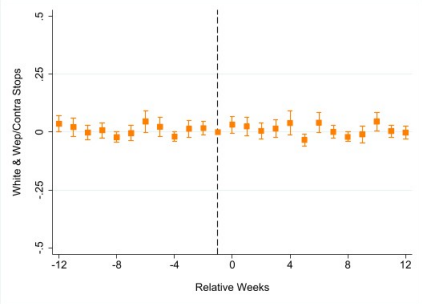
(a) Black & Wep/Contra



(c) Non-White & Wep/Contra



(b) Hispanic & Wep/Contra



(d) White & Wep/Contra

Figure 4.A14: Responsiveness in Wep/Contra Stops by Race to Gun Violence

This figure displays the dynamic treatment effects from estimating equation (4.1), where Y_{bpt} measures investigatory stops conditional on seizing weapons and/or contraband from Blacks (red-circle), Hispanics (blue-diamond), Non-Whites (green-triangle), or Whites (orange-square). I omit the first lead for comparative purposes, and denote this reference point with a dashed vertical line.

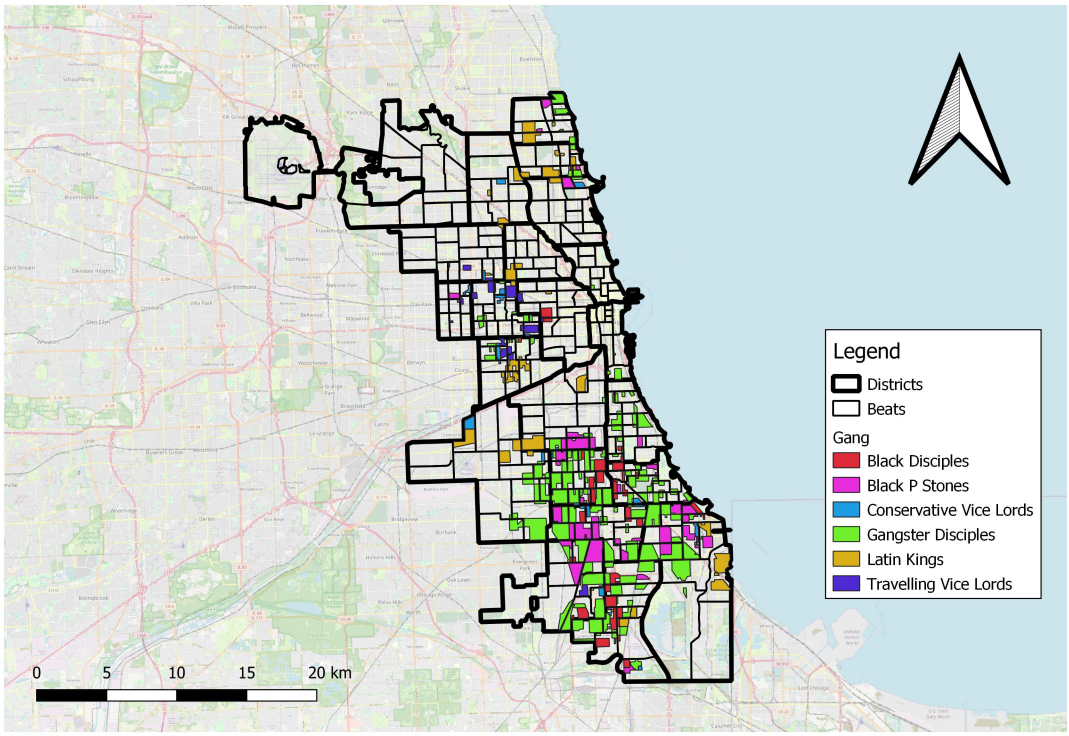


Figure 4.A15: Gang Territory in Chicago

This figure is the digitization of a gang territory map created by the Chicago Police Department. I include gangs accounting for at least 3% of all gang members in Chicago with exception to Four Corner Hustlers. The darker (lighter) boundaries correspond to districts (beats). The legend on the right-hand side of the figure provides a color-coding scheme corresponding to each gang.

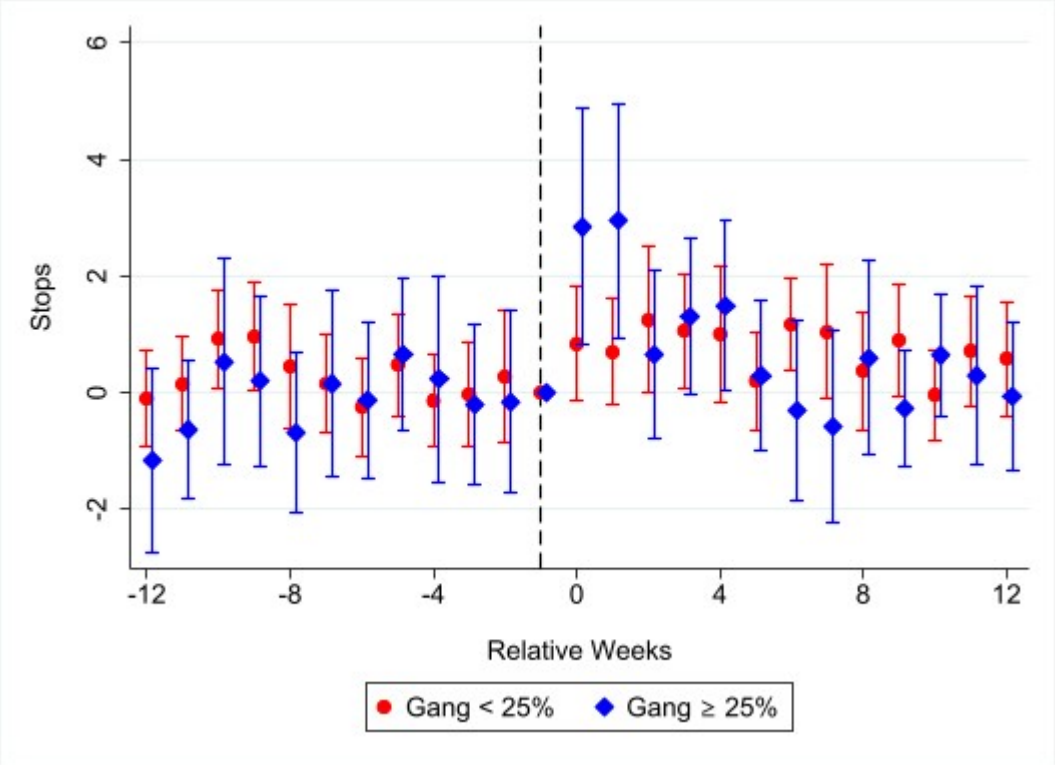


Figure 4.A16: Responsiveness in Total Stops by Gang Concentration to Gun Violence

This figure displays the dynamic treatment effects from estimating equation (4.1), where Y_{bpt} measures total investigatory stops, restricting attention to subsets by average pairwise gang concentration. The first subset includes only pairs with less than a 25% average (red-circle), while the second includes all remaining pairs (blue-diamond). I omit the first lead for comparative purposes, and denote this reference point with a dashed vertical line.

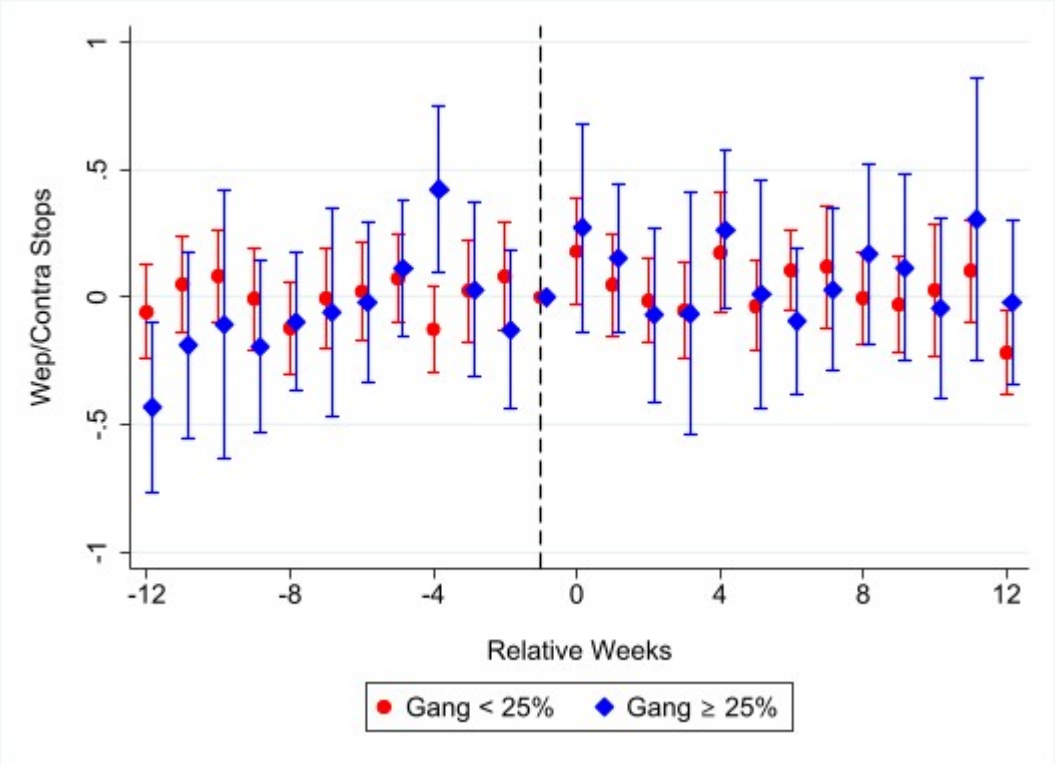


Figure 4.A17: Responsiveness in Wep/Contra Stops by Gang Concentration to Gun Violence

This figure displays the dynamic treatment effects from estimating equation (4.1), where Y_{bpt} measures investigatory stops conditional on seizing weapons and/or contraband, restricting attention to subsets by average pairwise gang concentration. The first subset includes only pairs with less than a 25% average (red-circle), while the second includes all remaining pairs (blue-diamond). I omit the first lead for comparative purposes, and denote this reference point with a dashed vertical line.

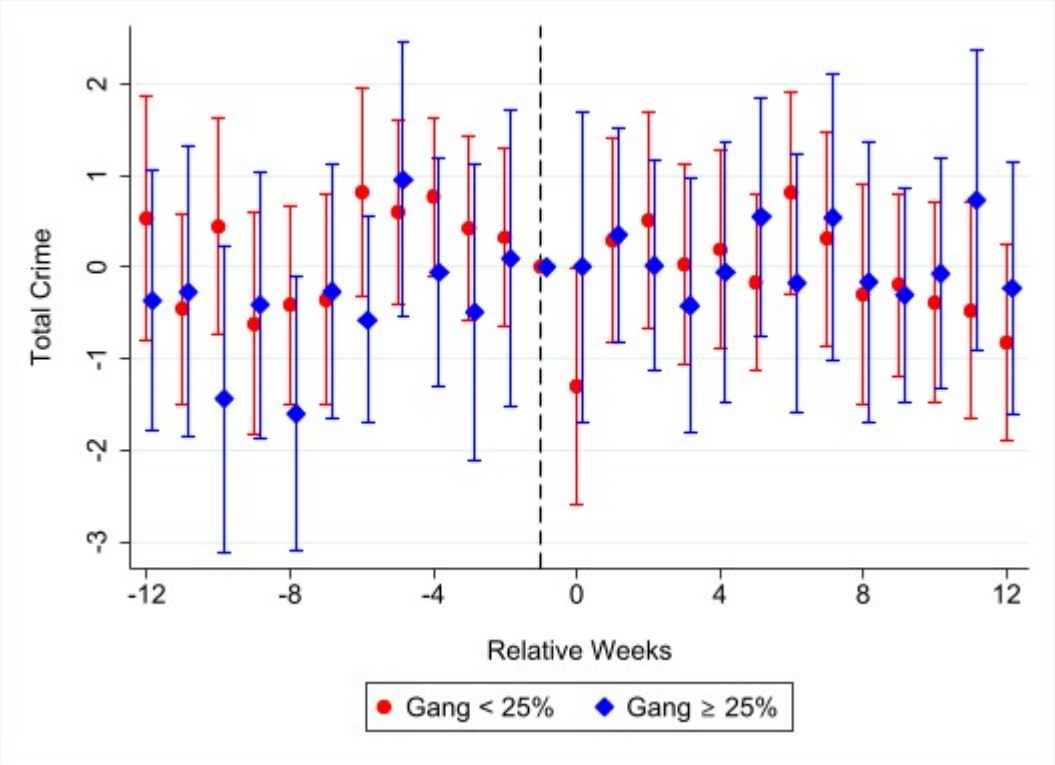


Figure 4.A18: Responsiveness in Total Crime by Gang Concentration to Gun Violence

This figure displays the dynamic treatment effects from estimating equation (4.1), where Y_{bpt} measures total crime incidents, restricting attention to subsets by average pairwise gang concentration. The first subset includes only pairs with less than a 25% average (red-circle), while the second includes all remaining pairs (blue-diamond). I omit the first lead for comparative purposes, and denote this reference point with a dashed vertical line.

Chapter 5: Insights into American Black Journalism: From the *Freedom's Journal* to the 21st Century

> *"In the grand struggle for liberty and equality now waging, it is meet, right and essential that there should arise in our ranks authors and editors, as well as orators, for it is in these capacities that the most permanent good can be rendered to our cause."*
>
> — Frederick Douglass, *The North Star* (December 3, 1847)

5.1 Introduction

The hallmark of a liberal democratic society is an unabridged free press. A free press permits an active marketplace of ideas that serves as an informative channel of dialogue between the voting age population and its elected officials. For the American Framers, protecting the freedoms of expression and speech was necessary for government to truly represent its people. Without free discourse, concerned citizens would have been denied a forum "to petition the government for redress of grievances". The Founders saw the first amendment to the U.S. Constitution as a way to preempt the formation of a government apathetic to the critiques and concerns of its people.

Despite constitutional protections, historically marginalized communities were often denied the freedoms of expression and speech. Regularly barred from traditional print publications, Black journalists founded their own media outlets to inform their neighborhoods on key issues, promote artistic expression, highlight Black success stories, address racial grievances, and influence people in power. Many famous Black activists launched social campaigns in the press. Frederick Douglass critiqued slavery in *The North Star*; Ida B. Wells promoted an anti-lynching campaign in the *Memphis Free Speech*; W.E.B. Du Bois opposed Booker T. Washington in *The Crisis*; Robert Abbott supported northern migration in the *Chicago Defender*; and Robert Vann championed the "Double-V" campaign in the *Pittsburgh Courier* (Washburn, 2006).

In light of the importance of the Black press, it is unsurprising that numerous archival collections and resources document these writings. Collections are stored in the New York and Boston Public Libraries, the Library of Congress, and numerous colleges and universities, e.g. the University of Arkansas' Microfilm collection. These projects are often exhaustive, in print form, and/or digitized but not presented in a structural form lending itself to data analysis. These features limit our ability to measure national, regional, and place-level trends in Black media representation. By extension, identifying determinants of the distribution of upstart Black print publications proves equally challenging.

In this paper, I overcome these limitations by converting a roughly 800-page national bibliography on Black print publications (Danky and Hady, 1998), covering nearly 180 years of media outlets, into a usable CSV file. In the book's foreword, Henry Louis Gates Jr. writes that this "... national bibliography constitutes the most complete census of the newspapers and periodicals published and edited by African-Americans between 1827 and the present."

I have two primary objectives with the extracted data. First, I will plot counts and rates per

capita in Black newspapers and periodicals over nearly two centuries. Interest lies in quantifying the most and least active periods in Black journalism, regional (South vs. Non-South) disparities in Black publications, Black literary activity at the city-level, and the most popular subject matter confronted by Black writers. Second, this paper will determine if rising Black literacy rates explains the creation of Black publications. On the one hand, profit maximization suggests that Black newspapers should be most prevalent in places with an expanding consumer base resulting from rising Black literacy rates. On the other hand, the burgeoning collection of Black writers may have prioritized pre-existing literate Black populations, or the influential White readership: Frederick Douglass was especially keen on impressing White readers to uplift the Black image in America (Washburn, 2006). I will examine the relationship between Black print publications and literacy using a fixed effects model over respondent-level census data spanning 1870-1930.

This paper will proceed as follows. The subsequent section (5.2) describes the data while outlining various limitations. Section 5.3 explores national, regional, and place-level trends, as well as the relationship between literacy and print media. The last section (5.4) concludes.

5.2 Data

The primary data source is Danky and Hady's "African-American Newspapers and Periodicals: A National Bibliography". The book includes a fairly comprehensive collection of 6562 unique print publications from 1827 to 1998. Each entry tracks a publication's start date, end date, frequency (daily, weekly, annually, etc.), location, and subject types. For clarity, I provide an example entry below:

2517 *Freedom's Journal* 1827-1829. Frequency: Weekly. New York, NY. Published

by Cornish and Russworm. Last issue 4 pages. Last volume 208 pages. Line drawings; Commercial advertising. Previous editor(s): John B. Russworm, Mar 16 1827-Mar 28 1829; Samuel E. Cornish, Mar 16-Sept 14 1827. Superseded by: Rights of All. LC card no. sn83-30455. OCLC no. 1570144. Subject focus and/or Features: Newspaper.[1]

The example publication is numbered 2517 with the title *Freedom's Journal*. The publication began in 1827 and ceased in 1829. Each issue was released weekly, and the place of publication was New York City. Additional information on issue and volume length, advertising, and editorial history follows. The entry is labeled as a newspaper.

I undergo rigorous data processing to map this information into a structured CSV file for each publication. I download the book as an html file and save as a txt file. I import the saved file into a text editor, Notepad++, to parse the data for consistency across entries. The goal is to have the entries as standardized as possible for successful extraction. I extract the information using a Python script and save the output as a CSV file. While the extraction was largely successful, there remains missing information. I fill in missing data rows and columns manually in STATA. Publications are dropped if they are missing start dates and/or locations. Foreign publications are likewise dropped. The cleaned dataset consists of 6366 publications, or 97.01% of all publications in the raw data. I drop the single entry with a 1998 start date since this year (the timing of publication) is likely underrepresented in coverage. I use this cleaned data set to plot national, regional, and place-level trends.

Prior to data analysis, it is important to highlight various data limitations. While start date

[1]Libraries storing the print publication are listed at the end of entries. I exclude this information since it is not relevant to this paper.

coverage is strong, end date coverage is poor. Therefore, counts and rates only reflect entry and not exit. In other words, the collective number of operational outlets at any given point in time is not observed. Frequency is often reported, but there are too many "Uncertain" entries and different categories to meaningfully group the information. Location coverage is comparatively strong, however it is often difficult to distinguish if the listed location is the place of publication, editorial address, or both. Finally, many entries have multiple subject focuses and/or features, meaning that subject breakdowns can overlap.

For the paper's second objective, I identify literacy rates using full sample census data from 1870-1930, excluding 1890 (no data). Only Black respondents in identifiable cities with full coverage from 1870-1930 and at least one Black print publication are included. Included cities must also have been part of the U.S. over this time interval. I also drop respondents with missing literacy data. Temporal coverage is explained by data availability. While literacy is measured as early as 1850, I choose 1870 as the earliest date since Black slaves were legally prohibited from reading and writing. The end date reflects the last Census literacy was measured. From 1940 onward, only information on educational attainment was surveyed.

5.3 Results

5.3.1 National, Regional, and Place-Level Trends

Figure 5.1 displays counts and rates of total Black print publications, Black newspapers, and Black periodicals (non-newspapers). I find that counts of new publications grow exponentially from 1820-1997, while rates assume a comparatively linear growth pattern. Counts of upstart newspapers peak around the turn of the 19th century and height of the Civil Rights Movement.

The distribution of rates favors the former peak. Meanwhile, counts of nascent periodicals peak during the Interwar years, Civil Rights Movement, and turn of the 20th century. The distribution of rates places nearly equal weight on these local maximums.

Inspection of publications by region, i.e. the South (former Confederacy) and Non-South, reveals disparities in Black media representation. Figure 5.2 displays these regional trends. While both regions account for roughly the same number of Black publications before 1900, regional differences grow exponentially over the next century. Perhaps most telling is that the smoothed regional difference in rates, beginning in 1820, strongly persists over the next 177 years. This finding suggests that path dependency is critical to long-term developments in access to media outlets.

Place-level results highlight the importance of the Great Migrations in explaining the rise of Black journalism. Many of the top-25 cities in number of Black print publications are Northern cities: New York ranks first (784), Washington D.C. second (548), and Chicago third (350). Nonetheless, major Southern cities, such as Atlanta (fourth-202), and places with historically black colleges and universities, such as Jackson, MS (ninth-86), Hampton, VA (18th-53), Tuskegee, AL (24th-41), and Wilberforce, OH (25th-41), rank among the Top-25 cities with the most publications. See Table 5.1 for the complete ranking list. Figure 5.3 confirms that Northern cities only began to dramatically outpace Southern cities in Black journalism around the timing of the first Great Migration.

Subject-level results suggest that Black writers had a diversity of interests. As displayed in table 5.2 newspapers, colleges-students, religion, poetry, education, general interest, politics, music, civil rights, and history round out the Top-10 topics most represented. Figure 5.4 displays the temporal relationships across subject matter in print publications.

5.3.2 Black Literacy Rates and Black Print Publications

The paper will now transition to the second objective: what is the relationship between Black literacy rates and print publications? Five years removed from the conclusion of Civil War, more than three in every five Black Americans were illiterate. Over the next 60 years, this rate rapidly converged towards complete literacy. Growth in literacy was evident in the Non-South and South, where regional differences started to close over time. See figure 5.5. With greater scholastic proficiency, a growing set of Black writers could have contributed to the simultaneous rise in Black representation in print media. Similarly, an expanding Black readership could have incentivized the entrance of new Black newspapers in the print media industry.

I estimate the following fixed effects model over repeated (full-sample) census cross sections from 1870-1930 to shed light on the relationship between Black literacy rates and print publications:

$$M_{p(i),t+1} = \alpha + \beta Lit_{it} + \mathbf{X_i}\gamma + \delta_p + \tau_{st} + \epsilon_{p(i),t+1};$$

where i = black respondent, p = place, s = state, t = census year; $M_{p(i),t+1}$ is Black print publications (per 100k) between census years t and $t+1$, e.g. 1870-1879, 1880-1889, ..., 1930-1939; Lit_{it} is a binary for respondent i being literate (read and write) in census year t; $\mathbf{X_i}$ is a vector of covariates (gender and age); and δ_p and τ_{st} are place and state-by-year FEs. I cluster standard errors at the place level. The paper's parameter of interest is β, which captures the impact of an additional literate Blacks on the number of new Black print publications per 100k over the next decade.

Table 5.3 displays results from estimating equation (5.3.2) over different sets of dependent

variables and fixed effects. Each supercolumn refers to a dependent variable: total publication rates, newspaper rates, and periodical (non-newspaper) rates. Within each supercolumn, columns from left to right build towards the preferred specification. I begin by controlling only for the covariate set. Then, I add place and decade fixed effects. Finally, I replace these individual fixed effects with a combined interactive fixed effect. The interaction between decade and state fixed effects absorbs secular differences across states over the 60 year interval. I prefer this term to the separate fixed effect terms given the pronounced evolution of race relations across states.

I find a null relationship between Black literacy rates and print publications. In fact, after internalizing secular trends across states, the estimates converge to zero irrespective of restricting attention to total, newspaper, or non-newspaper rates. These null results persist for regional breakdowns (figure 5.6) and subject breakdowns (figure 5.7)

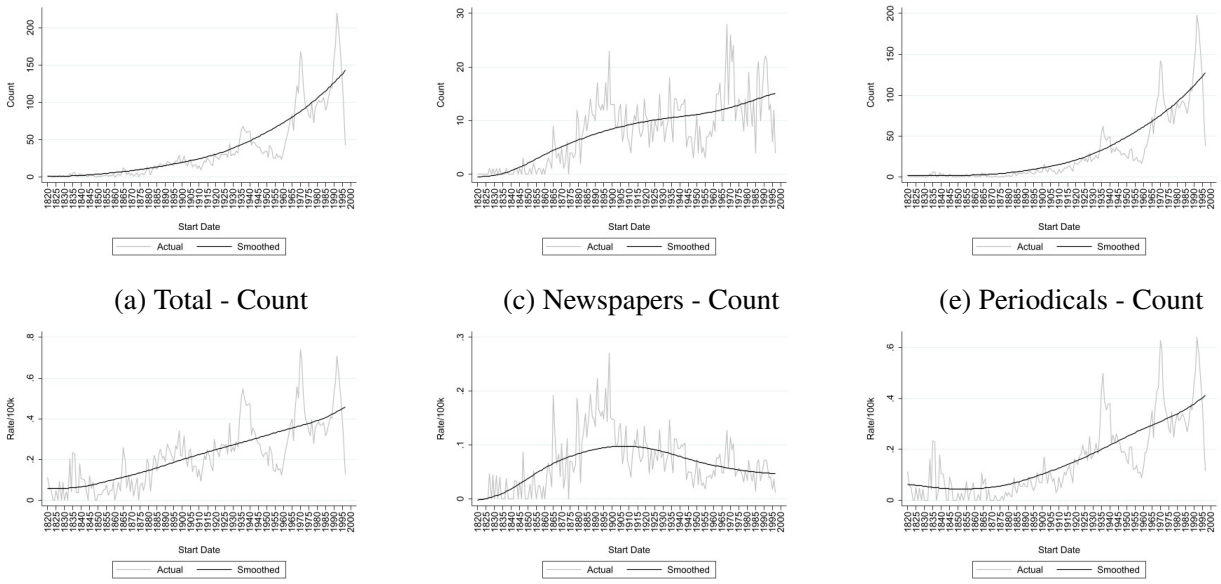
Why do we not see a positive correlation between literacy and publications? After all, even purely social objectives like uplifting Black respectability in America require sufficient financing. Black print publications also could not rely on advertising revenue to the same extent as mainstream publications, making subscription rates even more critical to their long-term survival (Washburn, 2006). One possibility is that the marginal subscription rate of newly literate Blacks was quite low. Washburn (2006) characterizes a widespread practice of subscribers sharing newspapers with the non-subscribed Black readership. This practice was so commonplace that some newspapers even hired agents to discourage the sharing of print publications. Another contributing factor could be newly literate debtors failing to fulfill their subscription payments. Uncollected payments were one of many financial woes plaguing the Black print publication industry (Washburn, 2006). Finally, it could be the case that existing publications absorbed the expanding consumer base and discouraged the entry of new Black print media outlets. These

explanations serve as plausible reasons for the null results. Future research should aim to pinpoint the leading mechanism at work.

5.4 Conclusion

The first amendment guarantees an unabridged free press. Nonetheless, Black writers were historically underrepresented in mainstream newspapers and periodicals that catered to a predominantly White audience. This reality sparked the creation of Black print publications. This paper plots the burgeoning growth in Black literary expression from 1820 to 1997. Counts grow exponentially, while rates per capita exhibit a more linear pattern. The aggregate rise in Black publications undermines important regional disparities tracing back to the antebellum era. Northern urban centers served as hot-spots for the creation of newspapers and periodicals. HBCUs likewise played a similar role. The dramatic rise in Black literacy, from under 40% nationally in 1870 to over 90% in 1930, is not linked to the distribution of upstart Black publications.

5.5 Tables and Figures

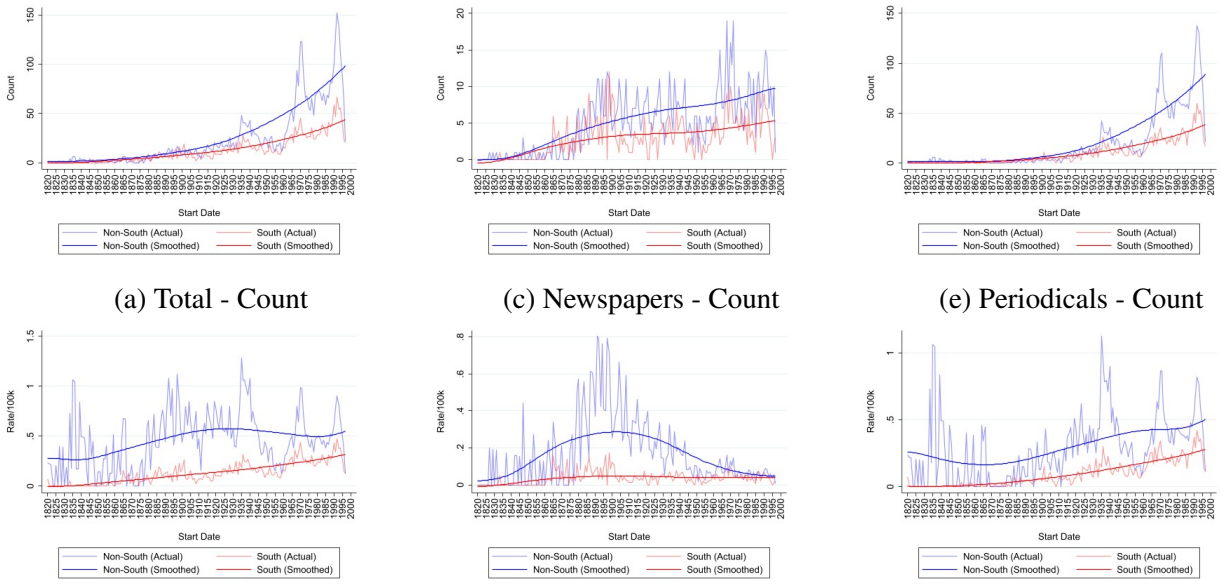


(a) Total - Count (c) Newspapers - Count (e) Periodicals - Count

(b) Total - Rate (d) Newspapers - Rate (f) Periodicals - Rate

Figure 5.1: National Trends - Black Print Journalism

This figure displays national trends in Black print publications from 1820 to 1997. These trends are explored for a number of breakdowns. I consider total print publications, newspapers only, and periodicals (non-newspapers) only, where each case is analyzed by count and rate (per 100k Blacks). These cases are labeled appropriately beneath each corresponding subplot. The noisy, light gray trend lines correspond to actual counts or rates. The darker line is the smoothed trend.



(a) Total - Count (c) Newspapers - Count (e) Periodicals - Count

(b) Total - Rate (d) Newspapers - Rate (f) Periodicals - Rate

Figure 5.2: Regional Trends - Black Print Journalism

This figure displays regional trends in Black print publications from 1820 to 1997. These trends are explored for a number of breakdowns. I consider total print publications, newspapers only, and periodicals (non-newspapers) only, where each case is analyzed by count and rate (per 100k Blacks). These cases are labeled appropriately beneath each corresponding subplot. The noisy, light blue (red) trend lines correspond to actual counts or rates for the Non-South (South). The darker blue (red) line is the smoothed trend for the Non-South (South).

Table 5.1: Top-25 Rankings of Number of Black Print Journalism by Place

Place	Count
New York, NY	784
Washington, DC	548
Chicago, IL	350
Atlanta, GA	202
Los Angeles, CA	176
Philadelphia, PA	135
Nashville, TN	126
New Orleans, LA	109
Jackson, MS	86
Detroit, MI	84
Baton Rouge, LA	75
Baltimore, MD	75
Milwaukee, WI	62
Boston, MA	59
San Francisco, CA	57
Cleveland, OH	54
Miami, FL	53
Hampton, VA	53
Houston, TX	52
Dallas, TX	50
St. Louis, MO	46
Oakland, CA	43
Birmingham, AL	42
Tuskegee, AL	41
Wilberforce, OH	41

This table displays counts in Black print publications from 1820 to 1997 at the place level. The Top-25 places with the most publications are listed from top (most) to bottom (least).

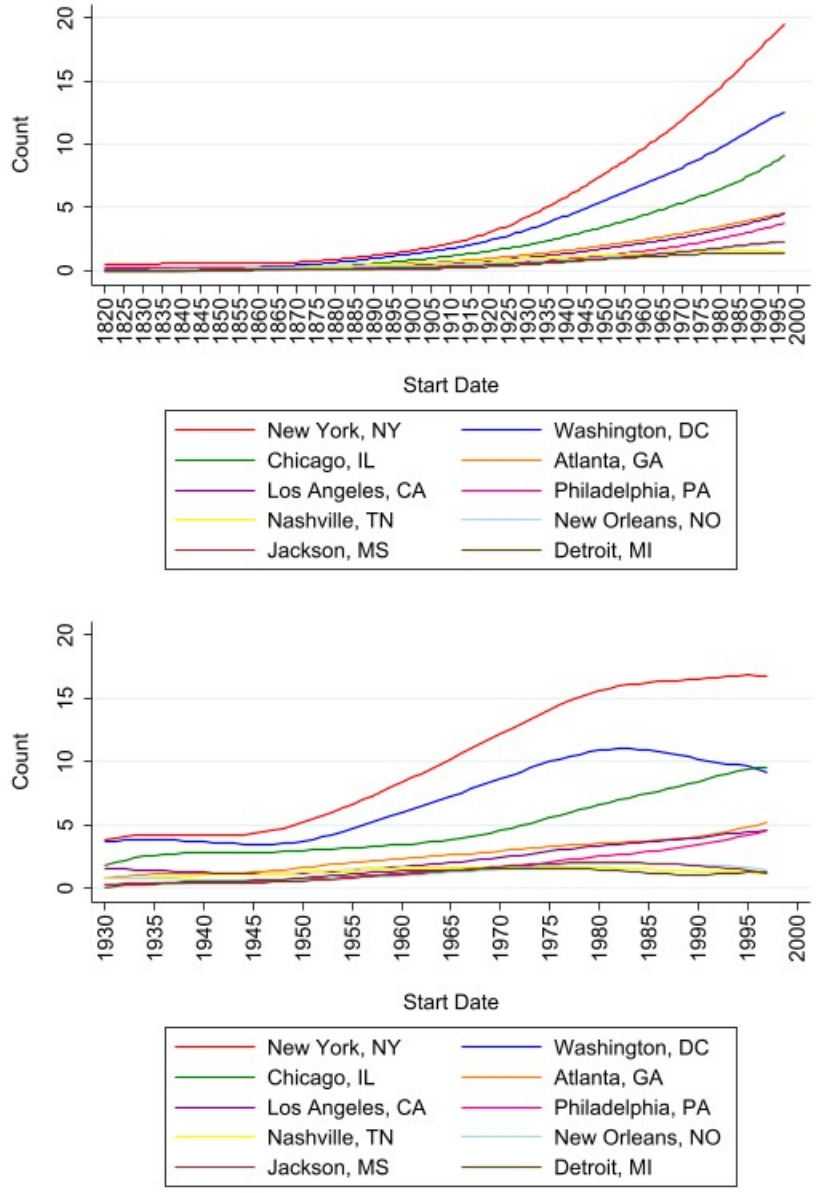


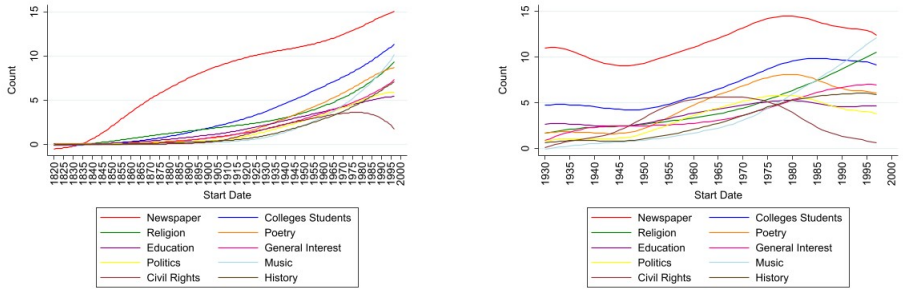
Figure 5.3: Superimposed Trends for Top-10 Places - Black Print Journalism

This figure displays variation in counts in Black print publications from 1820 to 1997 at the place level. Trend lines for the Top-10 places with the most publications are superimposed for comparative purposes. The bottom figure zooms in to temporal coverage between 1930 and 1997 to better discern place-level differences over this interval.

Table 5.2: Top-25 Rankings of No. of Black Print Journalism by Subject

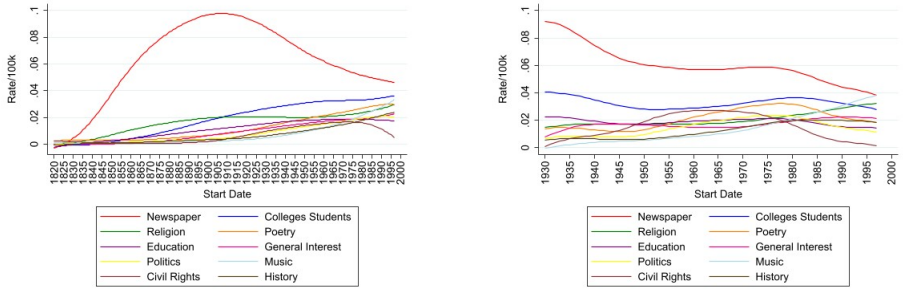
Subject	Count
Newspaper	1410
Colleges Students	563
Religion	430
Poetry	373
Education	323
General Interest	291
Politics	259
Music	242
Civil Rights	231
History	224
Business	218
Art	213
Short Stories	198
Colleges	193
Entertainment	175
Profiles	167
Book Reviews	163
Race Relations	157
Community Newsletter	145
Africa	144
Health	143
Women	142
Colleges Alumni	130
Essays	128
Howard University	128

This table displays counts in Black print publications from 1820 to 1997 at the subject level. The Top-25 subjects with the most publications are listed from top (most) to bottom (least).



(a) Total - Count - Full Coverage

(c) Total - Count - Post-1930



(b) Total - Rate - Full Coverage

(d) Total - Rate - Post-1930

Figure 5.4: Superimposed Trends for Top-10 Subjects - Black Print Journalism

This figure displays variation in counts and rates in Black print publications from 1820 to 1997 at the subject level. Trend lines for the Top-10 places with the most publications are superimposed for comparative purposes. Plots labeled "Post-1930" (as opposed to "Full Coverage") zoom in to temporal coverage between 1930 and 1997 to better discern subject-level differences over this interval.

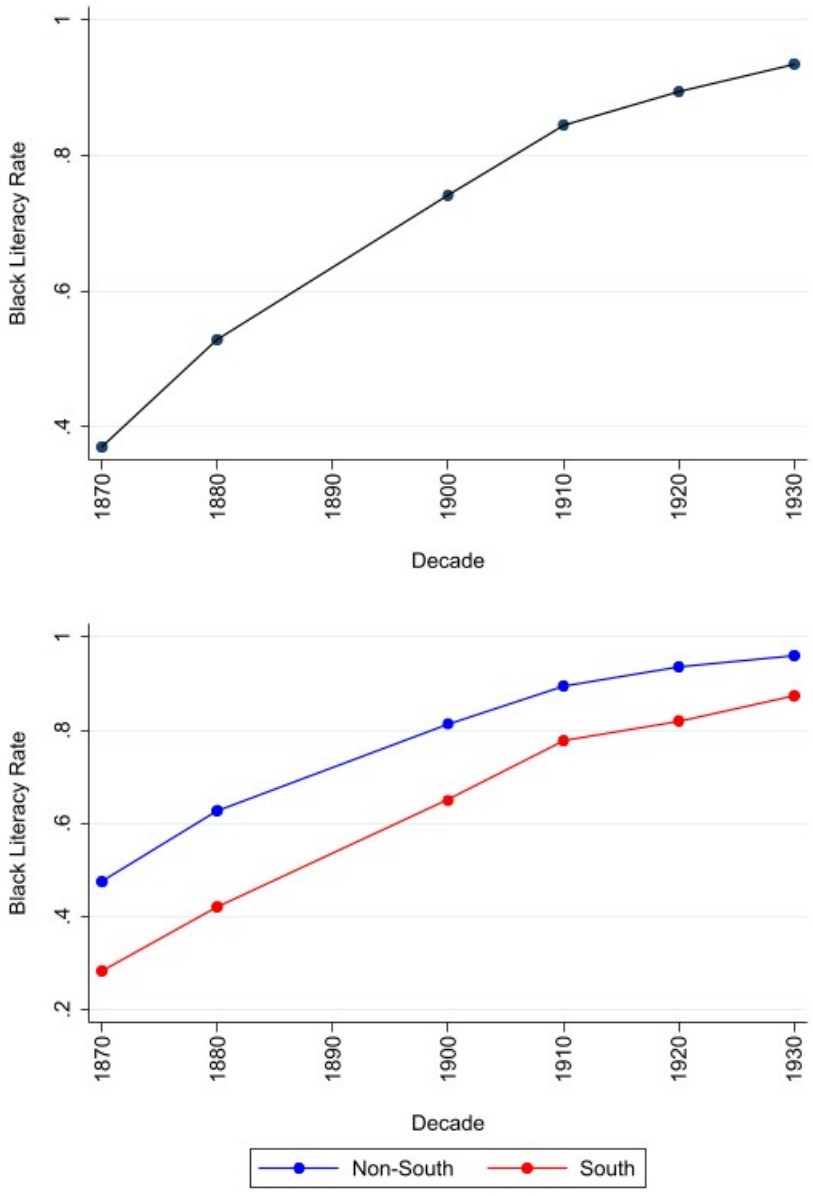


Figure 5.5: National and Regional Trends - Sample Black Literacy Rates

This figure displays variation in average literacy rates from 1870 to 1930 at the national (top) and regional (bottom) level.

Table 5.3: The Impact of Black Literacy on the Creation of Black Print Publications per 100k, 1870-1930

	Total			Newspaper			Non-Newspaper		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Literate	1.409	0.448**	-0.016	-0.767	0.283*	-0.025	2.176**	0.165	0.010
	(1.112)	(0.216)	(0.053)	(0.612)	(0.168)	(0.045)	(0.878)	(0.173)	(0.022)
Covariates	X	X	X	X	X	X	X	X	X
Place FEs		X	X		X	X		X	X
Decade FEs		X			X			X	
Decade-by-State FEs			X			X			X
Mean	8.657	8.657	8.657	3.427	3.427	3.427	5.230	5.230	5.230
SD	14.543	14.543	14.543	8.542	8.542	8.542	11.804	11.804	11.804
Obs.	5,942,408	5,942,408	5,942,408	5,942,408	5,942,408	5,942,408	5,942,408	5,942,408	5,942,408

This table displays results from estimating equation (5.3.2) across different dependent variable and specification combinations. The table consists of three supercolumns, each corresponding to a different set of publications per 100k Blacks: total, newspapers only, and Non-Newspapers (Periodicals). Within each supercolumn, I estimate over three different specifications. The leftmost column controls only for gender and age; the middle column adds decade and state fixed effects; and the rightmost column (preferred) substitutes the fixed effects for a decade-by-state fixed effect.

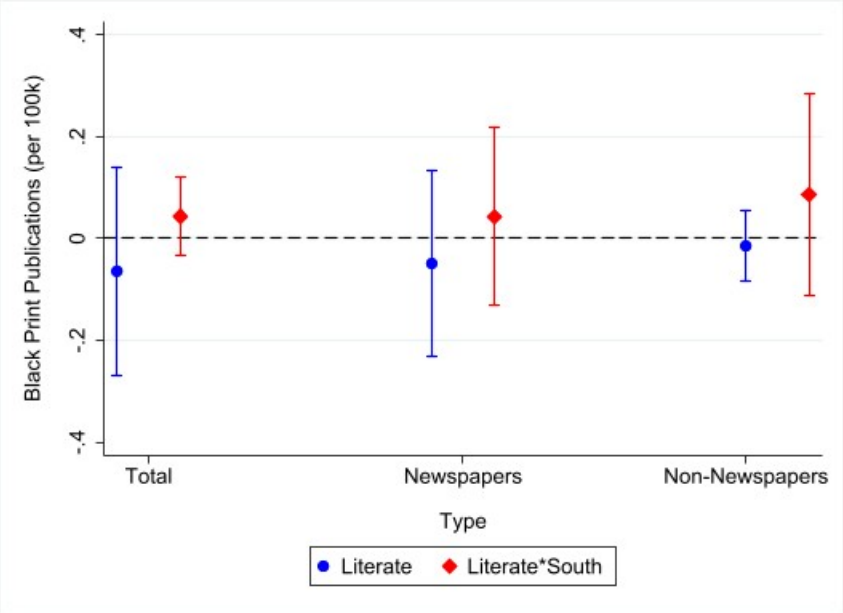


Figure 5.6: The Relationship between Black Literacy and Publications by Region

This figure displays results from estimating a variant of equation (5.3.2) permitting regional differential effects: an interaction term, $Lit_{it}South_i$ is included to the baseline specification. Each regional differential is estimated for total print publications, newspapers only, and non-newspapers (periodicals) only. Results in blue (red) correspond to the Non-South (South).

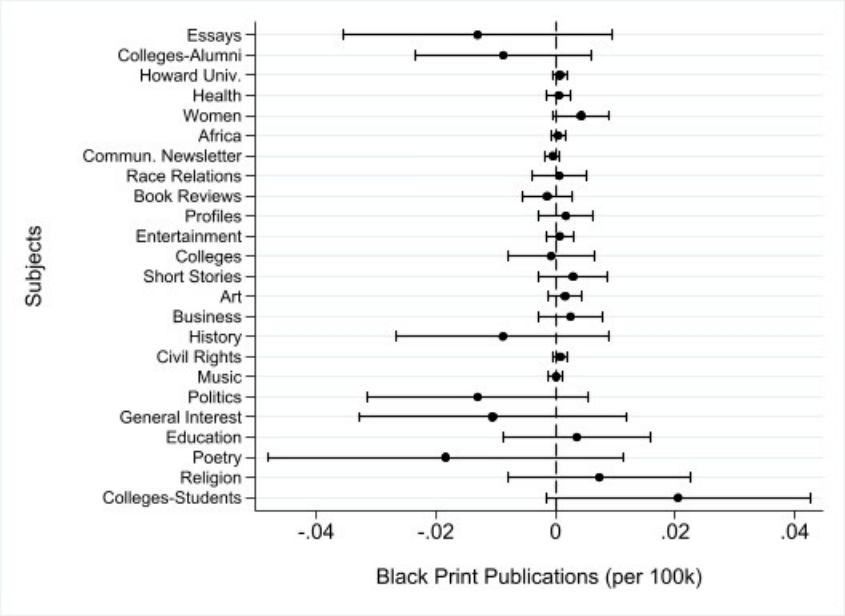


Figure 5.7: The Relationship between Black Literacy and Publications by Subject

This figure displays results from estimating equation (5.3.2) for total print publications by subject type. Each estimate corresponds to the leftward subject type listed on the y-axis.

Bibliography

ACLU, 2019. Racism in the rear view mirror: Illinois traffic stop data 2015-2017 , 1–15.

ACLU, 2022. Stop and frisk. https://www.aclu-il.org/en/campaigns/stop-and-frisk?msclkid=58f8ec82cf6411ec834bbc5063c7972d.

Adena, M., Enikolopov, R., Petrova, M., Santarosa, V., Zhuravskaya, E., 2015. Radio and the Rise of The Nazis in Prewar Germany *. The Quarterly Journal of Economics 130, 1885–1939. URL: https://doi.org/10.1093/qje/qjv030, doi:doi: 10.1093/qje/qjv030, arXiv:https://academic.oup.com/qje/article-pdf/130/4/1885/30637456/qjv030.pdf.

Alesina, A., Tabellini, M., 2024. The political effects of immigration: Culture or economics? Journal of Economic Literature 62, 5–46. URL: https://www.aeaweb.org/articles?id=10.1257/jel.20221643, doi:doi: 10.1257/jel.20221643.

Amarasinghe, A., Raschky, P.A., 2022. Competing for attention – the effect of talk radio on elections and political polarization in the us. URL: https://arxiv.org/abs/2206.13675, arXiv:2206.13675.

Ang, D., 2019. Do 40-year-old facts still matter? long-run effects of federal oversight under the voting rights act. American Economic Journal: Applied Economics 11, 1–53. URL: https://www.aeaweb.org/articles?id=10.1257/app.20170572, doi:doi: 10.1257/app.20170572.

Ang, D., 2023. The birth of a nation: Media and racial hate. American Economic Review 113, 1424–60. URL: https://www.aeaweb.org/articles?id=10.1257/aer.20201867, doi:doi: 10.1257/aer.20201867.

Angrist, J.D., Pischke, J.S., 2008. Mostly Harmless Econometrics: An Empiricist's Companion. Princeton University Press. URL: https://doi.org/10.1515/9781400829828, doi:doi: doi:10.1515/9781400829828.

Ash, E., Galletta, S., 2023. How cable news reshaped local government. American Economic Journal: Applied Economics 15, 292–320. URL: https://www.aeaweb.org/articles?id=10.1257/app.20210501, doi:doi: 10.1257/app.20210501.

Ba, B.A., Knox, D., Mummolo, J., Rivera, R., 2021a. The role of officer race and gender in police-civilian interactions in chicago. Science 371, 696–702. URL: https://www.science.org/doi/abs/10.1126/science.abd8694, doi:doi: 10.1126/science.abd8694.

Barrera, O., Guriev, S., Henry, E., Zhuravskaya, E., 2020. Facts, alternative facts, and fact checking in times of post-truth politics. Journal of public economics 182, 104123.

Barth, F., 1969. Ethnic Groups and Boundaries: The Social Organization of Culture Difference. Little, Brown and Company.

Bazzi, S., Ferrara, A., Fiszbein, M., Pearson, T., Testa, P.A., 2023. The Other Great Migration: Southern Whites and the New Right*. The Quarterly Journal of Economics 138, 1577–1647. URL: https://doi.org/10.1093/qje/qjad014, doi:doi: 10.1093/qje/qjad014, arXiv:https://academic.oup.com/qje/article-pdf/138/3/1577/50689646/qjad014.pdf.

Bernini, A., 2023. The voice of radio in the battle for equal rights: Evidence from the u.s. south. Economics & Politics 35, 163–226. URL: https://onlinelibrary.wiley.com/doi/abs/10.1111/ecpo.12213, doi:doi: https://doi.org/10.1111/ecpo.12213, arXiv:https://onlinelibrary.wiley.com/doi/pdf/10.1111/ecpo.12213.

Bernini, A., Facchini, G., Tabellini, M., Testa, C., 2023a. Black empowerment and white mobilization: the effects of the Voting Rights Act. Technical Report. National Bureau of Economic Research.

Bernini, A., Facchini, G., Tabellini, M., Testa, C., 2024. Sixty years of the voting rights act: progress and pitfalls .

Bernini, A., Facchini, G., Testa, C., 2023b. Race, representation, and local governments in the us south: The effect of the voting rights act. Journal of Political Economy 131, 994–1056. URL: https://doi.org/10.1086/722092, doi:doi: 10.1086/722092, arXiv:https://doi.org/10.1086/722092.

Black, E., Black, M., 1987. Politics and Society in the South. Harvard University Press.

Boone, C., Dube, A., Goodman, L., Kaplan, E., 2021. Unemployment insurance generosity and aggregate employment. American Economic Journal: Economic Policy 13, 58–99. URL: https://www.aeaweb.org/articles?id=10.1257/pol.20160613, doi:doi: 10.1257/pol.20160613.

Bordalo, P., Tabellini, M., Yang, D.Y., 2020. Issue salience and political stereotypes. Technical Report. National Bureau of Economic Research.

Broadcasting, 1962. Broadcasting: The Businessweekly of Television and Radio. Broadcasting Publications Inc. URL: https://www.worldradiohistory.com/Broadcasting-Yearbook.htm.

Broadcasting, 1965. Broadcasting: The Businessweekly of Television and Radio. Broadcasting Publications Inc. URL: https://www.worldradiohistory.com/Broadcasting-Yearbook.htm.

Broadcasting, 1967. Broadcasting: The Businessweekly of Television and Radio. Broadcasting Publications Inc. URL: https://www.worldradiohistory.com/Broadcasting-Yearbook.htm.

Broadcasting, 1969. Broadcasting: The Businessweekly of Television and Radio. Broadcasting Publications Inc. URL: https://www.worldradiohistory.com/Broadcasting-Yearbook.htm.

Broadcasting, 1973. Broadcasting: The Businessweekly of Broadcasting and Allied Arts. Broadcasting Publications Inc. URL: https://www.worldradiohistory.com/Broadcasting-Yearbook.htm.

Buccione, G., Knight, B.G., 2024. The Rise of the Religious Right: Evidence from the Moral Majority and the Jimmy Carter Presidency. Technical Report. National Bureau of Economic Research.

Bullock, C.S., Gaddie, K., 2014. The triumph of voting rights in the South. University of Oklahoma Press.

Bureau, U.C., 2021. Quickfacts: Chicago city, illinois. https://www.census.gov/quickfacts/chicagocityillinois.

Bursztyn, L., Egorov, G., Enikolopov, R., Petrova, M., 2019. Social media and xenophobia: evidence from Russia. Technical Report. National Bureau of Economic Research.

Cameron, A.C., Gelbach, J.B., Miller, D.L., 2011. Robust inference with multiway clustering. Journal of Business & Economic Statistics 29, 238–249. URL: https://doi.org/10.1198/jbes.2010.07136, doi:doi: 10.1198/jbes.2010.07136.

Cantoni, E., 2020. A precinct too far: Turnout and voting costs. American Economic Journal: Applied Economics 12, 61–85. URL: https://www.aeaweb.org/articles?id=10.1257/app.20180306, doi:doi: 10.1257/app.20180306.

Cantoni, E., Pons, V., 2021. Strict Id Laws Don't Stop Voters: Evidence from a U.S. Nationwide Panel, 2008–2018*. The Quarterly Journal of Economics 136, 2615–2660. URL: https://doi.org/10.1093/qje/qjab019, doi:doi: 10.1093/qje/qjab019, arXiv:https://academic.oup.com/qje/article-pdf/136/4/2615/40566481/qjab01

Carmines, E.G., Stimson, J.A., 1989. Issue Evolution: Race and the Transformation of American Politics. Princeton University Press.

Cascio, E.U., Washington, E., 2013. Valuing the Vote: The Redistribution of Voting Rights and State Funds following the Voting Rights Act of 1965 *. The Quarterly Journal of Economics 129, 379–433. URL: https://doi.org/10.1093/qje/qjt028, doi:doi: 10.1093/qje/qjt028,

arXiv:https://academic.oup.com/qje/article-pdf/129/1/379/30626348/qjt028.pdf.

Cash, W., 1941. The Mind of the South. Random House.

Census, 2012. County and city data book [united states] consolidated file: County data, 1947-1977. doi:doi: 10.3886/ICPSR07736.v2.

Chalfin, A., Hansen, B., Weisburst, E.K., Williams, Morgan C., J., 2022. Police force size and civilian race. American Economic Review: Insights 4, 139–58. URL: https://www.aeaweb.org/articles?id=10.1257/aeri.20200792, doi:doi: 10.1257/aeri.20200792.

Chalfin, A., McCrary, J., 2018. Are U.S. Cities Underpoliced? Theory and Evidence. The Review of Economics and Statistics 100, 167–186. URL: https://doi.org/10.1162/REST_a_00694, doi:doi: 10.1162/REST_a_00694.

Cheng, C., Long, W., 2018. Improving police services: Evidence from the french quarter task force. Journal of Public Economics 164, 1–18. URL: https://www.sciencedirect.com/science/article/pii/S0047272718300847, doi:doi: https://doi.org/10.1016/j.jpubeco.2018.05.002.

Cheng, C., Long, W., 2022. The effect of highly publicized police killings on policing: Evidence from large u.s. cities. Journal of Public Economics 206, 104557. URL: https://www.sciencedirect.com/science/article/pii/S0047272721001936, doi:doi: https://doi.org/10.1016/j.jpubeco.2021.104557.

Colella, F., Lalive, R., Sakalli, S.O., Thoenig, M., 2020. Acreg: arbitrary correlation regression.

Colussi, T., Isphording, I.E., Pestel, N., 2021. Minority salience and political extremism. American Economic Journal: Applied Economics 13, 237–271.

Conley, T., 1999. Gmm estimation with cross sectional dependence. Journal of Econometrics 92, 1–45. URL: https://www.sciencedirect.com/science/article/pii/S0304407698000840, doi:doi: https://doi.org/10.1016/S0304-4076(98)00084-0.

Cook, L.D., Jones, M.E.C., Logan, T.D., Rosé, D., 2022. The Evolution of Access to Public Accommodations in the United States*. The Quarterly Journal of Economics 138, 37–102. URL: https://doi.org/10.1093/qje/qjac035, doi:doi: 10.1093/qje/qjac035, arXiv:https://academic.oup.com/qje/article-pdf/138/1/37/47915527/qjac035.pdf.

Corp., R.D., 1959. Radio Annual Television Year Book (1959). New York: Radio Daily Corp. URL: https://www.worldradiohistory.com/Archive-Radio-Annual/Radio_Annual_1959.htm.

Corp., R.D., 1963. Radio Annual Television Year Book (1963). New York: Radio Daily Corp. URL: https://archive.org/details/ra00radi/mode/2up.

Cunningham, D., 2012. Mobilizing ethnic competition. Theory and society 41, 505–525.

Cunningham, D., 2013. Klansville, USA: The rise and fall of the civil rights-era Ku Klux Klan. Oxford University Press, USA.

Dallek, M., 2023. Birchers: How the John Birch Society Radicalized the American Right. Hachette UK.

Danky, J.P., Hady, M.E. (Eds.), 1998. African-American Newspapers and Periodicals: A National Bibliography. Harvard University Press, Cambridge, MA. URL: https://archive.org/details/africanamericann0000unse_w0f6/page/n5/mode/2up.

DellaVigna, S., Enikolopov, R., Mironova, V., Petrova, M., Zhuravskaya, E., 2014. Cross-border media and nationalism: Evidence from serbian radio in croatia. American Economic Journal: Applied Economics 6, 103–32. URL: https://www.aeaweb.org/articles?id=10.1257/app.6.3.103, doi:doi: 10.1257/app.6.3.103.

DellaVigna, S., Gentzkow, M., 2010. Persuasion: Empirical evidence. Annual Review of Economics 2, 643–669. URL: https://doi.org/10.1146/annurev.economics.102308.124309, doi:doi: 10.1146/annurev.economics.102308.124309, arXiv:https://doi.org/10.1146/annurev.economics.102308.124309.

DellaVigna, S., Kaplan, E., 2007. The Fox News Effect: Media Bias and Voting*. The Quarterly Journal of Economics 122, 1187–1234. URL: https://doi.org/10.1162/qjec.122.3.1187, doi:doi: 10.1162/qjec.122.3.1187, arXiv:https://academic.oup.com/qje/article-pdf/122/3/1187/5313017/122-3-1187.pdf.

DellaVigna, S., La Ferrara, E., 2015. Chapter 19 - economic and social impacts of the media, in: Anderson, S.P., Waldfogel, J., Strömberg, D. (Eds.), Handbook of Media Economics. North-Holland. volume 1 of *Handbook of Media Economics*, pp. 723–768. URL: https://www.sciencedirect.com/science/article/pii/B978044463685000019X, doi:doi: https://doi.org/10.1016/B978-0-444-63685-0.00019-X.

Devi, T., Fryer Jr, R.G., 2020. Policing the police: The impact of" pattern-or-practice" investigations on crime. Technical Report. National Bureau of Economic Research.

Di Tella, R., Schargrodsky, E., 2004. Do police reduce crime? estimates using the allocation of police forces after a terrorist attack. American Economic Review 94, 115–133. URL: https://www.aeaweb.org/articles?id=10.1257/000282804322970733, doi:doi: 10.1257/000282804322970733.

Draca, M., Machin, S., Witt, R., 2011. Panic on the streets of london: Police, crime, and the july 2005 terror attacks. American Economic Review 101, 2157–81. URL: https://www.aeaweb.org/articles?id=10.1257/aer.101.5.2157, doi:doi: 10.1257/aer.101.5.2157.

Dube, A., Lester, T.W., Reich, M., 2010. Minimum wage effects across state borders: Estimates using contiguous counties. The review of economics and statistics 92, 945–964.

Dube, A., Lester, T.W., Reich, M., 2016. Minimum wage shocks, employment flows, and labor market frictions. Journal of Labor Economics 34, 663–704. URL: https://doi.org/10.1086/685449, doi:doi: 10.1086/685449.

Durante, R., Pinotti, P., Tesei, A., 2019. The political legacy of entertainment tv. American Economic Review 109, 2497–2530. URL: https://www.aeaweb.org/articles?id=10.1257/aer.20150958, doi:doi: 10.1257/aer.20150958.

East, C.N., Miller, S., Page, M., Wherry, L.R., 2023. Multigenerational impacts of childhood access to the safety net: Early life exposure to medicaid and the next generation's health. American Economic Review 113, 98–135. URL: https://www.aeaweb.org/articles?id=10.1257/aer.20210937, doi:doi: 10.1257/aer.20210937.

Edmund, M., 2022. Gun violence disproportionately and overwhelming hurts communities of color. https://www.americanprogress.org/article/gun-violence-disproportionately-and-overwhelmingly-hurts-\communities-of-color/.

EFSGV, 2020. Nonfatal gun violence. URL: https://efsgv.org/learn/type-of-gun-violence/nonfatal-firearm-violence/#:~:text=Millions%20of%20Americans%20have%20been%20impacted%20by%20nonfatal,injuries%20leave%20lasting%20physical%2C%20psychological%2C%20and%20emotional%20scars.

Engist, O., Matzko, P., Merkus, E., 2024. Conservative talk radio and political persuasion in the us,1950–1970. Journal of Comparative Economics 52, 166–182. URL: https://www.sciencedirect.com/science/article/pii/S0147596723001154, doi:doi: https://doi.org/10.1016/j.jce.2023.12.004.

Enikolopov, R., Petrova, M., 2015. Chapter 17 - media capture: Empirical evidence, in: Anderson, S.P., Waldfogel, J., Strömberg, D. (Eds.), Handbook of Media Economics. North-Holland. volume 1 of *Handbook of Media Economics*, pp. 687–700. URL: https://www.sciencedirect.com/science/article/pii/B9780444636850000176, doi:doi: https://doi.org/10.1016/B978-0-444-63685-0.00017-6.

Enikolopov, R., Petrova, M., Zhuravskaya, E., 2011. Media and political persuasion: Evidence from russia. American Economic Review 101, 3253–85. URL: https://www.aeaweb.org/articles?id=10.1257/aer.101.7.3253, doi:doi: 10.1257/aer.101.7.3253.

Esposito, E., Rotesi, T., Saia, A., Thoenig, M., 2023. Reconciliation narratives: The birth of a nation after the us civil war. American Economic Review 113, 1461–1504. URL: https://www.aeaweb.org/articles?id=10.1257/aer.20210413, doi:doi: 10.1257/aer.20210413.

Evans, W.N., Owens, E.G., 2007. Cops and crime. Journal of Public Economics 91, 181–201. URL: https://www.sciencedirect.com/science/article/pii/S0047272706000831, doi:doi: https://doi.org/10.1016/j.jpubeco.2006.05.014.

Faulkenbury, E., 2019. Poll Power: The Voter Education Project and the Movement for the Ballot in the American South. Justice, Power, and Politics, University of North Carolina Press. URL: https://books.google.com/books?id=Jp-RDwAAQBAJ.

Faulkenbury, T.E., 2016. Poll Power: The Voter Education Project and the Financing of the Civil Rights Movement, 1961-1992. PhD Dissertation.

Fresh, A., 2018. The effect of the voting rights act on enfranchisement: Evidence from north carolina. The Journal of Politics 80, pp. 713–718. URL: https://www.jstor.org/stable/26550900.

Gagliarducci, S., Onorato, M.G., Sobbrio, F., Tabellini, G., 2020. War of the waves: Radio and resistance during world war ii. American Economic Journal: Applied Economics 12, 1–38. URL: https://www.aeaweb.org/articles?id=10.1257/app.20190410, doi:doi: 10.1257/app.20190410.

Garrow, D.J., 1978. Protest at Selma: Martin Luther King, Jr., and the voting rights act of 1965. Yale University Press.

Gavazza, A., Nardotto, M., Valletti, T., 2018. Internet and Politics: Evidence from U.K. Local Elections and Local Government Policies. The Review of Economic Studies 86, 2092–2135. URL: https://doi.org/10.1093/restud/rdy028, doi:doi: 10.1093/restud/rdy028, arXiv:https://academic.oup.com/restud/article-pdf/86/5/2092/29581463/rdy028.pdf.

Gentzkow, M., 2006. Television and Voter Turnout*. The Quarterly Journal of Economics 121, 931–972. URL: https://doi.org/10.1162/qjec.121.3.931, doi:doi: 10.1162/qjec.121.3.931, arXiv:https://academic.oup.com/qje/article-pdf/121/3/931/5409004/121-3-931.pdf.

Gentzkow, M., Shapiro, J.M., 2010. What drives media slant? evidence from u.s. daily newspapers. Econometrica 78, 35–71. URL: https://onlinelibrary.wiley.com/doi/abs/10.3982/ECTA7195, doi:doi: https://doi.org/10.3982/ECTA7195, arXiv:https://onlinelibrary.wiley.com/doi/pdf/10.3982/ECTA7195.

Gerber, A.S., Green, D.P., 2017. Field experiments on voter mobilization: An overview of a burgeoning literature. Handbook of economic field experiments 1, 395–438.

Giavazzi, F., Iglhaut, F., Lemoli, G., Rubera, G., 2023. Terrorist attacks, cultural incidents, and the vote for radical parties: Analyzing text from twitter. American Journal of Political Science .

Goldsmith, A., 2005. Wireless communications. Cambridge university press.

Gramlich, J., 2023. Pew research center. https://www.pewresearch.org/short-reads/2023/04/26/what-the-data-says-about-gun-deaths-in-the-u-s/.

Griffith Jr, B.G., 2009. Carl McIntire: Fundamentalism, civil rights, and the reenergized Right, 1960–1964. West Virginia University.

Grupp, F.W., 1969. The magazine reading habits of political activists. The Public Opinion Quarterly 33, 103–106.

Haines, M.R., 2010. Historical, demographic, economic, and social data: The united states, 1790-2002. doi:doi: 10.3886/ICPSR02896.v3.

Hannan, M.T., 1979. The dynamics of ethnic boundaries in modern states, in: Meyer, J.W., Hannan, M.T. (Eds.), National Development and the World System: Educational, Economic, and Political Change, 1950-1970. University of Chicago Press.

Hausman, D., Kronick, D., 2021. When police sabotage reform by switching tactics. Available at SSRN 3192908 .

Hemmer, N., 2016. Messengers of the right: Conservative media and the transformation of American politics. University of Pennsylvania Press.

Hendershot, H., 2011. What's fair on the air? Cold War right-wing broadcasting and the public interest. University of Chicago Press.

Hornbeck, R., Naidu, S., 2014. When the levee breaks: Black migration and economic development in the american south. The American Economic Review 104, 963–990. URL: http://www.jstor.org/stable/42920725.

Huang, R.R., 2008. Evaluating the real effect of bank branching deregulation: Comparing contiguous counties across us state borders. Journal of Financial Economics 87, 678–705. URL: https://www.sciencedirect.com/science/article/pii/S0304405X07001638, doi:doi: https://doi.org/10.1016/j.jfineco.2007.01.004.

Inc., G.R., 1964. The Finances of the Right Wing. Group Research Inc., Washington D.C.

Kershner, E., 2020. The largest police departments in the us. https://www.worldatlas.com/articles/the-largest-police-departments-in-the-us.html.

Key, V.O., 1949. Southern politics in state and nation. (No Title) .

Kirchmaier, T., Machin, S., Sandi, M., Witt, R., 2019. Prices, Policing and Policy: The Dynamics of Crime Booms and Busts. Journal of the European Economic Association 18, 1040–1077. URL: https://doi.org/10.1093/jeea/jvz014, doi:doi: 10.1093/jeea/jvz014.

Klick, J., Tabarrok, A., 2005. Using terror alert levels to estimate the effect of police on crime. The Journal of Law and Economics 48, 267–279. URL: https://doi.org/10.1086/426877, doi:doi: 10.1086/426877.

Kuziemko, I., Washington, E., 2015. Why did the democrats lose the south? using new data to resolve an old debate. NBER Working Paper 21703.

Kuziemko, I., Washington, E., 2018. Why did the democrats lose the south? bringing new data to an old debate. American Economic Review 108, 2830–67. URL: https://www.aeaweb.org/articles?id=10.1257/aer.20161413, doi:doi: 10.1257/aer.20161413.

LeRoy, W., 2024. Understanding policing in the aftermath of gun violence: Examining investigatory stops and crime in chicago. Journal of Public Economics 234, 105117.

Levitt, S.D., 1997. Using electoral cycles in police hiring to estimate the effect of police on crime. The American Economic Review 87, 270–290. URL: http://www.jstor.org/stable/2951346.

Levitt, S.D., 2002. Using electoral cycles in police hiring to estimate the effects of police on crime: Reply. American Economic Review 92, 1244–1250. URL: https://www.aeaweb.org/articles?id=10.1257/00028280260344777, doi:doi: 10.1257/00028280260344777.

Lopez, B.E., Haskins, P.A., 2021. The fight against rampant gun violence: Data-driven scientific research will light the way. URL: https://nij.ojp.gov/topics/articles/fight-against-rampant-gun-violence-data-driven-scientific-\research-will-light-way.

Martin, G.J., Yurukoglu, A., 2017. Bias in cable news: Persuasion and polarization. American Economic Review 107, 2565–99. URL: https://www.aeaweb.org/articles?id=10.1257/aer.20160812, doi:doi: 10.1257/aer.20160812.

Mastrobuoni, G., 2019. Police disruption and performance: Evidence from recurrent redeployments within a city. Journal of Public Economics 176, 18–31. URL: https://www.sciencedirect.com/science/article/pii/S0047272719300684, doi:doi: https://doi.org/10.1016/j.jpubeco.2019.05.003.

Matzko, P., 2020a. The radio right: How a band of broadcasters took on the federal government and built the modern conservative movement. Oxford University Press, USA.

Matzko, P., 2020b. Talk radio is at the heart of trumpism: [op-ed]. The New York Times .

Mello, S., 2019. More cops, less crime. Journal of Public Economics 172, 174–200. URL: https://www.sciencedirect.com/science/article/pii/S0047272718302305, doi:doi: https://doi.org/10.1016/j.jpubeco.2018.12.003.

Morgan, S.L., Pally, J., 2016. Ferguson, gray, and davis: An analysis of recorded crime incidents and arrests in baltimore city, march 2010 through december 2015 .

Olken, B.A., 2009. Do television and radio destroy social capital? evidence from indonesian villages. American Economic Journal: Applied Economics 1, 1–33. URL: https://www.aeaweb.org/articles?id=10.1257/app.1.4.1, doi:doi: 10.1257/app.1.4.1.

Olzak, S., 1992. The dynamics of ethnic competition and conflict. Stanford University Press.

Pierson, E., Simoiu, C., Overgoor, J., Corbett-Davies, S., Jenson, D., Shoemaker, A., Ramachandran, V., Barghouty, P., Phillips, C., Shroff, R., et al., 2020. A large-scale analysis of racial disparities in police stops across the united states. Nature human behaviour 4, 736–745.

Ruotsila, M., 2015. Fighting fundamentalist: Carl McIntire and the politicization of American fundamentalism. Oxford University Press.

Scheips, P.J., 2005. The role of federal military forces in domestic disorders, 1945-1992. Government Printing Office.

Schuit, S., Rogowski, J.C., 2017. Race, representation, and the voting rights act. American Journal of Political Science 61, 513–526. URL: http://www.jstor.org/stable/26379507.

Shafer, B.E., Johnston, R., 2006. The end of Southern exceptionalism: class, race, and partisan change in the postwar South. Harvard University Press.

Shi, L., 2009. The limit of oversight in policing: Evidence from the 2001 cincinnati riot. Journal of Public Economics 93, 99–113. URL: https://www.sciencedirect.com/science/article/pii/S0047272708001242, doi:doi: https://doi.org/10.1016/j.jpubeco.2008.07.007.

Shjarback, J.A., Pyrooz, D.C., Wolfe, S.E., Decker, S.H., 2017. De-policing and crime in the wake of ferguson: Racialized changes in the quantity and quality of policing among missouri police departments. Journal of Criminal Justice 50, 42–52. URL: https://www.sciencedirect.com/science/article/pii/S0047235217301289, doi:doi: https://doi.org/10.1016/j.jcrimjus.2017.04.003.

Soule, S.A., Dyke, N.V., 1999. Black church arson in the united states, 1989-1996. Ethnic and Racial Studies 22, 724–742. URL: https://doi.org/10.1080/014198799329369, doi:doi: 10.1080/014198799329369, arXiv:https://doi.org/10.1080/014198799329369.

Spenkuch, J.L., Toniatti, D., 2018. Political Advertising and Election Results*. The Quarterly Journal of Economics 133, 1981–2036. URL: https://doi.org/10.1093/qje/qjy010, doi:doi: 10.1093/qje/qjy010.

Tendian, E., NA. Getting started with #chicagoscanner. https://crimeisdown.com/data/ChicagoScannerGuide.pdf?msclkid=61a263fbcf6211ec8054a46d2ae6c274.

Trebbi, F., Aghion, P., Alesina, A., 2008. Electoral rules and minority representation in u.s. cities. The Quarterly Journal of Economics 123, 325–357. URL: http://www.jstor.org/stable/25098900.

USCCR, 1959. Report of the U.S. Commission on Civil Rights, 1959. U.S. Government Printing Office.

USCCR, 1961. Voting: 1961 Commission on Civil Rights Report. U.S. Government Printing Office.

USCCR, 1968. Political Participation: A Report of the United States Commission on Civil Rights. U.S. Government Printing Office.

Valelly, R.M., 2004. The Two Reconstructions: The Struggle for Black Enfranchisement. The University of Chicago Press.

Valentino, N.A., Neuner, F.G., 2017. Why the sky didn't fall: Mobilizing anger in reaction to voter id laws. Political Psychology 38, 331–350. URL: http://www.jstor.org/stable/45095168.

Vollaard, B., Hamed, J., 2012. Why the police have an effect on violent crime after all: Evidence from the british crime survey. The Journal of Law and Economics 55, 901–924. URL: https://doi.org/10.1086/666614, doi:doi: 10.1086/666614.

Wang, T., 2021a. Media, pulpit, and populist persuasion: Evidence from father coughlin. American Economic Review 111, 3064–92. URL: https://www.aeaweb.org/articles?id=10.1257/aer.20200513, doi:doi: 10.1257/aer.20200513.

Wang, T., 2021b. Waves of empowerment: Black radio and the civil rights movement. Technical Report. Working paper.

Wang, T., 2022. Media access and consumption in the civil rights era. AEA Papers and Proceedings 112, 224–28. URL: https://www.aeaweb.org/articles?id=10.1257/pandp.20221103, doi:doi: 10.1257/pandp.20221103.

Washburn, P.S., 2006. The African American newspaper: voice of freedom. Northwestern University Press.

Wasow, O., 2020. Agenda seeding: How 1960s black protests moved elites, public opinion and voting. American Political Science Review 114, 638–659. doi:doi: 10.1017/S000305542000009X.

Watters, P., Cleghorn, R., 1967. Climbing Jacob's Ladder: The Arrival of Negroes in Southern Politics. Harcourt, Brace & World, Inc.

Weisburd, S., 2021. Police Presence, Rapid Response Rates, and Crime Prevention. The Review of Economics and Statistics 103, 280–293. URL: https://doi.org/10.1162/rest_a_00889, doi:doi: 10.1162/rest_a_00889.

Weisburst, E.K., 2018. Safety in Police Numbers: Evidence of Police Effectiveness from Federal COPS Grant Applications. American Law and Economics Review 21, 81–109. URL: https://doi.org/10.1093/aler/ahy010, doi:doi: 10.1093/aler/ahy010.

Wright, G., 1986. Old South, new South: Revolutions in the southern economy since the Civil War. Basic Books.

Wright, G., 2013. Sharing the Prize: The Economics of the Civil Rights Revolution in the American South. Harvard University Press.

Xiong, H., 2021. The political premium of television celebrity. American Economic Journal: Applied Economics 13, 1–33. URL: https://www.aeaweb.org/articles?id=10.1257/app.20190147, doi:doi: 10.1257/app.20190147.

Yanagizawa-Drott, D., 2014. Propaganda and Conflict: Evidence from the Rwandan Genocide *. The Quarterly Journal of Economics 129, 1947–1994. URL: https://doi.org/10.1093/qje/qju020, doi:doi: 10.1093/qje/qju020, arXiv:https://academic.oup.com/qje/article-pdf/129/4/1947/30631615/qju020.pdf.

Zhuravskaya, E., Petrova, M., Enikolopov, R., 2020. Political effects of the internet and social media. Annual Review of Economics 12, 415–438. URL: https://doi.org/10.1146/annurev-economics-081919-050239, doi:doi: 10.1146/annurev-economics-081919-050239, arXiv:https://doi.org/10.1146/annurev-economics-