

DOES NONVIOLENCE WORK?

THE U.S. CIVIL RIGHTS MOVEMENT AND INSTITUTIONAL CHANGE*

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ABSTRACT. I study the effects of pro-civil-rights protests on legislator votes in the U.S. House during the Civil Rights Era. I identify the within-congressional-district effects using a fixed-effects strategy. My findings are threefold. First, peaceful protests made legislators more liberal on civil-rights and welfare issues but not on others, consistent with the goals of the Civil Rights Movement. Second, violent protests had no favorable effects and may even have backfired. Third, peaceful protests prompted the entry of the Republican Party into Southern politics but did not help it win elections. I show that the within-district effects in themselves were large enough to contribute significantly to the passage of major bills. Further, I explore threats to identification and alternative explanations for the results. I show that the effects of peaceful protests are robust and that the observed backfiring effect of violent protests may have been driven by newspaper reporting.

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1. INTRODUCTION

Around 508-507 BCE, Athenian democracy was revived by a mass protest that captured Isagoras, an aspiring tyrant.¹ Protests have served institutional change since then, as seen in the women’s suffrage movement and the U.S. Civil Rights Movement, as well as present-day movements in Hong Kong for democratic institutions and in France against labor law reforms. The most effective protest strategy, however, for many movements has been a topic of debate. Among them, the Civil Rights Movement defines an era of sustained and vibrant protest activity that saw competing ideas emerge about the best strategy.

In a majoritarian democracy, a minority that wants to have a pro-minority policy enacted must persuade the median voter. Critically, the median voter is a member of the majority. For the Civil Rights Movement, the economic incentives for persuasion were strong. Several of the civil-rights and welfare policies implemented in the 1960s had important effects: voting-rights laws increased minority voter turnout and public spending directed toward them (Cascio and Washington, 2014; Ang, 2019), and “war on poverty” laws reduced mortality (Bailey and Goodman-Bacon, 2015) and improved children’s health and economic outcomes in adulthood (Hoynes, Schanzenbach, and Almond, 2016). If the minority chooses to protest in order to persuade, it has two options to do so: peacefully or violently.² Anti-colonial movements had laid out arguments for both (García-Ponce and Wantchékon, 2015), and the thinking of figures such as Mahatma Gandhi and Frantz Fanon shaped the Civil Rights Movement.

Martin Luther King Jr. proposed that violence alienates the majority and thus backfires, whereas peaceful action makes them responsive.³ Malcolm X, by contrast, emphasized the threat inherent in violence when he talked about race riots and hoped that this threat would persuade the majority.⁴ Models of protest and conflict are in keeping with these narratives. In signaling models, the function of protests is to aggregate private information about an unobserved state of the world (Lohmann, 1993, 1994; Battaglini, 2017; Barberà and Jackson, 2017), or to reveal protesters’ unobserved type, thus affecting the median voter’s attitude toward them

¹See Aristotle, *The Athenian Constitution*, Part 20.

²Protests became a potent type of action to persuade the majority with the rise of the television in the late 1950s. But other types of action could also persuade. Writing about the woman suffrage movement, McConaughy (2013) documents suffragists’ state-by-state effort to build coalitions with enfranchised interest groups. This effort led ultimately to the 1920 ratification of the 19th Amendment.

³In 1957, Martin Luther King Jr. warned that violence leads to an inefficient equilibrium: “Men must see that force begets force, hate begets hate, toughness begets toughness. And it is all a descending spiral, ultimately ending in destruction for all and everybody.” In 1965, he described how violence psychologically inhibits cooperation: “Hate does something to the soul that causes one to lose his objectivity. The man who hates can’t think straight; the man who hates can’t reason right; the man who hates can’t see right; the man who hates can’t walk right.” He offered non-violence as an effective alternative, stating: “Love is basic for the very survival of mankind. [...] Love is the supreme unifying principle of life.”

⁴In 1964, Malcolm X advised African Americans to exercise their franchise thoughtfully, and threatened the government with violence if it did not take steps to protect African American civil rights: “In Jacksonville, those were teenagers, they were throwing Molotov cocktails. [...] It’ll be Molotov cocktails this month, hand grenades next month, and something else next month. It’ll be ballots, or it’ll be bullets. It’ll be liberty, or it will be death.”

(e.g., [Simpson, Willer, and Feinberg, 2018](#) on “reasonableness”). In models of conflict, violence threatens to impose costs on the majority ([Ellman and Wantchékon, 2000](#); [De Mesquita, 2010](#); [Besley and Persson, 2011](#); [Aidt and Franck, 2015](#); [Passarelli and Tabellini, 2017](#)). Violence also may affect voters’ propensity to participate in future protests and to turn out to vote, possibly affecting both the minority and the majority ([Enos, Kaufman, and Sands, 2019](#)). Whether these forces tilt the scales towards peaceful or violent protests is an important question.

This paper studies the effects of peaceful and violent pro-civil-rights protests on how politicians represented their congressional districts between 1960 and 1972. Protests may have had two types of effects: local (i.e., within-district) and cross-district. I identify the first of these using a fixed-effects strategy. My measures of representation are votes on the floor of the U.S. House of Representatives on civil-rights and welfare issues, as well as on issues related to crime, the environment, and defense. The identifying assumption is that districts that had protests were not on different trends than those that did not. I also test the robustness of the results to potential violations of this assumption.

I find that peaceful protests changed votes on civil-rights and welfare issues favorably: they pushed legislators in the liberal direction. Indeed, civil-rights and welfare issues were the most closely related to the claims of civil-rights protests. In contrast, violence had no effect on these issues, but evidence suggests that violence pushed representation in the conservative direction on crime-related issues, thereby hurting the movement. Because violence occurred mainly in urban areas, this backfiring effect is attributable to trends specific to large news markets. I find no effects on environmental, nor on defense-related issues, which were not a major focus of civil-rights protests.

Protests prompted the entry of the Republican Party into Southern politics. In the one-party system of the South, before 1960 often no Republican candidate appeared on the ballot. After 1960 this changed, and partly due to the movement. Peaceful protests increased Republican vote shares, and did so in the low end of the vote-share distribution. They also increased election turnout, indicating that the effect was at least partly mediated by the mobilization of new voters. Although I cannot determine whether the increase in Republican votes was driven by voters who supported or opposed civil rights,⁵ my results complement the findings that the reasons white Southerners left Democrats for Republicans were racial sentiments and opposition to civil rights ([Valentino and Sears, 2005](#); [Kuziemko and Washington, 2018](#)).

Counterfactual exercises indicate that protests contributed substantially to the margin with

⁵Nevertheless, the Republican Party took a markedly segregationist turn in 1964. Barry Goldwater, the party’s candidate in the 1964 presidential election, opposed the 1964 Civil Rights Act. Although the conservative and moderate factions of the Republican Party were starkly divided on whether to support Goldwater, he carried Louisiana, Mississippi, Alabama, Georgia, and South Carolina, states that had previously been strongly Democratic.

Meanwhile in the same year, a similar conflict played out within the Democratic Party, as the whites-only Mississippi Democratic Party and the integrated Mississippi Freedom Democratic Party both vied to be sole representatives of the state at the Democratic National Convention.

which several major bills were passed by the House. These margins were affected by three sets of factors: the local effects of protests, the cross-district effects, and other bill-specific factors. Of these, I identify the first. The local effect of peaceful protests increased the margin for the 1964 Economic Opportunity Act by 11.3 percent, the 1965 Elementary and Secondary Education Act by 12.0 percent, the 1968 Civil Rights Act by 27.3 percent, and the 1970 extension of the Voting Rights Act (VRA) by 12.5 percent. Violent protests, in contrast, may have reduced the winning margin for the 1968 Gun Control Act locally by 32.3 percent. The 1964 Civil Rights Act (CRA) and the 1965 VRA benefited less from local peaceful protests, as their margins were increased by only 3.0 percent and 4.6 percent, respectively. This is because the CRA and VRA were passed by large majorities and when the movement was only about midway through.

I assess and reject several alternative explanations for my findings. First, although district fixed effects control for time-invariant endogeneity in the incidence of protests, the findings can be spurious in the presence of differential trends. To absorb differential trends, I construct time-varying controls by interacting the time dummies with fixed district characteristics. Differential trends in racial composition or in political conditions, in black or white economic standing, or in education do not account for the effects of peaceful or violent protests. Second, my measure of protests is drawn from news reports in *The New York Times* which largely sourced its news from local newspapers (Roberts and Klibanoff, 2006). It appears that editors of some local papers were biased against the movement. As such, from markets in which only one local paper existed and was biased, the *Times* was likely to obtain biased information or none at all, leading to misclassification or underreporting of protests. I thus examine whether this affects my findings. I absorb differential trends for larger news markets, which leaves the effects of peaceful protests unchanged, and I find that the effect of violent protests is attributable to trends in larger news markets. Third, the legislative agenda itself may have changed in response to national protest activity. If representatives' policy positions remained the same but the new agenda magnified differences between representatives of districts with protests compared to those without, the findings are spurious. This last explanation implies heterogeneity in protest effects over time, which the data do not support.

This paper makes three major contributions. It shows that peaceful protests had substantial and favorable effects on lawmaking in the House, that violent protests had no such effects, and that peaceful protests foreshadowed the Southern realignment of Democrats and Republicans. Wasow (2020) shows that peaceful protests shaped public opinion, media coverage, and speeches in Congress, as well as increased the presidential vote shares of Democratic candidates in 1964, 1968, and 1972, whereas protester violence moved the focus towards social control. My paper shows that these effects went further and translated into changes in legislation. Kuziemko and Washington (2018) and Valentino and Sears (2005) establish that the Southern realignment began as Democratic vote shares were eroded by Northern Democrats' embrace of civil rights in 1963, and that its culmination after 1972 was linked to white Southerners' sentiment toward

blacks. Contributing to this understanding, I show that Republican gains began in the 1960s and were affected by peaceful protests. A varied literature has found protests to be effective in other contexts as well. These include corporate activism (Harrison and Scorse, 2010), congressional politics (Gillion, 2012; Madestam et al., 2013; Mazumder, 2018), the economic and political effects of race riots (Collins and Margo, 2004, 2007; Cunningham and Gillezeau, 2018; Enos, Kaufman, and Sands, 2019), and the dynamics of movement participation (Cantoni et al., 2019; Bursztyn et al., 2019). This literature, however, has not contrasted peaceful means with violence.

The remainder of the paper is structured as follows. Section 2 provides the historical context and illustrates the strategic decision to protest. Sections 3 and 4 present the data and introduce the empirical specification, and Section 5 discusses the results. Section 6 shows the robustness of the findings to alternative explanations. Section 7 discusses the counterfactual outcomes under no protests, and Section 8 concludes.

2. THEORETICAL MOTIVATION

All protest movements aim to affect institutions but they vary in their contexts and goals. This section describes the background of the Civil Rights Movement and discusses protesters' strategic considerations, to provide the basis for identification in Sections 5 and 6.

2.1. Historical context. The U.S. Civil War was fought fundamentally over the abolition of slavery (Aldrich and Griffin, 2018; Hall, Huff, and Kuriwaki, 2019). Abolition, however, brought African Americans only a brief period of freedom. After Reconstruction, a set of institutions gradually emerged in the South that came to be known as Jim Crow. These institutions aimed to disfranchise African Americans, and to restrict their economic mobility so as to keep them in low-wage occupations.

Jim Crow accomplished the first aim by requiring and discretionally applying literacy tests, requiring the payment of poll taxes, and exempting white voters from both requirements through grandfather clauses, as well as by choosing candidates in white-only primary elections in the Southern Democratic Party and not prosecuting lynchings to suppress political participation.⁶ Toward the second aim, these institutions segregated both the public school system and universities by race, channeled funding away from black schools (Margo, 1990; Aaronson and Mazumder, 2011; Naidu, 2012), established the system of debt peonage (Ransom and Sutch, 2001), and maintained segregationist norms in the labor market (Dewey, 1952; Wright, 2013).

For a long time, advocacy groups chose the path of piece-by-piece legal challenge to dismantle Jim Crow. Through these efforts, grandfather clauses were declared unconstitutional by the

⁶Aldrich and Griffin (2018, p. 106) state: "Lynchings were a common tactic designed to terrorize blacks and of course eliminate particularly outspoken opponents of Jim Crow. [...] blacks who tried too hard to register and vote or, worse, engaged in politics more fully, were sometimes lynched themselves." The authors quote South Carolina governor and senator "Pitchfork" Ben Tillman who declared, "We stuffed ballot boxes. We shot them. We are not ashamed of it."

Supreme Court in 1915 (*Guinn v. United States*). Subsequently, white primaries took two decades of legal work to abolish. In decisions in 1927, 1932, and 1935 the Supreme Court found various implementations of white primaries constitutional and unconstitutional. It was not until 1944 that the court's ruling put an end to the exclusion of blacks in primary elections (*Smith v. Allwright*). The well-known 1954 ruling in *Brown v. Board of Education* that found against school segregation took over half a century to achieve. In 1896, the Supreme Court found that the segregation of public facilities was consistent with the Equal Protection Clause of the Fourteenth Amendment (*Plessy v. Ferguson*). Thirty-one years later, in 1927 the court upheld this decision and ruled it constitutional to deny a Chinese American student enrollment in a white-only Mississippi school (*Lum v. Rice*). These rulings and the fact that, despite the court's 1954 decision, the South was ready to maintain school segregation for another fifteen years⁷ are a testament to the overwhelming resistance of the *de facto* institutions of the South to *de jure* civil-rights protections.

The Great Migration, the slow economic betterment of African Americans, the declining reliance of agriculture on manual labor, and the rapid diffusion of the television led to the late 1950s as a turning point in civil-rights groups' approach, giving way to protests rather than court action (McAdam, 2010). This was foreshadowed by the 1947 Journey of Reconciliation which challenged segregation on interstate bus lines by testing *Morgan v. Virginia*. Yet, the first major protest event, the year-long Montgomery bus boycott, came only in late 1955, and the Little Rock Nine's widely publicized attempt to integrate Little Rock Central High came in 1957. With the shift in methods, new movement organizations came to the fore. While the National Association for the Advancement of Colored People (NAACP) had been central to the movement's achievements in court, now the Congress of Racial Equality (CORE, formed in 1942), the Southern Christian Leadership Conference (SCLC, 1957), and the Student Nonviolent Coordinating Committee (SNCC, 1960) began mobilizing for sit-ins, rallies, and marches. Although these organizations were committed to remaining peaceful, others, such as the Black Panthers and the black supremacist Nation of Islam, were vocal about their belief that without the threat of violence, the oppression of blacks would continue.⁸ Further, whereas early protests aimed to put pressure on local and state authorities, television ownership among U.S. households reached 87 percent by 1960, which changed the nature of news consumption, opening the way for protests to effectively influence national public opinion.

The movement's efforts were not without reward. They sparked a series of legislation that is seen as one of the most significant policy changes in the United States during the 20th

⁷See *Green v. County School Board of New Kent County* (1968) and *Swann v. Charlotte-Mecklenburg Board of Education* (1971) for landmark Supreme Court rulings that led to court-ordered busing to facilitate school integration.

⁸Zinn (2002, p. 223), who advised SNCC, also thought that the threat of violence may be necessary: "To insist on perfect tranquility with an absolute rejection of violence may mean surrendering the right to change an unjust social order. On the other hand, to seek justice at any cost may result in bloodshed so great that its evil overshadows everything else and splatters the goal beyond recognition."

century.⁹ The 1964 Civil Rights Act, the 1965 Voting Rights Act, and the 1968 Fair Housing Act collectively outlawed racial discrimination in public accommodations, education, the labor market, electoral participation, and the housing market. These acts radically altered both employment norms (Donohue and Heckman, 1991; Wright, 2013) and the provision of public goods in the South. Further, voting-rights protections increased minority turnout (Ang, 2019), and the re-enfranchisement of African Americans led to increased local government spending targeted towards them (Cascio and Washington, 2014), and to congressional representation that was more supportive of civil rights (Schuit and Rogowski, 2017).

2.2. A model of protests. By 1960 it was clear that to see the legal protections that the Supreme Court adjudicated, it was essential to persuade the federal government to enforce them. Civil-rights organizations thus moved from legal challenge to protest, and were faced with two questions: (i) where should they protest and (ii) should they protest peacefully or violently?

The answers depended on the local and cross-district effects of protests, the cost of organizing, and the cost of potential backlash. A simple model that illustrates this considers three districts and a single agent who decides to protest. The agent’s goal is to persuade at least two of the three districts to support her policy. Suppose that the first district, ℓ_1 , is liberal and thus supports the agent’s policy, the second district, ℓ_2 , is mildly conservative and persuadable, and the third district, ℓ_3 , is very conservative and resists change. Any protest, whether peaceful or violent, invites violent backlash against the agent in ℓ_3 but not in ℓ_1 and ℓ_2 .

If the agent considers only the local effect, that is, the effect that her protest has within the same district, she never protests in ℓ_3 nor in ℓ_1 . Thus, the only district that can be persuaded locally is the mildly conservative ℓ_2 .

Cross-district effects can incentivize the agent to protest in ℓ_1 and ℓ_3 . Whether she so does depends on the local effect in ℓ_2 , the size of the cross-district effects, and the costs that she incurs. The backlash in ℓ_3 could be costly, but if the local effect in ℓ_2 is too small whereas the cross-district effect is large enough, the agent might protest in ℓ_3 to persuade ℓ_2 . Similarly, protest in ℓ_1 could have a cross-district effect that is enough to persuade ℓ_2 .

The choice between peace and violence comes down to whether one or the other has more favorable local and cross-district effects and costs. Violence as a threat is effective if the median voter’s response in ℓ_1 is to continue to support the agent’s policy and if in ℓ_2 it is to concede and also support it. It backfires, however, if the response instead is to increase social control.

This model tells us that whether protests occurred in a district depended on its political conditions, the cost of organizing, and the cost of backlash. This drives the empirical strategy of the paper. District fixed effects control for conditions and costs to the extent that they were

⁹In a 1999 Gallup poll, 58 percent of Americans listed the 1964 Civil Rights Act (CRA) as one of the most important events of the 20th century. This makes the CRA second amongst legislative events only to the 1920 ratification of the 19th Amendment, which recognized women’s right to vote. (Source: <https://news.gallup.com/poll/3427/most-important-events-century-from-viewpoint-people.aspx>, accessed on March 20, 2019.)

constant over time. Furthermore, changes in one district relative to another (i.e., differential trends) are not captured by fixed effects. The potential presence of such changes motivates the robustness checks in Section 6.

Finally, the local and cross-district effects on the median voters were a combination of the persuasion and mobilization of liberal and conservative voters. Thus in addition to effects on representation, we can expect effects on turnout and vote shares as well.

3. DATA

Section 2 presented how protests may have had a causal effect on lawmaking and why the choice between peace and violence may have mattered. The goal of Section 5 is to identify and estimate all such causal effects, characterizing lawmaking by legislator votes and election outcomes. In this section, I discuss the data that I use to measure these outcomes as well as protest activity and district characteristics.

3.1. Lawmaking.

3.1.1. Legislator votes in the U.S. House. I construct a range of legislator voting scores to measure representation. Each of these voting scores is the share of conservative votes among all the votes that the legislator cast on a particular set of roll calls.¹⁰ The sets of roll calls that I look at correspond to issue themes. I follow the roll-call classification of [Crespin and Rohde \(2018\)](#) and construct voting scores for four issue themes: (i) civil rights and welfare, (ii) crime, (iii) environment, and (iv) defense.

To determine whether *yea* or *nay* was the conservative position on a roll call, I rely on DW-NOMINATE ([Poole and Rosenthal, 1985, 2007; McCarty, 2011](#)). This methodology places voting in a two-dimensional issue space and estimates the issue position of each legislator (most often referred to as their *ideology* or *ideal point*) and of the *yea* and *nay* positions on each roll call. The two dimensions have no predefined meaning—in this, DW-NOMINATE is similar to a factor analytical approach to roll call voting ([Heckman and Snyder, 1997](#)). Whereas the first dimension reflects broader differences on the liberal–conservative scale, the second dimension is generally interpreted as picking up differences on regional issues (e.g., in the 1960s, segregation).

Although directly regressing legislator positions in the second dimension might present itself as the obvious choice to study effects on civil rights, this approach turns out to perform poorly. Because the meaning of the dimensions is not fixed, both the second *and the first* dimension encode relevant information about issues related to the Civil Rights Movement. Moreover, the

¹⁰Formally, I define a voting score as $s_{\ell t} := |\mathcal{R}(t)|^{-1} \sum_{r \in \mathcal{R}(t)} \mathbb{1}(\text{Vote}(\ell r) = \text{ConservativeVote}(r))$ where $\mathcal{R}(t)$ is the set of roll calls that were brought to the floor in a particular issue theme during congress t , $\text{Vote}(\ell r) \in \{\text{Yea}, \text{Nay}\}$ indicates the vote cast by the representative of district ℓ on roll call r , and $\text{ConservativeVote}(r) \in \{\text{Yea}, \text{Nay}\}$ indicates the conservative position on r .

first dimension carries the bulk of the explanatory power of DW-NOMINATE. Using the second dimension as a proxy for legislator positions on civil rights introduces substantial measurement error in the outcome. In turn, the disadvantage of regressing the first dimension is that it is an overall measure of legislator conservatism, not restricted to civil rights.

Instead, I proceed by taking the estimated *yea* and *nay* positions of each *roll call*. I define *yea* as the conservative position if DW-NOMINATE estimates it as more conservative than *yea* in the first dimension. I then calculate the voting scores as the shares of roll calls in the four issue themes that each legislator voted conservatively on. These voting scores serve as the main outcomes.

As alternative outcomes, I examine interest-group ratings of legislators. I use the ratings of the Americans for Constitutional Action (ACA) and the Americans for Democratic Action (ADA). These groups selected roll calls for each congress to construct evaluations of legislators. Each legislator was assigned a score by ACA, and another score by ADA. Analogous to my main outcomes, the interest-group ratings are calculated as the share of roll calls on which the legislator voted according to ACA's and ADA's positions. Figure 1 shows the party means of ACA and ADA scores for the 11 ex-Confederate states and elsewhere.

3.1.2. Election outcomes. For general elections for U.S. House seats, I obtain the party vote counts and compute the winning candidate's margin of victory from the *Candidate Name and Constituency Totals, 1788–1990* (ICPSR 2) data set. I obtain county-level election turnout from the *Electoral Data for Counties in the United States: Presidential and Congressional Races, 1840–1972* (ICPSR 8611) data set.

Table 1 shows election outcomes for Republican candidates in the average district in the sample, nationally as well as broken down by whether the district was in the South and whether it was more urbanized than the median district.¹¹ In the elections for the 89th Congress (Panel A), Republicans achieved a 42.94 percent average vote share outside the South but only 27.16 percent inside the South. This was partly because Republican candidates were less likely to run in Southern districts: only 71.43 percent of Southern districts had a Republican candidate, in contrast with 98.02 percent of those outside the South. This pattern is comparable to that by urbanization: 94.97 percent of districts with above-median urbanization, but only 89.36 percent of those that were below the median, had a Republican candidate. At 41.50 percent against 37.64 percent, however, Republicans still achieved a higher average vote share in less urbanized districts.

Slightly more Southern districts, 73.53 percent, had a Republican candidate by the 92nd Congress (Panel B). Republican vote shares also increased in the South. The gap between Southern and non-Southern Republican vote shares, however, barely changed, from 15.78 to 13.66 percentage points.

¹¹I measure urbanization as the district's population share inside the central city of a metropolitan area.

3.2. Protest events. To measure protest activity, I use McAdam, McCarthy, Olzak, and Soule’s *Dynamics of Collective Action* (DOCA) data set. DOCA covers protest events in the United States between 1960 and 1995, and the source of all information in DOCA is *The New York Times*. Among other characteristics, it codes certain details, including the reported race of participants, the claims that were identified, estimates of the number of protesters, whether the protesters used violence, and whether there was any property damage. Further, DOCA covers not only civil-rights protests but also riots and war protests.

I construct district-level protest counts by aggregating from the county level. I begin by counting county-level pro-civil-rights protests in the DOCA data. I then aggregate as follows. First, I intersect county and district boundaries. Second, I calculate the estimated population size in each intersection. Third, for each district, I compute the population-weighted average of county-level counts of each intersection that belongs to the district. This yields district-level protest counts that pool peaceful and violent protests.

To get counts of violent protests, I repeat this procedure for the subset of events in which protesters were indicated to have used violence. Such indications include whether the protesters threw bricks, firebombs, and so on, whether property damage was reported, or whether the event was a race riot. The reason that I include race riots is that they were responses to institutional racial inequality, and they shaped public opinion and policy (Button, 1978). Counts of peaceful protests follow as the difference between the pooled counts and the violent-protest counts.

The assumption implicit in this procedure is that protests affected everyone equally within the county and affected no one across county boundaries. Under this assumption, the resulting district-level protest measure can be interpreted as the number of protests to which the average resident of the district was exposed.

I construct measures of anti-civil-rights and war protests in the same way. Figure 2 shows the evolution of the resulting protest measures in the average district in the South and outside the South.

Table 1 shows the peaceful and violent protest histories of the average district. By the 92nd Congress, the last period in the sample, the average district had 20.51 peaceful protests and 6.58 violent protests. The majority of districts experienced both kinds: 77.36 percent of districts had at least some peaceful and 65.72 percent had at least some violent protests. Most of the recorded protest activity, however, was focused outside the South. The average district had 23.15 peaceful protests outside the South but only 10.79 in the South and, similarly, 7.75 violent protests outside the South but only 2.27 in the South. The breakdown by urbanization shows a similar picture, with more urbanized districts accounting for the majority of recorded protest activity.

Much of the protest activity of the Civil Rights Era did not unfold until the second half of the decade. The average district had only 10.22 peaceful and 0.74 violent protests by the 89th Congress (Panel A). These are 50 and 11 percent of the corresponding average protest counts

by the 92nd Congress (Panel B).

3.3. Other district characteristics. In the robustness checks in Section 6, I use a number of fixed district characteristics: radio ownership, phone ownership, the population share of blacks, educational attainment, unemployment, employment in agriculture, average family income, and the population share of those living in metropolitan areas. I obtain these from the 5-percent Integrated Public Use Microdata Series (IPUMS) sample of the 1960 census.

The smallest observable geographical unit in the 5-percent sample is what IPUMS calls a *mini-PUMA*. A mini-PUMA is a geographical block with a population of at least 50,000. Like counties, mini-PUMAs do not perfectly align with congressional districts. To assign each characteristic to districts, I intersect mini-PUMA boundaries with district boundaries, and whenever a mini-PUMA intersects with multiple districts, I calibrate the weights assigned to these intersections to minimize the discrepancy between the implied population estimate for the district and the actual district population. For this calibration, I get a measure of the actual district population from Adler (2018).

I also use data on the number of newspapers and newspaper circulation. These come from the *United States Newspaper Panel, 1869–2004* (ICPSR 30261) data set at the county level, and I aggregate them to the district level in the same way as with the other characteristics.

3.4. Sample size. The sample has 1,798 observations from 46 U.S. states. The unit of observation is district-by-congress. Two of the four states that are not in the sample, Alaska and Hawaii, are dropped because they did not vote in the 1948 presidential election. The remaining two states, Rhode Island and Vermont, are dropped because they do not have voter turnout estimates for the majority of the decade.¹²

Many of the 46 states that are in the sample have missing time periods due to redistricting. The U.S. Supreme Court’s landmark decisions in the 1962 *Baker v. Carr* case and subsequent cases prompted waves of redistricting. Boundaries were redrawn in 25 states for the 88th, 8 states for the 89th, 24 states for the 90th, 17 states for the 91st, and 6 states for the 92nd Congresses. Some states redrew district boundaries for consecutive congresses. In such states, districts between two consecutive waves existed only for one time period. The district fixed effects absorb all variation in these districts and, therefore, they need to be dropped.

4. EMPIRICAL STRATEGY

This section describes how protest activity in each two-year congress is aggregated into separate peaceful and violent protest histories. With the protest histories as a foundation, it then presents the main specification and the identifying assumptions.

¹²Voter turnout is one of the main outcomes in Section 5. Vote shares in the 1948 presidential election are used in the robustness checks in Section 6.

4.1. Notation and timing. I denote the outcome variable by $Y_{\ell t}$. For ease of exposition, consider an aggregate measure of protest history that pools peaceful and violent protests, denoted by $A_{\ell t-1}$. ℓ indexes congressional district and t indexes a two-year congressional term.

To understand timing, take the 89th Congress as an example. The 89th Congress met between January 3, 1965, and January 3, 1967. Elections for this congress were held on November 3, 1964. For measures of legislator ideology, $Y_{\ell 89}$ is constructed from roll-call votes during the 89th Congress of the legislator who represented ℓ . For election outcomes, e.g., Democratic vote share, $Y_{\ell 89}$ refers to the vote share in the November 3, 1964, elections. For measures of protests, e.g., $A_{\ell t-1}$, $A_{\ell 88}$ refers to the history of protest activity until November 3, 1964.

Therefore $Y_{\ell t}$ is always determined after $A_{\ell t-1}$. For legislator ideology, $Y_{\ell t}$ and $A_{\ell t}$ are contemporaneous. For election outcomes, $Y_{\ell t}$ is determined before $A_{\ell t}$.

4.2. Constructing protest history. I compute a population-weighted event count for each district ℓ during each two-year term t , as described in Section 3. This event count is denoted by $T_{\ell t}$. The aggregate measure of protest history, $A_{\ell t-1}$, is constructed as the cumulative sum of district ℓ 's past event counts, $\sum_{s \leq t-1} T_{\ell s}$. This formulation incorporates two assumptions: (i) $Y_{\ell t}$ has a memory—that is, it integrates not only events that occurred in the previous period but also earlier events—and (ii) $Y_{\ell t}$ has a constant marginal response to an additional event. In specification checks, I show that both (i) and (ii) are supported by the data.

The disaggregation by the use of violence is as follows. I first compute average counts of events that exhibit markers of violence by protesters. This is denoted by $T_{\ell t}^V$.¹³ Then I get an event count of peaceful protests as $T_{\ell t}^P := T_{\ell t} - T_{\ell t}^V$. In turn, my measures of protest history are $P_{\ell t-1} := \sum_{s \leq t-1} T_{\ell s}^P$ for events with peaceful protesters, and $V_{\ell t-1} := \sum_{s \leq t-1} T_{\ell s}^V$ for events with protester violence.

4.3. Specification. All of the specifications that I estimate distinguish between peaceful and violent protests and, therefore, replace $A_{\ell t-1}$ with two measures of protest history, $P_{\ell t-1}$ and $V_{\ell t-1}$. My main specification is

$$Y_{\ell t} = \beta_1 P_{\ell t-1} + \beta_2 V_{\ell t-1} + \lambda_{\ell} + \theta_{\text{South}(\ell) \times t} + \mathbf{X}'_{\ell t} \boldsymbol{\gamma} + U_{\ell t}. \quad (1)$$

λ_{ℓ} and $\theta_{\text{South}(\ell) \times t}$ are district and South/non-South time fixed effects, and $\mathbf{X}_{\ell t}$ is a vector of controls. For all specifications, this includes the district's anti-civil-rights and war protest histories. I augment the controls further in the robustness checks.

Table 13 shows estimates from specification checks and supports the main specification. These checks separate the first lag from the sum of the higher-order lags of protest activity. Thus they regress $Y_{\ell t-1}$ on $(T_{\ell t-1}^P, \sum_{k>1} T_{\ell t-k}^P)$ and $(T_{\ell t-1}^V, \sum_{k>1} T_{\ell t-k}^V)$, as opposed to the main

¹³I compute the average counts of (i) events that were riots or in which protesters were reported to have used violence, and (ii) events in which property damage was reported. To obtain $T_{\ell t}^V$, I take the average of (i) and (ii).

specification which regresses $Y_{\ell t-1}$ on $P_{\ell t-1} := \sum_{k>0} T_{\ell t-k}^P$ and $V_{\ell t-1} := \sum_{k>0} T_{\ell t-k}^V$. As I will discuss in Section 5, protests had no statistically significant effects on environmental and defense-related issues. Only on civil-rights and welfare issues did peaceful protests, and only on crime-related issues did violent protests have a significant effect. The corresponding estimates in Table 13 show that the memory assumption and the constant marginal response assumption, implied by the linear-sum specifications of $P_{\ell t-1}$ and $V_{\ell t-1}$, are appropriate. The point estimates are close to identical for the last-period peaceful protests ($T_{\ell t-1}^P$) and for the prior history of peaceful protests ($\sum_{k>1} T_{\ell t-k}^P$) on civil rights and welfare (column (1)). Likewise, for violent protests, the estimates are statistically indistinguishable from each other on crime (column (4)).

4.4. Identification. As discussed in Section 2, the incidence of peaceful and violent protests was driven by three factors: (i) the district’s prevailing political conditions, (ii) the cost of protest organizing, and (iii) the cost of potential violent backlash. The time-invariant components of (i)–(iii) are captured by the district fixed effect in equation (1). Further, (i) manifests as heterogeneous treatment effects across districts. Estimates of equation (1) capture an average of these treatment effects.

The threat to identification comes from the time-varying components of (i)–(iii). These appear, however, to drive a negligible portion of the endogeneity of peaceful protests. I test a wide array of specifications for the time-varying components and find that these do not change my conclusions. I discuss these specifications in detail in Section 6.

4.5. Overall effects. Under no underreporting, β_1 and β_2 show the marginal effects of protests. The presence of underreporting, however, even if non-differential, renders this interpretation impossible as the marginal effects are not identified. Both due to this and for easier interpretability, I discuss the effects of protests in terms of what I call the overall effects.

The overall effects show the impact that protests had in the average district in my sample by the end of the Civil Rights Era. They follow from taking expectation of equation (1) in the 92nd Congress:

$$\mathbb{E}(Y_{\ell 92}) = \underbrace{\beta_1 \mathbb{E}(P_{\ell 91})}_{\text{overall effect of peaceful protests}} + \underbrace{\beta_2 \mathbb{E}(V_{\ell 91})}_{\text{overall effect of violent protests}} + \mathbb{E}(\lambda_{\ell}) + \mathbb{E}(\theta_{\text{South}(\ell) \times 92}) + \mathbb{E}(\mathbf{X}'_{\ell 92})\gamma. \quad (2)$$

By focusing on these, I evaluate the effects of the Civil Rights Movement as a whole rather than the effects of the marginal protest in the era.

The overall effects are robust to a general form of unobserved underreporting. Let $P_{\ell t-1}^*$ be the true peaceful protest history and β_1 its true marginal effect, and let the observed protest history be related to it as $P_{\ell t-1} := \alpha_1 P_{\ell t-1}^*$. With $\alpha_1 > 0$ an arbitrary, unobserved, misreporting parameter, the fixed-effects estimator $\hat{\beta}_1$ converges in probability to β_1/α_1 . The overall effect of peaceful protests, however, is estimated consistently because the average of $P_{\ell 91}$ across districts ℓ

converges to $\mathbb{E}(P_{\ell 91}) = \alpha_1 \mathbb{E}(P_{\ell 91}^*)$ and, therefore, α_1 cancels out. As the counterfactual exercise in Section 7 also relies on the overall rather than the marginal effects, it is also robust to unobserved underreporting of this form.

5. THE EFFECTS OF PROTESTS

With the specification laid out, I now turn to discussing the estimates.

5.1. Legislator votes in the House. Table 2 shows coefficient estimates from regressing legislator voting scores. Column (1) implies a 2.60 percentage-point (pp; 95-percent confidence interval: $[-4.65 \text{ pp}, -.55 \text{ pp}]$) overall shift in the liberal direction on civil-rights issues in response to peaceful protests. The effect on welfare issues was similar: a 3.30 pp ($[-5.22 \text{ pp}, -1.29 \text{ pp}]$) overall liberal shift. The point estimates suggest a conservative shift in response to violent protests but the confidence intervals include zero: by 1.53 pp on civil rights ($[-.43 \text{ pp}, 3.48 \text{ pp}]$) and by 1.30 pp on welfare ($[-.69 \text{ pp}, 3.26 \text{ pp}]$). Because both peaceful and violent protests had similar effects on civil-rights and welfare issues, I pool these issues into a single theme. Column (3) implies that peaceful protests led to a 3.25 pp liberal shift on the combined theme ($[-5.22 \text{ pp}, -1.29 \text{ pp}]$). The point estimate for violent protests implies a 1.38 pp conservative shift ($[-.48 \text{ pp}, 3.24 \text{ pp}]$).

Conversely, peaceful protests had no clear effect on crime-related issues (column (4); $-.09 \text{ pp}$, $[-4.06 \text{ pp}, 3.89 \text{ pp}]$) but the estimate for violent protests implies a large conservative shift by 4.35 pp ($[2.42 \text{ pp}, 6.28 \text{ pp}]$). As I will discuss in Section 6, this effect of violent protests is attributable to differential trends in large news markets. Race riots were very likely to occur in urban centers which offers an explanation of this pattern. Although, strictly speaking, this failure of robustness does not rule out that the estimate is causal, at the least it calls it into question.

The ACA and ADA interest-group ratings reflect the effects that were found separately on civil-rights and welfare as well as on crime issues. The estimates for peaceful protests are similar in size but not statistically significant (-1.89 pp and 2.05 pp , respectively). The estimates for violent protests are large and indicate a conservative shift: by 2.57 pp on the ACA score ($[.34 \text{ pp}, 4.81 \text{ pp}]$) and by 3.46 on the ADA score ($[-5.62 \text{ pp}, -1.31 \text{ pp}]$). Overall, these patterns validate the voting scores that I use for the analysis and justify calculating detailed theme-specific voting scores rather than relying solely on interest-group ratings.

Protests had no effects on legislator votes on environmental or defense issues. A priori, we might have expected effects on environmental issues as the 1960s were arguably the beginning of the environmental justice movement (Bullard and Johnson, 2000). The point estimates, however, indicate no effects on this theme ($-.17 \text{ pp}$ for peaceful and $.59 \text{ pp}$ for violent protests). The voting score on defense issues serves as a placebo, and the point estimates are again very near zero ($.80 \text{ pp}$ for peaceful and $-.27 \text{ pp}$ for violent protests).

I test for the presence of interaction effects between peaceful and violent protests. None of the four issue themes shows such effects (Table 7, Panel B). In terms of the overall effect, the point estimates for civil rights and welfare and for crime indicate magnitudes no larger than .36 pp. As such, a potential strategic use of violence that would have aimed to force legislators to favorably respond to peaceful protests was not effective.

5.2. Election outcomes. The estimates indicate that peaceful protests increased turnout by an overall 1.11 pp (Panel A, column (1); [.23 pp, 2.00 pp]) and violent protests by .71 pp ([.08 pp, 1.34 pp]). This increased turnout helped Republican candidates. Peaceful protests increased the vote shares of Republicans by 1.51 pp (column (3); [.01 pp, 3.02 pp]) and decreased those of Democrats by 1.59 pp (column (2); [-3.08 pp, -.11 pp]). These effects imply that peaceful protests had a mobilization effect.

This mobilization effect benefited Republicans, and it benefited them in districts where their support was previously low. This was especially true in the South, where peaceful protests increased turnout by 5.50 pp (Table 4, Panel A, column (1); [2.86 pp, 8.14 pp]) and Republican vote shares by 14.61 pp (column (3); [9.07 pp, 20.16 pp]), with an almost equal opposite effect on Democratic vote shares (column (2); [-13.05 pp, [-18.72 pp, -7.39 pp]). Peaceful protests made it more likely that a Republican candidate would run in the district (column (4); 36.45 pp, [20.92 pp, 50.98 pp]). However, they made Republicans no more likely to win (column (5); -.92 pp, [-5.59 pp, 3.75 pp]).

The effects on the cumulative distribution functions further illustrate this point. For Southern Republicans, peaceful protests shifted probability mass from the low end of the support and, for Southern Democrats, shifted mass from the high end. To show this, I estimate a variant of the main specification (1) on the Southern subsample that replaces the vote share $Y_{\ell t}$ with the binary variable $\mathbb{1}(Y_{\ell t} \leq y_a)$ where y_a is the a -th decile of the distribution of $Y_{\ell t}$ (Duflo, 2001). Figure 3 shows the estimated overall effects and 95-percent pointwise confidence intervals for peaceful protests at each decile. Peaceful protests fostered the entry of Republicans into Southern politics but did not help Republicans get enough votes to win (Panel A). Conversely, peaceful protests eroded the partisan advantage of Democrats but not by enough that they would lose (Panel B).

As a consequence, elections became more competitive as measured by the winning candidate’s margin, although this effect was not statistically significant for the country as a whole (Table 3, column (4)). And whereas Democrats as a party did not lose their power due to the protests, individual politicians in the South were affected. To show this, I look at effects on the reelection of incumbents.

I call those legislators incumbent who were members of the 86th Congress (1959–1960), the congress that directly preceded my sample. These legislators were 21.78 pp less likely to serve in the 92nd Congress (1971–1972) due to peaceful protests (Table 4, Panel A, column (6); [-38.22 pp, -5.35 pp]). This definition of incumbency is preferable to the conventional one (i.e.,

legislators who represented the same district in the previous congress). Due to redistricting during the decade, the conventional definition leads to dropping a substantial number of districts which appear for only two congresses in the sample.

5.3. Incumbent response. The results in Subsection 5.1 show that protests meaningfully changed average legislator behavior. It is not clear, however, whether this effect was due to only newly elected legislators or whether protests shifted incumbents to the left as well. It turns out that, although incumbents may have responded as well, the response of new legislators was stronger.

I use the definition of incumbency as “served in the 86th Congress” from the previous subsection. I interact $P_{\ell t-1}$ and $V_{\ell t-1}$ in equation (1) with an indicator variable for this definition. If the coefficient on the interaction term with $P_{\ell t-1}$ ($V_{\ell t-1}$) is statistically significant, then incumbents’ response to peaceful (violent) protests was statistically significantly different from that of politicians who entered after the 86th Congress. If the sum of the base coefficient and the coefficient on the interaction term for $P_{\ell t-1}$ ($V_{\ell t-1}$) is statistically significant, then incumbents responded to peaceful (violent) protests statistically significantly.

Table 6 shows results from these specifications. The base coefficients indicate that new legislators responded to peaceful protests on civil-rights and welfare issues, and to violent protests on crime issues. Only their response on civil rights and welfare, however, survives the robustness checks of Section 6. Likewise, the p -values for no incumbent response indicate that incumbents responded to peaceful protests on civil-rights and welfare issues, and to violent protests on crime issues. Neither of these results, however, is robust, and they may have been driven by differential trends in districts with larger news markets.

5.4. Symbolic vs. substantive legislator response. Another remaining question is whether protests changed votes only on roll calls that passed or failed with a wide margin (symbolic response) or also on roll calls with more narrow margins (substantive response). A symbolic response may have served to advertise a legislator’s pro- or anti-civil-rights stance to constituents, whereas a substantive response more plausibly affected policy delivery.

To test for substantive legislator response, I define close roll calls as those that were decided by a 65-percent majority or less. I then re-calculate the voting scores on only close roll calls for each of the four issue themes, and estimate equation (1) with these on the left-hand side.

On close roll calls as well, peaceful protests made legislator votes more liberal on civil-rights and welfare issues, by 4.86 pp ($[-8.08$ pp, -1.64 pp]; Table 7, Panel A). Violent protests had a marginally significant opposite effect on these roll calls (2.70 pp, $[-.00$ pp, 5.40 pp]). This suggests that legislator response to protests was not merely symbolic but also substantive.

Neither peaceful nor violent protests had statistically significant effects on the three other issue themes, which may partly be due to smaller sample sizes. These issue themes had fewer close roll calls available to compute the voting scores.

6. ROBUSTNESS AND ALTERNATIVE EXPLANATIONS

The findings of Section 5 are valid so far as districts with protests did not follow different trends than did districts without. As long as this assumption of no differential trends holds, identification follows because the fixed effects absorb all endogeneity in protest incidence that is time-invariant. In this section, I consider potential sources of time-varying endogeneity based on the theoretical framework of Section 2 and estimate specifications with controls for differential trends. The effects of peaceful protests remain with these controls. I also consider alternative explanations that are consistent with my estimates, and explore specifications that absorb the channels that these explanations imply. Again, the effects of peaceful protests remain.

The effect of violent protests on crime-related roll calls is attributable to differential trends by newspaper markets. This is because violence occurred largely in densely populated districts. In turn, reported violent protest activity is strongly predicted by the size of the local newspaper market. The pattern is consistent both with the violent-protest effect's being a spurious artifact of news reporting and with a real causal effect, where incidence coincides with large newspaper markets.

6.1. Robustness. To assess robustness to time-varying endogeneity, I allow for semi-parametric differential trends in the outcome. The main specification assumes that the residual term in equation (1), $U_{\ell t}$, is uncorrelated with the protest histories, $P_{\ell t-1}$ and $V_{\ell t-1}$. Instead of assuming zero correlation, now I semi-parametrically specify $U_{\ell t}$ as

$$U_{\ell t} = Z_{\ell} \delta_t + \varepsilon_{\ell t}, \quad (3)$$

where $\varepsilon_{\ell t}$ is assumed to be uncorrelated but $Z_{\ell} \delta_t$ may be correlated with $P_{\ell t-1}$ and $V_{\ell t-1}$. Z_{ℓ} is a fixed district characteristic observed in 1960 or before, and δ_t is a time-varying coefficient to be estimated. I also compute the first principal components of sets of fixed district characteristics, and estimate specifications with these as Z_{ℓ} . This allows me to assess robustness when accounting for complete sets of related characteristics.

6.1.1. Conservatism. Conservatism in the district affects how much protesters could gain from a policy change. I test whether these drive the findings in two ways.

First, I re-estimate β_1 and β_2 with Z_{ℓ} defined as John F. Kennedy's 1960 presidential vote share. To illustrate how this might affect the estimates, suppose that districts where Kennedy had a lower vote share were more conservative. Before the 1965 Voting Rights Act (VRA), black voter registration in these districts might have been kept artificially low so that the mobilization effect of protests was small, and the persuasion effect dominated. Therefore more conservative districts might have attracted more peaceful and fewer violent protests. This yields a spurious peaceful protest effect in the main specification if, independently of protests, more conservative

districts experienced a liberal shift due to the VRA but more liberal districts did not. Adding $Z_\ell \delta_t$ to the specification absorbs the liberal shift of more conservative districts. It does so by allowing them to have larger differences from more liberal districts in some time periods and smaller differences in others.

Second, I look at Strom Thurmond’s 1948 presidential vote share as Z_ℓ . Kennedy’s vote share is a general measure of conservatism. Thurmond’s vote share is a more specific measure of popular support for racial segregation. Thurmond was elected governor of South Carolina in 1946. He ran for president as a third-party candidate, breaking away from the Democratic Party. During the Second World War, the Democratic federal government forbade firms in the defense industry from discriminating on the basis of race and guaranteed equal benefits to black and white veterans. After the war, Harry Truman ordered the desegregation of the military and expressed his support to eliminate poll taxes. Thurmond and other Southern Democrats opposed to these policies established the *States’ Rights Democratic Party* with the goal to fend off federal intervention against segregation.

In November 1948, Thurmond ran against Truman, the Democratic incumbent, and Thomas Dewey, the progressive Republican governor of New York. Thurmond’s platform represented a clear stance for continued segregation. Apart from the 11 ex-Confederate states, he was on the ballot in only Kentucky, Maryland, and North Dakota. Nationally, he received less than 3 percent of the popular vote, but in the South, he carried Alabama, Louisiana, Mississippi, and South Carolina.

Table 8 shows the coefficient estimates for peaceful and violent protests under different Z_ℓ . Neither Kennedy’s 1960, nor Thurmond’s 1948 vote share as Z_ℓ changes the results (Panel A). The estimates for peaceful protests on civil rights and turnout and for violent protests on crime remain statistically significant and similar in magnitude.

6.1.2. Racial composition. Along with how conservative a district was, its racial composition also affected protest incidence and the choice between peaceful and violent protests. To test whether this generates time-varying endogeneity, I compute the 1960 population share of blacks in the district and set it as Z_ℓ . Accounting for racial composition does not change the results (Table 8, Panel B).

6.1.3. Cost of protest organizing. Both conservatism and popular support for segregation indicate a less hospitable environment for protesters, increasing the cost of organizing protests. Other factors that could have affected costs were how urbanized the district was and how many resources protesters had (Andrews and Biggs, 2006). The factors that I test for are urbanization (share of the population who lived in a principal city [% in-metro] and outside the principal city but inside a metro area [% out-metro]), communication infrastructure (radio and phone ownership), the economic environment of blacks (black unemployment rate, share of blacks who worked in agriculture), and the socioeconomic status of blacks (share of blacks with a college

degree, average income of black families). None of these changes the conclusions for the effects of peaceful protests on civil rights and on election turnout (Table 9). The estimated effect of violent protests on crime roll calls drops when allowing for differential trends in more urbanized areas (as measured by % in-metro). The effect, however, remains statistically significant.

6.2. Alternative explanations. Albeit robust to endogeneity that arises from within the model of Section 2, there are alternative explanations that come from outside the model that are consistent with the main results. I discuss these below.

6.2.1. Heterogeneous voter response to national events. Instead of responding to local protests, voters may have responded to national events. If different voter types responded differently, and the incidence of protests was correlated with voter composition, the local protest effects that I measure would be spurious.

A natural test of this explanation is to invoke equation (3) and define Z_ℓ as the relevant dimension of voter heterogeneity. Doing so allows districts to follow heterogeneous trends along that dimension. For example, districts with a low white unemployment rate could have responded more favorably to news about pro-civil-rights legislation than did districts where white unemployment was high. This would have translated into a relative conservative shift of high-unemployment districts. By letting Z_ℓ be the white unemployment rate in 1960, adding $Z_\ell\delta_t$ to the specification absorbs this variation.

Following the layout of Table 8, Table 10 shows estimates under different Z_ℓ . Panel A tests whether allowing differential trends by various measures of district composition changes the estimates for the peaceful and violent protest coefficients. In addition to the measures tested before, measures of education, the white unemployment rate, and the average income of white families also do not change the results.

6.2.2. Newspapers and reporting bias. The protests that I observe are subject to measurement error. Although *The New York Times* had dedicated Southern correspondents and maintained bureaus in the South, only for major events did it send staffers on the ground (Roberts and Klibanoff, 2006). Most of its reporting was sourced through wire services and “stringers” (correspondents hired on an ad hoc basis to report on specific events). The two major wire services at the time were the Associated Press (AP) and United Press International (UPI). Both the AP and UPI maintained local bureaus in every state. Both distributed news that they received from local newspapers, and UPI was able to supplement this with reporting by its own staffers or by stringers. This points to three sources of measurement error. First, reports of some events may not have made it to the *Times* if the local news market was small or non-existent. Second, the content provided by local newspapers may have been biased. Third, the *Times* had a preference over which events to dedicate space to and report (Myers and Caniglia, 2004).

The first source, underreporting, does not affect my qualitative conclusions. Uniform underreporting does not bias the estimates of the overall effects because it rescales both the observed protest history and the fixed-effects estimand by the same factor. For any constant $\alpha_1 > 0$, $\beta_1 \mathbb{E}(P_{\ell 91}) = \alpha_1 \beta_1 \mathbb{E}(P_{\ell 91} / \alpha_1)$. Under non-uniform underreporting, the estimator is biased only for the magnitude but not for the sign of the overall effect.

The second source, local bias in the content, is unlikely to drive the qualitative results if there were no differential trends. In small news markets, low demand could have compelled editors to ignore some events, and the lack of competition could have made it easier for them to bias their reports on others. Under misclassification, for example if peaceful protests were falsely reported as violent, violent protest history would be inflated. This would bias the estimator for the violent-protest effect downward (toward the effect of peaceful protests). Peaceful protests, however, are estimated to have led to a liberal shift. Thus this type of misclassification cannot drive the estimate of a backfiring effect for violent protests. The analogous argument holds for misclassification in the other direction.

It is possible, however, that small and large news markets followed different political trends. The median congressional district had 1.66 newspapers per county. Newspaper markets varied in size: the 25th-percentile district had .92 and the 75th percentile had 2.86 newspapers per county. I test if the protest effects remain when allowing differential trends in the number of newspapers, or as another measure of market size, in the total circulation as a share of the population. Panel B of Table 10 shows the results. The effects of peaceful protests on civil rights and on election turnout remain. The effect of violent protests on crime drops in magnitude and loses statistical significance for the number of newspapers. This is because violent protests coincided with large news markets. Whether the effect of violent protests is spurious or real remains an open question.

The third source, the bias of the *Times*, could enter in two ways: the *Times* could have been more likely to report violent protests than peaceful ones, and it could have had a tendency cover events closer to New York City. The first of these would have led to underreporting of peaceful protests, which I have addressed above. The second, geographic bias, is partly addressed by the main specification: the district fixed effects control for the time-invariant part of the *Times*' bias. To test whether changes in its bias during the era drive my estimates, I let Z_ℓ be distance from New York City. This allows distance to have a time-varying slope in the specification. Panel B of Table 10 shows that the results do not change on any of the three outcomes when doing this.

6.2.3. Agenda setting. Instead of a change in policy positions, the estimates could be picking up a change in the agenda. Under this explanation, rather than the legislators' moving in the policy space, it would be the issues that were voted on that moved. Legislators who previously voted on the conservative side now may have found themselves on the liberal side of issues.

One implication of the agenda change explanation is that protests had different apparent

effects in some time periods (under one agenda) than in others (under the other agenda). To test whether this was the case, I interact $P_{\ell t-1}$ and $V_{\ell t-1}$ in equation (1) with indicator variables for whether t was after the 88th, the 89th, or the 90th Congress. If the coefficients on the interaction terms are statistically significant, they are evidence of time heterogeneity. This is not the case, however, for interactions of any of the three indicator variables with peaceful and violent protests (Table 11). There is no statistical evidence of agenda setting driving the results.

7. COUNTERFACTUALS

Sections 5 and 6 have established the robust effect of peaceful protests on representation on civil-rights and welfare issues, and the suggestive evidence of a violent-protest effect on crime-related issues. They leave open, however, the question of whether these effects were large enough to substantially affect lawmaking in the House. To determine whether they were, I model legislator votes on individual roll calls and evaluate the margin on major bills under the counterfactual of no protests.

I estimate the vote-level analog of the main specification, equation (1). Indexing individual roll-call votes by v , the analogous specification is

$$Y_{\ell v} = \beta_1 P_{\ell t(v)-1} + \beta_2 V_{\ell t(v)-1} + \lambda_\ell + \theta_{\text{South}(\ell) \times t(v)} + \mathbf{X}'_{\ell t(v)} \boldsymbol{\gamma} + U_{\ell v} \quad (4)$$

where $Y_{\ell v} \in \{0, 1\}$ indicates whether the politician who represented district ℓ voted conservatively ($Y_{\ell v} = 1$) or liberally on v , and $t(v)$ is the two-year congress in which v was voted on. As in the main specification, λ_ℓ and $\theta_{\text{South}(\ell) \times t(v)}$ are district and South/non-South congress fixed effects, and $\mathbf{X}_{\ell t(v)}$ is a vector of controls that contains anti-civil-rights and war protest histories.

7.1. Counterfactual margins. I calculate the local effect of protests on the margin on vote v in three steps. First, I construct the realized conservative support for v , and from this, calculate the realized margin. Second, I construct conservative support under the counterfactual of no protests, and get the implied counterfactual margin. Third, I calculate the contribution of protests to the realized margin using the counterfactual.

As building blocks, I define the following variables. Without ambiguity, depending on the context let N_v be either the set that contains all districts in the sample that voted on roll-call vote v , or the number of elements of this set. Let the “bill indicator” $B_v := N_v^{-1} \sum_{\ell \in N_v} Y_{\ell v}$ be the share of conservative votes on v among districts in the sample. The realized margin among these districts is $M_v := |(1 - B_v) - B_v| = |1 - 2B_v|$. Let $\widehat{B}_v^{-P} := N_v^{-1} \sum_{\ell \in N_v} (Y_{\ell v} - \widehat{\beta}_1 P_{\ell t(v)-1})$ be the counterfactual share of conservative votes on v under no peaceful protests. The corresponding counterfactual margin is $\widehat{M}_v^{-P} := |1 - 2\widehat{B}_v^{-P}|$. This implies that peaceful protests increased the margin among districts in the sample by $1 - \widehat{M}_v^{-P}/M_v$.

Due to data limitations, not every district that voted on v made it into the sample N_v . To

get an accurate picture of the effects on the margin, we need to consider the excluded districts as well. Let N_v^* denote *all* districts that voted on v and let $B_v^* := (N_v^*)^{-1} \sum_{\ell \in N_v^*} Y_{\ell v}$ be the share of conservative votes on v in N_v^* . The realized margin among all districts is $M_v^* := |1 - 2B_v^*|$. To construct the counterfactuals, I make the conservative assumption that districts that were not in the sample had no protests and, thus, had a zero local protest effect. Therefore, among all districts, the counterfactual share of conservative votes on v under no peaceful protests is $\widehat{B}_v^{*, -P} := (N_v^*)^{-1} \left(\sum_{\ell \in N_v^*} Y_{\ell v} - \sum_{\ell \in N_v} \widehat{\beta}_1 P_{\ell t(v)-1} \right)$. Then the counterfactual margin among all districts is $\widehat{M}_v^{*, -P} := |1 - 2\widehat{B}_v^{*, -P}|$, and the implied effect of peaceful protests on the margin is $1 - \widehat{M}_v^{*, -P} / M_v^*$. The counterfactual margin under no violent protests, $\widehat{M}_v^{*, -V}$, is calculated analogously.

7.2. Protest effects on major bills. Support for bills in the House was determined by three factors: the local effects of protests, the cross-district effects, and factors unrelated to protests. Although the counterfactual exercise illustrates the role of the local effects, the cross-district effects may have been large. The legislative history of the Voting Rights Act is one example that suggests this. In his March 1965 speech in Congress two days before the bill was introduced, President Johnson referenced the Selma protests eight times. The local effects on major bills may thus give cautious lower bounds on the total effects that protests had on lawmaking.

Protests had relatively small local effects on the 1964 Civil Rights Act (CRA) and the 1965 Voting Rights Act (VRA). Both were passed with wide margins and relatively early on in the decade. In the final House vote, the CRA was passed with a 38.3-percent margin (Table 12, column (2)). Without the local effect of peaceful protests, the margin would have been 37.2 percent (column (4)). This means that the local effect of peaceful protests delivered 3.0 percent of the final margin (column (5)). Similarly for the VRA, through the local effect, peaceful protests accounted for 4.6 percent of the final margin.

The era, however, also saw the passage of other major bills. The local effect delivered 27.3 percent of the margin on the 1968 Civil Rights Act. This bill contained the Fair Housing Act and introduced federal punishment for racial, religious, and ethnic hate crimes. It passed with an 18.6 percent margin but would have had a 13.5 percent margin without the local effect of peaceful protests.

The margin on the 1970 extension of the VRA was 12.5 percent due to the local effect. This is similar to major bills in the welfare issue theme for which the figure was between 7 and 12 percent (Panel B, column (5)).

By contrast, violence reduced the final margin by 32.3 percent on the 1968 Gun Control Act (Panel C, column (8)). The estimate indicates a large but not statistically significant local effect of peaceful protests.

These counterfactual results show that protests contributed significantly to the passage of major bills. Two caveats are worth noting. First, as mentioned above, these results do not reflect

the additional presence of cross-district effects. Second, protests likely affected the content of bills as well. In addition to garnering support for these bills, peaceful protests may have made the bills themselves more liberal as well.

8. CONCLUSION

Mass protests have been integral to many episodes of institutional change throughout history as well as today. For such protests, whether to remain peaceful or use violence is a perennial question. I identify the local effects of protests in the Civil Rights Era, and show that peaceful protests helped pro-civil-rights legislative efforts but violent protests did not. Although, in models of conflict, violence sends a threat that can force the majority to comply, the race riots of the 1960s were not seen as credible threats by the majority at the local level.

This result is the most pertinent to minority protests in majoritarian democracies. Protesters' best strategy may be different in settings in which the decision maker is not the median voter but a non-indigenous party bureaucracy such as in Hong Kong today. Likewise, in the anti-austerity protests of Europe in the past decade, violence may have been more effective if it reflected the policy demands of the majority. What the effects of violence are in these contexts is an important question that remains open.

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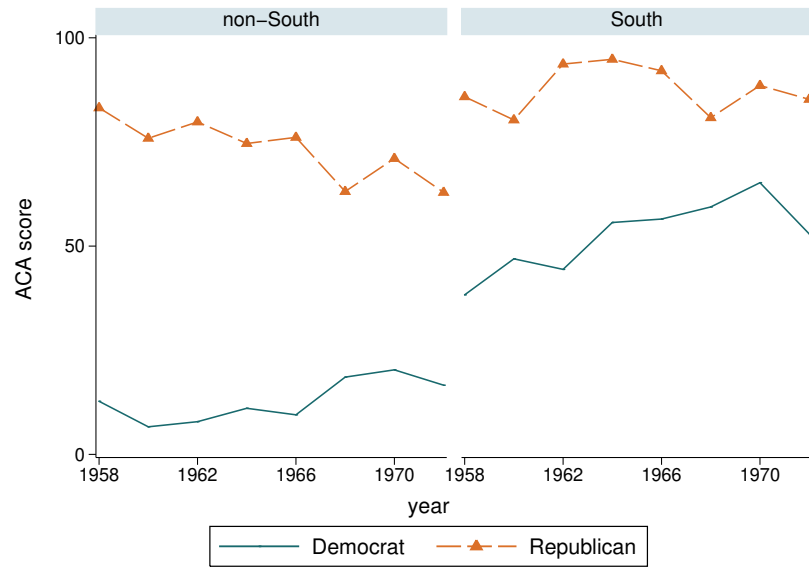
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A. Americans for Constitutional Election (ACA), a conservative interest group



B. Americans for Democratic Action (ADA), a liberal interest group

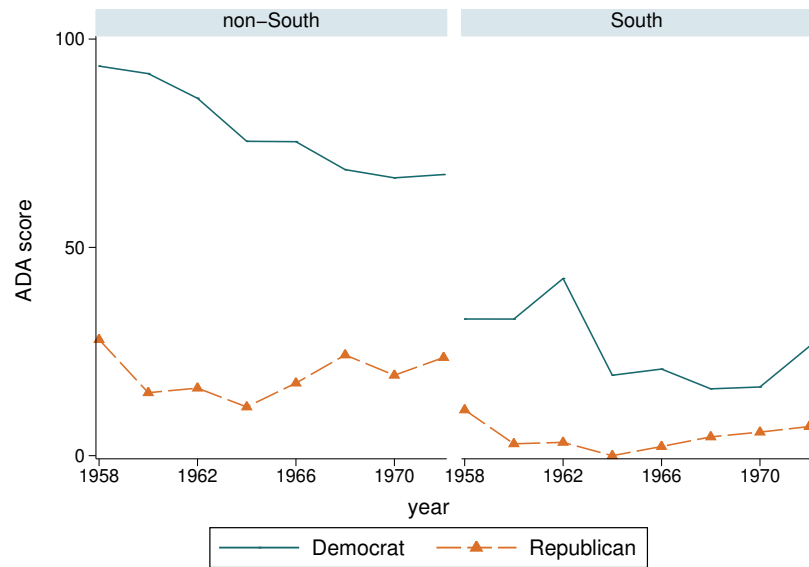


FIGURE 1: Average interest-group ratings in the 11 ex-Confederate states (South) and elsewhere (non-South)

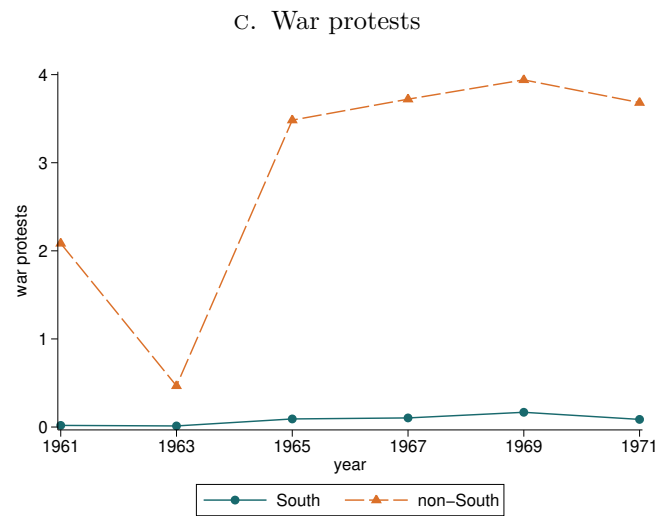
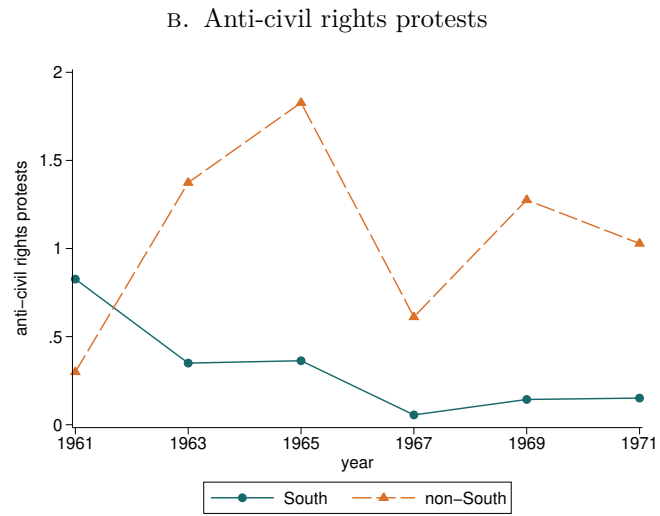
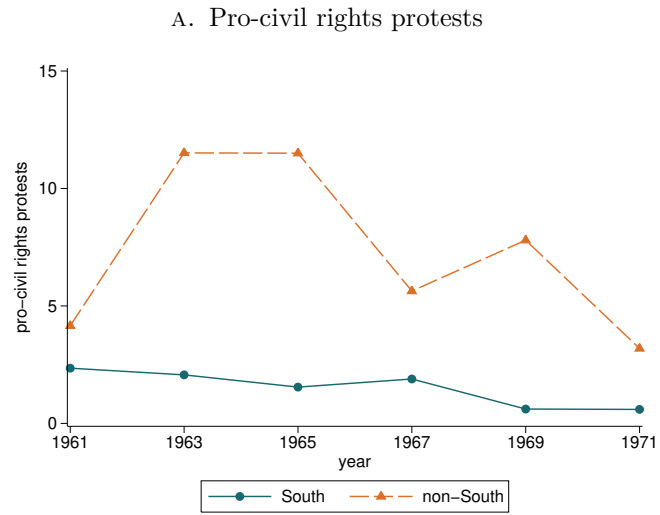
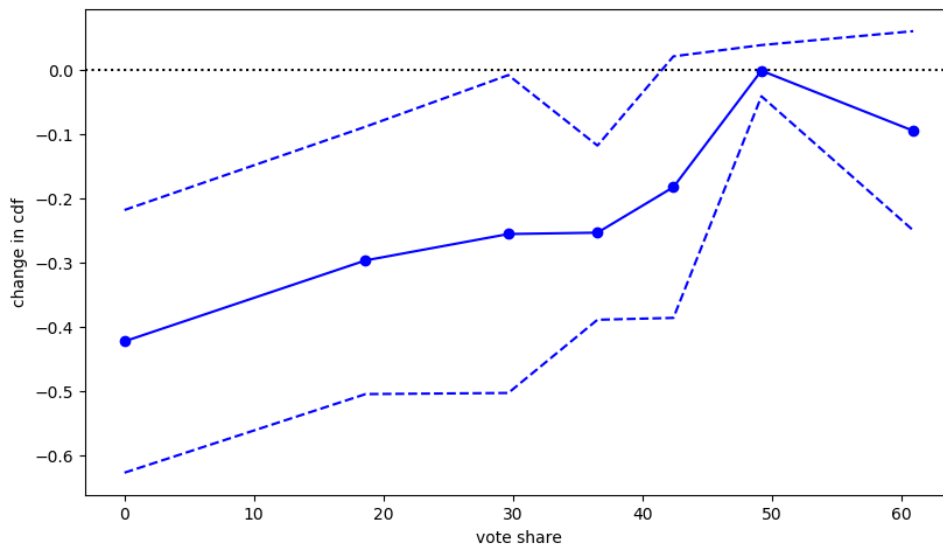
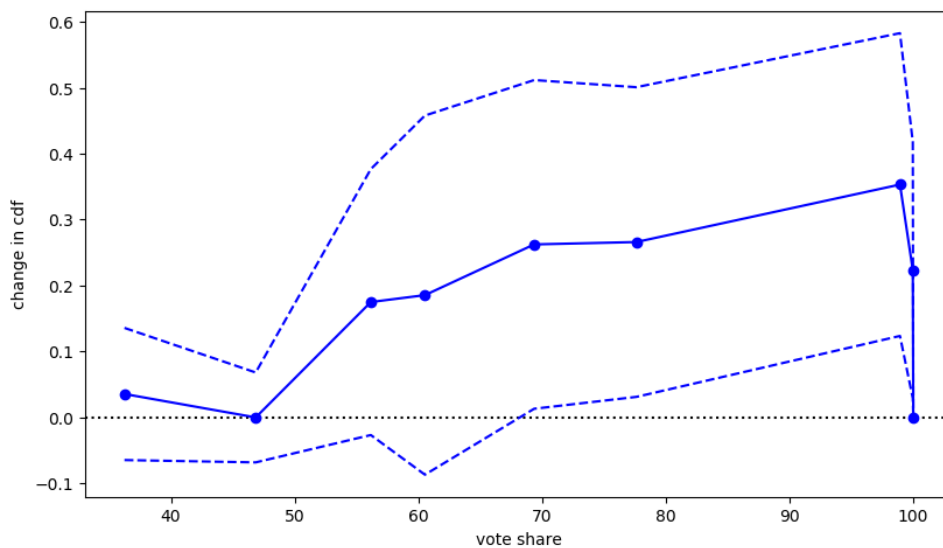


FIGURE 2: Regional means of population-weighted average event counts in congressional districts

A. Southern Republicans



B. Southern Democrats



Notes: The figures show estimates of $\beta_1 \mathbb{E}(P_{\ell 91})$ in the subsample of Southern districts from the specification $\mathbb{1}(Y_{\ell t} \leq x) = \beta_1 P_{\ell t-1} + \beta_2 V_{\ell t-1} + \mathbf{X}'_{\ell t} \boldsymbol{\gamma} + \lambda_{\ell} + \theta_{\text{South}(\ell) \times t} + U_{\ell t}$ where x are decile cutoffs of $Y_{\ell t}$. The dashed lines mark the 95-percent pointwise confidence intervals.

FIGURE 3: The effect of peaceful protests on partisan vote shares in the South

TABLE 1: Variable means across congressional districts

	all districts	by region		by urbanization ^a	
		South	non-South	above med.	below med.
<i>Panel A: 89th Congress (1965–1966)</i>					
Republican vote share	39.52	27.16	42.94	37.64	41.50
had Republican candidate	92.25	71.43	98.02	94.97	89.36
peaceful history	10.22	5.23	11.60	15.97	4.13
violent history	0.74	0.38	0.84	1.14	0.31
any peaceful history	59.95	64.29	58.75	72.36	46.81
any violent history	31.52	44.05	28.05	37.69	25.00
sample size	387	84	303	199	188
<i>Panel B: 92nd Congress (1971–1972)</i>					
Republican vote share	43.29	32.55	46.21	41.54	45.04
had Republican candidate	93.08	73.53	98.40	94.34	91.82
peaceful history	20.51	10.79	23.15	29.95	11.07
violent history	6.58	2.27	7.75	10.20	2.95
any peaceful history	77.36	85.29	75.20	89.94	64.78
any violent history	65.72	76.47	62.80	80.50	50.94
sample size	318	68	250	159	159

^aUrbanization is measured as the district’s population share in the central city of a metropolitan area. The district median is computed at the 92nd Congress. Due to this, more districts were above the median than below at the 89th Congress.

This table shows averages of election outcomes and protest histories across districts. “Peaceful history” and “violent history,” the construction of which is described in section 5, are pro-civil-rights protest totals from preceding congresses.

TABLE 2: Protests and representation

	civr. (1)	welfare (2)	civr. & welfare (3)	crime (4)	environment (5)	defense (6)	ACA score (7)	ADA score (8)
protest history								
...peaceful	-0.127** (0.0507)	-0.162*** (0.0531)	-0.159*** (0.0486)	-0.004 (0.0983)	-0.008 (0.0519)	0.039 (0.0640)	-0.092 (0.0628)	0.100* (0.0535)
...violent	0.232 (0.1509)	0.196 (0.1523)	0.210 (0.1436)	0.662*** (0.1491)	0.090 (0.1146)	-0.041 (0.2325)	0.391** (0.1727)	-0.526*** (0.1661)
sample size	1798	1798	1798	1684	1798	1798	1798	1798
mean outcome	45.481	38.237	40.962	62.835	37.451	52.195	47.295	40.456

Outcome values are scaled to fall between 0 and 100. Higher values in the outcomes indicate more conservative representation, except for the ADA liberalism score which is on a reversed scale. The effect sizes implied by the coefficients are discussed in the text. Standard errors are two-way clustered by district and state-by-congress. Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

TABLE 3: Election outcomes

	turnout (1)	partisan vote share		margin of victory (4)	incumbent won ^a (5)
		Dem. (2)	Rep. (3)		
protest history					
...peaceful	0.054** (0.0220)	0.010 (0.0104)	0.036*** (0.0128)	-0.107 (0.0795)	-0.134 (0.1168)
...violent	0.108** (0.0488)	0.016 (0.0268)	0.022 (0.0324)	0.100 (0.0948)	0.177 (0.1979)
sample size	1798	1798	1798	1798	1798
mean outcome	52.064	17.265	15.732	32.184	45.273

^aIndicates if the candidate who was elected served in the 86th congress. Outcome values are scaled to fall between 0 and 100. The effect sizes implied by the coefficients are discussed in the text. Standard errors are two-way clustered by district and state-by-congress. Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

TABLE 4: Election outcomes in and outside the South

	partisan vote share			Rep. ran (4)	Rep. won (5)	inc. won ^a (6)
	turnout (1)	Dem. (2)	Rep. (3)			
<i>Panel A: Southern subsample</i>						
protest history						
...peaceful	0.510*** (0.1215)	-1.210*** (0.2607)	1.354*** (0.2553)	3.378*** (0.6690)	-0.085 (0.2151)	-2.019** (0.7566)
...violent	-0.202 (0.3827)	0.949 (0.9476)	-0.417 (0.7270)	-0.482 (1.7673)	-0.444 (0.7531)	-0.886 (4.8291)
sample size	393	393	393	393	393	393
mean outcome	33.740	71.242	26.824	63.359	20.611	48.092
<i>Panel B: non-Southern subsample</i>						
protest history						
...peaceful	0.058** (0.0244)	-0.039 (0.0293)	0.033 (0.0298)	-0.011 (0.0207)	-0.005 (0.0942)	-0.091 (0.1262)
...violent	0.111** (0.0493)	0.059 (0.0651)	0.020 (0.0857)	-0.064 (0.0641)	0.280 (0.2577)	0.221 (0.1887)
sample size	1405	1405	1405	1405	1405	1405
mean outcome	57.190	49.465	47.574	98.434	50.178	44.484

^aIndicates if the candidate who was elected served in the 86th congress.

Outcome values are scaled to fall between 0 and 100. The effect sizes implied by the coefficients are discussed in the text. Standard errors are two-way clustered by district and state-by-congress. Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

TABLE 5: Effects on the distribution of the Republican vote share

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
protest history									
...peaceful	-0.029 (0.0169)	-0.050** (0.0173)	-0.052* (0.0251)	-0.023 (0.0189)	-0.013 (0.0154)	0.019 (0.0146)	0.006 (0.0231)	-0.001 (0.0248)	0.002 (0.0210)
...violent	-0.009 (0.0065)	-0.004 (0.0127)	0.010 (0.0114)	-0.023 (0.0172)	-0.027* (0.0139)	-0.033** (0.0136)	-0.011 (0.0172)	-0.004 (0.0189)	0.000 (0.0138)
sample size	1798	1798	1798	1798	1798	1798	1798	1798	1798
mean outcome	0.101	0.201	0.301	0.400	0.500	0.601	0.701	0.801	0.900

Outcome values are scaled to fall between 0 and 100. Standard errors are two-way clustered by district and state-by-congress. Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

TABLE 6: Incumbent response to protests

	civr. & welfare (1)	environment (2)	defense (3)	crime (4)
incumbent won ^a	6.066* (3.3971)	5.717* (3.1200)	5.308 (3.2502)	3.195 (3.9771)
protest history				
...peaceful	-0.163*** (0.0476)	-0.001 (0.0532)	0.049 (0.0661)	0.000 (0.0995)
...violent	0.341* (0.1833)	0.117 (0.1112)	-0.113 (0.2629)	0.771*** (0.1949)
protest history × inc. won				
...peaceful	0.041* (0.0210)	-0.010 (0.0302)	-0.002 (0.0411)	-0.004 (0.0407)
...violent	-0.366*** (0.1667)	-0.088 (0.1814)	0.156 (0.2602)	-0.275 (0.2695)
<i>p</i> for no incumbent response ^b				
...peaceful	0.009	0.843	0.514	0.968
...violent	0.853	0.868	0.868	0.016
sample size	1798	1798	1798	1684

^aIndicates if the candidate who was elected served in the 86th congress.

^b*p*-value for the null hypothesis that the coefficients for “protest history” and “protest history × incumbent won,” for the corresponding protest type, sum to zero.

Outcome values are scaled to fall between 0 and 100. Standard errors are two-way clustered by district and state-by-congress. Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

TABLE 7: Close roll calls and interactions between peaceful and violent protests

	civr. & welfare (1)	environment (2)	defense (3)	crime (4)
<i>Panel A: only close roll calls</i>				
protest history				
...peaceful	-0.237*** (0.0797)	-0.074 (0.0850)	-0.427 (0.3448)	-0.250 (0.5221)
...violent	0.411** (0.2084)	0.282 (0.2707)	-0.298 (0.3635)	1.025* (0.6170)
sample size	1798	1659	1220	706
<i>Panel B: all roll calls, adding an interaction term</i>				
protest history				
...peaceful	-0.154*** (0.0495)	0.006 (0.0483)	0.031 (0.0620)	-0.001 (0.0979)
...violent	0.237 (0.1678)	0.164 (0.1183)	-0.082 (0.2676)	0.681*** (0.1824)
...peaceful × violent	-0.001 (0.0011)	-0.002* (0.0009)	0.001 (0.0019)	-0.000 (0.0015)
sample size	1798	1798	1798	1684

Outcome values are scaled to fall between 0 and 100. Standard errors are two-way clustered by district and state-by-congress. Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

TABLE 8: Robustness to controls for time-varying endogeneity suggested by theory (political conditions)

Z_ℓ	civr. & welfare			crime			% Rep. (of pop.)		
	peaceful	violent	N	peaceful	violent	N	peaceful	violent	N
	history			history			history		
<i>Panel A: conservatism</i>									
% Kennedy	-0.143*** (0.0458)	0.145 (0.1392)	1798	0.024 (0.0959)	0.537*** (0.1609)	1684	0.034** (0.0135)	0.049* (0.0287)	1798
% Thurmond	-0.167*** (0.0491)	0.207 (0.1432)	1798	-0.015 (0.0991)	0.658*** (0.1492)	1684	0.037*** (0.0130)	0.022 (0.0324)	1798
1st PC	-0.141*** (0.0473)	0.145 (0.1370)	1798	0.003 (0.0955)	0.562*** (0.1563)	1684	0.037*** (0.0134)	0.032 (0.0310)	1798
<i>Panel B: demography</i>									
% black	-0.163*** (0.0477)	0.194 (0.1458)	1798	0.011 (0.0980)	0.539*** (0.1451)	1684	0.039*** (0.0136)	0.028 (0.0327)	1798

Outcome values are scaled to fall between 0 and 100. Standard errors are two-way clustered by district and state-by-congress. Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

TABLE 9: Robustness to controls for time-varying endogeneity suggested by theory (cost of organizing)

Z_ℓ	civr. & welfare		N	crime		N	% Rep. (of pop.)		N
	peaceful	violent		peaceful	violent		peaceful	violent	
	history			history			history		
% in-metro	-0.124** (0.0513)	0.069 (0.1450)	1798	0.087 (0.1057)	0.450*** (0.1609)	1684	0.044*** (0.0145)	0.034 (0.0309)	1798
% out-metro	-0.153*** (0.0465)	0.283* (0.1443)	1798	-0.007 (0.1009)	0.788*** (0.1686)	1684	0.035*** (0.0124)	0.011 (0.0298)	1798
% radio	-0.150*** (0.0456)	0.208 (0.1445)	1798	-0.017 (0.1005)	0.634*** (0.1430)	1684	0.036*** (0.0129)	0.019 (0.0327)	1798
% phone	-0.170*** (0.0475)	0.194 (0.1407)	1798	-0.013 (0.1007)	0.664*** (0.1479)	1684	0.039*** (0.0126)	0.013 (0.0348)	1798
% unemployed (black)	-0.146*** (0.0468)	0.140 (0.1356)	1798	0.022 (0.0960)	0.562*** (0.1435)	1684	0.035*** (0.0129)	0.031 (0.0320)	1798
% in agriculture (black)	-0.147*** (0.0489)	0.198 (0.1438)	1798	-0.008 (0.0986)	0.660*** (0.1553)	1684	0.036*** (0.0130)	0.024 (0.0348)	1798
% college (black)	-0.157*** (0.0491)	0.210 (0.1436)	1798	-0.010 (0.0993)	0.671*** (0.1492)	1684	0.036*** (0.0130)	0.025 (0.0323)	1798
average income (black)	-0.154*** (0.0467)	0.205 (0.1424)	1798	-0.006 (0.0964)	0.657*** (0.1500)	1684	0.037*** (0.0129)	0.016 (0.0349)	1798
1st PC	-0.142*** (0.0471)	0.184 (0.1415)	1798	-0.014 (0.0987)	0.673*** (0.1561)	1684	0.038*** (0.0129)	0.010 (0.0369)	1798

Outcome values are scaled to fall between 0 and 100. Standard errors are two-way clustered by district and state-by-congress. Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

TABLE 10: Robustness to controls for differential trends

Z_ℓ	civr. & welfare		N	crime		N	% Rep. (of pop.)		N
	peaceful	violent		peaceful	violent		peaceful	violent	
	history		history	history	history	history			
<i>Panel A: heterogeneous voter response</i>									
% high school	-0.150*** (0.0485)	0.264* (0.1432)	1798	-0.039 (0.1053)	0.722*** (0.1782)	1684	0.034*** (0.0121)	0.018 (0.0295)	1798
% any college	-0.142*** (0.0508)	0.253* (0.1445)	1798	-0.060 (0.1099)	0.700*** (0.1683)	1684	0.032*** (0.0119)	0.004 (0.0311)	1798
% college	-0.127** (0.0522)	0.179 (0.1390)	1798	-0.064 (0.1153)	0.676*** (0.1554)	1684	0.034*** (0.0122)	0.009 (0.0339)	1798
% unemployed (white)	-0.158*** (0.0486)	0.229 (0.1468)	1798	0.047 (0.1053)	0.704*** (0.1448)	1684	0.042*** (0.0123)	0.014 (0.0370)	1798
average income (white)	-0.125** (0.0505)	0.124 (0.1441)	1798	-0.001 (0.1004)	0.709*** (0.1756)	1684	0.042*** (0.0120)	-0.013 (0.0399)	1798
1st PCA	-0.136*** (0.0501)	0.202 (0.1418)	1798	-0.036 (0.1106)	0.703*** (0.1651)	1684	0.036*** (0.0118)	0.001 (0.0342)	1798
<i>Panel B: newspaper markets and reporting bias</i>									
number of newspapers	-0.137*** (0.0505)	0.132 (0.2060)	1798	0.053 (0.1022)	0.447 (0.3551)	1684	0.032** (0.0123)	-0.028 (0.0658)	1798
% circulation	-0.166*** (0.0495)	0.173 (0.1422)	1798	-0.015 (0.1013)	0.513*** (0.1591)	1684	0.035*** (0.0130)	0.036 (0.0293)	1798
distance to NYC	-0.142*** (0.0470)	0.280* (0.1468)	1798	0.014 (0.0985)	0.701*** (0.1843)	1684	0.037*** (0.0129)	-0.031 (0.0319)	1798
1st PCA	-0.161*** (0.0506)	0.393** (0.1960)	1798	0.004 (0.1041)	0.833*** (0.2787)	1684	0.035*** (0.0118)	-0.035 (0.0415)	1798

Outcome values are scaled to fall between 0 and 100. Standard errors are two-way clustered by district and state-by-congress. Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

TABLE 11: Heterogeneity by time (civil rights and welfare)

	(1)	(2)	(3)
protest history			
...peaceful	-0.282 (0.2610)	-0.147*** (0.0567)	-0.149*** (0.0557)
...violent	0.229 (0.1505)	0.213 (0.1487)	0.177 (0.1608)
protest history × post-88th congress			
...peaceful	0.085 (0.175)		
protest history × post-89th congress			
...peaceful		0.056 (0.080)	
protest history × post-90th congress			
...peaceful			0.011 (0.024)
sample size	1798	1798	1798

Outcome values are scaled to fall between 0 and 100. Standard errors are two-way clustered by district and state-by-congress. Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

TABLE 12: Counterfactual outcomes on major bills

	realized		counterfactual under								
	% cons. (1)	margin (2)	no peaceful protests			no violent protests			no protests		
			% cons. (3)	margin (4)	$1 - (4)/(2)$ (5)	% cons. (6)	margin (7)	$1 - (7)/(2)$ (8)	% cons. (9)	margin (10)	$1 - (10)/(2)$ (11)
<i>Panel A: civil-rights bills</i>											
1964 Civil Rights Act	30.8	38.3	31.4	37.2	3.0	30.8	38.4	-0.1	31.4	37.2	2.9
1965 Voting Rights Act	19.3	61.4	20.7	58.6	4.6	19.2	61.6	-0.3	20.6	58.8	4.3
1968 Civil Rights Act	40.7	18.6	43.2	13.5	27.3	40.4	19.2	-3.4	42.9	14.2	23.8
1970 ext. of Voting Rights Act	33.2	33.7	35.3	29.5	12.5	32.7	34.7	-3.0	34.8	30.5	9.4
<i>Panel B: welfare bills</i>											
1964 Econ. Opportunity Act	44.9	10.1	45.5	9.0	11.3	44.9	10.2	-0.5	45.5	9.0	10.8
1965 Elem. and Sec. Educ. Act	37.0	26.0	38.6	22.9	12.0	36.9	26.2	-0.7	38.5	23.1	11.3
1965 Social Security Act	27.6	44.9	29.2	41.7	7.1	27.5	45.0	-0.4	29.1	41.9	6.6
<i>Panel C: crime bills</i>											
1968 Gun Control Act	45.5	9.0	46.7	6.6	26.7	44.0	12.0	-32.3	45.2	9.5	-5.6

TABLE 13: Specification check

	civr. & welfare (1)	environment (2)	defense (3)	crime (4)
last-period protests				
...peaceful	-0.126** (0.055)	-0.064 (0.060)	0.033 (0.085)	-0.024 (0.112)
...violent	0.176 (0.150)	0.192* (0.108)	-0.042 (0.273)	0.734*** (0.184)
prior protest history				
...peaceful	-0.176* (0.101)	-0.058 (0.087)	0.055 (0.135)	-0.084 (0.169)
...violent	0.453* (0.231)	-0.197 (0.174)	-0.107 (0.322)	0.663** (0.327)
sample size	1798	1798	1798	1684

Outcome values are scaled to fall between 0 and 100. Standard errors are two-way clustered by district and state-by-congress. Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.